P. C. Kjaergaard

Connections between history and philosophy of mathematics. Informed frameworks, spontaneous philosophy,and philosophical challenges to mathematics

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CONNECTIONS BETWEEN HISTORY AND PHILOSOPHY OF MATHEMATICS
Informed frameworks, spontaneous philosophy, and philosophical challenges to mathematics
(Introduction to 10th Novembertagung)

Peter C. Kjærgaard

1 Connections between the history and philosophy of mathematics

In this paper I will offer three different perspectives on the relationship between the history and philosophy of mathematics. They should by no means be seen as exhaustive. Other perspectives can quite possibly yield productive reflections about this relationship. Yet, this discussion and the examples I will give are viewed from the perspective of the historian of mathematics and present three rather different challenges where philosophical issues are integral to the historical context and hence has to be given serious attention from the historian. I have dubbed these perspectives "informed frameworks", "spontaneous philosophy", and "philosophical challenges to mathematics". They should all be seen in an historical context and do not form part of a current agenda in contemporary philosophy of mathematics. My aim is to demonstrate that there are certain philosophical issues arising in specific historical contexts that historians of mathematics have to consider, not to support or inform certain paradigms of philosophy of mathematics. Since the first two perspectives would be most familiar to historians and philosophers of mathematics, I have decided to put the emphasis on the third and final perspective, illustrating it with a more elaborated case-study.

First, however, I will start out by a preliminary definition of the three perspectives. By "informed frameworks" I refer to interpretational strategies more or less explicitly carrying a philosophical agenda. This kind of philosophy usually has a prescriptive goal for the understanding of mathematics. This perspective is not defined narrowly to what we would call genuine philosophy, but also reaches out to sociological and semiotic readings of mathematics. The point for this is fairly obvious. One of the trademarks of the philosophy following in the footsteps of logical positivism has been its prescriptive character. Although the fundamental tenets of logical empiricism such as the propositional nature of scientific reasoning have been widely criticized and for the most part abandoned by the philosophical community, the prescriptive ambition still plays an important role in contemporary philosophy. When philosophy of science from the 1970s got under attack from the sociological informed studies of science, the prescriptive element also seemed to survive. Many of those who endorsed the ideology of the new sociology of scientific knowledge saw themselves *replacing* philosophy of science. In their mind philosophy of science represented an out-dated view on science that was no longer needed. But this did not imply that the new alternative to understand science would renounce prescribing how this understanding should be informed. Taking over the role of philosophy of science also meant taking over the prescriptive role it had enjoyed. What is rigorously understood by philosophy of science — or philosophy of mathematics for that matter — does not capture how extra–disciplinary the prescriptive ambition is. The importance in this context is that it has been instrumental to inform historians of science and mathematics of methodological and interpretational frameworks. Hence, the notion "informed frameworks".

By "spontaneous philosophy" I refer to philosophical issues generated by mathematical studies themselves. As opposed to the "informed frameworks" the philosophical agenda is not the driving force of the investigations. Obviously more or less explicit philosophical agendas — usually identified as the ideology endorsed by the person or groups under study — can be of significance in certain respects. But these rarely take the form of *commanding* a general framework for understanding science or mathematics. The word "spontaneous" could be a bit misleading implying a sort of naive armchair philosophy. This is not my intention. Rather, I want to make a distinction between philosophy as a guiding principle and philosophy arising spontaneously out of serious mathematical research. In the latter case the philosophical ambitions are at most of a secondary nature. However, there are several examples demonstrating that the philosophical problems encountered in these contexts have had a major impact on philosophy of science in general and the philosophy of mathematics in particular. This tells us that this "spontaneous philosophy" is by no means trivial. Furthermore, the generated philosophical problems are in most cases so interconnected with the mathematical work that a separation of the two would be artificial. We have to take seriously the philosophical content of these discussions; also when writing the history of mathematics.

