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THEOLOGY AND ITS DISCONTENTS: THE ORIGIN MYTH OF MODERN MATHEMATICS

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It is a fact and no myth at all that one small puzzling proof by David Hilbert in 1888 became the paradigm of modern axiomatic mathematics. Hilbert knew it was that important. He wrote a series of papers on applications and as we now know vastly underestimated them: a preliminary series of three went into the *Göttinger Nachrichten* and a longer polished version went to the maximally prestigious *Mathematische Annalen*. He consciously made it his emblem as he became "the Director General" of 20th century mathematics in the very practical image offered by his friend Hermann Minkowski (1973, p. 130). With time the affair grew into an origin myth, a titanomachy where new gods defeat the old, and specifically Hilbert defeats one Professor Paul Gordan of Erlangen. Gordan was the "King of Invariants" for reams of calculations on "Gordan's problem," the problem he made central to the then-thriving subject of *invariant theory* in algebra, namely to find *finite complete systems of invariants for forms* as explained below. Without actually finding these systems, Hilbert proved in a few pages what many people doubted and Gordan had not proved in 20 years: they exist.

In the myth Gordan denounced Hilbert's proof and his anathema rebounded against himself when he said:

This is not Mathematics, it is Theology!

The quote first appeared a quarter century after the event, as an unexplained side comment in a eulogy to Gordan by his long-time Erlangen colleague Max Noether (1914, p. 18). Noether was a reliable witness speaking to an audience that knew Gordan well but he says little about what Gordan meant. A series of Göttingen mathematicians took it up in succeeding decades.

The Hilbert 60th birthday issue of *Die Naturwissenschaften* highlights Hilbert's invariant theorem and Gordan's response to it but never mentions theology. See especially Hilbert's first biographer Blumenthal (1922) and the algebraist Toeplitz (1922). One year later Hilbert kicked off the quote's Göttingen career with a harshly negative interpretation as part of the foundations controversy in (1923, p. 161). Klein (1926, p. 330), who could be fanciful at times, lightly embellished Noether's version. Blumenthal (1935, p. 394) read Gordan and Hilbert as actually agreeing about a certain shortcoming of the 1888 theorem. Weyl (1944, p. 622) returned to Hilbert's negative evaluation but tried to give it more plausible grounds. When the famous mathematics commentator and popularizer Eric Temple Bell—the all time leader in mathematical narrative—wrote on *The Development of Mathematics* he emphasized that "*only main trends* of the past six thousand years are considered, and these are presented *only through typical major episodes* in each." To this end he gave Gordan's quote in two different places in the first edition and added a parody of it at the end of the second.[1] Textbooks still tell the story to build excitement around this proof supposedly "denounced at the time" when Hilbert

[1](Bell, 1945, pp. vii, 227, 429, 561).

created it (Reid, 1995, p. 49). But the quote is just as exciting read in the opposite way. Gerhard Kowalewski, who studied at Königsberg while Hilbert taught there shortly after finding his proof, said:

> Whenever such a powerful discovery is made one feels that a ray of light from a higher world penetrates our earthly darkness. That must be what Gordan meant by his saying. Hilbert was blessed throughout his life with such great illuminations, more than any other mathematician.[2]

It is an unusually extended, explicit, and pointed use of narrative in mathematics. It is narrative at its barest bones: protagonist Hilbert wins against antagonist Gordan's strongest blow—except on Kowalewski's reading where protagonist Hilbert wins Gordan's strongest praise. It most often functions with no serious explanation of what either Hilbert or Gordan did or even who Gordan was, and is among the best known and most widely repeated stories in mathematics. Gordan is almost completely unknown today for anything else. It functions as a story. It registers the pure excitement of Hilbert's proof. That excitement survives historical scrutiny, and even becomes more profound, but it could never have spread so far burdened with the particulars of Hilbert's proof let alone of Gordan's contribution. No detailed version could so well build esprit de corps among Hilbert's heirs, which is the manifest intention of every author I have found giving the quote. Hilbert, his biographer Blumenbach, and his protege Weyl disagree over what actually happened. They agree among themselves, and with the quite different reading by Kowalewski, and with the surprisingly subtle Bell, on the narrative force: Hilbert far outdid the older mathematician with this astonishing proof.

Every other aspect of the story was described quite otherwise at the time. Hilbert in 1888 said he found his proof "with the stimulating help of" this very Professor Gordan (Hilbert and Klein, 1985, p. 39). Gordan consistently supported Hilbert and the proof strategy and made no objection to its initial publication even though that first version was not entirely correct. The hitch came when Hilbert sent it to the *Mathematische Annalen*. Gordan felt it was not ready for the journal of record. He wanted a clearer argument and he wanted Hilbert to develop it further. Hilbert soon did develop it just the way Gordan wanted. To that end he proved his famous *Nullstellensatz*, a central case of the *Noether normalization theorem*, and other theorems which all became basic to 20th century algebraic geometry.[3] But not before he got his 1888 idea into the *Annalen*!

