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Combinatorics, representation theory and invariant theory: The story of a ménage à trois

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In honor of Adriano Garsia

1. Introduction

The joy of my being here among a group of old friends to celebrate the anniversary of one of my closest friends, Adriano Garsia, is haunted by temptation. As I look at you, condemned as you are to listen to me for the next fifty minutes, I feel that the opportunity I have long been waiting for is at last delivered to me on a silver platter: the opportunity, that is, of presenting the latest results of my own work to a public whose collective wisdom in combinatorics will seldom (if ever) again be reassembled. At last, I will inflict upon a competent (if helpless) audience one hour's lecture on the basics of supersymmetric algebra, on the concomitants of skew-symmetric tensors, or maybe on the characteristic-free resolution of Weyl modules.

At the sound of these words, we register a sudden decrease in the temperature of this hall. A chill is running down your spines at the prospect of being subjected to such a punishing sitting. Rest assured that I will not abuse the time you have generously allotted me; or rather, I will try not to do so deliberately.

We will dwell instead upon the one topic of unquestioned interest and timeliness among mathematicians of all times: gossip. Or rather, to use an acceptable euphemism, we will deal with the *history* of mathematics.

2. Cambridge 02138 in the fifties

The fifties were a great time to be alive, and the assembly of younger mathematicians that went through the universities in the Boston area would now fill the Who's Who of mathematics.

The center of mathematical activity was the MIT common room, recently renovated in 1957, which since that year has been allowed to freely deteriorate. At frequent intervals during the day, you would find in the MIT common room Paul Cohen, Eli Stein,

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and later Gene Rodemich, excitedly engaged in aggressive problem solving sessions and other mutual challenges to their respective mathematical knowledge and competence.

The leader of the problem solving sessions was without question Adriano Garsia, at times joined by Cameo appearances of Alberto Calderón, Jürgen Moser and John Nash. Often, a discussion that had started in the common room would be carried over uninterrupted through lunch at Walker, where mathematicians used to assemble around a large table, easy to spot in the hustle and bustle of faculty and students.

Norbert Wiener would often join the younger mathematicians at lunch; he loved to sit at the old wooden tables in Walker; he glowed under the stares he got from the undergraduates, and craved the fawning admiration of the younger mathematicians. The temptation to tease him was irresistible.

Let me tell you a Norbert Wiener story, one that only one other person in this room knows. Several of us were having lunch one day, sitting at the usual table in Walker. Norbert Wiener was at the head of the table, with Paul Cohen at his right; others at the table were Adriano Garsia, Arthur Mattuck, myself, and some other person whose name I cannot recall. Paul Cohen turned his head towards Wiener and asked, in a tone of mock candor: 'Professor Wiener, what would you do if one day, when you went home at the end of the day, you were to find Professor X sitting in your living room sopha?'

Cohen was alluding to a well-known mathematician who was known to indulge in the dubious practice of 'nostrification'. Norbert Wiener became red in the face and snapped back: 'I would throw him out and start counting the silver!' I leave it to you to figure out who Professor X is. By the way, term 'nostrification' was introduced by Hilbert, and the practice has been faithfully carried on by some of his students.

From time to time, the problem sessions in the MIT common room would be temporarily suspended, and would be replaced by 'ranking sessions', where all of us instructors would indulge in the favored hobby of younger mathematicians, namely, that of passing judgement on older mathematicians and listing them in strict linear order. I remember a heated discussion we had one day, on whether Professor Y should be rated as a first-rate second-rate mathematician, or as a second-rate first-rate mathematician. I cannot remember whose side Adriano took at that time. We would usually find ourselves on the same side. We both believed at the time that John Nash was the most talented mathematician we had ever met, and I do not believe either of us has changed his mind since.

Every time I had to enter the MIT common room, I would make sure to swallow an extra tablet of Nodoz. Eventually, I began to avoid the common room altogether. I could not take the heat, and Adriano kindly agreed to meet me privately to bring me up to date on the latest happenings. All the differential geometry I know I learned from these tutoring sessions with Adriano.