The notion of "philosophical challenges to mathematics" is probably the one which has received the least attention among historians and philosophers of mathematics. In a way it is related to the "informed frameworks" since it represents a philosophical agenda with a strong element of prescribing mathematicians how to understand their field. Yet, it differs with respect to the "informed frameworks" by representing historical cases. More specifically it concerns the *history* of philosophical challenges to mathematics. One could argue that this topic might be of interest to the historian of philosophy, but not to the historian of mathematics. Why should we care about what philosophers might think of this topic? Especially since they do not enjoy any prominence with respect to the development of mathematical tools, methods, theory, and so forth. In order to understand this we have to look at the context of these challenges and not least at how they were met by contemporary mathematicians. I will present such an example where a philosophical challenge caused a number of the Victorian scientific elite to invest far more time and energy by putting an extraordinary effort into refuting this challenge than if it had been of complete insignificance for them. By this example I will demonstrate that such challenges *mattered* for the mathematicians involved. The lesson is: when something matters to the historical actors, it should matter to the historian of mathematics regardless of *what* it is. As a consequence, when writing these specific chapters of the history of mathematics, there is no way escaping philosophical issues. However, no general conclusions about the importance of philosophy for mathematics can be drawn from this. The conclusions always depend on the specific historical context.

2 Informed frameworks

The idea lying behind the notion of "informed frameworks" is a very common one and hence rather familiar in philosophy of mathematics. The urge to prescribe how to interpret the history of mathematics has been prevalent in most twentieth–century philosophy of science and mathematics. This has been expressed in the efforts to extract a methodology from mathematics setting the standards for the understanding and further investigation of the nature of mathematics. Imre Lakatos' methodology of research programmes proves an apt example of how a certain form of rationality should serve as the guiding principle of a generalised methodology of science. Although critical towards the early positivist philosophy of science this ambition has been inherited by the naturalized philosophy of science promoted by PHILIP KITCHER among others. Although the idea has been abandoned that the development of science is best understood through the evaluation of its basic axioms and propositions, the overall ambition of extracting a specific methodology remains.

The same can be said of DAVID BLOOR's strong programme of the sociology of scientific knowledge. The assumption that mathematics is just another form of human knowledge and can be treated as such not only provides a strong interpretative framework, it also defines quite narrowly *how* the nature of mathematics should be interpreted. Another, but somewhat different example of how an "informed framework" guide historical studies is HERBERT MEHRTENS' distinction between modern and countermodern mathematicians during the early decades of the twentieth century. The moderns were those who accepted the formalist framework epitomized by Hilbert's programme. This represented in MEHRTENS' view a new form of seeing mathematics continued by twentieth century formalists as the BOURBAKI movement. MEHRTEN's assumption that it makes sense dividing mathematicians in moderns and countermoderns lends support from cultural, social and intellectual studies of modernity. It is nonetheless an interpretational strategy that asks certain questions and yield certain answers. Hence it can also be seen as an "informed framework" to interpret the history of mathematics.

3 Spontaneous philosophy

There are several examples of "spontaneous philosophy" in the discussion of foundations of logic and mathematics during the early decades of the twentieth century. This is the case for BERTRAND RUSSELL's logicist programme, DAVID HILBERT's formalistic programme, L. E. J. BROUWER's and HERMANN WEYL's intuitionistic interpretations, KURT GÖDEL's incompleteness theorems, and so forth. Those examples have all been studied in great detail. To illustrate my point I have chosen an example that has not received quite the same attention from historians of mathematics.

LUDWIG WITTGENSTEIN has never earned quite the fame in the philosophy of mathematics as he has in the philosophy of language.