The closest link of Gordan to Hilbert epitomizes the serious narrative problem here: Emmy Noether was Gordan's sole doctoral student and along with Hermann Weyl one of Hilbert's two greatest heirs. She worked on Gordan's problem for years in Hilbert's Göttingen with a framed picture of Gordan in her office. Sadly for historians this profuse conversationalist and scanty writer left just one brief purely technical footnote comparing the two men's work (Noether, 1919, p. 140). There are passionate accounts of her work by great mathematicians who knew her: Hermann Weyl (1935), Paul Alexandroff (1981), and Bartel van der Waerden

[2]Immer, wenn man eine so gewaltige Entdeckung gemacht wird, hat man das Gefühl, daß ein Lichtstrahl aus einer höheren Welt in unser irdisches Dunkel eindringt. Das wird wohl Gordan mit seiner Äusserung gemeint haben. Hilbert war sein ganzes Leben hindurch mit so großen erleuchtungen gesegnet, mehr als irgendein anderer Mathematiker. (Kowalewski, 1950, p. 25)

[3]See (Hilbert, 1993, pp. 136, 142) and Sturmfels' introduction to that book.

(1935). Historians have written terrific accounts of her life and parts of her work.[4] But efforts to capture her mathematics as a whole all shatter on the same rock. She went on farther than Hilbert using sharp abstraction to make as many things as possible utterly trivial and clear a quick path to genuinely hard problems. It is all too easy to split her work into banalities about the distributive law and crushingly sophisticated applications to things like Galois representations in number theory. This partition into the banal and the crushing makes mathematics (despite its etymology) look unlearnable—as if you can only stare at it in wonder.

Theology has proved peculiarly apt for this obfuscation, as we explore in more detail in section 5. To those who scorn Gordan it suggests angels dancing on pins. Kowalewski dissolves it into an ineffable "ray of light." Both evade the specifically mathematical wonder of Hilbert's proof and obscure Gordan's real contribution to Hilbert's work as well as Hilbert's profound originality. The real wonder of the proof goes far beyond 1888 to pervade modern mathematics. To know the depth of Emmy Noether and of modern algebra requires understanding Gordan and Hilbert as collaborators and especially the merger of Gordan's *symbolic method* with Hilbert's axiomatics in Noether's work. I take it as much the same wonder as Deligne conveys describing a characteristic Grothendieck proof as a long series of trivial steps where "nothing seems to happen, and yet at the end a highly non-trivial theorem is there" (Deligne, 1998, p. 12).

1. Gordan's problem

Paul Gordan was a very funny man, a Professor when that was a rare honor, and he traveled among the great mathematicians of Germany. They considered him good company. More than that he was a great collaborator. With Alfred Clebsch he created the Clebsch-Gordan coefficients used in spherical harmonics and especially in quantum mechanics for the eigenstates of coupled systems. With Felix Klein he did influential studies of algebraic equations. And he worked briefly with young Hilbert. Max Noether wrote of the work with Clebsch:

> Certainly Clebsch had the leading role overall but from 1864 on Gordan was a restless driving force behind him in daily uninterrupted deep conversation. He found no obstacle insurmountable and brought clarity in a socratic way.[5]

We need little of the mathematics since Hilbert's method was precisely to ignore most of the particulars. But before exploring the successive uses of the quote in section 4 we need some.

Algebra then as now studied polynomials, for example the quadratic

$$P(x) = Ax^2 + 2Bx + C$$

Nineteenth century algebraists preferred to make it *homogeneous* by adjoining a second variable y to give every term the same total degree:

$$F(x, y) = Ax^2 + 2Bxy + Cy^2$$

[4]See Brewer and Smith (1981); Tollmien (1990); Corry (1996); Kosmann-Schwarzbach (2004); Roquette (2005).

[5]Clebsch hatte zwar überall die führende Rolle, aber Gordan stand von 1864 an in täglicher ununterbrochener verstandnisvoller Aussprache hinter ihm als rastlos treibendes Element, dem keine Schwierigkeit unüberwindlich schien und das in sokratischer Weise Klarheit schuf. . . . (Noether, 1914, pp. 7–8)

A homogeneous polynomial is called a *form*. Thus $F(x, y)$ is the quadratic form in two variables. A form in one variable is just a constant multiple of a power of that variable

$$Ax^n$$

The cubic form in two variables is

$$F_3(x, y) = Ax^3 + 3Bx^2y + 3Cxy^2 + Dy^3$$

Any kind of work with the quadratic form will repeatedly refer to the quantity

$$\Delta_F = B^2 - AC$$

called the *discriminant* of $F(x, y)$. One use is familiar from high school even if not in this notation. To solve the equation

$$Ax^2 + 2Bxy + Cy^2 = 0$$

divide through by y^2 to get

$$A(\frac{x}{y})^2 + 2B(\frac{x}{y}) + C = 0$$

and apply the quadratic formula to find

$$\frac{x}{y} \;=\; \frac{-B + \sqrt{B^2 - AC}}{A} \quad \text{or} \quad \frac{-B - \sqrt{B^2 - AC}}{A}$$

The solutions are all pairs $\langle x, y\rangle$ with either of these ratios $\frac{x}{y}$. The discriminant Δ_F appears here, and obviously the two ratios coincide just when $\Delta_F = 0$.

