

Form and formalism in linguistics

Edited by

James McElvenny

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Preface

Notions of “form” have a long history in Western thought on language. When linguistics emerged as an institutionalized discipline in the early decades of the nineteenth century, its practitioners could look back on a multitude of senses and uses of “form”, embedded in a variety of conceptual schemes. Even though many nineteenth-century linguists sought to emphasize the novelty of their work and imagined a radical break with the “pre-scientific” past (see [Morpurgo Davies 1998](#): chap. 1), both their everyday practice and their theoretical views were permeated by an intellectual inheritance stretching back over centuries, in which “form” occupied a central place.

On a practical level, “form” has long been employed in a general sense to refer to the perceptible outer appearances of linguistic expressions, especially in connection with the inflectional variants of words. On a deeper theoretical level, there has often been an effort to find underlying motivations for these appearances and so conceive of “form” in senses loaded with metaphysical and epistemological significance. This was the path taken by such movements as the medieval Scholastics and the Enlightenment-era General Grammarians (see [Law 2003](#): chaps. 8 and 11), whose successors in the nineteenth century – despite often disavowing their predecessors – were similarly engaged in a search for the cognitive, biological or aesthetic bases of linguistic form.

A particularly prominent figure in nineteenth-century discussions of form in language was Wilhelm von Humboldt (1767–1835), whose writings served as the point of departure for many later scholars. For Humboldt and his followers, there is a sense in which all language is form and nothing else, in that language is the representation we make of the world which, in Kantian fashion, we shape according to our perceptive faculties. ‘The essence of language’, writes [von Humboldt \(1905 \[1820\]](#): 17), ‘consists in pouring the material of the phenomenal world into the form of thoughts.’ (*Das Wesen der Sprache besteht darin, die Materie der Erscheinungswelt in die Form der Gedanken zu giessen.*) A commonplace among the Humboldtians was to claim that each language has its own characteristic form of representation discernible in the form of its expressions. The task of the linguist is to capture these forms and analyse them for what they reveal about the mental,

cultural and physical life of language speakers (see Morpurgo Davies 1998: chap. 5; Trabant 1986; McElvenny 2016).

The centrality of form to linguistic scholarship continued into the structuralist era. The *Cours de linguistique générale* of Ferdinand de Saussure (1857–1913) famously contains the assertion that ‘language is a form and not a substance’ (*la langue est une forme et non une substance*) (de Saussure 1995 [1916]: 169). Following on from the earlier Humboldtian position, a fundamental tenet of structuralism is to conceive of languages as self-contained structures imposed on the material substrate of the world. In describing phonological, grammatical and semantic apparatuses of languages, the structuralist is engaged in an investigation of linguistic form (for a classical structuralist account couched in these terms, see Lyons 1968: 54–70).

In the generativist era, Noam Chomsky’s (b. 1928) efforts to construct an intellectual genealogy for his work involved an attempted appropriation of Humboldtian “form”, which rekindled awareness of these ideas in mainstream linguistics. In his *Cartesian linguistics*, Chomsky (2009 [1966]: 69–77) sought to assimilate Humboldtian form to his own innovation of generative rules as the underlying system that allows for the creative use of finite means to produce an infinite array of expressions.

The fecundity of “form” is visible not only in its polysemy, but also in the family of derivatives it has brought into the world, including such terms as “formal”, “formalized” and “formalist/formalism”. Like their parent, these terms defy concise definition, although when applied as labels to directions in linguistic research they generally imply concentration on internal systematicity to the exclusion of external explanatory factors alongside an inclination to abstraction and axiomatization – two tendencies that may in fact manifest independently of one another (cf. Newmeyer 1998). As is explored in several contributions to this volume, formalism as a research mindset is at home in many fields – such as logic, mathematics, aesthetics and literary studies – and represents an area of rich historical cross-pollination between linguistics and other disciplines.

In a separate but related sense, “formalism” as a count noun refers to the devices employed in the representation and analysis of phenomena. Various formalisms in this sense, along with the theoretical views to which they are tied, are also examined in the following chapters.

In composing this volume, we have come together as historians of science and philosophers of language and linguistics to take a critical look at notions of form and their derivatives, and the role they have played in the study of language over the past two centuries. We investigate how these notions have been understood

and used, and what this reveals about the way of thinking, temperament and daily practice of linguists.

The first contribution to our volume is Judith Kaplan's examination in ?? of the role of visual formalisms in representing genealogical relationships between languages. Engaging with some of the latest literature on material culture in the history of science, Kaplan explores how visual diagrams and metaphors helped in grasping relationships between languages in comparative-historical grammar, from the nineteenth century up to the present day. She finds that the tensions between the dominant models of language relationship – “tree” versus “wave” models – were typically conceived in a visual mode, whether this was explicitly represented in a diagram or initially described only as a visual metaphor. She observes shifting commitments to the realism of representations and mutual influences between linguists and those working in neighbouring sciences.

In ??, James McElvenny compares competing nineteenth-century accounts of “alternating sounds” – a cover term for the apparent unstable phonological variation found in “exotic” languages – for the different attitudes towards linguistic form that they reveal. The traditional view took alternating sounds to be a feature of “primitive” languages, which were assumed to have not attained the levels of formal arbitrariness characteristic of European languages. Franz Boas (1858–1942) famously refuted this view by insisting that all languages have fully developed phonologies and ascribed alternating sounds to perceptual error on the part of outside observers. Georg von der Gabelentz (1840–1893), on the other hand, embraced the phenomenon and wielded it against Neogrammmarian doctrine, the leading formal theory of his day. Both Boas and Gabelentz' positions can claim a measure of theoretical sophistication and at the same time contain obvious faults. McElvenny places these positions in their historical context and considers why Boas' view was so well received in linguistics while Gabelentz' was not.

?? turns to the links between linguistic, psychological and, above all, aesthetic theory in the work of Edward Sapir (1884–1939). In this chapter, Jean-Michel Fortis provides a detailed exposition of Sapir's writings on form in language, concentrating in particular on Sapir's notion of “form-feeling” and following the trail – in some places explicitly marked by Sapir himself and in others reconstructed by Fortis through terminological and conceptual detective work – to identify his sources of inspiration. Fortis places Sapir in a finely interlaced intellectual network spanning across contemporary *Gestalt* psychology and German art theory, with a heritage extending at least as far back as the Romantic period around the turn of the eighteenth to the nineteenth century.

The focus on Sapir continues in , where Els Elffers critically compares Sapir's

philosophy of science to that of Jerry Fodor (1935–2017) and examines the implications of their views for the treatment of linguistic form. Looking at Sapir’s arguments against the “superorganic” in language scholarship and Fodor’s proposal for “token physicalism”, she finds striking similarities between the two, despite their very different intellectual contexts: Sapir was responding to ideas in anthropology emerging from debates about the nature of the *Geisteswissenschaften* in contrast to the *Naturwissenschaften*, whereas Fodor was responding to logical positivism. Both scholars, however, concerned themselves with how best to demarcate the individual sciences, with the specific example of linguistics in mind, and settled on the principle of demarcating the sciences not according to their subject matter but the way in which that subject matter is conceived.

In ??, Bart Karstens undertakes a re-examination of the genesis of linguistic structuralism and its early interaction with Russian Formalism, a school of literary analysis from the early twentieth century. Karstens engages in a detailed investigation of the scholarly network around Roman Jakobson (1896–1982) and his role as a vector for the transmission of Russian Formalism first to the Prague School of structuralism in the 1920s and then later in the United States. While formalist doctrine was often heavily criticized by the early structuralists, Karstens shows that various formalist views informed elements of early structuralism.

A similar story of “resistant embrace” is told in ??, where John Joseph reconsiders the place of structuralism in French linguistics of the mid-twentieth century, before the onset of the “post-structuralist” period. Focusing on such figures as Emile Benveniste (1902–1976), Henri Meschonnic (1932–2000), Aurélien Sauvageot (1897–1988) and their closest contemporaries, Joseph demonstrates that each of these figures has a complex relationship to structuralism: at times criticizing the apparent premises of the approach while employing recognizably structuralist forms of analysis, or publicly avowing structuralism while straying away from its principles in their own work.

In ??, Ryan Nefdt surveys some of the radical changes in theory that generative linguistics has undergone in its short history and derives from them positive lessons for the philosophy of science. Amid the turbulence and instability that has characterized generative theory, he identifies one constant: the formal structures in language that generative linguists describe. With the durability of this constant in mind, he advocates for a position of structural realism in the philosophy of linguistics. Such a position, he argues, would allow linguists to escape pessimistic meta-induction – that is, the notion that we must necessarily expect our theories to one day be refuted and superseded – and allows them to step away from the ontology of natural languages, thereby securing the epistemolog-

ical basis of the formal approach to language.

The gaze of the last two chapters in our volume is directed towards current questions in the philosophy of linguistics, specifically the role of normativity and authority in language description. Drawing on a broad range of sources in linguistics and analytic philosophy over the past fifty years, Geoffrey Pullum, in ??, develops a new perspective on the classical distinction between prescriptivism and descriptivism in grammar. He contends that the value of a grammatical description lies in the precise, formalized account it provides of a particular set of linguistic practices, which can guide those who may wish to participate in those practices. In serving as a guide, every grammar has normative force, but is not necessarily prescriptive: the grammar-reader may follow its advice but is not compelled to do so.

In ??, Nick Riemer identifies the ideologies of language he sees embodied in the “unique form hypothesis”, the assumption that every linguistic expression can be reduced to a single, universally agreed underlying representation. While linguists might seek to distance themselves from this hypothesis and its implications, it is, argues Riemer, a recurring motif in linguistics, especially prominent in the teaching of the discipline. Its effects in education are particularly pernicious, since teachers, due to the exigencies of pedagogy, can usually offer no justification for the unique forms they present to their students other than their arbitrary authority, a practice that reinforces unreflective submission to authority, both at university and in life. Acknowledging that most linguists would shudder at such consequences, Riemer pleads for greater open-mindedness among linguists towards critique of the discipline’s foundations.

Although dealing with a broad range of topics from diverse perspectives and in different styles, this volume is the product of concerted collective effort. Each of us came to this project with existing ideas about form and formalism in linguistics. These ideas we set out in draft chapters, which we discussed in person at a meeting in Edinburgh in August 2018. After our meeting, we revised the chapters to reflect the insights gained through our discussion. It is these revised chapters, shaped and harmonized by our dialogue, that are contained in this volume.

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Chapter 1

Alternating sounds and the formal franchise in phonology

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A matter of some controversy in the intersecting worlds of late nineteenth-century linguistics and anthropology was the nature of “alternating sounds”. This phenomenon is the apparent tendency, long assumed to be characteristic of “primitive” languages, to freely vary the pronunciation of words, without any discernible system. Franz Boas (1858–1942), rebutting received opinion in the American anthropological establishment, denied the existence of this phenomenon, arguing that it was an artefact of observation. Georg von der Gabelentz (1840–1893), on the other hand, embraced the phenomenon and fashioned it into a critique of the comparative method as it was practised in Germany.

Both Boas and Gabelentz – and indeed also their opponents – were well versed in the Humboldtian tradition of language scholarship, in particular as developed and transmitted by H. Steinthal (1823–1899). Although the late nineteenth-century debates surrounding alternating sounds were informed by a number of sources, this chapter argues that Steinthal’s writings served as a key point of reference and offered several motifs that were taken up by his scholarly successors. In addition, and most crucially, the chapter demonstrates that the positions at which the participants in these debates arrived were determined not so much by any simple technical disagreements but by underlying philosophical differences and sociological factors. This episode in the joint history of linguistics and anthropology is telling for what it reveals about the dominant mindset and temperament of these disciplines in relation to the formal analysis the world’s languages.



1 Introduction

Phonology is in many ways the promised land of formal conceptions of language. The apparent orderly transmutation of sounds over time stimulated the mechanical minds of historical-comparative linguists, ultimately inspiring the Neogrammarians to their postulation of exceptionless sound laws. The vanguard of linguistic formalism in subsequent generations continued to look to sound patterns – although now chiefly in their synchronic aspect – as the pristine embodiment of the self-contained systems they sought. In this way, the classical American structuralist grammar sets out from the firm ground of phonology and ascends to increasingly less regular linguistic levels.

But a question that remained controversial into the last decades of the nineteenth century was just how far the formal franchise in phonology should be extended. Do the sound systems of all languages of the world meet the standards of arbitrariness and regularity identified in the Indo-European languages? An apparent phenomenon prevalent in the “primitive” languages of the Americas, Africa and the South Seas suggested limits to law-governed language. European scholars and adventurers who tried to learn and transcribe the words of these languages were frequently frustrated by the way in which native informants would seemingly change the pronunciation of the same word from utterance to utterance. From the perspective of present-day phonological theory, this phenomenon would be considered variously a manifestation of free variation, allophonic variation and difficulty perceiving articulations markedly foreign to the recorder’s own phonological system. Nineteenth century scholars, by contrast, conceptualized this phenomenon in a number of different, competing ways. These differences in conceptualization led to terminological instability, but a common cover term, also adopted here, was “alternating sounds”.

