ICHM

Report on the ICHM Co-Sponsored 27th Novembertagung on the History of Mathematics

Sandbjerg Gods, Sønderborg, Denmark, November 24–26, 2016

The 2016 Novembertagung on the History of Mathematics had 27 participants; 25 doctoral students or post-doctoral researchers and two invited speakers, Prof. Henrik Kragh Sørensen (University of Copenhagen) and Prof. Bart Van Kerkhove (Free University of Brussels). The participants came from 18 different research institutions in ten countries (Argentina, Belgium, Czech Republic, Denmark, France, Germany, Italy, Netherlands, Scotland, Spain). 22 of the 25 junior researchers gave short talks, all on the history or philosophy of mathematics. The program and abstracts are provided below.

In addition to the generous funding from the ICHM, we received funding from Aarhus University Research Foundation and the Centre for Science Studies at Aarhus University. We used the grant from ICHM to subsidize the cost of accommodation for the junior researchers and the travel costs of nine of the junior researchers. Participation in the Novembertagung was free of charge; the three grants covered accommodation and all meals for the duration of the conference. Most participants paid for their own travel to and from Sandbjerg Gods.

The 27th Novembertagung was a success and achieved the goals described in the grant application. Colin J. Rittberg from the University of Brussels will be the main organizer of the 28th Novembertagung.

On behalf of Antonella Foligno (University of Urbino) and myself,

Line Andersen, Centre for Science Studies, Aarhus University

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Program and abstracts

Thursday, November 24

13:45-14:15 Registration

14:15-14:30 Opening remarks

Session 1

14:30-15:00 Colin Rittberg: The many faces of PMP: Some thoughts on methodological heterogeneity

15:00-15:30 Fenner Tanswell: Conceptual engineering for mathematical concepts

15:30-16:00 Davide Crippa: History of mathematics or mathematical practice: Shall we let both flowers bloom?

16:00-16:30 Coffee

Session 2

16:30-17:00 Isabelle Lémonon: Madame de Chastenay: Reading and writing practices in basic mathematics in France after the Révolution

17:00-17:30 Nicola Oswald: Revisiting Hurwitz's work on quaternions

17:30-18:00 Michael Chalmers: George Bouligand’s notion of causality in mathematical and physical theories

18:00-19:00 Dinner

19:00-20:00 Invited lecture: Henrik Kragh Sørensen: “Lost in Translation”: Gaining access to past mathematical practice

Friday, November 25

08:00-09:00 Breakfast

09:00-10:00 Invited lecture: Bart Van Kerkhove: Empirically informed philosophy of mathematics

10:00-10:30 Coffee

Session 3

10:30-11:00 Sven Delarivière: Characterizing mathematical understanding: A functional, contextual approach

11:00-11:30 Joachim Frans: Explanatory value and explanatory depth of mathematical proofs

11:30-12:00 Matias Saracho and Jose Gustavo Morales: How to articulate and organize mathematical results that show the way by which a thing has been discovered methodically: A case-study

12:00-13:00 Lunch

13:00-14:00 Walk (weather permitting)

Session 4

14:00-14:30 Michael Friedman: Shifts in the mathematical practice of folding

14:30-15:00 Lisa Rougetet: The role of games in mathematical practice: The case of combinatorial games

15:00-15:30 Flavio Baracco: Hermann Weyl's philosophy of mathematics and phenomenology

15:30-16:00 Martin Janβen: Hilbert and the Drosophila: Axiomatics meets genetics

16:00-16:30 Coffee

Session 5

16:30-17:00 Martin Muffato: Practical arithmetics written in French during the seventeenth century

17:00-17:30 Guillaume Loizelet: Mathematical astronomy in early eastern arabic texts

17:30-18:00 Gatien Ricotier: Some Bourbaki's choices for the Integration of 1952

19:00- Dinner

Saturday, November 26

08:00-09:00 Breakfast

Session 6

09:00-09:30 Antonella Foligno: Hypothesis on the notion of material point: Between physics and mathematics

09:30-10:00 María de Paz: When mathematicians meet physics: Riemann and Neumann on the principles of mechanics

10:00-10:30 Antonietta Demuro: Just on the limit between the practical and theoretical viewpoint as we like it: Kampé de Fériet's approach to fluid mechanics

10:30-11:00 Coffee

Session 7

11:00-11:30 David Waszek: The use of formulas in the early history of the calculus of operations (Leibniz, Johann Bernoulli, Lagrange, Laplace, Arbogast, Servois)

11:30-12:00 Line Andersen: Acceptable gaps in mathematical proofs

12:00-13:00 Lunch

13:00-14:00 Next year

Session 1

The many faces of PMP: Some thoughts on methodological heterogeneity

Colin Rittberg
Vrije Universiteit Brussel

Aspray and Kitcher (1988) described the philosophical landscape as consisting of two camps; in contemporary terms: mainstream philosophy of mathematics and the philosophy of mathematical practice (PMP). PMP is thus presented as a unity. 20 years later, Mancuso (2008) already saw two diverse traditions in PMP. And in a recent paper, Carter (201?) identifies three major strands in PMP (historical; agent-based; epistemological).

PMP is not a homogeneous whole. Philosophers working in the field differ in opinion on such points as which methods are acceptable, what types of questions to ask and whether some aspects of mainstream philosophy of mathematics need to be corrected. There is disagreement amongst the philosophers of mathematical practice.

In this talk I will highlight some controversies present in PMP. My aim is to make visible a methodological debate which demands our critical attention.