By no coincidence, Δ_F is a form in the coefficients A, B, C. Each term has total degree two in these coefficients. But it has a much stronger property as well. We may replace x and y by linear combinations of new variables x', y':

$$\begin{aligned} x &= \alpha x' + \beta y' \\ y &= \gamma x' + \delta y' \end{aligned}$$

Here $\alpha, \beta, \gamma, \delta$ are any constants and are called the *substitution coefficients*. Then define a new form $F'(x', y')$ by:

$$F(x, y) = F(\alpha x' + \beta y', \gamma x' + \delta y') = \\ A'x'^2 + 2B'x'y' + C'y'^2 \;=\; F'(x', y')$$

Straightforward calculation shows that each single coefficient of $F'(x', y')$ depends on all three coefficients of $F'(x', y')$ by a somewhat lengthy equation:

$$\begin{aligned} A' &= \alpha^2 A + 2\alpha\gamma B + \gamma^2 C \\ B' &= \alpha\beta A + (\alpha\delta + \beta\gamma)B + \gamma\delta C \\ C' &= \beta^2 A + 2\beta\delta C + \delta^2 C \end{aligned}$$

But when you calculate the discriminant of $\Delta_{F'}$ using these new coefficients the complications largely cancel out to leave the multiple of Δ_F by a simple expression depending only on the substitution coefficients:

$$\Delta_{F'} = (\alpha\delta - \beta\gamma)^2 \cdot \Delta_F$$

This property became the definition of an *invariant* of a form: An invariant of the two-variable form $F_n(x,y)$ in any degree n is an expression I_{F_n} in the coefficients of F_n such that whenever a linear substitution turns F_n into a corresponding F'_n:

$$F_n(x,y) \;=\; F_n(\alpha x' + \beta y', \gamma x' + \delta y') \;=\; F'_n(x',y')$$

then the invariant is multiplied by some power of that expression in the substitution coefficients:

$$I_{F'_n} = (\alpha\delta - \beta\gamma)^m \cdot I_{F_n}$$

The quadratic form

$$F(x,y) = Ax^2 + 2Bxy + Cy^2$$

has infinitely many invariants but all are powers of the discriminant. That is, for any invariant I of this form, there is some natural number k such that

$$I = \Delta_F^k$$

In this sense the discriminant itself is a *complete system* of invariants for the quadratic form.

The degree four form

$$F_4(x,y) = Ax^4 + 4Bx^3y + 6Cx^2y^2 + 4Dxy^3 + Ey^4$$

also has infinitely many invariants, including:

$$i_{F_4} = AE - 4BD + 3C^2$$

$$j_{F_4} = ACE + 2BCD - C^3 - B^2E - AD^2$$

One page or so of straightforward calculation will verify that when a change of variable as above turns $F_4(x,y)$ into $F'_4(x',y')$ then

$$i_{F'_4} = (\alpha\delta - \beta\gamma)^4 \cdot i_{F_4} \quad \text{and} \quad j_{F'_4} = (\alpha\delta - \beta\gamma)^6 \cdot j_{F_4}$$

Long arcane calculations show that every invariant of $F_4(x,y)$ is a sum of products of powers of these. That is, the invariants $\{i_{F_4}, j_{F_4}\}$ make a complete system for the degree four form. For example the degree four discriminant Δ_{P_4} takes several lines to write in terms of the coefficients $A \ldots E$ but is neatly expressed as:

$$\Delta_{P_4} = 4i_{P_4}^3 - j_{P_4}^2$$

The matter grows rapidly more complicated in higher degrees. By fantastically long and not at all routine calculations Gordan found a way to produce a finite *complete system* of invariants for the homogenous form in two variables of any degree. That is, for each n Gordan's routine would yield a finite list of invariants of $F_n(x,y)$ such that every invariant of $F_n(x,y)$ is a sum of products of powers of these. Actually he found more, namely a finite complete system for the *covariants* which include the invariants plus analogous expressions involving the variables x, y.

Gordan used and improved the *symbolic method*. Consider the quadratic form $F(x,y)$. The method creates symbols α_0, α_1 as if it is the square of a linear form:

$$(\alpha_0 x + \alpha_1 y)^2 \;=\; Ax^2 + 2Bxy + Cy^2 \tag{1}$$

So the symbols α_0, α_1 are linked to the actual coefficients by:

$$\alpha_0^2 = A \qquad \alpha_0\alpha_1 = B \qquad \alpha_1^2 = C \tag{2}$$

This factorization simplifies the theory of invariants but on its face simplifies it far too much as it seems to imply that every discriminant is 0:

$$\Delta_F \;=\; B^2 - AC \;=\; (\alpha_0\alpha_1)^2 - \alpha_0^2\alpha_1^2 \;=\; 0$$

The whole point of the method was to use elementary algebra like

$$(\alpha_0\alpha_1)^2 = \alpha_0^2\alpha_1^2$$

There was no question of rejecting that equation. But Gordan and others blocked the unwanted equation by calling those terms *purely symbolic*, with no *actual meaning*, and refusing to put them in equations with actual coefficients. These terms have α to total degree four. In the symbolic method, a term in α has actual meaning comparable to the *actual* coefficients A, B, C only if it has α to degree exactly two. That is, calculations will use α in any degree but conclusions about the actual coefficients A, B, C can only use the powers $\alpha_0^2, \alpha_0\alpha_1, \alpha_1^2$. And this worked.

By these means Gordan (1868) found a finite complete system of invariants for every homogeneous form in two variables

$$P_n(x,y) = A_n x^n + A_{n-1}x^{n-1}y + A_{n-2}x^{n-2}y^2 + \cdots + A_0 y^n$$

More accurately, he gave a routine for finding such systems. His original routine was completely infeasible for forms of degree above 6. Over a decade he improved it so that even in degree 8 "if the system cannot actually be written out it can at least be closely described" (Gordan, 1875, p. 1). The symbolic method applies to any number of variables and Gordan tried to extend his finiteness theorem to any number of variables. Apart from a few special cases he made no serious progress until he met Hilbert (White, 1899).