One month in the spring of 1958 he was kind enough to find the time for a series of ten lectures on the theory of certain surfaces he had discovered, and whose theory he had developed in his first year at MIT. He had decided to name them 'Schottky surfaces' to honor Schottky, an otherwise obscure mathematician (or so we believed at the time). I never heard Schottky's name mentioned again until a couple of years ago, when Paul Erdős told me a startling story, which you will hear shortly.

Neither Adriano nor I had any inkling that we would end up working in combinatorics. In fact, the term 'combinatorics' was all but unknown. The problems of the day, those problems by which mathematicians test one another, were at that time more frequently drawn from analysis (they were problems such as one finds in Pólya and Szegö's collection) rather than from combinatorics, as is more often the case nowadays. Adriano was fond of reminiscing about two of his teachers, both of whom came from the same great German school of analysts as Pólya and Szegö: Karl Löwner and Marcel Riesz. After I met Karl Löwner and Marcel Riesz, I noticed a family resemblance.

At first, I did not believe some of the stories Adriano used to tell me about Marcel Riesz, but eventually I came to realize that they were true. For example, the story about Marcel Riesz's hiding his paychecks under the mattress instead of cashing them, and of how Adriano had to run to several banks all over town cashing checks for Marcel Riesz on the day before Marcel Riesz was scheduled to leave for Sweden.

Luckily, Adriano has kindly agreed to attend this meeting, and he will tell us some of the better stories about those wonderful years.

3. Alfred Young

Alfred Young believed his greatest contribution to mathematics to be the application of representation theory to the computation of invariants of binary forms. If he had been told that today we mention his name with reverence in connection with the notion of standard tableaux, he would probably have winced.

The story of standard tableaux makes an interesting episode of mathematical history. As you know, Alfred Young made his *debut* in mathematics with a difficult computation of the concomitants of binary quartics, a *tour de force* which took its lead from Peano's elegant and unjustly forgotten finiteness theorem.

As he proceeded to derive a systematic method for computing the syzygies holding among the invariants of such quartics, Young realized that the methods developed by Clebsch and Gordan for the computation of invariants and syzygies could not be pushed much farther. He went into a period of self-searching which lasted a few years, after which he published the first two papers of the series 'Quantitative substitutional analysis'. Both papers appeared in short sequence at the turn of the century. In these papers Young outlined the theory of representations of the symmetric group as we know it today. He proved that the number of irreducible representations of the symmetric group of order n equals the number of partitions of n, and he gave an explicit decomposition of the group algebra into irreducible components by means of idempotents.

To this day remain, Young's combinatorial construction of the irreducible representations of the symmetric group remains the simplest, though not the most elegant nor the easiest to handle. Alfred Young made no appeal to the theory of group representations (or group characters, as it was called by Frobenius, who had developed it); in fact, the word 'group' seldom appears in the seven hundred-odd pages of Young's collected papers. It is safe to surmise that Young was reluctant to rely upon group-theoretic arguments. For example, he never used the expression 'normal subgroup'.

It seems that Young's results irritated the leading algebraist of the time, namely, Professor Frobenius of the University of Berlin. What? This British upstart could get, using rudimentary combinatorial methods, what everyone in his sophisticated German school had been working upon for years? Frobenius carefully studied Young's two papers (while skipping Young's elaborate applications to invariant theory, a subject which Frobenius despised) and went to work. After a short while, Frobenius published a paper in which Young's results were properly rederived following the precepts of Frobenius's newly invented theory of group characters. Frobenius was a Bourbakist at heart, and his way of doing representation theory has prevailed to this day.

Frobenius went one up on Alfred Young, by discovering the character formula that now bears his name, a formula which Young had unforgivably missed.

Young was deeply hurt when he learned of Frobenius's work. Worse yet, at exactly the same time as Young was getting ready to publish his two papers on substitutional analysis, Frobenius assigned to his best student Issai Schur the thesis problem of determining all possible generalizations of the Binet–Cauchy formula for the multiplication of minors of matrices. In the year 1900, Issai Schur published his thesis, in which all irreducible representations of the general linear group were explicitly determined on the basis of their traces, what we now call Schur functions. Young realized the connection between his work and Schur's thesis; Young's two papers and Schur's thesis were embarrassingly close results, though by no means overlapping.

