Mathematisches Forschungsinstitut Oberwolfach

Report No. 04/2012

DOI: 10.4171/OWR/2012/04

Explicit Versus Tacit Knowledge in Mathematics

Organised by
Tom Archibald, Burnaby
Jeanne Peiffer, Paris
Norbert Schappacher, Strasbourg

January 8th – January 14th, 2012

Abstract. This workshop aimed to bring together an international group of historians of mathematics to reflect upon the role played by tacit knowledge in doing mathematics at various times and places. The existence of tacit knowledge in contemporary mathematics is familiar to anyone who has ever been given an idea of how a particular proof or theory "works" by a verbal analogy or diagrammatic explanation that one would never consider publishing. Something of it is felt by every student of mathematics, when the process of learning mathematics often amounts to training the right reflexes. In more advanced contexts, the tacit understanding that a particular technique, instrument or approach is "the one to use" in a given circumstance gives another familiar instance. Tacit knowledge, a term introduced by the philosopher M. Polanyi, contrasts with the explicit knowledge that in almost all historical mathematical cultures is associated with mathematical text. The workshop invited a use of the categories of tacit and explicit knowledge to achieve a better knowledge of how mathematical creation proceeds, and also of how cultural habits play a tacit role in mathematical production. The meeting intended to offer the possibility of significant innovation and enrichment of historical method, as well as new and compelling insight into the process of creating mathematics in different times and places. The meeting was intended to afford the opportunity for a presentation of selected case studies by leading experts and new scholars. In retrospect, as we hope these abstracts show, the results promise to be of significant interest not only to historians, but to the mathematical community more broadly.

Mathematics Subject Classification (2000): 01A.

Introduction by the Organisers

The aim of this workshop was to bring together an international group of historians of mathematics to reflect upon the role played by *tacit, as opposed to explicit knowledge* in doing mathematics at various times and places. Methodological discussions on the use of this concept alternated with specific case studies from the history of mathematics. The aim was to allow a better understanding of mathematical practices in given contexts. The theme impinges on the transmission of existing mathematics as well on the creation of new theories and results.

The existence of tacit knowledge in contemporary mathematics is familiar to anyone who has ever been given an idea of how a particular proof or theory "works" by a verbal analogy or diagrammatic explanation that one would never consider publishing. Something of it is felt by every student of mathematics, when the process of learning mathematics often amounts to training the right reflexes. In more advanced contexts, the tacit understanding that a particular book or paper or approach is "the one to use" in a given circumstance gives another familiar instance. The theme was specifically chosen for this meeting on the history of mathematics in view of its inspirational and unifying potential, and in the ways that it promised to shed light on methods for understanding mathematical texts and practices of the past. Originally, our plan was to look at cases that range from the most ancient history of mathematics to current developments. We include here the original list of examples, and the reader can compare this to the actual papers, which achieved a comparable breadth while highlighting rather different features:

- The difference between algorithmic mathematics (like in ancient Mesopotamia or medieval China) and proof-oriented mathematics in the Euclidean tradition and the intermediate stages, like Chinese two-column algorithmic texts which are proof-driven but not in the Euclidean style are all too often analyzed without taking into account the parts of the practice that remain tacit and are not spelled out in the text, contributing thus to give a biased image of that difference.
- Tacit knowledge is present in various ways throughout the mathematical exchanges of the seventeenth century. Correspondence by letters included knowledge on how to write a letter, without spelling out the rules of letter writing. In cases where these tacit codes were not applied, it is interesting to give an interpretation of this step aside. More generally, tacit rules of scientific exchange dictated what was to be made explicit or public in a mathematical proof, and which parts were not. On the mathematical level, curves were identified by a catalogue of properties, which was never explicitly listed in its entirety. For instance, as soon as a curve was found to have the property that its subtangent is the double of the abscissa, it was identified with a parabola.
- A good deal of the development of mathematics in the nineteenth and twentieth centuries can be viewed as a process of making the practice of mathematics increasingly explicit, thereby reducing the amount of tacit

knowledge and thus opening up a wide space of rational discussion and achievements. However, this tendency to greater technical explicitness, which is evident in the typical manuscripts posted by mathematicians on ArXiv every day, may induce historians of mathematics to neglect the persistence of tacit knowledge in the most recent mathematics. The identification of such tacit elements seems capable of affording significant insights into the development of mathematics today.

- Similarly, several large scale mathematical enterprises of the last 100 years like Bourbaki's *Éléments de mathématique* or – in a different manner – computer-based mathematical research, like the more recent projects towards automated theorem proving (ATP), appear at first as signposts of a massive pushing back of tacit knowledge. Looking more closely, however, at details like the occasional warning signs in the margins of Bourbaki's volumes, or at problems related with the user interface, one sees that these undertakings carry in fact their own heavy collection of tacit mathematical practice.
- Developments in the history of mathematics are often loosely described as moving from approximate, incompletely understood treatments, to fully explicit, formal statements and their rigorous proofs. (See for instance Breger's contribution to [1].) Paying attention to the kind of tacit knowledge which is mobilized before and after such a development often provides a much more satisfactory analysis of the historical process than the mere confrontation of precise *versus* imprecise methods. A case in point is the rewriting of Algebraic Geometry in the first half of the twentieth century. In a 1926 letter to Hermann Weyl, Salomon Lefschetz significantly described the Italian school of Algebraic Geometry, not as lacking rigor, but as requiring "a terrible entraînement ". Later attempts, by Francesco Severi and others, to defend their classical Algebraic Geometry against growing criticism would invariably insist on the fact that all those technical assumptions or arguments which the modern algebraists could not find in the Italian papers where indeed tacitly assumed, and well-known to all geometers raised in the Italian school. The question whether the category of tacit knowledge may render such arguments historically convincing appears quite difficult, and can only be decided by very detailed case studies.
- In contemporary mathematics, blogs and Wikis – the most famous probably being Terence Tao's – currently provide an extended form of oral culture in which less formal, formerly tacit approaches are written down and opened to a broad mathematical public according to shifting and variable rules.

The term "tacit knowing" or "tacit knowledge" which we explored here in its bearing on the history of mathematics, comes from a philosophical context, but has been mobilized before for the history of science. Michael Polanyi introduced "tacit knowing", or "tacit knowledge" in order to describe abilities which cannot be fully described or explained (see [4]). In the history of science, the concept has been mobilized in the study of the craft aspects of experimental science from the seventeenth century to the present day. The philosophical theory of tacit knowledge has been much discussed over the years for instance also in the context of mathematical education and curricula, which is not the purpose of the workshop proposed here. More recently, the sociologist Harry Collins reassessed this notion in [2], in particular distinguishing several types of tacit knowledge.

The theory of tacit knowledge marks a counterpoint to the "ideal of wholly explicit knowledge" which took shape through the scientific revolution of the seventeenth century. Among the different interpretations which have been given of the concept of "tacit knowledge", from a conscious under-articulation or deliberate secrecy to the strong thesis that there are specific kinds of knowledge that cannot in principle be fully articulated – the standard example being here riding a bike – the application to the history of mathematics will focus on the weak sense: tacit knowledge is what mathematicians selectively conceal, avoid articulating or under-articulate, consciously or not. This does include the possible concealment of information by mathematicians competing with others, as well as the case of descriptions which are left incomplete because their authors assume, or know by experience, that their readers share a certain knowledge with them. Tacit knowledge is then built on experience or action, and cannot be fully described by rules or words. It concerns any type of knowledge or skill used as subsidiary to the performance and control of a mathematical task. The notion of tacit knowledge could be applied to the history of mathematics, as suggested by Breger ten years ago who used the greater level of abstraction created by the ongoing development of mathematics to detect tacit elements in earlier texts. This is a challenging thesis but obviously history of mathematics should not be reduced to just re-reading old texts through the spectacles of more modern mathematical achievements.

At this point, more recent methods in the history of mathematics come to the rescue: following a tradition that can be traced back to Ludwig Wittgenstein and other authors of the 1930s and 1940s, the second half of the twentieth century has seen authors such as Imre Lakatos, Paul Feyerabend, and Hans-Jörg Rheinberger placing the detailed analysis of scientific practice at the heart of history of science. This goes hand in hand with the realization that tacit scientific knowing is acquired by the individual scientist through a social context or network whose members share a common know-how. Although unstated know-how tends to be difficult to identify in a single mathematical text, shared tacit knowledge or know-how is more accessible, often by way of comparison with other local mathematical cultures or broader networks. It also tells a lot about mathematical (and strategic) practices in a specified time period.

In the case of mathematics, Epple has adapted Rheinberger's approach to the history of mathematics in his book [3] on the history of knot theory. The notions of *epistemic objects* and *epistemic techniques* are his key concepts to describe the ways of the active researchers to handle the complex web of established theories ready for use, formal and informal operational skills to deal with new phenomena, and often vague general ideas about the kind of mathematical object under focus.

Furthermore, the mathematical tools made use of in specific contexts or sites are in most cases abstract techniques or objects, but may also be material devices, from the measuring rod and compass to the analog integrator and computer. In the Renaissance and early modern periods, the design and use of such instruments was a core feature of mathematical practice, and the tacit knowledge involved in acquiring the techniques of use or design was considerable. Yet such knowledge has left historical evidence: Albrecht Dürer, most famously, tried to describe explicitly what perpective artists were actually doing, including the gestures transmitted through long workshop traditions. One aim of the conference will be to assess the degree of continuity between these older traditions and those in evidence in more recent mathematical practice.

Our main objective for the conference proposed here was thus to use the peculiar bias of the distinction between tacit and explicit knowledge in order to re-invigorate discussions about how the analysis of social networks on the one hand and of the research practice of mathematicians on the other come together to afford a close-up understanding of the historical process which we call mathematics. Last but not least we hoped it would allow a better understanding of how mathematical practices depend on larger cultural habits, or are embedded in larger cultural contexts, including language, writing cultures, literary and rhetorical devices, and craft knowledge.

The abstracts below show that the chosen theme has proved inspiring for most of the speakers, whom it enabled to highlight aspects of the production and transmission of mathematics which have often been neglected. The use of instruments, which may imply a lot of bodily skills which can scarcely be transmitted through words, is typically an example of such an understudied aspect of mathematics. The practice of skills was also one of the starting points of Michael Polanyi in his 1958 *Personal Knowledge*, as Jeanne Peiffer recalled in her short introduction to the workshop. Before describing the tacit component of The Art of Knowing, Polanyi suggests to grasp “the nature of the scientist’s personal participation by examining the structure of skills” ([4, 49]and as his clue for this investigation, he takes what he calls the well-known fact “that the aim of a skilful performance is achieved by the observance of a set of rules which are not known as such to the person following them” (ibid.). For Polanyi, an art, a skill, which cannot be specified in detail – think at the famous example of riding a bike – cannot be transmitted by prescription, since no such prescription exists. It can be passed on only by example from master to apprentice. It follows that an art which has fallen into disuse for the period of a generation is altogether lost. And here questions for the historian come in, mostly methodological questions. How can we, as historians, recover not specified, not explicated skills, arts or knowledge ? Besides methodological reflections, a whole range of case studies have been presented by the participants of the workshop which have shown the various forms of tacitness.

Norbert Schappacher in his introduction briefly reminded the audience of Michael Polanyi's record:
(a) As a researcher, in particular as director of the chemical-kinetics research group in Fritz Haber's *Kaiser-Wilhelm-Institute* for physical chemistry and electrochemistry in Berlin-Dahlem starting in 1923; see [5], esp. chap. 2 and 3; cf. Polanyi's ranking among the leading scientists of the Kaiser-Wilhelm-Gesellschaft at the time in [6], vol. 2, p. 1254.
(b) As a thinker on economic theory, fighting the rather marxist tendencies of his brother Karl. Hachtmann in [5], vol. 1, pp. 31-32, points to text of Polanyi's from as early as 1930, on the return of investment into the sciences (*Rentabilität der Wissenschaften*) which kind of anticipated, in the concrete context of the *Kaiser-Wilhelm-Institutes* [KWIs] menaced by spending cuts after the big economic crisis, Pierre Bourdieu's later theory of the exchangeability of *actual, cultural, symbolic*, and *social* capital.
(c) Of the later unfolding of Polanyi's ideas of personal later tacit knowledge, driven by a desire (probably partly inspired by Ludwik Fleck) to balance Popper's so-called *Logic of scientific discovery* by more genuine descriptions of scientific practice, and by a more Gestalt-theoretic approach of scientific work, and its part in human culture at large.

More to the point of the subject of this meeting, i.e., the history of mathematics, Polanyi's letter to Lakatos of August 14, 1961 (from the Archives of the London School of Economics; thanks to H.J. Dahms for sharing it with us) was quoted, written in response to reading a draft of Lakatos' *Proofs and Refutations.*There one reads in particular : "If you are interested to find out as I am, how it can be that these procedures of acquiring what we call knowledge, do in fact lead to something that is knowledge, though it is, and must remain, impossible to define these procedures, or set up criteria of their success, without appealing to powers which are defined by no rules, then one feels that to speak of conjectures and refutations etc. as answering my question, is to beg it."

References

[1] Herbert BREGER, Emily GROSHOLZ (eds.), *The Growth of Mathematical Knowledge.* Dordrecht 2000.
[2] Harry M. COLLINS, textitTacit and Explicit Knowledge. Chicago 2010.
[3] Moritz EPPLE, *Die Entstehung der Knotentheorie. Kontexte und Konstruktionen einer modernen mathematischen Theorie.* Braunschweig 1999.
[4] Michael POLANYI, *Personal knowledge. Towards a post-critical philosophy.* London 1969.
[5] Mary-Jo NYE, *Michael Polanyi and his Generation. Origins of the social construction of science.* Chicago & London 2011.
[6] Rüdiger Hachtmann, *Wissenschaftsmanagement im "Dritten Reich". Geschichte der Generalverwaltung der Kaiser-Wilhelm-Gesellschaft.* 2 volumes. Göttingen 2007.

Workshop: Explicit Versus Tacit Knowledge in Mathematics

Table of Contents

Karine Chemla
How tacit is tacit knowledge? Or: Looking for sources to approach tacit knowledge . . . 139

Samuel Gessner
Tacit knowledge and mathematical instruments in early modern Europe . 140

David Aubin
Looping the Loop: Mathematicians and Bicycle Theory at the Turn of the Twentieth Century . . . 144

Ulf Hashagen
Explicit versus Tacit knowledge in scientific computing in Berlin (1870–1933) . . . 148

Christine Proust
Guessing an algorithm beyond numbers displayed on a clay tablet: a sample for Old Babylonian period . . . 150

Leo Corry
Euclid's II.5: Pure Geometry, Geometrical Algebra and Tacit Knowledge 153

Alain Bernard and Jean Christianidis
Explicitly tacit knowledge in Diophantus . . . 154

Marc Moyon
Understanding a Mediæval Algorithm: a Few Examples in Arab and Latin Geometrical Traditions of Measurement . . . 157

Veronica Gavagna
Tacit versus explicit knowledge in history of mathematics: the case of Girolamo Cardano . . . 161

Antoni Malet
The arithmetization of proportionality as tacit knowledge in early modern mathematics . . . 163

Felix Mühlhölzer
Our Knowledge of Standard Models: A Case of Tacit Knowledge? . . . 165

Volker Peckhaus
Hilbert's Formalism: Intuition and Experience . . . 166

Dirk Schlimm
On making mathematical inferences explicit: Pasch's reflections on logic 169

Erika Luciano
Tacit vs Explicit Images of Mathematical Logic: the reflexions of the School of Peano 171

Herbert Breger
Tacit Knowledge in Mathematics: Definition, Types, Examples 174

Philippe Nabonnand
The use of the word "implicit" in the works of Carnot and Poncelet 175

Frédéric Brechenmacher
Linear Groups in Galois Fields: A Case Study of Tacit Circulation of Explicit Knowledge 179

Caroline Ehrhardt
Explicit and tacit knowledge in the teaching of mathematics in the 19th century 184

Kirsti Andersen
An example in which Tacit Knowledge was transformed into an important concept in the mathematical theory of perspective 189

Umberto Bottazzini
Explicit versus Tacit knowledge in creating 'modern' analysis in the 19th century 189

Tatiana Roque
Different points of view on the reception of Poincaré's methods 191

Jeremy Gray
"The soul of the fact" – Poincaré and proof 194

Jessica Carter
The role of diagrams in contemporary mathematics 197

Christophe Eckes
Weyl and the kleinean tradition 199

Emily R. Grosholz
Fermat's Last Theorem and the Logicians 204

Abstracts

How tacit is tacit knowledge? Or: Looking for sources to approach tacit knowledge

Karine Chemla

This talk was conceived in conversation with the book by Harry Collins, *Tacit and explicit knowledge*, The University of Chicago Press, 2010. In particular, I followed him in discussing tacit knowledge in relation to, or in contrast with, explicit knowledge. When Harry Collins lists the disciplines that in his view have "take[n] tacit knowledge to be part of their concern", he recurrently does not include history in his list. My question was thus to understand whether this was contingent or whether there was a deeper problem there. In my view, the main problem historians face with the issue of tacit knowledge is a methodological one, related to sources. Whether one considers tacit knowledge with respect to the practice or with respect to the subject matter–facets that need to be addressed separately and in relation to each other–, sources are a thorny issue. In a first part of the talk, I indicated how some reasonings by historians depend on assumptions regarding tacit knowledge that are not made explicit and that require justification.

On the one hand, historians have to resist the temptation to derive, from the fact that we have no sources, the conclusion that we are dealing with tacit knowledge. To avoid this problem, one may be tempted to adopt the following strategy: concentrate on one given text and examine what is left tacit in the writing of the text. This could theoretically be done for two types of tacit knowledge: the knowledge that actors choose to keep tacit as opposed to that which is explicit in a given text (local level: to keep knowledge tacit at this level may derive from different types of motivations: making a text clear, establishing social distinction,...); the knowledge that is never made explicit, except incidentally, as a collective phenomenon (global level, linked to the professional culture in which one operates). However, to approach tacit knowledge in this way, by focusing on 'a text' requires determining what a text is, its extension, and this operation proves to be quite a tricky issue, as I explained by a choice of examples. As a result, the corpus of knowledge deemed tacit may be rather unstable, depending on the focus the historian chooses and the decisions taken with respect to what constitutes a text.

On the other hand, historians have to resist to refer to knowledge as tacit in the absence of any information on the oral dimensions that went along the use of the documents that are their sources. In other words, the act of communication needs to be taken into account fully in order to determine which knowledge may be deemed tacit in a given context. To avoid the difficulties attached to this question in relation to ancient history, the example given to illustrate this problem is a proof in Samuel's *Introduction à la théorie algébrique des nombres* (second edition, 1971). The same inscription, it is argued, does not make knowledge tacit, or rely on tacit knowledge, in the same way, whether it is inserted in the page of a book or written at the blackboard in relation to oral discourse. The pair oral/written must thus be considered in relation to the pair tacit/explicit, even though the relationship between the two is far from obvious and the documents on the oral dimensions of a given practice are difficult to find.

All these problems, I suggest, derive from the fact that in the first part of the talk, "tacit" was mainly a historian's category. I thus turn, in my second part, to a case study in which we can observe "tacit knowledge" as an actor's category. I do so by commenting the article by Lei Hsiang-Lin, "How Did Chinese Medicine Become Experiential? The Political Epistemology of *Jingyan*", *Positions*, 10:2 (2002), pp. 333-364. This article recounts struggles between practitioners of Chinese medicine and Western-style physicians in China in the 1910s and 1920s. It brings to light how the tacit knowledge attached to the practice of Chinese medicine became the topic of an explicit discourse in the context of these struggles, although it had never been considered before. Moreover, the article shows how actors attached this dimension to the mode of transmission of Chinese medicine, learning for an important lapse of time from a master. This remark designates the types of modes of transmission in a given context as an indicator of the amount and nature of tacit knowledge transmitted. More importantly, the case study shows that one can take the discourses about tacit and explicit in a historical perspective and wonder what are the motivations that lead to important changes in these discourses, similar to the changes discussed above. Lei Hsiang-Lin brings to light how actors from both sides correlated the value attached to a practice of science that made things explicit with knowledge coming from the "West". This fact highlights that these values tacit/explicit were and still are loaded politically for the actors. The remark suggests that it would be highly interesting to examine the history of the use of the values of tacit and explicit in the history of the historiography of mathematics since the 19th century and the political dimensions attached to this use, and this, until today!

In conclusion, probably the circumstances in which different professional cultures are brought in contact with each other are quite favorable for the production of sources in which knowledge tacit in each of them are made more explicit.

Tacit knowledge and mathematical instruments in early modern Europe

Samuel Gessner

It is not certain whether tacit knowledge (TK) is a useful concept to historically understand the role of instruments in the evolution of mathematics and mathematical culture. In this context, are there historical facts, which require TK as a descriptive or explanatory category, and which would remain otherwise unexplained? Can we more plausibly make sense of what happened historically if we take into account TK as a form of knowledge? Material and visual culture, like mathematical instruments, are mighty testimonies for manifest skill and/or knowledge, which does not, however, travel with them in explicit form. Although objects and images are silent on their own, they can be used to *infer* some of the knowledge that lay at their base, but this becomes possible only when they are being related to a cultural framework and their context. For the persons who manufactured the objects or designed the images this knowledge may have been explicit or tacit at the time. In any case, objects only seldom come with a textual explanation (although with instruments this might have been more frequent than with other objects like furniture or sculptures). Very grossly, in order to probe the fertility of the concept TK, I will here take as a starting point some materially existing instruments and, by looking at their design, try and infer part of the subjacent (mathematical) knowledge. We will discover that such an *inference* is indirect insofar it rests always upon some hypotheses about the procedures used to design the instruments. Then I will look at writings that treat such kinds of instruments and observe what type of knowledge is made explicit in these texts, and what type of knowledge remains unarticulated.

When we talk about instruments, where does TK come in? To examine this question let's distinguish different phases or modes in the "cultural life" of an instrument:

i) idea, conception, development
ii) manufacture, execution of the geometrical construction
iii) description in treatises and other writings
iv) use, handling

To go quickly through these stages I'm using the case study of the Regiomontanus type universal dial, a rectilinear universal sundial first published by Regiomontanus in 1474 [1], in German usually called "Uhrtäfelchen". It consists of a rectangular plate and a plumb line equiped with a sliding bead. The plate has a latitude scale that allows to precisely position the origin of the thread (according to latitude and the sun's position in the zodiac, i.e. date), eleven parallel hour lines and an additional zodiac scale to adjust the bead on the thread. This dial allows one, among other operations, to determine the time in equal hours by taking the height of the sun when the latitude and the date is known.

i) Regiomontanus remains silent about how he constructed the instrument, and also about whether or how he invented it. Theoretically he could have derived the device from his knowledge of the "sphere" (the geometrical doctrine of the cosmos), based on the assumptions of circular and uniform motion. Unmentioned go, as usual in gnomonics, a series of useful approximations in that the earth is assumed spherical but negligible in size in comparison with the orb of the sun, the annual motion of which is neglected in a day's course, and the refraction by the atmosphere, although mentioned in Sacrobosco, is ignored. Moreover, the construction involved among other geometrical constructions the socalled "manachus" from gnomonics, referred to in book IX Vitruvius' *De architectura*. This knowledge, the "sphere" and the "manachus", forms a part of century-old doctrines so that it had become part of the "forma mentis" to scholars of the 15th century. While it is possible today to reconstitute the necessary reasoning that leads from these assumptions to the configuration of the dial, it is difficult to tell, only based on Regiomontanus' *Kalendarium*, how the author went about conceiving the instrument.

One could of course measure and check the accuracy of the construction, e.g. the value used for the obliquity of the ecliptic, and so on. But it becomes above all necessary to look at the contemporary context, in particular gnomonic traditions that apparently have existed. In particular Regiomontanus could have derived his dial, as King [2] suggested, inspired by the knowledge of several similar kinds of instruments, some called *Organum Ptolomaei* and the quite popular *Navicula de Venetiis* in the form of a ship. Several instruments and manuscript descriptions from the 15$^{\text{th}}$ c. survive in at least two locations: England (about 15 mss. and several instruments) and Vienna. It is not impossible that Regiomontanus simply collated his version of the dial from one Peurbach's manuscripts [4].

ii) Now let's turn to TK in the workshop, the place where artisans work different materials to manufacture more solid, longer lasting, and sometimes also more precise instruments than those of paper. Craft relies on manual skills, and there seems to be a large part of TK involved in these manual procedures, the way to handle all kinds of tools, and the order of working procedures. However, I want to be concerned here with the portion of mathematical knowledge only, and foremost knowledge about geometrical construction procedures that goes into such manufacture and materializing.

We find the "Uhrtäfelchen" produced on a nice brass quadrant in deposit at the Museum of History of Science of the University of Lisbon (inv. nr. 1162). According to the signature on the instrument, it was made at the famous workshop of Arsenius, at Louvain, 1573, one hundred years after Regiomontanus' publication. During this century, plenty had been written on this kind of instruments in manuscript and print. This specimen in particular can be shown to be quite directly inspired by books authored by Oronce Finé as it conspicuously combines several instruments presented in the 4th part of the *Protomathesis* treating various dials and quadrants. Finé is explicit about the construction of the scales, but he does not give any proof, or even justification for it [5].

The device of the three-segment brachiolum. There is one minor feature here I want to mention, which has less to do with knowledge of gnomonics than with plain geometry. It is the design of the *brachiolum* that allows one to position the origin of the thread to any point of the latitude/zodiac grid. In some print copies, the Regiomontanus "Uhrtäfelchen" has a *brachiolum* of two parts. It is clear that a maker has to figure out that, when the first segment of the arm is longer than the sum of the further segments, the range of the extremity of the *brachiolum* is a ring. It has to be placed in such a way that the whole latitude scale stays within its reach. Arsenius attached the *brachiolum* above the scale, this makes it mechanically possible to point to a position very near the upper edge, which would not be so easy had he followed the recommendations by Finé who sets the attachment directly at the level of the highest latitude.

