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Emmy Noether's first great mathematics and the culmination of first-phase logicism, formalism, and intuitionism

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Abstract Emmy Noether's many articles around the time that Felix Klein and David Hilbert were arranging her invitation to Göttingen include a short but brilliant note on invariants of finite groups highlighting her creativity and perspicacity in algebra. Contrary to the idea that Noether abandoned Paul Gordan's style of mathematics for Hilbert's, this note shows her combining them in a way she continued throughout her mature abstract algebra.

1 Introduction

Emmy Noether's first major article in mathematics was a four-page note on invariants of finite groups, which she published while Felix Klein and David Hilbert were arranging her invitation to Göttingen. One page essentially solves the problem merely by stating it compactly, the next two pages give two explicit solutions, and the last page describes related work. The opening line correctly claims that these explicit solutions are shorter and simpler than the already known Hilbert-style proof that these solutions exist Noether (1916, p. 89).

The article grew out of Noether's work on Hilbert's algebra but she did not convert to that and abandon the old-fashioned symbolic algebra of her dissertation teacher Paul Gordan. On the contrary, this article marks her first step in merging Gordan's methods with Hilbert's, an approach to finite groups that she would continue to develop

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throughout the rest of her career, culminating in her plenary address at the 1932 International Congress of Mathematicians in Zurich (Noether 1916, 1926, 1929, 1932). And contrary to what is sometimes lazily supposed, and as Corry (1996, p. 170) justly says, Hilbert's axiomatic algebra was never abstract because it always had "immediate, intuitive significance" but Gordan on the other hand taught that highly practical tools can be remote from classical intuition.

This article offers three histories of Noether (1916). Each one explains how Noether came to the problem, how she solved it, and why she wrote it up as she did. Any one of them would be entirely satisfactory by itself if the others did not exist. One shows the article creating a symbolic formalism for calculating invariants in Gordan's style, another bases the work on Galois theory, and the third makes it Noether's creative response to Dedekind and Hilbert's algebra of groups and modules. After discussing Noether's generally ahistorical outlook the article closes by arguing that her abstract algebra unified Gordan's view of mathematics with Hilbert's.

2 Klein's triad

> Among mathematicians in general, three main categories may be distinguished; and perhaps the names logicians, formalists, and intuitionists may serve to characterize them.
>
> —Klein (1894, p. 2)

It is very likely that Noether knew these terms, which Klein published in English, because Klein was a family friend, Noether was certified to teach English, and all the mathematicians Klein gave as examples were household names from her childhood on. In any case Klein's typology illuminates the larger significance of her work. His logicians, formalists, and intuitionists did not urge different foundations for mathematics but used different working styles. Let us call these *first-phase* logicism, formalism and intuitionism. We cannot enter here into the lively history of how first L.E.J. Brouwer and then Hilbert co-opted the first-phase terms into the later foundational controversy with the meanings that they have today, but we will say that the way to this co-optation was cleared when Noether, among others, unified all three of these working styles so smoothly that the first-phase meanings became idle.

Klein explained his terms this way:

> (1) The word logician is used here, of course, without reference to the mathematical logic of Boole, Peirce, etc.; [it indicates an] ability to give strict definitions and to derive rigid deductions therefrom. The great and wholesome influence exerted in Germany by Weierstrass in this direction is well known. (2) The formalists among the mathematicians excel mainly in the skillful formal treatment of a given question, in devising for it an "algorithm." Gordan [...] must be ranged in this group. (3) To the intuitionists, finally, belong those who lay particular stress on geometric intuition, not in pure geometry only, but in all branches of mathematics.

> Clebsch must be said to belong both to the second and third of these categories, while I should class myself with the third, and also the first.
> (Klein 1894, p. 2)

Noether's dissertation was purely formalist in these terms. It relied on long symbolic calculations devoid of geometry and burdened with famously obscure definitions and deductions taken from Gordan. We will see that (Noether 1916) was a great step toward making her a logician. Furthermore, at just about the same time, Noether was absorbing Sophus Lie's geometrical group theory in order to produce her conservation theorems in mechanics. Geometric intuition would underlie much of her work after that. Most relevantly for us, her work directly descended from (1916) "introduced the idea of representation space" by which Emil Artin says "she enables us to use our geometric intuition" in group representation theory (Artin 1950, p. 67). Noether was hardly alone in bringing rigor, calculation, and geometry together. Easier communication and the sheer rising number of mathematicians required and facilitated a trend to uniform styles of expression and standards of rigor. Publishers, notably Springer-Verlag, disseminated these in ever more easily available books and journals. But Noether was a key player and the algebraic style which she made by combining these in her particular way would descend through van der Waerden and Bourbaki to become the central unifying device of twentieth century mathematics. Once Klein's three working styles all succeeded so well as to become largely common property of all mathematicians, they were idle as a trichotomy.