This chapter explores some responses from prominent language scholars in the mid- to late nineteenth century to the phenomenon of alternating sounds, and looks at what these responses reveal about the underlying philosophical commitments and sociological structure of the intersecting fields of anthropology and linguistics in this era. The investigation spans the intellectual worlds of America and Germany which, although closely intertwined, were organized around different disciplinary structures. The figures featured here who were active in America described themselves as anthropologists, for whom linguistic research was one of the “four fields” of American anthropology.¹ The corresponding German dis-

¹The essays contained in [Kuklick \(2008\)](#) provide an excellent comparative overview of the history of anthropology in America and Europe, including their disciplinary structures.

cussion, on the other hand, took place largely within the discipline of linguistics, in which the study of “exotic” languages was a niche pursuit. The exception is the work of H. Steinthal (1823–1899), who is put forward in this chapter as an inspiration to – and therefore link between – both the German and American worlds. His *Völkerpsychologie*, developed with his collaborator M. Lazarus (1824–1903), strove to offer an all-encompassing scientific account of human culture, history and society.²

The starting point for this chapter, in section 2, is the 1889 paper “On alternating sounds” by Franz Boas (1858–1942), a milestone marking the way to modern explanations of alternating sounds and modern views on the equality of all languages. Here Boas rebutted the received position of the American anthropological establishment, represented in particular by such luminaries as Daniel Garrison Brinton (1837–1899) and John Wesley Powell (1834–1902), which held that the alternating sounds observed in American languages were a manifestation of their alleged primitiveness. Boas argued, by contrast, that the alternating sounds were an illusion caused by the conflicts of incommensurable phonological systems in informant and ethnographer.

From a present-day perspective, this episode may seem like a simple case of science triumphing over naivety and prejudice. But arguments presented on both sides of the American debate could claim some degree of theoretical sophistication. Indeed, Brinton and Boas shared a key source of theoretical inspiration in the work of Steinthal, whose views were in turn anchored in the linguistic writings of Wilhelm von Humboldt (1767–1835). While phonological issues occupy at most a peripheral place in Steinthal’s work, aspects of his linguistic and psychological theory would seem to have informed the later debate. Section 3 offers an account of the nuanced views advanced by Steinthal and their possible links to later arguments.

Despite its now canonical status, the American debate was not the only reconsideration of principles of phonological regularity around the turn of the nineteenth to the twentieth century. In Germany, Georg von der Gabelentz (1840–1893), also drawing on the Humboldtian tradition as transmitted by Steinthal, affirmed the existence of alternating sounds, in a turn that could be seen as prefiguring key features of later phonemic theory. Like Boas, Gabelentz fashioned his treatment of alternating sounds into a critique of the linguistic establishment. But unlike Boas, Gabelentz’ goal was not to extend the formal franchise to all languages, but rather to redefine it and thereby challenge the comparative method

²For a detailed account of *Völkerpsychologie* from its beginnings with Steinthal and Lazarus to its later developments and ultimate fate, see Klautke (2013).

as it was practised at the time. Section 4 looks at Gabelentz’ proposals for alternative methods in historical-comparative linguistics and their rather unfavourable reception.

Finally, section 5 brings the American and German debates together to discuss what they reveal about the dominant mindset and temperament in the intersecting fields of linguistics and anthropology in relation to questions of the nature and correct treatment of linguistic form.

2 Alternating sounds in America

Boas’ (1889) “On alternating sounds” occupies a prominent place in the standard disciplinary narrative of linguistic anthropology as a text that helped to establish the scientific foundations of the field. According to this story, Boas overcame contemporary evolutionary prejudice by demonstrating that an alleged characteristic of “primitive” languages was in fact nothing more than an artefact introduced by insufficiently trained observers.³ Alternating sounds, in various guises, were a recurring motif in the description of exotic languages throughout the nineteenth century, but the two key figures against whom Boas developed his position were Brinton and Powell, the leading anthropologists of the previous generation.⁴

In the year before Boas’ seminal article appeared, Brinton reaffirmed several tropes about “primitive” languages in an 1888 address to the American Philosophical Society, “The Language of Palæolithic Man” which, in an 1890 volume of his collected papers, became “The earliest form of human speech, as revealed by American tongues” (Brinton 1890 [1888]). As the titles suggest, Brinton sought insights into the nature of the earliest stages of human language evolution through an examination of the supposedly characteristic features of American languages. While much of Brinton’s paper focuses on the lexical and grammatical properties of these languages, it begins with a discussion of their phonological features.

Primitive speech, in Brinton’s assessment, has not yet attained the levels of arbitrariness and fixedness that characterize the more developed languages: in European languages individual sounds carry no sense, words have fixed sound

³“Evolutionary prejudice” was the term later used by Boas’ student Edward Sapir (1884–1939) to describe the assumption that the world’s languages can be categorized according to their putative level of grammatical development (see Sapir 1921: 130–132).

⁴On the relationship between Boas, Brinton and Powell in the context of late nineteenth-century American anthropology, see Darnell (1988) and Darnell (1998). See also Laplantine’s (2018) preface to her translation of Boas’ (1911) *Handbook of American Indian Languages* for a succinct summary of his life and work in context.

forms, and the articulated word alone is enough to convey its meaning. American languages, by contrast, frequently attach meaning to individual phonetic segments (Brinton 1890 [1888]: 394), word meaning is often modified by such devices as “[t]one, accent, stress, vocal inflection, quantity and pause” (Brinton 1890 [1888]: 399) that are not reducible to graphic writing, and sounds in words can vary freely: “In spite of the significance attached to the phonetic elements, they are, in many American languages, singularly vague and fluctuating” (Brinton 1890 [1888]: 397). His concluding observation is that “[t]he laws of the conversion of sounds of the one organ into those of another have not yet been discovered; but the above examples, which are by no means isolated ones, serve to admonish us that the phonetic elements of primitive speech probably had no fixedness” (Brinton 1890 [1888]: 398-399).

Under the name of “synthetic sounds”, this same phenomenon of apparent fluctuating phonology in American languages found a place in Powell’s (1880 [1877]) *Introduction to the Study of Indian Languages*. Given Powell’s influential position as director of the Bureau of American Ethnology, which was founded on his initiative in 1879, the *Introduction* achieved widespread use in the recording of American languages, not only in projects officially sponsored by the Bureau, but also in the efforts of other researchers and amateurs, including Boas and his students (see Darnell 1998: 50-51).

Powell was very conscious of the difficulties associated with capturing the phonology of American languages, and his commitment to scientific rigour led him to commission the noted Sanskrit scholar and general linguist William Dwight Whitney (1827–1894) to devise a standardized alphabet for recording American languages. Despite Powell’s efforts to encourage its use, the alphabet was generally considered inadequate and impractical by many of those who worked for the Bureau. Whitney himself felt no great attachment to the alphabet, regarding its design and implementation not as a theoretical task but merely a matter of expedience (see Darnell 1998: 50-51). For Powell, however, the alphabet was a foundational element of language description: his *Introduction* opens with a sophisticated discussion of articulatory phonetics and the principles of accurate transcription, which observes a number of phonological peculiarities of American languages still recognized today, such as ejective consonants (“interrupted sounds”) (Powell 1880 [1877]: 1-16).

“Synthetic sounds” appear in this discussion as another characteristic of American phonologies. Powell (1880 [1877]: 12) speaks of the “indefinite character of some of the sounds of a[n American Indian] language”, although this is not due to the chaotic variation imagined by Brinton but rather because the sounds are

“made by the organs of speech in positions and with movement comprehending in part at least the positions and movement used in making the several sounds to which they seem to be allied”. That is, Powell believes these “synthetic” sounds are insufficiently “differentiated” – they are produced by articulating several simple sounds at once. Through historical sound change, such synthetic sounds have been simplified and disappeared from the European languages, but this is a process yet to take place in the American languages. In their present undifferentiated state, these sounds “will be heard by the student now as one, now as another sound, even from the same speaker.” There is, however, a trace of humility in Powell’s approach to the American languages, an admission that science may not yet have fully grasped the principles underlying this phenomenon: “When the phonology of our Indian tongues is thoroughly understood, much light will be thrown upon the whole science of phonology [...]” (Powell 1880 [1877]: 13).

In response to views of the kind put forward by Brinton and Powell, Boas argued that such sounds are not a peculiarity of primitive languages at all, but rather the result of perceptual error on the part of the language researcher. All languages, European and American alike, make use of a fixed and finite repertoire of the total range of sounds that can be produced by the human articulatory organs. When an observer encounters a sound in a foreign language that is not present in their native repertoire, they will “apperceive” it as a similar sound that is in their repertoire. A term with a long history and a diverse range of uses, “apperceive” became in the early nineteenth century part of the technical apparatus of John Friedrich Herbart’s (1776–1841) associational psychology, from where it was taken up into the *Völkerpsychologie* of Steinthal and Lazarus, and later into the *Bewusstseinspsychologie* of Wilhelm Wundt (1832–1920).⁵ Boas’ invocation of “apperception” is too fleeting and off-hand to align him with any specific school of psychology at the time, but his usage attests to a familiarity with contemporary psychological jargon and a desire to dress his own work in the latest technical garb.

According to Boas, the mapping from foreign to native sound that results through the process of apperception may vary from occasion to occasion, creating the illusion of alternating sounds. The presence of this perceptual filter on the part of the observer is demonstrated by the fact that “the nationality even of well-trained observers may be readily recognized” in the transcriptions they make of foreign sounds (Boas 1889: 51). Boas sums up his argument with the following words:

⁵For a recent survey of approaches to what can retrospectively be called “psycholinguistics” in this period, including the work of Lazarus, Steinthal and Wundt, see Levelt (2013).

I think, from this evidence, it is clear that all such misspellings are due to a wrong apperception, which is due to the phonetic system of our native language. For this reason I maintain that there is no such phenomenon as synthetic or alternating sounds, and that their occurrence is in no way a sign of primitiveness of the speech in which they are said to occur; that alternating sounds are in reality alternating apperceptions of one and the same sound. A thorough study of all alleged alternating sounds or synthetic sounds will show that their existence may be explained by alternating apperceptions. (Boas 1889: 52)

Boas was no doubt correct to impugn the perception of his colleagues in many cases where they accused American languages of phonetic fluctuation. But it must be acknowledged that the potential for cross-linguistic phonological interference was already well recognized in the literature of the time. Powell (1880 [1877]: 2) noted this difficulty in his own guide to transcription:

[T]here are probably sounds in each [Indian language of North America] which do not appear in the English or any other civilized tongue; [...] and further, [...] there are perhaps sounds in each of such a character, or made with such uncertainty that the ear primarily trained to distinguish English speech is unable to clearly determine what these sounds are, even after many years of effort. (Powell 1880 [1877]: 2)

As is shown in the following sections, this awareness of cross-linguistic interference is clear in many other contemporary and antecedent sources, where it co-existed with a range of different attitudes to alternating sounds. A scholar's stance in relation to these questions was therefore shaped to a very large degree by beliefs and commitments beyond the immediate language data.

A key motivation for Boas was of course to subvert the then current discourse of primitive languages and language evolution. But this was not his only aim, and indeed this subversion was at least in part beholden to other goals. Although he enjoyed mostly respectful and collegial relations with both Brinton and Powell, Boas was always engaged in a project to proclaim his superior scientific expertise and secure institutional support for his clique of students and adherents. The chief and most valid source of data in Boasian anthropology were the descriptions made and texts recorded by the scientifically trained observer in a fieldwork situation. By contrast, Brinton, the doyen of the previous generation, relied mainly on the critical philological analysis of written documents that had been collected and compiled by others (see Darnell 1988: 21-24). By diminishing

existing written documentation, Boas' critique undermined the legitimacy of the mode of research employed by Brinton and boosted his own fieldwork-oriented approach.

Even among confirmed fieldworkers, Boas' critique helped to assert the exclusive expertise of his own school. In later years, Boas developed a reputation for his domineering role in the world of Americanist anthropology, freely blocking the work of researchers who did not meet his frequently quite arbitrary standards (see Darnell 1998). Pointing out the technical inadequacies of his predecessors, as in the case of alternating sounds, served this end well. In his 1911 *Handbook of American Indian Languages*, which was explicitly intended to supersede Powell's (1880 [1877]) *Introduction*, Boas' doctrine of the conditioned apperception of foreign sounds is incorporated as part of the propaedeutic guide to the correct recording of American languages, as a simple and uncontroversial methodological principle (see Boas 1911: 16-18).

That assertions of expertise are a decisive factor in Boas' campaign is demonstrated by his enduring commitment to the possibility of objective observation in language documentation. While previous transcribers of American languages may have been afflicted with a phonological filter, the goal of the Boasian anthropologist must be to eliminate this interference altogether. Even after the importation and elaboration of phonemic theory in America, Boas maintained a preference for fine-grained phonetic transcription. It was not enough for the observer to simply enter the foreign phonological system; they had to step outside phonology and record the given phonetic datum as accurately as possible.⁶ Boas' zeal extended to correcting written texts from one of his native speaker informants, which were essentially phonemic in nature, to include as much phonetic detail as possible (see Anderson 1985: 204-208). Even the phonemic testimony of the native speaker did not pass Boasian muster.⁷

⁶Another perspective from which Boas' position should perhaps be explored is that of contemporary debates on the "personal equation" in recording data, which were prominent across the natural sciences (see Schaffer 1988) and also played a role in attitudes to fieldwork in anthropology (see Kuklick 2011). I thank Judith Kaplan for drawing my attention to these debates.

⁷A further piece of circumstantial evidence is perhaps Boas' work on a revised standard alphabet for American languages. After Powell's death in 1902, Boas was asked by William John McGee (1853-1912), Powell's successor at the Bureau of American Ethnology, to form a committee to update the Bureau's alphabet. The resulting system, published 1916, clearly contains many compromises between various conflicting constraints, but the overall Boasian impulse towards greater phonetic detail and specialist exclusivity is quite apparent (see Darnell 1998: 195-197).