Conceptual engineering for mathematical concepts

Fenner Tanswell
University of St Andrews/University of Stirling

Conceptual engineering is the philosophical approach in which we analyse concepts for their effectiveness at achieving the purposes we have for them, and refine or replace them if we find them wanting. The term comes from Simon Blackburn's *Think* (1999), and has been taken up for projects ranging the full spectrum of philosophy, from Sally Haslanger's *ameliorative project* of replacing gender concepts to achieve social justice, to Kevin Scharp's *Replacing Truth* of recovering a consistent theory of truth by replacing it with dual concepts of `ascending' and `descending' truth.

In this talk, I will outline the aims and methods found in conceptual engineering, then consider several examples of ways to apply ideas from it to the case of mathematical concepts, showing how this will provide helpful distinctions, tools and insights.

The philosophy of mathematical practice is heavily influenced by its Lakatosian roots, where the dialectical philosophy of mathematics that Lakatos proposes has the development of mathematical concepts through practices of proving as a central component. As such, conceptual engineering is not exactly unfamiliar to us. However, the new work coming out of this philosophical turn has a number of fruitful applications in mathematics which I will explore in this talk. For instance, Haslanger's distinction between manifest concepts (those we take ourselves to be using) and operative concepts (those we are using in practice) tracks the mismatch between the formalistic idea that all correct proofs need to be formal or readily formalisable, versus the reality of mathematical practice where we find rigorous informal proofs as the norm.

To finish, I will suggest that conceptual engineering can provide us with a better framework for investigating the relationship between formal and informal mathematics, which is an urgent project for modern mathematics.

History of mathematics or mathematical practice: Shall we let both flowers bloom?

Davide Crippa
Czech Academy of Sciences

In this talk I would like to look at how we are doing what we are doing, and I will inquire about the very meaning of the expression "history and philosophy of mathematical *practice"*. What kind of activity it refers, is there any difference between this and the activity we call "history and philosophy of mathematics"?

The distinction between philosophy of mathematics (PM) and philosophy of mathematical practice (PMP) seems to me fairly well understood, at least judging from books, articles and societies devoted to the cause of PMP. Hence I would like to discuss the other relevant distinction, the one between history of mathematics and history of mathematical practice. Can we take it as denoting two different approaches to the history of mathematics?

I will argue that this may be the case by discussing the example of the quadrature of the circle and its impossibility proofs in early modern geometry. In his classical booklet *Squaring of the circle* (1913) the mathematician E. W. Hobson claims that the quadrature of the circle makes a perfect subject for history of mathematics, because we can reconstruct its evolution from the origins to its final settlement in XIXth century: "the whole history of the problem lies in a complete form before us". Moreover, our vantage point allows us to understand the difficulties of the past, the fundamental turning points and the main advances which shaped the mathematical knowledge as it is now (or as it was at the beginning of XXth century).

But the example of the circle-squaring problem suggests another way to look at its history, not from our vantage point but in its own terms. We may call this approach: "history of mathematical practice". For instance, we can discuss James Gregory's theorem about the impossibility of the circle-squaring problem (*Vera circuli et hyperbolae quadratura*, 1667) - an episode also mentioned by Hobson - by asking which were its conceptual implications for Gregory and for his contemporaries, how it was received by them, why was it eventually criticized, and what was its circulation and reception. I will show how this analysis complements the more classical analysis in Hobson-style: answering these questions will increase our understanding of those mathematical ideas, aims and presuppositions that go with certain changes in the mathematical knowledge. In the end, I think we can distinguish two styles or ways of doing history of mathematics, each having its own advantages and disadvantages.

Session 2

Madame de Chastenay: Reading and writing practices in basic mathematics in France after the Révolution

Isabelle Lémonon
EHESS – Centre Alexandre Koyré

Victorine de Chastenay (1771-1855) is well-known by the historians of the french *Empire, Consulat* and *Restauration's* periods through her *Memoirs,* a direct testimony of the french court life and the many policy mutations occurring after the *Révolution*. She is also recognized as an author through her french translation of *The mysteries of Udolfo* by Ann Radcliff in 1797, and several publications on history and literature. But this "heir of the Enlightenment" also invested sciences as astronomy, botany, physics, mathematics...She wrote thousands of manuscript pages through 50 years of sciences's studies.

This talk aims at describing her reading and writing practices in basic mathematics (mostly geometry of triangles) by analyzing her manuscripts. Some of those are reading records and others form personal geometry lessons. Through this study, we have a privileged insight into the ways some women (and men) could study mathematics from the *Révolution* to the *Restauration in France*, when excluded from the University or the *grandes écoles* (*Polytechnique, École centrale, École normale*...).

Revisiting Hurwitz's work on quaternions

Nicola Oswald
University of Wuppertal

Quaternions are generalizations of complex numbers. They have been discovered (or invented) by William Rowan Hamilton about 175 years ago. The arithmetic of quaternions is of special interest with respect to representations of positive integers as sums of squares, a classical theme in number theory. This observation is due to Rudolf Lipschitz (1886) and was further developed by Adolf Hurwitz (1896) who used quaternions for an algebraic proof of Lagrange's four squares theorem. It appears that Hurwitz succeeded where the analyst Lipschitz failed, namely the correct notion of "quaternion integers" within the set of all quaternions. In 1919, only a few months before his death, Hurwitz published a first textbook on this topic entitled "Vorlesungen über die Zahlentheorie der Quaternionen". This booklet is written in a remarkably modern style; in the preface Hurwitz stresses the proximity of quaternion arithmetic to algebraic number theory. His last mathematical diary contains a bunch of further material on quaternions written before and after the publication of his textbook. In my talk I want to outline some ideas of a project concerning a re-edition of Hurwitz's book on quaternions.