Gordan's symbolic method has a common requirement with Noether's later algebra, namely that you *must* not ask about the concrete meaning of a calculation at the wrong time. In manipulating Gordan's bracket functions, or Noether's crossed product modules, if you try to keep track of what it all means in terms of actual polynomials you will be absolutely lost in irrelevant complicated details. Only key points of the calculation are put back into those terms. Gordan emphasizes that one of his key symbolic operations, called *Faltung*, has no non-symbolic meaning at all (Gordan, 1885–1887, vol. 2, p. 10).[6]

2. Hilbert's theorem

Hilbert addressed invariant theory in his (1885) Inaugural dissertation, and his (1887) shows he meant to revolutionize the field by several new methods that play no part in the 1888 proof but would reappear to some extent in Hilbert's (1891–92; 1893) response to Gordan's criticism.[7] By that time Hilbert's results plus further ones by Gordan would solve Gordan's problem. They would give a routine producing a finite complete system of covariants for a form of any degree in any number of variables—though it is apparently still true that no one has actually worked through it even for the ternary quartic that Emmy Noether would take as

[6]Different symbolic expressions of a single polynomial can give different results by *Faltung*. Every symbolic term can be gotten by *Faltung* of terms that express the 0 polynomial.

[7]These are notably differential methods related to the Lie-Klein theory of continuous groups which later served Hilbert as a framework for invariant theory; and irrational invariants closely tied to his discovery and use of the Nullstellensatz.

her dissertation topic.[8] In 1888 Hilbert found a far simpler and much more general result.

On Klein's advice Hilbert had visited Gordan, and wrote to Klein:

> With the stimulating help of Professor Gordan, meanwhile, an infinite series of brain-waves has occurred to me. In particular we believe I have a masterful, short, and to-the-point proof of the finiteness of complete systems for homogeneous polynomials in two variables. (Hilbert and Klein, 1985, p. 39)

Hilbert quickly extended his proof to any finite number of variables. It rested on the uncannily simple :

> Theorem I: For any infinite series
>
> $$\phi_1, \phi_2, \phi_3 \ldots$$
>
> of forms in n variables $x_1, x_2, \ldots, x_n$, there is some number m such that every polynomial in the series can be put in the form
>
> $$\phi = \alpha_1\phi_1 + \alpha_2\phi_2 + \cdots + \alpha_m\phi_m$$
>
> with $\alpha_1, \alpha_2, \ldots, \alpha_m$ suitable forms in the same variables. (Hilbert, 1888–1889, p. 450)

This was incredible. We are familiar with finiteness results all over mathematics but in 1888 this was not easy for anyone to understand including Hilbert. How can the first m terms determine all the rest of an arbitrary infinite series? Of course that puts it backwards. The theorem says each series taken as a whole determines an m. Even so, it remains amazing that every, arbitrary, infinite series is compounded out of some finite part of itself. Plus it was intuitively clear, and is provable today using the concept of recursive algorithm, that you cannot expect to find these bounds m in general.[9]

We know the theorem was hard for Hilbert to understand because, even after Gordan pressed him on it in Leipzig, he published an elaborate incorrect proof. He used a double induction between Theorem I and a companion:

> Theorem II: For any r infinite series
>
> $$\begin{gathered}\phi_1, \phi_2, \phi_3 \ldots \\ \psi_1, \psi_2, \psi_3 \ldots \\ \ldots \\ \rho_1, \rho_2, \rho_3 \ldots\end{gathered}$$
>
> of forms in n variables $x_1, x_2, \ldots, x_n$, there is some number m such that for each index k there is a solution to the system of equations
>
> $$\begin{aligned}\phi_k &= \alpha_1\phi_1 + \alpha_2\phi_2 + \alpha_m\phi_m \\ \psi_k &= \alpha_1\psi_1 + \alpha_2\psi_2 + \alpha_m\psi_m\end{aligned}$$

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[8] Kung and Rota (1984, p. 30). Noether calculated one set of 20 covariants and one of 331 such that *Faltung* of them would produce a complete system. That would be a finite calculation but she gave no estimate of how it would be. Experience suggests it would be humanly impossible.

[9] For each Turing machine take the series with $F_n = x^2$ if the machine does not halt by step n on input 0, and $F_n = x$ if it does. If it never halts on input 0 then $m = 1$ suffices since every $F = F_1 = x^2$. Otherwise m must be at least the step on which it halts so that $F_m = x$. To find m is solve the halting problem.

$$\dots$$

$$\rho_k \;=\; \alpha_1\rho_1 + \alpha_2\rho_2 + \alpha_m\rho_m$$

with $\alpha_1, \alpha_2, \dots, \alpha_m$ suitable forms in the same variables.

By free use of "one sees easily" and "appropriate" Hilbert gives a false derivation of Theorem II for n variables from Theorem I for the same number. The editors note that Theorem II is not even true as stated, but requires "certain dependencies among the degrees of $\phi_k, \psi_k, \dots \rho_k$" which Hilbert nowhere gives. Then he proves Theorem I for $n{+}1$ variables from Theorem II for n. The editors note that the latter proof can assume that for each k the degrees of $\phi_k, \psi_k, \dots \rho_k$ are each one less than the one before, which is also sufficient to prove Theorem I (Hilbert, 1888–1889, p. 451). This works but a different fix discussed in section 2.1 probably reflects Hilbert's thought. It is not a technically hard argument by standards of the time but the result was so unlooked for and the method so swift and elegant that it was very hard to follow—or even for Hilbert to get right.