For about twenty years after the turn of the century, Young did not publish another word. To the surprised colleagues who would make tactful inquiries, he would answer that he was learning to read German in order to understand Frobenius's work. But this was a white lie. Young was intensively working on going one up on Frobenius. And he did, when in 1923, almost twenty years after his second paper, he published his third paper on quantitative substitutional analysis. In this paper standard tableaux were first introduced to the world, their number was computed, and their relation to representation theory was described. Again, Young's methods were purely combinatorial, with not one iota of group theory or character theory. A new proof of Frobenius's character formula was given, which dispensed with the apparatus of representation theory as we know it today, and replaced it by combinatorial tecniques.

Again, there followed a period of shocked silence. It appears that the German algebraists were lending a deaf ear to Young's discovery. Had it not been for Hermann Weyl's intervention, Young's newly discovered standard tableaux might have been permanently exiled to Aberystwyth, Wales, together with apolarity and perpetuants.

Hermann Weyl, while writing his book on group theory and quantum mechanics in the late twenties, came upon Young's work and realized its importance. The term 'Young tableau' first appeared in print in Hermann Weyl's book 'Group theory and quantum mechanics' (a book that was rewritten and updated some forty years later by Leona Schensted). Since Hermann Weyl represented at the time the pinnacle of mainstream mathematics, it did not take long for Young's name to become a household word first among physicists, and then among mathematicians. Van der Waerden included standard tableaux in one of the later editions of his 'Modern Algebra', where he made use of some unpublished results of von Neumann. The geometer Hodge learned about standard tableaux from Young's associate, Professor D.E. Littlewood, and used them with great success in his study of flag manifolds. The algebras that are nowadays named after Hodge might more justly have been named after Young. Hodge himself explicitly acknowledges his total indebtedness to Littlewood and Young.

Alfred Young was generous with his ideas. Turnbull would come down to Cambridge (the *other* Cambridge) from St. Andrews in Scotland once a month to talk to Young. Philip Hall polished Young's ideas on symmetric functions, which were first made public in Aitken's 'Letter to an Edinburgh colleague'. The various equivalent definitions of Schur functions first appeared together in Philip Hall's paper 'The algebra of partitions', without any proofs; for several years after Hall's paper, no printed proofs of the equivalence of these definitions were available, and we had to construct our own.

Alfred Young's style of mathematical writing is one that has unfortunately gone out of fashion. It is based on the assumption that the reader is to be treated as a gentleman with a sound mathematical education, and gentlemen need not be told the lowly details of proofs. As a consequence, we sometimes have to puzzle nowadays on certain inferences for which Young omits any explanation out of respect for his readers.

Young's one and only student, G. De B. Robinson, wrote the ninth and last paper on quantitative substitutional analysis on the basis of notes left by his teacher. Robinson inherited all of Young's handwritten papers after his teacher's death. I shall not make any conjectures on the fate of these papers. Suffice it to say that, some fifteeen years ago, Alfred Young's collected papers were published by the Toronto University Press, at the price of ten dollars a copy, a prize subsidized by an unknown donor. They are still in print.

4. Problem solvers and theorizers

Mathematicians can be subdivided into two kinds: there are the problem solvers and there are the theorizers. To be sure, most mathematicians are a mixture of the two; however, it is easy to find extreme cases of either kind; I leave it to you to cite your favored examples of either kind.

4.1. Problem solvers

To the problem solver, the supreme achievement in mathematics is the solution of a problem that had been given up as hopeless. It matters little that the solution may be clumsy; what matters is that it should be the first, and a correct one. Once he solves a problem, the problem solver will permanently lose interest in it, and will listen to new and simplified proofs of his problem with an air of condescension, suffused with boredom.

The problem-solver is a conservative at heart. To him, mathematics consists of a sequence of challenges to be met, an obstacle course of problems. The mathematical concepts required to state mathematical problems are tacitly assumed to be eternal and immutable.

Mathematical exposition is regarded as an inferior undertaking. All new theories are viewed with deep suspicion, as intruders who must prove their worth by posing challenging problems before they are paid any attention. The problem solver resents all generalizations, especially those that may occasionally succeed in trivializing the solutions of one of his problems.