George Bouligand's concept of causality in mathematics and physical theories

Michael Chalmers
Paris-Sorbonne University/University of Lille

The French mathematician Georges Bouligand (1889-1979) made contributions to a diverse range of topics throughout pure mathematics and mathematical physics. In addition to his mathematical work, Bouligand published extensively his reflections on the philosophy of mathematics, his central concept, upon which he worked mainly in the 1930s, being that of causality in mathematical and physical theories - a term to which he lends his own broad and original meaning and which is intimately associated with the notion of a group in mathematics.

The idea of a *causal* demonstration

Given, for example, a simple proposition in geometry, there may be a number of ways of proving the statement. In some cases, methods could be employed that do not provide any information to the mathematician about why the statement is intuitively true. A proof which, by contrast, reveals the real reason *why* behind a mathematical fact is referred to by Bouligand as a causal demonstration[1]. To qualify as a causal demonstration, Bouligand requires in [1] that the proof be based on the minimum set of assumptions, as superfluous hypotheses would obscure the real reason for the result's validity. He notes that the causality of a demonstration depends to a large extent on the degree of generality with which we consider a given problem, stating that 'it is in searching for the broadest conditions under

[1] To illustrate the point through a counterexample, Bouligand considers the proof of Pythagoras' Theorem by constructing the altitude of a right-angle triangle and showing that the two resulting triangles are similar to the original. Properties of similarity then easily yield the result through simple algebra. Although the proof is straightforward, it does not satisfy the intuition as to why the result is true.

which a statement holds that we find its causal demonstration' [1]. He remains on guard against too rigid or restricted a definition of causality and sees the notion extending beyond a quality attributed to mathematic proofs to relations of interdependence in mathematical and physical theories [1].

Causality in mathematical theories

Bouligand's notion of causality is introduced in his book of 1936, *Structure des Theories, Problèmes Infinis*[2] [2] as a concept in the study of the structure of mathematical theories that 'favours an effort of coordination in a field of research where we are in possession mainly of information results' [2]. In [1], he sees the 'causal restoration' of different areas of mathematics as a general trend that has been prevalent since the work of Évariste Galois on the resolution of algebraic equations, while his central example is the development of differential geometry [2]. He considers the work of Bernhard Riemann on the foundations geometry and ultimately the characterisation of geometry by Klein and Lie in terms of properties invariant under groups of transformations as finally shedding light upon the 'problem of causality in the labyrinth of geometry' [1] and bringing back to light 'causal relations that were masked by the abuse of Cartesian methods' [4]. It is worth noting that Bouligand seems to equate the problem of 'restoring causality' with the challenge of formulating a mathematical theory in most general way by finding the minimum set of initial axioms [3]

In Bouligand's *Introduction à la Géométrie Infinitésimale Directe* [4] in which he presents, as Elie Cartan describes in his preface, '…a new differential geometry freed from the artificial restrictions…present for the sole purpose of convenience…', the notion of a *domain of causality* is introduced. Considering a geometry in the sense of Klein and Lie, he considers the group of transformations in question as a 'domain of causality, given that the causes within the domain determine the effects within the domain'. In [2], Bouligand formalises his reflections by defining a 'domain of causality', or domain of invariance, as the set of ways in which a mathematical proposition, or system of propositions, can be modified while still yielding a true statement. Bouligand regards this set as a mathematical group, as it is closed under the composition of modifications and also satisfies the required condition for inverses. As an example, in Klein's characterisation of geometry, the group of transformations associated with a given geometry is a domain of causality of the theorems of that geometry. To this notion, he adds the idea of the 'stability' of a statement P regarded as a function of the hypotheses H. P(H) is said to be stable if the function P(H) is continuous with respect to modifications of the hypotheses (a more detailed explanation of what is meant by 'continuity' is given in [2]).

The study of groups in the context of logic, exemplified by the ideas of Bouligand explained above, represents an area of interest in the philosophy of mathematics in 1935 when, at the *congrès international de philosophie des sciences*[3], there was a subsection of the philosophy of mathematics section entitled *logique et théorie des groupes*, in which Bouligand presented the above ideas [7]. Bouligand's notion of stability is further studied and presented at the same conference by Jean-Louis Destouches, whose work was started under Bouligand's direct inspiration. In [6] the explanation given for the interest in this new area of logic is due to a desire of mathematicians, physicists and philosophers alike to understand the reasons for the influential role played by the theory of groups in theories in physics (for example the theories of relativity).

Causality in physical theories

[2] This book introduces Bouligand's own ideas on the structure of mathematical theories and suggests how these considerations can be relevant in addressing fundamental issues in the study of infinite sets.

[3] This conference, held at the Sorbonne, was one of a series of two international congresses on philosophy, the second being held in 1937. Both put major emphasis on the subject of logic and attracted many of Europe's leading researchers in mathematical logic [6].

Bouligand's key example of the application of his mathematical causality to physical theories is the theory of relativity. Bouligand states in [1] that 'the close connection between group and causality seems to have guided the current of ideas from which the mechanics of special relativity emerged'. He notes that by subjecting the equations of dynamics and the equations of electromagnetism to the same geometry, characterised by a certain group of transformations (corresponding to a domain of causality), it became possible in the theory of special relativity to put together these two separate areas of physics into 'one logical synthesis' [3]. While the 'fundamental role of groups in the restoration of causality...for research in mathematical physics' [3] is emphasised, Bouligand nevertheless does not interpret the application of such mathematical theories as necessarily giving a correct and complete picture of physical reality: 'to try to adapt to reality certain schemes is no doubt all we will be able to do. And the most simple schemes that arise, from this point of view, are connected to certain groups'.

