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## Disciplines and Styles in Pure Mathematics, 1800-2000

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**ABSTRACT.** This workshop addressed issues of discipline and style in number theory, algebra, geometry, topology, analysis, and mathematical physics. Most speakers presented case studies, but some offered global surveys of how stylistic shifts informed the transition and transformation of special research fields. Older traditions in established research communities were considered alongside newer trends, including changing views regarding the role of proof.

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### Introduction by the Organisers

This interdisciplinary workshop brought together mathematicians, historians, and philosophers to discuss a theme of general interest for understanding developments in mathematics over a span of two hundred years. The emergence and development of various disciplines in pure mathematics after 1800 has now been studied in a number of special contexts, some of which played an important role in shaping the character of modern research traditions. It has long been understood that the period after 1800 saw a kind of emancipation of mathematical research from related work in nearby fields, especially astronomy and physics. This general trend not only led to a proliferation of special disciplines and wholly new fields of knowledge, it also went hand in hand with a variety of innovative styles, new ways of doing and presenting mathematics. Issues of style have long been central for historians of art and literature, but such matters have seldom been addressed in the historical literature on mathematics, despite the fact that mathematicians themselves have often been acutely aware of the importance of creative styles. During the last

few decades, however, there has been a growing interest in various shifts within the larger disciplinary matrix of mathematics during the nineteenth and twentieth centuries. This workshop therefore aimed to shed new light on these complex processes by considering ways in which issues of style—operating on the individual, communal, and national levels—shaped and guided research activities in important fields. Approximately half of the program dealt directly with mainstream fields in pure mathematics, addressing issues of discipline and style in number theory, algebra, geometry, topology, and analysis. A session on mathematical physics helped to round out this picture by looking across the usual disciplinary boundaries. Most of the speakers focused on subtler shifts in style and substance relevant to their special theme, but a few offered more global surveys of how stylistic shifts informed the transition and transformation of special research fields. Older traditions in established research communities were considered alongside newer trends, including changing views regarding the role of proof. The role of journals as a medium of communication, but also for staking out priority claims and molding disciplines, received considerable attention. Several speakers also dealt with problematic issues connected with the use and abuse of stylistic issues to promote special agendas. As a whole, this workshop broke significant new ground by showing through a rich variety of examples how stylistic and disciplinary factors affected major developments in mathematics over the last two centuries.

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## Abstracts

### Rigour and understanding – the case of algebraic curves

JEREMY GRAY

I discussed what might be meant by ‘style’ in mathematics, and offered a discussion of the conflicts between rigour and understanding, which is a place in which conflicts of style can often be found. One definition of style, adapted from the Oxford English Dictionary, would be a ‘manner of expression characteristic of a particular mathematician or a group or period; a writer’s mode of expression considered in regard to clearness, effectiveness, beauty, and the like’. Another, harder to work with, would be ‘Those features of mathematical composition which belong to form and expression rather than to the substance of the thought or matter expressed’. Terms such ‘algebra’, ‘analysis’, and ‘geometry’ would then refer not to styles, but to disciplines.

There would not be much point in talking about styles in mathematics if it did not enable us to do something. I think the concept can be used to illuminate misunderstandings: imperfect understandings by good mathematicians may arise from the use of different styles of exposition. For example, algebraic curves were approached in the mid-19th century in algebraic, algebrao-geometric, analytic, and even topological ways, all with different contemporary standards of rigour, and all with different aspects: parts that look easy and parts that look questionable. As commonly analysed, these are disciplinary differences, but I think we can see stylistic differences at work too, as I intend to show.

What Riemann introduced in his doctoral thesis (Riemann 1851) was a truly naïve yet profound idea. After Cauchy, he was the second mathematician to appreciate that within the class of functions from  $\mathbb{R}^2$  to  $\mathbb{R}^2$  there is a major subclass of functions from  $\mathbb{C}$  to  $\mathbb{C}$  and to begin to spell out their distinctive properties. This class is specified by insisting on the definition of complex differentiability: the derivative of a function from  $\mathbb{C}$  to  $\mathbb{C}$  at each point where it is differentiable is to be independent of the direction of  $dz$ . Riemann was not, however, primarily writing complex function theory. By 1857, in (Riemann 1857) he was using it and developing it to an extraordinary degree, in order to do something else: create a theory of Abelian functions. By now his approach was very abstract. His paper starts with an abstract branched covering of the sphere, it dissects this to obtain a polygon whose sides are identified in pairs, it does function theory on this polygon in order to deduce the existence of complex functions on the covering surface. Only then does it bother to show that to each branched covering and each choice of a set of poles there corresponds a family of algebraic curves.

In the obituary of Rudolf Clebsch, who died unexpectedly in 1872, his seven obituarists (Klein, Brill and Noether among the, see (von der Mühll et al 1874) recorded that it was difficult to see a branched covering completely, and it was difficult to know how to dissect it. Several good mathematicians retreated from Riemann’s position of 1857 to that of 1851. After Clebsch and Gordan, Brill and

Noether studied the general problem as one about algebraic curves in the plane  $\mathbb{C} \times \mathbb{C}$ . This loses the abstract setting where Riemann began and therefore loses the topological aspect of the theory entirely. Riemann's students Hattendorff and Prym were appalled, as their correspondence makes clear.

A different but nonetheless pedagogically motivated choice of style was adopted by another group of mathematicians, most of them with personal ties to Riemann, who wrote books, or large sections of books, about his ideas: see (Durège 1864), (Schlömilch 1866), and (Neumann, C.A. 1865) in Germany, and (Bertrand 1870) in France. They largely restricted their attention to elliptic functions and other simple Riemann surfaces. In his review of Bertrand's *Traité* (1870) Darboux welcomed this part as being the first to show a proper understanding of this difficult subject and therefore to offer the hope that Riemann's ideas would not be abandoned.

These books display a typical feature of the textbook style: numerous examples. They were written for audiences that wanted to learn the advanced calculus in some depth and who were willing to admit complex variables and complex functions. They took the Riemannian approach and inverted it, so that readers began with complex polynomials, appreciated that their many-valued nature could be understood via the picture of a covering, but (Neumann apart) dispensed with the idea of a covering space as a fundamental entity. I would tentatively suggest that what we see here are examples of a mathematician's writing style being determined by the context, and that these books might be examples of textbook style.

The uniformisation theorem of Poincaré (1883 and 1907) offered a novel way to understand Riemann's ideas, because it placed them in a new context. Riemann surfaces were now obtained not as quotient spaces rather (as is well-known) as total spaces of a branched covering. Where they come together is in their presentation of a polygon with its sides identified in pairs. Does thinking about style help us to see anything new? I think it does. While Hilbert objected, correctly, in describing his famous mathematical problems (Hilbert 1901) to the proof of the regularity of the parameterising functions, he could, with equal justice, have objected to the sheer vagueness of the description of the universal covering space. This leads me to propose the existence of another style of mathematics, which might be called the visionary or the naive, and which deals in loose presentations of big ideas, where the expectation is that when everything is tightened up the big idea – now possibly somewhat altered – will be seen to have been correct all along. This is not a style available to every mathematician, nor is it one adopted by every member of the select group that might be recognised by their peers as being talented enough to be licensed to provide such visions. When it is used it raises acutely the question of when a piece of mathematics is understood.

These considerations deepen one's suspicion that 'style' is a term that may rest on a misunderstanding: it is probably not possible to say the same thing in different ways. Choice of a style may determine the content of what a mathematician is going to say, by affecting his or her use of examples, techniques, use of other literature and in other ways. The textbook and the visionary style communicate

different things. One's choice of style affects what can be said and also how well it can be understood. What I called the textbook style may not be compatible with the visionary approach, readers needing one in order to do something new themselves may be disappointed, even frustrated, when they get the other one. This is not entirely a matter of the content or difficulty of what is being said, but also a question of how it is said.

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More details can be found in Bottazzini and Gray's forthcoming history of complex function theory.

### Learned journals and styles of mathematical communication

JEANNE PEIFFER

The lecture grew out of a research program which started in 2005 and which investigates learned journals as agents of communication and construction of knowledge in the seventeenth and eighteenth centuries. The creation of learned journals in the last third of the seventeenth century - *Journal des savans* in January 1665 followed by *The Philosophical Transactions* in March of the same year - eventually established a communication system, which has dominated the exchange and validation practices in science since for centuries- and which is today called into question by the dematerialisation of its medium. These first journals were not yet specialised and may be characterised by the publication, at regular intervals and under the same title, of extracts of recent books, original memoirs and scientific news, each issue giving voice to different themes and authors. The texts published were listed in annual bibliographies or indexes, and recapitulatory tables, which referred them to topics in a classification system. The latter offered access, on a

long-term basis, to the information published in the journal, which could thus be revived, completed, extended, discussed or controversially debated.

In the talk I examine the implications of this communication system on the production and assessment of reliable knowledge. More specifically, I am interested in its impact on the “style” of mathematical representation. Before the creation of learned journals, mathematicians communicated mostly by letters with a more or less wide circle of correspondents. Mathematicians were then writing for an exclusive group of readers and assumed that their readers possess an extensive tacit know-how that they widely shared with them.

## 1. TACIT KNOWLEDGE

A word may be in order to make clear what I understand here by tacit knowledge, or as I would prefer to say *shared know-how*. The term “tacit knowing” or “tacit knowledge” was first introduced into philosophy by Michael Polanyi in his *Personal Knowledge* (1958). The theory of tacit knowledge stands in opposition to the “ideal of wholly explicit knowledge” which took shape from the scientific revolution of the seventeenth century. There is a lot of discussion going on concerning the philosophical theory of tacit knowledge, especially in the context of mathematical education and curricula. Among the different interpretations which have been given – from a conscious under-articulation to the strong thesis that there are specific kinds of knowledge that are in principle incapable of articulation – I am especially interested in the first one: Tacit knowledge as something mathematicians consciously attempt to conceal, to avoid articulating or which they choose to under-articulate. This is particularly interesting if we understand mathematics also as a social practice in a wide sense. Tacit knowledge is then built on experience or action. Mathematical practitioners do not fully describe it because they assume, or know by experience, that their readers share this knowledge with them. It concerns knowledge of a set of procedures, methods, techniques, and strategies. Tacit mathematical knowledge is any type of mathematical knowledge used as subsidiary to the performance and control of a mathematical task (Ernest).

In their correspondence, mathematicians from the seventeenth century heavily relied on this tacit knowledge as I show by means of two examples well known in the historiography of mathematics: The catenary and the Florentine cupola. I put the emphasis on the communication strategies used by the authors involved in these famous contests.

## 2. EXAMPLE OF THE CATENARY

The problem was posed by Jacob Bernoulli, then a newcomer in the field, in the May 1690-issue of the *Acta eruditorum* and solved by Christiaan Huygens, Johann Bernoulli and Gottfried W. Leibniz<sup>1</sup>. Huygens applied the rules of epistolary communication and, in order to avoid discussions about the authorship of the result, hid his solution under a cipher which could at best be understood by a

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<sup>1</sup>Their solutions are published in the June 1691-issue of the *Acta eruditorum*.

mathematician knowing the result. He was writing for a very small circle of experts and was barely communicating. Leibniz was not willing to encode his solution. Instead he enumerated some properties of the curve (*supposita ejus constructione*), which in his eyes could allow Huygens to name the curve which describes the shape of a rope attached to two fixed points located on a same vertical plane. This was also what Johann Bernoulli did in the paper sent to Leipzig. While Huygens applied the rules he was used to follow with his correspondents and communicated first a cryptic solution, Leibniz and Bernoulli seemed confident that the greater publicity offered by journals could guarantee the authorship of the published solutions. It is important however to stress that none of the three authors gave a complete solution. They contented themselves with describing the properties of the curve and giving the construction of one single point on it. They clearly assumed a very strong tacit know-how from their (necessarily small) audience. Leibniz was very proud of the catenary and tried to make it widely known. In Italy, he contacted Bodenhause, a strong defender of Leibnizian methods and preceptor at the Medici court, to whom he communicated a complete solution. He agreed that Bodenhause might circulate the construction, but not his "*modum quadrandi*". His technique of squaring was new at that time and not at all systematic. Leibniz's correspondence with Bodenhause contains some evidence of the rules to follow when mathematicians make their solutions public, either by letters or by publication in journals<sup>2</sup>.

### 3. THE FLORENTINE CUPOLA

Vincenzo Viviani, Galileo's disciple and mathematician of the Tuscan court, formulated this problem in response to Leibniz catenary. The Florentine *Aenigma* concerns what is described as an ancient Greek temple of a circular basis, dedicated to geometry. The problem is to cut out of the surface of a hemisphere four equal windows in such a way that the remaining surface be equivalent to a square. While Viviani stuck to a local patron, the grand prince of Tuscany, to whom he offered a geometrical model as solution to his challenge problem - the squarable cupola is obtained by drilling into a sphere two equal cylinders having a diameter equal to the radius of the sphere and one single generatrix in common passing through the centre of the sphere - Leibniz put the emphasis on method and used the journals in order to inform a wider, mostly mathematical audience about his new and general method, the differential calculus, which he applied to the special problem of the Florentine cupola. We can observe in this example two different epistemologies, classical geometry versus calculus, but also two different communication styles conveyed by two different material forms, a letter addressed to a patron versus an analytical calculation in a journal.

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<sup>2</sup>"Es ist aber guth da wenn man etwas wirklich exhibiret man entweder keine demonstration gebe, oder eine solche, dadurch sie uns nicht hinter die schliche kommen", Leibniz to Bodenhause, March 23, 1691, in Leibniz, Akademie-Ausgabe III, 5, p. 80.

#### 4. CONCLUDING REMARKS

In order to conclude, I formulate the following hypothesis, which needs further discussion: With the creation of learned journals, a new style of mathematical communication slowly emerged. Memoirs tended to be more explicit, to rely less on tacit know-how, even if it was far from being absent, to include proofs and analytical calculations. The emphasis was on method, more than on results. All along the eighteenth century, periodicals developed their own rules and codes for the acceptability and validation of the results they were publishing. These rules differed from those applied in epistolary exchanges. As publication in a learned journal guaranteed the authorship of a result or a method, it might no longer be necessary to withdraw from the published version the demonstration, the calculations or a special technique. The periodical form which allows rapid publication of short pieces, in which one single aspect of a question may be studied, which gives the possibility to add to one author's findings an extension found by another, to discuss an aspect of a proof or to propose a variant of a proof, the periodical form which allows all this brings about what might be called a new "style" of communication, more explicit and putting more emphasis on methods, proofs, variants of proofs than on simple results.

In my eyes, the hypothesis formulated above needs further study and may be extended into a new research program: When and where do mathematicians make which parts of their methods public ? In which contexts do they practice an open style of communication ?

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### Issues of style in the 19th century Cambridge Mathematical Tripos

JUNE BARROW-GREEN

Since its inception during the early decades of the 18th century, the Mathematical Tripos examination has defined the teaching and examining of undergraduate mathematics at Cambridge. Enormous prestige (which stretched far beyond the boundaries of the University) was attached to attaining first place (senior wrangler) in the examination, and being a leading wrangler provided a passport to high-status employment outside academia. As the 19th century developed, the number of examination papers grew, and the event progressively turned into a problem-solving marathon. For those undergraduates who wished to gain a high place in the order of merit, it was essential to hire coach (private tutor).

The latter years of the 19th century saw a number of reforms to the Mathematical Tripos, due both to the need for curriculum changes (to take account of new and developing subjects) and the fact that such a highly competitive examination was having an adverse effect on student numbers. Nevertheless, despite these reforms, the numbers continued to decline, particularly in contrast to those for the Mechanical Sciences Tripos [1] and at the beginning of the 20th century moves were made for further reforms—notably to get rid of the order of merit—with strong support coming from outside the university, e.g. from the engineer John Perry [2]. In 1907, after a prolonged and heated debate, the order of merit was finally abolished.

It is clear that during the (long) 19th century the Mathematical Tripos was little short of a national institution with widespread influence outside mathematics. That a mathematical examination should have reached such an exalted state is remarkable and invites consideration about its (changing) style. However, before embarking on such a study, some discussion about style within the broader mathematical culture, both nationally and within Cambridge itself, is merited. For example, one could, albeit rather provocatively, characterise British mathematics during this period, with its proliferation of problem solvers and lack of a research structure, as being ‘amateur’ as opposed to ‘professional’ (acknowledging the complexity of such labels); although such an epithet would not of course sit comfortably in discussions limited exclusively to Cambridge. But in Cambridge other aspects of style, for example, the dominance of Newtonian style mathematics with its emphasis on geometrical as opposed to analytical proofs, play into the story. Then there is the question of the differences of style between Cambridge mathematics and the mathematics studied and produced elsewhere, notably in Oxford, but also in London and the Scottish universities.

In considering style within the Mathematical Tripos itself, it is not only the examinations<sup>1</sup> that require scrutiny but also certain elements of the teaching and learning process, and in particular the system of coaching and the content of textbooks.

In the mid-19th century, the mathematical coach was in a position of considerable influence, both as a transmitter<sup>2</sup> and as an orchestrator of mathematical style. The job of the coach was to train students to solve problems as efficiently as possible. During the period 1862-1888, the most successful coach, Edward Routh, trained almost 50 of the 990 wranglers, and his style of teaching, with its relentless drive towards ever-faster problem-solving, had a lasting influence on many of his students [5]. But learning through such hot-house methods not only stultified the student as an independent learner but also reduced his opportunity for intellectual development, as was testified by John Venn who reported on his experience of being coached by Isaac Todhunter in the 1850s ([6], 185). And in the 1870s

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<sup>1</sup>It should be remarked that in addition to the Tripos examination, there was also the Smith’s Prize examination, another gruelling test, which was open to leading wranglers. For further details, see [3]

<sup>2</sup>For a description of the coach’s role in this respect, see ([4], 117).

the coach's domination of the undergraduate mathematical training began to be challenged by the gradual reform of public teaching.

Many books written by Cambridge authors for Cambridge students were virtually useless to students elsewhere, so opined De Morgan in 1835. This was because they were so intimately connected with the form of the Tripos examination, and, as Warwick has described, Cambridge students were groomed to deal with the sketchiest of examples in textbooks [7]. From the 1840s, textbooks increasingly reproduced large numbers of Tripos problems.<sup>3</sup> Typical of Cambridge authors were Todhunter,<sup>4</sup> Routh and A.R. Forsyth, all of whom published books with the Tripos in mind. To give some examples: Todhunter's *Algebra* sold over 500,000 copies and was published in editions for use overseas; Routh's *Dynamics* (1860) was admired by Felix Klein who had it translated into German (1898); Forsyth's *Differential Equations* (1885) included more than 800 problems and was translated into German. The mixed success of the foreign editions of these books – Todhunter's overseas editions sold well while the translations of Routh and Forsyth made little impact – is revealing. Todhunter's books were used for teaching by Cambridge graduates working overseas (the Empire) who took with them not only the books but the Cambridge style of teaching, while Routh's and Forsyth's books had no such ambassadors: Germany had its own style of teaching.

As far as the examination papers themselves were concerned, these contained two types of question: bookwork and problems, and it was the latter that were all-important since they effectively determined the student's place in the order of merit. The extent to which the order of merit – with its place in the national consciousness (reports in *The Times* etc.) – helped not only to cement the very particular (and ultimately unsatisfactory) style and content of the examination papers, but also affected the style and content of the curriculum is evident from the debates surrounding the 1907 reforms. The order of merit, by enforcing constraint on the curriculum, both conspired against those wishing to specialise in pure mathematics and kept the study of mathematics apart from experimental physics. A good idea of the main arguments for reform can be gathered from the following extracts by two of its most ardent supporters, E.W. Hobson and G.H. Hardy:

*E.W. Hobson, Response to the Board of Mathematical Studies, 1906*

If one considers the ideas connected with such names as Cauchy, Riemann, Weierstrass, Lie and Cantor, it would be recognised that those ideas had never permeated the teaching of Cambridge mathematics to a sufficient degree to form a real school of mathematics which

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<sup>3</sup>As well as textbooks, there were books devoted solely to Tripos problems and their solutions, the most famous of which is Joseph Wolstenholme's *Mathematical Problems on the First and Second Divisions of the Schedule of Subjects for the Cambridge Mathematical Tripos Examination* [8]. For a book containing fully worked Tripos solutions, see [9].

<sup>4</sup>Isaac Todhunter, who was the most prolific mathematics textbook writer of the 19th century, was also the author of a number of histories, see [7]

should be in line with the best Continental schools. the mathematical instinct, which had been restricted to a comparatively narrow circle of ideas, had avenged itself by producing a very great amount of material in the shape of riders, problems, illustrative examples, often produced with almost diabolical ingenuity. The enthusiasm of the best men had thus been damped, while upon those of less capacity it had produced the natural effect of severe indigestion.

*G.H. Hardy, The Cambridge Review, October 1906*

That the papers are absurdly difficult almost every one admits. They are not only too difficult but difficult in the wrong way. The difficulties are not the inherent and inevitable difficulties of the subject matter – no one will ever make mathematics easy – but artificial difficulties which have been invented by the examiners for the purposes of examinations. It is a tradition that a Tripos question must be ‘neat’: it must contain some little point of difficulty that cannot be found in any of the books. And when once the ingenious little question is printed it is lost: it is the property of every lecturer and coach. I believe that the questions have been hoarded for years in order that their proud inventor may adorn his papers with them when his time comes to examine in the Tripos. The result is that the Mathematical Tripos is a thoroughly bad examination.