3 The place of the work in Noether's life

> There was nothing rebellious in her nature; she was willing to accept conditions as they were.
>
> —Weyl (1935, p. 430)

Weyl probably heard this from Noether's younger brother Fritz, who was very close to her, and it was probably true throughout her life in pretty, quiet Erlangen until 1915 when she was 33, the year she sent her (1916) to the *Mathematische Annalen*.[1] As is well known, her father Max Noether was a noted professor of mathematics at Erlangen who encouraged her education and admired her work. The other Erlangen mathematics professor was Gordan, who was more famous than her father, and eventually became her doctoral advisor when she turned to mathematics. She studied for one semester in Göttingen in 1903, but her dissertation of 1908 with Gordan pursued a huge calculation that had stumped Gordan forty years before and which Noether could not complete either. So far as I know no one has ever completed it or even checked it as far as she went. It was old-fashioned at the time, a witness to the pleasant isolation of Erlangen, and made no use of Gordan's own work building on Hilbert's ideas.[2]

[1] For biography of Noether see Dick (1981), Kimberling (1981), McLarty (2005), Roquette (2008) and especially Tollmien (1990).

[2] Gordan (1893, 1899, 1900).

She now supervised dissertations under her father's name. She was elected to the Circolo Matematico di Palermo in Italy in 1908 and the Deutsche Mathematiker-Vereinigung in 1909. By age 30 in 1912 she was known to and respected by most of the important mathematicians in Germany, and she was well into what anyone would have called a successful career as a mathematician, except, of course, that she had neither title nor salary. Noether probably saw no reason for any of this to change, and she would be known today in the history of women in mathematics if it had not changed—but change it did. When Gordan retired in 1910 he was replaced by two students of Hilbert: Erhard Schmidt (b. 1876) and Ernst Fischer (b. 1875), and Noether worked with both. She especially worked with Fischer and they credit each other extensively in their articles. In the next few years she became remarkably productive, publishing on classical invariant theory, and on transcendental numbers using Zermelo's axiom of choice. More importantly, conversations with Fischer especially led her to a series of four articles (1913, 1915, 1916, 1918a), of which the third is our chosen focus. The first lays out the whole series, so that the last incidentally shows how long Noether might hold on to an article between doing most of the work and finishing the publication. The second addresses Hilbert's 14th problem, is by far the longest, has the most technical results, and covers the most topics. The fourth applies her results to Galois theory.

Section 5 of (1915) includes the quantities $x_i^{(k)}$ and the polynomial $\Phi(z, u)$ we focus on and Noether reasonably says the result of our chosen article is "implicit" here (1916, p. 92).[3] The whole series shows Noether as a powerful creative algebraist such as no one could have imagined from her work before—although it pales in comparison to the work to come—and (1916) has the most astonishing proof and is closest to Fischer's question that motivated the whole series.[4] The histories offered here draw on the whole series and apply to the whole, but they lead especially to (1916).

In 1915 Noether went to Göttingen. Throughout the First World War and then the German Revolution Noether lived on the edge of poverty, became a radical socialist, a radical algebraist, and the teacher of "without exception all the better young German mathematicians" according to the influential Solomon Lefschetz at Princeton, who was himself unsympathetic to abstract algebra (Kimberling 1981, quoted p. 35). The article on finite groups examined here is the first brilliant show of her strength.

4 Theorem and proof

> Noether's proofs were (and remain) startling in their simplicity.
>
> —Jacobson (1983, Introduction p. 13)

We first comment on the basics of Noether (1916). A translation is included as an appendix, to avoid quoting most of it repeatedly. We comment on different details differently in each of the three histories.

[3] Noether thought (1915) might win her Habilitation. See Koreuber and Tobies (2008).

[4] See Noether (1915, p. 162, 1918a, p. 222).

The article opens with a standard problem in group theory of the time: find all the polynomial invariants of a finite group $\mathfrak{H}$ of linear transformations.[5] Noether wrote the i, j-th component of each matrix A_k as $a_{i\,j}^{(k)}$. By matrix multiplication each matrix A_k carries each series of quantities (*Größenreihe*—we would say a vector) $x_1 \dots x_n$ to a new series of quantities which Noether calls $x_1^{(k)} \dots x_n^{(k)}$. The specific equation is

$$x_i^{(k)} = \sum_{\nu=1}^{n} a_{i\,\nu}^{(k)} x_\nu$$

The familiar matrix calculation shows the result of applying A_j to $x_i^{(k)}$ is $x_i^{(jk)}$ the same as applying the composite $A_j A_k$ to x_i.

An *invariant* of $\mathfrak{H}$ is a polynomial $f(x_1 \dots x_n)$ whose value is unchanged by any action of $\mathfrak{H}$.[6] That is, for each A_k in $\mathfrak{H}$

$$f(x) = f(x^{(k)})$$

Here x abbreviates the series $x_1 \dots x_n$ and $x^{(k)}$ abbreviates the transformed series $x_1^{(k)} \dots x_n^{(k)}$. A simple example will help. Let $\mathfrak{H}$ contain just two matrices:

$$A_1 = \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix} \quad A_2 = \begin{pmatrix} -1 & 0 \\ 0 & -1 \end{pmatrix}$$

Because these are 2×2 matrices, the group acts on pairs of quantities x_1, x_2. Since A_1 is the identity matrix it acts by

$$x_1^{(1)} = x_1 \quad x_2^{(1)} = x_2$$

And A_2 multiplies each quantity by -1:

$$x_1^{(2)} = -x_1 \quad x_2^{(2)} = -x_2$$

So a polynomial $f(x_1, x_2)$ is invariant if and only if $f(x_1, x_2) = f(-x_1, -x_2)$, and it is easy to see that f meets this condition if and only if every term of f has even degree, so that the minus signs cancel out. The invariants x_1^2 and $x_1 x_2$ and x_2^2 give a *complete basis*. Every invariant polynomial $f(x_1, x_2)$ can be written as a polynomial combination of these three in at least one way; for example, the invariant $x_1^4 x_2^2$ can be written two ways:

$$(x_1^2)^2 x_2^2 \quad \text{or} \quad x_1^2 (x_1 x_2)^2$$

[5] As was common, Noether is silent as to the field they are linear over. Noether (1915, p. 163) notes that some steps require the field have characteristic 0. Noether (1926) introduces great new ideas to eliminate that assumption.