There are many reasons for that. A result is that many of the topics on which he ventured in the philosophy of mathematics, have recieved a rather superficial attention. One of these topics is his criticism of Hilbert's metamathematical programme and Gödel's theorems. This criticism grew out of a serious work within the framework of the logicist programme defined by Frege and Russell. The result of Wittgenstein's work with Russell in Cambridge from 1912–14 found its ultimate expression in his highly influential *Tractatus Logico-Philosophicus* published in 1921. Although this was Wittgenstein's last decidedly logical work, the criticism he had developed against meta-theoretical constructions such as Russell's theory of types survived in his philosophy of mathematics in the late 1920s and through the 1930s. The case of Wittgenstein fit the notion of "spontaneous philosophy" because his philosophical reflections were instigated by trying to solve mathematical problems requiring more than just mathematics. Russell realized this, and although he did not follow Wittgenstein all the way, he welcomed the argument that the fundamental problems of logic and mathematics had become philosophical at heart.

At the core of Wittgenstein's arguments was the idea that when working with formal systems it was not all about producing *solutions* to the unavoidable foundational questions these issues raised. Instead it was about *showing* that they were really not problems at all if seen in the right light. This was the argument Wittgenstein used against Russell's theory of types in the *Tractatus*. The same argument was later repeated both against Hilbert's metamathematics and Gödel's acceptance of this, according to Wittgenstein, wrong–headed way of framing foundational issues.

The theory of types was Russell's attempt to eliminate contradictions. The idea was to introduce different formal levels which, by definition, could not refer to themselves. The classical self–referential paradoxes could thereby be solved, according to Russell, since they were only apparently self–including. Totalities introduced in these paradoxes should not be explained by reference to themselves, but instead by referring to another level; a metalevel.

To Wittgenstein it was an unacceptable solution and worse, even an unnecessary solution. If the system was consistent, then it had no use for such a construction to save it. If it was not consistent, then no metatheory would be able to save the system anyway. In *The Big Typescript* written around 1930–31 Wittgenstein wrote

> *Through Russell, and especially through Whitehead, a cer-*

> *tain pseudo–exactness has appeared in philosophy which is the enemy of the true exactness. The reason for this is the misconception that a calculation can be a metamathematical foundation for mathematics.* [22, p. 540]

Likewise in an earlier manuscript he had stated: "There is no metalogic. Just as the word 'understanding' in the expression 'to understand a sentence' is not metalogical." [21, p. 79] In other words there was no metalanguage that was able to *explain* neither logic nor mathematics, or any other formal system for that matter. What was needed was *understanding*; and that could not be acquired through the construction of a metalevel. What had to be done instead, according to WITTGENSTEIN, was a work on a clarified presentation of mathematics which made its inner arrangement of symbols and relations obvious. That was the only way to make a presentation of mathematics without contradictions. For that purpose no metatheory could be of any help.

Without going into further detail of WITTGENSTEIN's philosophy of mathematics, it is sufficient for the purpose of the present paper to say that it originated in a discussion about the foundation of logic and mathematics (see [7]). The problems turned out to be philosophical, but they came from a discussion in pure mathematics. In other words, this is an example of serious work in logic and mathematics generating a philosophical discussion. What makes this case even more interesting is that Wittgenstein's contribution to this discussion also turned out to become one of the most influential ones for the following philosophy of science and twentieth century analytic philosophy in general.

4 Philosophical challenges to mathematics

Among philosophical challenges to mathematics as historical cases, there is at least one which is widely known and fairly well documented. I am thinking of the metaphysics of calculus which the philosopher BISHOP GEORGE BERKELEY was one of the first to raise. While mathematicians were occupied by trying to rigorize the calculus of both NEWTON and LEIBNIZ, BERKELEY attacked the soundness of the system. He objected that the mathematicians were proceeding mysteriously and incomprehensibly since they did not give the logic or reasons for their steps. This led him to question whether the mathematicians of the day acted like men of science in taking so much pain to apply their principles rather than trying to understand them. "In every other science," he wrote, "men prove their conclusions by their principles, and not their principles by their conclusions; (quoted from [5, p. 147]). Since BERKELEY's attack on NEWTON's method of fluxions, this metaphysical problem had remained the concern of leading mathematicians. Hence, in the mid–eighteenth century when D'ALEMBERT wrote the article "Différentiel" for the *Encyclopédie* he was most concerned about "the metaphysics of the *differential* calculus. This metaphysics, of which so much has been written, is even more important and perhaps more difficult to explain than the rules of this calculus themselves" [1, p. 342]. Half a century later JOSEPH–LOUIS LAGRANGE suggested that these metaphysical problems would disappear through proper use of his "theory of functions". Although the functions of calculus presented themselves in a natural way in geometry and mechanics, he argued, the theory of functions depended only on purely algebraic operations founded on the simple principles of calculus. This led LAGRANGE to announce that it