No study concerning the 19th century Mathematical Tripos would be complete without taking into account another significant aspect of Cambridge life: the importance attached to physical activity, and in particular competitive sports.<sup>5</sup> Competition was deeply embedded into undergraduate culture and the Mathematical Tripos was one (admittedly extreme) element of it. As Norbert Wiener remarked, when visiting Cambridge just before World War 1, young mathematicians “carried into their valuation of mathematical work a great deal of the adolescent ‘play-the-game’ attitude which they had learned on the cricket field.” ([10], 152) The competitive style of mathematical teaching and examining imposed by the Tripos did not exist in isolation. Furthermore, success in the gruelling examination schedule required physical, as well as mental, strength and stamina.

Finally, lest it should be thought that prior to the 1907 reform the Mathematical Tripos had nothing but a stifling effect on mathematical creativity, it should be emphasised that this was not the case. Consider, for example, the early contributions of Ebenezer Cunningham (senior wrangler 1902) and Harry Bateman (senior wrangler 1903) to the development of relativity theory. And Karl Pearson, reflecting at the end of his life on his experience of the Tripos, wrote: “Every bit of mathematical research is really a “problem”, or can be thrown into the form of one, and in post-Cambridge days in Heidelberg and Berlin I found this power of problem-solving gave one advantages in research over German students, who

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<sup>5</sup>The relationship between mathematics and athleticism at Cambridge is extensively discussed in ([6], Chapter 4).

had been taught mathematics in theory, but not by “problems”. The problem-experience in Cambridge has been of the greatest service to me in life and I am grateful indeed for it.” ([11], (27))

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### Proving Impossibility

JESPER LÜTZEN

Many of the most famous theorems in modern mathematics are impossibility results. As examples one can mention the impossibility of constructing the three classical problems (the quadrature of the circle, the trisection of the angle and the duplication of the cube) by ruler and compass, the impossibility of solving the quintic by radicals, Fermat’s last theorem, the impossibility of proving the parallel postulate, and the impossibility of proving the consistency of arithmetic (in a certain sense) (Gödel).

If mathematics is viewed as a theorem proving activity there is really nothing special about impossibility results. The ordinary rules of logic make it possible to reformulate any impossibility result as a universal “positive” statement. However, if one views mathematics as a problem solving activity, impossibility results obtain a special status. Indeed, they do not solve a problem so they are not real results within mathematics (viewed this way) but statements about mathematics. They are meta-statements limiting the problem solving activity. Since Greek antiquity mathematical results have required proof, but with their status as meta-statements, it is not obvious that impossibility statements would have been considered as amenable to proof.

For example, Pappos (ca. 340 AD) considered cube duplication and angle trisection as impossible with ruler and compass, not only because “plane” construction

had not been found but because he thought that such constructions were in principle impossible. Yet he did not point to the desirability or possibility of a proof of this impossibility.

Similarly, when Lagrange in 1770 gave up finding a solution of the quintic by radicals, he nevertheless published the methods he had developed in his attack on the problem because he thought they might help his successors find the solution. He also mentioned the possibility that the quintic might be unsolvable by these means, but he did not indicate that his methods might be useful in a demonstration of this impossibility. And soon after when Ruffini used them for this purpose, he was ignored in France.

Even in cases where impossibility theorems were proved they were often considered as less interesting than “positive” statements. For example, as Goldstein (1995) has pointed out, Fermat’s impossibility theorems in number theory were received less favorably than his positive statements, and Wallis even commented: “I do not see why he [Fermat] mentions them [negative propositions] as things of surprising difficulty. It is easy to think of innumerable negative determinations of this sort.” (Quoted from [1], p. 135)

One can interpret Gauss’ way of dealing with the construction of regular polygons by ruler and compass in a similar way. Indeed, in the *Disquisitiones Arithmeticae* he took great pains to prove how regular polygons can be thus constructed if their number of sides is a power of two multiplied by a product of different Fermat-primes. He also claimed that it was impossible to construct any other regular polygons, but he left out the proof. This decision indicates that he thought that the constructive part of the result was more important than the impossibility part.

In a similar vein one can mention that when Wantzel proved the impossibility part of Gauss result, as well as the impossibility of a ruler and compass construction of the trisection of the angle and the two mean proportionals (a generalization of the duplication of the cube) his proof was overlooked for a century.[2]

Of course impossibility theorems were proved already in Greek antiquity. In particular the important discovery of incommensurable line segments is a result of the impossibility of finding a line that measures both the side and the diagonal of a square (or a pentagon) a whole number of times. But if the Greeks realized the possibility of proving this theorem, why wouldn’t they have realized that they should seek a proof of the impossibility of the classical problems? I think the reason is that these two impossibility results are qualitatively different in nature. The nature of an impossibility result depends on what is impossible or does not exist: It could be

- a. An object in the theory (a common measuring line in the incommensurability proof, or a number triplet in Fermat’s last theorem)
- b. A construction procedure in a theory (the classical problems or the solution of the quintic)
- c. A proof of a theorem in a theory (the parallel postulate for example)

## d. A proof of a property of the theory (Gödel)

The steps from problem-type a. through d. above represent in a way a rise of meta-level. There are many examples showing that at a given period a problem of a certain level could be considered provable whereas problems of a higher meta-level were considered meta-statements that were not amenable to proof. This fact also reflects that different types of impossibilities require different types of proofs ranging from simple indirect proofs to proofs by models.

As an example of the fate of an impossibility result let us briefly consider the two classical problems: the trisection of the angle and the duplication of the cube, or more generally the two mean proportionals. The first mathematician who thought of proving these two results seems to have been Descartes (1637) (for an analysis of this proof and that of Montucla see [3]). Although he translated the problems into cubic equations and hinted at an algebraic proof he ended up giving an entirely geometric proof along the following lines: Since circles only have one curvature they can only be used to construct one mean between two limits. Since the two problems in question require the construction of two means (the two mean proportionals and the two trisecting lines) they cannot therefore be constructed with plane means.

A somewhat less peculiar proof was given by Montucla (1754). It was based on the cubic equations of the two problems, and was considered by the author to be a triumph of the new analytic geometry. According to Montucla, an equation can only be constructed geometrically (that is by a certain procedure) by the intersection of two algebraic curves that can intersect each other in as many points as the degree of the equation (here 3). But since circles and straight lines intersect in at most two points they are not sufficient to solve the two problems.

The main problem of this proof is that it has not succeeded in translating the procedure of construction by ruler and compass into algebra. The first proper translation (into successive solutions of quadratic equations) was given by Gauss (1801) and used by Wantzel in his proof. Wantzel's proof is not entirely clear and actually has a hole (discovered by Robin Hartshorne) so the first completely correct proof may have been the one by Petersen (1870) popularized by Felix Klein (1895). The impossibility of the quadrature of the circle was first proved in 1882 by Lindemann as a simple result of his proof of the transcendence of  $\pi$ . Contrary to Wantzel's proof, that was overlooked, Lindemann's result was immediately celebrated.

This indicates that such impossibility results had changed their status during the 19th century. In fact already around 1830 many impossibility results were stated and proved: Abel proved the impossibility of solving the quintic by radicals, Wantzel proved the impossibility of the two classical problems, Liouville proved the impossibility of expressing elliptic integrals (and solutions of certain differential equations) in finite form etc. However it seems to have been a young man's game. Only at the end of the century did impossibility theorems obtain full citizenship in

mathematics. The reason seems to have been a reformulation of what constitutes a solution of a problem in mathematics.

Already Abel emphasized that one should not ask: “Find the solution of the quintic by radicals” but rather: “Is the quintic solvable by radicals?” In this way the impossibility statement becomes a solution to the problem.

This view of impossibility statements was wholeheartedly endorsed by Hilbert in his 1900 talk on mathematical problems at the International Congress of Mathematicians. He pointed out that “in recent time (der neueren Mathematik) the question as to the impossibility of certain solutions plays a preeminent role”. So according to Hilbert this central role of impossibility theorems was of a recent date. In particular he stated that the problem of the proof of the parallel axiom, the squaring of the circle, and the solution of the quintic by radicals “have finally found fully satisfactory and rigorous solutions, although in another sense than that originally intended.” Of course the sense was different because the result did not turn out to be a construction (a solution) but an impossibility result.

By including impossibility as a possible “solution” of a problem Hilbert, just as Abel before him, believed that all problems could eventually be solved, either by exhibiting a solution or by proving that it was impossible. In Hilbert’s words: there would be no ignorabimus in mathematics.

To conclude, we have seen that before 1800 impossibility statements were often considered unimportant meta-statements about the problem solving enterprise that do not lend themselves to proof. During the period 1830-1900 impossibility theorems gained full citizenship in mathematics partly because new techniques for their proof were developed (pre-Galois theory, models) and partly because they were considered as real mathematical results (solutions). In this way impossibility theorems followed the general trend toward more conceptual and qualitative ways of thinking in mathematics, and their development paralleled that of existence theorems.

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### **The search for finite simple groups 1830–2010: changing styles of thought and proof over 180 years of group theory**

PETER M. NEUMANN

We began with a preliminary questionnaire, a survey designed with two purposes in mind: first to give me some idea of what I could reasonably expect of the audience; secondly, to remind colleagues of the basic theorems of finite group theory and the

very basic tools that had been shaped during and soon after the end of the 19<sup>th</sup> century and were already at that time being used in the search for finite simple groups:

- (1) How many of us have once taken a course in group theory?
- (2) recall Lagrange's Theorem?
- (3) recall Cauchy's Theorem?
- (4) recall Sylow's Theorems?
- (5) recall the definition of a simple group?
- (6) recall the Jordan–Hölder Theorem?
- (7) have seen Transfer Theorems—of Burnside? Of Frobenius?
- (8) have met Burnside's  $p^\alpha q^\beta$ -Theorem?
- (9) have met Frobenius groups?

### Finite simple groups (FSG)

In his letter to Auguste Chevalier written on 29 May 1832 Galois made a definition of a *décomposition propre* of a finite group. In modern terms it was a partition into cosets of a subgroup  $K$  of the group  $G$  such that each right coset is also a left coset. Nowadays we speak of  $K$  as being *normal* and express Galois' condition in the equivalent form  $Kx = xK$  for all  $x$  in  $G$ . Then the group  $G$  is said to be *simple* if  $\{1\}$  and  $G$  are the *only* normal subgroups.

*Examples:*  $\text{Cyc}(p)$ ;  $\text{Alt}(n)$  for  $n \geq 4$ ;  $\text{PSL}(2, p)$  for  $p \geq 5$ .

Of these three series of finite simple groups the first and third were known to Galois, the alternating groups appear not to have been.

### The search for FSG pre-1900

After Galois' *Œuvres* were published by Liouville in 1846 it took a few years until a consciousness of the importance of FSG became evident. Camille Jordan wrote about the pre-eminent significance of the concept in the preface to his *Traité des Substitutions et des Équations algébriques* (1870), and in the second part of the book he proved the simplicity of the alternating groups (though this proof is not completely correct) and of various classical groups over prime fields, such as  $\text{PSL}(d, p)$ ,  $\text{PSp}(2m, p)$ , and many of the finite orthogonal groups. By the turn of the century, when L. E. Dickson published his *Linear groups* (Leipzig 1901), the catalogue had been extended to include the remaining finite orthogonal groups, the finite unitary groups, all these groups (new and old) over arbitrary finite fields,

and five sporadic groups, namely the Mathieu groups discovered in 1861. Soon after this Dickson discovered simple groups that were analogues of some of the exceptional Lie groups.

To have a large collection of FSG is one thing; to know that it is complete quite another. Otto Hölder began a systematic search for FSG with his 1892 paper on those of orders up to 200. By the time Burnside wrote the first edition of his *Theory of groups of finite order* in 1897 much was known; the second edition of this book (1911) contains much more: new tools such as transfer theorems and character theory had come available; the simple groups whose orders are at most 2001 or products of at most 5 prime numbers were now known; Burnside had proved his famous  $p^\alpha q^\beta$ -Theorem (that a group of such an order cannot be simple unless its order is prime). Perhaps most important of all was an open problem that Burnside had identified. In 1911, following a discussion of special properties of groups of odd order, and echoing and up-dating a paragraph from the first edition of his book he wrote

The contrast that these results shew between groups of odd and of even order suggests inevitably that simple groups of odd order do not exist. A discussion of the possibility of their existence must in any case lead to interesting results.

This wonderful conjecture was finally and famously proved by Walter Feit and John G. Thompson in 1962 (published 1963).

### **Fast-forward to my life-time**

In 1955 Chevalley published a wonderful paper in which he gave a uniform way of constructing the finite groups of Lie type from the root systems describing finite-dimensional simple Lie algebras; up to this time the groups were defined by their geometries, and although there was a certain uniformity to the constructions of the classical groups, the groups associated with exceptional Lie algebras were handled differently. This paper produced new insight into the catalogue of known FSG. Also in 1955 Richard Brauer, in part with his student K. A. Fowler, showed that the elements of order 2 (involutions) in a group of even order have a very strong influence on its structure—for example, the order of a finite simple group of even order is bounded by a function of the order of the centraliser of any involution. Then in 1956 Philip Hall and Graham Higman published a paper on the Burnside Problem (on groups of finite exponent), which turned out to provide new and wonderful tools for the internal analysis of groups. John Thompson was not slow to see the power of the Hall–Higman ideas. In 1959 he published his proof of an old conjecture on Frobenius kernels—namely that they are direct products of their Sylow subgroups—and soon he started tackling Burnside’s conjecture on groups of odd order. As was mentioned above, the proof was published by Feit & Thompson in 1963. This paper (and the lasting influence of the Brauer–Fowler ideas) marked the start of a campaign. Simple groups in which the Sylow 2-subgroups were abelian, or dihedral, or of sectional 2-rank at most 4 were classified—the list

goes on and on. Simple groups with a huge variety of interesting centralisers of involutions were classified. Thompson classified the minimal simple groups—the FSG in which all proper subgroups are soluble (that is, have no non-abelian composition factors).

But then new simple groups appeared. The Suzuki groups, now recognised as twisted versions of 4-dimensional symplectic groups over certain fields of characteristic 2; Tits groups and Ree groups, which are similarly twisted versions of certain Chevalley groups. And not only new series of groups—there were also new sporadic groups that did not fit into any series: first the small Janko group of order 175 560, then more and more, until the Monster was announced in 1973 by Fischer and by Griess (independently), and finally proved to exist by Griess in 1980. In all there were 21 new sporadic groups added to the five Mathieu groups in the few years from 1965 to 1980.

Early in the 1970s Danny Gorenstein ( “The Godfather”) announced and advertised in lectures in Chicago, New York, London, Jerusalem, and various other places, an ambitious programme designed to complete the classification. As is well known the announcement of the success of of this programme, owing to the work of a huge number of mathematicians, among whom Gorenstein himself and Michael Aschbacher were pre-eminent, was made in 1980. The classification of the finite simple groups (CFSG) was complete.

### CFSG and Revisionism

Or was it? It emerged after a few years that a key paper by Geoffrey Mason on the so-called quasi-thin groups was incomplete and was unlikely to be published. Moreover, a proof that extended over hundreds of papers and thousands of pages—could it possibly be error-free? Gorenstein almost immediately started the “revisionism” project, a project to complete and civilise the proof. Although he died in 1992, the project lives on and is coming to completion. It is being published by the AMS. The original authors are Gorenstein (ob. 1992), Richard Lyons and Ron Solomon, so the series is familiarly known as GLS. Two volumes on the quasithin groups were contributed by Michael Aschbacher and Stephen Smith in 2004, closing the notorious gap. Apart from those 2 volumes GLS is expected to come to 11 volumes, of which 6 have already been published and one or two more should appear soon. Others, some in the new generation of algebraists, such as Inna Kortagina-Capdebosq, a former student of Solomon, have joined the team, and the project still looks an excellent one, nearly twenty years after its inception.

Should we have confidence? That is a matter of personal conviction. It is also a matter of how much effort one is prepared to invest in order to understand some of the ideas. I myself am not an expert, but I have read enough of the proof that I certainly do have confidence. There may be slips or errors—indeed, it is hard to believe that a proof coming to some 5000 or 6000 pages in all could possibly not suffer from some defects—but I am confident that the tools are there to correct them. And in any event, those who quote CFSG have acquired the comfortable

habit of flagging up its use—rather as analysts or topologists who need the Continuum Hypothesis draw attention to points where it is needed. Thus theorems based on CFSG may either be read as unconditional results (by those who believe in it) or as results into whose assumptions the hypothesis that CFSG is correct must be added. We win either way.

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### **Experimental mathematics in the 1990s: A second loss of certainty?**

HENRIK KRAGH SØRENSEN

In most traditional accounts, experiments — one of the corner-stones of modern natural sciences — have had no place in mathematics. However, during the 1990s, with the advent of high-speed computers and sophisticated software packages a new experimental flavour was brought to parts of mathematics leading to the gradual formation of a branch of so-called “experimental mathematics” with its own research problems, methodology, conferences, and journals. The purpose of this paper is to situate the institutionalization of experimental mathematics in discussions within the mathematical community during the 1990s.

Despite early success in 1976 with the computer-assisted proof of the Four Colour Theorem, the full impact of the computer on mathematical practice was not felt until the mid-1980s. In 1985, when a new Cray-2 supercomputer was being installed at the University of Minnesota at Minneapolis, a group of remarkable geometers including Benoît Mandelbrot, David Mumford and William Thurston began work on a proposal for a *Geometry Supercomputing Project* to be funded by the NSF. That project would explore the power of computers for “visualization as a tool for experimentation, exploration, and inspiration in research” [9, p. 11].

Members of the project were instrumental in founding the journal *Experimental Mathematics* in 1991 with David Epstein and Silvio Levy as its editors. The journal was devoted to publishing experiments, new theorems, algorithms, practical issues, computer programs, a program column, and surveys and miscellanea [6, p. 1]. In introducing the journal, the editors alluded to a possible division of labour between hypotheses and proofs that would later be taken up with more force by Arthur Jaffe and Frank Quinn in their suggestion for a “theoretical” mathematics [8]. As the editors of *Experimental Mathematics* explained, the journal “was founded in the belief that theory and experiment feed on each other, and that the mathematical community stands to benefit from a more complete exposure to the experimental process. The early sharing of insights increases the possibility that they will lead to theorems; an interesting conjecture is often formulated by a researcher who lacks the techniques to formalize a proof, while those who have the techniques at their fingertips have been looking elsewhere” [6, p. 1]. Eight years later, in the opening issue of 2000, the same editors could celebrate the “maturity of the journal” [5, p. 1]: The journal’s output had grown by 30% between 1992 and 1999 and would increase from 420 pages annually in 1999 to 640 pages a year from 2000. Thus,

the journal established itself and the experimental approach to mathematics on the horizon of mathematical publishing in the 1990s.

At Simon Fraser University in Vancouver, another group formed in 1993 around the brothers Peter and Jonathan Borwein at the *Centre for Experimental and Constructive Mathematics* (CECM). That group has focused more on symbolic algebra and the use of computational methods in number theory. In a paper published in the *Mathematical Intelligencer*, the group announced their definition of the field: “*Experimental Mathematics* is that branch of mathematics that concerns itself ultimately with the codification and transmission of insights within the mathematical community through the use of experimental [...] exploration of conjectures and more informal beliefs and a careful analysis of the data acquired in this pursuit” [4, p. 17]. Thus, they also argued for a more inclusive view of mathematics and envisioned experimental mathematics as a dual dialectic between the computer and the human mathematician and between experiments and proofs [3, p. viii].

Among the results obtained by researchers affiliated with the group at the CECM is the so-called *PSLQ algorithm* which can be used for interactive, computerized searches for integer linear combinations of mathematical constants; see also [10]. It takes as its input a vector of high-precision real numbers  $(x_1, \dots, x_n) \in \mathbb{R}^n$  and after a specified number of iterations produces either a very good suggestion for a non-trivial integer linear combination  $(m_1, \dots, m_n) \in \mathbb{Z}^n$ , such that  $\sum_{k=1}^n m_k x_k \approx 0$  with high precision or a lower bound on the coefficients.

Members of the CECM group put the PSLQ algorithm to use in proving a remarkable formula which allowed the computation of individual hexagesimal digits of  $\pi$  without the computation of the previous ones. The authors described their process as applying ideas generalized from similar expressions for  $\log 2$  and “a combination of inspired guessing and extensive searching using the PSLQ integer relation algorithm” [2, p. 905]. The CECM group would advocate searching for traditional proofs of conjectures such as those obtained from the first case of the PSLQ algorithm; and for the above-mentioned formula such a proof could be found. It relied on yet another use of computers in performing standard calculations that go into the lemmas; such uses are now widespread and largely uncontroversial.