We also note regarding causality in physical theories that, according to Bouligand in [1], the first instance of the use of the word 'causality' by a mathematician was in *Les Axiomes de la Mécanique* by Paul Painlevé [5], where the principal of causality is defined as follows in the context of classical mechanics [5] 'While the same conditions are realised in two different places and at two different times, the same phenomena will occur, transported only in time and space...' Bouligand sees this definition as a 'direct extension of the installation of translations and similarities at the basis of [Euclidean] geometry', thereby connecting Painlevé's causality with his own concept in mathematical theories, based on the idea of a domain of causality (domain of invariance).

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[5] Painlevé, Paul. *Les Axiomes de la Mécanique, examen critique.* Gauthier-Villars, 1922.

[6] Guillaume Marcel, « La logique mathématique en France entre les deux guerres mondiales: Quelques repères », *Revue d'histoire des sciences* 1/2009 (Volume 62), pp. 177-219

[7] Actes du congrès international de philosophie des sciences, vol. VI : Philosophie des mathématiques (Paris : Hermann, 1936)

Invited lectures

"Lost in Translation": Gaining access to past mathematical practice

Henrik Kragh Sørensen
University of Copenhagen

As historians and philosophers of mathematics, we are interested in gaining access to "mathematical practice". However, as this notion is highly elusive, we are restricted to indirect access which presupposes various forms of translation and interpretation.

In this talk I will address this complicated historiographical question through discussions of my experiences in accessing – and making accessible – the mathematics of the Norwegian Niels Henrik

Abel (1802-1829).

Empirically informed philosophy of mathematics

Bart Van Kerkhove

Vrije Universiteit Brussel

Löwe (2016) has recently argued that the research domain commonly (but a bit misleadingly) identified as the philosophy of mathematical practice can be considered as lying at the intersection of two research communities studying the same subject with different means and intended questions. On the one hand, philosophers of mathematics having (to an extent) taken the naturalistic turn, trading in traditional idealistic/abstract pictures for empirically informed accounts governing the actual mathematical craft. On the other hand, students of mathematics from a variety of disciplines such as cognitive or social sciences taking an interest in philosophical issues. Larvor (2016) has correspondingly characterized this field as "a loose [sic] association of philosophers, historians of mathematics, psychologists and researchers in other human sciences investigating mathematics and mathematicians" (p.2), while its representative body APMP through its website http://www.philmathpractice.org/about/ suggests "using the label philosophy of mathematical practice as a general term for this gamut of approaches, open to interdisciplinary work", but nevertheless with a view on forming "a coherent community".

A number of tensions arguably at the basis of this somewhat awkward situation will be explored. To begin, disciplinary borders still hamper the development of the kind of intense collaboration that seems to suggest itself through the above picture. In particular, either the second group of scholars remains relatively small to date, or the philosophical community does not appear to be able to reach out for them yet. (We'll highlight some examples.) Nevertheless, this is of the essence, as philosophers are simply unable to master all potentially useful auxiliary disciplines enough to find the most salient pieces of research, let alone conduct the relevant empirical work themselves. Further complicating this extrinsic disciplinary issue, there is the intrinsic methodological one of how best to proceed in order to produce the epistemically most effective empirical research. Experimental philosophy seems to be particularly in vogue nowadays, however as is the case with old school armchair introspection is likely to produce a fairly one-sided picture of the knowledge phenomena studied. Hence a case for a variety of 'triangulated' contributions in order to strengthen our grasp.

References

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B. Löwe, "Philosophy or Not? The Study of Cultures and Practices of Mathematics", in: S. Ju et al. (eds.), Cultures of Mathematics and Logic, Birkhäuser, 2016, pp.23–42.

Session 3

Characterizing mathematical understanding: A functional, contextual approach

Sven Delarivière
Vrije Universiteit Brussel

Mathematical understanding as a philosophical notion has gained some attention, but remains largely unexplored. By means of this presentation, I wish to kindle that exploration by giving a functional, contextual reading of the notion. To begin, I outline a functional framework for approaching understanding as (i) a property, (ii) possessed by a subject, (iii) concerning an object, and analyse each component functionally. The functional reading equates understanding with the possession of abilities (thereby sidelining the feeling of understanding so distrusted about the notion). Abilities are here taken as the actual or (limited) counterfactual performance of mathematically relevant activities. I reveal some benefits of this approach over others and address several worries concerning abilities without “real” understanding by appealing to theoretical misconceptions, agent misattribution or the notion of kludges. To continue, I couple each branch in my framework to parameters that should be able to express the quality of understanding. Each of these parameters, I further argue, needs to be seen under a contextual light which directs its spotlight to what it deems appropriate and narrows its focus to what threshold value it wills. Taking into account these factors would lead us to consider different kinds and qualities of understanding, making it possible to specify what is meant with the attribution of understanding in a particular context. Furthermore, the parameters introduced together with their contextual specifications may clear up some issues regarding epistemic luck for understanding.

Explanatory value and explanatory depth of mathematical proofs

Joachim Frans
Vrije Universiteit Brussel

In this talk I address the notion of an explanatory proof. The underlying idea of this is that not all mathematical activity is driven by justificatory aim. While all proofs show *that* a theorem is true, some proofs go further and also show *why* a theorem is true.

I will focus on the specific issue of multiple proofs that prove the same theorem. The goal of philosophical accounts of mathematical explanation is to be able to successfully distinguish explanatory from non-explanatory proofs. I will defend an epistemic and contextual approach to this problem. I take explanation to be essentially linked with understanding, locating explanation as that what grants understanding why the theorem is true for a certain agent. If we approach mathematical explanation this way, contextual elements play a crucial role. In many cases mathematical proofs will cast light on different aspects of the mathematical theorem. In order to speak of an explanation, there has to be a match between a proof and the knowledge, skills and epistemic interest of the agent. Historical examples, such as the diverging views on the role of visual information (e.g. Weierstrass and Von Koch), can convey this view that what is considered as explanatory is dependent on the context. In such cases, I will speak of the explanatory value of a mathematical proof.