A tacit error of design. While looking at this upper edge, on the Arsenius instrument, we realize that there is a problem: the latitude on the numbered scale is given as $61°30'$. But this uppermost zodiac scale will indicate 12 o'clock in the situation where the sun just showing up at the horizon at winter solstice. This is something that happens only at the northern polar circle, which is not $61°30'$ but about $66°30'$. What has happened? There are several possible reasons for such an error, but one might be that Finé didn't spell out that the upper edge represents the latitude of the polar circle.

iii) Writings about instruments constitute an important chapter in this story, as text is the medium of the "explicit". What part of knowledge was selected to be explained and what part has there remained tacit? The literature concerned with mathematical instruments of all kinds (astronomical, geodetical, nautical, artisanal, cosmographical, architectural, military etc.) is very rich and also heterogeneous. If we look beyond outright treatises, we find separate chapters in books on other topics, lecture notes, private correspondence, advertising leaflets, and more. For instance, the exposition of the universal dial becomes part of a lecture on the "Sphere" at the Jesuit college of Lisbon in 1621, as is apparent from surviving notes [3]. Different kinds of authors wrote for various audiences: mathematicians write about instruments, and apply the Euclidian methods to proving construction and modes of use (Aunpekh of Peurbach, Nunes, Commandino, Benedetti). Self-taught mathematicians like Finé, Tartaglia, and others provide the bulk of the typical instrument writings in the form of *De fabrica et usu* treatises. From the 16^{th} c. onwards more instrument makers are known to be active, at least they intervene more self-consciously on the scene and quite often publish booklets to explain and advertise their production, or their projects. What is left tacit and what is made explicit in these writings seems to be a function of all these parameters: author, intended audience, context, genre of text.

iv) *If* the instruments of the past were actually used, why and for which purposes represents a huge question mark. Use has left traces on certain instruments, but the particulars of their use, the gestures, the skill and the knowledge implied were of very volatile nature. This becomes partly evident when we read the texts concerned with the "use" of the instruments. By "Usus" of an instrument the literature usually refers to a series of problems and the corresponding procedures to solve them. When an instrument like Arsenius' 1573 quadrant came to someone's hand, she would discover, e.g. that there are the scales of a "shadow square" just from the label "Puncta vmbrę rectę", and "Puncta vmbrę versę" marked along a circular scale, each semi-quadrant being divided into 12 unequal parts, subdivided into 120. The knowledge about how to use such a shadow square was widely known, as the feature traditionally appeared on the back of astrolabes or on quadrants. Problems are solved by working out the similar triangles formed small scale on the instrument (by the engraved lines and a plumb line or alidade), and large scale (formed by "visual rays" or sun rays, the horizon), then, given two sides of the small triangle and one of the large scale triangle, by computing the forth proportional. The literature usually lists problems to measure distances, heights and widths. Now, in the case of this particular version we do not have the traditional square, but a projection of the equal divisions of the square onto a circular arc, so that "evidently" the corresponding angles are conserved. There is TK both in the recognition that the divisions of the familiar shadow square can easily be projected onto another line without altering the usual way of solving the problems. And the knowledge of how to handle the instrument for solving these problems is also widely tacit.

*

In conclusion, we consider that instrument manufacturing and instrument literature can be considered as interlinked traditions where new instruments and new writings would always be inspired by preexisting instruments and writings. In this process, where a passage from manufacturing tradition to textual tradition and vice-versa is implied, choices need to be made about what to turn explicit, and what to leave tacit (implicit). By finding an explicit form for part of the notions these are then exposed to scrutiny, and this will often *transform* the knowledge about an object (instrument), or involved concepts, or even a whole underlying theory. On the other hand, the universal dial and the shadow square we saw may be cases where knowledge becomes less explicit, which can result in a kind of decay of the object.

References

[1] J. Regiomontanus, *Kalendarium*, ([Nürnberg]: 1474).

[2] D.A. King, *World-Maps for Finding the Direction and Distance to Mecca: Innovation and Tradition in Islamic Science* (Leiden: Brill 1999).

[3] Ch. Gall SI, *In Sphaeram [...]* (Lisbon: 1621), Biblioteca Geral da Universidade de Coimbra, ms. 192.

[4] C. Eagleton, "Medieval Sundials and Manuscript Sources: The Transmission of Information about the Navicula and the organum ptolomei in Fifteenth-Century Europe", in *Transmitting knowledge: words, images, and instruments in early modern Europe*, S. Kusukawa,I. Maclean eds., (Oxford: Oxford Univ. Press 2006), p. 41–72

[5] O. Finé, "Liber secundus de caeteris horologiis, tum anularibus & cylindricis, tum in circulo atque circuli quadrante descriptis [...],(fol. 181–201) in *De solaribus horologiis, et quadrantibus libri IIII*, in *Protomathesis opus uarium, ac scitu non minus utile quàm iucundum* (Parisiis: [impensis Gerardi Morrhij & Ioannis Petri], 1532).

Looping the Loop: Mathematicians and Bicycle Theory at the Turn of the Twentieth Century

David Aubin

If mathematical theories purporting to account for human actions are so notoriously deficient, it may be because they fail to capture much of the tacit knowledge that such actions necessarily entail. The most famous example of this in the literature no doubt is bicycle riding. This example was provided by Michael Polanyi himself at the time he introduced his ideas about the role of "personal knowledge" in science—which later became the catchier "tacit knowledge"—and has since assumed an iconic status among sociologists of science and technology. "If I know how to ride a bicycle," Polanyi famously wrote, "this does not mean that I can tell how I manage to keep my balance on a bicycle" [10, p. 4]. Repeated over and over again, this banal observation has become the centerpiece example of tacit knowledge. The success of this image certainly owes to the direct way in which it appealed to anyone's experience. For most people who know how to ride a bike, just the idea of trying to make explicit everything that is involved in managing this art is enough to convey the complexity of any attempt at discussing the role of tacit knowledge in human activities.

However, for several scientists and mathematicians at the end of the 19th century and at the beginning of the 20th century, the mathematical theory of the bicycle was hardly perceived as lying outside the realm of their capacities. As I shall discuss there even was a prize offered in 1898 to the author of the best solution to the bicycle stability problem by the Paris Academy of Sciences in Paris. This in fact gave rise to a number of publications on the topic.

Then, in March 2011, a team of scientists from the United States and—where else?—the Netherlands published an article in the prestigious journal *Science* purporting to disprove assumptions about bicycle self-stability that had been held for 140 years [8]. This seemed striking enough that an earlier paper by the same team [9] had been reported about in the press. It was once thought wheels acted as a gyroscope to keep the bike upright. But, according to these new studies, the secret was that there "is no one secret." As many as 17 different parameters were crucial, from the radius and mass of the wheels to the position of the centre of the mass of the bicycle, to the angle of the steering axis. "That is why it has taken 120 years to get it right. We have not found anything simpler," some told *The Daily Telegraph.* The team showed that articles written more than a century earlier by Carvallo and Whipple were on the right track, though they attributed credit for cracking the problem to German engineer E. Dhring, who published his meticulous study in 1955.

In my view, four groups of questions arise when we consider the relationship between bike riding and tacit knowledge :

(1) How did bike riding become the paradigmatic example of tacit knowledge?
(2) What is the place assigned to mathematical knowledge in discussions about bike riding as tacit knowledge? Does this have a history? Did mathematical theories of the bicycle have an impact on bike riding as tacit knowledge?
(3) What type of knowledge about bike riding, tacit or not, was important in formulating a mathematical theories of the bicycle? What other types of tacit knowledge, if any, entered the formulation of mathematical theory of the bicycle
(4) What does a consideration of tacit knowledge teach us about the role of mathematics in mechanics at the turn of the 20th century?

As can readily be seen, my intent is to make more explicit the tacit knowledge mathematicians mobilized for providing accounts of engines such as the bicycle. For doing do, I thus wish to use to use Polyani's theory of tacit knowledge to revisit the mathematical theories that may have inspired him. In other word, I would like to "loop the loop." Now, that's when the story has become really fascinating to me, because this expression—"looping the loop"—indeed arose in the domain of cycling. It originally was the name given to a stunt that consisted in riding a bicycle down a very long ramp and around a looping slide at considerable speed so as to stick to the ramp by the centrifugal force. It was extremely popular in all western countries in 1903–1904.

To explore some of the issues raised by this topic, I first examine the relationship between discourse about tacit knowledge and mathematics in the case of the stunt, using in particular an article published by the mathematician Carlo Bourlet [4]. This allows me to show that mathematics comes as a way to explicate the rider's tacit knowledge and deny him of almost all the skills that attracted the public. Like in his previous studies about the velodrome, Bourlet focuses instead on the explicable knowledge of the ramp builder.

Second, I focus on the way in which cycling was understood as early as the late 1860s as something that could not be reduced to verbal or mathematical descriptions. Cycling was "an art," explained its early theoretician Louis de Baudry de Saunier, because it relied on the rider's "natural gifts of skillfulness and cool–bloodedness and because no precise mathematical rule will ever be able to rule over the exercise" [1, p. 5]. The tension between cycling as an art and the mathematical account of the act of balancing a bicycle is present in many account of bicyle practice. Bicycle stability is often—though not always—attributed to the rider's constant counterbalancing the fall. For this a mathematical formula was provided for example by the British engineer Archibald Sharpe: "a body of mass W lbs., moving in a circle of radius r, with speed v, has a radial acceleration, $\frac{v^2}{r}$; and must be acted on by a radial force $\frac{Wv^2}{gr}$ lbs." [11, p. 203]. This is seen to resurface almost word for word in Polanyi's writings [10, p. 6–7].

To further analyze the role played by mathematical thinking in Polanyi's discussion of bicycle riding, categories of tacit knowledge introduced by Harry Collins are useful. Collins distinguishes relational, somatic–limit, and collective tacit knowledge (or, RTK, STK, and CTK) [7]. I argue that Polanyi says three things about the mathematical theory of bike riding: (1) we know how to ride a bike without any knowledge of a mathematical theory, that is, STK is acquired without explicating RTK; (2) a mathematical theory of bike riding will be of no help to learn how to ride an actual bicycle, or explicated RTK does not help in acquiring STK; and (3) any mathematical theory, including the mathematical theory of the bicycle, relied on tacit knowledge that will never be entirely eliminated, i.e., the act of explicating RTK itself relies on tacit knowledge.

Polyani however failed to account for the simple fact that bicycles are self–stable. At sufficient speed, they do not require the rider adjust constantly the handle bar to keep his balance. In other words, there are many situation where a rider's tacit knowledge consist in no more than refraining from doing anything that might upset his bike's self–stability. The most valuable skill, then, lies perhaps not in riders, but in bike designers who are able—or not—of producing a highly stable engine. Of course, this skill may not be entirely tacit knowledge, and if it is, this seemed merely to be RTK.

This perspective is the third angle I want to adopt to study the relationship between tacit knowledge and mathematical theories in the context of bike riding.

From this perspective, one may understand why bicycle self–stability might be interesting to pose as a mathematical problem, as the Academy of Sciences did in 1898. Looking at the several detailed contributions submitted at the time, one encounters one of those fascinating examples of a problem studied from a great variety of mathematical perspectives. Boussinesq produced a classic study of mechanical equilibrium [5]; Bourlet extracted a mathematical memoir from his longer and more complete studies [3]; Carvallo introduced Lagrange multipliers to deal with the hoop and the bicycle [6].

To approach more systematically this set of publications from the point of view of tacit knowledge would be interesting. What to draw? How to chose coordinates, parameters and variables? How to derive differential equations of motion? What approximation to make in order to ease the solution of the problem? How to solve differential equations, analytically, numerically, graphically; when to use what? How to draw practical conclusions from the mathematical analysis? All of these questions needed to be answered in deriving a theory of the bicycle and, for most, there were no explicit rules telling the theorist how to do it. Once again, however, this may mostly lie in the realm of RTK.

As far as bicycle theories are concerned, one may consider whether being a cyclist oneself played a role. I would like to suggest that it may have. As a student of the École normale supérieure, Bourlet had caused quite a sensation in showing up to the Sorbonne in 1888, wearing shorts and riding his machine. His publication relied on a rather striking array of sources characterized by its eclecticism [2]. Besides the specialized treatises, a disparate group of periodicals were mentioned. They included scientific journals like the *CRAS* (article by Marey and Bouny), the semi–popular scientific press (articles by Guye, Hospitalier and Jacquot in *La Nature*, by Gérard Lavergne in *La Revue scientifique*); the technical press like the *Revue d'artillerie* and the *Revue mensuelle du Touring-Club de France*; and the specialized cyclist press (*La Bicyclette*, *Le Cycliste*, *Vélo-sport*). Many of the authors were barely identified by initials or by pseudonyms: The mysterious "Man of the Mountain" figured prominently among Bourlet's sources. Here at last, I see, CTK playing a great part in producing a mathematical theory of the bicycle.

From this limited study, I would like to draw the following preliminary conclusions:

- Bike riding become an classic example of tacit knowledge (in part) due to the fact there were unintelligible mathematical theories about it!
- Mathematical studies of the bicycle were helpful in transforming STK into RTK (or explicit knowledge).
- They relied quite a bit on mathematical explicit and relational tacit knowledge (RTK), perhaps a little on the practice of cycling (STK), but also in important ways on collective tacit knowledge (CTK) .
- Expanding our scope to other countries, the various approaches toward stability problems in mechanics that seemed to be present here foreshadowed research styles in the 20th century (French mathematical, German physical and British engineering approaches).

References

[1] L. Baudry de Saunier, *Histoire générale de la vélocipédie*, Ollendorf, Paris, 1891.
[2] Carlo Bourlet, *Nouveau traité des bicycle et bicyclettes.* 2 vols. Paris: Gauthier-Villars, 1898.
[3] Carlo Bourlet, Étude théorique sur la bicyclette, *Bulletin de la Société mathématique de France* **27** (1899): 47–67, 76–96.
[4] Carlo Bourlet, Le Passage de la boucle — Looping the Loop, *La Science au XXe siècle*, 1903, 88–91.
[5] Joseph Boussinesq, Aperçu sur la théorie de la bicyclette. *Journ. de Math.* (5) *5*, 1899, 117–135.
[6] Emmanuel Carvallo, Théorie du mouvement du monocycle et de la bicyclette. *Journ. de Pol.* (2) **5**, 1900, 119–188; (2) **6**, 1901, 1–118.
[7] Harry Collins, *Tacit and Explicit Knowledge*, Chicago: The University of Chicago Press, 2010.
[8] J. D. G. Kooijman, J. P. Meijaard, Jim P. Papadopoulos, Andy Ruina, and A. L. Schwab, A bicycle can be self-stable without gyroscopic or caster effects, *Science*, 2011.
[9] J. P. Meijaard, Jim P. Papadopoulos, Andy Ruina, and A. L. Schwab, Linearized Dynamics Equations for the Balance and Steer of a Bicycle: A Benchmark and Review, *Proceedings of the Royal Society* **A463** 2007, 1955–1982.
[10] Michael Polanyi, The Logic of Tacit Inference, *Philosophy* **41** 1966, 1–18.
[11] Archibald Sharpe, *Bicylces and Tricycles: An Elementary Treatise on Their Design and Construction with Examples and Tables*, Longmans, Green, and Co., London, 1896.

Explicit versus Tacit knowledge in scientific computing in Berlin (1870–1933)

Ulf Hashagen

It is a well known fact that Michel Polanyi regarded methods of measurement and data analysis as the key examples of tacit knowledge being largely resistant to verbalization and unequivocal codification [4, 5]. Whereas many studies on 18th and 19th century experiments and experimental methods have been published during the last decades, the general history of data analysis in the sciences was so far restricted to studies on the historical development of probability theory and statistics [3]. This lecture aimed at giving a "longue durée" survey of the development of scientific computing in astronomical research in Berlin in the German Kaiserreich in order to test if the concept of tacit knowledge can help to write a history of a scientific institution being preoccupied with data analysis.

The scientific institution to be considered here is the *Astronomische Rechen-Institut* (Astronomical Computing Institute) in Berlin founded by the influential astronomer Wilhelm Foerster (1832–1921) in 1873/74 in order to publish the astronomical almanac *Berliner Astronomisches Jahrbuch*—an annual publication describing the positions of the planets and stars during a year. Besides, by the founding of an Seminar for Scientific Computing at the Friedrich-Wilhelms-University in Berlin affiliated to the *Astronomische Rechen-Institut* Foerster made an effort to institutionalize scientific computing as a discipline at a German university in order to introduce students of mathematics and the exact sciences to the theory and practice of scientific computation. Whereas Foerster failed to institutionalize scientific computing as a new scientific "cross-discipline"—the seminar's disciplinary influence remained limited to astronomy and it proved not be possible to contend against the methodological ideal of pure mathematics in German universities and to overcome the disciplinary boundaries between mathematics, physics and astronomy—the *Astronomische Rechen-Institut* proved to be a successful innovation for German astronomy [2].

During the next decades the institute strengthened its institutional basis under the direction of a director (1874-1895 Friedrich Tietjen, 1896-1909 Julius Bauschinger, 1909-1922 Fritz Cohn) who was at the same time professor of theoretical and computational astronomy at the Friedrich-Wilhelms-University in Berlin. In 1900 more than half a dozen astronomers were employed as astronomical computers in order to compute the ephemerides of the astronomical objects. While the available sources give some hints that besides the forms of explicit knowledge on the determination of orbits of planets and asteroids (being published by the members of the *Astronomische Rechen-Institut* (e.g. [1])) many forms of tacit knowledge were used in this monotonous scientific work of data analysis, it proved unfortunately extremely hard to get deeper insight in this forms of knowledge, since no sources on the daily computational workload of the computers in the *Astronomische Rechen-Institut* and of the computations of the students in the Seminar for Scientific Computing are available. This must have something to do with the fact that the daily computational work of the astronomers was not seen as their real scientific work—therefore most of the computers worked on other astronomical problems after the end of the office hours in the *Astronomische Rechen-Institut* and used this work for their self-definition as scientists.

However, the research of historians of physics on 18th and 19th century experiments and experimental methods may be used as guideline for further research in the future. The historiographic methods of replication of historical experiments developed in history physics during the last decades [6], could probably applied successfully to the history of scientific computation and of data analysis in the sciences.

References

[1] J. Bauschinger, *Die Bahnbestimmung der Himmelskörper*, Engelmann, 1906.

[2] U. Hashagen, *Rechner für die Wissenschaft: "'Scientific Computing"' und Informatik im deutschen Wissenschaftssystem 1870-1970.*. In: U. Hashagen and H. D. Hellige (Eds.): Rechnende Maschinen im Wandel: Mathematik, Technik, Gesellschaft. Festschrift für Hartmut Petzold zum 65. Geburtstag, Deutsches Museum, München, 111-152.

[3] L. Krüger, L. and G. Gigerenzer and M. S. Morgan (Eds.): The Probabilistic Revolution, MIT Press, 1987.

[4] K. M. Olesko, *Tacit Knowledge and School Formation*, Osiris 1993, 8, 16-29.

[5] M. Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy*, University of Chicago Press, 1958.

[6] C. Sichau, *Die Replikationsmethode: Zur Rekonstruktion historischer Experimente*. In: P. Heering and F. Riess and C. Sichau (Eds.): Im Labor der Physikgeschichte. Zur Untersuchung historischer Experimentalpraxis, BIS-Verlag, Oldenburg, 2000, 9-70.

Guessing an algorithm beyond numbers displayed on a clay tablet: a sample for Old Babylonian period

Christine Proust

In this paper, I discussed a mathematical cuneiform text which seems, at first sight, to reflect an almost entirely tacit knowledge, as it doesn't contain a single word, but only the graphemes for 1 and 10. The image of the tablet is available online ([1], number P54479). In the same time, as this text was first understood and interpreted by Abraham Sachs (1947), I also discussed this later paper. I examined both the tablet CBS 1215 and Sachs' paper in order to show:

- How a lot of information may be conveyed by other means than words
- How modern interpretations of such a text may inform us more about the tacit knowledge of the modern observers than about the ancient methods.

Most of the arguments I develop here will be found in a chapter of the forthcoming book on mathematical proof edited by Karine Chemla ([3]).

Tablet CBS 1215 comes from illegal excavations, and thus its provenience is unknown. However, the paleography, the type of the tablet and the content of the text indicate that the document dates from the Old Babylonian period (early second millennium, OB in the following), and comes from southern Mesopotamia. The text is made of 3 columns on the obverse, and 3 columns on the reverse. As was usual at that time, the columns run from left to right on the obverse of the tablet, and from right to left on the reverse. The text is divided into 21 boxes. The entries in the boxes are: 2.5; 4.10, the double of 2.5 in sexagesimal place value notation (see below); 8.20, the double of 4.10, and so on until 2.5×2^{20}. In each box, the text ends with the same number with which it started.

The sexagesimal place value notation (SPVN) is a numerical system attested, in the Old Babylonian period, almost only in mathematical texts. The 59 "digits" of the numeration are noted by means of the repetition of the graphemes 1 and 10 as many times as necessary. The numbers over 60 are sequences of digits where a sign in a given place represent sixty times the same sign in the preceding place (on its right). No mark in the notation indicates the place of the absolute unit, that is to say, a number noted with three wedges may represent 3, 3/60, or 3×60, and so on. The notation is floating. The SPVN was taught to the young scribes during the first stage of their education in the OB scribal schools. Notions taught at the mathematical elementary level constitute a knowledge shared by the educated scribes of the time, and could be qualified as a kind of "tacit knowledge". This background included metrological systems, SPVN, and numerical tables, that is, a set of elementary results: reciprocals, multiplications, square roots, cubic roots. The reciprocal table, which was probably memorized by the educated scribes, is of special interest for what follows. This table provides the reciprocal of a standard list of regular numbers (regular numbers are numbers which admit a finite reciprocal in base 60). This list includes the following pairs: (2, 30); (3, 20); (4, 15); (5, 12); (6, 10); (8, 7.30); (9, 6.40); (10, 6); (12, 5); (15, 4); (16, 3.45); (18, 3.20); (20, 3); (24, 2.30); (25, 2.24); (27, 2.13.20); (30, 2); (32, 1.52.30); (36, 1.40); (40, 1.30); (45, 1.20); (48, 1.15); (50, 1.12); (54, 1.6.40); (1.4, 56.15); (1.21, 44.26.40).

The interpretation offered by Sachs ([4]) allowed historians to understand the tablet CBS 1215, which had remained mysterious until 1947. Sachs discovered the keys to the text of CBS 1215, as well as many parallel texts. In his article, after recalling some basic mathematical tools used by the ancient scribes, namely the SPVN and the reciprocal table, Sachs presents the general method for computation of reciprocals ([4], §12): "Let c denote the regular number whose reciprocal one wishes to find. Then choose two numbers a and b, such their sum is c and such a is a number which is found in the standard table of reciprocals." Thus, the reciprocal can be found by the formula:

$$\overline{c} = \overline{a+b} = \overline{a} - \overline{(1+b\overline{a})} \tag{1}$$

Then Sachs states that, "The boxed identity [the formula (1) above] is the key to the generally accepted Old-Babylonian procedure for finding the reciprocal of a regular number c which is not contained in the standard table of reciprocal." But from where comes this "generally accepted Old-Babylonian procedure"? In fact, Sachs' description of the procedure is based on the tablet VAT 6505, published by Neugebauer in 1935 ([2], vol. I, p. 270; [1], P25921). Indeed, VAT 6505 contains the only known worded text referring to the procedure for finding a reciprocal. The provenance of VAT 6505 is unknown. It dates probably form the OB period. It is now kept in Berlin. The text of VAT 6505 was originally composed of 12 sections, as attested in the colophon, 5 of which are preserved at least partially. The text is very systematic and repetitive, thus the reconstruction of the 7 damaged sections is possible. If we compare the seven first sections of the two tablets, we find in VAT 6505 the same numbers as in CBS 1215, in the same order. However, the Berlin tablet contains words, while the Philadelphia tablet does not. It appears that VAT 6505 provides explanations which seem to be absent from CBS 1215, as shown below in Table 1.

VAT 6505 #7	CBS 1215 #7	
1. 2,[13],20 is the *igûm*. [What is the *igibûm*?]	[2].**13.20**	18
2. [As for you, when you] perform (the operations),	**[40]**	1.30
3. take the reciprocal of 3,20 ; [you will find 18]	**[<u>27</u>]**	2.13.20
4. Multiply 18 by 2,10; [you will find 39]		
5. Add 1; you will find 40.		
6. Take the reciprocal of 40; [you will find] 1,30.		
7. Multiply 1,30 by 18,		
8. you will find 27. The *igibûm* is 27.		
9. Such is the procedure.		