3 Steinthal and the Humboldtian tradition

The American debate on alternating sounds was shaped by a number of influences: the three figures mentioned in the previous section – Brinton, Powell and Boas – all had broad backgrounds spanning the natural sciences and humanities that informed their attitudes and approaches (see Darnell 1998). But a central point of reference – in particular for Brinton and Boas – was the Humboldtian tradition of linguistic scholarship as it was interpreted and propagated by Steinthal. Boas had met Steinthal personally in Berlin and freely acknowledged Steinthal's influence on his linguistic research. Brinton was the leading Humboldt scholar in America and frequently cited Steinthal (see Bunzl 1996: 63–69; Trautmann-Waller 2006: 289–292). Although the questions of phonology and language documentation that lay at the heart of the American debate on alternating sounds are addressed only at the periphery of Steinthal's work, we see in his texts several threads unpicked and woven into the later accounts.

Steinthal's great achievement in linguistics was to construct a monolithic theoretical edifice dealing with issues ranging from the mental processes underlying individual language use to language evolution and typology, and to attempt an empirical demonstration of these principles through detailed investigations into the languages of the world. Through his collaboration with Lazarus from the 1850s onwards, Steinthal's linguistics became a central component of the broader project of *Völkerpsychologie*.⁸

Following Humboldt, Steinthal imagined an “idea of language” (*Sprachidee*), an ideal form towards which linguistic expression strives. The evolution of language passes through three stages on the way to the full realization of this ideal; these stages are recapitulated in child language acquisition and can be discerned in the contours of “primitive” languages (cf. Bumann 1965: 81–93). The first stage consists in self-awareness, the psychological attainment that distinguishes humans from animals. Unlike animals, humans can represent, share and understand their “intuitions” (*Ansschauungen*), which they “apperceive” (*appercieren*) in their consciousness. Here Steinthal invokes the core notion of “apperception” from Herbartian associational psychology; this is the same term that Boas would later use in generic form (see section 2 above).

At the first stage language is made up of nothing more than “reflex sounds” (*Reflexlaute*), which merely represent and communicate intuitions in an unanalyzed

⁸Trautmann-Waller (2006) is a comprehensive intellectual biography of Steinthal, which examines his linguistic work and *Völkerpsychologie* in depth. For studies of Steinthal's linguistics, see Bumann (1965) and Ringmacher (1996).

way. These sounds are brought forth through unreflected action and are solely mimetic in character. The further development of language occurs as speakers become increasingly aware of the thoughts they entertain in consciousness and begin to analyze them. At the second stage, language progresses beyond reflex sounds to a proper conscious analysis of thoughts. It is at this point that sentence structure develops, with a distinction between subject and predicate and individual words that can be abstracted from the sentence as a whole:

It is therefore already at the point where language first appears in its true quality, where it achieves its full intellectual character, that it breaks through onomatopoeia. And *words in their true conception develop only with the development of the sentence form; that is, simultaneously with the opposition of subject and predicate*, which soon establishes itself as the difference in the naming of things and expressions for circumstances and changes. The logical character of words seems to be decisively hostile to their onomatopoeic origin. (Steinthal 1881 [1871]: 424-425)⁹

At the third stage of evolution, language continues its ascent from its mimetic origins: the etymological bond between words and their meanings fades from consciousness and the connection between them becomes truly arbitrary.

For Steinthal, the crucial moment in language evolution is the second stage, since this is the point at which “inner linguistic form” (*innere Sprachform*) emerges (Steinthal 1881 [1871]: 425-426). “Inner linguistic form” is a term that first appears in Humboldt’s (1998 [1836]) introduction to his work on the Kawi language of Java. The term is therefore generally associated with Humboldt, even though, as Borsche (1989) definitively demonstrated, its elaboration into a theoretical construct is the later work of Steinthal. In Steinthal’s hands, inner form became a wide-ranging concept covering all aspects of the immanent structure of languages. Like “apperception”, “inner form” grew in the second half of the nineteenth century into a favourite but rather indefinite term in linguistic and philosophical scholarship. Despite the explosion of senses attached to the term in this period, its ultimate origin in Humboldt’s essay and its deep association

⁹Original: “Also gerade schon da, wo die Sprache zuerst in ihrer wahren Eigentümlichkeit auftritt, wo sie ihren vollen intellektuellen Charakter gewinnt, durchbricht sie die Onomatopoeie; und das Wort in seinem wahren Begriff entsteht erst mit der Satzform, also zugleich mit dem Gegensatze von Subjekt und Prädikat, der sich bald zu dem Unterschiede der Benennung von Dingen und der Ausdrücke für Zustände und Veränderungen festsetzt. Der logische Charakter des Wortes scheint dem onomatopoetischen Ursprunge desselben entschieden feindlich zu sein.” Italics in this quotation renders *Sperrung* in the original.

with Steintal's work remained foremost in the minds of those who employed it. Both Brinton and Boas keenly spoke this idiom and acknowledged the tradition with which it was aligned: Brinton constantly advocated for attention to the inner form of languages and Boas (1911: 81) set capturing the unique inner form of each language as the goal of the language sketches in his *Handbook* (cf. Darnell 1988: 98-105).

Steintal's typological efforts were aimed at assessing how far towards the "idea of language" the inner form had progressed in different languages and at identifying the grammatical means – such as morphological or syntactic structures – in which it manifests itself. His 1860 *Charakteristik der hauptsächlichsten Typen des Sprachbaues* provided a survey and classification of the world's languages, in which the primary division is between those language with properly developed inner form (*Formsprachen*) and those without (*formlose Sprachen*). This work was followed by his 1867 *Mande-Neger-Sprachen*, which subjected several Mande languages of Africa – Mandingo, Bambara, Soso and Vai – to a detailed examination that revealed alleged developmental deficiencies in all aspects of their inner forms. This examination is based on a philological analysis of existing written sources, similar to the preferred research practice of Brinton. The analysis proceeds from both a "phonetic" (*phonetisch*) perspective, which looks at the grammatical apparatus of the languages, and a "psychological" (*psychologisch*) perspective, which investigates how expressions are formed.¹⁰

In his "phonetic" examination of the Mande languages, Steintal found no way to distinguish individual words from the sentences in which they appear: there are allegedly no phonological processes observed to operate only at the word level distinct from the sentence as a whole. In their grammars, the languages supposedly rely on mechanisms that are not truly arbitrary, such as the "interjectional" process of reduplication, used for a variety of purposes in the languages. The grammatical affixes and particles that can be identified in the languages all seem to have transparent etymologies that link them to "material" words, which keep them bound to their mimetic origins.

From the "psychological" perspective, the Mande languages did not fare any better. Steintal's assessment of how various meanings are rendered using the lexical and grammatical means available in the languages reveals that the Herbartian processes of "isolation" (*Isolierung*) and "condensation" (*Verdichtung*) of "representations" (*Vorstellungen*) in the minds of speakers are not carried out properly. The inevitable conclusion for Steintal (1867: 255) is that the speakers of

¹⁰For a discussion of the historical background to this dual-perspective approach to language description, see McElvenny (2017: 2-6).

Mande languages have not completely raised their “intuitions” to the level of “representations”: “in the consciousness of the Mande negro the concrete intuition with its material relations is still dominant, and its conversion into representations is not carried out completely”.¹¹

Up to this point, Brinton’s account of the “primitive” phonological features of American languages accords well with Steinthal’s story of language evolution. The alleged lack of arbitrariness and fixedness Brinton identified in the sounds of American languages are features that could be expected of languages at Steinthal’s first stage of evolution. Steinthal in fact considered the possibility that a lack of arbitrariness in the earliest languages could lead to greater variability, since the sounds produced by reflex are bound to the mental moment and subject to all of its modifications:

We may think that language, as long as it is still the immediate creation of the excited soul, shares in the fluctuations and inequalities of these excitations. So just as the representation, even though its content is the same, is not always the same in its psychological behaviour – e.g. not always as lively and energetic to the same degree, vivid, strongly concentrated – the word, as the reflex of this representation, is not always the same. The energy of thinking expresses itself most immediately in intonation, then also in the sharpness of articulation, i.e. the clearness and definiteness of the sound. And both together most certainly influence the quality or even the content of the sound, the way in which it is articulated. (Steinthal 1867: 3-4)¹²

But such questions remained hypothetical for Steinthal. According to Steinthal (1867: 3-4), the “uncivilized peoples” (*culturlose Völker*) living today are not the *Natur-Völker* of the earlier stages of human evolution. He accepted a greater degree of variation in the sounds of the languages of “uncivilized peoples” because

¹¹Original: “im Bewußtsein des Mande-Negers ist die concrete Anschauung mit ihren materiellen Verhältnissen noch vorwiegend, und ihre Umsetzung in Vorstellungen ist unvollständig vollzogen.”

¹²Original: “Wir dürfen uns denken, daß die Sprache, so lange sie noch die unmittelbare Schöpfung der erregten Seele ist, auch an den Schwankungen und Ungleichheiten dieser Erregungen Theil hat. Wie also die, obschon ihrem Inhalte nach gleiche und selbe, Vorstellung doch in ihrem psychologischen Verhalten nicht immer gleich ist, z.B. nicht immer gleich lebendig und energisch, gleich anschaulich, gleich kräftig concentrirt: so lautet auch das Wort, als der Reflex dieser Vorstellung, nicht immer gleich. Die Energie des Denkens drückt sich am unmittelbarsten in der Weise der Betonung aus, dann auch in der Schärfe der Articulation, d. h. der Klarheit und Bestimmtheit des Lautes; und beides zusammen beeinflußt sicherlich die Qualität oder den Inhalt selbst des Lautes, die Weise seiner Articulation.”

these languages lacked the stabilizing and standardizing influence of an orthography, but even before the invention of writing, human language will “have established itself in the consciousness” and “its word forms [will] have crystalized in definite shape” ([...] *hat sich die Sprache im Bewußtsein gefestigt, sind ihre Wortformen in bestimmter Gestalt krystallisiert*; Steinthal 1867: 4-5).

In Steinthal’s estimation, the Mande languages find themselves in this situation: they stand uneasily on the threshold to the second stage of evolution, but their apparent phonetic inconstancy in comparison with European languages is not due to enduring mimetic reflexes but simply anarchy arising from the absence of a regulating instance. Steinthal (1867: 257-266) discounts the fact that the Vai do indeed possess a native writing system, since it is an imitation of European scripts fashioned without proper understanding of those scripts’ underlying principles. The result is a massive syllabary – of over 200 characters – lacking system and internal order, which is chiefly used by distinguished members of the community to write books containing “tales from the life of their authors, sayings, observations and fables – without any unity” (Steinthal 1867: 260).¹³ While the Vai may have a script, they do not have an orthography: they simply transcribe whatever pronunciation occurs to them as they write (Steinthal 1867: 264-266), and this can vary even within the same text.¹⁴

The perceptual problems to which Boas (1889) attributed alternating sounds in American languages were acknowledged by Steinthal in the case of the Mande languages. Steinthal (1867) critiqued the transcriptions found in all of his sources, commenting, among other observations, that the influence of the transcriber’s native phonology and writing habits had to be taken into consideration. On his English sources, he remarked:

Since we frequently have to rely on English works, the influence of the English ear and English orthography must be taken into account. However, although this influence may be responsible for some things, it is hardly responsible for everything. The same sources offer at times, both consciously and unconsciously, double forms, e.g. *bombong* and *bambang*, “hard” [...].

¹³Original: “Der Inhalt dieser Bücher besteht in der Erzählung von Ereignissen aus dem Leben ihrer Verfasser, in Sittensprüchen, Betrachtungen und Fabeln – ohne alle Einheit.”

¹⁴Steinthal (1852) presented an account of the development of writing from ideographic systems to alphabets. Like his language typology, this represented an evolutionary scheme in which language users became progressively more aware of the structure of their languages. The Vai syllabary has reached the upper echelons of a pure phonetic script – i.e. a script without ideographic elements – but has not reached the highest point of a full alphabetic script (Steinthal 1867: 262-264). The place of the Vai script in this hierarchy does not bear directly on the question of its consistency.

The most frequent alternation is perhaps that between *i* and *e*. (Steinthal 1867: 9)¹⁵

While phonology as such was never among Steinthal's core concerns, in his empirically oriented researches he was inevitably confronted with the practical difficulties that arise in reducing to writing the sounds of exotic languages with no native orthographic tradition. The dangers he identified in written materials produced by European observers were precisely those that Boas would later turn into the fatal failures of perception on the part of his predecessors. On the other hand, the corroboration Brinton provided for existing accounts of phonetic fluctuation in American languages could in principle be licensed by Steinthal's scheme of language evolution, although Steinthal explicitly denied that any language spoken today would still find itself at this most elementary stage. Steinthal accepted greater degrees of variation in the languages of "uncivilized peoples", but only because they lacked a standard imposed by authority.

4 Phonetic latitude and sound laws

Around the same time that Boas launched his attack against alternating sounds – but independently of the American debate – Georg von der Gabelentz marshalled related phonetic phenomena to mount a critique of the linguistic establishment in Germany. His opponents were the Neogrammarians, whose work was built upon an insistence on the exceptionless nature of sound change, and Gabelentz embraced the prospect of relative regularity in languages as a means to undermining this fundamental Neogrammarian tenet. As in the American context, a key theoretical reference in Germany – in particular for Gabelentz – was the work of Steinthal.

In his magnum opus, *Die Sprachwissenschaft*, von der Gabelentz (2016 [1891]: 341-384) undertakes an extensive investigation into contemporary linguistic typology that is essentially organized around the principles espoused by Steinthal.¹⁶

¹⁵Original: "Da wir mehrfach auf englische Arbeiten angewiesen sind, so darf hierbei der Einfluß des englischen Ohrs und der englischen Orthographie nicht unberücksichtigt bleiben. Indessen, er mag manches verschulden, schwerlich alles. Dieselben Quellen stellen zuweilen unbewußt und bewußt doppelte Formen auf; z.B. *bombong* und *bambang*, hart [...]. Am meisten vielleicht wechseln *i* und *e* mit einander."