Hilbert knew it was hard to follow, so that in lectures and in Hilbert (1888–1889) he gave first a simple proof for the case of one variable. A form in one variable is just a monomial and you only need to find the form of lowest degree in the series. Then he proved the case of two variables by an argument roughly parallel to the case of inhomogeneous polynomials in one variable familiar today. He noted that such a proof for three variables meets difficulties which "would only increase" in more variables (Hilbert, 1993, p. 128). Only then did he give a general induction, which was still nothing like as nicely organized as the Hilbert Basis theorem has been since Emmy Noether. Gordan (1893, p. 132) was the first to call it "Hilbert's theorem."

This result ignores everything about covariants except that they are forms, and does not itself show a form has a finite complete set of covariants. It immediately shows every form P has a finite set of covariants

$$i_1, \dots, i_m$$

such that every covariant i is a sum

$$i \;=\; a_1 i_1 + \dots + a_m i_m$$

with $a_1, \dots, a_m$ suitable forms in the same variables which need not be covariants. But a well-known averaging process proved the a_k can be replaced by covariants. These in turn are sums of multiples of the i_k. The degrees drop each time, so by induction i equals some polynomial combination of $i_1, \dots, i_m$. In other words $\{i_1, \dots, i_m\}$ is a finite complete set of covariants for P.

Three journal pages outdo Gordan's twenty year career (Hilbert, 1888–1889, pp. 450–52).[10] More precisely Hilbert does not claim to find the finite complete systems. As Noether (1914, p. 18) notes, the symbolic method continued to dominate efforts to find actual systems for decades. But Hilbert isolated finiteness *per se* as the key problem and he swept that problem away.

[10] Not to mention that another quick argument shows that all syzygies, that is all linear equations among $i_1, \dots, i_m$, are sums of a finite number of them. Weyl (1944) argues persuasively that Hilbert found this result before the finiteness of covariants, and (Meyer, 1892, p. 149) says Hilbert was the first to find it.

2.1. **Complicated caveats.** Hilbert's Theorem I corresponds to today's *Hilbert basis theorem* with three caveats. First, the modern theorem does not apply directly to homogeneous polynomials.[11] But this may actually explain the mistake in (Hilbert, 1888–1889, p. 451). Probably Hilbert convinced himself of his Theorems I and II by neglecting degrees—in effect dropping the requirement of homogeneity, leaving a simpler but still astounding result—and just assuming the degrees would work out. He can fairly say "one sees easily" how "appropriate" choices complete the intertwined proofs of the Theorems for that case. Again, though, as the editors of the *Annalen* say, Theorem II is false as stated. The degrees *do not* work out unless the degrees of $\phi_k, \psi_k, \ldots \rho_k$ are suitably related.

Even if we drop the homogeneity requirement, it remains that Hilbert states Theorem I only for polynomial rings. Conventions of the time implied these were polynomials with real or complex coefficients although many good mathematicians (certainly including Hilbert) knew they could be more general than that (they could lie in any algebraic number field, say, or complex function field). The Hilbert basis theorem today is stated for any ring finitely generated over any Noetherian ring.

The third caveat bears directly on constructive proof versus pure existence proof, and thus on all of the Göttingen re-constructions of Gordan's response to Hilbert. Hilbert stated his Theorems I and II for infinite series of forms, rather than for arbitrary sets of them. He knew that this restriction to countable sets created a problem for his applications to uncountable sets of (real or complex) forms. He took the trouble to make the applications countable by noting in somewhat vague terms that the set of all (real or complex) forms in a fixed list $a_0, \ldots, a_n$ of variables is countably generated:

> They clearly form a countable set, if we first select only the linearly independent ones. (Hilbert, 1993, p. 131)

Yet he knew as early as (1890, p. 203) that his Theorems hold for arbitrary sets of forms. He preferred to use series of forms for two reasons. First, at this time he considered series more concrete than arbitrary sets because he thought every actual series is

> ordered in some way, according to some given rule (Hilbert, 1993, p. 126)

Second he believed, what we do not today, that he could avoid using proof by contradiction by restricting to the countable case.

He was explicit that his proof of Theorem I for uncountable sets used contradiction (Hilbert, 1890, p. 203f.). It assumes there is some set of polynomials with no finite set generating it, derives a contradiction, and concludes that every set of polynomials must have a finite generating set. It is not constructive, in that it does not construct actual solutions. Today we say the countable case is not constructive either, even when the series of forms is given by a computable rule, as shown in footnote 2. But Hilbert did not see that.

Hilbert and Gordan both routinely gave instructions such as, given an infinite series of forms, "find the form of lowest degree." They felt this would be easy to do in practice in typical cases. Yet it requires an infinite search with no finite test to tell when it is done (except in special cases where some other information is

[11]Homogeneous polynomials do not form rings or modules since the sum of two homogeneous polynomials is not homogenous unless the summands have the same degree.

available). This is precisely the obstacle to finding a solution m in the footnote 2 case. Hilbert did not see any appeal to contradiction in his proof for a series of forms, although he knew the proof fell short of finding actual solutions. We return to this point below in the next section.