FIGURE 1. Table 1: comparison of section 7 of VAT 6505 and CBS 1215

After presenting what he called "the Technique" for reciprocal computation, Sachs "applied" it to VAT6505 as an "example". But in fact, one can easily guess that the opposite happened: first, the worded text VAT 6505 was interpreted; second, "the generally accepted Old-Babylonian procedure" was shaped as formulated in [4] §12. Indeed, this text does correspond well to the Sachs formula. Then, Sachs "applied" the "Technique" to CBS 1215. His interpretation is displayed in a table where the columns are headed by the elements of his formula ([4], 238-240). The rows related to #7 and #20 are reproduced below in Table 2.

c	b	a	$\bar{a}$	$1+\bar{a}b$	$\overline{1+\bar{a}b}$	$\bar{c}$
#7						
2,13;20	2,10	3;20	0;18	40	0;1,30	0;0,27
0;0,27						2,13;20
#20						
5,3,24,26;40 45,30,40 1,8,16 4,16	5,3,24,20 45,30, 0 1,8,0 4,0	6;40 40 16 16	0;9 0;1,30 0;3,45 0;3,45	45,30,40 1,8,16 4,16 16	0;0,0,1,19,6,5,37,30 0;0,0,52,44,3,45 0;0,14,3,45 0;3,45	0;0,0,0,11,51,54,50,37,30 0;0,0,1, 19,6,5,37,30 0;0,0,52,44,3,45 0;0,14,3,45
0;0,0,1,19,6,5,37,30 23,43,49,41,15 1,34,55,18,45 25,18,45 6,45	0;0,0,0,11,51,54,50,37 23,43,49,41,0 1,34,55,15,0 25,15,0 6,0	0;0,0,0,0,0,0,0,0,30 15 3,45 3,45 45	2,0,0,0,0,0,0,0,0 0;4 0;0,16 0;0,16 0;1,20	23,43,49,41,15 1,34,55,18,45 25,18,45 6,45 9	0;0,0,0,0,2,31,42,13,20 0;0,0,0,0,37,55,33,20 0;0,0,2,22,13,20 0;0,8,53,20 0;6,40	5,3,24,26;40 0;0,0,0,0,2,31,42,13,20 0;0,0,0,0,37,55,33,20 0;0,0,2,22,13,20 0;0,8,53,20

Figure 2. Table 2: "the technique" applied by Sachs to #7 and #20 of CBS 1215 ([4], 238-240).

When observing this table, we note that something does not fit the cuneiform text: after 'placing the semicolon arbitrarily" ([4], 227) Sachs makes an impressive use of zeros. The text of CBS 1215, however, does not use such marks. The numbers of CBS are written in floating notation, as is usual in the cuneiform texts.

Let us examine again CBS 1215 #7.

CBS 1215 #7		Commentary
[2].**13.20**	18	18 is the reciprocal of 3.20. 3.20 is a regular number, and at the same time the « trailing part" of the number 2.13.20, whose reciprocal is sought. Thus 2.13.20 is divisible by 3.20.
[40]	1.30	18 × 2.13.20 = 40, and the reciprocal of 40 is 1.30. That is, 2.13.20 is the product of the two factors 3.20 and 40. The reciprocal of these two factors are respectively 18 and 1.30, which are disposed in the right sub-column.
[<u>27</u>]		18 × 1.30 = 27, which is the reciprocal sought.

Figure 3. Table 3: interpretation of CBS 1215 #7

No addition occurs in this computation, unlike in Sachs' formula. The calculation is based on the factorization of the number whose reciprocal is sought. The regular factors appear as "trailing part" of the numbers. The numbers are arranged on the clay tablet in such a way that the factors of the sought reciprocal appear one after the other in the right sub-column. At the end of the factorization process, one has only to multiply the numbers placed in the right hand sub-column (see for example section #20 of CBS 1215).

To conclude, a worded text such as VAT 6505, is not more informative than a strictly numerical text such as CBS 1215. The information may be conveyed by media other than words. In CBS 1215, the disposition of the numbers provides explanation of the calculations, as well as a powerful tool in order to perform the algorithm.

Sachs based his interpretation on the idea that the numbers was decomposed into sums, although there is no sum in CBS 1215 and in the other known parallel sources. This idea was suggested to him by the worded text VAT 6505, considered as more explicative. The alleged presence of an addition in the calculation, as well as the use of an algebraic formula to describe the procedure, led Sachs to determine the order of magnitude of the numbers, and thus to use a lot of "zeros" (see Table 2). But the analysis of CBS 1215 shows that the calculation is based only on multiplications and reciprocals, which does not require any determination of the orders of magnitude of the numbers on which these operations act. This floating calculation confers simplicity and power to the computation.

References

[1] Englund, Robert K., Peter Damerow, and Jürgen Renn. *Cuneiform Digital Library Initiative.* University of California, Los Angeles/Max Planck Institute for the History of Science, Berlin (CDLI, http://cdli.ucla.edu/).

[2] Neugebauer, Otto. 1935-7. *Mathematische Keilschrifttexte I-III.* Berlin: Springer (MKT I-III).

[3] Proust, Christine. 2012. "Interpretation of Reverse Algorithms in Several Mesopotamian Texts" in *History of mathematical proof in ancient traditions: the other evidence*, edited by K. Chemla. Cambridge: Cambridge University Press.

[4] Sachs, Abraham J. 1947."Babylonian Mathematical Texts 1." *Journal of Cuneiform Studies* 1:219-240 (BMT I).

Euclid's II.5: Pure Geometry, Geometrical Algebra and Tacit Knowledge

Leo Corry

The "geometrical algebra" interpretation of Greek mathematics is a most peculiar instance of a historiographical approach that gives preeminence to "tacit knowledge", though in a sense of the term which substantially differs from any of those explicitly listed in the prospectus of the present workshop. Here the explicit historiographical claim is that Greek geometers had a full-fledged and well elaborated system of algebra thought, but that this algebra was systematically left out of the texts. Hence, in this interpretation, Greek mathematics was in its outer, explicit embodiment "geometry" but it was "algebra" in its tacit, underlying essence.

My talk considers some long-term historical developments related to the idea that the geometrical results embodied in the works of Euclid's *Elements* can be interpreted algebraically. More specifically, I approach this idea by focusing on a single result of the *Elements*, Proposition II.5. By analyzing selected texts produced in changing historical contexts, one can see that, while symbolic manipulation and other mathematical ideas that we typically associate with algebra were incorporated in various ways to variant versions of the proof of II.5 from as early as the Greek commentators of Euclid, none of these additions or their combination did ever imply a definite change of orientation that all subsequent authors felt compelled to follow. At various times and up until the nineteenth century, one can still find mathematicians who preferred, for different reasons and in changing circumstances, to move back and forth from a purely geometrical to a more algebraically-oriented approach to Book II of the *Elements*, and particularly to II.5. Thus, while (following Sabetai Unguru) I reject the main thesis of the "geometrical algebra" interpretation, I will try to indicate how the changing versions of the proof variously adopted and discarded arithmetic, algebraic and proto-algebraic elements. In some cases these elements appeared explicitly and in some other cases they appeared tacitly.

In a written version of this talk I have analyzed a long list of relevant mathematicians who have presented interesting versions of the theorem. I start with the ancient world by looking at the work of Heron of Alexandria. Then I move to the Islamic world and consider works by Al-Khowarizmi, Thabit ibn-Qurra and Al-Nayrizi. In the late Middle Ages I examine the works of Gersonides and Barlaam. Then I move to the European Renaissance, where I focus on the works of Ghaligai, Clavius, Tacquet, Bonasoni and Hérigone. Next is the British Versions of the theorem in the 16th and 17th Century, looking at the works of Recorde and Billingsley, Harriot and Oughtred, Wallis and Barrow. This is followed by an analysis of Euclid in Europe during the Late 18th and Early 19th Centuries, which is analyzed via the French Textbooks of Legendre, Peyrard, and Lacroix and some Italian Textbooks of the 19th Century. Finally, at the turn of the 20th century I consider those Historians of Mathematics who were involved with the Geometrical Algebra interpretation: Tannery, Heiberg, Zeuthen and Heath. Against the background of all the previously analyzed texts one can more easily see how the works of these historians emerged from a consideration not just of the original text of Euclid, but of that text together with all other versions that accompanied its development throughout history.

Explicitly tacit knowledge in Diophantus

Alain Bernard and Jean Christianidis

As far as the general theme of the workshop is concerned, we took our point of departure from Collins' fundamental remark that 'tacit' knowledge is only tacit because it is compared to explicit or potentially explicit knowledge, so that the tension between the two is really the important point ([5],7). Moreover, we decided to follow Collins' method, which basically consists in classifying type of explicit as well as tacit knowledge: so that, as far as Diophantus is concerned, we try to propose our own classification of kinds of tension between explicit and tacit knowledge. Finally, we took up K. Chemla's important remark, that explicit vs. tacit knowledge is not only a category of modern sociologists, but that it might be considered as a category of the actors themselves hence the title of our talk, which insists on this aspect that we find plainly relevant for Diophantus.

Our talk and our final classification was based on a recently published paper [3] in which we proposed a new analytical framework for the analysis of Diophantus's *Arithmetica* I-III. This work is itself based on a suggestion contained in the first lines of a paper published by one of us [4], namely that the interpretation of Diophantus's project would gain from being interpreted in the light of ancient rhetorical concepts and practice (like invention, *heuresis*). Finally, we are now working on a third paper, in which a critical comparison between the structure of Diophantus's *Arithmetica*, and ancient 'preparatory exercises' (*progymnasmata*) will be proposed. The talk helped us to clarify the argument of this paper.

Diophantus's *Arithmetica* was originally composed of a long introduction and thirteen books of arithmetical problems with their solutions, the unfolding of each solution following a relatively standard scheme. In the works mentioned above, we took into account only the three first books of problems, which are extant in Greek. Our first remark, around which we organized our presentation, is that the tension between tacit vs. explicit knowledge is actually present in both, let alone because some aspects left implicit in the introduction are made clear within the problems. We therefore proposed some remarks on the two parts (I = introduction, P = problems)

Concerning the introduction, we insisted on [I.1] the very first lines ([1] Arithm. 2.3-13), often neglected by historians although they contain heavy allusions to key concepts of ancient rhetoric, like the notion of invention (*heuresis*, lat. *inventio*), which is itself related to the notion of problem (*problêma* on which see [3], 5-12), the notion of progressive familiarity with the subject matter or the explicit parallel between the desire of the learner and the model provided by a master. We do believe that these hints at ancient rhetorical culture, which might be considered a first kind of (explicitly) tacit knowledge, should be taken into account for an understanding of the coherency of Diophantus's project as a whole.

[I.2] Furthermore, the bulk of Diophantus's introduction insists on the fact that "[arithmetical problems] are solved (*lyetai*) if you follow the way (*hodos*) that I will show" ([1], 4.10-11). What is first explained, within the introduction, are the elements of the 'machinery' that makes it possible to follow the way indicated. In a nutshell, the elements of this machinery are exposed in a specific order within the introduction: (a) About the numbers in the statement ([1], 2.18-4.10); (b) about the 'numbers' or 'species', which are the elements of the so-called "arithmetical theory" ([1], 4.12-6.21); (c) about the operations on the latter; and (d) about the equation and the operations on it. Concerning (b), we insist that there is here a new kind of explicitly tacit knowledge: for the 'arithmetical theory' is explicitly introduced as something already known to the reader: Diophantus refers to the 'arithmetical theory' by using the verb "it is approved", *edokimasthê*, in the past tense; moreover, only an "operational outline" of this theory is recalled in the introduction.

[I.3] Finally, the introduction ends up with an important statement, again often neglected by historians, that makes the transition with the problems ([1], 14.25-16.7). The statement is expressed in a very allusive language and comes back to the notion of progressivity of the problems (associated to the underlying learning) and to the notion of 'way', about which it then becomes clear that it is partly bestowed on the working out and examination of the problems themselves. We do believe that what is (here) tacitly at stake is the establishment of the correspondence between numbers in the statements and species of the 'arithmetical theory': learning how to build it is done through the problems and only there, and even the fact that it is done in a progressive and semi-systematic way is here always alluded to.

Concerning the series of problems contained in Arithmetica I-III, we insisted on the following observations: [P.1] the treatment uniformly follows a standard scheme that can be summarized in the following steps: (a) enunciation or statement of the problem; (b) transfer of the statement within the terms of the 'arithmetical theory'; (c) outcome of the transfer, as an equation; (d) solution of the equation; and (e) finding out the numerical values of the sought numbers called for. The 'generality' of the scheme is understood both from the fact that it corresponds, for its main part (a-d) to what is exposed in the introduction and from the fact that it is used in a repeated manner, even if some steps (mainly c-e) becomes more and more allusive as soon as progresses into the problems.

[P.2] Furthermore, what appears crucial is the step (b), for which we introduced in [3] 26-31 the notion of 'method of invention', meaning any repeated way, by which any correspondence between a number indicated in the statement, and an aggregate of species, is established. The basic result of our 2012 paper [3] is that these methods, once inventoried, are few in number (about 11) and that they are used in order of growing complexity all along the three first books of *Arithmetica*. Although these are not made explicit, in the sense that they are not named, in Diophantus, there are still made recognizable by stylistic or semantic 'marks' that can be identified within the text, so that we have again, here, a tension between tacit- and explicitness.

[P.3] We finally focused on one specific method of invention, the method of simulation [3] 46-53, which is each time bestowed on the clever use of familiar algorithms or procedures that we call 'simulators'. Although these simulators are explicitly stated within the solution, their provenance is rarely justified: it all happens as if the reader was meant to have a kind of 'procedural culture' enabling him to invent solutions from this tacit knowledge. Therefore we have here, again, a new kind of tension between tacit and explicit knowledge.

To conclude: from this provisory exploration, we see that there exists in Diophantus various forms of tension between explicit and tacit knowledge–our list is only a first attempt and could be easily developed by underlying more aspects of this sophisticated work. Meanwhile, the two most important of them, as far our current research is concerned, are the allusions to rhetorical culture (that we are now trying to make explicit in the work in progress on Diophantus and ancient *progymnasmata*) and the systematic resort to a kind of variegated 'procedural culture' that is also a kind of fundamental background to Diophantus's project (about which we also plan a new paper). We believe, indeed, that more exploration of these two backgrounds, either separately or in isolation from each other, is still needed and might open the way to a convincing contextualization of Diophantus's work, that is still lacking.

References

[1] *Arithm.* Diophantus's *Arithmetica* in Tannery's edition: Diophanti Alexandrini Opera Omnia cum Graeciis commentariis, edidit et latine interpretatus est P. Tannery, 2 vols. Leipzig: B.G. Teubner. (Reprint Stuttgart: B.G. Teubner 1974)

[2] Bernard, A. 2003. Sophistic Aspects of Pappus's *Collection*. *Archive for History of Exact Sciences* 57: 93-150.

[3] Bernard, A. and Christianidis, J. 2012. A new analytical framework for the understanding of Diophantus's *Arithmetica* I-III. *Archive for History of Exact Sciences* 66: 1-69.

[4] Christianidis, J. 2007. The way of Diophantus: some clarifications on Diophantus's method of solution. *Historia Mathematatica* 34: 289-305.

[5] Collins, H. 2010. *Tacit and Explicit Knowledge*. Chicago: Univ. of Chicago.

Understanding a Mediæval Algorithm: a Few Examples in Arab and Latin Geometrical Traditions of Measurement

Marc Moyon

Taking into account written texts from Arab and Latin traditions of the geometry of measurement[1], our main purpose is to describe several elements of algorithms in order to analyze how part of their explicitness and tacitness could help the historian of mathematics to understand computations.

After the introduction where the context is briefly exposed, we will focus on two different classical examples of the geometry of measurement. The first one is a series of problems on rectangles where additive relations on area, length and width are given, and it is necessary to find both length and width. The second problem is a sharing of land between heirs.

Introduction.

(1) How can we understand algorithms in mediæval texts which are often, at first sight, obscure for a present-day reader? (2) Are we able to describe elements which guarantee the correctness of those algorithms? And especially, how can the author, and later on readers and finally historians, of these kinds of algorithms,

[1] The geometry of measurement is called in Arabic classifications of sciences (from the ninth century) and in geometrical texts themselves : *ᶜilm al-misāḥa* or *ṣināᶜat at-taksīr*. In the Latin world, from the 12th century, this kind of texts belongs to the corpus of *Practica geometriæ*.

be sure that the given solution, following the algorithm step by step, answers the problem?

Here, the notion of *transparency* of an algorithm proposed by K. Chemla[2] could appear as a key concept. Unfortunately, reading algorithms given in mediæval texts of measurement, almost all, if not all, are not transparent. Indeed, each step gives us the number established by computation but it does not make the meaning of the computations and of the magnitudes explicit. We are in presence of tacit knowledge, at least in formulation. But, what kind of tacit knowledge is it exactly? Is it, for example, a tacit formulation wanted by the authors themselves to transfer their knowledge as clearly as possible or something else?

Thus, several other fundamental questions can be formulated by historians of mathematics: 1) How can we understand and interpret numbers in algorithms? 2) How and in how far are we legitimate to reconstruct steps in algorithms which seem lacking? and last but not least, 3) What kind of proof of the correctness of the algorithm could we establish?

These three questions strictly depend on what is tacit and explicit in mathematical texts. In most cases, only the mathematical tradition (here the geometry of measurement) and our knowledge of the cultural context (here, Islamic mathematics and its appropriation by Latin Europe) can help us to overcome difficulties. That is we want to show here.

Series of Problems: Let $A + \alpha w + \beta L$ and $L - w$ be given, with $\alpha, \beta \in \{-2, -1, 0, 1, 2\}$. L, w ? (If A area, L length and w width of a rectangle)

We focus on two main texts dealing with this kind of problem. The first one is the *Risāla fī t-taksīr* written by Ibn ᶜAbdūn from the tenth century[3]. The second one is the *Liber mensurationum* which is an Arab-Latin translation probably made in the twelfth century by Gherardo Cremona in Toledo. The author is only known by part of his name : Abū Bakr which is not sufficient to identify him[4].

These two texts are "texts of procedures", that is to say: they are exclusively composed of series of problems all structured on statement and algorithm of resolution. Geometrical or arithmetical proof does not complete the text.

This type of problem is interesting for several reasons, and in particular because the 'tacitness' can be specified at different levels[5]. The first level is about the

[2] *"The text of the algorithm mentions the evolution of the values computed, while also progressively providing a geometrical interpretation of the result for each step. Therefore, finally, the 'meaning' of the algorithm's result will be determined. The correctness of the procedure is established only if the meaning of the result corresponds to the magnitude sought"* [1], p.260

[3] This text is only known from one manuscript kept in the French National Library. It is interesting to add that, as far as we know, this copy comes from the Umarian Library of Segou (Mali) [4].

[4] We know this text thanks to, at least, 5 copies held in Paris (for 3 of them) and in Cambridge and Dresden [6].

[5] In order to respect the editorial lines of this Report, we cannot illustrate our example by the text. So, we restrict our purpose to the main ideas inviting to read several other works.

numbers used in the computations. Then, in order to understand the algorithm, we have to know precisely what magnitude each numerical value represents tacitly. It is a necessary condition to write a mathematical analysis authorizing us to formulate tacit steps. Thus, the fundamental question is to know the reason why the author didn't write some steps: it may be of his own volition or the text we know can be corrupted.

The second level is naturally linked to the previous one, it is about the correctness of algorithms. We have to render the tacit explicit, in particular giving a geometrical interpretation of the numerical problem[5]. Here, we must note the historical evidence with the *De arte mensurandi* completed by Johannis of Muris (14th c.). Finally, I would like to add that the alternative algorithm using *Algebra* given by Abū Bakr could be considered as a proof of correctness[6].

The last level of tacitness we would like to announce is linked to the organization of the series of problems. First of all, each problem is tacitly written to be taken in a general way, e.g. "each time that you have this kind of problem, do... ". This point is reinforced by the following one. Indeed, we think that authors organize their series of problems in order to elaborate a pseudo-theory with all possible cases[7].

Sharing land between heirs: a socio-cultural problem borrowed by authors of mathematics. A case study in the geometrical text of Ibn Ṭāhir al-Baghdādī([9], p.372–373).[8]

The structure of this problem is really different from the previous ones. It composed of a statement, a general algorithm, an example, a generalization with tacit conditions, computations and a proof by verification. Even if the algorithm given is totally explicit (no step seems to be tacit), it appears totally obscure and its reading is not sufficient to understand and generalize it. Indeed, several types of tacit knowledge appear necessary. We will give three major features. First of all, the type of sharing is determined *a priori*. In this case, the diagram helps us to understand the sharing. Secondly, the number of heirs is not the number of shares. But the last number is given by Islamic law which is not exposed in the mathematical text. Here, we are in a case where 'tacit' is explicitly mentioned with special emphasis on social and religious knowledge. It remains, for us (present-day readers), obscure due to the lack of explanations. Last but not least, Ibn Ṭahir presents a general algorithm. Each step is detailed and even executed for the readers to know exactly what computations to do. Each of them is explained by the general procedure, nevertheless the author doesn't indicate why this algorithm is correct. In particular, the author works on the number but not on the magnitudes they represent. The proof he writes is also restricted to checking if the shares are equal. The correctness of the algorithm remains a tacit data in this context:

[6]Abū Bakr will be followed by Fibonacci (13th c.) and then Johannis of Muris (14th c.) [8].
[7]See, for example, problems #23, #24 and #25 in the text of Ibn ᶜAbdūn [3].
[8]This problem can be read in an English version in [7], p.535–536

everything is done as if the reader knows for sure that this algorithm is correct.

Conclusion

Reading the geometrical texts from the *corpus* of misāḥa, I cannot agree with Polanyi's definition of 'Tacit Knowledge' quoted by the sociologist Collins in the first pages of his *Tacit and Explicit Knowledge*[2], e.g. knowledge that cannot be made explicit, that cannot be expressed in words, sentences, numbers or formulas.

Indeed, in this paper, the examples mentioned show that a part of the work of the historian of mathematics, is precisely to *make the tacit explicit*. However the 'tacit knowledge' should be defined.Indeed, in the case of our survey, knowledge is always transmitted from person to person by books even if we can not ignore the eventual apprenticeship but we can control or modelize it several centuries later.

We should not forget either that the authors write their texts in order to be read by their contemporaries who share *habitus*, common education and so on. These authors cannot guess that their texts will circulate in other regions (like the epistle of Ibn ᶜAbdūn written in Andalus and found in a sub-Saharan library). They cannot guess either that it will be chosen to be translated in order to be used in another linguistic tradition (in the case of the book of Abū Bakr) or to be borrowed (directly or indirectly) as an obvious source by posterior mathematicians to produce their own text (Johannis of Muris and Fibonacci with the *Liber Mensurationum*). Thus, historian has only a selection of texts which is the result of an historical and social process. In this case, tacit knowledge is 'tacit' only keeping in mind that the sources that we have are incomplete. The local and oral traditions cannot be the only answer to characterize or elucidate this 'tacit' as it is often made.

References

[1] K. Chemla, *Proof in the Wording: Two Modalities from Ancient Chinese Algorithms* In G. Hanna, H. N. Hahnke and H. Pulte (eds),*Explanation and Proof in Mathematics Philosophical and Educational Perspectives*, New-York/Dordrecht/Heidelberg/Londres: Springer, 2010, 253–285.

[2] H. Collins, *Tacit & Explicit Knowledge*, Chicago-Londres: The University Chicago Press, 2010.

[3] A. Djebbar, *ar-risāla fī t-taksīr li Ibn ᶜAbdūn: shāhid ᶜalā al-mumārasāt as-sābiqa li t-taqlīd al-jabrī al-ᶜarabī* [L'épître sur le mesurage d'Ibn ᶜAbdūn: un témoin des pratiques antérieures à la tradition algébrique arabe], Suhayl, Journal for the History of the Exact and Natural Sciences in Islamic Civilisation **5** (2005), 7–68 (in arabic).

[4] A. Djebbar and M. Moyon, *Les sciences arabes en Afrique. Mathématiques et Astronomie IXe-XIXe siècles*, Brinon-sur-Sauldre: Grandvaux-VECMAS, 2011.

[5] J. Høyrup, *Algebra and naive geometry: An investigation of some basic aspects of Old Babylonian mathematical thought*, Altorientalische Forschungen **17** (1990), 27–69, 262–324.

[6] M. Moyon, *La géométrie pratique en Europe en relation avec la tradition arabe, l'exemple du mesurage et du découpage: Contribution à l'étude des mathématiques médiévales*, PhD in History of Mathematics, University of Lille, 2008.

[7] M. Moyon, *Practical Geometries in Islamic Countries: the example of the division of plane figures*. In E. Barbin, M. Kronfellner and C. Tzanakis (eds), *History and Epistemology in*

Mathematics Education. Proceedings of the Sixth European Summer University, Vienna: Verlag Holzhausen GmbH, 2011, 527–538.

[8] M. Moyon, *Algèbre & Pratica geometriæ en Occident médiéval latin : Abū Bakr, Fibonacci et Jean de Murs.* In S. Rommevaux, M. Spiesser and M.-R. Massa-Estève (eds), *Pluralité de l'algèbre à la Renaissance*, Paris: Champion, 2012, 33–65.

[9] Ibn Ṭāhir al-Baghdādī, *at-Takmila fī l-ḥisāb ma^{c}a risāla lahū fī l-misāḥa* [The completion of Arithmetic with a tract on Mensuration]. Introduction and critical edition by A.S. Saïdan, Koweit : Manshūrāt ma^{c}had al-makhṭūṭāt al-^{c}arabīya, 1985.