¹⁶von der Gabelentz (1889) had already presented key parts of this section of his book in an address to the Saxon Academy of Sciences. An English translation can be found in McElvenny (In Press).

Gabelentz rejected the strong distinction between “formal” and “material” elements in language hypothesized by Steinthal and used by him to demonstrate the alleged inferior mental development of speakers of the Mande and other languages. Instead, argued von der Gabelentz (2016 [1891]: 380-384), linguistic form is the product of an aesthetic drive to achieve subjective self-expression.¹⁷ In his view, all shaping of linguistic expression, regardless of how transparently its origin shows through, is formal in nature (see McElvenny 2016). von der Gabelentz (2016 [1891]: 406-408) accepted, however, that in the most primitive stages linguistic forms would have been created spontaneously and freely, and only over time become constrained and fixed through force of collective habit.

Given the dominance of historical-comparative grammar in the disciplinary linguistics of his day, Gabelentz dedicates an entire “book”, or primary section, of his *Sprachwissenschaft* to this approach to language study. He finds that the principle of gradual fixing of the linguistic system applies also on the phonetic plane, and uses this principle both to critique the supposed exceptionless nature of sound change as promulgated by the Neogrammarians and as a means to explain how sound change can occur at all. “Fluctuating articulations” (*schwankende Artikulationen*), according to von der Gabelentz (2016 [1891]: 196), are a very real part of languages, and indeed they are the force driving sound change in the first place. If, as the Neogrammarians argued, sound change proceeded according to inviolable rules then everyone would always speak the same way. For sound change to occur, one speaker has to innovate a new pronunciation and then it has to spread to the rest of the speaker community. von der Gabelentz (2016 [1891]: 196-197) is very clear that he means not only variation in pronunciation between speakers, but also variation in the same speaker over the course of their lives and even from utterance to utterance.

The way in which Gabelentz describes the range of variation that each language allows in fact seems to evince an inchoate concept of the phoneme as an ideal sound which may have multiple realizations:

But languages, even the smallest dialects, distinguish only a certain number of sounds, which are related to individual phonetic phenomena like species to individuals, like circles to points; a language draws the boundaries more broadly or narrowly, but it always tolerates a certain degree of latitude. (von der Gabelentz 2016 [1891]: 35).¹⁸

¹⁷Jean-Michel Fortis, in Chapter 4 of this volume, examines similar aesthetic ideas in the work of Edward Sapir, and their possible connection to Gabelentz’ work.

¹⁸Original: “Die Sprache aber, und wäre es die kleinste Mundart, unterscheidet nur eine bes-

The “degree of latitude” allowed may vary from language to language, according to von der Gabelentz (2016 [1891]: 197-198), and this greater or lesser latitude provides the theoretical basis for countenancing the possibility of alternating sounds of a more extreme kind outside the familiar European languages.

A similar recognition of variation within limits is also a feature of Boas’ (1911) account of alternating sounds in the officially codified version of the *Handbook*. Here Boas admits variations in the realization of sounds in languages, but crucially he denies that the range or latitude of variation can vary from language to language: the American languages admit neither more nor less variation in their sounds than any other languages, and certainly no more than European languages. Taking the example of a sound in Pawnee, Boas (1911: 17) insists:

Thus the Pawnee language contains a sound which may be heard more or less distinctly sometimes as an *l*, sometimes an *r*, sometimes as *n*, and again as *d*, which, however, without any doubt, is throughout the same sound, although modified to a certain extent by its position in the word and by surrounding sounds. [...] This peculiar sound is, of course, entirely foreign to our phonetic system; but its variations are not greater than those of the English *r* in various combinations, as in *broth*, *mother*, *where*. (Boas 1911: 17)

Gabelentz’ theoretically grounded belief in varying degrees of latitude in pronunciation leads him, in contrast to Boas, to accept and repeat several well-known cases of alternating sounds from the corners of the world: von der Gabelentz (2016 [1891]: 202-204) offers examples from Samoan, Malay languages, Australian languages and of course various American languages. Gabelentz is willing to trust the data on alternating sounds delivered by scholars in the field, insisting that they are fully qualified observers who through extended immersion in the foreign language have had the opportunity to overcome the interference of their native phonology. Indeed, it is because they have become so accustomed to the phonological systems of the languages they record that they have developed the feeling for the languages that allows them to perceive the subtle alternating articulations:

We could raise the following objection: most of our informants were not schooled in the scientific observation of sounds; they judge the foreign

timtme Anzahl von Lauten, die sich zu den lautlichen Einzelerscheinungen verhalten wie Arten zu Individuen, wie Kreise zu Punkten; sie zieht die Grenzen weiter oder enger, immer aber duldet sie einen gewissen Spielraum.”

sounds according to their native language, and intermediate grades between these sounds seems at one moment to tend to one side and in another moment to another side. We may retort that at least some of these men have lived long enough among the aborigines that their ear has become as accustomed to the foreign language as it was previously to their native language. It is to this, or rather to their multilingual schooling, that they owe precisely this fine ability to hear that allows them to perceive those uncertain, fluctuating articulations. (von der Gabelentz 2016 [1891]: 204-205)¹⁹

In an inversion of the assignment of expertise effected by Boas, Gabelentz endorses the data and uses them to undermine the theoretical edifice of the Neogrammarians. Rather than refining sound laws to explain away exceptions, von der Gabelentz (2016 [1891]: 198) advocated statistical surveys that would embrace all variants observed, the deviants as well as the well-behaved regular forms. Gabelentz' model for this endeavour was perhaps the statistical analysis undertaken by William Dwight Whitney (1827–1894) of variant forms throughout the history of Sanskrit, published in his *Sanskrit Grammar* (Whitney 1896 [1875-1878]; cf. Silverstein 1971: vix-xx, xxii-xxiii).²⁰ Wilhelm Wundt similarly suggested that a statistical approach to the study of sound change may prove more fruitful than the absolutism of the Neogrammarians (see Formigari 2018).

Gabelentz' first steps towards applying a statistical method were taken in an 1893 address to the Berlin Academy of Sciences in which he tried to prove a putative genealogical relationship between the Basque and Berber languages.²¹ As von der Gabelentz (1893: 593-594) himself noted, the hypothesis that the Basques of southern Europe, whose language could not be aligned with any known family, were in some way related to the "Hamites" of North Africa was not a novel idea.

¹⁹Original: "Folgenden Einwand könnte man erheben: Die meisten Gewährsmänner waren nicht zu wissenschaftlicher Lautbeobachtung geschult; sie beurtheilten die fremden Laute nach denen ihrer Muttersprache, und Zwischenstufen zwischen diesen schienen ihnen bald nach der einen, bald nach der anderen Seite zu neigen. Darauf ist zu entgegnen, dass mindestens ein Theil jener Männer lange genug unter den Eingeborenen gelebt, um ihr Ohr an die fremde Sprache so zu gewöhnen, wie es vordem an die Muttersprache gewöhnt gewesen. Dieser, oder richtiger ihrer mehrsprachigen Schulung, verdankten sie eben das feinere Gehör, das sie jene unsicheren, schwankenden Articulationen empfinden liess."

²⁰von der Gabelentz (1894b) also later proposed using a statistical approach for the typological study of languages. McElvenny (2018) offers an English translation of this text.

²¹For a comprehensive account of this episode, including Gabelentz' initial address, the subsequent book-length publication (von der Gabelentz 1894a), and the reaction of Gabelentz' colleagues, see Hurch & Purgay (In Press).

That no linguistic proof of this relationship had yet been given, he contended, was due to the inflexibility of the comparative method as it was practised at the time. The comparative method needed to be ramified to accommodate the radical mutability of linguistic form that had been discovered in regions beyond the familiar Indo-European context, as in Indo-Chinese and Melanesian sources:

The belief in the constancy of the outer and inner linguistic form is among the achievements to which our science clings most tenaciously, and the facts that could shake this belief are for their part newly acquired and poorly known, since they are in the territory of Indo-Chinese and Melanesian. (von der Gabelentz 1893: 594)²²

Looking across Basque dialects, von der Gabelentz (1893) postulated extremely irregular sound correspondences between apparently cognate words, leading him to the conclusion that “they offer a picture of phonetic wildness which, as far as I know, must be one of a kind in the world of languages” (*sie geben ein Bild lautlicher Verwilderung, das meines Wissens in der Sprachenwelt kaum Seinesgleichen hat*; von der Gabelentz 1893: 596). He found a similar situation in the Berber languages. On this basis, von der Gabelentz (1893: 604) assumed the existence of a “prehistoric period of the most uncertain articulation” (*vorgeschichtlichen Periode der unsichersten Articulation*) in these languages, “where the phonetic images appeared before the soul only in vague outlines, as if they were drawn with a mop or paint-roller” (*wo die Lautbilder der Seele nur in vagen Umrissen vorgeschwebt haben, als wären sie mit dem Wischer gezeichnet oder mit dem Vertreiberpinsel gemalt*). To bring order into this chaos, Gabelentz employed his statistical method, tabulating the frequencies of putative correspondences across the Basque dialects, the Berber languages and between these two groups.

The extraordinarily large latitude in pronunciation of the kind attributed to Basque and Berber is, von der Gabelentz (1893: 606) argued, characteristic of languages “at a lower level of culture” (*auf niederer Culturstufe*). At this cultural level, articulated forms are only rejected when they cannot be understood. This lack of constraint on variation leaves linguistic forms subject to the temperamental and corporeal contingencies of the moment, as in Steinthal’s conception of the first stage of language evolution. Distant analogues of such cases can even be observed in Indo-European languages, claimed Gabelentz, offering the example

²²Original: “Der Glaube an die Beständigkeit der äusseren und inneren Sprachform gehört zu den Errungenschaften, an denen unsere Wissenschaft am zähesten festhält, und die Thatsachen, die ihn erschüttern könnten, sind ihrerseits neuer Erwerb und wenig bekannt, da sie auf indochinesischem und melanesischem Gebiete liegen.”

of an uneducated Saxon from Germany (the same example with a more moderate moral occurs also in [von der Gabelentz 2016 \[1891\]: 398](#)):

In this way a strange thing can happen, that a very indefinite sound image appears before the soul, and yet the mouth produces a very clearly articulated sound, although not always the same one, but rather at one moment this one and then at another that one, depending on chance and mood. [...] I can offer an example of an at least distant analogue of this from our own languages. The Saxon, who does not distinguish between *d* and *t*, between *i* and *ü*, *e* and *ö*, *ei* and *eu*, *äu*, can in the heat of the moment pronounce the *d* as *t* and – when he is talking about deep, dark, terrible things – turn all *i*, *e* and *ei* into *ü*, *ö*, *eu* in a kind of onomatopoeia. ([von der Gabelentz 1893: 606-607](#))²³

Needless to say, Gabelentz' attempted reform of the comparative method did not gain a foothold. The exceptionless dismissal of Gabelentz' approach may not, however, have been so much due to his underlying premises as to his cavalier treatment of his sources. Even among those who could be expected to sympathize with Gabelentz' proposal, the criticism was widespread that he had not properly curated or analysed the Basque and Berber data, which led him to obvious errors in presentation and interpretation (cf. [Hurch & Purgay In Press](#)). [Brinton \(1894\)](#), for one, in his brief review of the 1894a expanded book version of Gabelentz' Basque and Berber studies, did not criticize Gabelentz' underlying views on variation, but did note that he had not properly distinguished between cognates and loan words in his analyses.

Hugo Schuchardt (1842–1927) – one of the most prominent contemporary opponents of the Neogrammarians, as he acknowledged himself (see, e.g., [Spitzer 1928 \[1922\]](#)) – was similarly unimpressed by Gabelentz' methodological laxness, despite being sympathetic to the motivating idea of the radical mutability of linguistic forms. In a review of [von der Gabelentz \(1894a\)](#), he questioned the wisdom of taking such an adventurous course in comparing these languages when the more conventional and uncontroversial methods had yet to be tried properly:

²³Original: "So kann das Seltsame geschehen, dass der Seele ein sehr unbestimmtes Lautbild vorschwebt, und doch der Mund ein sehr scharfes hervorbringt, aber nicht immer dasselbe, sondern bald dieses bald jenes, je nach Zufall und Stimmung. [...] Aus unserem Sprachkreise wüsste ich wenigstens entfernt Analoges anzuführen. Dem Obersachsen, der zwischen *d* und *t*, zwischen *i* und *ü*, *e* und *ö*, *ei* und *eu*, *äu* nicht unterscheidet, kann es geschehen, dass er im Affecte jedes *d* wie *t* ausspricht, und dass er, wo es sich um tiefe, dunkle, grausige Dinge handelt, alle *i*, *e* und *ei* lautmalend in *ü*, *ö*, *eu* verwandelt."

The Kabyle and Tuareg words that the author [Gabelentz] compares to the Basque words differ from these greatly for the most part. He does indeed attempt to explain this on the basis of muddled and washed out phonetic confusion. However, even if I do not dispute this possibility in general, it still seems to me that we should for the time being – that is, as long as further and more careful examinations of Basque phonetic history are not available – not seek refuge in this “last resort”. (Schuchardt 1893: 334)²⁴

Gabelentz’ freewheeling approach, commented Schuchardt (1893: 334), offers no credible way to navigate language history. It could just as easily be used to link Basque to the languages of the Caucasus or the Ural as to those of North Africa. Although there were linguists dissatisfied with the rigid system-building of the Neogrammarians and prepared to face the messiness of the raw data, Gabelentz’ scheme did not present a viable alternative for them.

