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ABOUT THE COVER: INVARIANTS

YURI TSCHINKEL

Consider the linear space of homogeneous polynomials of degree d in two variables x and y with complex coefficients,

$$V = \{\sum_{i=0}^{d} a_{i,d-i}x^i y^{d-i} \ \mid\ a_{i,d-i} \in \mathbb{C}\}.$$

It carries a natural action of $G = SL_2(\mathbb{C})$ via linear substitutions of the variables. In 1868 P. Gordan proved that the algebra $\mathbb{C}[V]^G$ of G-invariant polynomials on V has a finite basis and that the module of algebraic relations between the generators (called *syzygies*[1]) is also finitely generated, and he provided explicit upper bounds for the degrees of the generators and relations. Previously, Cayley (1821-1895) had solved the problem for forms of degree 3 and 4 in two variables.

The generalization of this result to forms in several variables was one of the main challenges of invariant theory in the 19th century. Hilbert learned about this problem from Lindeman, his thesis adviser ([3], p. 1). In the introduction to his doctoral thesis he already distinguishes between *general methods* in deriving invariants and *applications* to specific problems, e.g., to spherical functions in the case at hand. Further inspiration came from conversations with Hermite ([2], p. 9) in Paris, Kronecker in Berlin ([2], p. 38) and Gordan himself.[2]

On September 6, 1888, Hilbert sent Klein his first groundbreaking paper, which not only solved the main problem of invariant theory but led to major advances in algebra, laying the foundation for the theory of fields, rings and modules ("...the sketched methods are not without power..." [2], p. 41). Three months later he adds:

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[1] Attributed to Sylvester; see [4], p. 189. In Cayley, "A second memoir upon quantics" (1856), one finds: *... integrals not connected by a linear equation or syzygy (such as* $\lambda P + ... = 0$*) are said to be asyzygetic.* Hilbert uses the term *abgeleitetes Gleichungssystem* (derived system of equations) and applies the word *syzygy* only in the context of invariant theory.

[2] "With the encouraging help of Prof. Gordan ..." ([2], p. 39).

"... I would like to have my recently submitted paper viewed only as a preliminary report; of course, I will most gratefully accept any suggestions regarding editorial changes, as you advised. Only I would like, as much as possible, to avoid a delay in publication" ([2], p. 43).

Around this time, Klein began to solicit Hilbert's opinion on submissions to the *Mathematische Annalen*. Here are excerpts from their correspondence regarding a paper by Cayley. Following a request by Klein ("inform me what you think about it"), Hilbert replied that the main result was wrong, that the paper proved at most a "trivial theorem," and that even this theorem "has been demonstrated much better and more completely by Clebsch, Hermite, Gordan, Brioschi.... Hopefully you can convince Cayley of his obvious error" ([2], p. 47). After confirming Hilbert's judgment with Gordan, Klein reports offering Cayley a choice between "retracting his note" and printing it "with your objections immediately afterward," to which Cayley, "with the stubbornness of the old gentleman, chose the second alternative" ([2], p. 50). Hilbert, citing Cayley's amiability toward him (and Lindemann's unequivocal advice), decides to "leave the refutation of Cayley's note to others," adding: "I also believe that Cayley has not kept a copy of his manuscript and hence no longer knows exactly what is in it. As soon as he sees his manuscript in print, he will himself realize that he has made a mistake" ([2], p. 51). Cayley's paper appeared the same year in *Mathematische Annalen*, followed by a refutation by the Danish mathematician J. Petersen the following year ([2], p. 50).

Meanwhile, Hilbert's own paper was meeting with disapproval on different grounds. Gordan wrote to Klein ([2], p. 65):

> *You ask for my view of Hilbert's paper.*
>
> *Unfortunately, I must say that I am very dissatisfied with it. It is very important and also correct, so on that count I have no reproach. The reproach relates rather to the proof of his fundamental theorem, which does not meet even the most moderate expectations which one has for mathematical proofs. It is by no means enough that the author makes the matter clear to himself, rather one wishes that he construct the proof according to definite rules.*
>
> *... Hilbert has spurned developing his thoughts out of each other according to formal rules; he thinks it suffices that no one contradict his proof, then everything would be in order. With this he cannot teach anyone; I can only learn that which is made as clear to me as the rules of one times one.... He counts on the importance and correctness to suffice. This may hold for the first discovery but for a detailed* Annalen *paper it is not enough.*

Gordan's main objection (*definite rules*) was that the proof was not *constructive*; i.e., no effective algorithm for the computation of the invariants was presented, in sharp departure from all previous approaches, including Gordan's work on binary forms.