We can go beyond such context-dependent claims about the explanatory values of different mathematical proofs of the same theorem, by asking the question whether these proofs differ in explanatory depth. By explanatory depth, I mean a measure in terms of which explanations can be ranked according to a *degree* of explanatory value. In particular, I will defend that my approach is capable of dealing with some aspects of the challenge to develop an account of explanatory depth. In order to do this, I will focus on the epistemic aspects of my approach and defend there are different types of understanding related to understanding why the theorem is true. The depth of an explanation can consequently be related to granting more types of understanding. Moreover, some types of understanding will allow to distinguish between degrees of depth. However, this will not work for all types of understanding. Consequently, I will also discuss the limits of this account, where it seems impossible to make a clear cut between the depth of several explanations.

How to articulate and organize mathematical results that show the way by which a thing has been discovered methodically: A case-study

Matias Saracho and Jose Gustavo Morales
National University of Cordoba

According to contemporary readers, when it comes to organizing the presentation of research results, western cultures of mathematics ever since Euclid have been setting the emphasis on axiomatic-deductive models. The idea crystallizes in the modern axiomatic conception of rigor which requires that only those aspects concerning the r e a s o n i n g structure underlying the validity of results ought to be made explicit in textbook presentations. The specificity of problem solving activities would then be accordingly neglected, with a view to attaining higher levels of abstraction required by systematic theory building. From a historical perspective, however, some texts composed by leading mathematicians bring to light a different way of articulating and organizing results which makes explicit some of the most fundamental cognitive strategies that come into play, as well as the corresponding working tools and procedures for problem-solving.

We will select our case-study from Leibniz's early mathematical research on quadrature problems (1675-1676). Our interest in the topic was originally inspired by a consideration of Descartes' defense of his style of presentation of geometrical results (*Géométrie*, 1637) which valued above all the cognitive aspects involved thus motivating his requirement that the articulation of mathematical results as presented in the text ought to show the way by which "the thing has been discovered methodologically". This view opened the way to a different style of textbook presentation in mathematics, a tradition which for many scholars started with 17th century mathematical analysis. Such is the case with texts organized around problems and algorithms, and procedures for problem solving as displayed in Leibniz's *Quadrature Aritmétique du Cercle, de l'Ellipse et de l'Hyperbole* (DQA), a text composed by Leibniz towards the end of his mathematical studies in Paris (1672-1676). As part of our conceptual background we rely upon research by Chemla (2003) and Grosholz (2016) who note that some research traditions organize results in terms of paradigmatic problems in order to exhibit the generalization of procedures. Moreover, the view that the generalization of procedures around paradigmatic problems allows for "a deeper and broader understanding" of the methodological aspects concerning mathematical practice offers the key to establishing an interesting connection between historical case-studies in mathematics and the teaching and understanding of mathematical results.

In our historical case-study will focus on Leibniz's exposition in DQA, a text that aims to provide a general method to solve quadrature problems for curvilinear figures where the results obtained are organized around two fundamental problems, the squaring of the circle and the hyperbola. The intermediary reasoning steps involved in the resolution exhibit a procedure that aims at obtaining precise determination of the area of the circle and the hyperbola, at the same time that it indicates how the procedure might be extended to a new family of problems, the squaring of "transcendental curves". In so doing, Leibniz displays the reasons why the results presented are correct but also shows the way procedures can be extended on the basis of "paradigmatic problems" alreadysolved.

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Session 4

Shifts in the mathematical practice of folding

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Folding a piece of paper is usually associated to Origami, but since the 90s of the 20th century this activity has gained considerable attention from mathematicians, inspired by the determination of the Huzita-Justin Axioms in 1989 for folding-based-geometry. However, this was not the first time in history of science that folding served as a mathematical practice. In my paper I would like to survey two transformations in the mathematical practices concerning the mathematization of folding, from the end of the 19th century to the end of the 20th century.

During the end of the 19th century one may note an increasing interest in the mathematical occupation with folding. This is to be seen especially with the manuscript written in 1893 by Tandalam Sundara Row in India: *Geometrical exercises in paper folding*. Row's book deals with an attempt to construct Euclidean plane geometry using only a *physical* folding of paper – i.e. using neither axioms nor tools from analytic geometry. Row's manuscript was recommended by Felix Klein in 1895, and this was due to Klein's recommendation, but also due to the kindergarten movement of Fröbel, that the mathematics of paper folding became popularized.

A shift in the way folding was practiced in mathematics might be seen in the celebrated discovery of Margharita P. Beloch (1879–1976) in the 1930s, extending Row's work. Beloch formalized a fold which allows, when possible, to construct *physically* by paper folding the common tangents to two parabolas. As a consequence, Beloch's work showed that segments whose length are cubic roots can be constructe by paper folding – a construction which was proved impossible, using only straightedge and a compass, in 1837 by Pierre Wanzel. Hence it was shown that the fold could surpass the classical constructions of plane geometry. It is also here that one may also locate the beginning of the emphasis on computability via folding. This discovery may be seen as a forerunner for yet another transformation, taking place at the end of the 20th century.