The problem solver is the role model for budding young mathematicians, eager to prove their worth by following his courageous lead. When we describe to the public the conquests of mathematics, we take famous problem solvers as our shining heroes.

4.2. *Theorizers*

To the theorizer, by contrast, the supreme achievement of mathematics is a theory that sheds sudden light on some incomprehensible phenomenon. To him or her, success in mathematics does not consist in the solution of problems, but in their trivialization. His or her moment of glory comes with the discovery of a new theory that does not solve any of the old problems, but renders them as irrelevant as crossword puzzles.

The theorizer is a revolutionary at heart. To him or her, mathematical concepts received from the past are to be regarded with deep suspicion, as imperfect instances of more general concepts yet to be discovered. Mathematical exposition is thought to be a more difficult undertaking than mathematical research.

According to the theorizer, the only part of mathematics that will survive are the definitions. Great definitions are what mathematics permanently contributes to the world. Theorems are tolerated as a necessary evil, since they play a supporting role — or rather, as the theorizer will reluctantly admit, an essential role — in the understanding of definitions.

The theorizer often has trouble being recognized by the community of mathematicians. His or her frequent consolation is their certainty, which may or may not be borne out by history, that his or her theories will survive long after the problems of the day have been forgotten.

If I were a space engineer looking for a mathematician to help me send a rocket into space, I would choose a problem solver. But if I were looking for a mathematician to give a good education to my child, I would unhesitatingly choose a theorizer.

5. Hermann Grassmann and exterior algebra

Alfred Young was more of a problem solver than a theorizer. But one of the greatest mathematicians of the nineteenth century is a theorizer all the way; I mean of course Hermann Grassmann. Everyone agrees that Grassmann's one great contribution to mathematics is a new definition, namely, the definition of exterior algebra, a definition which he spent his entire life understanding and developing.

Grassmann never solved any of the problems that were fashionable in his day, in fact, he never solved any problems whatsoever, except those which he himself had posed. He never contributed to the mathematics of the nineteenth century, to invariant theory, to elimination theory, to the theory of algebraic curves or to any of the current fads. What is worse, to the dismay of his contemporaries, he rewrote some of the mathematics of his time in the language of exterior algebra, and he was the first to show that much classical physics could be simplified in the notation of exterior algebra, thereby anticipating the calculus of exterior differential forms that was to be developed by Elie Cartan in the next century. The work of Gibbs and later of Dirac would have been considerably simplified if Gibbs and Dirac had had even a fleeting acquaintance with exterior algebra. Instead, exterior algebra ended up as another missed opportunity, as Freeman Dyson might say.

It is not surprising that Grassmann was not entirely welcome among mathematicians. Anyone who comes up with a new definition is likely to make enemies. No one wants to be told to drop what he or she is doing and start paying attention to the intrusion of foreign ideas. Grassmann made a great deal of enemies, and the animosity against his great definition has not entirely died out to this day.

The reactions against Grassmann make a humorous chapter in the history of mathematics. For example, Professor Pringsheim, the dean of German mathematicians, and the author of over one hundred substantial papers on the theory of infinite series, both convergent *and* divergent, kept insisting that Grassmann should be doing something relevant instead of writing up his maniacal ravings. 'Why doesn't he do something useful, like discovering some new criterion for the convergence of infinite series!' he asserted with all the authority that his position conferred upon him.

The invariant theory community, led by Clebsch and Gordan, also loudly protested that Grassmann's work was pointless, since it did not contribute one single result to the invariant theory of binary forms. They were dead wrong, but they would not be proved wrong for at least another fifty years.

Not even Hilbert paid attention to Grassmann. In the second volume of Hilbert's collected papers I have found only one mention of Grassmann, in a footnote. And even the editor of Grassmann's collected papers, Eduard Study, only partially understood exterior algebra. Study's last book, called 'Vector algebra' (in German '*Einleitung in dfie Theorie der Invarianten lineärer Transformationen auf Grund der Vektorrechnung*') would have greatly benefitted from an injection of exterior algebra; it is clear, however, that Study did not feel comfortable enough with exterior algebra to use it in his work.

Evil tongues whispered that there was really nothing new in Grassmann's exterior algebra, that it was just a mixture of Möbius's barycentric calculus, of Plücker's coordinates, and of von Staudt's algebra of throws.