5 Conclusion

In the last decades of the nineteenth century, the phenomenon of alternating sounds was instrumentalized in different ways by scholars hoping to advance their various academic and disciplinary agendas. In America, Boas denied the reality of the phenomenon as part of a project to assert the scientific superiority of the anthropological school he was busily building up. In Germany, Gabelentz moved in the opposite direction, embracing the phenomenon as way of undermining the hegemony of Neogrammarian linguistics. The positions of both Boas and Gabelentz – and indeed also their rivals – were informed in no small way by the mid-nineteenth-century writings of Steinthal, who developed a unified theory of the psychological basis and evolution of language with a strongly empirical accent.

Although both Boas and Gabelentz indulge in exaggeration and caricature in their critiques, and exhibit obvious faults in elaborating their own positions, their views have had very different fates in the received histories of linguistics and anthropology. External factors no doubt play a role here: Boas achieved institutional dominance and is feted as the founding father of modern American an-

²⁴Original: “Die kabyliſchen und tuaregiſchen Wörter, die der Verf. zu baſkiſchen Wörtern ſtellt, weichen von dieſen zum groſſen Theil ſehr ſtark ab. Zwar ſucht er das aus einer verworrenen und verwaſchenen Lautirung zu erklären: aber wenn ich auch im Allgemeinen die Möglichkeit einer ſolchen nicht beſtreite, ſo dünkt mich doch, wir ſollten vorderhand, d. h. ſo lange nicht mehr und ſorgfältigere Unterſuchungen über die baſkiſche Lautgeſchichte vorhanden ſind, hier nicht zu dieſer “ultima ratio” unſere Zuflucht nehmen.”

thropology, while Gabelentz died early and disappeared into relative historical obscurity.

The different fates of their views on alternating sounds are perhaps also indicative of the temperament of linguistics and anthropology as disciplines. Despite his apparent hostility to later conceptions of the phoneme, Boas' attack on the notion of alternating sounds is celebrated for expanding the formal franchise, making all languages equal subjects under the laws of linguistics. Gabelentz' efforts to problematize the comparative method, by contrast, could find no supporters: his dismembering of current historical linguistics offered no practical alternative. Boas is more welcome than Gabelentz in fields that place a premium on technical progress, conceived positivistically as the ability to capture and catalogue phenomena within a universalizing system. This case study offers informative parallels to the "resistant embrace" of structuralism in France that John Joseph (Chapter 7, this volume) sketches and the "unique form hypothesis" that Nick Riemer (Chapter 10, this volume) imputes to present-day linguistics.

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Chapter 2

Linguistics as a “special science”. A comparison of Sapir and Fodor

Els Elffers

Independently of each other, the linguist-anthropologist Edward Sapir (1884–1939) and the philosopher of mind Jerry Fodor (1935–2017) developed a similar typology of scientific disciplines. “Basic” (Fodor) or “conceptual” (Sapir) sciences (e.g. physics) are distinguished from “special” (Fodor) or “historical” (Sapir) sciences (e.g. linguistics). Ontologically, the latter sciences are reducible to the former, but they keep their autonomy as intellectual enterprises, because their “natural kinds” are unlike those of the basic sciences. Fodor labelled this view “token physicalism”. Although Sapir’s and Fodor’s ideas were presented in very different periods of intellectual history (in 1917 and 1974) and in very different intellectual contexts (roughly: *Geisteswissenschaften* and logical positivism), the similarity between them is striking. When compared in detail, some substantial differences can also be observed, which are mainly related to contextual differences. When applied to linguistics, Sapir’s and Fodor’s views offer a perspective of autonomy, albeit it in different ways: for Fodor, but not for Sapir, linguistics is a subfield of psychology.

1 Introduction

In 1974, Jerry Fodor (1935–2017) introduced “token physicalism”, a non-reductive variety of physicalism, which applies to “special sciences”.¹ According to Fodor, special sciences, such as economics, psychology and linguistics cannot be entirely reduced to physics, which is a *basic science*. Such a reduction would imply that special sciences actually disappear as autonomous sciences.

According to Fodor, special sciences retain their autonomy, because reduction is possible only with respect to the events they describe (“tokens”), not with

¹Token physicalism belongs to a larger class of non-reductive types of physicalism. Supervenience physicalism and emergentism are other members. John Stuart Mill (1806–1873) is generally regarded as an early representative of non-reductive physicalism.



respect to properties or natural kinds (“types”). For example, economic events such as monetary exchanges are, ultimately, physical events, but, from a physical point of view, very heterogeneous ones. There is no single physical natural kind corresponding to the economic natural kind “monetary exchange”, because such exchanges may involve “strings of wampum, [...] dollar bills, [...] signing one’s name to a check” (Fodor 1974: 103). Physics, according to Fodor the only “basic science”, develops taxonomies of physical phenomena in terms of physical properties. Special sciences develop their own taxonomies of, ultimately, physical phenomena as well, but in other terms, not belonging to the vocabulary of physics.

In this article, I will compare Fodor’s token physicalism with ideas of Edward Sapir (1889–1939), presented in an article published in 1917. I will argue that Sapir’s ideas are highly similar to Fodor’s. Despite differences, Sapir’s conceptual sciences and historical sciences resemble Fodor’s basic and special sciences to such a degree that, in this respect, Sapir can be regarded as Fodor’s predecessor.

Historians, including historians of linguistics, apply the concept “predecessorship” in different and partially unfounded ways. In section 2, I will briefly discuss this problem and present my own view of predecessorship, including its implications for the concept “predecessorship of token physicalism”.

In the sections that follow, I will argue that this concept applies to Sapir. Section 3 will discuss Sapir’s distinction of conceptual and historical sciences in detail. In section 4, Fodor’s token physicalism is further analyzed. Together, these sections present a picture of similar theories, developed in different periods, intellectual contexts, and with different motivations. Sections 5–7 presents a systematic comparison of both theories. In sections 8–10, both theories will be discussed in a broader context, both chronologically and intellectually.

The views of both Sapir and Fodor were presented without any special focus on linguistics. In linguistic circles, their views are not well known. In section 12, I will explore the linguistic implications of Sapir’s and Fodor’s varieties of token physicalism.

2 Pitfalls of predecessorship

“Predecessorship” belongs, together with some other concepts (e.g. “influence” or “source”), to the more dangerous instruments of the historian’s toolbox. They are applied in multifarious and sometimes confusing ways. Present-day dangers of “predecessorship” can be partially attributed to the belated influences of older

approaches to intellectual historiography:

1. The exegetical “history of ideas” approach, with its focus on isolated and quasi-immutable “ideas” or “themes”, and their march through history.
2. The historicist approach (in one of many meanings of this term)² of interpreting chronological sequences of events in causal, teleological or developmental terms.

Unwarranted claims of predecessorship are corollaries of (1) and (2).³ I mention an example of both, to be found in historiography of linguistics:

- Ad 1. In [Antal \(1984\)](#), the entire history of linguistics is interpreted in terms of an alternation of two themes: “psychologism” and “objectivism”. Hermann Paul (1846–1921) is thus presented as a “psychologistic” predecessor of Noam Chomsky (b. 1928). The term “psychologism” applied to approaches as far apart as those of Paul and Chomsky, is, however, almost meaningless, and so is the predecessorship conclusion based upon it.
- Ad 2. In Chomsky’s ([2009 \[1966\]](#)) well-known *Cartesian Linguistics*, the seventeenth-century Port-Royal Grammar is presented as a – still imperfect – predecessor of twentieth-century generative grammar. This claim has been amply criticized as being based upon an incorrect and biased interpretation of seventeenth-century grammar, and as a specimen of presentism, Whig history and an ancestor hunt. All these defects are rooted in the historicist idea of chronology as a series of developmental steps towards the present.

Although pitfalls (1) and (2) are well known today, the danger of unwarranted claims of predecessorship still exist. It is natural for historians to compare phenomena over time. Discovering similarities easily creates “the temptation to discern and extract pervasive themes or patterns running through and manifested in the succession of events and activities” ([Robins 1997 \[1967\]](#): 7-8). Moreover,

²The meaning of “historicism” applied here is related to the meaning of other “-ism” terms such as “psychologism” or “scientism”; these terms claim to reveal “where” the essence of things has to be looked for. This meaning of “historicism” has to be distinguished from, e.g., Popper’s use of the term (cf. [Elffers 1991](#): 43).

³Cf. [Elffers \(1991: ch. 2 & 3\)](#) for a more thorough and comprehensive discussion of these influences in present-day intellectual historiography, and for more details of the alternative approach, briefly indicated on p. 2 as reconstruction of earlier scientific ideas “as problem solutions in the context of the contemporary intellectual state-of-the-art”.

historicism (conceived in the above manner) is still influential in the way it permeates our common historical vocabulary, which “presents history as a ‘stream’ which proceeds irresistibly [...]. Metaphors talking of ‘progress’ [...] constitute examples: ‘avant-garde’ art, advanced technique [...], locutions like ‘keeping pace with’ [...] or ‘being in advance of’ one’s time as well as clock-metaphors, such as ‘turning back’ or ‘stopping’ the clock [...]” (van der Dussen 1986: 131, transl. E. E.)⁴

If the above pitfalls are avoided, the establishment of predecessorship relations in intellectual history may become a more complicated task, but it continues to be interesting and rewarding; indeed, even more so, because we are now disregarding superficial historical similarities as well as irrelevant later developments. Instead, we are thoroughly analysing and comparing the actual contents of scientific ideas, which are carefully reconstructed as problem-solutions within the context of the contemporary intellectual state of the art.

Following this approach, I assume that a predecessor of token physicalism was similar to Fodor with respect to the questions that Fodor answered by postulating token physicalism, and to the answers themselves.

Questions – predecessors of token physicalism are involved in:

- a. ontological questions concerning basic categories of entities,
- b. epistemological questions about the basic categories of separate disciplines.

Answers: Predecessors of token physicalists present theories which take into account questions (a) and (b) and assume that for one or more “basic” disciplines, the categories of (a)-answers and (b)-answers are identical. For other, “special”, disciplines, the categories of (a)-answers and (b)-answers are non-identical.⁵

Predecessorship thus conceived is typically unconstrained by terminology. Terminological identity may conceal fundamental differences in content, and vice versa. Consequently, predecessors of token physicalism may apply quite different terms from those used in the above preliminary assumption. The only requirement is that the content of their statements can be interpreted in terms of this

⁴“...waarbij de geschiedenis wordt voorgesteld als een ‘stroom’, die onweerstaanbaar [...] voortgaat. Metaforen waarin over een ‘vooruitgang’ wordt gesproken [...] zijn hier voorbeelden van: ‘avant-garde’ kunst, geavanceerde techniek [...] het spreken van een ‘meegaan’ met de tijd [...] of zijn tijd ‘voorui’ zijn, evenals klok-metaforen, zoals de klok ‘terugdraaien’ of ‘stilzetten’ [...]”

⁵Against this background, I regard Seuren’s (2016: 827-832) claim that a scholar much earlier than Sapir, Hypolyte Taine (1828–1893), anticipated Fodor’s token physicalism as unconvincing. Seuren presents quotations to support his view, but none of them suggests a distinction comparable to the distinction between (a) and (b).

assumption. This also applies to Fodor himself: Fodor (1974) does not use the term “ontological” at all, and he sometimes gives the term “epistemological” a rather specific meaning.⁶ But the content of his statements meets the requirement of being interpretable in the above terms, as I hope to show below.

3 Sapir: against a “superorganic”

Sapir’s 1917 article is titled “Do we need a ‘Superorganic’?” It is a reaction to the anthropologist Alfred Kroeber’s (1876–1960) article “The Superorganic” (Kroeber 1917). Both articles were published in subsequent issues of *American Anthropologist*, an anthropological journal that is still quite prominent in the field.⁷

Both Kroeber and Sapir were students of Franz Boas (1858–1942), the “founding father” of American anthropology. Kroeber, who became an influential American anthropologist, argues in his 1917 article against the reduction of anthropology to biology. He states that human cultural behaviour, unlike animal behaviour, cannot be explained through an appeal to inheritance plus Darwinian adaptation, nor to personal psychology. The forces of culture, a superorganic and autonomously developing entity, are the main determinants. For anthropology, this superorganic is the actual object of research.⁸

When Sapir wrote his critical article, he was working as a director of the Anthropological Division of the Geological Survey of Canada in Ottawa. This was a very productive period in his career. Anthropological linguistics, which included the investigation and description of American Indian languages never studied by academics before was his main area of research. He exchanged correspondence with Kroeber over a period of many years.

In “Do we need a ‘Superorganic’?”, Sapir begins by welcoming Kroeber’s “salutary antidote” to the trend of applying methods used by the exact sciences to the study of culture. But he also feels that Kroeber “has allowed himself to go further than he is warranted in going” on “two points of considerable theoretical importance” (Sapir 1917: 441). Although only the second point directly concerns our subject, I will also briefly discuss the first one, because there is, according to Sapir, a connection between them.

⁶“Epistemological” as used in Fodor (1974: 113) refers to the “context of discovery”. This deviates from the usual reference, which is primarily to the “context of justification”.

⁷Sapir’s article appeared in the section “Discussion and correspondence”. Another comment to Kroeber’s article by A. A. Goldenweiser (1917) was included in the same section.

⁸Herbert Spencer (1820–1903) coined the term “super-organic” to focus on social organization, in the first chapter of his 1898 *Principles of Sociology*, entitled “Super-organic Evolution”.