The second shift occurs with axiomatization of this geometry. In 1989 the first mathematical conference on origami and folding was held, called "Origami Science and Technology", which brought not only the above mentioned axiomatization but also Beloch's discovery back to the public sphere, as well as the 1980 discovery of Hisashi Abe, proving that a trisection of an angle is also possible via folding. With this axiomatization, an emphasis was put not only on the axioms needed for this geometry, but it was shifted to another practice: Computational origami and algorithmic foldability. Different questions were asked within this practice: which crease patterns can be folded into an origami shape that use exactly given creases, or, which crease patterns derive from actual origami? This practice relies not only on computer programs but also on the theory stemming from the discipline of computer science, ranging from computability questions with specific algorithms to the question of NP-hardness regarding certain folding problems and procedures; it also points to other relations between visuality and materiality within the practice of mathematical paper folding.

Therefore, I intend to examine these two shifts of the mathematical practice with respect to folding: these reflect not only the different mathematical cultures, but also the major epistemological changes that are to be seen within geometry.

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The role of games in mathematical practice: The case of combinatorial games

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Combinatorial games are no chance finite games, with perfect information, played alternately by two players (Chess, Checkers, Tic-Tac-Toe or Dots-and-Boxes are examples of such games, among many others). They present a winning strategy (a sequence of optimal moves that leads to a win), which can now be formalized and expressed using graph theory and set theory notions. Combinatorial games fall within two different kinds of a mathematical practice: first, they are played by “amateur” players who seek out a strategy to win the game, yet without knowing that they actually deal with a mathematical process; secondly, they are “pretexts”, “opportunities” to develop and exhibit some mathematical results. This presentation fits into the second case and will focus on the interest mathematicians have developed for combinatorial games and their analyses.

If the term “combinatorial game theory” appeared in the 1970s, combinatorial games started to be really considered as mathematical objects in the 1950s when an article written by Cedric Smith and Richard Guy was published in 1955. This article gave a classification of octal games (specific class of combinatorial games) together with a theorem rediscovered by the two authors, which is now called the “Sprague-Grundy theorem”. The latter is a fundamental result for combinatorial game theory and was proved independently by Sprague in 1935 and Grundy in 1939. It has made possible to consider not only a particular game and its proper analyze, but also a whole class of games, the analyze of which comes out of one particular game: Nim. This presentation will draw attention to this change of the status of a game and its mathematical resolution by taking a closer look at Emanuel Lasker (1931), Roland Sprague (1935) and Patrick Grundy's (1939) works.

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Hermann Weyl's philosophy of mathematics and phenomenology

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Since Kant published his *Critique of Pure Reason* the problematic relation between intuitive and conceptual knowledge in mathematical reasoning has been at the core of many debates within mathematical community. According to Kant intuitions play a necessary role and they are strongly interwoven with the conceptual development of mathematics. At the beginning of the twentieth century many mathematicians were working for a proper foundation of mathematics. Many of them were inspired by Kant's philosophy (Brouwer, Hilbert, Poincaré, ...), but only a few mathematicians considered Husserl's phenomenology as a prominent starting point. Husserl claimed that his philosophy was successful in clarifying Kantian investigations on human reasoning, even if a completely different theoretical framework were to be addressed. Hermann Weyl was influenced by phenomenology since he was a graduate student, and in *The Continuum (1918)* he stated clearly that his approach was inspired by Husserl's philosophy. This work deals with the conceptual problem posed by the continuum in analysis and it aims to contribute to "critical epistemology's investigation into the relations between what is immediately (intuitively) given and the formal (mathematical) concepts through which we seek to construct the given in geometry and physics". In the present talk I will investigate this work shedding light on its connections with phenomenology. I will further point out similarities and differences with Brouwer's intuitionism and Hilbert's formalism. In this respect, I aim to clarify some aspects of Weyl's philosophy of mathematics in relations with other foundational programs. Although few problems have to be addressed, I argue that Weyl's investigations on the continuum shows how fruitful can be a phenomenological perspective with regard to foundational issues in mathematics.

Hilbert and the Drosophila: Axiomatics meets genetics

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In 1930 David Hilbert (1862-1943) was awarded honour citizenship of his hometown Königsberg. At the 91st meeting of German natural scientists and physicians in Ko ̈nigsberg, Hilbert received the honour and provided the celebrated address “Naturerkennen und Logik” (“The Knowledge of Nature”). In this address, Hilbert (1930/1996) emphasized the advantages of the axiomatic method by an application to the current biological research:

The oldest and best known example of the axiomatic method is Euclid’s geometry. But I should prefer to illustrate the axiomatic method quite briefly with a striking example from modern biology. Drosophila is a small fly, but our interest in it is great; it has been the object of the most careful, and the most successful breeding experiments. This fly is usually grey, red- eyed, unspeckled, round-winged, and long-winged. But there are also flies with deviant special characteristics: they are yellow rather than grey, they are white-eyed rather than red-eyed, etc. These five special characteristics are often coupled. That is, when a fly is yellow, it is also white-eyed and speckled, split-winged and stump-winged. And when it is stump-winged the it is also yellow and white-eyed, and so on. But now with suitable cross- breeding later generations exhibit a smaller number of deviations from these commonly occurring couplings – indeed, the percentage is a definite constant. The numbers which one thus finds experimentally tally with the linear Euclidean axioms of congruence and with the axioms for the geometrical concept 'between', and so the laws of heredity result as an application of the axioms of linear congruence, that is, of the marking-off of intervals; so simple and exact, and at the same time so wonderful, that probably no fantasy, no matter how bold, could have devised it. (Hilbert, 1930/1996, 1158f)

The presentation's aim is the historical reconstruction of the given quotation. Historical reconstruction, as it is used here, is understood as a scientific process on two levels. On the first level, an identification and analysis of three fields of investigation is presented; more specifically, the fields are the axiomatic method, linear geometry and Drosophila's heredity. The key question for this analysis is the following: What is the axiomatic method, linear geometry, and Drosophila's heredity, respectively, from Hilbert's point of view? Based on the analysis' results, a synthesis of the three parts follows on the second level. In this context, the historical reconstruction of Hilbert's axiomatic example is intended.