However, discussions emerged within the mathematical community over the need for traditional proofs of the more complicated computer-generated insights. Taking his inspiration from the new use of computers in visualization and proof, the science journalist John Horgan wrote an article entitled “The Death of Proof” for the *Scientific American* in 1993 [7]. There, Horgan captured the new dilemma of mathematics in the subtitle: “Computers are transforming the way mathematicians discover, prove and communicate ideas, but is there a place for absolute certainty in this brave new world?” and he suggested that the notion of proof was becoming an anachronism in mathematics.

A deliberate provocateur, the Rutgers mathematician Doron Zeilberger suggested in 1994 that “[a]s wider classes of identities, and perhaps even other kinds of classes of theorems, become routinely provable, we might witness many results

for which we would know how to find a proof (or refutation); but we would be unable or unwilling to pay for finding such proofs, since ‘almost certainty’ can be bought so much cheaper” [11, p. 14]. Continuing the argument that mathematics was discovering new lands and extending great frontiers, Zeilberger suggested: “I can envision an abstract of a paper, c. 2100, that reads, ‘We show in a certain precise sense that the Goldbach conjecture is true with probability larger than 0.99999 and that its complete truth could be determined with a budget of \$10 billion’” [11, p. 14]. Such provocation was met with fierce reactions, and George Andrews expressed the thoughts of a more conservative part of the community when he wrote: “Zeilberger has proved some breathtaking theorems [...]. However, there is not one scintilla of evidence in his accomplishments to support the coming ‘...metamorphosis to nonrigorous mathematics.’ [...] [H]e has produced exactly no evidence that his Brave New World is on its way” [1, p. 17]. Such discussions thus touched upon the epistemology of mathematics: It was obvious that so-called experimental methods could provide new heuristics for generating mathematical hypotheses, but whether new experimental methods also be allowed into the justificatory parts of mathematics was a very controversial issue, indeed, within the community.

In conclusion, the previous description has illustrated that to the protagonists of experimental mathematics in the 1990s, experimental mathematics was characterized not by a specific subject matter of mathematics, but rather by a technology (the computer), a somewhat vaguely specified methodology (the experiment) and a vision for an infrastructure (the electronic dissemination).

Based on these analyses, the development of experimental mathematics in the 1990s is not fruitfully analyzed within a disciplinary setting: Despite the developments of infrastructure and institutionalization, experimental mathematics remained cross-disciplinary in its subject matter, and its methodology and technology is increasingly integrated in most branches of mathematical research.

Instead, it is clear that efforts were made during the late 1980s and 1990s by the protagonists of experimental mathematics to promote an experimental approach as a style for doing mathematics. During that period, research institutions and journals were established, and software was developed to facilitate the methodology of interactive experimentation. However, aspects of that style were contested within the mathematical community and in the broader scientific and intellectual milieu. In particular, discussions about the conception of proof went to the core of the mathematical enterprise and an immediate reaction on the part of experimental mathematics was to confine the experimental approaches to the realm of heuristics and still demand traditional proofs. Such discussions over the potential epistemic roles of experiments in mathematics are still active within circles of experimental mathematics and within the community interested in the so-called *philosophy of mathematical practice*.

Some of the philosophical parts of this talk are being published in [10], whereas other parts are being prepared for publication.

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## On the Identities of Algebra in the 19th Century

CAROLINE EHRHARDT AND FRÉDÉRIC BRECHENMACHER

It is our aim to question whether algebra can be considered as a mathematical “discipline” during the 19th century or whether algebra took on much more varied and changing identities than the ones which can be described by resorting to a single category such as the one of “discipline”. In short, we are referring to the category “discipline” as identifying a corpus of specialized knowledge which resorts to institutionalized practices of transmissions and to a group of actors who are identifying themselves as “specialists”. This category must be considered as a dynamical one: as a result of the actions of the groups of experts, the definitions and delimitations of disciplines are in constant evolution. The use of the adjective “disciplinary” in expressions such as Kuhn’s “disciplinary matrix” or Bourdieu’s “disciplinary habitus” usually aims at taking into account both the social dimension and the cognitive or epistemological aspects of this category. Even though we cannot go into any further detail on the uses of the category “discipline”, these preliminary remarks are meant to highlight that, when wondering about the history of mathematical disciplines, it would be highly artificial to distinguish between internal and external approaches. If, indeed, one would consider Algebra as an immanent discipline for the purpose of a historical investigation, such an investigation would not only result in cutting slices of the mathematics of the past through a retrospective glance, but it would also miss the various social mechanisms of

intellectual differentiations through which mathematicians had given some algebraic identities to their work or had come to consider themselves as “algebraists”. In order to determine whether algebra can be considered as a discipline or not during the 19th century, it is thus convenient to analyze the roles that had been devoted by mathematicians to algebra within the mathematical sciences (including analysis, arithmetic, mechanics etc) as well as the relative and changing consensus about what should be “inside” or “outside” algebra. Two different situations are considered within the frames of two different periods of times as well as two different scales. First, we examine the meanings of the terminology “Algebra” in the first half of the 19th century through different kind of sources related to both the teaching of mathematics and to the academic sphere. Second, we appeal to a micro analysis of a controversy which opposed Jordan and Kronecker in 1874. Both approaches raises similar issues about the relevance of the “disciplinary model” for the history of algebra in the 19th century and highlight the major roles played by some practices specific to some communities, milieus, institutions or networks on the shaping of the identities given to algebra by various groups of actors.

### **I. How can we define what was Algebra at the beginning of the 19th century (1800-1835)?**

A usual approach is to look for the actors point of view, using a dictionary, or an encyclopedia. In the *Encyclopédie des gens du monde*, 1833, t. 1, p. 670, the definition is inherited from Condillacs philosophy: algebra is described as the language of calculation and confounded with analysis, that is with the analytical way to solve a problem (as opposed to the synthesis associated to geometry) (Bézout, Lacroix, Cauchy, 1843). A comparison with another definition, coming from the mathematician Gergonne and explicitly directed against the Condillacian analytical domination, is instructive : the distinction between algebra/analysis on the one side and synthesis on the other side is described as not relevant, because both ways of reasoning can be used to answer “algebraic questions” i.e questions related to general magnitudes on the one hand (including differentiation, integration, logarithms etc), and to the resolution of equations on the other hand (Gergonne, *Sur les méthodes de l'analyse et de la synthèse mathématiques*, 1813). Hence, distinction we would spontaneously make today between algebra and analysis by appealing to the notions of infinity or limits was not relevant at the beginning of the XIXth century. Actually, at that time, the ontological issues at stake were more related to ways of reasoning than to concepts. Moreover, the distinction between Algebra and Analysis was resorting to a hierarchy between the two designation: the first, considered as a tool for the second, did not have any specific object of inquiry. However, this image is somehow contradicted if we take another historical approach to reach the actors point of view by looking more closely to the mathematical contents, practices and problems hidden behind these general definitions. In the Procès verbaux of the Academy of science, algebra was not

a category often used by the geometers (as opposed to Analysis) and thus cannot be considered as an autonomous field of investigation in France at that time. In that institution, algebra was not defined by the specific part of mathematical knowledge or objects it was supposed to deal with, but by the kind of problem it was about. Another way to understand what were the problems and practices of algebra is to look to a field where “algebra” seems perfectly defined, namely the one of the teaching of mathematics. Algebra is one of the three parts of the high schools curriculum, together with arithmetic and geometry. In the case of Algebra, the more widely used textbooks until the 1840s had been written by Lacroix for the Ecoles Centrales, in 1799, and it partly took up the Bézouts and Clairauts textbooks written at the end of the XVIIIth century. Algebra thus inherited from a tradition, and, as such, had a real legitimacy. The *Cours complet* de Lacroix, the *Elements d’algèbre* and the *Compléments des éléments d’algèbre*, for instance cannot be considered as a the result of a vulgarizing process that would simplify the algebra developed by the geometers of the Academy but they highlight an autonomy of algebra within these textbooks which presented the subjects in the order they arose the ones from the others : the epistemological framework of the algebra one could learn from Lacroixs textbooks was totally different, and quite autonomous, from the normative image of scholarly algebra we have alluded to before. However, some similarities can be found if we look to the contents of these textbooks: algebra is introduced as the science of problems solving by the mean of equations, whose fundamental tool is symbolical calculation (including logarithms, exponentials, numerical solutions, finding the limits of the roots etc.). Lacroix pays a lot of attention to the concrete aspects of equations solving and explains that as the algebraic solution leads to calculations that are not effective, numerical solutions have to be developed : the fundamental skill the students had to acquire was the ability to calculate an approximate value of the roots of a given equation. On the one hand, this apprenticeship of algebra as a practice of calculation is perfectly coherent with the finality of the preparation for the competitive exam of the Ecole polytechnique where most of the questions dealt with analytical geometry, and nearly none of them took algebra as a subject. Hence, the algebraic methods that students learned in textbooks were actually used as a tool to solve the equations that one finds in more general problems. On the other hand, this context gives a very specific status to the advanced part of algebra, namely the one which is exposed in the second textbook and which is the closest to what we would call algebra today but was out of the core of knowledge that was needed for the problems students had to solve during the examination. Algebra as an autonomous field of knowledge, with specialized contents, was not a part of the mathematical culture that was instilled into the students of the French high schools. Finally, to conclude this first part, I could say that Algebra does not seem to be an autonomous field of knowledge at the beginning of the XIXth century, neither at the academy nor in the field of teaching. It cant be separated from what geometers called analysis neither from the core of knowledge it was dealing with, nor for its methodology. Then, a definition I would like to propose is the practice

of solving problems with the help of equations, which leads to concrete values.

## II. Practices of the solution of equations at the beginning of the 19th century.

As we shall see, the identity of algebra as an activity of solving problems with equations leads to questions for which we can't separate external and internal approaches. First, there is a hierarchy inside this field, which is created by how mathematics were taught. The differential and integral calculus was the core of the curriculum of the Ecole polytechnique and mastering this knowledge was what made a real geometer of a student. In this context, the numerical solution of equations was a kind of initiation to more advanced questions. On the contrary, the study of general (or algebraic) solution of equation was completely interrupted after the high school, if ever it was actually taught. Then, these problems had no strong link with the mathematical research: on the one hand, the would-be geometers were not trained to use this kind of knowledge and know-how, and, on the other hand, they didn't need them to be scientifically recognized. Moreover, this hierarchy was linked to the development of mathematics and to some epistemological conceptions : to make the theory of equations progress after Lagrange's *Traité de la résolution des équations numériques de tous les degrés*, one had to concentrate upon numerical solution, using the differential calculus if it was needed. Moreover, it would have had no sense to distinguish between one kind of tool or another because all of them were part of the "mathematical analysis" which was, according to Fourier "as broad as the Nature itself". Analysis was a general way of reasoning, that was not restricted to mathematics: its importance for human mind had been emphasized by the *Idéologues* and by Comte. Second, this hierarchy in the values associated to the different kind of equations one can solve was reproduced in the everyday work of the Academy. There were very few works sent at the Academy, which dealt with general solutions of equations. Most of the papers on this topic were aiming to improve methods for the equations of degree 3 or 4 and seemed to be directly linked with the social space of teaching. On the contrary, those who gain recognition for their research on the solution on equations were, in a large majority, the ones who studied the "advanced" part of the question, namely differential equations. Most of them were former students of the Ecole polytechnique, so they shared with the ones who would assess their work a specific way to deal with these questions. Besides, the evaluating practices of the Academy shows that the association of the problem of solving equations and the practice of effective calculation of numerical solutions can be considered as a value, namely that it was "the good way" to deal with such a question. In fact, the efficiency and the cleverness of the calculation processes are always underlined in the reports, and are a criterion to assess the papers. When one looks to the reports of the academy, one can see that the issue at stake was not whether these papers were "Algebra" or not. In fact, in the words of that time, all of

this was analysis, and the only criterion was to determine whether it was in conformity with the analytic approach to a problem. In this approach, the method of decomposition of the problem in simpler steps, the goal of making theory and applications work together, and the non-restrictive use of all the mathematical objects needed for that, may they be “elementary”, or “transcendant” can not be separated. Moreover, as we have seen, the positive values attributed to this specific way to undertake the solution of equations is strongly linked with the structure of the French mathematical milieu, whose heart was the Ecole polytechnique.

### III. The 1874 controversy between Camille Jordan and Leopold Kronecker.

Throughout the whole of 1874, Jordan and Kronecker were quarrelling over the organisation of the theory of bilinear forms and this opposition sheds some light on two conflicting perspectives on the identities taken on by algebra within mathematics. The controversy was originally caused by Jordan’s ambition to reorganise the theory of forms on the model of the algebraic organisation he had given to the theory of groups of substitutions in his *Traité* of 1870 and through what he designated as the algebraic notion of “canonical form”. In 1866, two papers published by Christoffel and Kronecker had laid the foundations of a theory whose main problem was the characterisation of bilinear forms - given  $P = \sum A_a x_a y$  and  $P' = \sum B_a x_a y$ , find the necessary and sufficient conditions under which P can be transformed into P’ by using linear substitutions - and whose methods were to look for invariants which would be unaltered by linear transformations. It was actually the problem of the simultaneous transformations of two forms P and Q which would shortly become the main question of the theory. Although the determinant of the “network”  $P+sQ$  was a polynomial invariant, the roots of the characteristic equation  $|P + sQ| = 0$  would provide a complete set of invariants only under the condition that no multiple roots existed. The general resolution to this problem had been given by Weierstrass in 1868 (for  $|P + sQ|$  not always equal to 0) who introduced a complete set of invariants computed from a comparison of the algebraic decompositions of the determinant  $-P+sQ-$  and its successive minors. Since then, Weierstrass’ elementary divisors theorem had become the main result of the theory of bilinear forms. Jordan thus stroke at the heart of the theory when he claimed that “the problem of the simultaneous reduction of two functions P and Q is identical to the problem of the reduction of a linear substitution to its canonical form”, and to the theorem he had stated in his 1870 *Traité*. When he responded to Jordan’s claims for the greater “simplicity” and “generality” of his methods of canonical reduction, Kronecker did not only reject the originality and validity of the latter methods but also the algebraic theoretical organisation relating to them. To Kronecker’s opinion, resorting to such algebraic expressions was legitimate provided that they would remain in their proper places of “methods” as opposed to the “notions” relating to the “other disciplines” - such as arithmetic - it was algebra’s duty to serve. What can we learn from this controversy? From

the standpoint of the contemporary discipline of linear algebra, Jordan's canonical form theorem for matrices with coefficients belonging to an algebraically closed field is equivalent to Weierstrass' elementary divisors theorem. Much a do about nothing ? There is actually a lot to learn from this controversy if we are wondering about the history of disciplines such a linear algebra. Methodologically speaking we shall wonder about Kronecker's and Jordan's opposite perspectives on algebra without focusing on the issues about the origins of abstract notions most authors have been dealing with while studying the history of linear algebra. The identity of the 1920-1930's discipline of linear algebra has often served as a lens for looking into the past, selecting relevant texts and authors, thereby giving structure to its own history while other identities that did not fit in this retrospective theoretical glance have stayed out of sight. The question therefore arises as to the identities and significations taken on by Algebra itself and the controversy between Jordan and Kronecker illustrates that we cannot take for granted that this identity resorted to the one of a single established discipline. We shall thus wonder about the multiple identities that algebra has been taking on within different time periods, corpuses, communities, public spheres, institutions etc. as well as within the work of individual mathematicians. What was specific to Jordan and Kronecker as individuals ? To the public spheres of the Academies ? To journals ? To communities and institutions ? Should this controversy be described as an opposition between Berlin and Paris a few years after the Franco-Prussian war ? Between group theory and the theory of forms ? It is impossible to tackle such complex issues on the interrelation of individual aspects of mathematical work and collective phenomena by distinguishing between an internal and an external approach to the history of mathematics. As we shall see, technical operator processes were usually resorting to cultural aspects specific to collective phenomenon of circulations of texts that have to be described.

#### **IV. The opposition of two practices. Cultural features peculiar to networks of texts.**

The 1874 controversy was underlain by an opposition over two practices - Jordan's algebraic practice of canonical reduction and Kronecker's arithmetic practice of invariant computation. The complex identities taken on by these two practices were resorting to the various networks of texts in which these practices circulated and highlight three different meanings given by Kronecker to algebra. First, Jordan and Kronecker's practices shared a common identity which can be considered as a common algebraic knowledge in the second half of the 19th century. In his addresses to the public spheres of both the Paris and Berlin Academies, Kronecker did not resort to the identity of a discipline to identify the importance of Weierstrass' theorem for Algebra but he alluded to a long history referring to the work of Lagrange, Cauchy, Jacobi etc., i.e. to a network of texts that can neither be identified to a theory nor to the resolution of a single problem but laid on a peculiar equation : the " equation to the secular inequalities in planetary theory ". For

the public sphere of the Academies, the algebraic nature of Weierstrass theorem was thus identified by the historical identity of a network of authors covering the period 1766-1874 and by alluding to what can be considered as a shared history, or a common algebraic culture at the time. The reference to this network was used in order to identify the algebraic identity of a practice which consisted in solving some specific linear systems by some polynomial expressions, this practice passed from a method to another, a theory to another<sup>1</sup>. Before the 1870s, even though this practice could not be identified to any mathematical identity that would be contemporary to us, it was not limited to some procedures but resorted to some cultural aspects specific to the network in which this practice circulated and that could not be dissociated from its “algebraic” status (such as specific meanings taken on by the terms “forms”, “transformations” etc.) Second, in addition to the broad algebraic culture identified by the network of the “equation etc.”, within *Crelle’s Journal* and the *Monatsberichte* in which he published his papers, Kronecker opposed to Jordan a second identity of algebra as opposed to arithmetic and which was related to another, more local, network. The opposition between an arithmetical and an algebraic theory of forms was indeed referring to a specific network of texts in which such a distinction had been introduced in the 1850s by considering different kind of classes of equivalences. This distinction therefore resorted to a cultural practice specific to a network involving especially Hermite and Kronecker which developed an “algebraic theory of form” based on the relations between Sturm’s theorem and quadratic forms which implied new considerations on the relations between algebra and arithmetics<sup>2</sup>. Third, within the Berlin community, Kronecker was also appealing to the local identity of the “theory of bilinear forms” as developed by Christoffel, Weierstrass and himself (which T. Hawkins has designated as “Berlin style of linear algebra”). And it was only within this community that Kronecker would develop a third identity of algebra which resorted to a more individual agency claiming the superiority of arithmetic over algebra. Kronecker especially blamed the tendency of algebraic expressions (such as Jordan’s canonical form) to develop formal (that is non effective) approaches which resorted to the extractions of roots of equations of arbitrary degrees. Kronecker appealed to the tradition of Gauss on behalf of his claim that the theory of forms should be considered as belonging to arithmetic and should consequently focus on the characterisation of equivalence classes in establishing arithmetical invariants thanks to some effective procedures such as g.c.d.s computations (i.e. invariant factors of a matrix in a principal ring). As long as they could not be effectively computed because they resorted to the solution of “general” algebraic equations, explicit algebraic formulas such as Jordan’s canonical

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<sup>1</sup>From the standpoint of linear algebra this practice could be considered as a method giving the general polynomial expressions of the eigenvectors of symmetric matrix  $A$  (such expressions are given by the columns of the matrix of cofactors computed from the polynomial matrix  $A - \lambda I$ )

<sup>2</sup>Compare with the discussion in [Goldstein and Schappacher, 2007, p. 52] about the “research field” status (in the sense of Bourdieu / as opposed to a discipline) of the “arithmetical algebraic analysis” that developed from Gauss work.

form had thus to be rejected because of their formal nature.

## Conclusion

These two case studies show that the identities taken on by algebra during the 19th century are neither restricted to the problems of origins or diffusion of abstract approaches that would later be related to the identity of algebra as a “mathematical discipline”, nor to the question of a transition from the “established discipline” of the theory of equation to another one that would be structural algebra. At the beginning of the 19th century, the word “algebra” was not even only related to ways of reasoning that one would spontaneously call “algebraic” today. In fact, the question of “what was algebra” during the 19th century seems to be strongly linked to the specific practices developed in particular mathematical networks, or in particular local communities. It is therefore compulsory to analyze the modalities of circulation of texts between networks, or between the different strata that structured local communities, like research and teaching. In the case of teaching and research, this circulation phenomena challenges the usual interpretation of knowledge that would go “up to down”, from research to teaching. In fact, at the beginning of the 19th century, algebra was much more “institutionalized” in high schools than it was at the Academy and there was also a relative autonomy of the “high school” algebra. Later on in the 19th century, Algebra took on various and changing identities depending on the communities, networks, disciplines, public spheres, institutions, journals, etc., in which “algebraic practices” were circulating. Such practices also often circulated between and interacted with various fields - such as mechanics, arithmetic or geometry - and passed from one theory on to another before the time of the emergence of a unifying disciplinary framework when algebra would take on a fundamental role in the organization of mathematical knowledge. More generally, the question of the identities taken on by Algebra raises the one of the development of a disciplinary system in the mathematical sciences. In order to understand what was algebra during the 19th century, it is compulsory to analyze how the actors themselves described their own activities. The problem is that neither the word “discipline”, nor the word “specialty” were used by the actors. Until the end of the 18th century, the divisions among sciences and arts depended on different ways of thinking (memory, imagination, reasoning, etc.). During the 19th century, they began to be divided in accordance to their objects, their principles, and their methods. It was a long-term process, and the case of Algebra shows that it was not a straight road.