This letter reaches Hilbert via Hurwitz and strikes him as "extraordinarily harsh" ([2], p. 63). His defense to Klein ends as follows:

> *Incidentally the accused proof should be its own advocate, and I myself take comfort in the fact that it is altogether impossible for a mortal to put words together in such a way that not even a dot on an* i *can be displaced.*
>
> *As readily as I otherwise accept advice with pleasure and without any oversensitivity, in this case I am not in a position to modify or retract any part, and for*

this paper that is - said in all humility - my last word, as long as no specific and unanswerable objection is raised to my method of reasoning.

This was not the last word from the 28-year-old Hilbert. The problem of an *effective* computation of invariants continued to occupy him. In 1892 he sent another paper to Klein:

I have insured the manuscript so highly because it would take me unspeakable effort to reproduce it in case of loss; the rough draft consists of a heap of illegible scraps of paper and marginal notes through whose perpetual revision I have dictated the manuscript to my fiancee.

(He marries her 2 weeks later.) He adds ([2], p. 85):

I have carefully looked through and thought through the manuscript one more time and confess that I myself am very pleased with the paper.

In fact, while introducing many important concepts and proving fundamental results such as the Hilbert Nullstellensatz, he did not quite achieve his main goal of bounding explicitly the degrees of the generators of the invariant algebra $\mathbb{C}[V]^G$; the last step uses a non-constructive argument. A constructive proof for general connected and semi-simple groups G was first completed in 1981 ([5]; see also [1], [6]).

The cover of this issue of the *Bulletin* shows Minkowski's letter to Hilbert, written in Bonn on February 9, 1892 ([8], p. 45):

That it could only be a question of time until you resolved the old invariant problems, that hardly even the dot on the i *was missing, was beyond doubt for me long ago. But I am truly glad that this is going so fast and succeeding with such surprising ease, and I congratulate you for that. Now that you have discovered smokeless powder with your latest theorem, after Theorem I released smoke only to Gordan's eyes, it is really time to raze to the ground the castles of these robber barons Stroh, Gordan, Stephanos, and whatever they are all called, who ambushed solitary traveling invariants and locked them in dungeons, with the risk that no new life will sprout from the castle ruins. If you were not so radical, and if you could not use your creative powers much more productively, you could do a service to mathematicians if you put together that material in this area on which one can build.*

After a brief report on a visit by Lipschitz and his ideas about the distribution of primes and partitions, the letter ends with a comment on mathematical terminology and its reception:

Lipschitz created a wild confusion for me in my work. In his opinion, new notions should not be designated by words which one is used to connecting with ideas not completely matching the new applications; since he is of course right, I have very boldly changed a few notations, and as a result my paper sounds to me in places like an absolute rag magazine.[3]

Acknowledgments

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[3] *Bierzeitung*

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Courant Institute, New York University, 251 Mercer Street, New York, New York 10012; and Mathematisches Institut, Bunsenstr. 3-5, 37073 Göttingen, Germany

E-mail address: `tschinkel@cims.nyu.edu`

Bonn, den 9. Februar 1892.

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Lieber Hilbert.

Zunächst mutatis mutandis eine repetitio Deiner eigenen Vertheidigung. Einige neu hinzukommende Milderungsgründe unterdrücke ich, da von einer Anklage mit Sicherheit nichts verlautet hat. Gänzlich unberührt von allen Mathematiker-Bewegungen, und gänzlich arm an Mathematiker-bewegenden Fortschritten, hätte ich, von Tag zu Tage trübsinniger und unerfreulicher werdend, vielleicht noch einige Heilige in meinem Kalender mit zwei Tintenstrichen gekreuzigt, ohne mich Dir in Erinnerung zu rufen, wenn nicht der Brief von Hurwitz wieder heute etwas Leben in meine Bude gebracht hätte.

Dein H. Minkowski