Tacit versus explicit knowledge in history of mathematics: the case of Girolamo Cardano

Veronica Gavagna

Girolamo Cardano, Niccolò Tartaglia, Ludovico Ferrari and Rafael Bombelli – the so-called Italian Algebraic School of the Renaissance – were the heirs of the abbacus tradition, which flourished in Italy mainly from the 14th up to the 16th century. It is difficult evaluating in detail the features of this heritage, first of all because the abbacus mathematics, transmitted essentially by manuscripts, is still largely unresearched (with some geographical exceptions). And so, some techniques, concepts and results which appear novelties at first sight, at a deeper analysis become aspects of a form of tacit knowledge, shared by practical mathematicians. The concept of number developed in the abbacus milieu, for example, deeply influenced the algebraist of the Renaissance. Cardano clearly explained this concept at the very beginning of his *Practica arithmetice* (1539) [1]: the only true number is the natural one, but positive fractions and radicals are to be considered numbers "by analogy", because defining elementary operations in each set of 'numbers' is allowed. When Cardano found square roots of negative numbers (*radices sophisticae*) in the solution formula of the third degree equation (the so-called "irreducible case", *Ars magna* 1545), he was not worried about foundational questions, but he asked himself if they behaved "by analogy" like numbers. The first step to be carried out was to establish whether they were positive or negative quantities. Although he realized that the quantities could not be considered negative or positive, but were "a third sort of thing", Cardano tried to give them a sign, even attempting to formulate a new rule of signs appropriate to his own needs. After noting the failure of this approach, Cardano tried to find a solution formula that did not contain roots of negative numbers, but his efforts were not rewarded. In his *Algebra* (1572), Bombelli, who shared the same concept of number as Cardano, reconsidered the problem of the sign of expressions having the form $b\sqrt{-1}$ and introduced the new signs – rather than imaginary numbers – "more than minus" (*più di meno*) and "less than minus" (*meno di meno*), for which he established appropriate rules of multiplication. On this basis, Bombelli founded an arithmetic of Cardano's sophistical quantities allowing him to make sense of the irreducible case of cubic equations and, in the special cases where it was easy to extract the *linked cubic roots* $\sqrt[3]{a \pm b\sqrt{-1}}$, also allowing him to solve such equations, obtaining the real roots. Furthermore the engineer Bombelli, differently from Cardano, deeply influenced by "Euclidean education", did not hesitate to provide a geometrical proof of the existence of real roots in the irreducible case, because he accepted the use of sliding squares instead of ruler and compass or, in other words, he accepted the idea of determining a point in an approximate way. In his *Ars magna*, Cardano showed the Euclidean representation of the solution formula by decomposition of a cube into other cubes and parallelepipeds, but this decomposition was possible only when the third degree equation had a non-negative discriminant. In his *De regula aliza* (1570), Cardano showed that the solution of a irreducible equation could be represented as an intersection of a parabola and a hyperbola, but he bitterly concluded that, although simple from the geometrical point of view, the construction was difficult to translate into arithmetical terms. Moreover, he added, without any real justification, that he did not find the construction fully satisfactory, probably – I suppose – because of the impossibility of using only ruler and compass. Cardano seemed to refuse abbacus heritage with respect to geometrical approach. When he and his pupil Ferrari, in the context of the famous challenge Tartaglia vs Ferrari, proved all the *Elements* using a straightedge and a fixed opening compass instead of a variable opening compass (and slightly changing the Third Postulate), he decided to publish this (relevant) result in the philosophical work *De utilitate*, thinking it was interesting from the purely mathematical point of view, but not really useful, even if a fixed opening compass was an instrument commonly used by craftsmen. While Cardano remained firmly connected to the Euclidean spirit, mathematicians like Bombelli and Tartaglia, got instruments and techniques by practical geometry. Tartaglia, for example, devoted the Fifth Part of his *General Trattato* (1560) to "geometers, draftsmen, perspectives, architects, engineers and mathematicians" and the aim of this treatise is just using ruler and fixed opening compass to prove Euclidean propositions. Moreover, Tartaglia, who translated the *Elements* into vernacular Italian (1543), was often guided in his translation by tacit knowledge based on practical experience: a comparative study of the *General Trattato* and the *Elements* is necessary to definitively describe this influence. On the other side, this case study shows that the relationship between the Renaissance Italian algebraists and their mathematical milieu, both tacit and explicit, is an issue to explore in order to deeply understand some of the main development of mathematics in 17th century.

References

[1] V. Gavagna, *Medieval Heritage and New Perspectives in Cardano's* Practica arithmetice, Bollettino di Storia delle Scienze Matematiche, **1** 2010, 61–80.

[2] V. Gavagna, Radices sophisticae, racines imaginaires: *the Origin of Complex Numbers in the Late Renaissance*, in A. Angelini, R. Lupacchini, *The Art of Science. Perspectival Symmetries Between the Renaissance and Quantum Physics* (forthcoming)

The arithmetization of proportionality as tacit knowledge in early modern mathematics

Antoni Malet

To be precise in which sense the term 'arithmetization' is used here, we bring in Euler's understanding of ratios in his *Elements of algebra* (*Vollständige Anleitung zur Algebra*, 1770), the first chapter of which introduces quantities, all expressed in numbers, as determined by the ratio between the given magnitude and the chosen unit. Therefore, the notion of ratio is first taken for granted and used without explanation. In subsequent chapters, 'geometrical ratio' is defined as what answers to the question, How many times is A greater than B? The quotient of A divided by B expresses the ratio (A:B). Two ratios are equal when their quotients are equal.

Euclid's definitions of ratio and proportionality met with widespread criticism in early modern mathematics. They were already misunderstood and subject to major mistranslations within the medieval Euclid. It is nonetheless to be stressed that medieval mistaken translations were corrected by Tartaglia, and right translations of Euclid's definitions appear in all the 16th-century 'canonical' editions of the *Elements*, including Commandino, Clavius and the English translation of Billingsley and Dee. My contribution aims to map the changing status of Euclid's definitions and the ways in a tacit arithmetization of ratios contributed to transform this notion. My talk presents evidence of the criticism some influential authors, such as Galileo and Wallis, and authors of influential 17th-century textbooks, such as A. Tacquet and Milliet Dechales, I. Pardies, A. Arnauld, B. Lamy, and C.-R. Reyneau, addressed against Euclid's definitions. Then, it analyzes their alternative definitions taking its cue from the critique Isaac Barrow levelled against some of them in 1666 (published in 1683). Finally, it discusses in terms of collective tacit knowledge the success of their anti-Euclidean views over against Barrow's mathematically more cogent ones.

To quote an example, Tacquet (on the authority of his teacher, Gregory of Saint Vincent) claims that Euclid's definition of equality of ratios does not explain the 'nature' (sic) of equal ratios, but only provides a property the two ratios have in common. He suggests that he and his readers already know what makes two ratios equal – this is why it must be proved that the equimultiple property is found in equal ratios (in Tacquet's tacit sense) and only in them – something which, says Tacquet, neither Euclid nor any of his followers did. Alternatively, adds Tacquet, we can take Euclid's definition as a new way of construing equality between ratios (different to the tacit understanding). From now on 'two ratios are equal' only means that the equimultiples of their antecedents and consequents behave in the manner required by Euclid. Tacquet defines ratio as the way in which antecedents contain or are contained by consequents, and the equality of ratios consists in that their antecedents equally contain or are contained by their consequents. This line of thought is shown to be present in Dechales, Arnauld, Lamy and Reyneau. It is taken to an extreme by Wallis, who claims in 1657 that in any ratio (A:B), the quotient A/B determines the number of times the antecedent A contain the consequent B. He defined the equality of ratios by means of the equality of their quotients.

Barrow never took the assumption that all magnitudes are measured or numerically expressed seriously. He stressed that magnitudes can only be compared one to another geometrically, for what they are. If the measure of magnitudes is defined in terms of the ratio between any given magnitude and the chosen unit; therefore it requires the notion of equal ratios. If measured magnitudes are used to define the equality of ratios, the argument is circular. Barrow points out that for general magnitudes the quotient A/B is only defined as the magnitude C such that (A:B) = (C:1). Therefore, we are in full circle, since the definition of quotient requires the definition of equality of ratios.

Barrow dismissed the idea that general ratios could be characterized or identified with numerical quantities of some kind, because of the impossibility of determining which quantities correspond to irrational ratios. Barrow was willing to use the notion of ratio to define mathematical objects such as roots in general (and others), but he claimed Euclid's Elements did not allow the use of proportional means, or roots, to define ratios, because it entails circular thinking also. Barrow's position has a peculiar anti-philosophical slant: The disagreements among mathematicians about the definition of ratio is a consequence of putting too much emphasis on the definition of words. Criticizing Euclid's definition by the likes of Tacquet and Dechales, Barrow assimilates to the philosopher's infatuation with the perfect definition of general, abstract notions. He claims that ratios have no essential nature other than what can be deduced from Euclid's mathematical definition of equality. Facing a choice between sound mathematics and tacit knowledge "embodied in the alleged intelligibility of 'customary names' (sic)" Barrow prefers Euclid's difficult but mathematically sound definition over definitions apparently more intelligible but mathematically untenable.

It seems weird that Barrow's criticism was ignored. It was not answered at the time, and subsequent mathematicians (including Roberval, Simpson, Heath) upheld Barrow's views, as we do now. In what sense may it be said that Barrow's (that is, Euclid's) understanding of ratio and equality of ratios was overwhelmed by the rise of tacit knowledge about the arithmetical nature of continuous magnitudes and ratios? Notice that Barrow warns his readers that it is logically impossible to express quantities numerically, if the notions of ratio and equality of ratios are not available. He also makes the point that it is logically impossible to express ratios by their quotients, if the numerical expression of the terms of ratios is not available. Yet Barrow was an isolated figure and his views were neglected. The knowledge that made Barrow's views marginal can be assimilated to collective tacit knowledge (in Harry Collins's terms) in two senses. First, it was not transmitted by 'strings' of mathematical deductions and formal proofs. Different authors used different idioms and different formulations for the new notions of ratio and equality of ratios. Secondly, the strength of the new notions is social. It comes from (almost) everybody taking them up. Faced with the limitations of Euclid's notions and the impossibility of cogently applying Euclid's propositions to new, not yet fully mathematically formulated notions, (almost) every mathematical author sidestepped Euclid and took up the new notions. Euclid's and Barrow's were no longer pertinent not because they were wrong, but because they were no longer in tune with social practice.

References

[1] Arnauld, A. *Nouveaux Elemens de Geometrie*. Paris, 1667.
[2] Barrow, I. *Lectiones Mathematicae XXIII*. London, 1683.
[3] Dechales, Claude-Francois Milliet, 1621-1678. *Euclidis Elementorum libri octo. : Ad faciliorem captum accommodati*. Lyon, 1660.
[4] Euler, L. *Vollständige Anleitung zur Algebra*, 2 vol. St. Petersburg, 1770.
[5] Lamy, Bernard, 1640-1715. *Les elemens de geometrie, ou De la mesure du corps: qui comprennent les elemens d'Euclide & l'Analise*. Paris, 1685.
[6] Reyneau, C.-R. La Science du calcul des Grandeurs en general, ou les Elemens des Mathematiques, 2 vol. Paris, 1714–1736.
[7] Tacquet, André. *Elementa geometriae planae ac solidae* (libri 1-6, 11-12). Anvers, 1654.
[8] Wallis, J. *Mathesis universalis*. Oxford, 1657.

Our Knowledge of Standard Models: A Case of Tacit Knowledge?

Felix Mühlhölzer

Many formalized theories, typically first-order theories, have nonstandard models, but precisely how do we distinguish between standard and nonstandard models, and especially how do we single out the standard ones which, as we are often prone to say, we actually "have in mind" and actually intend to "refer to"? And if one says, for example in the case of arithmetic, that one knows what one means by "natural number", and if one understands this as knowledge about the standard model, can it be explicit knowledge or might it be inevitably tacit? This sort of problem has bothered many people (Skolem, Bernays, Dummett, Putnam, Manin...) but I think that it is deeply muddled because in its usual formulations the distinction between "reference" and "interpretation" is blurred. The quite common talk about an "intuition" that one has about the standard models, an intuition that goes beyond what model theory can afford, is confused because it belongs to the domain of used signs within which the notion of "reference" has its place, and it does not involve the notions of "interpretation" and "model" as they occur in model theory, which concern only signs that are considered as purely mathematical entities and are not used. Our alleged "intuition" of standard models belongs to the realm of used signs, and it concerns reference and not interpretation. That is, it has nothing specific to do with models in the model theoretic sense.[1]

[1]This contrast between used and non-used signs is elaborated in my papers "Wittgenstein and Metamathematics", forthcoming in: *Wittgenstein: Zu Philosophie und Mathematik*, ed. Pirmin Stekeler-Weithofer, Verlag Felix Meiner; and "On Live and Dead Signs in Mathematics", forthcoming in: *Formalism and Beyond. On the Nature of Mathematical Discourse*, eds. Michael Detlefsen and Godehard Link, ontos Verlag.

The impression that our knowledge of standard models might be a case of tacit knowledge, and maybe even a particularly deep one, is beset by the confusion just explained. The knowledge of standard models and of the difference between them and the nonstandard ones is a purely mathematical one and does not raise any specific problems that go beyond problems concerning mathematical knowledge in general. And our alleged intuition of standard models has nothing specific to do with models in the model theoretic sense. To think otherwise is simply a delusion.

What is the connection between the realm of the used signs of our mathematical practice and the realm of the not used signs that model theory is about? It consists in the transformation of the used signs into the petrified formulae considered in model theory. Precisely the specific way in which this petrification occurs in model theory gives rise to the nonstandard models. This fact is not altered by Tennenbaum's Theorem which, in the case of Peano arithmetic, singles out the standard models as precisely those in which addition is recursive. This singling out is not based on the connection between practice and model theory but on maneuvers belonging to model theory that are disconnected from the practice. So Tennenbaum's Theorem cannot help to 'solve' our problem of singling out the standard models.[2] This remains a pseudo-problem which has not to be solved but to be dissolved.

The idea that the knowledge expressed by the exclamation, "But I know what I mean by 'natural number'!" might be able to single out the standard models of arithmetic is confused, but there remains the exclamation itself and the specific sort of knowledge that is thereby meant. This knowledge can now be investigated without the further thought that it has anything to do with model theory. It seems to be knowledge that we have with respect to our mathematical competence, and might not this be inevitably tacit? I think that also this idea is confused, but the investigation of it must be postponed to another occasion.

Hilbert's Formalism: Intuition and Experience

Volker Peckhaus

David Hilbert first presented his formalism in his *Grundlagen der Geometrie* of 1899 ([1]), where he proposed an axiomatic system for Euclidean Geometry. He regarded mathematics as a mental construction accessible with the help of a sign system designating the elements of this construction. Basic objects are "thought things" (*entia rationis*), according to I. Kant, empty concepts with no reference to reality. The relations between these thought things are defined implicitly by the axioms of geometry. These axioms differ from the traditional ones. Traditional axioms are propositions which cannot be proved, but for which proofs are not necessary because of their evidence. In Hilbert's approach, there is, however, no appeal to evidence or any extra-mathematical reality. His axioms are justified by meta-axiomatic investigations of independence, completeness, and consistency.

[2] That such a solution might be possible is argued for in: Halbach, Volker/Horsten, Leon: "Computational Structuralism", *Philosophia Mathematica* 13 (2005) 174-186.

For Hilbert, the consistency of a concept within an axiomatic system guarantees its existence. Existence of an object is therefore simply given by its possibility. Consistency proofs for axiomatic systems became the main task of research in foundations. They were usually given by presenting an arithmetical model (relative consistency proofs). Consistency proofs for arithmetic (via logic) and for logic itself (via a direct consistency proof) produced problems. In the 1920s this led to a concentration on the notion of proof (Proof Theory). In his later work, Hilbert distinguished between formal mathematics, i.e., mathematics propper, and contentual mathematics (*inhaltliche Mathematik*), i.e., proof theory. The main problem dealt with in Proof Theory was, how to deal with infinity using only finite proof methods. For this purpose he invented ideal elements, i.e., concepts accepted in formal mathematics for which a finite proof had not yet been found. Ideal elements are taken as if they were proved with finite means (cf., e. g., [4], [6]). Hilbert's formal mathematics is therefore conservative in the sense that it provided a reconstruction of given mathematics.

In this approach, mathematics is regarded as a man-made rational science, therefore it presupposes rationality. This is expressed by Hilbert with an extra-mathematical "axiom of reasoning", or "axiom of the existence of an intelligence", called "the 'a priori' of the philosopher": "I have the ability to imagine things and signify them by simple signs a, b, $\dots$, X, Y, $\dots$ in such a completely characteristic way that I can always recognize them unambiguously; my reasoning operates with these things in this designation in a certain way according to certain laws, and I am able to recognize these laws by self-observation and describe them completely" ([2], p. 219).

Given this characterization of Hilbert's formalism, there seems to be no room for tacit knowledge. Even the transcendental, extra-mathematical precondition, the demand for rationality, is formulated explicitly. In formal mathematics the mathematician operates with signs on the paper designating mathematical concepts according to rules justified in Proof Theory. Everything is perfectly explicit.

But Hilbert also distinguished between working with an axiomatic system and establishing or finding an axiomatic system. This distinction is connected to what he called the two tasks of mathematics: the *progressive task* of creating, developing systems of relations, and investigating their logical consequences, and the *regressive task* of giving theories found by experience a firmer structure and a foundation which is as simple as possible. Within the regressive task, the preconditions are investigated, distinguishing everywhere between assumptions and logical inferences ([3], p. 18). The last is topic in Hilbert's lecture courses where he reflects on mathematical practice. Setting up an axiomatic system (axiomatic method) is the place of tacit knowledge in Hilbert's view of mathematics. It is the creative part of the axiomatic enterprize. Axiomatization serves to make explicit implicitly given assumptions and convictions. In this task, criteria are accepted which are excluded from formal mathematics. The selection of the axioms is based, e.g., on interest, and the intuition of the experienced mathematician, who sometimes works unconsciously, evaluating some candidates for axioms on the base of a "certain triviality" adhering to them.

The question is, whether Michael Polanyi's theory of tacit knowledge can deal with Hilbert's axiomatic method as a method to make the implicit explicit. Polanyi considers tacit knowledge "by starting from the fact that we can know more than we can tell"([8], p. 4), but he does not even give a definition of knowledge. So he confuses (scientific) knowledge, belief, faith, assumptions, opinions, even skills. Everything is knowledge, albeit most of it tacit/personal knowledge. His alternative to the standard scientific paradigm of objectivity is the "acknowledgement of a beauty that exhilarates and a profundity that entrances us" ([7], p. 15) and "recognizing rationality in nature" ([7], p. 13). He does not consider a concept of objectivity regarded as intersubjectivity which, on the one hand, would avoid the problems connected to exaggerated notions of objectivity, but which is, on the other hand, the minimal condition or a scientific discourse avoiding the solipsism of personal beliefs. This notion would furthermore keep mathematics off metaphysical spheres. Polanyi claims: "A mathematical theory can be constructed only by relying on *prior* tacit knowing and can function as a theory only within an act of tacit knowing, which consists in our attending *from* it to the previously established experience on which it bears. Thus the ideal of a comprehensive mathematical theory of experience which would eliminate all tacit knowing is proved to be self-contradictory and logically unsound" ([8], p. 21). Polanyi correctly hints at the mutual relationship between tacit knowledge and a theory formulated according to criteria of rigidity and objectivity. But he makes by no means clear why it should be self-contradictory and logically unsound to evaluate a theory as such, not with respect to the context of its creation.

Polanyi throughout conflates the psychology of scientific practice with philosophy of science. He obviously ignores the distinction between genesis and validity due to Rudolf Hermann Lotze (1817–1881). Lotze's distinction comes close to similar differentiations between *quid iuris* and *quid facti* questions (Kant, Popper) or between the context of discovery and the context of justification (Reichenbach).

In concluding: Polanyi's theory of tacit knowledge is question begging: There is no claim of full objectivity as stated in the advent of his theory. This is a result of modern epistemology, in particular of the critical approach to human reason as proposed by Kant. So there is no need for a "Post-Critical Philosophy" (subtitle of Polanyi's *Personal Knowledge*). Polanyi gives up or ignores useful distinctions like the ones between art and science, genesis and validity, or the notion of objectivity as intersubjectivity. Having accepted these distinctions would have prevented him from some of his far-reaching conclusions. Formalist Hilbert was already beyond this point. With his distinction between progressive and regressive tasks in mathematics, he had the tools at hand to avoid Polanyi's naivete. In progressive formal mathematics the standards of rigidity and uncompromising objectivity (in the sense of intersubjectivity) hold. The regressive side is the domain of mathematical creativity, above all governed by tacit knowledge.

Polanyi writes: "I pointed out how everywhere the mind follows its own self-set standards, and I gave my tacit or explicit endorsement to this manner of establishing the truth. Such an endorsement is an action of the same kind as that which it accredits and is to be classed therefore as *a consciously a-critical statement*. This invitation to dogmatism may appear shocking: yet it is but the corollary to the greatly increased critical powers of man" ([7], p. 268). This invitation to dogmatism is an invitation to go back to dark medieval times. Who is this mind that sets its own standards? If some sort of abstract agent is meant, we are thrown back to a naive metaphysical realism, unworthy of today's state of reflection on science. If this mind has a bearer, it is by no mean clear, that this bearer is unable to learn and change his convictions due to better insights. But then no dogmatism is needed. With these overcasted speculations Polanyi does his theory of tacit knowledge no good service.

References

[1] D. Hilbert, "Grundlagen der Geometrie," in *Festschrift zur Feier der Enthüllung des Gauss-Weber-Denkmals in Göttingen*, ed. by the Fest-Comittee, Teubner: Leipzig 1899, 14th ed. [5].

[2] D. Hilbert, *Logische Principien des mathematischen Denkens*, E. Hellinger's lecture notes of summer semester 1905, library of the Mathematics Institute, University of Göttingen.

[3] D. Hilbert, *Natur und mathematisches Erkennen. Vorlesungen, gehalten 1919–1920 in Göttingen. Nach einer Ausarbeitung von Paul Bernays*, ed. David E. Rowe, Birkhäuser Verlag: Basel, Boston and Berlin 1992.

[4] D. Hilbert, "Neubegründung der Mathematik. Erste Mitteilung," *Abhandlungen aus dem Mathematischen Seminar der Hamburgischen Universität* 1 (1922), pp. 157–177.

[5] D. Hilbert, *Grundlagen der Geometrie. Mit Supplementen von Paul Bernays*, ed. by Michael Toepell, Teubner: Stuttgart and Leipzig 1999. 14th ed. of [1].

[6] D. Hilbert and P. Bernays, *Grundlagen der Mathematik*, Vol. I, Springer-Verlag: Berlin and Heidelberg 1939, ^{2}1968.

[7] M. Polanyi, *Personal Knowledge. Towards a Post-Critical Philosophy*, Routledge & Kegan Paul: London, and The University of Chicago Press: Chicago 1958, corrected ed. 1962, paperback ed. 1978, reprinted 1992.

[8] M. Polanyi, *The Tacit Dimension*, The University of Chicago Press: Chicago 1966, with a new Foreword by Amartya Senn, 2009.

On making mathematical inferences explicit: Pasch's reflections on logic

Dirk Schlimm

The geometer Moritz Pasch (1843–1930) is famous for his pioneering work in axiomatics: In his *Vorlesungen über Neuere Geometrie* (1882) [3] he gave the first axiomatic presentation of projective geometry, the first axiomatization of the betweenness relation, the first formulation of 'Pasch's axiom'. This work was highly influential for the development of the contemporary view of mathematics and it was a direct influence to Peano and Hilbert. In addition, Pasch also developed an empirical epistemology for geometry and analysis, and in the course of this work also he became more and more interested in the logical inferences that are licensed in mathematics [8]. This talk discusses Pasch's attempts to contribute to a 'renewal of logic' [5, p. 232] and discusses his motivations as well as the possible reasons for why this project led to a dead end.

From early on Pasch distinguished what was later called the 'context of discovery' from the 'context of justification', and he insisted that mathematical proofs are only justified if they are presented as logical deductions from explicitly stated assumptions. Moreover, Pasch's reflections were always guided by the goal of being relevant to mathematical practice and he often took his own axiomatic work as the starting point of his methodological considerations. Instead of identifying some basic building blocks for formal languages (e. g., connectives, quantifiers, predicate symbols) as Frege did, Pasch's starting point were inferences as they actually occur in mathematical texts. In his analysis, he identified three basic kinds of inferences: the premise and conclusion are synonymous, the conclusion presents only a part of the content of the premise, and the conclusion contains the content of more than one premises [4, p. 5]. According to Pasch, such basic inference steps could not be analyzed further, and so he concluded that our ability to *decide* in a finite amount of time whether we are presented with a genuine basic inference step or not is a fundamental prerequisite for understanding mathematical proofs [5]. This ability remains necessary also when the propositions of a proof are formalized, where Pasch understood formalization, as replacing 'material words' [*Stoffwörter*] by meaningless symbols. The inferences then depend only on the 'joins' [*Fügemittel*], which we would characterize as the logical components of propositions. An example that Pasch discusses in [6] is the inference from 'If two points are endpoints of a straight segment, then they are not endpoints of another straight segment' to 'If two points are endpoints of a straight segment, then there is no other straight segment of which the points are endpoints'. Given that Pasch identifies 'two', 'point', 'endpoint', 'straight', and 'segment' as the material words in these statements, the formalized versions of these two are 'If α βs are εs of a γ δ, then they are not εs of another γ δ' and 'If α βs are εs of a γ δ, then there is no other γ δ for which the βs are εs'. Thus, even in the case of a formal inference (in Pasch's understanding) the fundamental requirement of decidability remains: 'It must be presupposed that the reader is able to decide any such question [i e., whether a proposition follows from another] on the basis of one's understanding of the joins' [6, p. 253]. In the end, Pasch thought that a distinction between the *content* and the *guise* [*Einkleidung*] (this terminology was also used in [1]) of propositions might yield a promising approach and he hoped that 'the indicated path will lead to the main features of a logic that does justice to the accomplishments of mathematics' [5, p. 232]. However, how we could determine whether propositions have the same content was not addressed by Pasch and he did not develop these ideas further.

Approaches that begin with primitive building blocks to model actual phenomena are sometimes referred to as *synthetic*, while those that start with the phenomena are called *analytic* [7, p. 529]. Using this terminology, we can characterize Pasch's approach to logic as an analytic one, Frege's as synthetic. Both approaches have their well-known problems, namely to find a systematic basis and the fit with actual phenomena. Pasch focused on mathematical practice and contentful reasoning, which left hims struggling with what we now call the surface structure of language and ultimately led to unsurmountable problems, like those of identity of propositions and sameness of content. Because of this, and despite the fact that Pasch knew some of Frege's, Peano's, and Hilbert's work, he failed (or refused) to latch on to the development of modern logic. By looking sympathetically at such failed attempts we get a better understanding of the conceptual difficulties that were involved in arriving at the modern conception of logic.