The first point concerns Kroeber's denial of any influence by individuals on the course of cultural history. Sapir admits that the influence of individuals is mostly highly exaggerated by historians. He fully recognizes that individual thought and action are very much moulded by cultural traditions, and that the cultural influence of most individuals is nil. If it is not nil, broader cultural conditions are necessary to trigger this influence. But this does not obviate the influence of at least some individuals – such as Napoleon, Jesus, Shakespeare or Beethoven – on cultural history, according to Sapir. A total social determinism goes too far.

The second point concerns the nature of social phenomena. Kroeber claims that they are built out of organic phenomena but are not reducible to organic phenomena, just as organic phenomena are built out of inorganic phenomena but are not reducible to them. A superorganic social “force” is assumed, which is manifested in social history.

Sapir regards the above analogy as false. The types of irreducibility are entirely dissimilar. Sapir's ontology is triadistic. He assumes three basic types of entities: inorganic, organic and psychic. Social phenomena are not a fourth type, as Kroeber feels they are, but “merely a certain philosophically arbitrary but humanly immensely significant *selection* out of the total mass of phenomena ideally resolvable into inorganic, organic and psychic processes” (Sapir 1917: 444, italics Sapir). Social phenomena are, therefore, not at all conceptually irresolvable but experientially irresolvable. Conceptual irresolvability is what separates inorganic, organic and psychic phenomena; these are, in Sapir's terms “true conceptual incommensurables” (Sapir 1917: 445). Experiential irresolvability is entirely different: it refers to classes of directly experienced phenomena, demarcated not in terms of ontology, but in terms of values that determine their selection. These classes are studied in historical sciences. Conceptually demarcated classes are studied in conceptual sciences.

Sapir illustrates his distinction between types of science using the example of geology:

Few sciences are so clearly defined as regards scope as geology. It would ordinarily be classed as a natural science. Aside from paleontology, which we may eliminate, it does entirely without the concepts of the social, psychic or organic. It is, then, a well-defined science of purely inorganic subject matter. As such, it is conceptually resolvable, if we carry our reductions far enough, into the more fundamental sciences of physics and chemistry. But no amount of conceptual synthesis of the phenomena we call chemical or physical would, in the absence of previous experience, enable us to construct a science of geology. The science depends for its *raison d'être* on a

series of unique experiences, directly sensed or inferred, clustering about an entity, the earth, which from the conceptual standpoint of physics is as absurdly accidental or irrelevant as a tribe of Indians or John Smith’s breakfast. The basis of the science is, then, grounded in the unique relevance of particular events. To be precise, geology looks in two directions. In so far as it occupies itself with abstract masses and forces, it is a conceptual science, for which specific instances as such are irrelevant. In so far as it deals with particular features of the earth’s surface, say a particular mountain chain, and aims to reconstruct the probable history of such features, it is not a conceptual science at all. In methodology, strange as this may seem at first blush, it is actually nearer, in this respect, to the historical sciences. It is, in fact, a species of history, only the history moves entirely in the inorganic sphere. In practice, it is, of course, a mixed type of science, now primarily conceptual, now primarily descriptive of a selective chunk of reality. (Sapir 1917: 445)⁹

As examples of “chunks of reality” studied by historical sciences, Sapir also mentions, next to the earth, “France, the French language, the French Republic, the romantic movement in literature, Victor Hugo, the Iroquois Indians, some specific Iroquois clan, all Iroquois clans, all American Indian clans, all clans of primitive peoples.” Sapir (1917: 446) stresses that none of these terms has any relevance in a purely conceptual world, whether organic, inorganic or psychic.

These examples are not selected arbitrarily. Sapir wants to show (i) that historical sciences apply to “history” in a much wider sense than what it ordinarily indicates, (ii) that historical sciences not only study directly experienced entities, but also more abstract entities.

Sapir elaborates on (ii) in order to explain two further differences between types of science: “such concepts as a clan, a language, a priesthood” might suggest a similarity with “the ideal concepts of natural science”, which also “lack individual connotation” and appear in generalized laws. Logically, both sets of concepts are involved in similar operations such as observation, classification, inference, generalization etc. “Philosophically”, however, the concepts are distinct, because, in actual fact, the social concepts are not “ideal” at all; they are “convenient summaries of a strictly limited range of phenomena, each element of which has real value”:

⁹In this quotation, physics and chemistry are both mentioned as fundamental sciences of the inorganic. In 1917, reducibility of chemistry to physics was not at all as generally accepted as it is today (cf. Hetttema 2012: 13 & 17-18).

Relatively to the concept “clan” a particular clan of a specific Indian tribe has undeniably value as a historical entity. Relatively to the concept “crystal” a particular ruby in the jeweler’s shop has no relevance except by way of illustration. It has no intrinsic scientific value. Were all crystals existent at this moment suddenly disintegrated, the science of crystallography would still be valid, provided the physical and chemical forces that make possible the growth of another crop of crystals remain in the world. Were all clans now existent annihilated, it is highly debatable, to say the least, whether the science of sociology, in so far as it occupied itself with clans, would have prognostic value. (Sapir 1917: 446-447)

A corollary of this difference is the different status of laws in both types of science. A sociological law is a generalization, an abbreviation for a finite number of phenomena. Exceptions occur, and the laws become “more and more blurred in outline with the multiplication of instances”, whereas this multiplication makes natural laws “more and more rigid” (Sapir 1917: 447). Natural laws cover an indefinitely large number of phenomena and have to be exceptionless: an exception necessitates a new formulation of the law.

Sapir concludes his article by connecting his two criticisms of Kroeber: if the nature of historical phenomena had been sufficiently clear to him, he would have felt no need to invoke a “superorganic” force as unique explanans in history, and to deny individual force.

4 Fodor: against reductive physicalism

Fodor’s article is titled “Special sciences (or: the disunity of science as a working hypothesis)”. It was published in 1974 in *Synthèse*, a well-known philosophical journal that is still published. It takes as its starting point the “typical thesis of positivistic philosophy of science [...] that all true theories in the special sciences should reduce to physical theories in the long run” (Fodor 1974: 97). This thesis, and its foundation in a materialist ontology, were the cornerstones of the Unity of Science movement, to which Fodor’s title alludes. This movement was narrowly related to logical positivism during the first decades of the twentieth century. Since those days, questions about the unity of science and about reductivism have never disappeared from the philosophical agenda.

When Fodor wrote “Special sciences”, he was a professor in the departments of philosophy and psychology at the Massachusetts Institute of Technology. Philosophy of mind and language was his central subject of research. He had already

published widely on many themes related to this area. In 1975, his seminal book *The language of thought* would appear. In “Special sciences”, psychology is by far the science that receives the most attention.

Fodor addresses a problem that results from the positivistic assumption that the subject matter of a special (i.e. non-physical) science, such as psychology, is part of the subject matter of physics. A generally accepted inference from this assumption is that psychological theories must reduce to physical theories. This causes methodological problems for psychology; the discipline should actually disappear as a separate science. Fodor (1974: 98) wants to “avoid the trouble by challenging the inference”.

Assuming that sciences are about events, Fodor claims, in agreement with the physicalists, that “all events that the sciences talk about are physical events [...]” (Fodor 1974: 100). He calls this doctrine “token physicalism”. But he rejects the stronger reductionist doctrine of “type physicalism”, which claims that, in addition, every property mentioned in the laws of any science is a physical property. Token physicalism claims that, for example, every psychological event is identical to a neurological event, but not every psychological property is identical to a neurological property.

The reason why type physicalism is too strong a thesis is that interesting generalizations in special sciences are often about events whose physical descriptions have nothing in common. Moreover, the question “whether the physical descriptions have anything in common is, in an obvious sense, entirely irrelevant to the truth of the generalizations, or to their interestingness, or to their degree of confirmation, or, indeed, to any of their epistemologically important properties [...]” (Fodor 1974: 103). As an example of such a generalization, Fodor refers to Graham’s law, an economic law about monetary exchanges. In the above *Introduction*, this example was already mentioned to illustrate the wildly different physical events which correspond to the concept of “monetary exchange” (transactions with bills, cheques etc.). These events do not correspond to a natural kind in physics. Similarly, although psychological events correspond to neurological events, “there are no firm data for any but the grossest correspondence between types of psychological states and types of neurological states, and it is entirely possible that the nervous system of higher organisms characteristically achieves a given psychological end by a wide variety of neurological states” (Fodor 1974: 105).¹⁰

¹⁰Fodor refers to the physiological psychologist Karl Lashley as a defender of this claim. He also acknowledges that there is much “psychology and brain” research throughout the world, which is based upon the assumption that psychological types correspond to neurological types

Fodor further supports his token physicalistic view by arguing that his view explains (i) that laws of special sciences have exceptions, (ii) why there are special sciences at all.

- Ad i. Given the assumption that, in a special science law, physical counterparts of the antecedent as well as the consequent consist of heterogeneous disjunctions, the counterpart “law” cannot be a genuine physical law.¹¹ Exceptions occur when the physical counterpart of an instantiation of the antecedent of a special science law has no lawlike connection with one of the disjunctive physical counterparts of the consequent. According to Fodor, this is a common situation in a special science such as psychology: there are always exceptions to psychological generalizations which are “uninteresting from the point of view of psychological theory” (Fodor 1974: 111).
- Ad ii. According to reductionists, special sciences exist for practical, “epistemological” (cf. note 6) reasons. If neurons were not so small and brains were on the outside of the head, we would do neurology instead of psychology. Fodor does not agree: even if brains were on the outside, we would not know what to look for, lacking “the appropriate theoretical apparatus for the psychological taxonomy of neurological events”. Moreover, he assumes that such a corresponding taxonomy does not necessarily exist, that “quite different neurological structures can subserve identical psychological functions [...] In that case the existence of psychology depends not on the fact that neurons are so sadly small, but rather on the fact that neurology does not posit the natural kinds that psychology requires” (Fodor 1974: 113).

Special sciences exist autonomously, because other taxonomies are required alongside the taxonomy which suits the purpose of formulating exceptionless basic physical laws. The other taxonomies are necessary for the formulation of important generalizations in areas of knowledge such as psychology or economics.

5 Similarities and differences

The last two sections show two scholars struggling for a plausible philosophical reconstruction of science in general and its division into separate disciplines in

(Fodor 1974: 105).

¹¹This is a very brief and simplified presentation of a complex argument, presented in Fodor (1974: 109).

particular. Independently from each other and separated by nearly six decades, they devised a nearly identical theory.¹² According to this theory, boundaries between disciplines are not merely determined by the kind of stuff they investigate. Although some (“conceptual” or “basic”) sciences can be demarcated along these lines, other (“historical” or “special”) sciences are demarcated in a different way. Their object of investigation consists of heterogeneous stuff, but is homogeneous by its relevance to the purposes of the area of knowledge to which they belong.

For Sapir, the theory was a welcome alternative to Kroeber’s ontological way of rescuing the autonomy of sociology and anthropology through the assumption of a superorganic force. For Fodor, the theory was a welcome alternative to reductive physicalism, with its problematic methodological requirements, especially for psychology.

Due to these different backgrounds, the theories have a different “appearance”. In Fodor’s discourse, subtle logical properties of scientific theories are taken into account, as was (and is) usual in positivistic-oriented philosophy of science. In Sapir’s and Kroeber’s discourse, this approach is entirely absent, also in conformity with what was (and is) usual in philosophy of non-exact sciences.

In the following sections, Sapir’s and Fodor’s theories will be compared in more detail. Their common basic idea is elaborated in partially different ways by both scholars. Part of these differences can be shown to be related to the intellectual context in which the theories were developed.

In the rest of this article, I will use Fodor’s term “token physicalism” to refer to the common view of Sapir and Fodor.¹³ In the same vein, I will adopt Fodor’s terms “basic science” and “special science” for the similar types of sciences distinguished by both scholars.

My comparison is almost entirely based upon the articles just discussed. Neither Sapir nor Fodor elaborated their theory further in later publications. Fodor, however, returned to the subject in his article “Special sciences: still autonomous after all these years”, published in 1997. This article consists of a defence of his view against the criticism of (Kim 1992). In the course of this defence, some aspects of token physicalism are presented in more detail than before. An addition, which is relevant to our comparison with Sapir, is that special sciences are now explicitly described in functionalistic terms. Their physically heterogeneous natural kinds are functionally homogeneous, in the same way as physically het-

¹²Of course, Fodor *could* have read Sapir’s article, but I regard this as improbable. As far as I know, Fodor never refers to Sapir. Moreover, Sapir’s intellectual activities and viewpoints were unrelated to Fodor’s area of interest, or even repugnant to him (cf. Pullum 2017).

¹³The literal meaning of the term has to be bracketed in Sapir’s case, because of his trialistic ontology.

erogeneous types of artefacts (can openers, mousetraps) are functionally homogeneous Fodor (1997: 160). This characterization was lacking in the 1974 article, although “psychological functions” are mentioned. The term “functional” must be interpreted in a very broad sense, because it is equally applied to biology, psychology and geology. The last mentioned example of a special science is a new one, and identical to Sapir’s example. Like Sapir, Fodor (1997: 160) claims that mountains are made “of all sorts of stuff”, but that “generalizations about mountains-as-such [...] serve geology in good stead”.

Taking into account the 1997 additions to Fodor’s theory, the views of Sapir and Fodor, as presented in section 3 and 4 can be schematically juxtaposed in the following way:

	Basic sciences		Special sciences	
	<i>Sapir</i>	<i>Fodor</i>	<i>Sapir</i>	<i>Fodor</i>
<i>Sciences</i>	Physics, Chemistry, Geology, Biology, Psychology	Physics	Sociology, Anthropology, Linguistics, Geology, (Cultural) History	Psychology, Linguistics, Biology, Geology
<i>Demarcation</i>	Ontological	Ontological	Experiential	Functional
<i>Exceptions of laws?</i>	No	No	Yes	Yes

The table shows that Sapir’s and Fodor’s varieties of token physicalism are different at two points: (i) their selection of basic and special sciences, (ii) their characterization of special sciences. As to (i), we may ask how far the differences can be related to contemporary ontological assumptions. As to (ii), we may ask how far apart the standpoints actually are, given the similarity of both scholars’ general view of the special sciences. Likewise, we may ask how far their agreement about the issue of exceptions to laws actually goes, given the different motivations of these ideas, observed earlier. I will discuss these three issues in separate sections.