The standard objection that was raised against exterior algebra was expressed by the notorious question 'What can you prove with exterior algebra that you cannot prove without it?'

Whenever one hears this question raised against some new piece of mathematics, one may rest assured that one is likely to be in the presence of something important. In my time, I have heard it raised against random variables, against Laurent Schwartz's theory of distributions, against idèles and against Grothendieck's schemes, to mention only a few instances. A proper retort to this silly question might be the following answer: 'You are right. There is nothing in yesterday's mathematics that you can prove with exterior algebra, and that could also not be proved without it. Exterior algebra is not meant to prove old facts, it is meant to disclose a new world. Disclosing new worlds is as worthwhile a mathematical enterprise as proving old conjectures'.

The first mathematician to understand the importance of exterior algebra was Peano, who published a beautiful short introduction to the subject (in Italian: *Calcolo geometrico secondo l'Ausdehnungslehre di Grassmann, Torino, Bocca, 1888*). Unfortunately, Peano was at the time fresh out of school teaching at the Pinerolo military school, and his audiences to what must have been beautiful lectures on exterior algebra consisted of Italian cavalry officers and cadets. No one living beyond the Alps read Peano's book. Three hundred copies were printed of the first and only edition.

It took almost one hundred years before mathematicians realized the greatness of Grassmann's discovery. This is the fate that is meted out to mathematicians who make their living on definitions.

6. Definition and description in mathematics

One of the great achievements of mathematics in this century is the idea of precise definitions ensconced in an axiomatic system. A mathematical object must and can be precisely defined; this is the only way we have to make sure we are not dealing with pure fantasy.

While stressing the importance of definition, our century has given short shrift to an older notion, the notion of description of a mathematical object. Description and definition are two quite different enterprises, and they are sometimes confused with each other. You can realize the difference between definition and description by performing the following thought experiment. Suppose you are trying to teach a new mathematical notion to your class. You know that you cannot get away with just writing a definition on the blackboard. Sooner or later, you must describe what is being defined. Nowadays, one of the more common ways of describing a new mathematical object is to give several equivalent definitions of it. Philosophers have long puzzled over this strange phenomenon, whereby completely different definitions can be given of the same mathematical object.

In ages past, mathematical objects were described before they could be properly defined, except in geometry, where Euclid set the standards early in the game. But except in geometry, the need for a precise definition was not even felt. The mathematics of the past two centuries confirms the fact that mathematics can get by without definitions, but not without descriptions. Physicists have long been aware of this priority.

To be sure, mathematicians of all times have claimed that definition is a *sine qua non* of a proper mathematical presentation. But mathematicians, like all people, have seldom practiced what they preached. Let us consider some glaring examples.

The most notorious is the field of real numbers, which was not rigorously defined by current standards of rigor until Dedekind came along, very late in the game. Shall we infer from the lack of a rigorous definition, that all work on the real numbers that came before Dedekind is to be discarded as nonsense? Certainly not.

Another equally glaring example is the concept of a tensor. When I was an undergraduate at Princeton, Professor D.C. Spencer defined a tensor in class as 'an object that transforms according to the following rules'; this is the description of a tensor that you will also find in Luther Pfahler Eisenhart's textbook in differential geometry, still considered to be the best introductory textbook in the subject. It was clear to everyone that such a nonsensical statement was not a definition. In fact, every time such a characterization of a tensor was stated, it was followed by a slight giggle. Nevertheless, the lack of definition of a tensor did not stop Einstein, Levi Civita and Cartan from doing some of the best mathematics in this century.

As a matter of fact, the first correct definition of a tensor did not become current until the fifties, under the influence of Chevalley I believe, or perhaps I should say Bourbaki. Even more amazing, the first completely rigorous definition of a tensor was given just at the time when tensors were going temporarily out of fashion.

A lot of mathematical research is spent in finding suitable definitions to justify statements that we already know to be true. The most famous instance of such a situation is the Euler–Schläfli–Poincaré formula for polyhedra, which was believed to be true in great generality long before a suitably general notion of polyhedron could be defined.