[6] Noether here calls polynomials "integral rational functions," following the general tendency in Dedekind, Hilbert, and Weber to favor the field of rational functions over the ring of polynomials.

Noether essentially solved the problem merely by noting that an invariant is any polynomial $f(x)$ that is equal to the average of all its transformed values:

$$f(x) = \frac{1}{h}\sum_{k=1}^{h} f(x^{(k)}) \tag{1}$$

Any A_j acting on the two sides turns the left side to $f(x^{(j)})$; and it leaves the right hand sum unchanged as it merely permutes the terms, turning each $f(x^{(k)})$ into $f(x^{(jk)})$. So each $f(x^{(j)})$ equals that one sum. But since each term in the sum contains just one of the $x^{(k)}$, *every* permutation of the series of quantities leaves the sum unchanged, which solves the problem! The $\mathfrak{H}$ invariant polynomials are precisely those equal to some polynomial *symmetric* in the transformed series of quantities $x^{(k)}$.

Note carefully what it means to be symmetric in several series of quantities. Noether was not now talking about functions that remain unchanged when the quantities in any one series

$$x^{(k)} = \{x_1^{(k)} \dots x_n^{(k)}\}$$

are permuted among one another. She was talking about polynomials in several series $x^{(1)} \dots x^{(h)}$ that are unchanged when these series are interchanged entirely with one another. The superscripts are permuted while the subscripts are unchanged, so each $x_i^{(j)}$ is changed to some $x_i^{(k)}$.

Noether said it was "well known" that every polynomial symmetric in several series is some polynomial combination of the "elementary symmetric functions" of these several series.[7] To find all the invariants for the group $\mathfrak{H}$, one takes the finitely many elementary symmetric functions of the several series $x^{(1)} \dots x^{(h)}$ and replaces each quantity $x_i^{(k)}$ by its equivalent in terms of the original $x_1 \dots x_n$ and the matrix coefficients $a_{i,j}^{(k)}$. This gives a finite set of invariant functions of $x_1 \dots x_n$. Every invariant function of them is some polynomial combination of these. In short, these form a finite complete system of invariants for $\mathfrak{H}$. She gives a second complete system, which we shall not consider, using power sums of these same $x_i^{(k)}$.

Symmetric polynomials were part of classical nineteenth century algebra. The deep difficulty in 1916, and still today, is to understand the $x_i^{(k)}$, which Noether called quantities (*Größen*). They cannot simply be numbers or elements of some field, for, to get the basis of invariants we must distinguish $x_i^{(k)}$ from $x_p^{(j)}$ whenever $i \neq p$ or $k \neq j$, and make the appropriate substitution in terms of the correct corresponding sum

$$\sum_{\nu=1}^{n} a_{i\,\nu}^{(k)} x_\nu \quad \text{or else} \quad \sum_{\nu=1}^{n} a_{p\,\nu}^{(j)} x_\nu$$

[7] Noether (1916, p. 90) has not one but two footnotes to this well-known fact, neither citing a proof. It is not in Weber (1895, 1912). It is stated without proof in *Encyklopädie der mathematischen Wissenschaften* Bd. 1, Teil 1B, 3b. Junker (1893) proves it in terminology resembling Noether's. These polynomials were Gordan's bread-and-butter means of forming invariants but I do not find this result stated in his work. Inspired by Noether, Weyl (1939, p. 37) proves the result as the "First Main Theorem" of invariant theory.

Each $x_i^{(k)}$ must register the identity of x_i and A_k. But how? Before giving the histories of Noether's article we look at the basic options at the time.

5 Algorithmic versus the new algebra

The difference between Gordan-style algorithmic and Dedekind–Hilbert-style axiomatics comes down to the difference between variables as calculating symbols and variables as objects varying over the elements of some algebraic structure. That distinction was hard to make in 1916. Weber (1912), a textbook that Noether cites and very likely owned, favors the traditional identification of "algebra" with algorithmic and explains it this way:

> In analysis one is accustomed to understand a "variable" as a sign which takes successively different values. Algebra uses the word variable as well but in a different sense. Here it is a mere calculating symbol (*Rechnungssymbole*) with which one operates by the rules of calculating with letters (*Buchstabenrechnung*). (1912, p. 47)

This may seem clear. In analysis the variables vary over the real or complex numbers (or some related set) while algebra often calculates with letters by formal rules. Weber's larger textbook says letter calculation is the chief tool of algebra. But his fuller explanation shows the distinction is unstable:

> The use of this tool is so general that one often uses the word *Buchstabenrechnung* as a synonym for algebra.... Equations between letter expressions are so-called identities [when] two expressions set as equal can be deformed by applying the calculating rules so that the two come to agree exactly. Then one gets correct numerical equations from the letter equations when the letters are replaced by any kind of number whether real or complex, so long as one has not tried to divide by zero. The letters in such equations may also be called variables because one may think, without falling into contradiction, that successively one after another numerical value is set for the letters. (Weber 1895, p. 20)