6 Which basic and special sciences?

Fodor recognizes one basic science, physics, which is in conformity with the positivistic discourse he connects with. In the same vein, he also mentions chemistry as a science that has been successfully reduced to physics.

His most important example of a special science is psychology. The anti-reductionist

defence of the autonomy of this science is his central aim, and directly relevant to his work as a cognitive psychologist. In his seminal book *The language of thought* (1975), the text of “Special sciences” is included in the introductory chapter, which presents the foundations of the psychological and linguistic approach described and applied in the rest of the book.¹⁴

Linguistics is not explicitly discussed in the 1974 article.¹⁵ However, Fodor has always incorporated linguistics in psychology, following Chomsky’s views and elaborating this connectedness in more detail than Chomsky did (cf., e.g., Fodor (1985: 149), quoted in footnote 26, and Rey (1991: 278)). The language of thought bears clear witness to this approach. So there can be no doubt that, for Fodor, linguistics is a special science. Other special sciences, such as economics and geology, are dealt with as instructive examples.

Sapir distinguishes three irreducible ontological categories: inorganic, organic and psychic. Inorganic sciences are physics, chemistry, and, partially, geology; psychology is the basic science of the psychic. Sapir does not mention examples of organic sciences, but we may assume that biology is the main, or even only, example of this category.

Sapir does not present arguments in favour of his trialistic ontology. He simply claims that “the organic can be demonstrated to consist objectively of the inorganic plus an increment of obscure origin and nature”. There is “a chasm between the organic and the inorganic which only the rigid mechanists pretend to be able to bridge. There seems to be a unbridgeable chasm [...] between the organic and the psychic, despite the undeniable correlations between the two. Dr. Kroeber denies this *en passant* [...]” (Sapir 1917: 444).

These quotations show that Sapir is aware of the existence of divergent ontological ideas, but he does not feel obliged to supply arguments for his own view. This is not surprising when we take contemporary ontological thought into account. Vitalism, the idea that organic nature is created from chemical elements plus the action of a “vital force” had been waning over several decades, but was not at all extinct (cf. Beckner 1967). Psychology was, despite some reductionistic attempts, still largely regarded as studying purely mental entities. This applies, for example, to Gestalt psychology, an approach Sapir found appealing (cf. Sapir 1994: xvi).

An example of a special science is, for Sapir, in the first place, social science,

¹⁴There are some minor differences between the article and the book section. The book section contains more notes and is extended by some final paragraphs.

¹⁵There is, however a note reference to Chomsky’s (Chomsky1965) statements about natural language predicates, to support Fodor’s claim that natural kind predicates of the special science cross-classify the physical natural kinds.

including anthropology, the common discipline of Kroeber and himself. Other examples are history – cf. Sapir’s term “historical sciences” – and, partially, geology. Given the above examples of “chunks of reality” studied by historical sciences, we can add linguistics (cf. “the French language”) and literary history (cf. “the romantic movement in literature”).

In summary, Sapir’s and Fodor’s examples as well as ideas about the position of separate disciplines in their dichotomy are partially different. This is mainly due to their different basic ontologies and their implications, especially for psychology. A remarkable conclusion about linguistics is that its status of special science has a different meaning for Sapir and Fodor. For Sapir, a language is an ontologically heterogeneous entity. So linguistics is not reducible to psychology, nor to any other basic science. Fodor includes linguistics in psychology, but for him, psychology is itself a special science, due to ontological irreducibility. In section 12 I will return to this issue.

7 Characterizing special sciences

The categories/types/natural kinds of special sciences are ontologically heterogeneous, but *experientially* (Sapir) or *functionally* (Fodor) homogeneous. At first sight, these characterizations are dissimilar. Experiences are direct and unique, functions are conceptualized regularities. Therefore, when both scholars conclude that a certain discipline belongs to the special sciences, their reasons for the classification appear to be different. On the other hand, their common focus on areas consisting of human institutions (clans, economics) or “interesting” phenomena (mountains) suggests that they may share the same basic insight, but reconstruct it in different terms.

The shared example of geology may serve to clarify this point. For Sapir, geology is a special science, because it “depends, for its *raison d’être*, on a series of unique experiences, directly sensed or inferred, clustering about an entity (the earth, a mountain chain)” (Sapir 1917: 445). For Fodor, it is essential that mountains, however ontologically heterogeneous, enter into generalizations that “serve geology in good stead. [...] unimaginably complicated to-ings and fro-ings of bits and pieces at the extreme *micro-level* manage somehow to converge on stable *macro-level* properties” (Fodor 1997: 160). On the next page, these macro-level properties are equated with functional properties, as in psychology and biology.

My hypothesis is that these different characterizations are connected to the different discourses in which both scholars are operating. Sapir conceives of “historical sciences” as comparable to *Geisteswissenschaften*, referring to Rickert

(1913 [1896]). This class of sciences is often characterized as “ideographic”, and is contrasted with the “nomothetic” *Naturwissenschaften*. Hence Sapir’s emphasis on particular, directly experienced events and on “the unique or individual, not the universal” (Sapir 1917: 446). At the same time, the above citation also refers to “inferred” experiences and later on, “such concepts as a clan, a language, a priesthood” are denied individual connotation and supposed to be involved in the same operations as natural science concepts: “observation, classification, inference, generalization, and so on” (Sapir 1917: 446), exactly the operations Fodor frequently refers to with respect to all sciences.

Fodor’s suggestion that, in special sciences, the generalizations are all of the functional type has, in turn, to be taken with a grain of salt. When applied to geology, the term “functional” is almost meaningless. Sapir’s appeal to “a certain philosophically arbitrary but humanly immensely significant selection out of the total mass of phenomena”, quoted above, seems to be a more adequate – but for Fodor undoubtedly too subjective – characterization of what special sciences are about, although he does not eschew the term “interesting”.¹⁶ So Sapir and Fodor appear to appeal to the same insight, worded differently.

There is another difference between Sapir’s and Fodor’s ideas about special sciences. In Sapir’s examples, ontologically heterogeneous features are simultaneously realized, for example in the earth, or a mountain chain. In Fodor’s special sciences, they are realized in different events (the “tokens”) at different moments, for example in various monetary transactions.¹⁷ This difference is not entirely watertight, however. Sapir refers to events too (cf. the quotation on p. 29). His incorporation of history in the special sciences and examples such as “the French Republic, the romantic movement in literature, Victor Hugo” also suggest that the heterogeneous counterparts of special science entities may be events. Fodor’s extension of the class of special sciences to geology and his comparison with artefacts, in turn, implies that he also recognizes the possibility of simultaneous presence of heterogeneous features.

Certainly Sapir and Fodor did not have *exactly* the same idea of special sciences in mind. But their ideas were more similar than their formulations suggest at first

¹⁶In “Special sciences”, there are some references to the alleged “interestingness” or “importance” of the natural kinds of a special science. Compare the following passage about monetary exchange: “The point is that monetary exchanges have interesting things in common. But what is interesting about monetary exchanges is surely not their commonalities under physical description” (Fodor 1974: 103-104).

¹⁷Consequently, Fodor’s presentation of the physical counterparts of a special science predicate as a disjunction does not apply to the physical counterparts in Sapir’s examples. In these cases, they constitute a conjunction.

sight.

8 Laws and exceptions

Sapir and Fodor are both convinced that special science laws have exceptions. For both scholars, scientific practice is an important argument. Sapir describes this practice and contrasts it with natural science practice: “If, out of one hundred clans, ninety-nine obeyed a certain sociological ‘law’, we would justly flatter ourselves with having made a particularly neat and sweeping generalization; our ‘law’ would have validity, even if we never succeeded in ‘explaining the one exception’” (Sapir 1917: 447). According to Fodor, the idea that laws of special science are exceptionless has to be rejected because it “flies in the face of fact. There is just no chance at all that all the true, counter-factual supporting generalizations of, say, psychology, will turn out to hold in strictly each and every condition where the antecedents are satisfied” (Fodor 1974: 111).

Both Sapir and Fodor thus take the requirement of *historical adequacy* (conformity to clear cases of scientific practice) for philosophy of science seriously and derive a strong argument for exceptions to special science laws from actual scientific practice. When it comes to *philosophical adequacy*, however, their arguments differ widely. Sapir appeals to his above-mentioned claim that special sciences are about particular events. “Laws” are actually abbreviations for a finite number of phenomena. Sapir admits that this is a complicated issue and adds here a footnote about Rickert for further reading.

Fodor’s argument is entirely based upon the disjunctive character of the antecedent and the consequent of the physical counterpart of special science laws. The resulting physical “law” is not a genuine law (cf. section 3 above) and this explains why special science laws have exceptions.

With respect to philosophical adequacy, Sapir’s as well as Fodor’s explanation appeals to the pseudo-lawlike character of special science “laws”. However, the ways in which pseudo-lawlikeness is argued for are different.

Summarizing the last three sections, we may conclude that some aspects of token physicalism are elaborated in different ways by both scholars. These differences can be shown to be related to the temporal and intellectual context in which the theories were developed.

9 Getting involved

In the following three sections, Sapir’s and Fodor’s token physicalism will be embedded in a wider context. The rise and development of their theories can be further clarified in this way. There is, firstly, the preliminary question of how they got involved in the problem of relations between disciplines and, secondly, whether their similar solutions were based on any clues in their intellectual environments. Finally, we may ask what, in general, became of Sapir’s and Fodor’s token physicalism. Neither Sapir nor Fodor was a specialist in general philosophy of science. During his student years, Sapir did not follow a philosophy programme, but his education in Germanic philology certainly yielded some knowledge of German philosophy, the breeding ground for the distinction between *Naturwissenschaften* and *Geisteswissenschaften*. Fodor was educated in philosophy. He was a pupil of Hilary Putnam and acquired a thorough knowledge of philosophy of science, but philosophy of mind became his specialism. Like many scientists, especially in the humanities and the social sciences, both scholars became involved in the broader issue in philosophy of science of relations between disciplines through problems in their scientific work or through reflection on this work.

In Sapir’s case, his master thesis on Herder’s *Ursprung der Sprache* (Sapir 1907) bears witness to an early interest in the foundations of linguistics, but he did not become involved in foundational issues again until 1917. Kroeber’s article seems to have been the direct incentive for Sapir’s development of token-physicalistic ideas. He must have been dissatisfied with Kroeber’s ontological answer to the question of what social sciences are about. Sapir’s title “Do we need a ‘super-organic?’” reveals an Ockhamian line of approach: we must, if possible, avoid an unnecessary appeal to unknown and questionable entities such as Kroeber’s superorganic force. Token physicalism supplied a promising alternative.

In Fodor’s case, there is not, as far as I know, such a direct “external” occasion for his development of token physicalism. My hypothesis is that there was an “internal” occasion. As well as Putnam, Noam Chomsky, his MIT colleague, became very influential to Fodor’s intellectual development. Fodor adopted Chomsky’s mentalistic approach of claiming psychological reality for linguistic categories, rules etc. When Fodor wrote “Special sciences”, he was probably simultaneously writing *The language of thought*, a book which went further than Chomsky in postulating mental, and even innate, entities, structures and operations in the cognitive systems of thinking and communicating humans. Token physicalism could furnish a foundation for this approach by emphasizing the autonomy of

psychology. The fact that the text of “Special sciences” constitutes the second section of the *Introduction* to *The language of thought* is an indication that Fodor saw it that way.¹⁸ The final paragraphs of the *Introduction*, absent in “Special sciences”, confirm this suggestion. Compare the concluding sentences: “It has [...] been the burden of these introductory remarks that the arguments for [...] the physical reduction of psychological theories are not, after all, very persuasive. The results of taking psychological theories literally and seeing what they suggest that mental processes are like might, in fact, prove interesting. I propose, in what follows to do just that” (Fodor 1975: 26).

10 Clues to token physicalism

Both Sapir and Fodor present their varieties of token physicalism as new ideas. Indeed, there were no earlier theories with this content. But there certainly were ideas of others which functioned as substantive building blocks or as sources of inspiration for their views.

Both scholars only refer briefly to fellow scholars in their texts. Apart from Kroeber, Sapir only refers to Rickert, in the footnote reference mentioned above (the only footnote in the article). Sapir characterizes Rickert’s *Die Grenzen der naturwissenschaftlichen Begriffsbildung* as “difficult but masterly” and continues: “I have been greatly indebted to it.” This is understandable: Rickert’s way of distinguishing *Geisteswissenschaften* and *Naturwissenschaften*, not in terms their subject matter, as other philosophers would have it, but in terms of their ways of concept formation, appears to have inspired Sapir directly (cf. Anchor 1967). Therefore, I do not share Silverstein’s doubts about this indebtedness to Rickert: “While Sapir, in his paper, expresses his debt to Rickert [...], it is clearly Boas’ discussion of 1887, the very phraseology and terms of which he repeats, that underlies his discourse” (Silverstein 1986: 70, fn.5).