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## Style and Rigor in Mathematics

HAROLD M. EDWARDS

One of the primary goals of my work in the history of mathematics is to make known the way in which Leopold Kronecker’s effort to base the mathematics of his and preceding generations on what he called “generalized arithmetic”—the algebra of polynomials with integer coefficients—was overruled at the end of the 19th century and never revived. This is a question of “style” in a very broad sense. Kronecker’s style of algorithmic and finitistic mathematics, which bases concepts and proofs on concrete polynomial constructions, satisfies my own demands, both aesthetic and technical, but is so antithetical to today’s transfinite set-theoretic constructions that it is rejected as unworkable today. The prevailing belief is that there is only one rigorous way to do mathematics and that it must be followed. My thesis is that mathematics would be enriched by opening the forum to other styles of thought and presentation.

I was encouraged by James Pierpont’s statement in his essay “Mathematical Rigor, Past and Present,” [5] that “Personally [I] do not believe that absolute rigor will ever be attained and if a time arrives when this is thought to be the case, it will be a sign that the race of mathematicians has declined.” Pierpont, after making this statement, so surprising to today’s mathematicians, goes on, not to attempt to describe rigor, but to “pass in review some examples of what were regarded at the time as good mathematical demonstrations.”

My article “Euler’s Definition of the Derivative” [2] presents a view of Euler’s standards of rigor that is very different from Pierpont’s, who states that, “Judged by modern standards [Euler’s] demonstrations are quite worthless.” I believe that, carefully read and properly understood, Euler’s demonstrations are as rigorous and

convincing as modern mathematics, as I tried to show in the specific case of Euler's treatment of derivatives. It is rejected today for reasons of style, not of rigor. The modern reader tends to believe Euler is describing limits in an inadequate way, but in fact Euler's definition the derivative does not involve limits at all.

A key attitude of the second half of the 19th century was expressed by Richard Dedekind when he said [1]: "My efforts in number theory have been directed toward basing the work not on arbitrary representations [*Darstellungsformen*] or expressions but on simple foundational concepts and thereby—although the comparison may sound a bit grandiose—to achieve in number theory something analogous to what Riemann achieved in function theory, in which connection I cannot suppress the passing remark that in my opinion Riemann's principles are not being adhered to in a significant way by most writers—for example, even in the newest works on elliptic functions; almost always they disfigure the theory by unnecessarily bringing in forms of representation [*Darstellungsformen* again] which should be results, not tools, of the theory." (Pierpont's translation.)

I summarized the argument I made against this statement of "Riemann's principles" in a talk I recently gave [3] with the title "The Algorithmic Side of Riemann's Mathematics." Invoking Riemann's work on the Riemann-Siegel formula, on establishing the analytic continuation and the functional equation of the zeta function, on transforming hypergeometric functions, and on conceptualizing and working with "Riemann surfaces," I tried to show that Riemann was not only a master of what Dedekind called *Darstellungsformen*, but also that they were very much tools, not results, of his theories.

Dedekind's attitude is repeated and even amplified in David Hilbert's statement in the introduction to his famous *Zahlbericht* [4] that, "I have sought to avoid Kummer's vast computational apparatus and thereby to realize Riemann's fundamental principle that proofs should be effected not by computation but solely by concepts" (my translation). To me, this approach to the subject not only deprives his readers of the experience of Kummer's fertile and beautiful techniques but is a degradation of their rigor insofar as Hilbert replaces the banned "computational apparatus" with "constructions" that are transfinite algorithms that fall far short of what Kummer and his student Kronecker would have regarded as rigor.

Kronecker's ideas of rigor are indicated in a famous statement, "If I still have the time and the energy, I will myself show the mathematical world that not only geometry but also arithmetic can point the path to analysis, and certainly a more rigorous one. If I cannot do this, then another will who comes after me, and the world will recognize the inexactitude of the types of proof now employed in analysis" (my translation). This and other indications Kronecker gave, as well as the body of his mathematical work, show clearly that he wanted to base all of his concepts and proofs on finite (but not necessarily practical) algorithms and computations. Pierpont shows sympathy and even admiration for Kronecker's view.

I differ from Pierpont, however, when he casts L. E. J. Brouwer as the mathematician of Pierpont's time whose principles were closest to Kronecker's. From the point of view of style, Kronecker and Brouwer could hardly be more different. Kronecker was a product of a classical German *Bildung* while Brouwer was a mystic. Kronecker was primarily interested in mathematics, not the philosophy of mathematics, and he was a careful student of both the classics of mathematics and the work of his contemporaries, while Brouwer worked in mathematics primarily to validate his philosophical principles, and worked in the new field of topology in idiosyncratic ways. Kronecker was a banker, while Brouwer was a prophet.

In my opinion, the association of Brouwer with Kronecker's program has done great damage to a proper understanding of what Kronecker's vision for mathematics was.

In conclusion, I believe that a broad exploration of various styles — in the sense of the word I have tried to indicate — would enrich mathematics and promote rigor in the only way that remains possible if one agrees with Pierpont that absolute rigor is a mirage. It would release the stranglehold that set theory currently has on mathematics and promote approaches to topics like number theory, algebraic geometry, and the classical theory of functions that are better adapted to these topics and that use more constructive, direct, and comprehensible methods.

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### Arithmetisation of Algebra and Structural Style

JOSÉ FERREIRÓS

In 1894 Richard Dedekind published a third, innovative version of his celebrated theory of ideals. It presented a different foundation of the core of this theory, based on development of a theory of "modules" (Z-modules in today's parlance), and it elaborated views that Dedekind had held from the 1870s, featuring a revolutionary exposition of Galois theory. Understanding Dedekind's vision of a new algebra is an important goal, since it is undeniable that his methods played a key role in developments from 1890 to 1940, i.e., in the emergence of modern algebra. We considered Dedekind's programmatic statements and aspects of his detailed presentation in 1894, establishing on their basis that Dedekind's approach must be understood as an *arithmetisation* of algebra. This program was certainly forward looking, but it also presents surprising traits. In the course of the presentation we

discussed the meaning of arithmetisation for Dedekind, contrasted our views with the influential analysis offered by Leo Corry, and analysed the abstract nature of Dedekind's methods as perceived by contemporaries like Frobenius or Hurwitz. Attention was given to the contrast between Hilbert's *Zahlbericht* and Dedekind's 1894 work, to the first appearance of the "ascending chain condition" famously linked with Noether's name, and to Dedekind's reformulation of Galois theory (see Dean, *unpubl.*) which underscores his rejection of the traditional focus on equations and his preference for an "arithmetisation" of algebra.

Dedekind strove to link intimately his advanced mathematical contributions with his foundational work. His understanding of "arithmetisation" was based on the view that "arithmetic (algebra, analysis) is only a part of logic" (Dedekind 1888). It suffices to know that the most important concept of "logic" was for him that of a *mapping* [Abbildung], and next the concept of *set* [System, Mannigfaltigkeit]. Thus in Dedekind's view *pure mathematics has to be focused on the number systems, but it is expected to employ set-theoretic or even map-theoretic methods.*

From the early 1870s Dedekind favoured a conception of algebra as the theory of the "relationship [or family relations] between the different fields" [*Verwandschaft zwischen den verschiedenen Körpern*], where by a field he meant a subfield  $K \subset C$ . This view of algebra recommended some radical departures from tradition, which in fact would not be adopted by the mathematical community. In particular Dedekind consciously tried to distance his work from the traditional focus on equations, and even from reliance on polynomials or other "forms of representation." This same radical attitude explains why Dedekind opted for the set-theoretic methods of his theory of modules, regarded by Emmy Noether as a model of methodology, and for a map-theoretic approach to Galois theory employing groups of field automorphisms, which would be taken up and popularized by Emil Artin. On this basis I proposed that the correct interpretation of Noether's "*es steht alles schon bei Dedekind*", is methodological: The "all" in Dedekind was the new methods he developed, the spirit of his mathematical style, including axiomatic analysis - but not the modern abstract structures (see Corry 2003).

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## Mathematics in Spain: 1800-2000. From translated to self production

CARLOS SUAREZ ALEMAN

Toward the beginning of the 19th century, textbooks produced by Spanish scientists were only composed of translations from foreign, principally French textbooks ([15]). There were two important exceptions. The first was by Josef Chaix Isniel (1765-1809) who published the book *Instituciones de Calculo Diferencial e Integral* in 1801 [8]. In this book, Chaix developed the principles of calculus and surface curves as well as curves of double curvature theory following Euler, Clairaut and Monge. An important aspect is that he declared himself a follower of the idea of using limits for the foundation of calculus as opposed Lagrange's method. At the time this approach was advancing in France, it was entering in Spain for the first time. We are preparing a facsimile edition of this book accompanied with articles about Isniel and his life together with notes about his works.

The second original writer was Jose Mariano Vallejo y Ortega (1779-1846), author of the book *Compendio de Matemáticas Puras y Mixtas* from 1819. In the third edition (1835), he demonstrated a method for the numerical solution of polynomial equations. In the fourth edition of this book (1840) the reader is informed on the front page that it contains: "A new, simple, general and reliable method to find real roots of numerical equations of any degree, even those that resist all means and resources offered by mathematics, *even those provided by the infinitesimal calculus*" [14]. Vallejo contributed a formulation of the *Regula Falsi* and secant methods in almost contemporary terms, hoping that this could yield a calculus-free approach to the problem of solving equations numerically.

The second half of 19th century in Spain was an epoch of many translations. Important books were translated into Spanish, including the geometry of Chasles, works on determinant theory, Galois theory or elliptic functions, etc. In this way, by the end of the century some mathematicians in Spain began to establish contacts with mathematicians from other countries.

We should emphasize the work of the Spaniard Antonio Portuondo Barceló (1845-1927), whose book *Ensayo sobre el infinito* from 1880 is an isolated work unrelated to any previous writing by the author. Still, he was rather proud of it, as shown by his publishing an excerpt of it thirty-two years later, in 1912, under the title "Les Lois Infinitésimales dans l'Analyse Mathématique" in the *Revue Générale des Sciences Pures et Appliquées*. The book is devoted to the development of a theory about the ordering and valuation of infinitesimal laws, computations with infinitesimals and it presents a series of theorems needed in the development of mathematical analysis. In the Nota he added remarks on an analysis of the possible relationships between two infinitely small (resp. large) functions, written in a spirit very similar to that of DuBois-Reymond's 1875 article, though we believe that Portuondo did not know it ([12]).

At the end of the 19th century Ventura Reyes Prosper (1863-1922) became the first Spanish mathematician to publish in foreign journals. It is known that he developed a lasting friendship with Klein and Lindemann. He worked principally in synthetic geometry and in 1887 published "Sur la géométrie non-Euclidienne"

in *Mathematische Annalen* where he simplified a proof of Klein about the construction of the fourth harmonic point. In 1888, he published “Sur les propriétés graphiques des figures centriques (Extrait d’une lettre adressé a Mr. Pasch)” in *Mathematische Annalen* about a proof of Desargues Theorem for a special case.

The first half of the 20th century was marked by the Great Wars and by the Dictatorship of General Franco, which slowed the development of mathematics in Spain. However, there is at least one important figure to be mentioned: Julio Rey Pastor (1888-1962). He was a student of Schwarz, Frobenius, Schur and Schottky in Berlin, of Carathéodory, Hilbert and Courant in Göttingen and was the most important and influential Spanish mathematician at the beginning of the 20th century. In fact many important mathematicians were considered successors to Rey Pastor: Ricardo San Juan, Sixto Rios, Antonio de Castro, etc. and many mathematicians consequently dedicated their articles to Pastor.

Much less well known is Norberto Cuesta Dutari (1907-1989), an interesting example of an ‘outlier mathematician’ (see [7]). He served as chairman at the University of Salamanca from 1958 until his retirement in 1977. Cuesta’s most important work was published under the title *Matemática del Orden* by de Academia de Ciencias. This was his doctoral thesis published in a series of four articles in *Revista Matematica Hispano-Americana*. He never published in a foreign language, probably a consequence of the dictatorship.

Cuesta’s case shows the importance of investigations about Spanish mathematicians who did their work under the dictatorship. Interesting, yet unknown works whose dissemination was limited because they were only available in Spanish, will probably be discovered in this process.

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## How to Define Geometry of Numbers as a Discipline?

SÉBASTIEN GAUTHIER

The aim of the talk is to address the question of the usefulness of the notion of “discipline” as a category of analysis for historians through the example of the geometry of numbers. “Discipline” appears as a natural category because, for instance, it is a category of the actors. But they seem to have a spontaneous knowledge of what their discipline is which makes the notion difficult to use for historians. We can illustrate this point quoting André Weil speaking about number theory [6]:

“Perhaps, before I go on, I ought to say something about what number-theory is. Housman, the English poet, once got one of those silly letters of inquiry from some literary magazine, asking him and others to define poetry. His answer was “If you ask a fox-terrier to define a rat, he may not be able to do it, but when he smells one he knows it.” When I smell number-theory I think I know it, and when I smell something else, I think I know it too.”

We show in the talk that questioning what could characterize the geometry of numbers as a discipline is a fruitful heuristic approach to study its developments.

### 1. THE GEOMETRY OF NUMBERS: A NATURAL CATEGORY FOR MATHEMATICIANS

It is usually considered that the domain of researches “geometry of numbers” began at the end of 19<sup>th</sup> century with the work of Hermann Minkowski who introduced the expression *Geometrie der Zahlen*. A first way to grasp the geometry of numbers as discipline is to use classifications of mathematical domains, for example, the *Jahrbuch über die Fortschritte der Mathematik* and the Mathematical subjects classification used by the *Mathematical Reviews*. The study of these classifications give information about the perception of the geometry of numbers by the community of mathematicians from a collective point of view. It suggests that the geometry of numbers is considered as a subdiscipline of number theory and that this conception remains stable throughout the 20<sup>th</sup> century.

The discourses of mathematicians about the geometry of numbers confirm this observation. Moreover, from Minkowski to contemporary comments, one can find the same definition for the geometry of numbers: it would consist of using a geometrical point of view to solve arithmetical problems. This definition which

seems to characterize the geometry of numbers in the long-term raises several problems, in particular, there is no reason that geometry, arithmetic (and their concatenation) have the same meaning for mathematicians involved in researches in the geometry of numbers. The study of specific cases shows these differences and also reveals various disciplinary conceptions for the geometry of numbers. This has been illustrated in the talk with the examples of Minkowski, Mordell and Davenport.

## 2. DIFFERENT DISCIPLINARY CONCEPTIONS: MINKOWSKI, MORDELL AND DAVENPORT

The work of Minkowski about the geometry of numbers can be accurately described with internal criteria. It is characterized by:

- fundamental objects: lattices, distance functions or, alternatively, convex bodies,
- a fundamental theorem: Minkowski's convex body theorem,
- a fundamental method: the one used by Minkowski to prove the fundamental theorem.

This conception of the organization of the geometry of numbers is not shared by Mordell and Davenport. Their work is structured around the study of different problems, for example, the minimum of the product of  $n$  linear forms and some particular cases of this question. Consequently, they are interested in the different objects which appear in previous problems and they use many results and many methods to study these objects. In the case of Mordell and Davenport, we have to consider collective aspects of their work that we can see through correspondences, seminars and teaching. This example shows another disciplinary configuration different from Minkowski: here, there is not a limited core of objects and methods but the geometry of numbers is more defined by a set of collective practices.

## 3. THE GEOMETRY IN THE GEOMETRY OF NUMBERS

The study of the mathematical practice of Minkowski, Mordell and Davenport enable us to clarify what the general description of the geometry of numbers – the use of geometry in number theory – means for these mathematicians [3]. For Minkowski, the geometry is associated with intuition and often consists in the geometrical representation of the question studied.

The meaning of the geometrical point of view in Mordell's and Davenport's work is the interpretation of an arithmetical problem in terms of the research of a lattice point in a fixed domain.

## 4. CONCLUSION

We have several evidences that the geometry of numbers is perceived as a natural discipline by mathematicians. They seem to have a discourse of continuity about this subject as shown by the general definition of the geometry of numbers which appears in the long-term.

But at another scale of analysis, the study of the practices of mathematicians gives another image of the geometry of numbers. There is in fact no obvious characterization of the geometry of numbers as discipline. “Disciplines are historical objects”<sup>1</sup> and an aim for the historians should be to determine what define a discipline in each specific situation. In that sense the category of discipline can be useful as it helps to reconstruct the developments of a subject like the geometry of numbers.

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### Two episodes in late 19th century research on convex bodies: Style and towards a discipline

TINNE HOFF KJELDSSEN

The beginning of the modern theory of general convex sets is attributed to Hermann Brunn and Hermann Minkowski. They both figure prominently in introductions to text books and monographs on convexity [1]. In such introductions it is neither revealed that Brunn and Minkowski studied convex bodies independently of each other, for very different reasons, and in very different ways, nor that it was Minkowski’s strand of thoughts that led to the development of a theory of convexity.

In this talk Brunn’s and Minkowski’s approaches to convex bodies were compared and discussed in the framework of the theme for the conference, in order to understand why they began investigations of bodies only characterized by the property of convexity, and how they did it. It was argued that there was a big difference in style between the two, and that the “move” towards a discipline of convexity can be seen as a consequence of Minkowski’s work.

Hermann Brunn (1862-1939) took up the study of what we today would call general convex bodies in his inaugural thesis *Ueber Ovale und Eiflächen* from 1887.

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<sup>1</sup>G. Lenclud, “L’anthropologie et sa discipline”, in [1], 69–93.

In the preface he explained that his thesis consists of “Elementary geometrical investigations of a special kind of real curves and surfaces - ovals and egg surfaces” [2]. He defined objects that he named “Oval”, “volles Oval”, “Eifläche” and “volles Eifläche”. These objects correspond to the boundary of a convex body in the plane, the boundary together with its inner points, and the corresponding objects in three-dimensional space. Brunn’s thesis is a study of various kinds of properties about these objects. In the last part of his thesis he dealt with cross sections in “egg”-forms and extremal properties. It is in this section we find Brunn’s version of what became known as the Brunn-Minkowski inequality. Minkowski used his version of the inequality in a paper from 1901 [6] to give a new and more rigorous proof of the extremal property of the sphere, whereas Brunn claimed that it could not be used to prove this property [2].

It is unclear why Brunn began to study ovals and egg-forms. It seems that he just started from scratch so to speak, as he wrote at the end of the thesis:

however, the complete work is intended to show that also geometrical figures of unusual few specialized construction laws after all allow some statements that are not quite obvious [2]

Brunn was committed to Steiner’s views on geometry, as he himself phrased it

I was not entirely satisfied with the geometry of that time which strongly stuck to laws that could be presented as equations quickly leading from simple to frizzy figures that have no connection to common human interests. I tried to treat plain geometrical forms in general definitions. In doing so I leaned primarily on the elementary geometry that Hermann Müller, an impressive character with outstanding teaching talent, had taught me in the Gymnasium, and I drew on Jakob Steiner for stimulation. [3]

Here Brunn was referring to the controversy of the 19th century between the analysts and the synthesists, and he held the opinion that using synthetic methods was the right way to argue in geometry.

In contrast to Brunn, Hermann Minkowski (1864-1909) was one of the leading mathematicians of his time. He came to - or stumbled over - convexity due to the way in which he approached the minimum problem for positive definite quadratic forms in  $n$  variables. The minimum problem is to find the minimum value of such a form for integer values of the variables not all zero. The innovative element of Minkowski’s work was that he approached the minimum problem for  $n$  dimensions geometrically by interpreting a positive definite quadratic form geometrically, constructing a lattice of congruent parallelotopes through which he reformulated the minimum problem to the problem of finding the smallest distance in the lattice. He was then able to find an estimate for the upper bound of the quadratic form for integer values of the variables by a simple comparison of two volumes [4]. At some point, probably around 1891, Minkowski realized that the essential property for his argument for comparing the two volumes was the property of convexity.

He then introduced a radial distance between two points along with its corresponding unit ball, or “Eichkörper”, as he named it. He argued that the

“Eichkörper” of a radial distance for which the triangular inequality holds is convex, or nowhere concave as he called them at that time, and vice versa that every nowhere concave body which has the origin as an inner point, is the “Eichkörper” of a certain radial distance for which the triangular inequality holds. He then reformulated his result for the minimum problem into his famous lattice point theorem:

Ein nirgends concaver Körper mit einem Mittelpunkt in einem Punkte des Zahlengitters und von einem Volumen  $= 2^n$  enthält immer noch mindestens zwei weitere Punkte des Zahlengitters, sei es im Inneren, sei es auf der Begrenzung. [7]

Minkowski reached his results through geometrical intuition, as he explained in the advertisement for his 1896 book *Geometrie der Zahlen*:

I have chosen the title Geometry of Numbers for this work because I reached the methods that give the arithmetical theorems, by spatial intuition. Yet the presentation is throughout analytic which was necessary for the reason that I consider manifolds of arbitrary order right from the beginning. [7]

If we compare Brunn’s and Minkowski’s styles, we can see that Brunn was what could be called a purist in style. He had one context of argumentation, namely within synthetic geometry. Minkowski on the other hand used a diversity of styles which provided him with several contexts of argumentation. He used geometrical intuition in number theory, and he treated the geometry of numbers analytically. Thereby, his work became situated in a much richer context than Brunn’s which created connections between various (sub)disciplines of mathematics. New ideas such as the lattice point theorem and the field of geometry of numbers, and new objects such as the “Eichkörper” and nowhere concave bodies, emerged.