3. Gordan and the development of Hilbert's invariant theorem

Gordan refereed Hilbert's fuller version of the invariant theorem for the *Mathematische Annalen*:

> Sadly I must say I am very unsatisfied with it. The claims are indeed quite important and correct, so my criticism does not point at them. Rather it relates to the proof of the fundamental theorem which does not measure up to the most modest demands one makes of a mathematical proof. It is not enough that the author make the matter clear to himself. One demands that he build a proof following secure rules.... Hilbert disdains to lay out his thoughts by formal rules; he thinks it is enough if no one can contradict his proof, and then all is in order. He teaches no one anything that way. I can only learn what is made as clear to me as one times one is one. I told him in Leipzig that his reasoning did not tell me anything. He maintained that the importance and correctness of his theorems was enough. It may be so for the initial discovery, but not for a detailed article in the *Annalen*. (Hilbert and Klein, 1985, p. 65).

Hilbert saw the report and complained sharply to Klein. Klein accepted the paper which became Hilbert (1890) and also wrote back:

> Gordan has spent 8 days here.... I have to tell you his thinking about your work is quite different from what might appear from the letter reported to me. His overall judgment is so entirely favorable that you could not wish for better. Granted he recommends more organized presentation with short paragraphs following one another so that each within itself brings some smaller problem to a full conclusion. (Hilbert and Klein, 1985, p. 66)

Gordan complains that instead of giving a proof Hilbert only feels no one can contradict him—and indeed Gordan does not want to contradict him since Gordan too believes the result is *true*. Is this an obscure way of complaining that Hilbert used proof by contradiction? I don't think so. Gordan and most of his contemporaries were far too quick with their reasoning to notice the difference between a statement and its double negation. Without that distinction you cannot sharply distinguish proof by contradiction from direct proof. And published proofs at the time are often unclear on that very distinction. As noted in section 2.1, Hilbert was clear about it in principle but was not reliable at identifying proofs by contradiction in fact. Neither Gordan nor Hilbert was at all troubled by instructions like: search through an infinite series for the term of lowest degree. That looked easy enough to do in most practical situations—although it is actually the problem which footnote 2 proves unsolvable.

Apparently Gordan meant just what he said: the proof was not clear to him. When he published his own version he added that Hilbert's ideas offered more help with calculating specific systems than Hilbert had bothered to use:

> The proof Hilbert has given is entirely correct in substance; yet I feel a gap in his explication as he is satisfied to prove the existence of [solutions] without discussing their properties. To repair this gap I give a somewhat different proof with the explicit remark that I would not have succeeded at finding it had not Hilbert shown the value for invariant theory of certain ideas which Dedekind, Kronecker, and Weber developed for use in other parts of algebra. (Gordan, 1893, pp.132–3)

Klein wrote reasonably to Hilbert "So Gordan makes peace with the new development. This was no small thing for him, and for that reason he deserves a lot of credit" (Hilbert and Klein, 1985, p. 86).

Hilbert also made great progress on actually finding the systems of invariants in Hilbert (1893). He created the basics of modern algebraic geometry in order to do this, most famously his *Nullstellensatz* (Hilbert, 1993, Sturmfels' Introduction). A few years later Hilbert would say that knowing how many basic invariants a form has is not enough "as it is even more important to know about the in- and covariants themselves," but that need not mean knowing what they are in detail, since the sheer uninteresting complexity makes "actually calculating the invariants ... pointless" for higher degree polynomials (Hilbert, 1993, pp. 61 and 134).

That was a sharp difference from Gordan. While Gordan knew better than anyone that calculations above degree 6 were hopelessly impractical he would never call them pointless. He worked to make Hilbert's insights extend the feasible range and (as Max Noether said) his key contribution was explicit ways of ordering polynomials for the calculations. In effect he created the *Groebner bases* now basic to computational algebra (Eisenbud, 1995, p. 367). These bases together with Hilbert's (1893) methods made the invariant theorem entirely constructive. Even with computers, though, no one has yet made it feasible for degrees more than one or two higher than Gordan handled (Sturmfels, 1993).

4. The mythic quote

The mythic quote first appeared in print 25 years later in a eulogy to Gordan by his close friend Max Noether, who emphasized Gordan's sense of humor, and who is a reliable witness:

> Gordan—at first rather rejecting of this conceptual argument: "This is not Mathematics, it is Theology!'—twice gave closer treatment to Hilbert's finiteness theorem which is the basis of the proof. He used various criteria to order the given forms F so that they clearly produce a finite module. First he did this in a complicated way specific to invariants, and then in a simple general way. (Noether, 1914, p. 18)[12]

Noether's use of the word "conceptual" places the remark in a context familiar in Göttingen at the time. Göttingen mathematicians credited Dirichlet and Riemann

[12]"Rather rejecting" translates the German "*gegenüber mehr ablehnend*."

with a new *conceptual* working style. Minkowski took it as the starting point of modern mathematics:

> The modern age of mathematics dates from the other *Dirichlet principle*, namely to overcome problems by a maximum of insightful thought and a minimum of blind calculation.[13]

But Noether gives no more explanation of what Gordan meant.