At least one hundred years of research were spent on singling out a definition to match the Euler–Schläfli–Poincaré formula. Meanwhile, no one ever entertained any doubt of the formula's truth. The philosopher Imre Lakatos has documented the story of such a search for a definition in thorough historical detail. Curiously, his findings, which were published in the book '*Proofs and refutations*', were met with a great deal of anger on the part of a section of the mathematical public, who held the axiomatic method to be sacred and inviolable. Lakatos's book became for a while anathema among philosophers of mathematics of the positivistic school. The truth hurts.

Hermann Grassmann was a great believer in description; by-and-large, he did not bother to give definitions in the current sense of the term. His descriptive style, coming precisely at the time when the axiomatic method was becoming a fanatical devotion among mathematicians, is very probably one more reason why his work was not read. As a matter of fact, the first rigorous definition of exterior algebra was not given until the forties, by Bourbaki, in Chapter 3 of his or her 'Algèbre', which is perhaps the best written of all of Bourbaki's volumes. Each successive edition of this chapter '*fait regretter les précédentes*', as the French say.

Every mathematician of my generation and the preceding learned exterior algebra from Bourbaki. For example, Emil Artin did so in the early fifties, motivated by the

Galois cohomology he was inventing at the time. I cannot refrain from telling you a story about myself. Sometime in 1951, I traveled from Princeton to New York to visit Stechert-Hafner on Fourth Avenue, a huge four-floor academic bookstore that has since gone bankrupt.

Stechert-Hafner looked more like a warehouse than like a bookstore; books were spread all over in no particular order, ready to be shipped to some college library.

As I came out of the elevator on the third floor, I walked up to a lady who was working with an adding machine, and who seemed the only person present. After a few minutes' wait, the lady turned her eyes toward me. She looked at me squarely and, before I could speak a word, she said: 'I know your type! You want the Bourbáki books!' She was right. She gave me a huge discount, and I will never forget her.

7. Bottom lines

How do mathematicians get to know one another? Professional psychologists do not seem to have studied this question; I will try out an amateur theory. When two mathematicians meet and feel out each other's knowledge of mathematics, what they are really doing is finding out what each other's bottom line is.

It might be interesting to give a precise definition of the concept of a bottom line; in the absence of a definition, we will describe some typical examples.

To the algebraic geometers of the sixties, the bottom line was the proof of the Weil conjectures. To generations of German algebraists, from Dirichlet to Hecke and Emil Artin, the bottom line was the theory of algebraic numbers. To the Princeton topologists of the fifties, sixties and seventies, the bottom line was homotopy. To the functional analysts of Yale and Chicago, the bottom line was the spectrum. To many combinatorialists today, the bottom line is either the Yang–Baxter equation, or the representation theory of the classical groups, or the Schensted algorithm. To some algebraists and combinatorialists of the next ten or so years, the bottom line may be elimination theory.

I will shamelessly tell you what my own bottom line is. It is placing balls into boxes, or, as Florence Nightingale David put it with exquisite tact in her book '*Combinatorial chance*', it is the theory of distribution and occupancy.

We resort to the bottom line when we are asked to write a letter of support for some colleague. If the other mathematician's bottom line is agreable with ours, then our letter is more likely to be positive. If instead our bottom lines disagree, then our letter is likely to be restrained.

The most striking example of mismatch of bottom lines was told me by Erdős. When David Hilbert, then a professor at the University of Konigsberg, was being considered for a professorship at Gottingen, the Prussian ministry asked Professor Frobenius to write a letter in support of Hilbert's candidacy. Here is what Frobenius wrote: 'He is rather a good mathematician, but he will never be as good as Schottky'.

Allow me to tell you two more personal stories.

In 1957, in my first year as an instructor in Cambridge, I occasionally had lunch with Oscar Zariski, who liked to practice his Italian. One day, while we were sitting in the main room of the Harvard Faculty Club, he stared at me in the face with his fork in his hand and said, loud enough for everyone to hear: 'Remember! Whatever happens in mathematics happens in algebraic geometry first!' As a matter of fact, algebraic geometry has been the bottom line of mathematics for almost one hundred years; but perhaps times are changing.