There are other differences that help to explain why the further development came out of Minkowski’s strand of thoughts. First, Minkowski’s mathematics on convexity came out of his effort to solve mathematical problems that were recognised as important problems. This was not the case with Brunn. Second, Brunn’s and Minkowski’s objects were very different in nature. Brunn’s were quasi-empirical objects that existed in plane and space. Minkowski worked on abstract mathematical entities that “lived” on  $n$ -dimensional manifolds. Third, Minkowski’s work was much more general and convex bodies had kind of proved themselves as powerful tools in his work.

The comparison shows the differences in the way Brunn and Minkowski worked with mathematics and produced new knowledge, as well as the significance of these differences for whether their investigations of the new objects had the potential to develop into a mathematical discipline [5].

Minkowski then began to study convex bodies for their own sake publishing several papers on convexity where he worked on convex bodies completely detached from quadratic forms and number theory as such. He gave a systematic treatment of convex bodies in 3 dimensions, introduced the now standard notions of distance

function for convex bodies with the origin as an inner point, supporting hyper planes, separating hyper planes, mixed volumes etc.

From there on the number of manuscripts and materials on convex bodies or using convex bodies multiplied and around 1932 Neugebauer suggested that Tom Bonnesen (1873-1935) and Werner Fenchel (1905-1988) should collect, organize and systematize all of this. The result of their joint effort was the book *Theorie der konvexen Körper* [1]. The appearance of Bonnesen's and Fenchel's monograph can be seen as a the first step of the formation of convexity as a (sub)discipline of mathematics. It indicates that there at that time were a specialized body of knowledge and a group of practitioners.

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### **The evolution of the concept of projective space (1890-1935): from geometry to algebra**

JEAN-DANIEL VOELKE

This talk constitutes a summary of a forthcoming paper. It analyses the evolution of the concept of projective space from the epoch of Poncelet until our days and gives an example of the change of status of a discipline (projective geometry). It is divided into four parts. I begin by presenting the current definitions of projective space. I'll then examine the origins of these definitions. In the third part, I'll explain in which context the notion of projective space appeared at the end of the 19th century. I'll end by showing how this notion evolved during the 1930s and finally took its current shape.

Let us begin with the modern definitions. Nowadays, there are two manners of defining a projective space: an axiomatic one and an analytic. In the first one, a projective space is defined as a set of objects (points and straight lines) satisfying some axioms. In the second one, a projective space of dimension  $n$  is defined as the set of sets of  $n + 1$  homogeneous coordinates chosen in a field, or as the set of straight lines of a vectorial space of dimension  $n + 1$ . If you suppose that the theorem of Desargues is valid in an axiomatic projective space (it is the case if the dimension is greater than 2), you can introduce coordinates and show

that it is isomorphic to an analytic projective space on a field. Consequently, the axiomatic and analytic ways meet. These two definitions have their roots in the two main streams in projective geometry in the 19th century: the synthetic method of Poncelet, Steiner and von Staudt and the analytic one of Möbius, Plücker and Hesse.

I would like to discuss some characteristics of these mathematicians' research which are necessary to understand the origins of the concept of projective space. I'll have to distinguish the first group, Poncelet, Steiner and von Staudt, from the second one, Möbius, Plücker and Hesse. From my point of view, the crucial issue is the treatment of the elements at infinity. Indeed I shall later argue that the concept of axiomatic projective space appeared when mathematicians stopped differentiating proper from ideal elements. Let us first recall that the notion of ideal point or at infinity was introduced by Desargues. For him, such points were represented or conceived as the intersection of parallel lines. At the beginning of the 19th century this idea was commonly accepted by professional mathematicians. In their books, Poncelet, Steiner and von Staudt, the users of the synthetic method, first considered an affine space which they completed by ideal points. But they didn't conceive them as ordinary points. They rather conceived them as a linguistic tool which allows unification of some statements. Consequently, even if these mathematicians dealt with projective geometry, the concept of projective space was absent from their writings.

The situation among the users of the analytic method was a little different. At the end of the 1820s, Möbius and Plücker defined systems of homogeneous coordinates. Their main motivation was to gain simplifications in calculation and not to develop an analytic tool for calculating with ideal points. This possibility rather appeared as a by-product of their research; however, they used it without any problems. In practice they made less discrepancy between proper and ideal points than Poncelet and Steiner. A decade after Möbius and Plücker, Hesse began to use a particularly simple system of homogeneous coordinates. The ordinary coordinates  $x, y$  from a point are replaced by homogeneous coordinates  $X, Y, Z$  satisfying  $x = \frac{X}{Z}, y = \frac{Y}{Z}$ . Hesse's motivation was similar to the one of Möbius and Plücker: it is often more practical to calculate with homogeneous polynomials. Hesse didn't mention the issue of ideal points. A few years later his method was adopted by Cayley and generalised to  $n$  dimensions. It was then used by Italian mathematicians as D'Ovidio and Segre and at the beginning of the 1880s a new concept appeared in Italy: the linear space. The points of such a space are the sets of  $(n + 1)$  real or complex homogeneous coordinates. In fact, a linear space is the same thing as an analytic projective space. I encountered this last term the first time in a paper written by Killing in 1893. In conclusion, the situation was not the same among the users of the synthetic method as among the ones of the analytic method. The framework of the second ones was already the analytic projective space.

I will now discuss how the notion of projective space appeared during the 1890s. After the axiomatization of geometry given by Pasch in 1882, a new interest for

this issue developed among mathematicians, especially in Italy. In 1891 Segre published a paper in which he noticed that nobody had yet given a system of postulates characterising the linear space of  $n$  dimensions. In other terms, Segre called for an axiomatization of the analytic projective space. His paper challenged two young mathematicians: Amodeo and Fano. The result was the publication of two papers resolving the issue proposed by Segre. I consider that these papers marked the birth of the axiomatic projective space. Significantly Amodeo used this term. In the axiomatics of Amodeo and Fano two coplanar straight lines always cut each other in a point and there is no distinction between proper and ideal points. The classical process of complementation used by Poncelet and his successors disappeared and we have from the very beginning the axiomatic projective space. The method of Amodeo and Fano was then used by Pieri in several papers on the axiomatics of projective geometry published at the end of the 19th century. He insisted on the independence of this geometry from the euclidean one and regularly used the term “projective space”. He also formulated a specific system of axioms of the complex projective space. This paper showed that there are indeed different projective spaces. At the beginning of the 20th century, after the publication of Hilbert’s *Grundlagen der Geometrie*, new research in axiomatics was undertaken in America. The main mathematician was Veblen. After writing several papers on this subject, he published a book *Projective geometry* with Young in 1910; this book remained the reference in this field for many years. It adopts the axiomatic method and offers a synthesis of the main recent research done in this area. If we focus on the biography of projective space, this book is important because this concept is presented simultaneously from the analytic and the axiomatic points of view. Veblen and Young define an analytic projective space of 3 dimensions as the set of all proportional quadruples of numbers not all equal to zero. They show that such a space constitutes a model of an axiomatic projective space. Reciprocally, they show that coordinates can be defined in such a space and that it is isomorphic to an analytic one. Finally, Veblen and Young take into consideration finite projective spaces too. For all these reasons, we can say that the notion of projective space became mature with their book.

I would now like to examine the subsequent evolution of the notion of projective space during the 1930s. As Corry showed it in his book *Modern Algebra and the Rise of Mathematical Structures*, this period was characterised by the development of a structural conception of algebra. It influenced the conception of projective geometry too. In the middle of the 1930s, Birkhoff and Menger showed independently that projective space can be defined as a finite, modular, complemented lattice. At the same time, Schreier and Sperner published their famous book *Analytische Geometrie* in which projective geometry constitutes only a chapter. They chose the analytic definition of projective space and remarked that there is a correspondence between the points of such a space and the straight lines of a vectorial space. With these different publications, projective geometry lost its independence and was integrated into more general algebraic theories. Projective space was conceived as a particular lattice or as the set of straight lines of a vectorial space.

Such a definition was adopted for example by Bourbaki or by many authors who included projective geometry in treatises of linear algebra. At the beginning of the 1950s, it seemed that projective geometry wasn't a geometrical discipline any more but an algebraical one. Nowadays, opinions are less extreme. The time of ideologies is past. The different approaches of projective geometry coexist peacefully and mathematicians pass easily from one to the other one depending on their needs.

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### Oscar Zariski and Alexander Grothendieck

ROBIN HARTSHORNE

Oscar Zariski and Alexander Grothendieck, each in his own way, brought radical changes to the field of algebraic geometry in the twentieth century. The purpose of this talk is to give a brief look at the lives of these two men, to say what they did, and to compare and contrast their styles of doing mathematics.

Oscar Zariski was born into a Jewish family in Kobrin, Russia in 1899. He studied in the Russian gymnasium and at the University of Kiev until 1920. When

Kobrin became a part of Poland, he could obtain a Polish passport and travelled to Italy. At the University of Rome (1921-1927) he learned algebraic geometry at the feet of the great Italian masters Castelnuovo, Enriques, and Severi. During this time he also met and married a young Italian woman, Yole Cagli, who remained with him until the end of his life. Even while admiring the accomplishments of the Italian algebraic geometers, Zariski felt the need for new methods to go farther. He accepted a position at Johns Hopkins University in Baltimore, which he held from 1927-1945, with visits to other places such as Harvard, Illinois and Sao Paulo in between. During this time he was able to visit Lefschetz in Princeton, and apply his topological methods to the study of the fundamental group of algebraic varieties. During this time also he wrote his book on Algebraic Surfaces (1935), in which he explained the ingenious geometric methods of the Italian school. As he says in the preface to his collected works, he may have succeeded, but at a price. "The price was my own personal loss of the geometric paradise in which I was living theretofore." [[4], vol. I, p. xi]. He felt the need for more solid algebraic foundations for the geometry. These he created, drawing on the work of E. Artin, E. Noether, and Krull, in a series of foundational papers from 1939 to 1944. For the rest of his life he worked on many aspect of algebraic geometry, using these foundations and creating more tools as he went along. His main contributions were to the resolution of singularities, the classification of algebraic surfaces, the theory of linear systems and the Riemann-Roch problem, the theory of birational transformations and minimal models, the theory of holomorphic functions and, in his later years, equisingularity. He also expanded the domain of the subject, recognizing the need to work over non-algebraically closed groundfields, and over fields of characteristic  $p > 0$ .

As a result of Zariski's work it was no longer possible for a researcher in algebraic geometry to work in the purely geometric style of the Italian masters: one had to have proper algebraic foundations for one's results. (Unfortunately, I do not have space in this paper to report on the parallel algebraic foundations developed by van der Waerden, André Weil and others.) Zariski's report to the International Congress of Mathematicians in 1950 summarized the algebraic transformation that had overcome the field of algebraic geometry since his student days in Rome. This article makes curious reading today, since it came on the eve of the next great revolution in algebraic geometry due to Serre and Grothendieck.

Alexander Grothendieck was born in Berlin in 1928 to a Russian Jewish anarchist father and a German mother. His early years were chaotic as his parents lived on a shoestring. From age 6-11 he lived with a foster family in Hamburg while his parents went to fight in the Spanish Civil War. In 1939 he was sent to France, where he lived with his mother in an internment camp. His father was deported to Auschwitz in 1942. He was able to attend school, and later (after the war) study mathematics at the University of Montpellier. In 1948 he went to Paris with an introduction to H. Cartan. Cartan sent him to Nancy to work with Laurent Schwartz. Schwartz reported in his autobiography that he had just finished a big paper with Dieudonné with fourteen unsolved problems at the end.

They suggested that Grothendieck might try his hand at one that appealed to him. They did not see him for a while, but when he reappeared several weeks later, he had done half of them with deep and difficult solutions, and introducing a number of new concepts. They realized they were dealing with a first-rate mathematician [quoted in [2], p. 161]. This was the beginning of his intensive work (1950-56) on topological vector spaces.

Then, with Serre's paper *Faisceaux algébriques cohérents* (1955) and his experience in the Cartan seminar came Grothendieck's plunge into algebraic geometry. During the period 1957-1970 he worked essentially non-stop, producing several lifetimes' worth of mathematics, and rewriting the foundation of algebraic geometry in terms of the theory of schemes, a vast generalization of the old algebraic varieties. His work during this time includes

- The *Eléments de Géométrie Algébrique* (EGA) with J. Dieudonné, 8 volumes, 1960-67.
- *Séminaire de Géométrie Algébrique* (SGA) with various coauthors, SGA 1- SGA 7, 1960-69 (more than 6000 pages in all)
- 15 talks to the *Séminaire Bourbaki* on his new results
- Not to mention numerous other published articles and seminar talks.

After 1970 he largely withdrew from the world of research mathematics. Although he continued to teach at the University of Montpellier until his official retirement in 1988, and produced several unpublished manuscripts of mathematics, he shunned most other mathematicians. Since 1991 he has lived in an unknown location in southern France, and refused most contact with other people.

Now a few comparisons between the works of these two men. Both left their birthplaces and moved through several different cultures. Both changed the way algebraic geometry was done. Zariski's work was always grounded in the many examples of classical geometry he learned in Italy. In his ICM talk (1950) he says "The Italian geometers have erected, on somewhat shaky foundations, a stupendous edifice: the theory of algebraic surfaces. It is the main object of modern algebraic geometry to strengthen, preserve, and further embellish this edifice, while at the same time building up also the theory of algebraic varieties of higher dimension." Zariski brought in techniques of algebra to strengthen the base, and tested his methods on specific problems such as the resolution of singularities. He developed new tools as needed for other problems.

On the other hand Grothendieck explains [[3], 2.17] how he saw a vast panorama opening in front of him, which went far beyond the techniques necessary for any particular problem, and he felt the need to explore in all its detail this new unexpected world and map it carefully. He generalized everything: the base field became an arbitrary ring, the old varieties became schemes, even the notion of topological spaces and sheaves were transformed, new cohomologies appeared. And in all this work, examples hardly played any role at all.

Another difference is that Zariski mostly worked alone. Except for his book *Commutative Algebra* (1958-60), written jointly with his student Pierre Samuel, almost all of Zariski's books and papers are singly authored. Grothendieck on the

other hand had such a continual outpouring of new ideas that he needed a whole army of collaborators to join in his work, Thus the EGA are written jointly with J. Dieudonné; most of the volumes of SGA have coauthors, including Demazure, M. Artin, Verdier, Berthelot, Illusie, Deligne, and Katz. While I attended courses of lectures by Zariski, my real apprenticeship in mathematics was as a participant in Grothendieck's vast program: I wrote the lecture notes for his seminar on local cohomology in 1961 [SLN 41], and ran the seminar on his theory of duality that was written up as Residues and Duality [SLN 20, 1966].

Another striking difference is that Zariski's published papers span 58 years: 1924-1982, while Grothendieck's published work is confined to his 20 years of intense activity 1950-1970.

Grothendieck's style of working is perhaps best described by his image of the sea rising: one develops the general theory all around a problem so that each step seems simple, and at the end the problem is submerged. Here are his words [[3], p. 502]:

*"La mer s'avance insensiblement et sans bruit, rien ne semble se casser rien ne bouge l'eau est si loin on l'entend à peine Pourtant elle finit par entourer la substance rétive, celle-ci peu à peu devient une presqu'île, puis une île, puis un îlot qui finit par être submergé à son tour, comme s'il s'était finalement dissous dans l'océan s'étendant à perte de vue. . ."*

This is not to say that Grothendieck did not care about problems. One that concerned him throughout his career was the "Weil conjectures" relating the topology of a variety in characteristic 0 to the number-theoretic properties of its reductions (mod  $p$ ). These were finally solved by Deligne using the apparatus set up by Grothendieck. But more characteristic are his great generalizations of theorems known in special cases, such as the Riemann-Roch theorem to an arbitrary proper morphism of schemes, or his generalization of Serre's duality theorem for a smooth projective variety to a projective morphism of schemes with arbitrary singularities.

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### Milnor, Serre, and the Cartan Seminar

JOHN MCCLEARY

In discussions of *style* with my colleagues in the arts and humanities, I learned that the notion of style quite simply treats how something is said, while *subject* treats what is said. For historians of mathematics, it is mathematics that is said, and to

consider style is to consider how it is said. For critics of art forms, the notion of style exists when there is **synonymy**, that is when there is a possibility of choosing between alternative forms of expression about the same thing. Generally speaking, research mathematical papers do not treat the same thing—even if the topic is the same, the point of a paper is to present a new way of seeing, a different category of generalization, or even an error of insight of the other paper. The subjects of my title are universally accepted to be masters of ‘saying,’ that is, their writings and work are considered as models of style in writing. I ask:

- Is it possible to extract from their successes some notion of style that might be useful in the history of mathematics?
- Can we contrast their work with contemporary work and find ways to explain certain developments?
- Is the *excellence of style* modelled in these works a factor in the development of the sub-disciplines to which they contribute?

To frame this discussion, let me introduce one more notion of style, developed by the sociolinguists, namely, *stylistics*. How we say things is certainly the purview of linguistics for whom many different issues arise. To describe phenomena like dialect, they have introduced the notion of *register*, which includes the properties within a language associated to a particular purpose or in a particular social setting. An example is the use of dialect, but register covers other notions as well, and it is often shorthand for formal/informal style. The study of register is based on three important aspects of a situation:

*field*: the activity associated with the language used;

*tenor*: the specific role of the participants between whom the statement is made;

*mode*: the symbolic organization of the situation.

These notions parallel questions about place, time, and community raised by Moritz Epple in a paper on knot invariants in Vienna and Princeton in which the issue of the “specifics of the mathematical language” is raised.

It is a notion in stylistics to consider the formality of the use of language. Serre, in an *Intelligencer* interview, expressed his taste in mathematical writing “Precision combined with informality!”

In my lecture, I considered the cases of Serre’s thesis and Milnor’s first papers in topology. We can ask Epple’s questions: “How were mathematicians at particular places and times led to try out certain definitions and concepts? How did particular mathematical problems emerge? How was it possible to frame conjectures that might eventually become theorems? Which means of proof were available at particular places and times and how did mathematicians put them to use? How did they manage to convince others of the relevance of their definitions as well as of the correctness of their theorems and proofs? How did – in and by all these activities – perceptions of and ideas about particular mathematical notions or objects change?” This leads to a discussion of the roles played by 1) the key questions in topology at the time; 2) the actors in Paris in 1950, in Princeton in 1955; 3)

the main examples that guided the key insights, and 4) the influence of resulting work. The role played by the Séminaire Cartan and Séminaire Bourbaki for Serre was pivotal. The community of topologists and the high standard for exposition of the 1950's faculty at Princeton played a similar role for Milnor. As in Epple's analysis of the development of knot invariants, the local features are crucial. In the framework of stylistics, locality determines the resulting language. The impact of the major results of the Serre's thesis and Milnor's first topology paper was greatly increased by their ability to write with precision and informality.

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**Stilarten, Variations du style, Denkstile: Fleck's critical "Lehre vom Denkstil" and the "Stilarten mathematischen Schaffens"**

MORITZ EPPLE

The talk discussed uses of the notion of 'styles' applied to science, and mathematics in particular, of the years immediately following the takeover of the Nazis in Germany in 1933. The question was raised whether Ludwik Fleck's now famous *Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollektiv*, published in 1935 in Basel, must be seen in the context of a broader discourse of the time, strongly shaped by the intervention of authors favouring the idea of racial styles such as Hans F. K. Guenther, whose pamphlet *Rasse und Stil*, published in 1926, was widely read and republished several times. The same context is of course relevant for a historical understanding Ludwig Bieberbach's infamous *Stilarten mathematischen Schaffens* of 1934.

In the talk I briefly recalled Bieberbach's polemics against Jewish colleagues and the strong international reactions it provoked. The claim was made that most politically sensitive and internationally active European mathematicians were aware of Bieberbach's racist initiative and of the public controversy with Harald Bohr that followed in Summer 1934. In particular, I argued, this is probably true

for Claude Chevalley, who was well acquainted not only with Germany but also with several of the mathematicians attacked by Bieberbach.

In this light, the short article *Variations du style mathématique*, published in 1935, acquires a second meaning. Even if the Bieberbach affair was not mentioned in this article at all, it, too, can be placed among the international reactions to Bieberbach's racist conception of mathematical styles. Indeed, a closer analysis of Chevalley's arguments reveals that his idea of changing *literary* and *epistemic* styles of mathematical writings and mathematical research is not just incompatible with Bieberbach's exhortations, but is actually defending many of the mathematicians and modern, formal mathematical approaches that Bieberbach was attacking.