The axiomatic method – as it is understood in the presentation – is an essential element of the mathematical practice since Euclid's days. With his exemplary axiomatization of the laws of Drosophila's heredity, Hilbert pleaded – and that is the presentation's conclusion – for a wider interpretation of the axiomatic method. Following this idea axiomatic thinking is not limited to mathematics or well-established fields of physics.

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Session 5

Practical arithmetics written in French during the seventeenth century

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Practical arithmetics have spreaded in Europe since the fourteenth century, as an answer to the development of the trade between countries.

In France, during the seventeenth century, the number of published practical arithmetics (texts dealing with "How to calculate ?" and "How to realise this or that operation ?") strongly rises. We have indexed two texts published in French language during the sixteenth century and only ten for the eighteenth century, but ninety-one during the seventeenth century.

Considering that, I will give an overview of the treatises available. Where do they come from, and what are their characteristics: what (mathematical) approach do they folllow, how are they laid out, etc...

Besides, that brings up some questions about the mathematical context: who are the different actors (authors and readers) ? And what is the situation of mathematical teaching in France ?

In this talk I will present the data collected and analysed so far, and some of the interest points that they reveal.

Mathematical astronomy in early eastern arabic texts

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Tremendous development of mathematical astronomy occured in the eastern arabic speaking area in between the VIIIth and the XIVth centuries.

The texts on this field are refered to (a posteriori in most cases) as hay'a texts, an arabic term without greek nor english counterpart (the usual translation "hay'a" is "configuration").

A genuine specificity of those texts is the balance between a purely theoretical point of view (basically as it can be found in Ptolemy's Almagest) and a more physical point of view (as in Ptolemy's Planetary Hypotheses), a balance which is quite distinct from what can be found in western texts (both arabic and latin) and thus is an important issue to investigate.

Recourse to textual evidences, as the only reliable markers of those mathematicians' practice, appears to be the most promising way to tackle this problem.

Some Bourbaki's choices for the Integration of 1952

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Bourbaki's Book VI: Integration (1952) is one of the most criticized part of the Éléments de mathématique. One of the main reasons for this is the choice to build all the theory on Radon measures on locally compact spaces instead of working with an "abstract" measure theory.

The talk will try to elucidate a few reasons of Bourbaki's choice, going through earlier versions of Book VI, such as the “Diplodocus” which was based on an "abstract" measure theory. This evolution of the theory since Lebesgue, the mathematical interests of the members and/or the "whole” Bourbaki group, "the architecture of mathematics" according to Bourbaki, and social interactions are inverstigated. With the help of Bourbaki's Archives, the evolution of this topic can be followed from the first "proto-Bourbaki meeting" of 1934 to the published book of 1952.

Session 6

Hypothesis on the notion of material point: Between physics and mathematics

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My talk aims to analyse the concept of the *material point* by promoting a hypothesis on its possible development. The idea involves considering the *material point* as a part of a phenomenon that Enrico Giusti called the *objectification of procedure*: his theory states that objects often come on the scene simply as investigative tools – demonstration tools or a parts of these – and, in a later stage, they become more explicit or common elements used for solving problems. Only after these stages, can they become new objects of study. Therefore, my problem is not to establish who was able to identify them, rather to find an answer to the question *when, and in which demonstrations, does the idea of the material point came out*.

First, the topic of my essay should be defined. The *material point* is a body which has dimensions that can be considered negligible compared to the problem being examined. Moreover, all the forces acting on it can have the same point of application; therefore, the *material point*, in this sense, is a useful mathematical abstraction in the analysis of some physical phenomena.

In the first part of the talk I will deal with the leading hypothesis that the abstractions behind the concept of the *material point* were already present in Archimedes' demonstrations relating to centres of gravity. In order to validate my hypothesis, I will show the proposition numer XIV of the *Methos* in which appears the most important refining of the concept of centre of weight. I would suggest that in this proposition, Archimedes is paving the way for the introduction of a series of mathematical abstractions that will create, *gradually*, the modern concept of the *material point*; these abstractions require a good degree of methodological opportunism, which Archimedes mixed with mathematical precision.

On a later stage I will focus on the Renaissance period, in which took place the development of the so-called *scientia de ponderibus* – which studies static problems and the notion of centre of gravity. It was in this context in the XIV century that *The Equilibrium controversy* broke out and began to spread. To afford my hypothesis I will analyse Giudobaldo dal Monte's *Liber Mechanicorum, and in particular the* propositions *II* and *III*. In this treatise there is a good example of the context in which the abstraction of the *material point* takes a new step towards its process of *objectification*. From this analysis, it is possible to prove that del Monte used, very confidently an abstract theory to solve a practical problem, laying the groundwork for the development of the concept of the *material point*. I am suggesting that, at the base of the *Liber Mechanicorum* there is the following theoretical procedure: *the bodies placed in equilibrium at the extremities of the scale's arms are considered bodies devoid of size, as if they were just points submitting to any possible movement*.

However, the concept of the *material point* is not explicit yet; we must wait until the XVI and the XVII centuries when Isaac Newton's and Leonhard Euler's works appeared. They both wanted to create – thanks to axioms, definitions and logical deductions – a rational mechanic science, used to show the unquestionable character of medieval mechanics.

Consequently through these stages, the *material point* became an analytical body created from the abstraction of some aspects of material bodies. It becomes an *elementary* tool, useful to describe the dynamical and kinematical behaviour of a wide range of bodies. The validity of the methods used in equilibrium's study gave rise to a new mathematical subject, after a long process of objectification, which started with Archimedes' mechanics and ended with Newton and Euler after having been rediscovered in the Renaissance.