> *is more natural and more simple to consider the derivation of functions right away without employing the metaphysical circuit of the infinite small or the limits; and it would give the differential calculus a pure algebraic origin to make it depend solely on this derivation.* [6, p. 4]

That the differential calculus was conceived to have a pure algebraic origin would, according to LAGRANGE, put an end to the "geometrical metaphysics". LAGRANGE's hopes were met. The metaphysics of the calculus survived, but in a way that he had not anticipated. Since the story of BERKELEY's critique have been told many times, I will not go into any further detail. But it does connect with the next case which is also concerning a philosophical challenge to the Newtonian calculus. This time through the German philosopher G. W. F HEGEL's criticisms which were informed by LAGRANGE and provocatively thrown in the midst of British men of science in the mid–1860s by JAMES HUTCHISON STIRLING. This incident sent shock–waves through the British scientific and philosophical communities instigating several spin–off controversies.

In one of these controversies the philosopher CLEMENT MANSFIELD INGLEBY intended to remedy the bad press that metaphysics had received in Britain during this debate; the problems had been solved he felt and the time seemed ripe for a new reconciliation of science and metaphysics. This view was put forth in the journal *Nature* in 1871. He had attempted to persuade P. G. TAIT to take metaphysics more seriously and accept a distinction made by INGLEBY between mathematicians and metaphysicians. TAIT was somewhat reluctant to do so and ended up dismissing Ingleby's distinction entirely. Instead, he celebrated the mathematicians — among whom counted several natural philosophers like G. G. Stokes, William Thomson, and James Clerk Maxwell. The metaphysicians — and especially Hegel — were ridiculed. Thereby Tait angled for supporters against that certain type of metaphysical thinking Ingleby represented through the common disdain for Hegel among natural philosophers in Britain at that time.

Behind all this lay a discussion on the Hegelian calculus which had been going on in the Royal Society of Edinburgh since the autumn of 1868. The reason why Ingleby chose to discuss metaphysics at all in this context was a paper by W. Robertson Smith on Hegel's criticism of Newton's calculus read before the Royal Society of Edinburgh May 17 1869. Ingleby in his note to *Nature* carefully tried to avoid the subject of the Hegelian calculus by discussing the relationship between metaphysics (i.e. philosophy) and mathematics in general terms. However, the discussion at the Royal Society of Edinburgh was defined by a presentation of the Hegelian calculus in the philosopher James Hutchison Stirling's pen.

Robertson Smith was not a fellow of the Royal Society at that time (he was elected in 1871). On his behalf Tait communicated the paper and made his remarks in that connection on which Ingleby later commented. There is nothing in the *Proceedings of the Royal Society of Edinburgh* that suggests an animated discussion, nor that there should have been any metaphysicians of Ingleby's kind present. On the contrary, the fellows seem to have agreed with the intent and conclusion of Smith's paper, which subsequently was chosen for publication in the *Transactions.*

Smith — being an able mathematician and working as Tait's assistant at the time — aimed in his paper at denigrating the mathematical value of Hegel's discussion of the fluxional calculus. Based on a reading of Lagrange, Hegel had claimed in his *Logic* to evolve the true principles of the calculus in a form free from the alleged inconsistencies of the usual process. To clear his way Hegel engaged himself in a sharp polemic against Newton and his followers, an attack which had been received "with great satisfaction by metaphysicians" [12, p. 555]. The Scottish natural philosophers, on the other hand, certainly did not share this excitement and unanimously backed Smith up in his verdict that "[i]n short, in this and other cases Hegel makes errors of a mathematical character sufficient to show that his knowledge of the calculus was absolutely worthless" [12, p. 556].