References

[1] Gottlob Frege. Über den Zweck der Begriffsschrift. *Jenaer Zeitschrift für Naturwissenschaft*, 16, 1882. Reprinted in [2], pp. 97–105.

[2] Gottlob Frege. *Begriffsschrift und andere Aufsätze*. Georg Olms Verlag, Hildesheim, 1993.

[3] Moritz Pasch. *Vorlesungen über Neuere Geometrie*. B. G. Teubner, Leipzig, 1882.

[4] Moritz Pasch. *Grundlagen der Analysis*. B. G. Teubner, Leipzig, 1909.

[5] Moritz Pasch. Die Forderung der Entscheidbarkeit. *Jahresbericht der Deutschen Mathematiker Vereinigung*, 27:228–232, 1918.

[6] Moritz Pasch. Die axiomatische Methode in der neueren Mathematik. *Annalen der Philosophie*, 5:241–274, 1926.

[7] Dirk Schlimm. Learning from the existence of models. On psychic machines, tortoises, and computer simulations. *Synthese*, 169(3):521–538, 2009.

[8] Dirk Schlimm. Pasch's philosophy of mathematics. *Review of Symbolic Logic*, 3(1):93–118, 2010.

Tacit vs Explicit Images of Mathematical Logic: the reflexions of the School of Peano

Erika Luciano

Since the end of the 19th century, the Peano School presents itself and is presented as a group of scholars whose aim was to make their own scientific works as explicit as possible. In fact, according to Peano, all the hypotheses and deductive steps, even the most banal, must always be made explicit both in publications and in teaching practices. Moreover, the ideography was constructed in such a way that the meaning attached to the symbols was completely and unambiguously clarified. At the level of history as well, each definition and proposition was to be accompanied by an explicit account of its origin and development.

In actual fact, however, this historiographical depiction is less precise than might be supposed. In fact, in spite of the intentions declared, there are frequent cases of 'missing'- that is tacitly used - propositions in the works of the Peano School. Besides, the contrast between tacit and explicit regards the very adoption of the symbols, because some of Peano's collaborators confine the ideography to the private sphere of their research, masking its use in publications. As regards the historical aspects as well, that which is explicit in the Peanian written production is comprised of only a set of notes which includes bibliographical references and transcriptions of extracts from sources. What is implicit concerns all the rest, that is 1) the majority of primary and secondary literature used and 2) if there exists a non-naive historiographical conception underlying the compilation of these notes. Further, as regards this kind of extra-mathematical elements, it is necessary to bear in mind that a stereotyping of roles within the Peano School becomes evident fairly early on. According to it, G. Vailati was 'the philosopher', G. Vacca 'the historian', M. Pieri 'the geometer', and so on. This makes it even more difficult to distinguish the tacit from the explicit dimension, because some of Peano's collaborators were prone to remain silent about certain components of their own research, leaving to their colleagues, who were considered the 'specialists' in one area or another, the task of explaining them.

In light of this overall picture, first of all we determine how, in the particular case-study of the Peanian logical-foundational studies, the terms of the tension tacit *vs* explicit are to be specified, taking into consideration the fact that, with this expression, we allude to a very composite body of knowledge, constructed by a community of scholars.

As far as the side of explicit is concerned, the problem consists in choosing a printed work that is, or can be considered as, representative of the activities carried out by the School of Peano. Among the possible alternatives [1] we have chosen the three last editions of the *Formulaire de Mathématiques* (1901, 1902-03, 1908) to represent the explicit side of the dichotomy because

- this is the only work that, at least along a general line, can be considered representative of the School of Peano *as a whole*; [2]
- this is the work for which we possess the majority of the unpublished sources, which makes it possible to identify some of the tacit aspects of the mathematics produced in the Peano School;
- both inside and outside the School, this treatise was indicated as the *summa* of the research of Peano and his collaborators.

Instead, we have chosen to entrust the opposite side of the tension, that is the tacit aspect, to the oral dimension that surrounds the editing of the *Formulaire*. In fact, the close relationships that mature between the members of a working group are based upon everyday contact which is essentially oral in nature and lead

[1] They include, for example, the journals *Rivista di Matematica* (1891-1906), *Schola et Vita* (1925-1939) and some works by individual members of the Peano's School, such as the inaugural lecture given by Pieri in Catania, *Uno sguardo al nuovo indirizzo logico-matematico delle scienze deduttive* (Annuario R. Univ. Catania, 1906-07, p. 21-82), or the article by A. Padoa, *Logica matematica e matematica elementare* (Atti II Congresso Associazione Mathesis, Livorno, 1902, p. 186-200), called 'the manifesto of the Italian logicians'.

[2] In effect, according to U. Cassina, the Peano School comprised some forty researchers, but only a part of those took part in editing the *Formulaire*. These are the ones that we indicate as the 'first generation' of Peano's students, including G. Vailati, G. Vacca, C. Burali-Forti, A. Padoa, M. Pieri, the second generation being constituted, with few exceptions, by the Peano's interlinguistic collaborators and by the group of his female students who worked with him during the last years of his life. Further, some students of this first generation provided just very marginal additions to the *Formulaire*; others stopped the collaboration after the first edition, other ones after the third or the fourth.

inevitably to the creation and the socialization of a massive amount of tacit mathematical knowledge. Further, these oral testimonies, and in particular the conversations which many of Peano students remember vividly, can be reconstructed today thanks to the correspondences among Peano, Vacca, Vailati, Pieri, Padoa and Burali-Forti, the manuscripts conserved in the *Peano-Vacca Archive* in Turin and the volumes of the *Formulaire* with Peano and Vacca's autograph notes.
Once our dichotomy is fixed in terms of orality *vs* publication in the last three editions of the *Formulaire*, the mathematics produced in the Peano School becomes a collection of tacit elements. Thus we discover that remaining altogether implicit are the proofs of the Cantor-Bernstein theorem devised by Vacca with the help of Vailati and Burali-Forti, and the systems of postulates of arithmetic proposed by Vacca and Pieri. Examples of this same tenor could be listed by the dozen.
It is also possible to discern a criterion, obviously tacit, used by Peano to determine, from among the proposals submitted to him, what to promote to the level of publication in the *Formulaire.* The tacit dimension is in fact more evident with regard to meta-theoretical problems such as the criteria for choosing the primitive concepts and propositions; the consistence and independence of the axioms; the ways of schematizing language between the opposing poles of natural language and ideographical symbolism; the sensitivity with regard to adherence to physical or psychological reality of mathematical concepts, as opposed to their abstract and formalist connotations, etc. The divergences regarding these questions - or at least the differences in opinion among Peano's collaborators - are remarkable. In the absence of an agreement, Peano seem usually choose to gloss over such reflections in the *Formulaire*, to the point of rendering the positions of individuals indistinguishable, or even leaving them entirely implicit, omitting reference to the literature that lay behind them, which also differed notably from student to student.[3]
In light of this context, we illustrate in the second part of the talk:

- an example of a result by Padoa (his method for proving the independence of the arithmetical postulates), which was promoted in its essence to the explicit degree, after having circulated for a very brief time in tacit form;
- an example of a critical comment, by Vailati, about the chapter *Logique* of the *Formulaire*, which remained tacit in its entirety forever, in spite of attempts to render it explicit in the two last editions of the treatise;
- an example of a contribution by Pieri and Padoa (an hypothetical-deductive system for the Euclidean geometry based on two primitive concepts), which remained tacit almost completely and almost forever, even though it had

[3] There was a circulation of volumes and articles that were shared by Peano's entourage. Not by chance, the same publications by L. Couturat, E. Huntington, B. Russell, among the few to be cited in the *Formulaire*, are present in the personal libraries of Peano, Vacca and Vailati. Flanking this, however, was a set of readings (G. Frege, D. Hilbert, F. Brentano, A. Naville, . . .) recommended by the individual members of the School, appreciated by *some* but not *all* of the colleagues. In a few cases this second type of references was noted by Peano in his *marginalia* and even more rarely it reached the level of explicit quotations in the *Formulaire*.

arrived 'a step away' from being made explicit in the fourth edition of the *Formulaire*.

Taking into account these examples, the question arises whether Burali-Forti, Padoa, Vacca and Vailati's allusions to Peano's 'bad tendencies' towards his collaborators are true or not. According to our analysis, it appears plausible to maintain that Peano was led to render and maintain explicit in the *Formulaire* all and only those results (his own as well as others) that entered into the spirit of the treatise as *he* conceived it and that *he* held to be best from the scientific or didactic point of view, inviting the individual contributors themselves to make explicit, through articles, the contributions that had not found a place in the treatise. Faced with proposals for substantial modifications, Peano could become quite cutting. The fact that the dissemination of the views with respect to meta-mathematical problems is prevalently entrusted to the tacit dimension is further ascribable to the cultural context, which made it politically opportune to emphasize some implications while downplaying others, and which drove the School of Peano to make explicit only what was widely shared, in the attempt to consolidate the image of a cohesive group of researchers.

References

[1] E. Luciano, C.S. Roero, *La Scuola di Giuseppe Peano*, in C.S. Roero (ed.), *Peano e la sua Scuola fra matematica, logica e interlingua. Atti del Congresso internazionale di Studi (Torino 2008)*, Torino, Dep. Sub. Storia Patria, 2010, p. 1-212.

Tacit Knowledge in Mathematics: Definition, Types, Examples

Herbert Breger

Tacit knowledge in mathematics is defined to be knowledge that is essential for the understanding of a mathematical theory although it cannot be deduced from the axioms resp. the presuppositions which were generally accepted at that time[1][2][3]. Three arguments were given in order to show that there is tacit knowledge in this sense. Firstly, a computer to which the axioms of a particular theory are given cannot produce a textbook of that theory. Secondly, experts use undefined words like beautiful, natural, deep, profound, exotic, elegant in order to express statements about the importance of a notion or a proof or on the structure of a theory. This is knowledge on the meta level. Thirdly, it is well-known in mathematical education that understanding a proof line by line does not necessarily imply to understand the proof as a whole.

In the second part of the talk a number of types of situations in which tacit knowledge occurs were discussed. There is a tacit knowledge of axiomatisation (illustrated with the examples of Eilenberg and Steenrod's axiomatisation of algebraic topology and Klein and Lie's considerations of groups as well as Eilenberg and Mac Lane's introduction of natural transformations). Historians are well aware of the problems of hindsight which originate from seeing a theory in the past with "modern eyes"; the theories of Barrow, Leibniz and Newton on tangents and areas were compared for that purpose. Finally notation, definitions, know how for problem solving, procedures becoming objects and the decisions about the trivial were discussed.

References

[1] H. Breger, *Know-how in der Mathematik*, in: Rechnen mit dem Unendlichen, ed.: D. Spalt, Basel, Boston, Berlin 1990, pp. 43–57.

[2] H. Breger, *Tacit Knowledge in Mathematical Theory*, in: The Space of Mathematics, eds.: J. Echeverria, A. Ibarra and T. Mormann, Berlin, New York 1992, pp. 79–90.

[3] H. Breger, *Tacit Knowledge and mathematical progress*, in: The Growth of Mathematical Knowledge, eds.: E. Grosholz and H. Breger, Dordrecht, Boston, London 2000, pp. 221–230

The use of the word "implicit" in the works of Carnot and Poncelet

Philippe Nabonnand

In this paper, we discuss a tentative by Lazare Carnot and Jean-Victor Poncelet to introduce the categories of explicit and implicit in mathematics at the beginning of 19th century. The background is an epistemology of generality (valorization of general problem, of general proof, of general results, of general theorems... [1]). During the first half of 19th century, Carnot, Poncelet and many others agreed to give pure geometry the status of unsurpassable rigor. They also agreed to consider that this way to tackle problems of geometry lacked generality. Comparison with analytic geometry was often cruel and justified the reproach against pure geometry in linking proofs too exclusively with the consideration of a particular figure. Initially, they concluded that the power of analytic geometry was linked with the use of formulas without worrying about the sign or even the existence of the element. Carnot and Poncelet proposed, in their own way, to generalize the framework of the pure geometry by providing proof methods that allow overcoming the precept that a geometrical proof is relative to a particular figure.

Carnot and Poncelet had two different objectives. The former supported a program of reform of mathematics. For Carnot, there were only absolute quantities and the most important step was to rid mathematics of nonsense that are signed (negative or positive) and imaginary quantities. He criticized both methods of analysis and geometry and proposed procedures which didn't use positive, negative or imaginary quantities. All operations must be effective. The latter had a program of improvement of pure geometry. His aim was to propose procedures in pure geometry as powerful as those used in analysis and analytic geometry. With the intention to justify the procedures he proposed, his strategy was to argue that they have a common ground with those of analysis and analytic geometry. In Poncelet's opinion, this ground was not explicit but tacitly used in analysis[2].

[1] See [Chemla 1998], [Nabonnand 2001].

[2] There are many occurrences of the word "tacit" in articles where Poncelet justifies his point of view.

I will follow how Poncelet and Carnot, each with their different ways, used the argument of generality and introduced the notion of implicit. I will focus essentially on the development of this line of argument in two of Carnot's treatises, *De la corrélation des figures de géométrie* and *Géométrie de position* and in Poncelet's articles preparatory to the *Traité des propriétés projectives des figures.*

Carnot and the rejection of signed and imaginary quantities. In his two treatises, the objective of Carnot was to implement geometric methods in which the introduction of the sign "$-$" or consideration of elements that become imaginary reduce to operations that Carnot described as "executable". He didn't reject the use of signs like $+$ or $-$ or even complex signs, but he demanded that in the last instance, we can reduce to executable operations. The starting point of Carnot's argument was the observation that considerations of position, though attached to a figure, may be subject to change of signs when they are applied to other figures. These figures, obtained by changing continuously the relative positions of elements of the initial figure are called corrélative. The idea is that by expressing relevant properties of figures by formulas and by specifying how changes of positions affect these formulas, position relations may be subject to a general treatment. Carnot distinguishes explicit formulas, that are immediately applicable to the studied system, from implicit ones that cannot be applied to the studied system without having to undergo some changes of signs. The figure or the system to which properties are related to decide if the formulas are explicit or implicit is called primitive system, the other figures or systems which can be obtained from a primitive system, "c'est-à-dire tous ceux qu'on peut considérer comme les différens états d'un même système variable qui se transforme par degrés insensibles," are called correlative. These notions are relative.

Carnot's problem was to systematize the link between correlations of figures and the passage of implicit formulas to explicit formulas. Carnot's argument came in two stages: first, justifying a correct use of negative and imaginary quantities in algebra and geometry without giving them any status and thus avoiding the mistakes that we necessarily arrive when "we think with absurd quantities." Then, in giving his method of correlation systems in a general framework and arguing that the methods he proposed fall within this general method, Carnot highlighted, on one hand, the pointlessness of granting any ontological status to these quantities, and, on other hand, he ensured the soundness of his views in geometry. Carnot showed that his method of correlation systems already operated in related fields of geometry and that in fact it is a general epistemological process.

The method recommended by Carnot may be summarized as follows: he refers the problem to a particular figure and draws up a tableau that represents all relevant properties of this figure, then taking this figure as standard of comparison, he proposes to extend this description to correlative figures, "d'y rapporter par des tableaux additionnels [appelés tableaux de corrélation], toutes celles dont la construction est essentiellement la même."

Poncelet and the principle of continuity. Poncelet had devoted a great part of his work to establish general procedures in pure geometry; for Poncelet, generality was inherent in the object under study, justifying the project to look for general methods in pure geometry. At the same time, noticing that generality comes within the notion of figured magnitude (grandeur figurée) (which is opposite to that of absolute magnitude), Poncelet claimed that pure geometry must change its object of study. It is should no longer study absolute properties of particular figures; the geometers have to take an interest in the general properties of figured magnitude. Poncelet proposed two complementary ways: 1) focus on projective properties of figures and 2) admit a principle of preservation of properties of figures (or principle of continuity). As we are here interested by Poncelet's views about explicit/implicit, we will stress the principle of continuity. To analyze Poncelet's arguments, we will follow a paper, dated 1818, *Considérations philosophiques et techniques sur le principe de continuité dans les lois géométriques*, in which he attempts to defend the admission in pure geometry of a principle of continuity. If Poncelet took much of the terminology introduced by Carnot and the idea of studying the change of sign within the correlation of figures, he did not follow him in his denial of status to negative or imaginary quantities [Poncelet 1815]. Beyond the relevance of his critiques of Carnot's discussion, it is important to note that for Poncelet, persuading his readers of the merits of the use of negative and imaginary quantities in analysis and analytic geometry was a necessity. Indeed, from Poncelet's view, the law of signs in geometry is dependent on a principle of continuity whose principal justification was its tacit use in analysis. The comparison between the methods of analytic geometry and pure geometry lead him to conclude that the essential difference between them was that the former used a principle of continuity which is expressed as:

"Graphic properties found for the original figure remain without changes other than those moving parts, for all correlative figures that can be supposed to come from the first. [...] Metric properties found for the original figure remain applicable without changes other than those change of signs, for all correlative figures that can be supposed to come from the first." (Poncelet 1818, p. 318)

The adoption of this principle was the principal reason why "la Géométrie des modernes semble l'emporter de beaucoup sur celles des Anciens." Why was this principle first accepted without question in analytic geometry? According to Poncelet, the answer was to look at the formalism of the equations and "at the habit [...] to extend the meaning and application of a same formula or equation for all states of the system to which it relates regardless the relative positions of system components or even of their existence." In fact, the representation by purely literal equations allows abandoning the explicit reasoning, i.e. reasoning in which one never loses the object of view and which is in immediate relation to a particular figure. The admission of the principle of continuity allows the practice of implicit reasoning. At the end of reasoning, if the negative or imaginary factors disappear in the statement of the final result, the result is considered as real and applicable. Poncelet proposed to follow the same program in the exposition of pure geometry.

Its objective was to determine the consequences of adopting the principle of continuity in pure geometry, to identify new forms of demonstrations that follow, and thus to justify and a notion of implicit reasoning in pure geometry. The idea of Poncelet was that geometrical properties that apply to a particular configuration will (except for changes in signs that correspond to changes in position) continue to apply to correlative figures (that is, all real and absolute states of the same system that is transformed by imperceptible degrees.). Then, it is sufficient to prove the property by an explicit reasoning by considering a figure where all objects and relations are real to get its validity for all correlative figures. With the principle of continuity, it is possible to extend a property to all correlative figures, provided that if this principle gives a conclusion "about the permanence of relations, it does not decide on the nature and absolute existence of objects and variables that are concerned by these relations." When these objects disappear, these relations are far from becoming absurd or insignificant because their non-applicability expresses a certain position of the actual system. The principle of continuity establishes the validity of some relations beyond the reality or unreality of certain objects, the generality of the reasoning is so ensured because we are not directly involved with a particular figure, and because we can consider the set of formulas associated with a figure.

References

[1] Karine Chemla, Lazare Carnot et la généralité en géométrie, *Revue d'Histoire des Mathématiques*, 4 (1998), 162-190.
[2] Lazare Carnot, *De la Corrélation des figures*, Paris: Duprat (1801)
[3] Lazare Carnot, *Géométrie de position*, Paris: Duprat, (1803).
[4] Philippe Nabonnand, "Deux droites coplanaires sont sécantes," in Lise Bioesmat-Martagon (dir.), *Éléments d'une biographie de l'espace projectif*, Nancy: PUN, (2011).
[5] Philippe Nabonnand, L'argument de la généralité chez Carnot, Poncelet et Chasles, in D. Flament & P. Nabonnand (dir.), *Justifier en mathématiques*, Paris: Éditions de la MSH, (2011).
[6] Jean Victor Poncelet, Sur la loi des signes en géométrie, la loi et le principe de continuité (1815), in *Applications d'analyse et de géométrie qui ont servi de principal fondement au Traité des propriétés projectives des figures*, t. 2, Paris: Gauthier-Villars, (1864), 167-295.
[7] Jean Victor Poncelet, Considérations philosophiques et techniques sur le principe de continuité dans les lois géométriques (1818), in *Applications d'analyse et de géométrie qui ont servi de principal fondement au Traité des propriétés projectives des figures*, t. 2, Paris: Gauthier-Villars (1864), 296-362.
[8] Jean Victor Poncelet, *Traité des propriétés projectives des figures*, Paris: Bachelier (1822).

Linear Groups in Galois Fields: A Case Study of Tacit Circulation of Explicit Knowledge

Frédéric Brechenmacher

This paper aims at stressing some aspects of my works on the history of algebra in the 19th and 20th century, which are related to the the tacit vs the explicit in the ways some groups of texts hold together.[1] These aspects raise issues as to the historian's choices of a corpus of reference and of a scale of analysis. They also address the more general problem of articulating the individual and collective dimensions of mathematics. Indeed, although the category "algebra" points to some collective organizations of knowledge, this category took on changing identities in different times and spaces. Until the 1930s, "algebra" was especially not usually referring to an object-oriented discipline.[7] In France, for instance, algebra was, on the one hand, traditionally considered in the teaching of mathematics as an "elementary," or "intermediary," discipline encompassed by "the higher point of view" of analysis. On the other hand, algebra was also pointing to some procedures that made a "common link" between researches in the various branches of the mathematical sciences. What was explicitly identified as "algebraic" therefore often pointed to some implicit circulations between various theories. This situation makes it customary to study carefully the ways texts were referring one to another, thereby constituting some shared algebraic cultures.
I shall introduce this paper by making explicit how such issues came up in my research work, before focusing on a case study on "linear groups in Galois fields" at the turn of the 20th century. Although the latter designation may seem to make explicit some collective interests for a theory revolving around a specific object, i.e. $Gl_n(F_{p^n})$ (p a prime number), we shall see that this designation actually supported the implicit reference to a specific algebraic culture that had developed over the course of the 19th century.

1. Introduction : the "versus"

Throughout the whole of 1874, Jordan and Kronecker were quarrelling over the organization of the theory of bilinear forms.[2] The controversy started with a public quarrel of priority over two theorems before turning into a private correspondence. The epistolary communication was mostly devoted to making explicit some tacit relations between some texts of Weierstrass, Christoffel, and Kronecker. After Jordan had made himself familiar with this collective of texts, the quarrel eventually went public once again. These successive episodes highlight that even though the "theory of bilinear forms" may have been considered at first sight as the explicit reference to a collective of mathematical methods and notions, it was through some tacit intertextual relations that this theory was making sense. Moreover, the tacit dimension of the theory was recognized by the actors themselves:

[1] An extended version of this paper is available at http://hal.archives-ouvertes.fr/aut/Frederic+Brechenmacher. *Ce travail a bénéficié d'une aide de l'Agence Nationale de la Recherche : projet CaaFÉ (ANR-10-JCJC 0101)*

both Jordan and Kronecker agreed that it was no more than a "slight fault" to fail to grasp the relevant underlying intertextual relations, which had thus to be made explicit through some direct communication.

2. The tacit vs the explicit in building some networks of texts

The 1874 controversy highlights the problem of the selection of the corpuses in which a given text is making sense, i.e., the identification of some networks of texts. But such networks cannot be identified as webs of quotations.[8] Not only do practices of quotations vary in times and spaces but intertextual relations may also be implicit. My approach to this problem consists in choosing a point of reference from which a first corpus is built by following systematically the explicit traces of intertextual relations. A close reading of the texts of this corpus then gives access to some more implicit forms of references. For instance, in the case of the 1874 controversy, both protagonists were referring to the "equation to the secular inequalities in planetary theory."[2] Such an explicit reference implicitly pointed to a network of texts that had been published over the course of the 19th century in various theoretical frameworks. The 1874 controversy opposed two attempts to turn this traditional algebraic culture into an object-oriented theory, which Jordan aimed to root on group theory while Kronecker laid the emphasis on the theory of quadratic forms.

3. Linear groups in Galois fields

In the framework of a collective research project,[2] a database of intertextual references has been worked out for all the texts published in algebra in France from 1870 to 1914.[3] One of the subgroups of this corpus gives rise to a coherent network of texts which involved mainly French and American authors from 1893 to 1907.[4] Let us characterize further this network by looking at its main shared references. These were, on the one hand, some French papers published in the 1860s, and, on the other hand, Moore's introduction of the abstract notion of Galois field in 1893 in addition to Dickson's 1901 monograph on *Linear groups with an exposition of the Galois field theory.* We shall see that the two times and spaces involved here point to a shared algebraic culture that can neither be identified to a discipline nor to any simple national or institutional dimension.