In analysis variables take successively different values, while in *Buchstabenrechnung* we may, if we wish, think that they do! This is not a clear distinction. Weber was deeply concerned with principles and did not merely intend a pragmatic orientation for beginners. That was just the best he could do.[8]

Furthermore, Weber underrated how far symbolic calculation might differ from numerical. In the symbolic method that Gordan taught to Noether, an equation $P = Q$ need not even imply $P^2 = Q^2$. Indeed, if the first equation holds with P a numerical expression and Q a symbolic one, then the second is not even well-formed and using it would lead to contradiction. Given the general binary quadratic form

[8] Gottlob Frege argued decisively that mathematics cannot all be formal but took no position on the legitimate role of formalism. In personal communication Jamie Tappenden says Frege knew the symbolic method for invariants but recorded no position on it, and Frege's sense of the formal shifted over time.

$$Ax^2 + 2Bxy + Cy^2$$

the method creates symbols a_1, a_2 with

$$a_1^2 = A \quad \text{and} \quad a_1 a_2 = B \quad \text{and} \quad a_2^2 = C$$

so that

$$(a_1 x + a_2 y)^2 = Ax^2 + 2Bxy + Cy^2$$

The symbols a_1, a_2 occur to total degree 2 in these equations. A numerical term can be equated to a symbolic expression only when each symbol pair such as a_1, a_2 occurs in the expression to total degree 2 (Gordan 1887, Vol. II, p. 2). For example, setting the values $A = C = 1$ and $B = 0$ gives theorems on the particular quadratic form $X^2 + Y^2$. But this substitution into the symbolic equation $(a_1 a_2)^2 = a_1^2 a_2^2$ would show $0 = 1$ this way:

$$0 = B^2 = (a_1 a_2)^2 = a_1^2 a_2^2 = AC = 1$$

The symbolic equation is correct and indispensable for Gordan. But it contains a_1 and a_2 with total degree 4, so numerical substitution is not allowed.

Today we use sets to model axioms on one hand; and symbolic calculation by string matching, usually computerized, on the other. In the first, variables in the axioms range over elements of those sets. The second uses finite symbol strings and is often regarded as a coding of arithmetic. Noether's algebra would do a great deal in the 1920s and 1930s to advance the first although she took little interest in logic or foundations. The second could not become clear until Kurt Gödel, Emil Post, Alan Turing and others produced the modern idea of algorithm in the 1930s. But the Weber quotes above show that nineteenth century mathematicians already made an important while unclear distinction between symbolic variables and variables which take successively different values.

6 Noether (1916) as symbolic algebra

The history of Noether (1916) closest to her own words depicts it as formalist in Klein's sense, an example of Gordan's influence: each $x_i^{(k)}$ is a mere symbol explicitly indicating a group element A_k and a quantity x_i. The calculations above constitute the proof and need no interpretation. Noether calculates with the central tools of Gordan's theory, which she called elementary symmetric polynomials in several series of quantities and he called the elementary symmetric polynomials *polarized* in those several series.[9]

[9] For each $k < n$ the coefficient of z^{n-k} in Noether's $\Phi(z, u)$ is the polarization of the k-th elementary symmetric polynomial $e_k(x_1 \ldots x_n)$ as defined by Gordan (1875, p. 4). Polarization is a device which can be defined either by polynomial expansion or by mixed partial derivatives, so that the complete polarization

On this history, Noether used the kind of tools Gordan taught her and solved the kind of problem he solved. In fact Noether gave a slicker solution than Gordan to a more far-reaching problem. Gordan had found a complete basis of invariants for the (infinite) group of *all* linear transformations of binary (i.e., two-variable) forms of any fixed degree n, Noether found a complete basis of invariants for any finite group acting linearly in any finite number of variables. Noether's problem had wider scope, she found a more elegant solution than is yet known for Gordan's, and her method led to vastly more further research.

There is, however, a mathematical problem. On this interpretation the proof is merely plausible and attractive. Noether never stated a criterion for the correctness and completeness of the symbolic rules, still less did she prove that they are complete. It is far from clear that these rules yield all the relevant equations on actual polynomials, and this is a mathematical defect no matter how productive the proof was to be in the long term. But this is no objection to the historical account. If anything it supports the account, since Gordan's method had the same defect to higher degree. It was extremely persuasive at the time, and universally accepted, and his results have since been confirmed by other means. But possibly no one ever understood Gordan's proofs. As Max Noether said in his eulogy of Gordan

> For each work he compiled volumes of formulas, very well ordered, but providing a minimum of text. His mathematical friends undertook to prepare the text for press and correct the printer's proofs. They could not always produce a fully correct conception and one often misses the deeper ground on which the considerations are laid. Only a few of his publications, and especially the earliest, express Gordan's specific style: bare, brief, direct, uninterrupted theorems one after the other. (Noether 1914, p. 5)

Possibly Gordan did not think of anything we would call proofs. He calculated. But he offered difficult theorems to show that in any given case these calculations will come to an end with a complete set of invariants. The published proofs are opaque, and at least one recent attempted reconstruction failed. Gian Carlo Rota worked several years with Joseph Kung on Gordan's method, and they eventually reported "We chose to describe and make rigorous [Gordan's] original notation and follow it as closely as possible." They succeeded beautifully at that, only to add that they could not find rigorous proofs resembling Gordan's: "A similar salvage operation could not, unfortunately, be carried out on the proofs" (Kung and Rota 1984, p. 28).