SMITH had good reasons to believe that his paper would be well received. In January TAIT had been communicating another paper where SMITH had been castigating JOHN STUART MILL's view on geometrical reasoning. This was chosen as an "amusing and instructive example of the way in which logicians are accustomed to dogmatise upon the theory of sciences that they do not understand" [10, p. 477]. This paper apparently amused the fellows of the society, and with TAIT and THOMSON in the lead encouraged them, already at this point, to a general discussion of metaphysicians and mathematics. TAIT remarked, it was recorded, "that an excellent and interesting instance of the incapacity of metaphysicians to understand even the most elementary mathematical demonstrations, had been of late revival under the auspices of D. J. H. STIRLING" [10, p. 483].

JAMES HUTCHISON STIRLING had in 1865 published *The Secret of Hegel* which promoted Hegel's philosophy while opposing some of the generally accepted foundations of mathematics and physical science. BISHOP BERKELEY, HEGEL, and STIRLING, had all accused NEWTON of relying on a mere trick to make his method of fluxions work. The fact was, according to TAIT, that NEWTON showed his profound knowledge contrary to the metaphysicians beliefs, and demonstrated a method giving the results true to the second order of small quantities. This method gave the rate of increase of a quantity at a particular instant, but the metaphysicians only measured after that instant occurred, whereas the Newtonians measured both before and after the instant in question. "The metaphysicians cannot see this", TAIT argued, "and Dr. Stirling speaks with enthusiastic admiration of the clear–sightedness and profundity of Hegel in detecting this blunder, and for it 'harpooning Newton'." STIRLING and his fellow metaphysicians were all put in place, being harpooned themselves, and hence TAIT could conclude that "[a]ny one who is not metaphysician can see at once the superior accuracy of Newton's method" [10, p. 484].

WILLIAM THOMSON supported TAIT's censure against the metaphysicians who on their part were left unsupported. By setting the metaphysicians against "the rest of the world", THOMSON strengthened TAIT's position and further alienated NEWTON's philosophical antagonists. No one should mistake the metaphysical claims made by STIRLING on HEGEL's behalf as genuinely challenging the patented mathematics of physical science.

The Harpoon–allusion stems from *The Secret of Hegel* where STIRLING confidently wrote that "[i]t must be admitted that Hegel has succeeded here in striking his harpoon into that vast whale Newton" [15, Vol. II, p. 363]. In SMITH's later recapture of the events he dates the controversy to WILLIAM WHEWELL's early attack on Hegel's way of dealing with questions of physics. The field of discussion was thereafter enlarged by STIRLING with his charge against certain alleged imperfections in mathematicians' treatment of the fluxional calculus, and NEWTON's in particular. These passages were virtually a provocation to mathematicians, which made TAIT allude to HEGEL in a university lecture in the autumn of 1868. This effected an "express and personal" challenge from STIRLING December 21 same year in the *Edinburgh Evening Standard* where the harpoon–passage was brought to TAIT's attention for the first time [13, p. 495]. STIRLING concluded in his Christmas note by asking TAIT whether or not even in his opinion HEGEL was right and NEWTON wrong.

This was the background for the discussion February 1 1869, where THOMSON and TAIT found opportunity of alluding to the matter and "without entering into detail showed by concrete examples of varying velocity, such as are offered by railway trains and steamboats, that Newton's process was that which was naturally suggested by his physical conception of a fluxion, and that Hegel's criticism was based on an unnatural (and therefore incorrect) view of the problem" [13] TAIT subsequently encouraged his assistant, SMITH, to take up the matter more fully, and on May 17, SMITH had finished his paper which was then read by Tait for the Royal Society of Edinburgh.