4. On nations and disciplines

Moore's 1893 "abstract" notion of Galois field has been assumed to highlight the influence of "trendsetting German mathematics" on the emergence of both the "Chicago algebraic research school"[11] and the "American mathematical research

[2]CaaFé : Circulations of algebraic and arithmetic practices and knowledge (1870-1945) : France, Europe, U.S.A ; http://caafe.math.cnrs.fr

[3]The corpus has been selected by using the classification of the *Jahrbuch*. On Thamous database of intertextual references, see http://thamous.univ-rennes1.fr/presentation.php

[4]On the one hand, Jordan, Borel and Drach, Le Vavasseur, de Séguier, Autonne, etc. On the other hand, Moore, Dickson, Schottenfels, Wedderburn, Bussey, Brger, Miller, Manning, etc.

community."[9] Here two kinds of categories have been used for making explicit some collective dimensions of mathematics, i.e., on the one hand, some national, or more local, collectives of mathematicians (the U.S.A., Germany, Chicago) and, on the other hand, some mathematical disciplines (abstract algebra). But even though the roles played by German universities in the training of many American mathematicians have been well documented, the influence of this institutional framework on mathematics has been assumed quite implicitly. Here two difficulties arise. First, the role attributed to "abstract algebra" reflects the tacit assumption that the communication of some local tacit knowledge should require direct contact. Second, both disciplines and nations are actors categories, which even though they were much involved in public discourses at the time,[12] cannot usually be directly transposed to the collective dimensions of mathematical developments.[5]
Recall that Moore's 1893 paper was read at the congress that followed the World Columbian exposition in Chicago. The world fair was the occasion of much display of national grandeur. The German delegation especially presented an exhibit of the German universities in which Klein was delivering a series of lectures. Klein was also the glorious guest of the congress while Moore was both the host of the congress and one of its main organizers. The latter's concluding lecture was a tribute to Klein's *Icosahedron.* It indeed aimed at generalizing to a "new doubly infinite system of simple groups," i.e., $PSL_2(F_{p^n})$, what was then designated as the three "Galois groups," i.e., $PSl_2(F_p), p = 5, 7, 11$, involved in the modular equations that had been investigated by Galois, Hermite, and Klein.[9] The generalization consisted in having the analytic form of unimodular binary linear fractional substitutions $\frac{ax+b}{cx+d}$ operate on a finite "field" of letters indexed by Galois number theoretic imaginaries.
The nature of the relevant collective dimensions nevertheless change if one shifts the scale of analysis from institutions to texts. As we shall see, even though he had aimed at celebrating the emergence of some abstract researches in the U.S.A. in the framework of the Göttingen tradition, Moore actually collided to the implicit collective dimension that was underlying the use of the analytic representation of substitutions on number theoretic imaginaries.[4]
In 1893, Moore initially appealed to Klein-Fricke's 1890 short presentation of Galois imaginaries but had not yet studied the references to Galois, Serret, Mathieu, Jordan, or Gierster which he cited from Klein-Fricke's texbook. On the one hand, what Moore designated as a Galois field corresponded to Cauchy's approach to higher congruences,[8] as developed later by Serret. On the other hand, Moore's "abstract finite field" was actually close to Galois's approach. Moore's remark that "every finite field is in fact abstractly considered a Galois field" thus echoed the connection between two perspectives on number theoretic imaginaries, as it had already been displayed in textbooks such as Serret's in 1866.
But even more dramatically, Moore's system of simple groups had actually already been introduced by Mathieu in 1861. As a result, before the publication of the proceedings of the congress in 1896, Moore and his student Dickson struggled to access the tacit collective dimension of some texts published in France in the 1860s,

especially by appealing to Jordan's 1870 *Traité des substitutions et des équations algébriques*. This appropriation resulted in the publication of a train of papers on "Jordan's linear groups in Galois fields." Moore's 1893 paper thus eventually resulted in the circulation of some works that were foreign to Klein's legacy. This situation highlights the difficult problem of identifying the scales at which various forms of collective dimensions play a relevant role, especially in respect to the articulation of the collective dimensions of texts with the ones of actors, such as disciplines or nations.

5. The analytic representation of substitutions

Let us now characterize more precisely the collective dimension to which Moore collided to in 1893. Given a substitution S operating on m letters a_i, the problem of the analytic representation consists in finding an analytic function ϕ such that $S(a_i) = \phi(i)$. Hermite's 1863 solution to this problem for the cases $m = 5, 7$ would especially influence Dickson's 1896 thesis. But the analytic representation also requires an indexing of the letters. As had been shown by Galois, if $m = p^n$, the indices can be considered as the "imaginary solutions" of the congruence $x^{p^n} - x \equiv 0(modp)$ that generalized the indexations given by the roots of Gauss's cyclotomic equations. Moreover, the use of such analytic forms went with some specific procedures. The procedure of reduction of "linear" substitutions $(i, ai + b)$ into combinations of cycles $(i, i + 1)$ and (i, gi) had especially played a key role in Galois's characterization of solvable irreducible equations of prime degree, which roots are permuted by substitutions of "a linear form" (x_i, x_{ai+b}).[4] In modern parlance, Galois's theorem and its proofs boil down to showing that the linear group is the maximal group in which an elementary abelian group (the cyclic group F_p^* in the case $n = 1$) is a normal subgroup. It was in attempting to generalize this theorem to equations of degree p^n that Galois introduced the number theoretic imaginaries.
Later on, in the 1860s, Jordan investigated further general linear groups as "originating" from the problem of finding the analytic form of the maximal group in which $F_{p^n}^*$ is a normal subgroup. He laid the emphasis on the procedures of reductions of the analytic representations of linear substitutions. Jordan's canonical form theorem especially gave a generalization to n variables of the reduction of $(i, ai + b)$ into $(i, i + 1)$ and (i, gi). For Jordan, such reductions were the very "essence of the question" because they were supporting links between various branches of mathematics such as number theory, the theory of equations, crystallography, mechanics, analysis situs, differential equations, etc.[6] Jordan's 1870 *Traité* played a key role in the development of a specific algebraic culture based on the reduction of the analytic representation of n-ary linear substitutions. This culture can not only be be traced in France in the works of authors such as Poincaré, Picard, Autonne, Cartan, Séguier, etc., but it also circulated in the U.S.A. after Dickson's 1896 thesis.[6]

6. Conclusion

The network of texts that revolved around "Jordan's linear groups in Galois field" at the turn of the 20th century had underlying it a specific algebraic culture based on procedures of reductions of the analytic forms of substitutions. It was because they shared this culture that some French and American authors were able to interact with each others. Communication was nevertheless partial and was actually mostly limited to some shared practices, such as the use of Jordan's canonical form. A telling example is the new formulation that was given repeatedly and independently to Jordan's "origin" of the linear group as the theorem stating that the group of automorphisms of an elementary abelian group $F^*_{p^n}$ is $Gl(F_{p^n})$ (Burnside, Moore, Levavasseur, Miller, Dickson Séguier).
We have seen that the systematic investigation of explicit traces of intertextual relations also sheds light on some more implicit collective forms of references, such as the one that lied beneath expressions such as "linear groups in Galois fields." This situation highlights the crucial role played by some networks of texts in the shaping of some algebraic cultures at a time when "algebra" was not yet referring to an object-oriented discipline.

References

[1] J. Boucard, *Un "rapprochement curieux de l'algèbre et de la théorie des nombres" : études sur l'utilisation des congruences en France de 1801 – 1850*, Thèse de doctorat, Université Paris 6, 2011

[2] F. Brechenmacher, La controverse de 1874 entre Camille Jordan et Leopold Kronecker, *Revue d'histoire des mathématiques*, **13**, 187-257.

[3] F. Brechenmacher, L'identité algébrique d'une pratique portée par la discussion sur l'équation l'aide de laquelle on détermine les inégalités séculaires des planètes (1766-1874), *Sciences et techniques en perspective*, **II, 1** (2007), 5-85.

[4] F. Brechenmacher, Self-portraits with évariste Galois (and the shadow of Camille Jordan), *Revue d'histoire des mathématiques*, **17** (2011), 271-369

[5] F. Brechenmacher, Galois Got his Gun, *http://hal.archives-ouvertes.fr/aut/Frederic+Brechenmacher/ to appear.*

[6] F. Brechenmacher, On Jordan's measurements, *http://hal.archives-ouvertes.fr/aut/Frederic+Brechenmacher/ to appear.*

[7] F. Brechenmacher, C. Ehrhardt, On the Identities of Algebra in the 19th Century, *Oberwolfach Reports*, **7**, 1 (2010), 24-31.

[8] C. Goldstein, Sur la question des méthodes quantitatives en histoire des mathématiques : le cas de la théorie des nombres en France (1870-1914). *Acta historiae rerum naturalium necnon technicarum*, **3** (1999), 187-214.

[9] C. Goldstein, Hermite's strolls in Galois fields, *Revue d'histoire des mathématiques*, **17**, (2011), 211-270.

[10] K. Parshall, Defining a mathematical research school: the case of algebra at the University of Chicago, 1892-1945, *Historia Mathematica*, **31** (2004), 263-278.

[11] K. Parshall, D. Rowe, *The Emergence of the American Mathematical Research Community (1876-1900): J.J. Sylvester, Felix Klein, and E.H. Moore*, Providence, American Mathematical Society, 1994.

[12] L. Turner, *Identities, agendas, and mathematics in an international space*, PHD thesis, Aarhus University, 2012.

Explicit and tacit knowledge in the teaching of mathematics in the 19th century

Caroline Ehrhardt

The theory that the kind of research one does has to do with the kind of training one has received has been widely exploited by scholars working on research schools. Yet the way in which this process actually takes place has not been so extensively studied. In fact, it is just as though because it is being labeled tacit a priori, the process of acquiring craft skills and their values, in schools and universities, remains almost invisible to historians [Olesko 1991; Warwick 2004]. To what point can we say that something that appears as tacit in research practices was actually made explicit during the training, or, in other words, how exactly specific ways or learning mathematics could be associated to knowledge or skills that would become tacit while doing research? In this paper, I will explore this issues taking examples from the learning and teaching of mathematics in the first half of the 19th century in France.

1. Explicit and tacit knowledge in the learning of mathematics for the French competitive exams.

From the beginning of the 19th century onwards, the French preparatory classes were organized to train students for scientific competitive exam of engineer schools, and in particular for the Ecole polytechnique [Belhoste 2003]. The knowledge the students had to master was published in official programs, and explained in textbooks that were widely used: the explicit knowledge shared by students was arithmetics, some algebra, elementary geometry, a little of analytical geometry, some statics and the solution of triangles. However, the official program did not say anything about the way by which this knowledge had to be acquired: what were the methods of work of students? What were the mathematical practices that were encouraged to master? What was the hierarchy between the themes, the criteria of evaluation etc.?

1.1. **Memorization and calculation as techniques for solving geometrical problems.** The 19th century was a period of development of publications specialized in education and, in particular, it was the time when the first "annals" where published. One of them, Ritts *Manual des aspirants à lEcole polytechnique* [Ritt 1839], tells us about what was expected from the students: Ritts aim was to make explicit what remained implicit in the programs and could help the students to pass the exam. First, Ritt recommended that the students should know perfectly the art of calculation. Second, students should know by heart a large range of issues that they could find in textbooks, that is to say questions about the lessons they had attended to. Moreover, all the themes that were mentioned in the programs did not play the same role in the students training: the more difficult questions (that would differentiate between the ones who would pass the exam and the others) were mostly about analytical geometry. To solve them, one had to choose from a range of techniques and tools the ones that would be useful in that particular case. This is why it was useful to have memorized several methods and proofs. To solve the problems quickly, it was needed to master calculation. Hence, these habits of calculation and memorization had an "operatory" dimension, and an important skill that students had to acquire was how to combine them in order to solve problems. Moreover, this context created an implicit hierarchy between the items of the program: some deserved a lot of attention, while others, like numerical and algebraic computations were only tools.

Sufficiency and good "writing style" as skills that distinguishes the better students. The reports about the concours général, another competitive exam that played a very important social role, also make explicit some other implicit skills that it was important for students to master. First, the students were not only asked to solve the proposed problem. They had to solve it thanks to several methods. In the case of geometry, it was by analysis and synthesis. In the case of numerical algebra, it was by different methods that provided different approximations. Second, the way to write the proofs and to expose the results was an important criterion of evaluation. For instance, Legendre justified his choice of subject in 1812 underlying that the "question is different enough from analogous ones so that his solution, well presented, justifies that students has acquired knowledge to a sufficient level to obtain the prize". Conversely, in 1819, the commission didn't attribute the first prize because "even the better exam paper was lacking of this elegance one has the right to call for the exam paper which wins the prize". The same features can be found in the competitive exam for the Ecole preparatoire, which would become the Ecole normale suprieure in 1830 [Ehrhardt, 2008]. Even if the criteria of evaluation had not been explicitly given by the examiners, one can see if one compares all the exam papers of the year 1829, that the examiners emphasized the writing qualities of the candidates. For instance, they remarked that the solution proposed in one of the exam paper is "rigorous and very elegant" and regret, conversely, that some calculation are "badly run" or "not very elegant". Hence, the evaluation is not made only about the correctness of proofs and calculation, but also about tacit values about what is a good proof. Moreover, the students who received the best assessment were those who wrote the longer answers, going beyond what was necessary to the question. For instance, in 1829, the assignment only required the candidates to expose a method to obtain the superior and inferior limit of the roots of an equation, but those who gave only one method were at the bottom of the grading scale. Hence, there was a tacit rule – which candidates knew and practiced– that consisted in telling the largest number of things they could about the topic they were asked about. Hence, the sufficiency of what the candidates wrote was an important, although implicit, criterion of the quality of their exam paper.

1.2. **What to teach? Tacit knowledge in the teaching of mathematics.** One can see how the practices and values favored by competitive exams were effectively transmitted to students by looking to the work of the teachers of mathematics, which is described in the Ministry inquiry about the Ecoles centrales

(1799). As far as the contents are concerned, all the teachers mostly used the same materials: books by Bézout, Lacaille, Bossut, then Lacroix. The regularity in the answers shows that there was a shared tacit knowledge of what they had to do. First, an important feature was to make the students practice mathematics by themselves, but also to learn and memorize several methods and to apply them to problems. Second, they didn't teach everything that was in the books, and often chose contents that could be applied, from a general point of view or depending on the local context. Hence, already in the last years of the 18th century, the teachers did know the king of abilities they had to train their students to, and it appears that these abilities are exactly the same as the ones that were considered as the "good ones" by the examiners, which we have seen in the first part.

2. From learning to research: the case of algebra

The official program of the preparatory classes (classes préparatoires) didn't include the part of mathematics that was probably the more fundamental tool for mathematical research at the time, namely differential and integral calculus. Moreover, the textbooks that were widely used at that time, like the ones by Lacroix by instance, focused on theorems and proofs, and compiled problems that would train students to solve a large range of mathematical questions. Hence, the explicit knowledge learnt in preparatory classes was clearly not sufficient to practice the kind of mathematical research that was done within the Academy of science, namely analysis. However, analysis was as much a theoretical knowledge based on calculus as a specific way to practice mathematics, which consisted in decomposing the problems in more elementary questions. The ideological program of the Ecoles centrales, at the very end of the 18th century, was precisely aimed at reaching this point: "Focusing on the methods which are the more convenient for the elements of Algebra and which often encompasses the seeds of the others, you will train your pupils to understand the latter as soon as they would be right in front of them, and you will make the analytical language uniform in all his scope". Hence, this secondary level training allowed the students to acquire practices of working and reasoning, ways to make mathematics and to think about it that could influence every mathematical activity that these students could make after their training, including mathematical research.

Let's take the example of Algebra to illustrate this point [Ehrhardt, 2010; Ehrhardt 2012]. The two textbooks of Algebra by Lacroix [Lacroix an VIII; Lacroix an XIII], which were the more widely used at the time, allow us to know more precisely what the students learnt. In complement of the theoretical parts related to algebraic calculation and on the theory of polynoms, Lacroix paid a lot of attention to the solution of equations as a concrete and practical problem. For the students, it was not enough to know the purely theoretical results: what they really had to know was how to calculate an approximate value of the roots of a given equation, either while looking for the better approximation, either for the one who was the easier to obtain. So, this apprenticeship of algebra was a practice of calculation oriented toward practical solution to problems. Besides, in this specific case, there is a strong correlation between the ways of doing algebra that were emphasized in the textbooks and what was asked to students during the exams. Actually, while giving two solutions instead of one, the students of the Ecole préparatoire showed that they were able to improve the numerical calculation and to make their solution more efficient in practice. In the same way, Ritt's book shows that algebra was not a topic studied for itself, but only to provide methods of calculation to solve other kinds of problems.

Coming now to the Academy of science, one can see that the main reference about Algebra at the beginning of the 19th century was Lagrange's treatise [Lagrange 1797]. This book was held up as an example in many papers sent to the Academy dealing with equations for the years 1795-1835, and it is very often the only reference given in the reports about them written by the academicians. In fact, Lagrange's research had generated the two main implicit strands of research on equations at the beginning of the nineteenth century. First, solving an equation algebraically meant finding an explicit formula or an algorithm that may enable mathematicians to express the roots in a finite form from the coefficients; if progress happened, it could come only from simpler methods. Second, the aim pursued through the theory of equations was above all practical; the method was only valuable if it ended by an approximate value of the roots – even if it had to cross the borders of algebra and use analytical tools for that. Applicability was much more important than methodological purity or the cleverness of the argument. And this is exactly what Lacroix stressed in his textbooks. So, from that point of view, the training in algebra, which consisted in knowing several processes that would provide a given approximation in a certain number of steps, and to apply them with a calculation without mistake, was transmitted in mathematical practice of research. Moreover, these tacit values about equation solving can be found in the assessment practices of the Academy. In fact, 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. The efficiency and the cleverness of the calculation processes are always underlined in the reports, and are a criterion to assess the papers. For instance, in 1813, Legendre wrote that Budan's method was valuable because it could make the transformations of equations easier, avoiding doing some multiplications over the coefficients. On the contrary, in 1820, Cauchy criticized the memoir sent by Salvaget because the method he developed was more complicated than the ones already known.

3. Conclusion: Relative autonomy of teaching and research

Tacit knowledge in scientific activities is often defined as something that comes from apprenticeship, and that cant be written in textbooks, for instance. What does it add to take the learning of mathematics as a point of departure for investigation? The answer could be that tacit knowledge appears here not necessarily as tacit as it first seemed. In fact, focusing on teaching, one can find actors who are not mathematicians for most of them, but who do express what specifically comes from teaching, the kind of habits, know-how that characterize apprenticeship. Hence, what is tacit or explicit partly depends on the kind of sources and actors we are looking at. Second, in the examples emphasized here, training and research were deeply coherent: in such cases, the (implicit) hypothesis according to which tacit knowledge would come from teaching seems to work. One could ask, however, whether it is a general characteristic of tacit knowledge or not. And in fact it seems to me that it almost and above all depends of the structure of the mathematical field at the time one is looking at.

Actually, in the first half of the 19th century, the whole system of preparatory classes was oriented toward the Ecole polytechnique. Moreover, the only way to gain scientific recognition was to have his research approved by the Academy. But since most academicians were polytechnicians, there was only one way to train scientific scholars, and that was the way that leaded to "the right way" to practice mathematics. Moreover, the mathematicians of the Academy played a very important role in the educational system. Poisson, and then Poinsot, were also working in the Ministry of Public instruction. There were academicians who wrote the subject for the Concours general. They also evaluated the students for the exams at the university. Some of them also evaluated the math teachers, as they were also inspectors. For instance, Lacroix was not only an academician in the eyes of math teachers. We learn from his correspondence that he was also a kind of model for them. Conversely, the group of mathematic teachers was still in the making at the time. Until the 1830 and there has not been a unique way of training for teachers. Some of them were coming from the Ecole normale, which was closed from 1822 to 1826, and then from the Ecole préparatoire, but most of them were coming from elsewhere. On the same way, not all of them had passed the "agregation" that would become progressively the only competitive exam in France for becoming teacher in high schools. On the same way, journals like Gergonne's Annales de mathématiques pures et appliquées, of the Journal of the Ecole polytechnique, or the Correspondance mathématique et physique by Quetelet were partly addressed to teachers and students, but their authors were also quite often famous mathematicians.

In fact, all these features show that the relative autonomy of teaching in preparatory classes and mathematical research was quite weak at the time, and that is certainly one reason why tacit knowledge has many ways to be transmitted and to circulate from the one to the other and conversely.

References

[1] Belhoste B., 2003, La formation d'une technocratie. L'Ecole polytechnique et ses élèves de la Révolution au Second empire, Paris, Belin.

[2] Ehrhardt C., 2008, "Evariste Galois, un candidat à l' école préparatoire en 1829", Revue d'histoire des mathématiques, vol. 14, fascicule 2, p. 289-328.

[3] Ehrhardt C., 2010, "A Social History of the 'Galois Affair' at the Paris Academy of Sciences", Science in Context, vol. 23.1, p. 91-119.

[4] Ehrhardt C., 2011, Evariste Galois. La fabrication d'une icône mathématique, Paris, éditions de l'EHESS.

[5] Lacroix S. F., an VIII, Éléments d'algèbre à l'usage de l'Ecole centrale des Quatre-nations, Paris, Courcier, an VIII.
[6] Lacroix S. F., an XIII, Compléments des éléments d'algèbre à l'usage de l'Ecole centrale des Quatre-nations, Paris, Courcier.
[7] Lagrange J. L., 1797, Traité de la résolution des équations numériques de tous les degrés, Paris, Duprat.
[8] Olesko K. 1991, Physics as a Calling. Discipline and Practice in the Könisberg Seminar for Physics, Ithaca, Cornell University Press.
[9] Ritt G., 1839, Manuel des aspirants à l'Ecole polytechnique contenant un très grand nombre de questions recueillies dans les derniers examens de concours, avec les solutions, Paris, Hachette.
[10] Warwick A., 2003, Masters of Theory. Cambridge and the Rise of Mathematical Physics, Chicago, The University of Chicago Press.

An example in which Tacit Knowledge was transformed into an important concept in the mathematical theory of perspective

Kirsti Andersen

In the early fourteenth century Italian painters started to experiment with creating depth in their pictures of architectural compositions. One of their means was to draw some lines, that in the three dimensional space are parallel and orthogonal to the picture plane, as converging lines in their images. Gradually it became a rule of thumb that all lines that are orthogonal to the picture plane are depicted as having a common convergence point.

In the first half of the fifteenth century perspective was introduced as a mathematical discipline, thereafter some theorists attempted to explain and prove the correctness of various methods of constructing perspective images. In doing so, they often tacitly assumed the practitioners' rule of thumb.

The real breakthrough in the mathematical theory of perspective took place with a work published by Guidobaldo del Monte in 1600. He had struggled for a dozen years to get to the bottom of the question of why perspective constructions function and found the solution by introducing the general concept of a vanishing point. Lines orthogonal to the picture plane have as their vanishing point the practitioners' convergence point.

Explicit versus Tacit knowledge in creating 'modern' analysis in the 19th century

Umberto Bottazzini

What is 'tacit' knowledge? In his *The Tacit Dimension*, Polanyi avoided to give a definition, but referred to it "by starting from the fact that we can know more than we can tell"([1], p. 4). Does Polanyi's concept of tacit knowledge help in better understanding the historical development of mathematics? Or, in other words, is it a helpful tool to the researcher in the history of mathematics? How does tacit knowledge relate to explicit knowledge as far as mathematics is concerned? Trying to answer these questions in the talk I have considered the case-study of continuity in the emergence of 'modern' analysis – as Cauchy called it – beginning by considering Euler's 1748 definition of a continuous function. It is interesting to remark that in doing it Euler referred to a somehow prior (and tacitly assumed) knowledge of what the word "continuous" means. Indeed, in vol. 2 of his *Introductio in analysin infinitorum* (1748) having observed that "curved lines can be described by the *continuous* [my emphasis] mechanical motion of a point, which presents the entire curved line to the eye at one time", Euler stated that "a *continuous* curved line is so defined, that it is expressed by a *single definite function of* x." [my emphasis] ([4], vol. 2 p. 4). Accordingly, if different parts of the curved line are expressed by different functions of x Euler called curved lines of this kind "discontinuous" or "mixed and irregular". Following Euler, the 18th-century mathematicians "tacitly" assumed his concept of continuity of functions. So did e.g. D'Alembert in his 1747 paper on vibrating strings, and explicitly repeated in his 1752 paper on the same subject by stating that the solution of the string equation is given by a unique analytic expression [i.e. is continuous in Euler's terms]. "In all the other cases – D'Alembert went on – the problem cannot be solved even by my method it might surpass the capacity of known analysis." ([3], p. 358) On the other hand, Euler (1755) replied that the initial shape can be given by any polygonal figure given by possible "discontinuous" curves. "The various parts of the curve are therefore not connected with each other by any law of continuity, and it is only by the description that they are joined together [...] it is impossible that all this curve should be included in any equation." ([5], p. 250) Euler considered what nowadays are called 'weak' solutions of the functional equation $y = \frac{1}{2}f(t+x) + \frac{1}{2}f(t-x)$ which can be (in modern terms) piecewise smooth. According to Euler, "the consideration of those functions (not subject to any law of continuity) opens to us an entirely new field of analysis". In spite of this, apparently Euler's discovery did not produce any shift in the mathematicians' "tacit knowledge" of what a continuous function was. The "law of continuity", which Euler was referring to, still justified Fourier's 1807 claim that an infinite series of continuous (trigonometric) functions like $y = \cos u - \frac{1}{3}\cos 3u + \frac{1}{5}\cos 5u - \frac{1}{7}\cos 7u + \ldots$ for an increasing number of terms tends more and more to approximate a (continuous) line in the (u, y)-plane which is composed of parallel straight lines and perpendicular straight lines (*which are themselves part of the line*). A similar observation, Fourier added, applies to the series $y = \sin u - \frac{1}{2}\sin 2u + \frac{1}{3}\sin 3u - \ldots$ ([6], pp. 159–160). In his *Cours danalyse* (1821) Cauchy reformulated Fourier's claim as a (celebrated) theorem stating that the sum of a convergent series of continuous functions is continuous (in his own sense). Even though Lagrange in his *Théorie des fonctions analytiques* (1797) had implicitly referred to a 'local' notion of continuity as opposed to the 'global' (ie. Eulerian) one, it was only in Cauchy's *Cours danalyse* that the former concept of continuity replaced the latter one: "The function $f(x)$ is continuous with respect to x *within the given limits* [my emphasis] if, within these limits, an infinitely small increase of the variable always produces an infinitely small increase of the function itself" (([2], p. 43) Abel's well known remark that Cauchy's theorem admits 'exceptions', and the subsequent correction made by Cauchy in 1853, raise an interesting point with respect to the issue 'tacit' *vs* explicit knowledge. In fact, Abel's example of 'exception' was nothing else than Fourier's second series mentioned above. Apparently, Abel considered it as an 'exception' to Cauchy's theorem precisely on the basis of Cauchy's definition of continuity (and convergence). And 'tacitly', ie. without mentioning Abel at all, Cauchy reformulated his theorem. In the talk I have also illustrated the 'tacit' use of the term 'continuous function' made by Cauchy when referring to functions of a complex variable, as he did for instance when stating his integral theorem (1825) and his integral formula (1831). Actually, the phrases "finite and continuous" were to be used repeatedly by Cauchy to refer to a class of appropriate complex functions that he was unable to characterize more precisely until the late 1840s i.e. until he made explicit the 'tacit' knowledge involved (and in doing it he discovered that the 'right' concept involved was analyticity instead of continuity). Summing up, the case-study of continuity shows that Polanyi's notion of 'tacit knowledge' may be useful to gain a better understanding of the historical development even though this does not provide any evidence to support his conclusion that "the ideal of a comprehensive mathematical theory of experience which would eliminate all tacit knowing is proved to be self-contradictory and logically unsound." ([1], p. 21)

References

[1] M. Polanyi, *The Tacit Dimension*, The University of Chicago Press: Chicago 1966.