7 Noether (1916) as Galois theory

> Primarily two great general concepts lead to a mastery of modern algebra ... the concepts of *group* and *field*.
>
> — Weber (1912, p. 180)

Footnote 9 continued
of a degree n polynomial in one variable series is a polynomial in n different series, linear in each series. See Weyl (1939, p. 5) or Neusel (2006, pp. 123ff.).

A second history makes Noether (1916) a gem crystallized from a mass of more technical Galois theory. When Noether took up mathematics the paradigm finite groups of linear transformations were Galois groups as seen by Dedekind. The prestigious textbooks Weber (1895, 1896, 1912) propagated this theory, and the first also pursues the project announced by Hilbert (1887) at the start of his career to reform invariant theory by using the new field theory. Weber's account of invariants of finite groups centers on a Galois resolvent $\Phi(t)$ closely related to Noether's $\Phi(z, u)$.[10] This explains why the Galois resolvent solution comes first in Noether (1916). A shorter more elementary solution using power sums comes second.

Weber showed that every Galois field K over the rational numbers has a *primitive element*, that is a single element $\varrho \in K$ such that every element $a \in K$ can be expressed as a rational function $q(\varrho)$ of ϱ:

$$a = q(\varrho) = \frac{f(\varrho)}{h(\varrho)} \quad \text{for } f, h \text{ polynomials with rational coefficients}$$

Lagrange's theorem shows this is the same as saying no non-identity element of the Galois group leaves ϱ unaltered. This was central to Dedekind–Weber Galois theory and is proved for example by Weber (1912, p. 258). Weber called the minimal polynomial of any primitive element a *Galois resolvent* (1912, p. 240).

For any Galois field K over the rational numbers and subgroup $\mathfrak{H}$ of its Galois group, the fixed field $K^{\mathfrak{H}}$ consists of the invariant elements for $\mathfrak{H}$: those $x \in K$ such that $x = A_k(x)$ for every A_k in $\mathfrak{H}$. Lagrange's theorem gives the key relation to resolvents: *When Φ is any Galois resolvent of K over a fixed field $K^{\mathfrak{H}}$, then the elements of the fixed field $K^{\mathfrak{H}}$ are exactly the quantities expressible as rational functions of the coefficients of Φ.* To see this it is enough to consider K as a Galois extension of the extension of the ground field k by the coefficients of the resolvent.

The basic converse Galois problem is to take a finite group $\mathfrak{H}$ and find an extension of the rational numbers with Galois group $\mathfrak{H}$. It is not yet known which groups $\mathfrak{H}$ admit solutions. Hilbert (1892, p. 123) used indeterminate polynomials to relate this to rational function fields. Noether stated Hilbert's result concisely:[11]

> Let this equation
>
> $$f(x) = x^n + F_1(\lambda_1 \cdots \lambda_r)x^{n-1} + \cdots + F_n(\lambda_1 \cdots \lambda_r) = 0$$
>
> have Galois group $\mathfrak{H}$ over the field $\mathbb{Q}(\lambda_1 \cdots \lambda_r)$ of rational functions of the parameters $\lambda_1 \cdots \lambda_r$ with rational number coefficients. Then the parameters can be replaced in arbitrarily many ways by rational numbers so that the resulting polynomial

[10] Noether cited Weber (1899, §57–58), which corresponds to Weber (1896, §41–42).

[11] Hilbert and Noether both applied this to any "number field" Ω, by which they mean any subfield of the complex numbers. For simplicity we state it over the rational numbers.

$$g(x) = x^n + a_1 x^{n-1} + \cdots + a_n = 0$$

has Galois group $\mathfrak{H}$ over the rational numbers. (Noether 1918a, p. 222)

The theorem does not say which finite groups $\mathfrak{H}$ admit such polynomials $f(x)$.

Noether found a sufficient condition that is still pursued in research today (Swan 1983). Consider the field $\mathbb{Q}(\lambda_1 \ldots \lambda_h)$ of rational functions over $\mathbb{Q}$ with one parameter for each element of the group $\mathfrak{H}$, and the field of invariants $\mathbb{Q}(\lambda_1 \ldots \lambda_h)^{\mathfrak{H}}$ for the action of $\mathfrak{H}$ permuting the parameters. Noether's general results show there is a finite complete system for these invariants. Her (1918a, p. 226) shows that if there is such a basis with all its elements algebraically independent then there is a polynomial with Galois group $\mathfrak{H}$ over $\mathbb{Q}(\lambda_1 \ldots \lambda_h)$. By Hilbert's theorem, then, this polynomial parameterizes many polynomials with that Galois group over the rational numbers.

The general results center on fields of rational functions invariant under some group of permutations of the variables. Noether called these fields "*Lagrangesche Gattungsbereiche*" in reference to Lagrange's theorem and Kronecker's *Gattungsbereiche*.[12] But permuting variables takes polynomials to polynomials and a rational function

$$\frac{f(x_1 \ldots x_h)}{g(x_1 \ldots x_h)}$$

expressed in least terms is invariant if and only if each of $f(x_1 \ldots x_h)$ and $g(x_1 \ldots x_h)$ is invariant. So it is enough to study invariant polynomials.