Similarly, in the field of the history of science in general, Ludwik Fleck's monograph of 1935 can be read as a counter-text to racist accounts of styles in science. As in Chevalley's case, a closer reading shows that Fleck's discussion of the variations of the notion of the syphilitic disease and of the laboratory and medical practices surrounding it aims at an open and critical understanding of the role of styles in scientific practice. Not only were Fleck's "thought collectives" - the social carriers of styles of thought - explicitly construed in a way prohibiting identification with racial collectives, he also pointed out that in any research practice, a fixation on one given style of thought would imply a severe limitation of research. His case study actually shows that decisive scientific innovations, and what he calls the emergence of new scientific facts, were always coupled to *changes* of a given style. On the other hand, Fleck was convinced that collective scientific practice was always based on adhering to certain styles of thought - so that the historical development of research always involves a plurality of thought styles in continuing variation.

So far, there is no indication of a direct awareness on Fleck's side of the Bieberbach affair in Germany. He was, however, on close terms with several leading mathematicians in his hometown Lemberg, including Hugo Steinhaus, from whom he might have learned about the affair. Further research may provide new insights here. On the other hand, Fleck's monograph was read and misunderstood by Nazi reviewers as supporting the notion of racial styles. To such reviews Fleck reacted in later publications, e.g. in the article *Das Problem einer Theorie des Erkennens* of 1936. There, he described the value of his theory in the following words: "If it only throws to the ground that malicious and hard-necked mystification with which fanaticists of their own style fight human beings adhering to a different style, then it proves its cultural value."

Taken together, Chevalley's and Fleck's considerations may warn us against simplistic uses of the notion of styles in science. An insight they share is that styles are always in variation, and that a 'fanaticism of one's own style' may be a dangerous thing.

## Weierstrass's algebraic style in complex function theory

UMBERTO BOTTAZZINI

“The more I think about the principles of function theory - and I do it incessantly - the more I am convinced that this must be built on the foundations of algebraic truths. Consequently it is not correct to take the ‘transcendent’, to express myself briefly, as the basis of simple and fundamental algebraic propositions”. This ‘confession of faith’ that Weierstrass made in a letter to Schwarz on Oct. 3 1875, can be assumed as a guideline to Weierstrass’s approach to complex function theory.

In the late 1840s Cauchy had published the refined version of his approach to complex function theory, including the Cauchy-Riemann equations, the integral theorem, the integral formula, and the calculus of residues. Weierstrass avoided resorting to Cauchy’s ‘transcendental’ methods. Instead, he chose the power series approach because of his conviction that the theory of analytic functions had to be founded on simple ‘algebraic truths’. He claimed furthermore that his view had been especially strengthened by his study of the theory of analytic functions of several variables.

Weierstrass followed this approach in 1854 in an epoch-making paper where he presented a solution of the Jacobi inversion problem in the hyperelliptic case. There Weierstrass limited himself to providing a hint of the mathematical developments needed to support his results (most of them being simply stated without proof). Two years later he resumed this subject in more detail, and provided the required proofs. In particular, Weierstrass proved that the solutions of Jacobi problem could be expressed in terms of *al*-functions, which are single-valued functions expressed as quotients of convergent power series, and other *Al*-functions whose analytical form he was able to determine. Despite his successes he could not obtain an analytical definition of the two functions whose ratio represented an arbitrary Abelian function. “Here”, he stated “we encounter a problem that, as far as I know, has not yet been studied in its general form, but is nevertheless of particular importance for the theory of functions”. He promised to treat this problem in a continuation of the paper, but such a publication never appeared.

In 1857 a completely new approach to the theory of Abelian integrals was published by Riemann. What Riemann did surpassed by far anything Weierstrass had been able to produce. After summarizing the main features of his 1851 Dissertation – including the fundamental role of the Cauchy-Riemann equations, the idea of a surface multiply covering the Riemann sphere and the related, topological ideas of crosscuts and genus of a surface – he proved an existence theorem for a function with prescribed behaviour at branch-points and singularities via the Dirichlet principle. Then he provided a classification of Abelian functions (integrals) into three classes according to their singularities, and finally, he gave a complete solution of Jacobi inversion problem by determining the theta series expressing the Jacobi inverse functions of  $p$  variables. After the publication of Riemann’s paper Weierstrass decided to withdraw the publication of his own research. Even though Riemann’s work “was based on foundations completely different from mine, one

can immediately recognize that his results coincide completely with mine”, Weierstrass later stated. “The proof of this requires some research of algebraic nature” which he was able to publish only some ten years later in a paper that was flawed by some inaccuracies that he himself later recognized in a letter to Borchardt in 1879.

In response to Riemann’s achievements, by the early 1860s Weierstrass began to build the theory of analytic functions in a systematic way on arithmetical foundations, and to present this in his lectures. According to Weierstrass, this provided the foundations of the whole of both elliptic and Abelian function theory, the latter being the ultimate goal of his mathematical work. Weierstrass’s approach was successful in providing the “systematic foundation” of the theory of analytic functions of one variable. This included the distinction between poles and essential singularities, the discovery of natural boundaries and the establishment of representation theorems based on the idea of prime factors. He also involved his students, in particular Schwarz, in his research program. Schwarz in fact performed the function for Weierstrass of recapturing several of Riemann’s theorems in a way more acceptable (and indeed more rigorous) than Riemann had first presented them. Among these there were special cases of the Riemann mapping theorem and the Dirichlet principle.

In a paper (1880) Weierstrass showed that “the concept of a monogenic function of one complex variable does not coincide completely with the concept of a dependence that can be expressed by means of (arithmetical) operations on magnitudes” (contrary to what Riemann had stated). In fact, he proved that the domain of (uniform) convergence of a series may be built up of different, disjoint regions as shown by the series

$$F(x) = \sum_{\nu=0}^{\infty} \frac{1}{x^{\nu} + x^{-\nu}}$$

which is uniformly convergent for  $|x| < 1$  and  $|x| > 1$ . In either domain this series represents a monogenic function which cannot be analytically continued into the other region across their common boundary. This result allowed him to show the intimate relationship between two theorems that “did not coincide with the standard view”, namely 1) the continuity of a real function does not imply its differentiability; 2) a complex function defined in a bounded domain cannot always be continued outside it.

Eventually, in a lecture at the Mathematical Seminar in 1884 he pointed out that in his approach he avoided the use of Cauchy’s integral theorem. He repeated his criticism of Riemann’s general concept of a complex function based on Cauchy-Riemann equations, and claimed that his own theory (based on simple arithmetical operations) could “easily” be extended to functions of several variables. In spite of his claims, however, he was largely blocked in his search for a good theory of complex functions in several variables, and for the properties of Abelian functions. This had another much more important consequence : Weierstrass’s limited interest in publishing his ideas. Instead, Weierstrass was very much concerned with

his role as a founder of a mathematical school. As he confessed in a letter to Schwarz on 18 Aug. 1888 “As things now stand, I fear - and I have all reason for that - that in a short time the most influential chairs at our universities will be taken by people of whom nobody thinks at all nowadays”. And it will be brought to the Ministry of Education the opinion that “Weierstrass himself had achieved really noteworthy things, but only second-rate people went out of his school so that one has to conclude that his road was not the right one.” According to the picture of the ‘mathematical school’ at Berlin in the early 19th century provided by Volterra, apparently Weierstrass’s fear was well-founded. Indeed, commenting on the former students of Kronecker and Weierstrass who had gone on to become professors at Berlin, Volterra wrote to his wife on Feb. 18, 1904 : “They are suggestive figures because they make us think, and there are certainly great ruins. They make us think because they show that Weierstrass and Kronecker with all their genius did not succeed at anything that they hoped to accomplish. They have created neither a new mathematics nor a school of their own. Their genius has been more advantageous to France than to their own pupils.”

### Hermite’s Weierstrass

TOM ARCHIBALD

The adoption of certain aspects of what might be termed Weierstrass’ mathematical style by Charles Hermite affords an interesting case study in how a set of local practices, associated with the personal, authorial, and even thinking style of one individual, may be successfully transplanted to another context despite marked stylistic differences. Many papers presented at this meeting provided us with examples of the distinctions between personal style (open versus closed, normative versus accepting of difference, etc.) and the style of presentation of a given mathematical author. In the paper we couched the discussion of the style of a mathematical text by comparing the kinds of parameters there present with those in a literary text. Some specific stylistic markers at the textual level include: the level of precision, the level of formality, the concern with generality, the use of text versus formulas, the presentation of examples, notational innovation, statement of unsolved problems, length, scope, and accessibility [1].

Hermite and Weierstrass differed markedly on many of these scores. The paper of Bottazzini at this meeting gives a fine account of aspects of Weierstrass’s personal and mathematical style. Weierstrass keenly felt the failure to impose what he considered to be definitive contributions to analysis on his students, reflecting his closed and normative personal style and a strongly formal, highly rigid mathematical structure as the core of his presentation of a subject. Aspects of Hermite’s style include a strong preference for the concrete in both problems and results. This reflected his own “naturalism” in mathematics, the idea that mathematical objects were in fact given as transcendent realities outside of the human mind. His admiration for Weierstrass’ results derived from an appreciation of this concreteness, and he consistently reformulated what were, in Weierstrass’ presentations,

non-constructive existence proofs (for example the Weierstrass Factorization Theorem of 1876), making them into constructive representations under somewhat more restrictive hypotheses. The resulting representations could then be worked with directly to produce the kind of results resting on pattern recognition that had occupied Hermite for his entire career.

Hermite's teaching, notably the *Cours d'analyse* of the early 1880s, thus incorporated central results from Weierstrass' work, but in a framework far removed from the formal approach of Weierstrass himself. The lectures include the factorization theorem and its generalization by Mittag-Leffler (once again transformed to a constructive mode); a series expansion for the reciprocal of the  $\Gamma$ -function; and some of Weierstrass' canonical representations for elliptic functions.

The paper concluded that despite Hermite's eclecticism, his conversational and "common sense" notions about mathematics, these stark differences from Weierstrass did not act as an obstacle to the transmission of Weierstrass' results to an alien environment, the Paris of the *Grandes Écoles* in the 1880s and 1890s.

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### Style in French Treatises on Analysis: From Tannery to Godement

JEAN MAWHIN

Between the end of the XIX<sup>th</sup> century and the end of the XX<sup>th</sup> century, the contents of treatises and textbooks on algebra, geometry and probability has been substantially modified. In the case of analysis, the difference in contents is not so large, but the style has considerably changed. We compare it in several classical treatises of analysis in French language due to Jules Tannery [7], Camille Jordan [5], Édouard Goursat [4], Georges Valiron [8], Jean Favard [2], Jean Dieudonné [1], Laurent Schwartz [6] and Roger Godement [3].

The publishing period of those books covers more than one century, their size goes from one to nine volumes, their contents varies from basic differential and integral calculus until harmonic analysis and even algebraic topology, and their public is either the students of the *École polytechnique* or of the *Faculté des sciences* of a French University (essentially Paris). The authors are all recognized mathematicians, including one Fields Medal (Schwartz), five members of the *Académie des Sciences* of Paris (Tannery, Jordan, Goursat, Dieudonné, Schwartz), and three members of the Bourbaki group (Dieudonné, Schwartz, Godement). With the exception of Jordan, who graduated at the *École polytechnique*, all the authors are former students of the *École normale supérieure* of Paris.

The analysis of the contents, style, language and even typography of those treatises leads to the following conclusions.

The style of Tannery in his *Introduction à la théorie des fonctions d'une variable* (1886) is purely discursive, clear and elegant, and the rigor satisfies the standards

of the XXI<sup>st</sup> century. No definition or statement is printed in italics or preceded by the corresponding term. There are many historical references (suppressed in the second edition of 1904), no exercises and no pictures (introduced in the second edition). The book uses typographical characters of uniform size and italics is employed only to distinguish special words. The first edition has one volume and the second one two.

The rigor of the second edition (1893) of Jordan's *Course d'analyse de l'École polytechnique* is strongly improved with respect to the first one (1882), and the style is clear, definitely less elegant than Tannery's one, but more geometrical. The 3 volumes book is also written in a discursive style, with few historical references, no exercises and some pictures. The typographical characters have uniform size but italics is used to distinguish the statements, rarely preceded by 'Lemma', 'Theorem' or 'Corollary'.

Although contemporary to the two previous ones, Goursat's *Cours d'analyse mathématique* (1902) is less concerned with rigor, but the language remains clear and the scope is significantly wider. The discursive style is conserved, with many historical references and some pictures. Two novelties : the introduction of exercises (a few of routine type and many theoretical complements), and the use of smaller characters for material to be kept for a second lecture. Statements are only identified by the use of italics.

The discursive style of Valiron's *Cours d'analyse mathématique* (1942) is not substantially different from Goursat's one. The rigor is intermediate between that of Tannery and of Goursat. Each chapter is preceded by a historical introduction, exercises follow Goursat's tradition and there are few pictures. The whole book is printed in small size, a feature probably due to the war restrictions. Only the use of italics identifies the statements, and the scope is somewhat narrower than Goursat's one.

Even if its classical Gauthier-Villars typography and binding makes it looking very similar to Goursat's treatise, Favard's *Cours d'analyse de l'École polytechnique* (1960) is somewhat different at the level of generality and abstraction. It is the first of those treatises to introduce and use the language of functional analysis, and to cover functions between Banach spaces. The style is quite discursive and not especially elegant, and the clarity depends upon the chapters. It follows Goursat with respect to the historical references, the exercises, the use of italics and of small characters, and the scope. Commercially, it has been, without any objective reason, the less successful of our selection.

The influence of Bourbaki in the style, presentation and level of abstraction of Dieudonné's *Éléments d'analyse* (1962) is evident. With the exception of the introductions to the various chapters, the style is as dry as rigorous, not discursive at all, and the statements are only identified by the numerical reference system (chapter - section - item) also adopted for the formulas, and widely used in the references to the used material in the subsequent proofs. This is very demanding to the reader and of no help is estimating the importance of a statement. Even if Dieudonné's style has been qualified as 'geometrical', there are no pictures in

the book. The numerous exercises follow Bourbaki's tradition of being essentially theoretical complements. The typographical characters have uniform size and italics are only used to distinguish terms. Despite its 9 volumes, Dieudonné's treatise does not cover all the topics considered by Goursat or Favard, but of course also deals with other more recent ones.

The formalist tendency of Dieudonné has not been pushed further by Schwartz in his *Cours d'analyse* (1967), even if the level of abstraction is similar. The first edition, reproduced from typewritten notes, keeps the flavor of the oral style, without any prejudice for rigor, but with a large number of inspiring comments. Every statement is underlined and, for the first time in our selection, preceded by Lemma, proposition, theorem or corollary, the proofs being preceded by 'proof'. There are no exercises and very few pictures. The scope is more limited than in Dieudonné's treatise, even in the second enlarged edition of 1991, written with the collaboration of K. Zizi, and using, for the first time in our selection, LaTeX word process. The main novelties are a more general and more elegant treatment of measure and integration (partly departing of Bourbaki's approach followed in Dieudonné and Godement), and a better covering of set theory, topology and Fourier analysis. On the other hand, the chapters on the algebraic topological applications of differential forms and on complex functions have disappeared in the second edition.

A complete return to a discursive style characterizes the *Analyse mathématique* (1998) of Godement. One can undoubtedly speak of a colorful style, without prejudice to clarity and rigor, with many comments about the history and the motivations of the main concepts and results, as well as about non mathematical topics. LaTeX word process is again used, and there are no exercises and very few pictures. The statements, in italics, start with 'lemma', 'theorem', 'corollary' or 'proposition'. Besides the basic concepts and results of differential and integral calculus, the emphasis is upon complex analysis, harmonic analysis and modular forms.

Considering the various styles of seven former students of the *École normale supérieure*, of three members of the Bourbaki group, of two groups of three contemporary writers, of three courses written for the students of the *École polytechnique* and three ones written for those of the *Faculté des sciences de Paris*, one is tempted to conclude from this analysis, with Buffon, that

*le style c'est l'homme !*

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## New light on Sofja Kowalewskaja

EVA KAUFHOLZ

Sofja Kowalewskaja, as the first woman to obtain her doctoral degree with a mathematical thesis and the first to hold a position as professor at a University after the Renaissance, naturally became the topic of many scholarly and belletristic publications since her untimely death in 1891 at the age of 41. Unfortunately, however, even the best recent studies present only a rudimentary contextualisation of the central themes in her life. Thus, my doctoral research aims to provide a richer elaboration of the relevant historical background while giving special consideration to three topics that ran through her fascinating life. Alongside her political interests and the way she was depicted in contemporary accounts of friends and acquaintances, I will focus on her role in the mathematics of her time, the topic I briefly addressed in my talk.

As a mathematician Sofja Kowalewskaja served as a link in a network of mathematicians interested in analysis. Those mathematicians worked in four mathematical centres: Stockholm, St. Petersburg and the two most important locations for mathematical research of the time, namely Paris and Berlin. Somewhat surprisingly, however, her relationships with important members of this network have never been carefully scrutinized. Especially in the case of Gösta Mittag-Leffler, who earlier had studied under Charles Hermite and Karl Weierstrass, a careful analysis of the subtle dependency that existed between him and Kowalewskaja will shed light on the process that helped her to appear on the mathematical landscape. More than 500 letters between the two are kept at the Institut Mittag-Leffler in Stockholm and most have never been evaluated before. They provide new evidence of the important role Kowalewskaja played for Mittag-Leffler as an editor of *Acta Mathematica*, since due to her excellent contacts she could arrange international publications. In his letters Mittag-Leffler constantly stresses the fact that *Acta* is an international journal – to what extent this was due to Kowalewskajas efforts has yet to be investigated.

In Paris mathematicians like Hermite and Henri Poincaré belonged to the network mentioned above. Although Kowalewskaja did not take up an extensive correspondence with them, nevertheless both seem to be of great importance when analyzing her mathematical background. Hermite's connection with the Weierstrassian school stems from the fact that, even though the relations between France and Germany after the Franco-Prussian-War were strained and reception of German art and literature in France had ceased, he took a strong interest in mathematical results obtained in Germany and incorporated Weierstrassian methods into his lectures.[2] Hermite and Mittag-Leffler constantly discussed Kowalewskaja's mathematical work and other interests via letters [1], and whenever a recommendation

of Kowalewskaja was needed, Hermite was happy to write on her behalf. That he took great interest in the Russian mathematician can also be seen from the fact that, in cooperation with his colleague Joseph Bertrand he ensured that Sofja was able to win the Prix Bordin in 1888.

Poincaré, a student of Hermite, was one of the most original mathematicians of his time. In 1889 he won the contest held in honour of King Oscar II of Sweden, in which Hermite, Weierstrass and Mittag-Leffler served as judges. This prize was strongly associated with *Acta*, which announced the contest and published the prize-winning papers. Poincaré's career and the success of *Acta* were a symbiotic process from the beginning. Securing Poincaré's work on Fuchsian functions that was published in the first issue of *Acta*, helped establish the journal's reputation provided Poincaré with a possibility to make his results internationally known.[5] Most of the few letters between Kowalewskaja and Poincaré concern papers that were submitted for publication in *Acta*<sup>1</sup>. But in Stockholm Kowalewskaja gave a lecture course on Poincaré's qualitative theory of differential equations, an indication of the importance she ascribed to his work. Nonetheless, so far his impact on the way she treated questions of mathematical physics has not been investigated, and so my goal is to evaluate the influence of other mathematicians besides to Weierstrass on her work.

As is well known, Kowalewskaja was a private student of Weierstrass between 1870 and 1874, and therefore is considered to be a member of a group of mathematicians working on complex analysis that came to be identified as the Weierstrassian school of mathematics. As Thomas Hawkins noted, this school can be characterized not only by the specific methods like analytic continuation used in complex analysis, but more generally by the use of rigorous methods and the consideration of special cases as opposed to the generic treatment of problems as customary at this time.[4]

Like Kowalewskaja, Carl Runge got his mathematical education in Berlin. Even though he only moved there in 1877, when Kowalewskaja was living in Russia, they became friends and started a correspondence. In fact, Runge was the first person to be notified of Kowalewskaja's death via a letter by Mittag-Leffler. The network of contacts becomes clearly visible from a letter Kowalewskaja wrote to Mittag-Leffler in 1881. We learn that Dmitrieff Selivanov, a young student of Pafnuty Chebyshev, was sent to Berlin so he could participate in the lecture courses of Weierstrass. There he befriended Carl Runge and subsequently became a friend of Kowalewskaja as well.

Chebyshev, too, can be considered a part of the network, due to his contact with several European mathematicians. As Paul Butzer points out, those contacts were not maintained via letters, since Chebyshev rarely wrote, but instead travelled to Europe on a regular basis. As far as Kowalewskaja is concerned, one can not speak of a correspondence on a regular basis, even though no one ever got more letters from him than her. But since she visited him during her trips to Russia and

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<sup>1</sup>Those letters can be seen as scans at the homepage of the Institut Poincaré: <http://www.univ-nancy2.fr/poincare/chp/hpcoalpha.xml>

translated some of his articles into French so that they could be published in *Acta Mathematica* [3], it seems promising to analyse whether his mathematical work had any impact on hers.