The second story is more somber. One day, in my first year as an assistant professor at MIT, as I was walking down one of the long corridors of MIT, I met Professor Z, a respected senior mathematician with a solid international reputation. He stared at me in the face and shouted: 'Admit! All of lattice theory is trivial!'

I did not have the presence to answer that von Neumann's work in lattice theory is deeper than anything Professor Z has done in mathematics.

Those of us who have reached a certain age remember the visceral hatred of lattice theory that was widespread from around 1940 to around 1970, and that has not completely disappeared. Such unusual and widespread instance of dislike for an entire field cannot be simply attributed to personality clashes. It is more likely to be explained by localizing certain abysmal differences among the bottom lines of the mathematicians of the time.

If we begin such a search, we are likely to conclude that the field that is normally classified as algebra really consists of two quite separate fields. Let us call them algebra one and algebra two, for lack of a better language.

Algebra one is the algebra whose bottom lines are algebraic geometry or algebraic number theory. Algebra one has by far a better pedigree than algebra two, and has reached a high degree of sophistication and breadth. Commutative algebra, homological algebra, and the more recent speculations with categories and topoi are exquisite products of algebra one. It is not infrequent to meet two specialists in algebra one who cannot talk to each other, since the subject is so vast. Despite repeated and dire predictions of its demise, algebra one keeps going strong.

Algebra two has had a more accidented history. It can be traced back to George Boole, who was the initiator of three well-known branches of algebra two, namely: in the first place, Boolean algebra, in the second place, the operational calculus that views the derivative as an operator D, on which Boole wrote two books of great beauty, and finally, invariant theory, which Boole initiated by remarking the invariance of the discriminant of a quadratic form under the action of Sl_2.

Very roughly speaking, between 1850 and 1950, algebra two was preferred by the British and the Italians, whereas algebra one was once a German and lately a French preserve. Capelli and Young's bottom line was firmly in algebra two, whereas Kronecker, Hecke and Emil Artin are champions of algebra one.

At the beginning, algebra two was largely cultivated by invariant theorists. Their objective was to develop a notation suitable to describe geometric phenomena which is independent of any choice of a coordinate system. In pursuing this objective, the invariant theorists of the nineteenth century were led to develop explicit algorithms and combinatorial methods. The first combinatorialists, MacMahon, Hammond, Brioschi, Trudi, Sylvester, were invariant theorists. One of the first papers in graph theory, in which the Petersen graph is introduced, was motivated by a problem in invariant theory. Clifford's ideal for invariant theory was to reduce the computation of invariants to the theory of graphs.

The best known representative of algebra two in the nineteenth century is Paul Gordan. He was a German, perhaps the exception that tests our rule. He contributed a constructive proof of the finite generation of the ring of invariants of binary forms which has never been improved upon, and which foreshadows current techniques of Hopf algebra. He also published in 1870 the fundamental results of linear programming, a discovery for which he has never been given proper credit.

Despite his achievements, Paul Gordan was seen as an intruder by specialists in algebra one. '*Er war ein Algorithmiker*!' said Hilbert when Gordan died.

Gordan's student Emmy Noether became an ardent apostle of algebra one; similarly, van der Waerden, a student of General Weitzenböck, an algebra two hero, intensely disliked algebra two throughout his career. In the thirties, algebra two was enriched by lattice theory and by the universal algebra of Philip Hall and his student Garrett Birkhoff.

Despite these notable advances, algebra two has always had a harder time. You would not find any lattices, any exterior algebra nor even any mention of tensors in any of the editions of van der Waerden's '*Modern Algebra*'. G.H. Hardy subtly condemned algebra two in England in the latter half of the nineteenth century, with the exclamation 'Too much $f(D)$!' G.H. Hardy must be turning in his grave now.

But by now you must have guessed the conclusion of this long tirade. Algebra two has come of age; in the last twenty years or so, it has blossomed, and it has acquired a name of its own. The bottom line for most of us here is algebra two. In fact, a suitable name has finally been invented for algebra two: algebraic combinatorics. In celebrating the anniversary of one of the foremost representatives of algebra two, Adriano Garsia, we are also rejoicing that our field, algebraic combinatorics, after a tortuous history has at last found its own bottom line, together with a firm place in the mathematics of our time.

26 July 1994