Noether saw that the key fact on Galois resolvents does not use the special features of Galois field extensions. For, let a field be extended by adjoining any finite list of quantities $x_1 \ldots x_n$ with no equations imposed on the quantities. This yields the ring of polynomials over the field in these variables. Let any finite group $\mathfrak{H} = \{A_1 \ldots A_h\}$ act linearly on these polynomials in whatever way, not necessarily by permuting the variables $x_1 \ldots x_n$. An invariant polynomial is one equal to its own average over the group action, and that average is a symmetric function of the transformed quantities $x_i^{(k)} = A_k(x_i)$. The well-known theorem on symmetric functions shows that these invariants are all polynomial functions of the coefficients of any "Galois resolvent."

According to this history Noether adopts a good bit of apparatus from Weber without mentioning it. Following Weber (1899, p. 232) a primitive element in this context is any quantity $f(x_1 \ldots x_h)$ such that no non-identity element A_i of $\mathfrak{H}$ leaves it unaltered; and a Galois resolvent is a minimal polynomial for a primitive element. The Hilbert irreducibility theorem gives primitive elements by forming an indeterminate linear combination of the x_i:

$$-u_1 x_1 - \cdots - u_n x_n,$$

and each parameter u_i and can be determined in arbitrarily many ways that make the result primitive.

[12] Kronecker's term is well explained by Goldstein and Schappacher (2007, p. 82), but Noether's definition is not actually a case of it.

None of this is explicit in Noether (1916). Explicitly she formed the polynomial

$$\Phi(z, u) = \prod_{k=1}^{h} \left(z + u_1 x_1^{(k)} + \cdots + u_n x_n^{(k)}\right)$$

whose roots are h different conjugates of one indeterminate linear combination:

$$-u_1 x_1^{(k)} - \cdots - u_n x_n^{(k)}$$

She treated the $x_i^{(k)}$ as distinct variables when expanding the polynomial. Then she evaluated them in terms of the original x_i and A_k to get a polynomial

$$z^h + \sum G_{\alpha\alpha_1\cdots\alpha_n}(x) z^{\alpha} u_1^{\alpha_1} \ldots u_n^{\alpha_n} \begin{pmatrix} (\alpha + \alpha_1 + \cdots + \alpha_n) = h \\ \alpha \neq h \end{pmatrix}$$

where each $G_{\alpha\alpha_1\alpha_n}(x)$ is a polynomial in the original $x_1 \ldots x_n$, invariant under the action of $\mathfrak{H}$. Conversely, any invariant is a polynomial function of the coefficients of the resolvent $\Phi(z, u)$ for some choice of values for $u_1 \ldots u_n$. So it is certainly a polynomial in these $G_{\alpha\alpha_1\alpha_n}(x)$. The $G_{\alpha\alpha_1\alpha_n}(x)$ give a finite complete system for $\mathfrak{H}$.

On this history, Noether first treated the $x_i^{(k)}$ as formal variables for *Buchstabenrechnung* in order to expand the polynomial $\Phi(z, u)$, and then she evaluated them as field elements, but she merged these two steps without comment into one equation. She never distinguished them, and she soon abandoned the framework of *Lagrangesche Gattungsbereiche* in favor of rings and modules.

8 Noether (1916) as moving toward representation modules

The third history uses ideas that Noether had not clarified for herself in (1916) but was already reaching toward, and would soon develop.[13] Noether knew the works of Dedekind, Frobenius, and Schur cited in Fischer (1915, 1916) and Fischer named her as a collaborator on those articles. Though she would first publish the thought years later, she already knew that:

> Frobenius developed the theory of hypercomplex systems and their representations – especially the representation theory of finite groups – in a unified way. It was founded on Dedekind's concept of group determinant.... But Frobenius got his conceptually unified and transparent results by toilsome (*mühevolle*) calculations. (Noether 1929, pp. 641–642)

Her new methods would give proofs as unified and transparent as the results.

Throughout this section notation not taken from Noether (1916) is based on her (1929), especially §15 on representation modules. According to Noether (1929, pp. 680, 682) the "usual" (*übliche*) definition of a *representation* of a group $\mathfrak{H}$ over a

[13] This reading is particularly inspired by Neusel (2006).

field K correlates each element $i \in \mathfrak{H}$ with an $n \times n$ matrix A_i over K, so that group multiplication in $\mathfrak{H}$ corresponds to matrix multiplication of the A_i. Her (1916) used a common variant of this where $\mathfrak{H}$ was conceived of as a group of matrices A_i in the first place.

Noether used *representation modules* to shift attention from matrices to their action on the module (today called a *vector space*) of n-tuples of elements of K. As was to be crucial to her mature ideas, she also replaced the group $\mathfrak{H}$ by its *group ring* over K (Roquette 2002), but the group ring sheds no light on (1916) and we just mention it briefly below.

From this point of view, the quantities $x_1 \dots x_n$ in (1916) are basis elements for a module we may call V.[14] Noether's key insight in (1916) was to track the action of $\mathfrak{H}$ on V before performing it, but she failed when she tried to express this by equations. She tried to define an n-tuple of transforms $x_1^{(k)}, \dots, x_n^{(k)}$ for each A_k by equations:

$$x_i^{(k)} = \sum_{\nu=1}^{n} a_{i\,\nu}^{(k)} x_\nu \quad \text{for all } 1 \leq i \leq, \tag{2}$$

which fails. Her argument requires $x_i^{(k)} \neq x_i^{(j)}$ when $k \neq j$ while it could easily happen that the right hand sums are equal when evaluated in V.