Evidently, Kowalewskaja was not the only connecting point for the mathematicians I mentioned, who were part of overlapping networks. Some of them corresponded, others visited each other regularly, and not a few even called each other friends. Apart from their obvious common interest in analysis, they all were connected through their interactions with Sofja Kowalewskaja. There exist letters from all those persons mentioned written to her while she was in Stockholm, and most of these have never been taken into serious account in the literature at hand. But considering them in the process of contextualising Kowalewskaja and the way she moved in these various circles. I hope to show her as a window on the mathematics of her time, thereby shedding new light on important mathematical centres and interactions between them.

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### Disciplines and Styles in Pure Mathematics, 1800–2000

SCOTT A. WALTER

The contributions of Hermann Minkowski to the theory of relativity are remarkable from the point of view of discipline and style. First of all, by the end of the 19th century, German mathematicians no longer contributed to theoretical physics. From the 1870s, theoretical physics emerged in Germany as an autonomous sub-discipline of physics, in parallel with the construction of new physical institutes (cf. Jungnickel & McCormmach, 1986). Minkowski was known for foundational contributions to the geometry of numbers, and from 1902, held a chair in pure mathematics at the University of Göttingen. His interest in physics deepened in Göttingen, extending from fluid dynamics and capillarity to electron theory and heat radiation (Corry 2004). Upon reading works on relativity theory by Poincaré,

Planck, and Einstein, Minkowski conceived an ambitious program to reformulate the laws of physics in four-dimensional, Lorentz-covariant terms (Walter 2008). In 1908 he published the first relativistic theory of the electrodynamics of moving media, which he couched in a new four-dimensional matrix calculus. Physicists were impressed by his theory, but preferred, with Einstein and Laub (1908), to translate it into three-dimensional terms. Minkowski then focussed, in his next publication, on the merits of his four-dimensional formalism alone. His celebrated Cologne lecture, *Raum und Zeit*, described the requirement of covariance of laws of nature with respect to the transformations of the inhomogeneous Lorentz group as the essence of relativity theory. Thereby, Minkowski claimed, the theory of relativity was well-adapted for exploitation by mathematicians. Despite scattered protests, theoretical physicists like Arnold Sommerfeld, Max Planck, and Max Laue recommended a four-dimensional approach for research on relativity, such that by 1911, this approach had come to dominate the pages of Planck's journal, the *Annalen der Physik*. Collectively, mathematicians contributed a quarter of all articles on relativity published from 1909 to 1915 (Walter 1999). Thus questions of discipline and formalism (or style) are important for understanding the history of relativity theory.

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#### Dealing with inconsistencies—a matter of style?

TILMAN SAUER

The talk had two parts: after an extended introduction, in which I presented some thoughts about the role of inconsistencies for the dynamics of science, I discussed an episode of Einstein's early work on a unified field theory, in which I illustrated some of those thoughts about inconsistencies.

Inconsistencies and contradictions are a curious phenomenon in the history of science. Explicit contradictions are those that are being developed at some point in the history of a particular branch of science to be of the explicit form  $A \wedge \neg A$ . They immediately render the conceptual framework that allowed a deduction of such a contradiction invalid, at least in parts. More interesting than this negative consequence is the fact that an explicit contradiction usually contains hints as to what part of the relevant conceptual framework needs to be revised and how. In actual science, examples where inner inconsistencies are being pushed to the point of an explicit contradiction are the exception. Inconsistencies, on the other hand, i.e., implicit contradictions that are inherent in the semantics of scientific concepts but that are not elaborated fully to the point of an explicit contradiction, are found frequently and ubiquitously, especially in periods of crisis. They, too, it seems to me, play an important role in the development of conceptual frameworks.

The role of contradictions and inconsistencies appears to be very different in pure mathematics and in physical sciences. In pure mathematics, inconsistencies are unacceptable. Unless they point to explicit mistakes in a mathematical theory, they usually indicate deep foundational problems. In the case of mathematized natural sciences, scientific concepts carry an external reference that frequently creates inconsistencies with the semantics that is being established by its embedding in a larger conceptual framework and by the means and rules of deduction. Inconsistencies in the natural sciences can be tolerated to a much greater extent, since the empirical content of scientific concepts may stabilize inconsistent connotations. The way scientists deal with inconsistencies can be very different. One strategy to deal with inconsistencies is to follow a heuristics that secures the consistency of axiomatic systems from the beginning. This strategy was formulated programmatically by Hilbert as the “axiomatic method.” Physicists may also look for inconsistencies in the empirically meaningful conceptual frameworks as pointers to scientific progress and insight into the nature of things. It seems that Albert Einstein’s eminent early productivity was to some extent due to his ability to locate conceptual inconsistencies and to explore implicit contradictions to the point that they could be resolved by way of fundamental conceptual revision.

Examples of inconsistencies and contradictions that have played a role in the history of relativity theory include the inconsistency between the relativity principle, the constancy of the speed of light, and the classical addition theorem for velocities. This inconsistency was resolved by Einstein’s special theory of relativity, at the heart of which was a reinterpretation of the concept of simultaneity. With the establishment of the special theory of relativity, an inconsistency that had already appeared between electromagnetic field theory and the concept of Newtonian action-at-a-distance forces became more prominent since the postulate of Lorentz covariance precluded infinite propagation of causally efficacious physical forces. This inconsistency was only resolved with the realization that Einstein’s field equations allow for solutions that describe gravitational waves. A more specific example of a contradiction was the rotating disc paradox that pointed to the

breakdown of Euclidean geometry in the case of accelerated motion. The breakdown of Einstein's so-called *Entwurf*-theory, the immediate precursor of the general theory of relativity was occasioned by three contradictions. First, the empirically observed anomaly of the perihelion advance of Mercury could not be reproduced when explicit numbers were being calculated. Second, Einstein realized in September 1915 that Minkowski spacetime in rotating Cartesian coordinates is not a solution to the *Entwurf*-equations despite the fact that the case of a rotating frame of reference had been a heuristic point of departure for the derivation of those equations. Third, Einstein at some point realized that a mathematical derivation of the equations on the basis of a variational principle did not render the equations unique despite the explicit claim that it did. All three contradictions were resolved with the advent of the final field equations of general relativity. Curiously, it can be argued that the realization of internal inconsistencies also played a role in Hilbert's independent path toward general relativity in late 1915. He, too, had found a "hair in the soup" of Einstein's *Entwurf*-theory, which appears to have been the fact that Einstein made a mistake in his variational calculation. More seriously, it can perhaps be argued that Hilbert's revision of an earlier version of his famous 1915 paper on the *Foundations of Physics*, as extant in the form of page proofs, was occasioned by a reflection on the consistency of an axiom system that initially included three axioms, one of which was then dropped in the published paper.

Other interesting examples of inconsistencies and contradictions in theoretical physics include the recurrence and time reversal paradoxes in the context of statistical mechanics, Gibbs' paradox and the concept of indistinguishability of particles in Bose-Einstein statistics, the negative energy solutions of Dirac's equations and their interpretation as antiparticles, the EPR paradox and the notion of quantum non-locality, the infinities of quantum electrodynamics and the notion of vacuum fluctuations.

By way of illustrating the more general considerations laid out so far, the talk focussed on an episode of Einstein's search for a unified field theory of gravitation and electromagnetism, that will be documented in the forthcoming Volume 13 of the *Collected Papers of Albert Einstein* [1]. The question was posed whether the original motivation for the unified field theory program, i.e., the quest for a mathematical representation that "unifies" in some sense the two then-known fundamental fields, carries the same heuristic power as the analysis of a true conceptual inconsistency. While the program itself may be criticized on these grounds, one can nonetheless observe the productive role of contradictions in the actual mathematical elaboration of specific approaches to the problem. In early 1923, on board the ocean liner that took him back to Europe from his visit to Japan, Einstein drafted a manuscript for a paper that took up Eddington's affine field theory of 1921. Before sending off his manuscript, Einstein realized a contradiction between an implication of his theory and its intended physical interpretation. As a consequence, he revised his theory. Fortunately, extensive notes and calculations have been preserved that allow us to some extent to reconstruct Einstein's reflections.

Such a reconstruction illustrates the heuristic role of inconsistencies that have been made explicit in the elaboration of the mathematical representation. Einstein finally sent off for publication a revised manuscript in which he had resolved the earlier contradiction [2]. But, as he eventually found out, he had not solved the task of constructing a final unified field theory.

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**Reactions to the introduction of group theory into quantum mechanics**

MARTINA R. SCHNEIDER

Group theoretic methods, or rather methods based on representation theory of groups, were introduced into quantum mechanics from 1926 onwards. By the beginning of 1930, they were used in four areas: foundational questions (H. Weyl, E. Wigner); quantum numbers, atomic and molecular spectra (Wigner, J. von Neumann, Weyl, E. E. Witmer); molecular bond (W. Heitler, F. London, Weyl); Dirac wave equation (Weyl, von Neumann, B. L. van der Waerden). Whereas today group theory is an integral part of quantum theory and particle physics, its introduction to quantum physics was not without difficulties in the late 1920s. The paper explores the following questions:

- (1) How was the group theoretic method received by quantum physicists at the time?
- (2) How did mathematicians introduce group and representation theory to physicists?
- (3) Was there any impact on pure mathematics due to the involvement of quantum physicists in group theory?

**ad 1)** In secondary literature one often comes across the picture of two clearly divided “camps” within the community of quantum physicists. The one camp consisted of physicists who used the method, the other, a larger one, of those who rejected it. This picture is too simplified. One can find a whole spectrum of reactions between these two poles. After J. C. Slater determined a group-free method to calculate the multiplets of atoms with many electrons in 1929, the rejection of the group theoretic method gained momentum. According to Slater, a lot of scientists were delighted: “Slater has slain the ‘Gruppenpest’.” [Mehra/Rechenberg 2000, p. 508]

The term “Gruppenpest” (group plague) became a catchword for those who rejected the group theoretic method. It had been coined by the physicist P. Ehrenfest in Leiden in 1928. Ehrenfest, however, did not reject group theory – a fact that is often overlooked. He wanted to understand group theory and organized a series of

talks on the topic in Leiden. Consequently, some of his students (H. B. G. Casimir, G. Uhlenbeck) took up the new method and developed it further.

Other physicists, like A. Sommerfeld, were also interested and acknowledged its importance to quantum mechanics. W. Heisenberg [1928] wrote a very positive review of Weyl's monograph on group theory and quantum mechanics. However, like Ehrenfest, Sommerfeld too expressed the difficulties he had understanding representation theory.

But there were also more sceptical voices. D. Hartree, for example, was not sure if he should learn group theory because he feared that it might turn out to be of no value just as he was beginning to understand it. Hartree also feared that theoretical physics would come under the regime of mathematicians [Gavroglu 1995, p. 55 f.]. In this respect, P. A. M. Dirac took a totally different, almost opposite point of view. Dirac [1929, p. 716] was of the opinion that group theory should be seen as part of quantum mechanics because he believed that quantum mechanics was the theory of all quantities which are not commutative. As one of the founders of quantum mechanics, Dirac held this self-confident, but rather singular point of view. However, he didn't keep to this viewpoint.

Analyzing the reasons why group theory was so controversially debated among physicists, I have come to the conclusion that the main reason is that only very few physicists were familiar with group and representation theory at the time. As far as I know, the fact that those fields belonged to modern mathematics was never a reason for physicists to reject them. What was recognized by some physicists was that representation theory is different in style to the mathematics they were familiar with, namely to analysis [Wigner 1931, p. V].

**ad 2)** By comparing the three monographs on the group theoretic method by Weyl, Wigner and van der Waerden published in 1931/32 one arrives at differences and similarities which cannot be explained by a professional/disciplinary distinction between the three authors. This is illustrated by two examples: the proof of the uniqueness theorem and the treatment of Slater's group-free method.

The uniqueness theorem says that the decomposition of a representation of a group into irreducible ones is unique (up to isomorphism and order). Wigner [1931] proved the theorem only for groups of finite order. In his proof he worked with characters. In doing so, he gave the readers a method to decompose a given representation into its irreducible components. For Weyl [1931] and van der Waerden [1932] the uniqueness theorem was important enough to create an extra section named after it. Of course, their proofs included the case of groups of infinite order, which, like the rotation groups, were of central importance to quantum mechanics. Weyl gave a highly structural proof depending on a lot of algebraic concepts introduced specifically for this very purpose. Van der Waerden's approach was totally different. He based his proof on two simple lemmata on groups. These were easy to prove. Then he used the concept "group with operators" (which was central to his introduction to representation theory) to transfer these lemmata to representation spaces and thus to prove the uniqueness theorem. His proof was very elementary compared to Weyl's and no additional concepts had to be introduced.

The second example shows that things are more subtle. Here different dynamics can be seen: Weyl ignored Slater's method, Wigner 'groupified' it, and van der Waerden optimized it [Schneider 2010, chapter 13].

Summing up, the two mathematicians chose very different approaches. Weyl's was more conceptual and mathematically elaborated. Van der Waerden's was structural but tailored to the demands of physicists, at least as far as he was aware of them. His goals were elementarization and operationalization of mathematical methods. Van der Waerden clearly differentiated very carefully between the requirements of both sides, the mathematicians' and the physicists'. The physicist Wigner also chose an elementary approach but at the same time tried to show the power of group theory wherever he could.

**ad 3)** Indeed, as early as the 1930s the involvement of quantum physicists in group theoretic methods had an impact on the development of representation theory. Wigner determined the unitary infinite-dimensional irreducible representations of the inhomogeneous Lorentz-group in 1939. Casimir and van der Waerden [1935] gave the first purely algebraic proof of complete reducibility of semi-simple Lie-groups. Here a network of physicists and mathematicians played a vital role:

Weyl proved the complete reducibility of semi-simple Lie-groups in 1925/26. He did this with methods he called "transcendental". By this he basically meant methods of analysis, integration over manifolds. Like some other mathematicians, Weyl himself was not too happy about these because the theorem itself is a purely algebraic statement and thus it should be able to be proved algebraically.

While Pauli was an assistant in Hamburg he participated in a course of lectures on hyper-complex systems, i.e. algebras, by Emil Artin. According to Pauli, Artin had explained at the beginning of the lecture that he could not treat continuous groups in the lecture because there was no algebraic proof of the theorem of full reducibility of representations of semi-simple continuous groups. Pauli was impressed by the fact that Artin, a representative of the algebraic school, preferred the ascetic omission of an entire field of application rather than include a method which he considered inadequate [Meyenn 1989, p. 114]. With respect to the conference's topic 'Style', this episode clearly highlights the influence of a wide-spread methodological style in mathematics which can be expressed by "purity of methods".

Pauli was appointed professor in Zürich and, in September 1931, Casimir, one of Ehrenfest's students, became his assistant. Pauli gave Casimir the problem of finding an algebraic proof of the full reducibility. So here a mathematical problem becomes an object of research for physicists. One might speculate why this was the case. In letters from Casimir to Ehrenfest we find several reasons: Firstly, Pauli realized that this was a serious problem for mathematicians. Mathematicians lacked an algebraic proof and were keen to have one. Secondly, Pauli thought that theory in physics was not getting anywhere and so it would be better to do mathematics. And finally, Pauli was convinced that the proof could not be difficult.

Casimir started working on the proof, and after a year he had developed a purely algebraic proof for the three-dimensional rotation group  $SO_3(\mathbf{R})$ . His proof was based on an object which is today known as the Casimir-operator and which he had introduced in his PhD-thesis with Ehrenfest. Casimir sent his partial solution to van der Waerden whom he knew from the series of lectures on group theory and quantum mechanics organized by Ehrenfest. Van der Waerden quickly generalized Casimir's proof to apply to all semi-simple Lie-groups.

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### **Poincaré's approach to electrodynamics: *Sur la dynamique de l'électron.***

ALBERTO COGLIATI

I would like to offer a brief historical excursus on the different interpretations of Lorentz's covariance of Maxwell's equations before Einstein's special relativity (SR). We will consider physicists and mathematicians working on the same topic with very different approaches; I do not know whether one is entitled to speak of different styles (I would prefer to speak of different epistemologies and different educational backgrounds) or not. However we will certainly deal with different disciplines: namely, electrodynamics of moving bodies and transformation groups theory.

As is well-known, SR was born to remedy the following inconvenience: although the symmetry group of Maxwell's equations (ME) does not coincide with the Galilean group of classical mechanics, experimental data suggest that it is impossible to reveal any influence of the motion of the Earth on the electromagnetic

phenomena; the well-known experiments carried out by Michelson and Morley with the interferometer and by Trouton and Noble with the charging and discharging of a plane condenser are just a few manifestations of this conflict between theoretical predictions and experimental facts.

Often overlooked, but the first one to deal with the problems connected with the symmetry properties of the equations for the electromagnetic field is the father-founder of the theory himself: J. C. Maxwell. In paragraphs 600 and 601 of his illustrious *Treatise on electricity and magnetism*, Maxwell studies the equations for the electromotive intensity when these equations are referred to a reference frame which moves with uniform velocity with respect to the ether. Assuming that the electric field and the vector potential transform according to certain laws, he demonstrates that these equations are covariant under Galilean transformations, that is they take on the same form whether they are referred to a moving reference frame or to a reference frame at absolute rest. Despite the inexactness of this statement (it is correct only if we disregard terms of order higher than first in the aberration ratio), however, from a historical point of view, Maxwell's concern with transformation properties of the equations for the electromagnetic field is important, because it is connected with the applicability of the principle of relative motion and more generally with the dynamical properties of the ether surrounding ponderable matter in motion.

Over the years that follow the publication of Maxwell's electromagnetic theory, the necessity for giving a proper description of the electromagnetic phenomena in moving dielectrics is soon reflected in further knowledge of symmetry properties of ME. The first to give a systematic account of the electrodynamics of moving bodies which is based on the study of the symmetries of ME is the Dutch physicist H. A. Lorentz.

Within the context of Lorentz's electrodynamics, the covariance of ME under what is now known, after its introduction by Poincaré, as the Lorentz group, is stated in the so-called theorem of corresponding states. For the sake of brevity, I will only describe Lorentz's analysis contained in [3]; the other formulations of the theorem, which are contained in [4] and in [5], differ from the previous one for the fact that they do not disregard terms of order higher than the first in the aberration.

Lorentz's starting point consists of writing down ME for a physical system at rest; then, he operates a Galilean boost and he introduces primed quantities (coordinates and fields), which are such that they satisfy, at first order in the aberration, equations of the same form as those of a system at rest, in order to trace back the resolution of a moving physical system to that of a system at rest. It is important to underline that the primed quantities are not identified with the physical quantities that are measured by a moving observer, but that they are considered as auxiliary quantities describing a fictitious system at rest with respect to the ether. Obviously, understood so, the covariance of ME does not guarantee the validity of the principle of relativity.

Before Einstein, the only one to doubt this interpretation of the covariance of ME was Poincaré. In an important paper, [7] written in 1900 on the occasion of the 25th anniversary of Lorentz's doctorate, by marking a deep discontinuity with Lorentz's orthodoxy, Poincaré gives a passive interpretation (that is relativistic) of Lorentz's transformations; Poincaré's intention consists of demonstrating that a violation of the principle of reaction in Lorentz's electrodynamics implies a corresponding violation of the principle of relativity which shows itself in the fact that a moving observer does measure the primed quantities introduced by Lorentz and in the consequent existence of the so-called Liénard force. .

Five years later (in 1905), however, Poincaré's approach towards covariance properties of electrodynamics seems to change completely in favour of an active interpretation which, closely following Lorentz' standpoint, considers Lorentz's transformations (whose algebraic group structure is for the first time recognized) as active transformations acting on physical systems and not on reference frames. The following quotation taken from [8] is particularly enlightening on this subject. The context is a discussion of the contraction of electrons.

*Supposons un électron unique animé d'un mouvement de translation rectiligne et uniforme. D'après ce que nous venons de voir, on peut, grâce à la transformation de Lorentz, ramener l'étude du champ déterminé par cet électron au cas où l'électron serait immobile; la transformation de Lorentz remplace donc l'électron réel en mouvement par un électron idéal immobile.*

Now the question is: why does Poincaré change his mind? In other words, why is he then convinced that the active interpretation introduced by Lorentz is to be preferred to the passive one that he discovered in 1900? Some hypotheses have already been proposed (see [10]); I will limit myself to add the observation that, contrary to what he stated in 1900, in 1905 Poincaré thinks that Lorentz's electrodynamics, in the emended version set forth in [5], is fully capable of assimilating the principle of relativity. As a consequence of this, he no longer needs to adhere to a passive interpretation in order to reveal a violation of the principle of relativity.

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### **Mittag-Leffler and mathematics at *Stockholms Högskola*: some reflections related to the issues of “style” and “discipline”**

LAURA TURNER

Disciplines, in particular within mathematics, are not just subject groupings. They are collective practices associated with a body of knowledge/autonomous field of knowledge/academic division, with canonical texts; the term “discipline” thus implies a specialization, or a specific domain of application. With this in mind, disciplinary leaders (often thought of as “heroes”) are practitioners who stand at the head of a community, through which one must gain entrance if one seeks to become a practitioner or adherent, pointing to many sociological interpretations of disciplines and the issue of rites of passages. These leaders typically seem to have a spontaneous or tacit knowledge of disciplinary boundaries and practices. One criterion for the existence of an academic discipline is the capacity for reproduction. This necessitates a body of students (the “disciples”) and teaching practices, and a style of communication where practitioners must publish in the “right” places according to the disciplinary boundaries. Clearly, then, discipline formation is connected to factors and developments both internal and external to the body of knowledge itself.