Hilbert was the first to link the Gordan quote to foundations. As he was just beginning to formulate his own proof theoretic *program* to justify transfinite mathematics Hilbert wrote:

> P. Gordan had a certain unclear feeling of the transfinite methods in my first invariant proof [i.e. of the finiteness of complete systems] which he expressed by calling the proof "theological." He altered the presentation of my proof by bringing in his symbolic method and thought he thereby stripped off its "theological" character. In truth the transfinite reasoning was only hidden behind the formalism. (Hilbert, 1923, p. 161)

But in plain, published fact Gordan did not use the symbolic method in his work on Hilbert's proof.[14] And Gordan never spoke for finitism. He apparently considered the matter in practical terms, the way most people did at the time, as it was put in a textbook a few years later: Hilbert's 1888 method "gives practically no information as to the actual determination of the finite system whose existence it establishes" (Grace and Young, 1903, p. 169). But *theology* deals with the infinite so Gordan's words suited Hilbert's reading.

Klein repeats Max Noether rather closely but adds color:

> Gordan rejected [the proof] at the start: "This is not Mathematics, it is Theology!" Later he said "I have convinced myself that even Theology has its advantages." In fact he later later simplified Hilbert's basic theorem himself.[15]

Blumenthal (1935) tries to make Hilbert and Gordan agree:

> The finiteness proof for invariant systems had a gap which Gordan's criticism especially stressed. "This is not Mathematics," he said, "this is Theology." Hilbert himself put it this way: "[The theorem] gives us absolutely [*durchaus*] no means of exhibiting such systems of invariants by a finite number of steps which can be laid out at the start of the calculation."[16]

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[13]von dem anderen DIRICHLETschen Prinzipe, mit einem Minimum an blinder Rechnung, einem Maximum an sehenden Gedanken die Probleme zu zwingen, datiert die Neuzeit in der Geschichte der Mathematik. (Minkowski, 1905, p. 163)

[14]Gordan (1893, 1899, 1900)

[15]Gordan war anfangs ablehnend: "Das ist nicht Mathematik, das ist Theologie." Später sagte er dann wohl: "Ich habe mich überzeugt, daß auch die Theologie ihre Vorzüge hat." In der Tat hat er den Beweis des Hilbertschen Grundtheorems selbst später sehr vereinfacht. (Klein, 1926, pp. 330–31)

[16]Der Beweis für die Endlichkeit des Invariantensystems wies noch eine Lücke auf, die besonders Gordans Kritik herausgefordert hatte. "Das ist keine Mathematik," sagte er, "das ist Theologie." Hilbert drückt sich darüber selbst folgendermaßen aus: "(Er) gibt durchaus kein Mittel in die Hand, ein solches System von Invarianten durch eine endliche Anzahl schon vor Beginn der Rechnung übersehbarer Prozesse aufzustellen. (Blumenthal, 1935, pp. 394–95) The Hilbert quote is from (Hilbert, 1891–92, p. 12).

Weyl (1944, p. 622) keeps Hilbert's harsh evaluation but on new grounds. He drops the transfinite as a theme since it just does not appear in any of Gordan's work. He turns to *existential arguments*, that is arguments to show something exists without actually finding it, as Hilbert's 1888 proof did for finite complete systems of invariants:

> When Hilbert published his proof, . . . Gordan the formalist, at that time looked upon as the king of invariants, cried out: "This is not mathematics, it is theology!" Hilbert remonstrated then, as he did all his life, against the disparagement of existential arguments as "theology," but we see how, by digging deeper, he was able to meet Gordan's constructive demands.

Weyl used the term "formalist" in an already-outdated sense. By the time he wrote this Brouwer had defined "formalism" as mathematics freely using classical formal logic and the transfinite without regard for intuition (Brouwer, 1912). Weyl used it here in the nineteenth century sense of mathematicians who seek *formulas* and *algorithms* (Klein, 1894, p. 2). In fact Weyl's account is anachronistic in several ways. Gordan responded to Hilbert's proof well before it was published. He was the first person to hear about it, probably over beer with Hilbert. And Hilbert did not remonstrate over existential arguments in his first published response to the Gordan quote but rather over the infinite as we just saw (Hilbert, 1923, p. 161). Hilbert had begun treating constructivism as an explicit issue by then (Sieg, 1999, pp. 27ff.) but he did not associate it with Gordon on theology.

The serious anachronism though, which makes Weyl's account unacceptable as history, is to read 1920s constructivism into 1890s Gordan. Certainly "[Gordan] was an *algorithmiker*" (Noether, 1914, p. 37). But there is no evidence that he rejected other mathematics. And *algorithm* then did not mean what it does now. Meyer (1892, p. 187) aptly calls Gordan's method an *algorismus* meaning a framework for formal calculation.[17] It is not a specific calculational routine and so not an "algorithm" in our sense today. Our sense today was established only after the 1930s. According to the Theseus Logic, Inc., website, "The term algorithm was not, apparently, a commonly used mathematical term in America or Europe before Markov, a Russian, introduced it. None of the other investigators, Herbrand and Gödel, Post, Turing or Church used the term. The term however caught on very quickly in the computing community." Gordan liked setting up a good framework for calculation. He was good at it. There is no evidence that he thought all mathematics should be constructive.

On the other hand, when Kowalewski reads Gordan as calling Hilbert's proof a "ray of light from a higher world," this has more to do with the kindly enthusiastic Kowalewski than with Gordan (Kowalewski, 1950, p. 25).