[2] A.-L. Cauchy, *Cours d'analyse algébrique*, De Bure: Paris 1821. In *Œuvres complètes d'Augustin Cauchy* (2), vol. 3.

[3] J.-B. le R. D'Alembert, Addition au mémoire sur la courbe ..., *Memoires de l'academie des sciences de Berlin* 6, 1752, pp. 355–360.

[4] L. Euler, *Introduction in analysin infinitorum*, 2 vols, Bousquet: Lausannae 1848. In *Opera Omnia* (1), vols 8-9.

[5] L. Euler, Remarques sur les mémoires précédens de M. Bernoulli, *Memoires de l'academie des sciences de Berlin* 9, 1755, pp. 196-222, in *Opera Omnia* (2), vol. 10, pp. 233–254.

[6] I. Grattan-Guinness (with J: R. Ravetz), *Joseph Fourier, 1768-1830*, The Mit Press: Cambridge, Mass.

Different points of view on the reception of Poincaré's methods

Tatiana Roque

Today Poincaré is known as the founder of the theory of dynamical systems. The revolutionary aspect ascribed in retrospect to his "new methods" has often been regarded on the basis of an evolution spanning more than a century, which is divided into different stages, with major discontinuities. Thus, following a first legacy of works produced in the United States after 1913 by Birkhoff, the Soviet Union school of Andronov emerged, succeeded by Lefschetz during the Second World War; then, the works of Peixoto and Smale appear in the 1950s and 1960s. Several interpretations of these discontinuities have been proposed. Most of them ended up by entailing some consideration of the roles played by the formalist debates of the turn of the century, or about the higher importance accorded to quantum mechanics over the classical problems of celestial mechanics.

I propose here some new viewpoints on the reception of Poincaré's methods. In the first place the discussions about the definitions of stability in celestial mechanics after Poincaré, especially in the works of Lyapunov, Levi-Civita and Birkhoff, are analyzed. In these works the qualitative criterion is gradually made explicit. Secondly, I study the ways whereby astronomers regarded the innovations introduced by each scientist in his own times. Such an approach allows us to consider Birkhoff's interest in Poincaré's works within the broader context of the reception of Poincaré's methods in the United States. I particularly look into the case of the analysis of neighborhoods of periodic solutions by means of the method of analytic continuation. This type of analysis has often been regarded as an extension of the rigor of Cauchy's analytic methods to the field of celestial mechanics. Following the methodology of Goldstein, Brechenmacher and others, it is possible to identify a network of texts using the method of periodic solutions in theoretical and practical astronomy in the turn of the century. By looking more closely into these works, I treat the question of reception from a new perspective.

Astronomers were interested in the power of new methods to furnish alternative ways for the practice of astronomy and also frameworks to discuss the legitimacy of these practices. But the discussion about the kind of rigor put forward by the method of periodic solutions involves more than the theoretical conception expressed by the epistemological problem of stability (equivalent to the convergence of series). The direct character of this method enables another kind of research concerning practical determination of orbits and comparison with observations. It is thus possible to propose a refinement of the initial question: what could it mean to appeal to the rigor of modern mathematical methods in the astronomical context of the time? Series developments were seen as inexact but, above all, the calculations demanded by successive approximations started to be considered too massive and tedious.

The nineteenth-century observatory was a place where the quantitative spirit was valued most highly. Astronomy was the science where one has most frequently the occasion to carry out long and complicated computations, and the hierarchical place one occupied within the observatory was determined by ones mathematical knowledge. But the hypothesis here is that at the turn of the century there were different levels of disciplinarization of work in observatory mathematics. Besides a practical computational mathematics (devoted to the construction of tables), there was room for a more theoretical computational mathematics (with the aim of numerical determination of orbits). There was not only a division of work between computers and those who had to find coherence in the data and give them a more general sense. From inside the avalanche of numbers some astronomers started to realize that they needed more "exact", or direct methods – and periodic solutions turned out to be one of these methods – that were not so far from practical astronomical issues: numerical construction of particular solutions, determination of perturbed motions, comparison with observations.

My project also includes the investigation of the possible birth of a new community of astronomers, with increased mathematical demands. New methods, like the ones introduced by Poincaré, were seen as capable of rescuing the astronomers who were drowned in oceans of calculations. This case study can help to determine the specific character of Poincaré's position in relation to the ensemble of French, English and American astronomers. The development of this position between years 1896 and 1905 also illuminates the reception of the works of Poincaré in U.S. universities and observatories.

To grasp the singularity of this works is an opportunity to understand, in the same time, the singularity of a milieu: the one of astronomy and celestial mechanics in the turn of the century. Poincaré can be seen perhaps as the last product of a culture that would almost immediately change the values attached to calculations and to a disciplinarized work with numbers. In this way we could start to give a meaning to the word "qualitative" when associated with his works.

References

[1] D. Aubin, *A History of Observatory Sciences and Techniques*, Astronomy at the Frontiers of Science, ed. J.-P. Lasota, Berlin: Springer (2011), 109–121.

[2] D. Aubin, *Observatory Mathematics in the Nineteenth Century*, Oxford Handbook for the History of Mathematics, ed. Eleanor Robson and Jacqueline Stedall. Oxford: Oxford University Press (2009), 273–298.

[3] D. Aubin and A. Dahan-Dalmedico, *Writing the history of dynamical systems and chaos: longue durée and revolution, disciplines and cultures*, Historia Mathematica **29** (2002), 273–339.

[4] J. Barrow-Green, *Poincaré and the three body problem.* New York, London: American Mathematical Society and London Mathematical Society (1997).

[5] G.D. Birkhoff, *Fifty Years of American Mathematics*, Semicentennial Addresses of the American Mathematical Society. New York: American Mathematical Society (1938), 270–315.

[6] F. Brechenmacher, *La controverse de 1874 entre Camille Jordan et Leopold Kronecker*, Revue d Histoire des Mathématiques **13(2)** (2007), 187–257.

[7] F. Brechenmacher, *L'identité algébrique d'une pratique portée par la discussion sur l'équation l'aide de laquelle on détermine les inégalités séculaires des planétes (1766-1874)*, Sciences et Techniques en Perspective, IIe série **1** (2007), 5–85.

[8] F. Brechenmacher, *Self-portraits with Évariste Galois (and the shadow of Camille Jordan)*, Revue d'histoire des mathématiques **17(2)** (2011), 271–369.

[9] D.D. Fenster and K.H. Parshall, *A Profile of the American Mathematical Research Community: 1891-1906*, The History of Modern Mathematics, ed. Eberhard Knobloch and David E. Rowe, vol. 3. Boston: Academic Press (1994), 179–227.

[10] C. Goldstein, N. Schappacher and J. Schwermer (eds.) *The Shaping of Arithmetics after C.F. Gauss's Disquisitiones Arithmeticae*. Berlin: Springer (2007).

[11] C. Goldstein, *L'arithmétique de Pierre Fermat dans le contexte de la correspondance de Mersenne: une approche microsociale*, Annales de la faculté des sciences de Toulouse **18** (2009), 25–57.

[12] J. Gray, *Poincaré, Topological Dynamics and the Stability of the Solar System*, The Investigation of Difficult things, ed. P.M. Herman e Allan E.Shapiro. Cambridge: Cambridge University Press (1997), 503–524.

[13] J. Gray, *Linear differential equations and group theory from Riemann to Poincaré*. Boston, Basel, Stuttgart: Birkhäuser (1986).

[14] G.W. Hill, *On the Part of the Motion of the Lunar Perigee Which Is a Function of the Mean Motions of the Sun and Moon*, Acta Mathematica **8** (1886), 1–36 (initially published and distributed privately in 1877).

[15] G.W. Hill, *George William Hill, 'Remarks on the Progress of Celestial Mechanics since the Middle of the Century'*, Bulletin of the American Mathematical Society **2** (1896), 125–136.
[16] F.R. Moulton, *On the periodic solutions of the restricted problem of three bodies*, Transactions of the AMS **7** (1906), 537–577.
[17] F.R. Moulton, *An Introduction to Celestial Mechanics.* New York: The MacMillan Company (1914).
[18] S. Newcomb, *Reminiscences of an astronomer*, Boston: Houghton Mifflin (1903).
[19] K.H. Parshall and D.E. Rowe, *The Emergence of the American Mathematical Research Community, 1876-1900: J. J. Sylvester, Felix Klein, and E. H. Moore.* Providence: American Mathematical Society and London: London Mathematical Society (1994).
[20] T. Roque, *Stability of Trajectories from Poincaré to Birkhoff: approaching a qualitative definition*, Archive for History of Exact Sciences **65** (2011), 295–342.
[21] S. Schaffer, *Astronomers mark time: discipline and the personal equation*, Science in Context **2** (1988), 115–146.
[22] E.T. Whittaker, *Report on the progress of the solution of the problem of three bodies*, BAAS (British Association for the Advancement of Science) Report (1899), 121–159.
[23] C. Wilson, *The Hill-Brown Theory of the Moon's Motion: Its Coming-to-Be and Short-Lived Ascendancy (1877–1984).* New York: Springer (2010).

"The soul of the fact" – Poincaré and proof

Jeremy Gray

For Poincaré, the uninteresting part of proof was rigour, the interesting part is the role a proof plays in understanding a piece of mathematics. As he put it in *L'Avenir*, in 1908 (see [8]) : "Rigour is not everything – but without it there is nothing." Nonetheless, he cared about rigour, as his correspondence with Fuchs in 1880 demonstrated, as did his attention to uniform convergence, and his work on asymptotic series

He had reluctant criticisms of rigour. Proofs, he argued in (*L'Avenir*), can be too large, and well-chosen terms, such as 'uniform convergence' would encapsulate progress and prevent rigorous proofs from becoming almost incomprehensibly too long. Likewise, calculation should be an irreducible minimum, and never blind. Furthermore, proofs can be wrong in kind – e.g. in potential theory, where they do not mimic the actual processes involved. More-or-less intuitive proofs based on an appeal to Dirichlet's principle are without value for the mathematician, he said in 1905 [1] but are of the right sort to satisfy a physicist because they leave the mechanism of the phenomena apparent. More rigorous arguments for the existence of solutions depended on convergence arguments but this convergence was usually too slow, and the approximations involved too complicated for such approaches to yield effective numerical procedures.

And in any case, rigour is not enough. He observed in 1905, see [6], reprinted in *Science et méthode*, that Hilbert had exposed the formal character of reasoning in geometry, and remarked that even if the same was done for arithmetic and analysis, mathematics could not be reduced to an empty form without mutilating it and the origin of the axioms would still have to be investigated, however conventional they were taken to be. In *L'Avenir* he remarked that logical correctness is not all. "A lengthy calculation that has led to a striking result is not satisfying until we understand why at least the characteristic features of the result could have been predicted." And because it is not order per se, but only unexpected order that has a value, the mechanical pursuit of mathematics would be worthless, " *"A machine can take hold of the bare facts, but the soul of the fact will always escape it".*

So the problem for Poincaré was: How to proceed? Isolated facts had no appeal for him, but a class of facts held together by analogy brings us into the presence of a law, and as he continued in *L'Avenir*, "The importance of a fact is measured by the return it gives – that is, by the amount of thought it enables us to economise" (after Mach). He argued that the elegance of a good proof reflects an underlying harmony that in turn introduces order and unity and "enables us to obtain a clear comprehension of the whole as well as its parts. But that is also precisely what causes it to give a large return." The aesthetic response to mathematics was regarded by Poincaré as a sign of its efficacy, and this pair of ideas then shaped the rest of his address.

Flashes of insight, on the other hand, although convincing at the time, can mislead. As he put it in his address to the Parisian Society of Psychologists in 1908 (see his [10]), "The unconscious provides points of departure for calculations that must be made consciously, but operates by chance. And one must be careful, for the unconscious presents these ideas with a feeling of certainty even when, on rational analysis, they prove to be worthless."

There was, however, an in-built activity of the mind that was capable of providing knowledge, and that was our ability to reason by recurrence, and this allows for the growth of knowledge. And, he implied in his [4] in 1902, "Who doubts arithmetic?" (Perhaps no-one in 1900, when he made these remarks at the Paris ICM.)

Importantly, Poincaré argued (at the ICM in 1897, [2]) that mathematics and physics are inseparable. Mathematics, he said, is not a mere provider of formulae for physics. Indeed "The first reason why the physicist cannot give up mathematics is: it provides him with the only language he can speak." On the other hand, "The only natural object of mathematical thought is the integer [...] It is the external world that has imposed the continuum upon us, which we would have invented without doubt, but we have been forced to invent. Without it there would be no infinitesimal analysis all of mathematical science would reduce to arithmetic or to the theory of groups."

Poincaré repeatedly stressed the conventional element in mechanics – the equality of action and reaction, the definition of force, ... These claims, he said, are not increasingly well confirmed experimental results – they have been elevated to the status of conventions.

It is interesting to see how much of Poincaré's views make him a sceptic à la Wittgenstein, Kripke, and Kusch. He was a sceptic about physics, for he agreed that we rely on the testimony of experts and on a shared communication with others "No discourse – no objectivity" he said in 1902 (see [5]). He argued that we speak a shared family of languages, natural, scientific, mathematical which work because of a shared set of conventions, and we have ideas about what we would do if our statements conflict or communication failed. None of this involves knowing about meanings or have particular mental states. Conventionalism is surely much more akin to a language game, and if scepticism is criticised for implying relativism, and if it is relativism to permit faultless disagreement, Poincaré's geometric conventionalism is relativist.

But Poincaré was not a sceptic about pure mathematics. He believed that we *know* what reasoning by recurrence is in an almost Kantian fashion. But recall that for Poincaré mathematics and physics are inseparable, and his deepest commitment was to discovery in mathematics. Now, no serious philosophy of mathematics can ignore or mistreat the role of discovery: without it there would be no mathematics! As Poincaré said, even "the next generation of leading mathematicians will need intuition, for if it is by logic that one proves, it is by intuition that one invents" in the first volume of *Enseignement mathématique* (see [3]).

In conclusion: For Poincaré, a good proof in mathematics was enabling; it was a new and valid use of the terms it involves; it rested on general ideas capable of wider application (by analogy); it showed that some things are the case; it explained why some things are the case; and in physics it dealt in relations that would survive changing beliefs or practices about objects.

References

[1] Poincaré, H. 1890. Sur les équations aux dérivées partielles de la physique mathématique, *American Journal of Mathematics*, 12, 211–294. In *Oeuvres* 9, 28–113.

[2] Poincaré, H. 1897. Sur les rapports de l'analyse pure et de la physique mathématique, *Acta mathematica* 21, 331–341. In *La valeur de la science*, (1905b).

[3] Poincaré, H. 1899. La logique et l'intuition dans la science mathématique et dans l'enseignement, *Enseignement mathématique*, 157–162. In *Oeuvres* 11, 129–133.

[4] Poincaré, H. 1902a. Du rôle de l'intuition et de la logique en mathématiques, *Comptes rendus du IIe Congrès international des mathématiciens* Paris, 115–130. In *La valeur de la science*.

[5] Poincaré, H. 1902b. Sur la valeur objective de la science, *Revue de métaphysique et de morale* 10, 263–293. Rep. with modifications, as 'La science est-elle artificielle?' and 'La Science et la Réalité' in *La valeur de la science*, 213–247 and 248–276.

[6] Poincaré, H. 1905a. Les mathématiques et la logique, *Revue de métaphysique et de morale* 13, 815–835. Modified in *Science et Méthode* (1908b).

[7] Poincaré, H. 1905b. *La valeur de la science*, Flammarion, Paris.

[8] Poincaré, H. 1908a. L'avenir des mathématiques, *Revue générale des sciences pures et appliquées* 19, 930–939. Also in *Atti del IV congresso internazionale dei matematici*, 1909, 167–182. Only partially reprinted in *Science et Méthode*, and in the English trl. "The Future of Mathematics" in *Science and method*.

[9] Poincaré, H. 1908b. *Science et méthode*, Flammarion, Paris.

[10] Poincaré, H. 1909. L'invention mathématique, *Année psychologique* 15, 445–459. In *Science et Méthode*.

The role of diagrams in contemporary mathematics

Jessica Carter

The talk consists of three parts. The first introduces the topic of visualisation in mathematics. The second and the main part of the talk shows how visualisation plays a role in contemporary mathematics, in particular how diagrams serve as generators of both concepts and proofs and how, sometimes, some kind of mental imagination is needed in order to see that something is the case. Finally addressing the question: 'Why is visualisation so fruitful for mathematics?' I offer a couple of suggestions about what is achieved by diagrams.

Throughout history diagrams have played a significant role in mathematics. Analysis was long founded on geometry. Most notably diagrams were an integral part of Greek mathematics. Indeed [4] argues that for the Greeks, 'diagram' is a metonym for proposition. During the 19th century, however, diagrams - or pictures - became discredited. Analysis moved towards an arithmetic foundation. Concepts of *function, continuity* and *differentiability* were developed so that it became possible to form the geometrically unintuitive "continuous, but nowhere differentiable, function". Even in geometry, diagrams were disapproved of, here in the famous words of Pasch:

> For the appeal to a figure is, in general, not at all necessary. It does facilitate essentially the grasp of the relations stated in the theorem and the constructions applied in the proof. Moreover, it is a fruitful tool to discover such relationships and constructions. However, if one is not afraid of the sacrifice of time and effort involved, then one can omit the figure in the proof of any theorem; indeed, the theorem is only truly demonstrated if the proof is completely independent of the figure. ([5], 43)[1]

Note that Pasch does not dismiss the use of diagrams as such. He stresses that use of diagrams is a fruitful tool for for discovery, and that they display relations. These are major points of the present paper.

I further note that attitudes towards proofs range from the view that a diagram "has no proper place in a proof as such" (Tennant) to a much more relaxed view towards proofs as such, here expressed by Vaughan Jones: "Proofs are indispensable, but I would say they are necessary but not sufficient for mathematical truth, at least truth as perceived by the individual"([3], p. 208).

The main focus of the talk is on what role diagrams play in (generation of) proofs. It is not discussed whether diagrams can be used to obtain rigorous proofs. I argue, opposing the view that diagrams play no role in proofs, that in practice they do. I do this by showing an example from contemporary mathematics, more precisely from Free probability theory, where diagrams were used to find certain results.

[1] Translation from Mancosu, P. et. al. 2005. *Visualisation, Explanation and Reasoning styles in Mathematics.* Springer, p. 14.

The actual example concerns determining the value of the following expression.[2]

$$\mathbb{E} \circ Tr_n[B_1^* B_{\pi(1)} \cdot \ldots B_p^* B_{\pi(p)}].$$

In this expression the B's are so called Gaussian Random Matrices (GRM's), i.e., they are matrices whose entries are complex valued Gaussian distributed random variables. The focus is on the indices, the $\pi(i)$. π denotes a permutation on the set $\{1, 2, \ldots p\}$. It is the case that the value of the above expression depends on properties of this permutation. These properties are found by representing these permutations by certain diagrams as pictured in Figure 1.

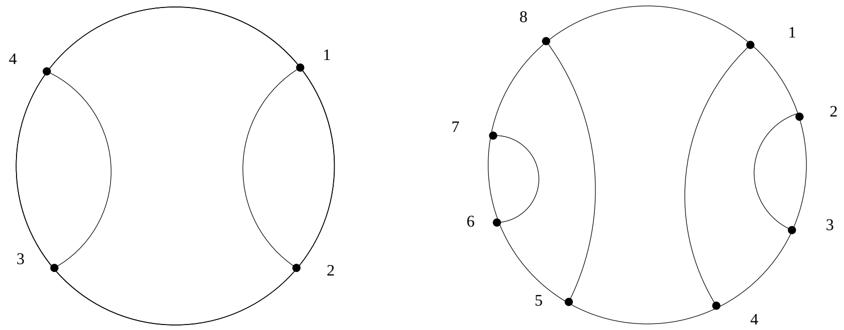

FIGURE 1. The picture on the left is a representation of the permutation π, $(12)(34)$. The picture on the right shows a permutation that is obtained from π, which is also used to obtain the value of the expression.

I show:

(1) That diagrams like these inspire definitions and proof strategies.
(2) That sometimes they work as mental frameworks or images in parts of proofs.

I also note that there are two types of visualisation at play, namely where visualisation is taken as representations that can be written down on paper, and as a sort of mental picture which helps us to see that something is the case. (The details can be found in [1].)

The theme of the workshop is tacit knowledge. I briefly explain how the topic of the talk relates to this theme. The proposal "invites a use of tacit and explicit knowledge to achieve a better knowledge of **how mathematical creation proceeds** and also how cultural habits play a tacit role in mathematics production".

The results presented here demonstrate how certain tools - diagrams - are used to obtain results in mathematics. These diagrams are completely removed from the articles presenting the results. So they are tacit in the sense that they *could*

[2]The mathematical details can be found in [2].

be made explicit, but for some reason it is not valued among mathematicians to present them in articles. When giving talks, however, diagrams may be shown. This particular case study also shows that there is a gap between how results are found and the way that they are supposed to be (re)presented: As text and linearly. What is tacit - perhaps in a deeper sense - is the question concerning the fruitfulness of visual tools in mathematics. A full explanation of this fact has not yet been provided. In the final part I point to some aspects of what is achieved by using diagrams. First I note that some diagrams shown *display relations*. So they are actually diagrams in the sense that Peirce uses the term in his semiotics. Diagrams are precisely icons (i.e., signs that represent by virtue of likeness) that represent relations. In other cases visual tools show constructions. Many examples can be found in Euclid's Elements. I also point to an example from the case study presented here that pictures a construction.

Finally concluding about what is achieved by diagrams or visualisation, I note that they display relations (that may not be visible from the textual representation of the problem), they afford multiple interpretations (which is not wanted by symbolic representation), and, as shown in the case study, they can be used as tools for experimentation.

References

[1] Carter, J. 2010. Diagrams and Proofs in Analysis. *International Studies in the Philosophy of Science* 24, 1-14.

[2] Haagerup, U., and S. Thorbjørnsen 1999. Random matrices and K-theory for exact C^*-algebras. *Documenta Mathematica* 4: 341-450.

[3] Jones, V. 1998. A credo of sorts. In *Truth in mathematics*, edited by H.G. Dales and G. Oliveri, 203-231. New York: Oxford University Press.

[4] Netz, R. 1999. *The shaping of Deduction in Greek Geometry.* Cambridge: Cambridge University Press.

[5] Pasch, M. 1882. *Vorlesungen über neuere Geometrie.* Springer.

Weyl and the kleinean tradition

Christophe Eckes

Die Idee der Riemannschen Fläche (first edition 1913) is generally considered as the first rigorous and systematic presentation of Riemann's ideas in complex analysis. This monograph derives from a lecture course given by Weyl as a Privatdozent at the university of Göttingen during the winter-semester 1911-1912. In the first part of this book, Weyl builds up Riemann surfaces by using a generalization of Weierstrass' analytic continuation: he combines Riemann's viewpoint and Weierstrass' viewpoint in complex analysis. Then, he defines abstractly the concept of a bi-dimensional topological manifold by using a list of axioms (§ 4) [1], p. 17-18). In fact, he simplifies a first definition of a topological surface given by Hilbert in a supplement to the *Grundlagen der Geometrie*[2], p. 234-235. Furthermore, Weyl shows that a Riemann *surface* is a one-dimensional complex analytic *manifold* (§

7, [1], p. 36). In the second part, he proves anew the *Dirichlet's principle*[1] which is located at the core of Riemann's *Dissertation* (1851). In accordance with Hilbert's prescriptions, Weyl proves the uniformization theorem by using this principle ([1] § 19, p. 141-148).

A "modern" demand for rigour. In the preface of his monograph, Weyl seems to reject categorically intuition and geometric representations in complex analysis because they lead to mistakes and confusions ([1], p. III). Weyl employs the category of *modernity* to qualify a so-called "demand for rigour". Moreover, he claims that *intuitive representations* must be systematically replaced by *set-theoretically exact proofs*. According to him, set theory satisfies all the modern requirements for rigour. In order to clarify this point, let us recall that Zermelo is a professor at Göttingen until 1910. His "Untersuchungen über die Grundlagen der Mengenlehre" contain a first "rigorous" axiomatization of set theory[2]. It seems that Weyl is fully convinced by Zermelo's work on set theory ([5], p. 93-95, 109-113). Moreover, he believes that the *continuity* property can be fully deduced from set-theoretical considerations.