In any case, neither Rickert nor Boas developed anything comparable to token physicalism. Both scholars adopted the distinction between *Geisteswissenschaften* and *Naturwissenschaften*. But both assumed a much deeper chasm between the two types of science than Sapir did, by restricting *Geisteswissenschaften* to a “value-laden” (Rickert) or “affective” (Boas) focus on *individual* entities and regarding all generalizing thought as proper to natural sciences only.¹⁹ We ob-

¹⁸The *Introduction* is titled “Two kinds of reductionism”. Its two sections are named after views Fodor argues against: “Logical behaviorism” (about Wittgenstein’s and Ryle’s views of psychology) and “Physiological reductionism”.

¹⁹See Anchor (1967) and Silverstein (1986) for Rickert’s and Boas’ views, respectively.

served above that Sapir was also inclined to take into account the individuality of the phenomena described by special sciences. But he also recognized their clustering into abstract, generalized entities, which are subjected to operations such as “classification, inference, generalization, and so on” in these sciences. This view strongly deviates from Rickert’s and Boas’ views and is similar to Fodor’s view. Both Sapir and Fodor claim that special sciences share their general methodology with basic sciences.

Fodor does not mention any indebtedness. Nevertheless, token physicalism is often regarded as similar to Putnam’s idea of *multiple realizability*, presented in several publications in the nineteen sixties (cf. Putnam 1960, Rey 1991: xiii). Multiple realizability is the thesis that the same mental property can be implemented by different physical properties. Actually, without mentioning the term, Fodor (1974: 105-106) refers to this idea. He explicitly mentions Putnam’s reference to computers as possible providers of physical counterparts of psychical events. Connections are also observed with Davidson’s *anomalous monism*, which, like Fodor’s theory, restricts the links of the physical and the psychical to the level of events (cf. Davidson 1970, Rey 1991: xxxi). Fodor does not refer to Davidson’s theory, but a reference to Davidson (1970) in *The language of thought* (p.200) proves that he knew about it. So Fodor’s idea of how psychology reduces to physics was clearly prepared by other philosophers he knew about. Fodor, however, extended Putnam’s and Davidson’s solutions to the mind-body problem to a thesis about sciences in general, their typology and their characteristics as intellectual enterprises.

11 What became of token physicalism?

Neither Sapir’s nor Fodor’s version of token physicalism was elaborated further by their authors after the publications discussed above. Two further questions will be explored now:

- a. Did token physicalism, as presented in these publications, play a role in their later work?
- b. Did token physicalism play a role in the work of later scholars?

Answering these questions exhaustively is far beyond my limited state of knowledge, but this does not prevent me from making some tentative suggestions.

As to the first question, token physicalism, not surprisingly, “sets the stage” for Sapir’s and Fodor’s further research in their respective “special sciences”.

Sapir presents and practises linguistics and anthropology as autonomous sciences; Fodor's "psychosemantics" is also practised autonomously, without any appeal to specific brain states.²⁰ But in their writings, token physicalism is not at all prominent; it is a background framework rather than a major discussion theme.

For example, Sapir does not refer at all to his typology of sciences in his 1929 article "Linguistics as a science", although the main theme of this article is the relation between linguistics and other sciences.²¹ The conclusions drawn – e.g. that linguistics is not "a mere adjunct of either biology or psychology" (Sapir 1929: 214) – are in line with those drawn in 1917, but they are attained without any appeal to the distinction between conceptual sciences and historical sciences. The same is true of the passage about the definition of language in the first chapter of Sapir's seminal book *Language* (1921). There, Sapir claims that language cannot be defined "as an entity in psycho-physical terms alone" and that language can be discussed "precisely as we discuss the nature of any other phase of human culture – say art or religion – as an institutional or cultural entity, leaving the organic and psychological mechanism back of it as something to be taken for granted" (Sapir 1921: 10-11).

A clear echo of Sapir's discussion with Kroeber can be found in Irvine's reconstruction of Sapir's lectures on the psychology of culture, presented in the 1930s (Sapir 1994). In a lecture on "difficulties of the social sciences", Sapir mentions the problem that "the culturalist [...] cannot be absolutely sure of the limits or bounds of what he is dealing with", unlike physicists, who "know what particular corner of the universe they are dealing with". Another difficulty is the essential uniqueness of cultural phenomena. Referring to Rickert, Sapir contrasts the physicist, who deals with a conceptual universe, covering all possible phenomena, in an abstract way allowing for one hundred percent accuracy, with the social scientist, who studies all actual and unique phenomena, without this same level of accuracy (Sapir 1994: 56-57). In another lecture, Sapir explicitly refers to his discussion with Kroeber. A sentence literally repeated from Sapir (1917: 444) concludes the passage: "Social science is not psychology, not because it studies the resultants of superpsychic or superorganic, but because its terms are differently demarcated" (Sapir 1994: 245). But again, none of these claims is argued for in terms of an explicit and general typology of sciences, as presented in Sapir (1917).

²⁰*Psychosemantics* is the title of a 1987 book by Fodor. I apply the term here to the totality of Fodor's work on cognitive psychology and its relations to semantics.

²¹In Sapir (1929), linguistics is emphatically presented as a science aiming at generalization, explanation, laws etc., which is at odds with Silverstein's (1986) idea that the views presented in Sapir (1917), as he interprets them in Boasian terms (cf. p.10), permeate Sapir's entire oeuvre.

Fodor now and then refers to token physicalism after 1975. Like Sapir, he wrote an article which surprisingly omits the subject (“Some notes on what linguistics is about”, 1985). In an article about the mind-body problem in *Scientific American* (Fodor 1981), one paragraph is devoted to a brief explanation and defence of token physicalism as part of the solution to this problem. In his books *Psychosemantics* (Fodor 1987: 5-6) and *The elm and the expert* (Fodor 1994: 39), the “special science” status of psychology is mentioned but, as in Sapir’s case, without a reference to the broader context of token physicalism and the issue of typology of sciences.

The inconspicuous role of token physicalism in Fodor’s work cannot be better illustrated than by the obituaries that appeared after his death on 29 November 2017. Of the eight obituaries I read, all paying ample attention to the content of Fodor’s scientific work, only one, Rey (2017), mentions token physicalism.

Our second question about the role of token physicalism in the work of later scholars receives a negative answer in Sapir’s case. As far as I know, Sapir’s token physicalism *avant la letter* was not discussed by other scholars. His distinction between conceptual and historical sciences was neither adopted nor criticized by his linguistic or anthropological colleagues. Autonomy versus reducibility was an important and controversial issue for all humanities and social sciences, before and after 1917, but Sapir’s solution does not seem to play any role in this multi-faceted discussion.²²

Fodor’s token physicalism, on the other hand, became a rather popular issue in philosophical discussions, and remains so up to the present day.²³ Many philosophers of science have analysed and commented on Fodor’s views. A considerable portion of their reactions are critical and try to vindicate some variety of reductive physicalism. As an example of the broad impact of Fodor’s token physicalism, I would mention *The Electric Agora* (“a modern symposium for the digital age”), which devoted a Special to “Jerry Fodor’s ‘Special sciences’” in 2015. After a brief introduction about “one of the most influential essays in the philosophy of science since the Second World War” (Kaufman n.d.: 1-2), thirty comments follow.

In areas outside philosophy, I found very few reactions to Fodor’s token-physicalistic ideas.²⁴ It is sometimes suggested that all practising cognitive scientists now adopt Fodor’s line of thought and proceed without any appeal to neurology. For example, Jones (2004) claims that “this [token physicalism] has been the consensus view among cognitive scientists since at least the mid-seventies”, due

²²This tentative conclusion is based on a search in Google and Google Scholar for *conceptual science*. No items were found containing this expression in the Sapirean sense.

²³The most recent article devoted to token physicalism I found is DiFrisco (2017).

²⁴I found only six non-philosophical items via a Google Scholar search for *token physicalism*.

to Fodor's 1974 article. This is, however, an overstatement. In the same article, when talking about belief states, Jones claims that their reduction to physical neurological states "has been at the centre of numerous research projects in the behavioural and brain sciences for decades" (Jones 2004: 423). This recent observation shows the lasting validity of an earlier claim by Fodor himself that many psychologists are type physicalists who believe that every psychological kind predicate is lawfully related to a neurological kind predicate and that "there are departments of psycho-biology or psychology and brain science in universities throughout the world whose very existence is an institutionalized gamble that such lawful coextensions can be found" (Fodor 1974: 105, cf. footnote 10).

In summary, Sapir's token physicalism seems not to have left traces in the work of later scholars.²⁵ Fodor's token physicalism was only partially influential as a programme for research in cognitive science. But it did become the subject of a lively philosophical debate that is still ongoing.

12 Linguistics as a "special science"

Recently, a newly appointed professor of Dutch Linguistics at Leiden University claimed, in his inaugural lecture, that linguistics is in crisis because it is thought it may become superfluous fairly soon. Language, as a cognitive phenomenon, can now be investigated through brain research, so why should there be a separate discipline of linguistics alongside neurology? The answer is that the role of linguistics has not yet become entirely irrelevant because the help of linguists is still necessary for the correct interpretation of the neurocognitive data (Barbiers 2017).

This line of argument, which presupposes correspondences between linguistic and neurological natural kinds, is a clear example of reductive, type-physicalistic thought. Such a radically reductive view of linguistics is not new, but recent developments in neurolinguistics have made it much more prominent and much more applicable (and actually applied) in research practice. But it is not, and never was, the only view. On the contrary, there are many linguistic approaches that do not make any appeal to neurology, either because of a more autonomous psychologicistic conception of cognitive-linguistic research, or because of a more

²⁵There might be traces in work I did not consult: later publications by Kroeber, or his correspondence with Sapir, which lasted for several decades. On p. 40, Silverstein (1986) was mentioned for drawing attention to Sapir's token physicalism from the perspective of the history of linguistics.

radically autonomous, non-psychologistic view of linguistics (cf. Botha 1992; Elfers 2014).

Thus far, token physicalism does not play a role in discussions about these approaches. This might be due to the context in which it was introduced – anthropology in Sapir’s case, philosophy of science in Fodor’s case. Neither (Sapir 1917) nor (Fodor 1974) explicitly refer to linguistics but, as was argued in section 6, both scholars certainly incorporated linguistics in the category of special sciences, even if this incorporation has a different meaning for Sapir’s and Fodor’s varieties of token physicalism. For Sapir, psychology is a basic science. Linguistics, as a special science, has a non-psychological status. Language belongs, with art and culture, to the category of “human institutions” (cf. the quotation on p. 42). For Fodor, only physics is a basic science; psychology is a special science. Given Fodor’s psychologistic view of linguistics, linguistics is also a special science.²⁶

Can token physicalism, if plausible at all, play a relevant role in the discussions of linguistic approaches examined above? A positive answer seems possible. The conception of linguistics as a “special science” can play a supportive role in the argumentation of both psychologists and non-psychologists. For psychologists, token physicalism can help to justify the fact that they do not appeal to neurology. Thus far this justification is often lacking or unconvincing. For example, many cognitive linguists (cf., e.g., Langacker 1999) make strong statements about mental architecture and processes, without discussing questions of neurological reality. Chomsky (1987: 5–6) claims that such discussions are unnecessary, because chemists, too, “have not stopped to discuss ‘abstractly construed’ molecule elements, the periodic system and so on”. The analogy fails, because chemists *could* apply the vocabulary of atomic physics instead, whereas linguists are far from knowing what corresponds neurologically to their psychological-linguistic categories. Token physicalism provides a better justification for not discussing neurological equivalents of linguistic natural kinds.

For non-psychologists, the autonomy of linguistics often implies a rather problematic ontological status of language. Like Kroeber, they look for an ontological answer to questions of non-reducibility. For example, according to Cooper (1975), language belongs to a separate “linguistic reality”, Itkonen (1978) assumes a non-empirical “social reality” which incorporates language, Katz 1981 localizes lan-

²⁶Fodor (1985: 149) claims that “it is nomologically necessary that the grammar of a language is internally represented by speaker/hearers of that language”. In itself, Fodor’s token physicalism allows for an “institutional” interpretation of language as well (cf. his discussion of economics). Jones (2004: 422–423) regards multiple realizability as a typical feature of institutional facts in general.

guage in an abstract “Platonist” realm. In all cases, there is, apart from Ockhamian considerations, the problem of explaining the interaction of these separate realms with the psychological realm of actual use and knowledge of language. Without suggesting that token physicalism offers ready-made solutions, I feel that it has certain advantages: language use consists of (psycho-)physical events (tokens) and the linguist’s constructs they instantiate (types, natural kinds) are epistemologically but not ontologically autonomous.²⁷

Of course, this extension of the discussion area in the final paragraphs of this chapter is too fragmentary. But it may give an impression of how the idea of linguistics as a “special science” can play a role in discussions of linguistic approaches.

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²⁷For non-psychologists, Sapir’s variety of token physicalism is, of course, a better example than Fodor’s. Ironically, Itkonen is the only one of these more recent scholars who made a thorough study of Sapir’s work, including his anthropological publications, but he seems to have missed Sapir (1917), and his interpretation of Sapir’s view as identical to his own non-empirical view of linguistics is a mistake (cf. Itkonen 1978: 62–65).

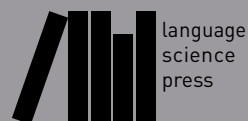
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Form and formalism in linguistics

“Form” and “formalism” are a pair of highly productive and polysemous terms that occupy a central place in much linguistic scholarship. Diverse notions of “form” – embedded in biological, cognitive and aesthetic discourses – have been employed in accounts of language structure and relationship, while “formalism” harbours a family of senses referring to particular approaches to the study of language as well as representations of linguistic phenomena. This volume brings together a series of contributions from historians of science and philosophers of language that explore some of the key meanings and uses that these multifaceted terms and their derivatives have found in linguistics, and what these reveal about the mindset, temperament and daily practice of linguists, from the nineteenth century up to the present day.