When mathematicians meet physics: Riemann and Neumann on the principles of mechanics

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During the 19th century the foundations of mechanics where widely discussed. This led to the changing of a discipline that, at the beginning of the century was included among the 'mathematical disciplines' as rational mechanics, and, by the end of the century, was considered much closer to the physical (natural) sciences. Several reasons have been argued for this transformation, such as the development of new branches of mathematics (e.g. non-Euclidean geometries), the emergence of the concept of energy and of the new electromagnetic theory, the rising of a phenomenical or more nature-linked conception of natural science and, lately (Pulte 2009), the coming out of a new mathematical view, in which pure mathematics should remain isolated from whatever is not mathematical, that is, the more nature-linked branches of mathematics, should leave the domain of pure mathematics and be included in a different scientific domain.

It is interesting to study this discipline change by researching on the introduction of new epistemological categories to describe the role played by the principles of mechanics, especially, when these categories are introduced by working mathematicians. This is precisely the case of Bernhard Riemann and Carl Neumann, which characterized these principles as ‘hypotheses’ in 1853 and 1870, respectively. The aim of this talk is to analyze this ‘hypothetical conception’ of the principles of mechanics by deepening into the mathematical (and scientific) views of these authors. This analysis could bring into play new elements for the understanding of the transformation of mechanics and could be also fruitful to relate the introduction of these epistemological categories to the mathematical conceptions of their authors, in order to clarify the role of assumptions (hypothesis) in the constitution of physical-mathematical theories.

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Just on the limit between the practical and theoretical viewpoint as we like it: Kampé de Fériet's approach to fluid mechanics

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The turbulence theory is a part of the fluid mechanics in which an agreement between the theory and laboratory experiments is very important. Since the early 30s, frequent practical applications, as wind tunnels and hot-wire anemometers, allow a significant progress in the theoretical models and, consequently, an advancement in the turbulence research. Joseph Kampé de Fériet (1893-1982), as mathematician and director of the Institut of Fluid Mechanics in Lille (IMFL), has successfully attempted to go beyond his theoretical background in order to take part in the experimental studies of his laboratory team.

The discussion focuses on his research during the two decades 30s-40s. Let us highlight two features. On the one hand, he uses a probabilistic approach to provide a contribution to the mathematical formalism of the statistical turbulence theory. On the other hand, he performs experimental researches about atmospheric turbulence, for example aerological expeditions and laboratory tests. In this context, I present how his approach brings some important results into the problem of turbulence in the international framework, by using similar methods of the Great Britain, United States and Germany. That ensures him a scientific collaboration with them, particulary with Von Karman's scientific community.

Session 7

The use of formulas in the early history of the calculus of operations (Leibniz, Johann Bernoulli, Lagrange, Laplace, Arbogast, Servois)

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According to the usual contemporary definition, a mathematical proof is at bottom a sequence of sentences, each of which follows from the preceding ones or is an axiom or assumption. In some mathematical practices, however, proofs contain elements other than sentences, and so do not easily fit this definition. The proofs in Euclid's /Elements/, for instance, often make essential use of geometrical diagrams (see Manders, 2008). Can we still consider them proofs? How did Euclid use the diagrams to ensure reliable reasoning? Motivated by epistemological questions of this kind, the use of diagrams in mathematics has been an important focus of study in recent years, particularly for scholars associated with the philosophy of mathematical practice (see for instance the papers in Mumma et al., 2012).

My goal in this presentation would be to argue that the use of symbolic formulas in proofs can raise some of the same issues as the use of diagrams: formulas, or symbolic expressions more generally, cannot unproblematically be treated like sentences. Indeed, we will see that formulas are sometimes used not so much in order to assert specific claims ("such-and-such a complex relation holds between quantities x,y,z"), but rather as displays from which various pieces of information may be extracted: for instance, I may derive and write down a complex formula linking x and y only in order to observe that x is given by a second-degree polynomial in y. In other words, formulas are not just asserted; they are inspected, much like diagrams are.

To substantiate this claims, I will examine the use of formulas in several episodes of the early history of the calculus of operations, in the late seventeenth and eighteenth centuries. By calculus of operations, I refer to the manipulation of operators, for instance the "d" of differentiation, as algebraic quantities whose powers correspond to iterated applications of the operator. The earliest incarnation of this idea is Leibniz's "analogy between powers and differences", developed in 1695 in correspondence with Johann Bernoulli (see Leibniz, 1849, volume III/1). Leibniz and Bernoulli's idea will be taken up again by Lagrange (1772); various authors will then try, in the last decades of the eighteenth century, to put these methods on a solid footing, starting with Laplace (1773).

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Acceptable gaps in mathematical proofs

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According to the traditional account of when a mathematician is justified in believing that a mathematical proposition *p* is true, she must have gone through the proof of *p*, step by step. Don Fallis (2003) argues that this account fails as a descriptive account of when she is justified *in a manner that is accepted by the mathematical community* in believing that *p*. A mathematician can be justified, in this manner, in believing that *p* when there are gaps in the proof that neither she nor anybody else has traversed. He argues for this by giving examples of proofs with universally untraversed gaps that are accepted by the mathematical community. In this talk, I expand on Fallis' account on the basis of interviews with seven mathematicians about their refereeing practices. The interviews indicate that the gaps that are acceptable in published proofs are those that can rather easily be traversed or be seen to be traversable by a large majority of the relevant experts. Referees are guided by this criterion and must often ask authors to provide more details. This puts a limit on the amount and character of universally untraversed gaps in proofs.

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