To sum up, we may think that Weyl's monograph on Riemann surfaces consists in making intuitive representations more *explicit* in *analysis situs* and in *complex analysis*. To this end, he refers simultaneously to axiomatic method — in a hilbertian vein — and to set theory — in accordance with Zermelo's last achievements. However, he does not consider the "modern demand for rigour" as an end in itself. Moreover he expresses scepticism about a so-called "formalist" conception of mathematical knowledge ([1], p. VI). Where does this scepticism come from? How to explain the apparent contradiction between a " modern demand for rigour " and a (counter-modern) criticism of formalism?

Klein's legacy. In fact, Weyl's argument against formalism reminds us of Klein's conclusive remarks in his famous address delivered at Göttingen in 1895 and entitled "Über Arithmetisierung der Mathematik"([6], p. 232-241). More precisely, we would like to underline the importance of Klein's legacy in Weyl's work at three levels. (1) A *mathematical level*: Klein (1882)[7] and Weyl share obviously the idea following which Riemann surfaces are located at the foundation of function theory. (2) An *epistemological level*: logic and intuition are complementary in the development of mathematics. On the one hand, logical rigour is necessary in order to avoid all the mistakes generated by a *naïv* intuition, on the other a refined intuition is required in the construction of mathematical knowledge. (3) A *pedagogical level*: intuitive representations must play a central role in a basic course (cf. Weyl's introductory course in complex analysis[8].

For instance, at the end of his *Habilitation lecture*, Weyl paraphrases Klein's metaphor:

> The science of mathematics may be compared to a tree thrusting its roots deeper and deeper into the earth and freely spreading out its shady branches

[1] A first rigorous proof of this principle is given by Hilbert in his article [3], p. 184-188.

[2] [4], p. 261-281.

> to the air. Are we to consider the roots or the branches as its essential part? Botanists tell us that the question is badly framed, and that the life of the organism depends on the mutual action of its different parts.[9]

This image is employed by Klein in order to illustrate the complementarity between intuition and logic in the construction of mathematical knowledge. Moreover, in "Über Arithmetisierung der Mathematik" and in his "Lectures in mathematics" in Chicago (1893), Klein defines a series of guidelines in the teaching of mathematics. In particular, he claims that intuition is absolutely necessary in an introductory course:

> Two classes at least of mathematical lectures must be based on intuition ; the elementary lectures which actually introduce the beginner to higher mathematics (...) and the lectures which are intended for those whose work is largely done by intuitive methods, namely, natural scientists and engineers.(*Ibid.*, p. 248.)

The modern demand for rigour can only be satisfied in more advanced courses. Accordingly, in his elementary course in complex analysis (1910-1911), Weyl refers to physical and geometric representations before introducing and defining mathematical concepts. This intuitive approach reminds us of Klein's lecture course on algebraic functions of a complex variable[10]. On the other hand, in his more advanced course devoted to Riemann surfaces (1911-1912), Weyl's reasoning is more abstract. For instance he considers systematically the concepts of a topological surface, of a Riemann surface, etc., independently from their realizations and he defines them axiomatically. The organization of these two courses shows that Weyl satisfies Klein's requirements in his teaching practice: the intuitive approach comes before the "logical element" which must be introduced progressively.

Let us recall that in 1909-1910 Klein organizes a seminar entitled "Mathematik und Psychologie" at Göttingen. This seminar is devoted to epistemological and pedagogical issues in mathematics. During the second session (3. november 1909), Weyl makes a review on "L'enquête sur la méthode de travail des mathématiciens" (*L'enseignement mathématique* 1905-1908)([11], p. 6). "L'enquête" is a wide survey based on a questionnaire mainly addressed to European mathematicians. One of its goals is to describe the psychological roots of mathematical knowledge. Weyl sums up the results of "l'enquête" in his review. His involvement in Klein's seminar shows that he is close to Klein during the period 1908-1913.

To sum up, Weyl's monograph on Riemann surfaces is generally considered as the first modern and rigorous presentation of Riemann's geometric ideas in function theory. Under this assumption, it is just characterized by one fact : making Riemann's and Klein's intuitive approach more *explicit* by using a generalization of Weierstrass' analytic continuation, Hilbert's axiomatic method and Zermelo's rigorous presentation of set theory. This standpoint must be relativized because it doesn't take into account the fact that Weyl's monograph derives from a lecture course. Accordingly, this book must be analyzed in function of Weyl's *teaching practice* which is deeply influenced by Klein. Moreover, in his *Habilitation lecture*, Weyl alludes simultaneously to Klein's "Arithmetisierung der Mathematik" and to Hilbert's *Grundlagen der Geometrie* without articulating these to references. This under-articulation characterizes a tacit knowledge in Weyl's early work at an epistemological level.

Klein and the unity of mathematics.
In 1930, Weyl becomes professor at Göttingen after Hilbert's retirement. Shortly before, Weyl gives a famous address on the occasion of the inauguration of the Mathematics Institute at Göttingen (3. December 1929) ([12]). This talk is merely an homage to Klein. Weyl aims at describing Klein's contributions in pure mathematics and his underlying conception of the unity of mathematics. According to Weyl, Klein's work in (pure) mathematics is mainly characterized by the combination between separated "disciplines", distinct theories and different methods. Moreover Klein's way of unifying mathematics is based on two ingredients : (1) intuition and (2) group theory.

Weyl claims for instance that "Das Hauptorgan von Kleins mathematischer Methodik war das *intuitive, die Zusammenhänge erschauende Verstehen*"(*Ibid.*, p. 294). We can find exactly the same assumption in Weyl's late writings on constructive and axiomatic procedures in mathematics.[3] The *group* concept plays a central role in Klein's *Erlanger Programm* (1872) and also in his *Vorlesungen über das Ikosaeder* (1884). Weyl underlines this fact in his homage to Klein: " *Die Gruppe blieb seit jener Zeit der beherrschende Gesichtspunkt von Kleins mathematischen Schaffen* " ([12], p.294). In fact, Weyl does not comment neutrally Klein's work in pure mathematics. He implicitly refers to an *epistemic value* concerning the fruitfulness of a research. A mathematical production is all the more fruitful since it implies several new connections between separate domains. This *tacit knowledge* guides Weyl in his practice of mathematics. Accordingly, when Weyl claims for instance that Noether is an "algebraist", his judgement is a little bit pejorative. In other words, Weyl continues a kleinean tradition, which consists in producing mathematics by combining very different domains. Moreover, it becomes a criterium in order to evaluate productions due to other mathematicians.

More precisely, Klein's work is characterized by a series of links between *group theory* and Riemann's *geometric ideas* in *analysis situs* and in complex analysis.

> "*Verbindung von Galois und Riemann*" hieß die Parole. — Durch diese Tendenz, die Schleusen zu öffnen, welche die Kanäle des mathematischen Denkens fast hermetisch gegeneinander abschlossen, hat Klein sicherlich auf die nachfolgende Mathematikergeneration nachhaltig gewirkt.(Ibid., p. 294)

Weyl belongs to this "nachfolgende Mathematikergeneration". He continues this project in very different ways during all his scientific career. For instance, he

[3][13], p. 15: "The chief organ of Klein's own productivity was this intuitive perception of interconnections and relations between separate fields (...) In the time of Klein's productivity (which had passed when I entered the University of Göttingen in 1904) the intuitive realization of inner connections between various domains had been the most characteristic feature of his achievements. Typical is his book on the Icosahedron in which geometry, algebra, function- and group-theory blend in polyphonic harmony".

develops group-theoretical methods in the theory of covering surfaces (1913-1916). Moreover, the theory of linear Lie groups and their algebras plays a central role in the resolution of Weyl's problem of space (1921-1923), i.e. the characterization of the so-called "infinitesimally pythagorean manifolds" (differentiable manifolds with a metric structure defined by a non-degenerate quadratic form). Conversely, in his article on Lie groups (1925-1926), Weyl uses Riemann's "geometric ideas" in *analysis situs* to prove the complete reducibility theorem (for complex semi-simple Lie groups). On this occasion, he generalizes the theory of covering surfaces to Lie groups.

conclusion : a short synthesis of Klein's legacy. At the end of his *Habilitation* lecture (1910), Weyl alludes to Klein's viewpoint following which *refined intuition* and *logic* are complementary in mathematics. At the same time, Weyl criticizes a formalist conception of mathematical knowledge. Moreover, his introductory course in complex analysis (1910-1911) satisfies the prescriptions formulated by Klein in the teaching of mathematics. More generally, during all is scientific career, Weyl is impressed by Klein's way of unifying mathematics and he continues Klein's project which consists in relating Riemann's geometric ideas to group theory. Klein's underlying conception of the unity of mathematics is all the more important in order to analyze Weyl's contributions in mathematics and his epistemological viewpoint on mathematical sciences.

References

[1] H. Weyl, *Die Idee der Riemannschen Fläche*, Leipzig, Teubner, 1913.

[2] D. Hilbert, "Über die Grundlagen der Geometrie", *math. Ann.*, **56**.

[3] D. Hilbert, "Über das Dirichlet'sche Prinzip", *Jahresbericht der Deutschen Mathematiker-Vereinigung* **8** 1900.

[4] E. Zermelo, "Untersuchungen über die Grundlagen der Mengenlehre ", *mathematische Annalen*, **65**) 1908.

[5] H. Weyl, "Über die Definitionen der mathematischen Grundbegriffe", (Habilitation lecture, 1910), *mathematisch-Naturwissenschaftliche Blätter*, **7**.

[6] F. Klein, "Über Arithmetisierung der Mathematik" (1895), in *Gesammelte mathematische Abhandlungen*, Bd. II, Berlin, Springer, 1922.

[7] F. Klein, *Über Riemanns Theorie der algebraischen Funktionen und ihrer Integrale*, Teubner, Leipzig, 1882.

[8] H. Weyl, *Einführung in die Funktionentheorie*, Bearbeitet von R. Meyer und S. J. Patterson, Basel, Birkhäuser, 2008.

[9] F. Klein, "The Arithmetizing of Mathematics" translated by Isabel Maddison, Bryn Mawr College, *Bulletin of the American Mathematical Society*, Vol. **2**, n°2, p. 248-249.

[10] F. Klein, *Über Riemanns Theorie der algebraischen Funktionen und ihrer Integrale*, Leipzig, Teubner, 1882.

[11] F. Klein, *Protokollbuch der Seminare Felix Kleins*, "Wintersemester 1909-1910 / Mathematik und Psychologie."

[12] H. Weyl, "Felix Kleins Stellung in der mathematischen Gegenwart", *Die Naturwissenschaften*, **18**, 1930, p. 4-11, reprinted in GA, Bd. III, p. 292-299.

[13] H. Weyl, "Axiomatic Versus Constructive Methods in Mathematics", *The mathematical intelligencer*, Vol. **7**, n°4, 1985.

Fermat's Last Theorem and the Logicians

Emily R. Grosholz

Reasoning in mathematics often generates internally differentiated texts because thinking requires us to carry out two distinct (though rationally related) tasks in tandem. Analysis requires us to engage in the abstract, more discursive project of theorizing, what Leibniz called analysis or the search for conditions of intelligibility (of problematic objects) or solvability (of objective problems). Reference requires more practical, concretely realized strategies for achieving the clear and public indication of what we are talking about. In a standard logic textbook, the universe of discourse is the set of individuals invoked by the general statements in a discourse; they are simply available. And predicates and relations are treated as if they were ordered sets of such individuals. In real mathematics, however, the discovery, identification, classification and epistemic stability of objects is problematic; objects themselves are enigmatic. It takes hard work to establish certain items (and not others) as canonical, and to exhibit their importance. Thus reference is not straightforward. Moreover, neither is analysis; the search for useful predicates and relations, for procedures which may be generalized into methods, and for problems that may engender families of problems, is just as difficult as the identification of overarching principles, and just as necessary for the organization of mathematical knowledge. We investigate things and problems in mathematics because we understand some of the issues they raise but not others; they exist at the boundary of the known and unknown.

Fermat's Last Theorem (1630) states that the equation xn + yn = zn, where xyz not equal 0, has no integer solutions when n is greater than or equal to 3. Fermat, Euler, Dirichlet, Legendre and Lamé made early contributions to the problem; Sophie Germaine and Ernst Eduard Kummer produced more general, and generalizable, results in the 19th century, relating the theorem to what would become class field theory in the 20th century. The striking feature of Wiles' proof, to people who are not number theorists, is that it does not seem to be about integers! This is because the proof hinges on a problem reduction: the truth of Fermat's Last Theorem is implied by the truth of the Taniyama-Shimura conjecture: every elliptic curve over Q is modular. This identification is important because then its L-function has an analytic continuation on the whole complex plane, which makes Wiles' proof the first great result of the Langland's Program, and a harbinger of further results. Wiles' proof is not only about the integers and rational numbers; it is also about much more 'abstract' and indeed somewhat ambiguous objects, elliptic curves and modular forms. Moreover, the culmination of Wiles' proof, where analysis has invoked cohomology and the machinery of deformation theory, also involves quite a bit of down-to-earth number-crunching. The two tasks of reference and analysis within this proof generate mutually disparate discourses which are themselves internally heterogeneous; for the proof to go through, all these elements must be brought into rational relation by various strategies of integration.

A notable feature of Andrew Wiles' proof of Fermat's Last Theorem is that it invokes cohomology theory (inter alia) and thus Grothendieck's notion of successive universes, which, from the point of view of set theory, become very large; and yet the detail of the proof stays on relatively low levels of that vast hierarchy. This bothers logicians, who find Grothendieck's theoretical setting logically extravagant. Colin McLarty offers foundations for the cohomology employed in Wiles' proof at the level of finite order arithmetic; he uses Mac Lane set theory, which has the proof theoretic strength of finite order arithmetic, and Mac Lane type theory, a conservative extension of the latter. Angus Macintyre is re-working aspects of the proof (bounding specific uses of induction and comprehension) to bring it within a conservative n-th order extension of Peano Arithmetic. Meanwhile, the significant re-working and extension of the proof by number theorists [8], and two recent articles by Mark Kisin [10][11] proceeds independently of logic, in the sense that number theorists don't seem particularly concerned about the logical complexity of their methods. On the one hand, we see number theorists choosing powerful methods that usefully organize their investigations into relations among numbers, and make crucial computations visible and possible. On the other hand, we see logicians analyzing the discourse of the number theorists, with the aim of reducing its logical complexity. Should number theorists care whether their abstract structures entail the existence of a series of strongly inaccessible cardinals? Will the activity of logicians produce useful results for number theorists, or is it enough if they answer questions of interest to other logicians, such as whether in fact Fermat's Last Theorem lies beyond the expressive strength of Peano Arithmetic (and thus might be a historical and not merely artificially constructed example of a Gödel sentence)?

As I have argued above, mathematical discourse must carry out two distinct tasks in tandem, analysis and reference. In the case of number theory, the referents are integers and rational numbers in one sense and additionally, in a broader sense given the problem reduction at the heart of Wiles' proof, modular forms and elliptic curves. For logic, the referents are propositions and sets (and perhaps also formal proofs), or, if we include the broader range of category theory as part of logic, categories (and perhaps also functors and toposes). Thus what is an aspect of analysis for the number theorist is an aspect of reference for the logician. Moreover, techniques of calculation that preoccupy the number theorist remain tacit for the logician because they directly involve numbers, and considerations of logical complexity that concern the logician remain tacit for the number theorist because they are not conditions of solvability for problems about numbers. This disparity is inescapable, but it is also positive for the advance of mathematics. For when what remains tacit in one domain must be articulated in another in order to bring the domains into rational relation, novel strategies of integration must be devised.

References

[1] A. Altman and S. Kleiman, *An Introduction to Grothendieck Duality Theory*, New York: Springer, 1970.

[2] C. Breuil, B. Conrad, F. Diamond, R. Taylor, *On the modularity of elliptic curves over*, in: Journal of the American Mathematical Society **14**, 2001 pp. 843–939.

[3] C. Cellucci, *Filosofia e matematica*, Rome: Editori Laterza, 2002.

[4] C. Cellucci, *La filosofia della matematica del Novecento*, Rome: Editori Laterza, 2007.

[5] G. Cornell, J. Silverman, G. Stevens, *Modular Forms and Fermat's Last Theorem*, New York: Springer, 1997.

[6] H. Darnon, F. Diamond, R. Taylor, *Fermat's Last Theorem*, in: Conference on Elliptic Curves and Modular Forms, Dec. 18-21, 1993 (Hong Kong: International Press): pp. 1–140.

[7] P. Freyd, *Abelian Categories*, New York: Springer, 1964.

[8] G. Grosholz, *Representation and Productive Ambiguity in Mathematics and the Sciences*, Oxford: Oxford University Press, 2007.

[9] R. Hartshorne, *Algebraic Closed Fields*, New York: Springer, 1977.

[10] M. Kisin, *Modularity of 2-adic Barsotti-Tate representations* Inventiones Mathematicae **178 (3)**, 2009 pp. 843–939.

[11] M. Kisin, *Moduli of finite flat group schemes, and modularity* Annals of Mathematics **170 (3)**, 2009 pp. 1085–1180.

[12] A. Macintyre, *The Impact of Gödel's Incompleteness Theorems on Mathematics* in: Kurt Gödel and the Foundations of Mathematics: Horizons of Truth, Vienna: Proceedings of Gödel Centenary, 2006, pp. 3–25.

[13] B. Mazur, *Modular Curves and the Eisenstein ideal* Publications Mathematiques, Institut des Hautes tudes Scientifiques **47** 1977, pp. 133-186.

[14] C. McLarty, *A Finite Order Arithmetic Foundation for Cohomology* 2012, Forthcoming.

[15] C. McLarty, *What Does It Take to Prove Fermat's Last Theorem? Grothendieck and the Logic of Number Theory* The Bulletin of Symbolic Logic **16 (3)** 2012, pp. 359–377.

[16] K. Ribet, *Galois Representations and Modular Forms* Bulletin of the American Mathematical Society **32 (4)** 1995, pp. 375–402.

[17] D. Schlimm, *Analyzing Analogies in Mathematical Domains*, 2012, Forthcoming.

[18] A. Wiles, *Modular elliptic curves and Fermat's Last Theorem* Annals of Mathematics **142** 1995, pp. 443–551.

Reporter: Jemma Lorenat

Participants

Prof. Dr. Kirsti Andersen
Van Reigersbergenstraat 232
NL-1052 WS Amsterdam

Prof. Dr. Thomas Archibald
Department of Mathematics
Simon Fraser University
Burnaby , B.C. V5A 1S6
CANADA

Prof. Dr. David Aubin
Institut de Mathematiques de Jussieu
Histoire des Sciences Mathematiques
Case Postale 247
4, place Jussieu
F-75252 Paris Cedex 05

Prof. Dr. June E. Barrow-Green
Faculty of Mathematics & Computing
The Open University
Walton Hall
GB-Milton Keynes MK7 6AA

Dr. Alain Bernard
78 Bd. Arago
F-75013 Paris

Prof. Dr. Henk J. M. Bos
Van Reigersbergenstraat 232
NL-1052 WS Amsterdam

Prof. Dr. Umberto Bottazzini
Dipartimento di Matematica
Universita di Milano
Via C. Saldini, 50
I-20133 Milano

Prof. Dr. Frederic Brechenmacher
Institut de Mathematiques de Jussieu
Case 247
Universite de Paris VI
4, Place Jussieu
F-75252 Paris Cedex 05

Prof. Dr. Herbert Breger
Eichstraße 7
30161 Hannover

Prof. Dr. Sonja Brentjes
Departamento de Filosofia y Logica y
Filosofia de la Ciencia
Universidad de Sevilla
C/S. Fernando, 4
E-41004 Sevilla

Prof. Dr. Jessica Carter
Institut for Matematik og Datalogi
Syddansk Universitet
Campusvej 55
DK-5230 Odense M

Prof. Dr. Karine Chemla
Directrice de recherche CNRS
3, square Bolivar
F-75019 Paris

Prof. Dr. Renaud Chorlay
REHSEIS
Universite Paris 7
Centre Javelot
2 Place Jussieu
F-75251 Paris Cedex 05

Prof. Dr. Jean Christianidis
Dept. of History & Philosophy of Science
Athens University
University Campus
Ano Ilisia
15771 Athens
GREECE

Dr. David Corfield
School of European Culture & Languages
University of Kent
Canterbury
GB-Kent CT2 7NF

Prof. Dr. Leo Corry
The Cohn Institute for the History
and Philosophy of Science and Ideas
University of Tel Aviv
Ramat Aviv
Tel Aviv 69978
ISRAEL

Christophe Eckes
Dept. de Mathematiques
Universite Claude Bernard Lyon I
43, Bd. du 11 Novembre 1918
F-69622 Villeurbanne Cedex

Prof. Dr. Caroline Ehrhardt
Service d'histoire de l'education
Institut national de recherche pedagog.
45, rue d'Ulm
F-75230 Paris cedex 05

Prof. Dr. Moritz Epple
Goethe-Universität Frankfurt
Historisches Seminar
Wissenschaftsgeschichte
60629 Frankfurt am Main

Prof. Dr. Veronica Gavagna
Dipartimento di Matematica
Universita degli Studi di Salerno
I-84100 Salerno

Dr. Samuel Gessner
CIUHCT
Polo da Universidade de Lisboa (CHC-UL)
Faculdad de Ciencias de Univ. de Lisboa
Edificio C4 - Piso 3 - Gabinet 30
P-Lisboa 1749-016

Dr. Jeremy John Gray
Faculty of Mathematics & Computing
The Open University
Walton Hall
GB-Milton Keynes MK7 6AA

Prof. Dr. Emily Grosholz
Fondation Deutsch de la Meurthe
37 Blvd. Jourdan
F-75014 Paris

Dr. Ulf Hashagen
Forschungsinstitut Deutsches Museum
Museumsinsel 1
80538 München

Prof. Dr. Tinne Hoff Kjeldsen
IMFUFA, NSM
Roskilde University
Postbox 260
DK-4000 Roskilde

Prof. Dr. Jens Hoyrup
Roskilde Universitetscenter
Postbox 260
DK-4000 Roskilde

Eva Kaufholz-Soldat
Fachbereich Mathematik
Johannes Gutenberg Universität
Staudingerweg 9
55128 Mainz

Dr. Ralf Krömer
Universität Siegen
Fachbereich 6: Mathematik
Emmy-Noether-Campus
Walter-Flex-Str. 3
57068 Siegen

Jemma Lorenat
Dept. of Mathematics and Statistics
Simon Fraser University
Burnaby , B.C. V5A 1S6
CANADA

Prof. Dr. Erika Luciano
Dipartimento di Matematica
Universita degli Studi di Torino
Via Carlo Alberto, 10
I-10123 Torino

Prof. Dr. Antoni Malet
Departament d'Humanitats
Universitat Pompeu Fabra
Ramon Trias Fargas 25-27
E-08005 Barcelona

Prof. Dr. Colin McLarty
Department of Philosophy
Case Western Reserve University
Cleveland , OH 44106
USA

Dr. Marc Moyon
IUFM du Limousin
Universite de Limoges
209 Boulevard de Vanteaux
F-87036 Limoges

Prof. Dr. Felix Mühlhölzer
Philosophisches Seminar
Georg-August-Universität Göttingen
Humboldtallee 19
37073 Göttingen

Dr. habil. Philippe Nabonnand
Archives Henri Poincare
Universite Nancy 2
91 avenue de la Liberation
F-54001 Nancy Cedex

Prof. Samuel James Patterson
Mathematisches Institut
Georg-August-Universität Göttingen
Bunsenstr. 3-5
37073 Göttingen

Prof. Dr. Volker Peckhaus
Universität Paderborn
Institut für Humanwissenschaften:
Philosophie
Warburger Str. 100
33098 Paderborn

Prof. Dr. Jeanne Peiffer
Centre Alexandre Koyre
CNRS-EHESS-MNHN
27, rue Damesme
F-75013 Paris

Prof. Dr. Christine Proust
Equipe REHSEIS du Laboratoire SPHERE
Universite Denis Diderot - Paris 7- CNRS
Case 7093
5, rue Thomas Mann
F-75205 Paris cedex 13

Prof. Dr. Volker Remmert
Bergische Universität Wuppertal
Wissenschafts- und Technikgeschichte
Historisches Seminar, Fachbereich A
Gaußstr. 20
42119 Wuppertal

Prof. Dr. Clara Silvia Roero
Dipartimento di Matematica
Universita degli Studi di Torino
Via Carlo Alberto, 10
I-10123 Torino

Prof. Dr. Tatiana Roque
Instituto de Matematica
Universidade Federal do Rio de Janeiro
C.P. 68530
Rio de Janeiro 21945-970
BRAZIL

Prof. Dr. Norbert Schappacher
I.R.M.A.
Universite de Strasbourg
7, rue Rene Descartes
F-67084 Strasbourg Cedex

Prof. Dr. Dirk Schlimm
Department of Philosophy
McGill University
855 Sherbroke St.W.
Montreal QC H3A 2T7
CANADA

Dr. Martina Schneider
Geschichte der Mathematik und der
Naturwissenschaften, Institut f. Math.
Universität Mainz
Staudingerweg 9
55099 Mainz

Prof. Dr. Reinhard Siegmund-Schultze
University of Agder
Fakultet for teknologi og realfag
Gimlemoen 25 J
Serviceboks 422
N-4604 Kristiansand

Dr. Ivahn Smadja
Universite Paris 7 - CNRS
Laboratoire SPHERE UMR 7219
Equipe Rehseis, Case 7093
5 rue Thomas Mann
F-75205 Paris Cedex 13

Dr. Jacqueline Stedall
The Queen's College
GB-Oxford OX1 4AW

Prof. Dr. Dominique Tournes
Laboratoire d'informatique et de
mathematiques
Parc technologique universitaire
2, rue Joseph Wetzell
F-97490 Saint Clotilde

Laura Turner
Laboratoire de Mathematiques de Lens
Faculte des Sciences Jean Perrin
Universite d'Artois
Rue Jean Souvraz SP 18
F-62307 Lens Cedex