In my paper I aimed to reflect upon the various criteria for discipline formation in connection with the Swedish mathematician Gösta Mittag-Leffler’s (1846 – 1927) central role in the promotion of specialized, research-level mathematics at the newly-founded *Stockholms Högskola* and the development of a research community there during the early- to mid-1880s. I have characterized his actions there as reflecting a “mission” which stemmed from his studies under Weierstrass at Berlin during the mid 1870s. This mission centred about promoting the research imperative through advanced study in Weierstrassian analysis and encouraging his students to engage in research connected to his own work on the Mittag-Leffler Theorem.

Specifically, I aimed to reflect upon whether or not the notion of “discipline formation” might enhance our understanding of Mittag-Leffler’s aims and the impact of his actions in this context, as well as the ways in which this particular case study might contribute to a more refined definition of discipline as an analytical tool.

I posed the questions:

- Is discipline formation a useful or meaningful way to understand what Mittag-Leffler tried to establish at *Stockholms Höghskola*? (and how useful or meaningful can it be as an analytical tool before a clearer definition has been established?) Did Mittag-Leffler possibly also have a stylistic “mission”?
- ...or is this development, in its rather unique setting better understood through different tools (for example, the notion of a research school)? For instance, what might have been the role of *Acta Mathematica*, an *international* journal of mathematics produced in a specific local context, in discipline formation in Scandinavia and beyond?
- How do local factors (*ex. peripheral nature of Sweden; unique position of Mittag-Leffler, who was well-connected and with powerful contacts, etc.; presence of an international journal; existence of Stockholms Höghskola, which was devoted to research, ...*) change/influence our understanding of discipline formation?

To attempt to establish answers, it is necessary to recall Mittag-Leffler’s own contributions to complex analysis, one of the most important of which was the theorem which bears his name. This work was tightly bound to Weierstrass’ lecture series and research in analysis, and the earliest (1876) version was inspired by work Mittag-Leffler learned from Weierstrass in 1875. Mittag-Leffler utilized Cantor’s theory of sets of points to generalize this theorem between 1882 and 1884, and taught his own results and those connected to them in Stockholm from 1881.

In 1877 Mittag-Leffler wrote to his former mentor Hjalmar Holmgren about the advantages of Weierstrass’ function theory, specifically its simple and systematic foundation. Mittag-Leffler appreciated not only the quality of the material presented by Weierstrass, however, but also the nature of the teaching he received in Berlin. Specifically, he experienced a tight link between research and higher education and an emphasis on training future researchers. The hiring of the Russian mathematician Sofia Kovalevskaya at *Stockholms Höghskola* was also linked to Mittag-Leffler’s allegiance to Weierstrass and his mathematical approach.

It is important, however, to understand that Mittag-Leffler’s success in instituting Weierstrassian analysis and the Berlin style of teaching in Stockholm was due in part to several important factors specific to Sweden and *Stockholms Höghskola*. Specifically, the höghskola, which promoted academic freedom (there was no curriculum, no examinations, and no formally registered students) and its emphasis on (scientific) research essentially allowed Mittag-Leffler to teach whatever he liked, however he liked (among others see [Bedoire and Thullberg(1978)] and [Stubhaug(2007)]). This allowed him to focus his teaching on one branch of mathematics rather than introducing a little bit of everything (a strategy in which he firmly believed in the early 1880s, see [Heinonen(2006)]), to train his students through seminars, and even to put them in touch with current research developments, both Cantor’s and his own.

One might wonder, then, whether or not Mittag-Leffler’s promotion of a specific body of knowledge (Weierstrassian analysis) and professional practices to his

students might be construed as evidence of discipline formation. It is clear, for instance, that based on the early work of two of his earliest students in Stockholm, namely Ivar Bendixson (1861–1935) and Edvard Phragmén (1863–1937), as well as their correspondences with Mittag-Leffler, Mittag-Leffler actively encouraged research from his students, and even gave them particular problems on which to work. In turn, they both produced original research results closely related to Mittag-Leffler’s own (which, I note, Mittag-Leffler in turn communicated to students in his later lectures). Not only did their work reflect Mittag-Leffler’s research interests, but these students also understood their results within a function-theoretic framework — they considered themselves to be doing work in the theory of functions.

Studying Phragmén’s career is particularly interesting, for it suggests that at least a small fraction of a second generation of Swedish analysts had aims toward a third generation. As a result of this specialized, research-oriented study he received in Stockholm, as well as his involvement (under Mittag-Leffler’s direction) in editorial work and critical readings for *Acta Mathematica*, Phragmén took on Mittag-Leffler’s mission in 1887 when he visited James Joseph Sylvester in Oxford. His goal for this journey: “to attempt to introduce the theory of functions in England” ( “att söka införa funktionsteori i England”), to be achieved either through lectures, or by the writing of a textbook on the subject in the English language.

These ideals or strategies connected to discipline formation were further manifested in connection with *Acta Mathematica*, a journal whose contents were predominantly works in analysis. This points to an extended network of practitioners.

Returning to the questions posed earlier in this paper, this case study demonstrates the complexity of the various issues at hand in connection with discipline formation, the necessity of separating general issues from those specific to very particular contexts, and the fact that that issues internal to mathematics cannot be separated from social and cultural developments. In studying issues connected to discipline formation it is therefore necessary to consider many different “layers” of social spaces, such as the teaching carried out at particular institutions, the development of mathematics within particular local or national contexts, the journals utilized in the communication of knowledge and the networks of practitioners associated with them, and the correspondences between practitioners.

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## National Styles in Mathematics Revisited

REINHARD SIEGMUND-SCHULTZE

In a 1996 paper [1] I have tried to discuss the notion of mathematical style in a somewhat broader manner. In my talk in Oberwolfach I restricted the discussion to a much narrower point. I took the existence of differences in ‘styles’ in mathematics as given (e.g. between the Riemann and Weierstrass schools in function theory) and asked under which circumstances certain styles could be described - at the same time - as ‘national styles’ in mathematics.

My interest in the topic of national styles in mathematics has originated in two contexts: *First*, my studies on maths in the Third Reich, where the discussion, largely triggered by Ludwig Bieberbach, on alleged German/Aryan styles in mathematics as opposed to Foreign/French/Jewish styles plays a certain, although rather infamous role [2]. *Second*, my work on the comparison of the German and American science/mathematics systems, and, in particular, on emigration from the German into the American system [3]. Roughly said, the first kind of studies helps you to understand the ideological atmosphere better under which mathematics was done in the Third Reich, although you have basically to find the “sense in the nonsense” [[2], 116]. However, this discussion does not allow one to connect different manners (or styles) of doing basically the same mathematics or even preferences for one or the other mathematical discipline - which everybody instinctively feels to exist - to any historically relevant invariant or marker, be it ‘race’, or ‘nation’ or ‘philosophy.’

As to the second level of comparison: When comparing the German and American mathematical systems, one is - in spite of considerable changes on both sides - empirically confronted with several historically rather stabile political and sociological traditions. Differences persist on the level of philosophical traditions and the educational systems, for instance the different transition from high schools to the university, the funding systems (private universities), the heavy teaching loads an the American college, the ‘American spirit of cooperation’ (versus alleged ‘European individualism’) and the ‘democratic system of departments’ in the U.S. which historically have been very often quoted as well. As an important source we have of course here the book by Karen Parshall and David Rowe from 1994 [4]. For the discussion of ‘national mathematical styles’ the main methodological problem seems to be to which extent these differing national traditions translated or still translate into different research traditions, in the selection of research areas and working styles. The different institutional settings of applied mathematics in Germany and the U.S. in the first half of the 20th century which were related with these generally different traditions have for instance been described by Gert Schubring and myself.

G. D. Birkhoff talked in 1938 about certain American works which he summarized under “Special Analysis” (Wiener on Tauberian Theorems, Hille, Tamarkin and Widder on Laplace-Transforms, and L.L. Silverman on summation of divergent series) [5]. He complained that these fields were marginalized because mathematics

was considered as “serious business” in the U.S. rather than as “a means of exercising talent for free invention” like in Europe. The frequent talk among American mathematicians of the 1930s about the ‘German Algebra’, which was often related - if in a convoluted way - to Emmy Noether’s structural approach, was very much influenced by the existing political circumstances, in particular immigration [[3], 285].

While European countries continue to imitate American traditions in research funding, it has been repeatedly stressed also in recent years that the U.S. still needs the continuous import of the European traditions in mechanics, classical analysis and differential equations. In 1989 the American students Coifman and Strichartz of the Polish immigrant-analyst Antoni Zygmund stressed the unabated importance of these European traditions [6]. Inasmuch as that import is brought about today by graduate students from abroad, this seems connected to certain insufficiencies of the American system to produce a large enough number of promising young mathematicians. With respect to graduate and doctoral students the influx of Asian nationals has brought new aspects into this question in recent decades. Hermann Weyl saw it as one of his major tasks in the U.S. to maintain or to re-introduce “reflection” (Besinnung) in the ever faster growing world of modern mathematics [[3], 294]. Weyl pleaded for the employment of European mathematicians with knowledge of the history of mathematics, such as M. Dehn, E. Hellinger, O. Blumenthal, and O. Neugebauer. But Weyl was mostly unsuccessful in this endeavour.

The thrust of the tentative arguments of my contribution is the following:

- (1) Different styles in mathematics often exist in different social groups of mathematicians
- (2) These social groups are even today often funded on a national base or otherwise influenced by national traditions (education, philosophy etc.)
- (3) An exaggeration of these differences into “national” styles is often politically and propagandistically motivated
- (4) No conclusions for innate abilities or styles of individual mathematicians belonging to these groups are possible
- (5) Even conclusions as to the ‘average’ style in certain groups of mathematicians are only possible in so far relevant conditions for the scientific socialization are taken into account: this rules out racism but it opens for the consideration of particular conditions of socialization which for instance affected Jewish or black minorities.

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## Remarks on style, purity of method, and the role of collectives for the history of mathematics between the World Wars.

NORBERT SCHAPPACHER

The remarks sketched here were intended as a contribution to launching the final discussion.

### 1. Style

During the past week, we have heard several, different proposals to render the category of ‘style’ more precise and efficient for historiographical accounts. Of these I would like to recall here the following:

(1) As in, say, music history (my standard example is the “style of the Vienna classic”, i.e., Haydn, Mozart and Beethoven), the notion of style, applied to works of art, or more generally, texts, allows for instance to relate a score, or text, whose author is unknown, to a well identified group of such documents. Such groupings can be formalized to a certain extent by way of a set of rules; and rules associated with different styles can be compared. For example, Chevalley in his *Variations du style mathématique* [1], discussed by Moritz Epple in his talk, discusses the *style d’une époque* like for instance the style des  $\varepsilon$  à la Weierstrass. This seems very analogous to the notion of style of the Vienna classic. Note in particular that Chevalley groups all of (Western) mathematics in the period in question under this heading, just as the musical category covers all genres of music, from opera to string quartets. Such a notion of style may provide criteria for the classification of the historical material. Another example along these lines would be the (style of) Algebraic analysis of the 18th century. But also more local examples of the same function of the category “style” can be given, for instance the SGA and other series of texts produced by the Grothendieck school all have a *Leitfaden (Fil d’Ariane)* in the beginning.

(2) Jean Mahwin’s comparison of various French treatises on Analysis according to a fixed set of criteria brought to light individual variations of style, as did John MacCleary’s comparison of Serre and Milnor. The last word along these lines in both cases was: *Le style, c’est l’homme*. I wonder if there are not two readings of this sentence, which might be called a French and a German one. In French,

it is the style that a man shows off which creates his personality. In German, or at least in a certain 19th century, romantic strand of the theory of style, the *physiognomical temptation* lurks, i.e., the tendency to interpret the description of individual styles as the expression of the essence (*Wesen*) of the individual. This reading was consciously exploited by Bieberbach for his political goals, when he (and Teichmüller) pretended that one could recognize Edmund Landau's racial incompatibility with his Aryan students in the way that  $\pi$  was defined in his analysis textbook. Even independently of this revolting example, it is obvious that the historian using the category of style must do everything in his power to ban the physiognomic interpretation.

(3) Tom Archibald had another notion of style which I interpreted as an attempt to overcome the problem alluded to on the first two days of our conference: that different styles are rarely just varying ways to express an invariant content. Tom explained that the category of style becomes immediately relevant when one studies the *displacement of a content* from one place or context to another. The only occasion so far where I have used the category of style in my own work was precisely such a case of a migrating content: to describe what sets the first chapters of André Weil's *Foundations of Algebraic Geometry* from 1946 apart from Bartel L. van der Waerden's series of articles *Zur Algebraischen Geometrie* in the *Mathematische Annalen* during the 1930s. Here very basic mathematical ingredients which Weil takes over from van der Waerden – specifically the universal domain, generic points and specializations – bridge the gap between those two sets of texts, which however turn out completely different: Weil deliberately cast his text in book format, used a Bourbaki-like internal referencing system underscoring the sheer dimension of the stringent systematicity. It was Weil's presentation which instituted a new practice of doing Algebraic Geometry. The migration of scientific content makes the style comparisons both viable and useful.

## 2. Purity of method

The notion of purity of method came up in Martina Schneider's lecture, in particular via Emil Artin who was not prepared to accept as adequate an analytic proof of an algebraic theorem. There are many other examples of similar reactions. Oswald Teichmüller for instance wrote ([3], p. 693): "Aber ich möchte etwas so Geometrisches wie diese konforme Abbildung nicht gern aus Reihenentwicklungen schließen." Helmut Hasse induced Max Deuring not to publish a proof, which involved lifting a situation back from characteristic  $p$  to characteristic 0 and using complex analytic arguments there, of an important property of the ring of correspondences of a curve over a finite field, because he considered it "unfair."

On the French side, reading Chevalley's text as the (probably first) manifesto of the Bourbaki programme, the adequate proofs seem to fall freely into place: "On peut donc dire que les définitions constructives de l'analyse, si elles ont les

premières permis les raisonnements rigoureux, ont eu souvent l'effet de cacher profondément la nature de ce qu'elles cherchaient à définir ou de confondre indûment des domaines mathématiques en réalité distincts les uns des autres. De là résultent les complications inutiles qui se rencontrent dans beaucoup de démonstrations classiques, du fait de l'emploi de méthodes n'ayant rien à voir avec le résultat escompté, on pourrait dire : *de méthodes n'admettant pas le même groupe de transformations que le résultat.*" ([1], p. 380) With hindsight, this description brings to mind the definition of natural transformations by Eilenberg and MacLane at the very beginning of category theory.

On a larger scale, the whole development of Algebraic Geometry in the 1930s and 1940s, and also that of Probability Theory, globally fall under this theme: exclusively algebraic (in Zariski's case: *arithmetic*) methods were installed for Algebraic Geometry, measure theoretic ones for Probability Theory, stripping away, at least from the mathematical part of its development, all application-related complications.

But a more interesting question may be, to which extent German and French approaches of such questions can be told apart - in spite of the intense contact between young mathematicians afforded at the time by Rockefeller grants.

### 3. *The role of collectives*

In Moritz Epple's talk, we heard about Ludvik Fleck's *Denkkollektive*, even though nobody used this methodology in his/her talk. Discussing the history of science in the 1930s, various discourses and practices around collective enterprises naturally come into the picture. This point is made for France at the end of Catherine Goldstein's recent article [2]. She compares Gaston Julia's exhortation of a group of students in 1934 which is based on patriotism, discipline and a patriarchal family model to a 1963 speech by Pierre Cassou in honour of Châtelet stressing the unity of scientist and citizen, and the Bourbaki group and the tension it cultivated between the individualist outlook of its members and their collective group identity.

How do these choices compare to the German *Kameradschaftlichkeit* which appears to have marked Ernst August Weiss's mathematical (and political) student training camps after 1933? How do they compare to the climate of the working group of advanced students like Ernst Witt and Teichmüller in Göttingen between 1934 and 1936 who wrote up the classification of local fields under Hasse's indirect supervision? And can Hasse's initiatives to try and integrate amateurs like, Otto Grün and Kurt Heegner, into professional mathematical work be seen in this context?

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**The discipline “mathematics” in a general scientific journal in the 1920s and the 1930s in France : “mathematics” versus “mathematical sciences”, which identity?**

HÉLÈNE GISPERT

I consider the notion of discipline on the global scale of mathematics as a whole and not on the scale of any specific mathematical fields. I will question it from the point of view of mathematics in general, studying one of the two French famous general scientific journal, la *Revue générale des sciences pures et appliquées* and look at the contents, the authors, the editorial politic, the intended readership of this “high scientific popularisation” journal as said by its editors. The *Revue* claims to address itself only to the intellectual public, and to popularise science - “science which creates the elite” - with the following cultural agenda: “sans négliger les hautes recherches spéculatives, faire une large part aux méthodes et travaux industriels démontrant l’alliance féconde de la science et de l’industrie”.

The identity of mathematics, as far as this journal is concerned, can be approached by several factors: 1/ the mathematical contents we can read in its pages; 2/ the actors we encounter there, that is the authors - mathematicians who have written on mathematics or on other topics, other scientists and chroniclers - but also the different kinds of intended readers: mathematicians, scientists, students, teachers, “amateurs”, users; 3/ third factor, places, linked to these actors: from where do they write and, linked to the readership, for whom do they write.

What can catch a reader, when skimming through the *Revue*, is identified by the terms “sciences mathématiques” and deals with a very large spectrum of topics : from “pure” topics in analysis, geometry, but also astronomy and mechanics to the most applied or industrial ones in all these branches as we can see in these slides. There is nothing-specific in the pages of the bi-monthly issues of the journal identifying just “mathematics” even if at the end of each annual volume, the editors undertake *a posteriori* a rearrangement in an analytic table of contents which combines the titles and subtitles “mathematics” and “mathematical sciences”. One of the major issues at stake in this general journal about the identity of mathematics, is then the status of “mathematics” versus “mathematical sciences” which gather also astronomy and geodesy, and, depending of the section, mechanics and civil engineering or general and applied mechanics. What is then the specific territory of mathematics? What is in and what is out? What is its relative place? What are

its characteristic features for the reader as for those who make the journal? Actually, what is at stake under the adjective “mathematical” (mathematical sciences) or under the noun “mathematics” which identify a specific discipline.

I have pointed out that mathematics appears, during the inter-wars period, first of all as a discipline that is taught, in France and abroad, in different places - universities, engineers schools, secondary schools -, second as a mainly useful discipline for various applications in various professional domains by all kinds of audiences. It is far less evident, reading the *Revue*, that mathematics is an affair of research, of current research, and, if so, to see which researches are the more visible. Mainly, the respective territories of what is labelled “mathematics” and what is labelled “mechanics” in the *Revue* - for teaching as for research - is not so clear, specially in the thirties, considering both actors (authors as intended readers) and places. More over, chroniclers take the occasion to advocate in favour of applied topics, of applied mathematics and lay the emphasis on an opposition, and even an conflict, between pure and applied mathematics, mathematics mostly having therefore in the *Revue* an applied identity among the mathematical sciences.

### **Axiomatics Between Hilbert and the New Math: Diverging Views on Mathematical Research and Their Consequences on Education**

LEO CORRY

”New Math” is a term commonly used to denote a series of high-school and primary education programs developed after 1958 - following the Soviet launching of the Sputnik - first in the USA, then in many European countries, and subsequently all over the world. Indeed it is the best-known, and typically derided, of the many educational reforms in mathematics devised throughout the twentieth century. Curiously, little serious historical research has been devoted to it. My talk is an outline of a genealogy of ideas leading from the modern axiomatic approach developed by Hilbert since the beginning of the twentieth century to the basic principles behind the New Math in its various local contexts. The genealogy suggested takes two different paths: one via R.L. Moore in the USA, and one via Bourbaki in France. David Hilbert is widely acknowledged as the father of the modern axiomatic approach in mathematics. The methodology and point of view put forward in his epoch-making *Grundlagen der Geometrie* (1899) had lasting influences on research and education throughout the twentieth century. Nevertheless the way in which it came to be understood and practiced by mathematicians of the following generations, including some who believed they were developing Hilbert’s original line of thought, often deviated from Hilbert’s own conception of the role of axiomatic thinking in mathematics and in science in general. The topologist Robert L. Moore was prominent among those who put at the centre of their own research an approach derived from Hilbert’s recently introduced axiomatic methodology. Moreover, he actively put forward a view according to which the axiomatic method would serve as a most useful teaching device in both graduate and undergraduate teaching in mathematics and as a tool for identifying and

developing creative mathematical talent. Some of the basic tenets of the Moore Method for teaching mathematics to prospective research mathematicians were adopted by the promoters of the New Math movement. The Bourbaki movement in France introduced an approach and style that became emblematic and highly influential, based on a highly idiosyncratic use of structural and axiomatic style. Because of their style and approach they sometimes declared themselves to be the “true heirs of Hilbert”. Still, in important senses they deviated from Hilbert’s views. Their style and their personal influence were decisive in establishing the New Math ideas in France.

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