Noether (1916) postpones evaluating the sums in V by taking the $x_i^{(k)}$ collectively as basis elements for a new module we name W. Intuitively each $x_i^{(k)}$ represents a pair (A_k, x_i) indicating that A_k is to act on x_i but, so to speak, has not acted yet. The group $\mathfrak{H}$ acts naturally on W with each A_j taking each basis element $x_i^{(k)}$ to the basis element $x_i^{(jk)}$, indicating that the product $A_j A_k$ is to act on x^i.[15]

Noether (1916) could not use distinct modules V and W because at that time she had no proper way to relate different modules. She had not adopted the idea of homomorphism which would become central to her heritage.[16] Rather than our Eq. 2 above which Noether gave unnumbered at the start of (1916) she should have defined a homomorphism $W \to V$ from W to V making each basis element $x_i^{(k)}$ of W correspond to a sum in V:

$$x_i^{(k)} \to \sum_{\nu=1}^{n} a_{i\,\nu}^{(k)} x_\nu \quad \text{for all } 1 \leq i \leq n \tag{2'}$$

[14] Connoisseurs will note the confusion between a basis element x_i and the x_i component of a vector in that basis, reproducing the confusion explicit on the Noether (1929, p. 669) for example. Noether and others slowly became more precise about this during the 1920s.

[15] Today we would say that W is the tensor product of the group ring of $\mathfrak{H}$ (with basis elements A_k) and the vector space V (with basis elements x_i) so W has a basis of formal products $A_k \otimes x_i$.

[16] Inexplicit ideas of homomorphism occur in Dedekind (McLarty 2006). Frobenius and Schur used the term "homomorphism" around 1900 (Curtis 1999, p. 155). It spread when Schur used it in three articles in 1924 all reviewed in *Zentralblatt* by Noether. She used the adjective "homomorph" in (1927a, 1927b). Krull, Schreier, and Artin took up the term and van der Waerden (1930) made it standard. As to its history in English, it first appears on JSTOR in an 1932 article by Levitzki citing Noether (1929) for the definition.

The left hand basis element $x_i^{(k)}$ of W corresponds to the right hand sum in V. We can call this the *evaluation* homomorphism from W to V.

Rather than her Eq. 1, Noether should have said a polynomial function $f(x_1 \ldots x_n)$ on V is invariant if and only if the evaluation homomorphism carries $f(x^{(k)})$ to the original $f(x)$, for all $1 \le k \le h$, which is equivalent to saying it carries the average to $f(x)$.

$$f(x^{(k)}) \to f(x) \quad \frac{1}{h}\sum_{k=1}^{h} f(x^{(k)}) \to f(x) \tag{1'}$$

Since each term $f(x^{(k)})$ in the average depends on just one series of variables $x^{(k)}$, every permutation of the series leaves it unchanged. The invariants on V are exactly the images of polynomials on W symmetric in the series $x^{(k)}$.

By the well-known theorem, polynomials symmetric in the variable series $x^{(k)}$ are all polynomial functions of the completely polarized elementary symmetric polynomials in one series. These are finite in number. Applying the evaluation homomorphism to these finitely many polynomials on W gives a finite complete invariant base for polynomials on V. This argument is rigorous by modern standards and indeed is used in Smith (1995) and Neusel (2006).

9 Noether and history

Historians have to ask which of these histories recounts Noether's thoughts. Noether did not have to ask, and did not have to know. Certainly from 1913 to 1915 she thought about symbolic calculation, and about Galois theory, and about modules and representations, and there may be no way to separate the influences. There is no reason to suppose she did.[17]

Noether was not a historical thinker.[18] She assiduously credited earlier work in every article she wrote, she was keenly interested in past figures as colleagues, that is as sources of currently valuable ideas, and she edited works and correspondence by Dedekind (Dedekind 1932; Noether and Cavaillès 1937): but she never regarded earlier mathematicians as historical subjects.

She wrote as a colleague and not as a historian when she said, for example, that her conservation theorems "rest on combining formal methods of calculus of variations with Lie's group theory" (1918b, p. 235) or that her isomorphism theorem occurs "in a somewhat more special form" in Dedekind (Noether 1927a, p. 41n). She knew these remarks wildly understate her advances. Some historians feel she was being modest,

[17] Influenced by Weyl and by the apt semi-historical account in Neusel (2006) the author began by believing something like the third history would show Noether converting from Gordan's viewpoint to Hilbert's. Close reading of Noether (1916) made the first history more plausible, while reading her other articles around that time led to the second history.

[18] Her longest historical account is (1929, pp. 641–642) on 30 years of hypercomplex systems, which she worked in for 15 of those years. She contrasted different approaches but within each one she described the accumulation of theorems and gave almost nothing on developing concepts.

but we know she was not modest. Her student and later colleague Taussky-Todd (1981, p. 84) says Noether irritated some people by bragging. Rather, she wrote these references to acknowledge colleagues she had learned from, and from whom she would learn more—albeit she never met them and they were no longer alive. She was not interested in tracking how she changed their ideas.