5. The stakes in theology

Basically Gordan's line on theology, sharply excerpted by Noether, supported so many interpretations because it did not say at all what it meant. Gordan seems to have been joking, and rather generously joking given what a blow the first three pages of Hilbert (1888–1889) had to be to him. We have one other piece of

[17]Meyer (p. 100) says, what seems to be more or less true, that the *Mathematischen Annalen* was founded to accommodate Clebsch, Gordan, and others on invariant theory.

theology from him. Today we usually take modular functions to be defined on the upper half complex plane. Klein took them as defined on the whole complex plane, with terrible singularities all over the real axis: "the demons live there, as Gordan says" (Klein, 1926, p. 47). Gordan was a funny man.

The myth has carried so well, though, because *theology* lends itself to mythification. The most widely influential source of the story (incontestably so in English prior to Constance Reid (1970)) was Eric Temple Bell (1945) who tells the story three times with rising urgency and plays on all the associations of theology.[18] First he has Gordan "exclaim" in "protest" and the reason is:

> A proof in theology, it may be recalled, usually demonstrates the existence of some entity without exhibiting the entity or providing any method for doing so in a finite number of humanly performable operations. (Bell, 1945, pp. 227-8)

Then he has the "exasperated" Gordan "cry out" in "distress," and calls Gordan "prophetic," because

> Theologians are not noted for their tolerance of one another's creed, as was demonstrated once more in the half-century of mathematics following Hilbert's proof of his basis theorem [i.e. the theorem we have followed–CM]. (Bell, 1945, pp. 429-30)

Finally, in the second edition, Bell contrasts finitists to intuitionists who admit some infinity and on this ground he allies intuitionism to theology:

> The strict finitist rejects the infinite as a pernicious futility inherited from outmoded philosophies and confused theologies; he can get as far as he likes without it.
>
> . . . we may allow ourselves one of the few anecdotes in this book. It echoes Gordan's outraged cry when he read Hilbert's finiteness proof in the algebra of quantics. A devout intuitionist closed his New Testament after reading *The Gospel according to Saint John* for the first time in his life with the ecstatic whoop, "This is not theology, it is mathematics!" (Bell, 1945, p. 561)

There is no identifiable referent for this story. Bell leans heavily on *anekdota* as meaning *not published*. The intuitionist is not Kronecker with his God-created integers since Bell has just cited Kronecker as a finitist critic of intuitionism. The only plausible candidate is Brouwer but no one else links Brouwer to the Gospel according to John. More likely it is a fictitious paradigmatic intuitionist.

The point is that precision is not the point. Theology is exciting. It is about unseen existence. It is about the infinite. It has passionate, even ecstatic arguments. I do not mean theology never aims at precision. I mean this appropriation of it aims elsewhere—and has different aims in different versions.

Bell connects this bit of theology directly to another:

> The two most aggressive factions of mathematical theologians—in Gordan's sense—of the 1930's, the abstract-algebraist and the topologist, found much to dispute. According to an expert observer bulletining from the front in 1939, "In these days the angel

[18]Bell (1937) is famous for stereotyping, romantic inaccuracy, and its inspiring influence on many young mathematicians. His (1945) would be a great contribution to the history of 20th century mathematics if more people read it.

> of topology and the devil of abstract algebra fight for the soul of each individual mathematical domain." ... May the better angel win, if anything is to be won. (Bell, 1945, p. 430)

The expert is Hermann Weyl (1939, p. 500) and clearly his angel is Luitzen Brouwer while his devil is Emmy Noether. It is a thrilling image and no one can deny the expert mathematical passion Weyl put into it. But it is not history and in the long run the very idea of a rivalry between algebra and topology could only hold up mathematical understanding and progress.

Brouwer and Noether were friends and shared key students notably Paul Alexandroff (McLarty, 2006). That synergy led to group-theoretic algebraic topology and all the modern cohomology theories. Solomon Lefschetz briefly disdained algebra in his topology but then took it up and he commissioned the first joint paper by Samuel Eilenberg and Saunders Mac Lane in the series that led to the creation of category theory (Eilenberg and Mac Lane, 1942). Early 20th century topologists and algebraists generally saw each other as allies creating the new mathematics—except precisely for Hermann Weyl whose geometrical sense drew him to topology and alienated him from algebra.

We come back to Emmy Noether. She was in the most obvious sense a joint heir of Gordan and Hilbert. And she passionately sought to unify all mathematics in an algebraic axiomatic way. Corry has shown how Hilbert's axiomatics are never purely formal, nor even aim to found new subjects but always "aim to better define and understand *existing* mathematical and scientific theories" (1996, p. 162). He aimed to organize classical subjects by paring each problem down to its stark essentials. For that very reason his axioms always have reference. They refer to the classical structures that motivate them. Gordan's algebra, on the other hand, was in his own terms "purely symbolic" so that "no meaning can be assigned to it (*Es kann ihm keine Bedeutung unterlegt werden*" (Gordan, 1885–1887, vol. 2, p. 10). Noether's axiomatics combined the two.[19] Her axioms create new subjects. They need not have classical referents. They are generally taken to have no specific referent, and sometimes understood to create new referents for themselves. But there is no use grappling with those conceptual ontological issues until we can make it as clear as one times one equals one how all of this *is* Mathematics.

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[19] Another topic is how she synthesized the Lie-Klein sense of invariance and symmetry with this algebraic axiomatics. She combined the whole Erlangen-Göttingen nexus of Lie, Klein, Gordan, and Hilbert. Richard Dedekind is omitted here only because his influence on Noether is so utterly pervasive and already well documented.

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