The great question about Noether in this period is whether and how she made the "transition from Gordan's formal standpoint to the Hilbert method of approach", as stated by Weyl (1935, p. 206). I have already said she did not. But Noether herself left little by way of a comparison of the two men: A footnote in her (1919, p. 140n) cites a "direct proof" of Hilbert's basis theorem by Gordan in 1899. Soon after that, Noether and Schmeidler (1920, pp. 3, 15) say Gordan's proof generalizes more aptly to their problem than Hilbert's. Her (1923, p. 178) calls Gordan's calculations "unsurveyably vast (*unübersehbar*)" and notes that they could not prove the finiteness theorem for classical invariants in many variables, while Hilbert's concise method could. These remarks are too thin. A serious account of Gordan's and Hilbert' influences on Noether will have to be more indirect.

10 Gordan's student and Hilbert's algebraist

Noether was keenly aware that Gordan produced only a tiny fraction of the mathematics that Hilbert was to produce, and that her dissertation under him led nowhere. However, Gordan's lasting influence on her was not in the dissertation or the particulars of his symbolic method, but in his general approach to concepts and calculations, as described below. Later, when talking among friends, she would call her dissertation "Mist," "Formelngestrupp," and "Rechnerei": that is crap, a formula-thicket, and mere reckoning.[19] By then Noether could look back at a string of articles each establishing a new field of mathematics, and conversations that she never even published but which established new methods in topology and algebraic geometry. The dissertation was poor indeed in that company. By 1932 it appears that she had completely forgotten Gordan's symbolic method,[20] which makes sense because that method is solely adapted to one problem which she had left more than 20 years before.

The key is to see that, while Noether came to define the Hilbert school in algebra, hers was not Hilbert's own algebra! Hilbert's algebra stayed close to classical structures like subfields of the complex numbers and polynomials over these fields. He used algebraic axioms to isolate the key points for each problem but not to describe new more remote structures:

> Hilbert's own use of the axiomatic method involved, by definition, an acknowledgment of the conceptual priority of the concrete entities of classical mathematics, and a desire to improve our understanding of them, rather than a drive to encourage the study of mathematical entities defined by abstract axioms devoid of immediate, intuitive significance. (Corry 1996, p. 170)

[19] The first appears in Dick (1981, p. 17), the latter two in Alexandroff (1981, p. 99).

[20] Letter to Hasse April 14, 1932 (Lemmermeyer and Roquette 2006, p. 158).

Gordan on the other hand taught that perfectly concrete calculations, notably in invariant theory, can lead far beyond the classical structures and intuitions. He made Noether a "formalist," understanding that weird but carefully adapted calculating devices can solve problems. So in (1916) she calculated boldly with the poorly explained terms $x_i^{(k)}$. Over the next few years Hilbert and Dedekind made her a "logician" able to give strict definitions and deductions. That was sadly far from Gordan's strength. When she combined those influences, she created representation modules and crossed products as calculating tools which graduate students today meet in *group cohomology*.[21] They have a reputation for being abstract, and indeed they are abstract in the sense of being remote from intuitions about classical structures. Yet they organize concrete calculations, most notably in their use in proving Fermat's Last Theorem (Washington 1997; McLarty 2010).

Gordan and Dedekind both taught that conceptual organization can "predict the results of calculations" (Dedekind 1996, p. 102). Contrary to legend Gordan never doubted Hilbert's finiteness proof for classical invariants, still less did he reject it for being merely an existence proof.[22] He was probably dismayed at first to have his life's work so roundly outdone—but he volubly confessed he had been outdone. He spent years using Hilbert's ideas to get better calculations and specified in print that he could do this only because Hilbert had seen "the value for invariant theory of certain ideas which Dedekind, Kronecker, and Weber developed for use in other parts of algebra" (Gordan 1893, pp. 132–133).

Gordan would have taught Noether that organization *is* the art of calculation, when calculation means actually getting answers and not merely knowing a way to get them in principle. Gordan's *Erlanger Programm* (1875) stresses the difficulty of improving his procedure, which works in principle for binary forms in all degrees, so as to make it feasible in practice for degrees greater than 6. The great calculator Gordan knew perhaps more viscerally than anyone else ever has how *every* procedure for solving a problem necessarily becomes infeasible for large enough cases of that problem. Conversely, he knew that organizing ideas without algorithms, as Hilbert (1889, pp. 450–452) did, may yet radically improve calculations. Gordan's work on Hilbert's proof produced the basic idea of *Groebner bases* now central to computational algebra.[23]

Noether absorbed all this and went beyond. The idea of modules as independent structures shows the influence of Hilbert and Dedekind, and often came to Noether through Weber's textbooks; but abstract algebra in the specific sense of freely forming agile new kinds of structures and morphisms unconstrained by classical intuitions yet tailored for some specific calculation is not to be found in Hilbert's work, and only a little in that of Dedekind or Weber. Indeed, it is not really in Noether before her (1927a, 1927b)—and it just begins to show in Noether (1916).

[21] See Mac Lane (1988), Basbois (2009).

[22] See fuller arguments on this in McLarty (2011).

[23] See Eisenbud (1995, p. 367). At the same time Hilbert (1893) proved the Nullstellensatz and a crucial case of Noether normalization, which with Gordan's work made the classical invariant theorem entirely constructive for n-ary forms in all degrees for all n. Even with current computers, though, the problem quickly becomes infeasible. See Sturmfels (1993).

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