The members of the Royal Society of Edinburgh were, in other words, quite familiar with HEGEL's attack on the Victorian mathematical consensus. Consequently they fashioned a uniform stance against that kind of metaphysical intimidation on natural knowledge and the methods of achieving it. This outlook was consolidated by the publication of SMITH's meticulous refutation of HEGEL's challenge to the Newtonian interpretation of the calculus. SMITH started out by mentioning WILLIAM WHEWELL's comments in the Cambridge Philosophical Society on HEGEL's Quixotic attempts to cast discredit on NEWTON's law of gravitation, and on the mathematical demonstrations of KEPLER's laws given in the *Principia*. While talking on a fundamental antithesis of philosophy — between thought and things, theory and fact, necessary and experimental truth, etc. — WHEWELL had argued that "we can have no knowledge without the union, no philosophy without the separation of these two elements" [19, p. 4]. HEGEL had to some extent rightly pointed out that the progress towards the identity of fact and idea had to be traced in the history of science, "which view, however, he has carried into detail by rash and blind conjecture" [19, p. 74].

In *Philosophy of Nature*, HEGEL had denied the validity of NEWTON's deduction of KEPLER's laws from the law of gravitation, while claiming instead that the law of gravitation could be derived from KEPLER's third law through an obscure philosophical proof "based on an elementary confusion of symbols and the failure to understand the very meaning of mathematical proof", as CAPEK has put it ([2, p. 110]; see also [15, Vol. II, p. 391]).

However, at the time WHEWELL called attention to HEGEL's views, it would have been hard to find anyone supporting these ideas, SMITH reasoned in his paper, and even "to hint that the astounding arguments of the *Naturphilosophie* flowed from any deeper source than self–complacent ignorance" [11, p. 491]. During the 1860s this had changed, and the philosophy of HEGEL was now beginning to have a more direct and significant influence on British philosophy, notably through the writings of STIRLING, dubbed "the most powerful of our living metaphysicians" ([11]; see also [8, pp. 438–445]).

What instigated SMITH's counteraction, which was supported by the members of the Royal Society of Edinburgh, was STIRLING's confident remark that HEGEL's remarks were "perfectly safe from assault" and that NEWTON was guilty of an obvious "mathematical blunder". [15, Vol. II, pp. 391 and 365]. Smith wanted to demonstrate that NEWTON knew what he was doing, and that HEGEL for the want of solid knowledge had been led astray by a misunderstanding of LAGRANGE's analytical methods and as a consequence was "swamped in hopeless absurdity". Although attempts had been made to justify HEGEL's objections to Newton by categorizing them as philosophical objections, "the question is, after all", SMITH maintained, "one of plain truth and error" [11, p. 493]. It is worth noting that SMITH did not address STIRLING himself, but instead those who might be but had not yet been influenced by STIRLING's writings. He explicitly remarked that a confirmed Hegelian was not likely to be influenced by any reasoning that he or his supporters could offer. The danger was that STIRLING should be able to convert people into antagonists of not only an important scientific icon, but also the leading scientific ideology. Apart from the meticulous refutation of HEGEL's results on a technical basis, SMITH's paper was overflowing with contemptuous and derogatory remarks enforcing the image of Hegel as a charlatan with dubious intentions, scooting away on conceptual confusion and a superficial knowledge of contemporary science.

Although the discussion of Hegelian metaphysics and mathematics among Victorian scientists has been altogether neglected by historians, the structure of the arguments against Hegel's philosophy of nature that Smith advanced in his criticism of Stirling, has been a matter of some contention in a more recent discussion of Hegel's charge against the Newtonian science. John Findlay has supported the view that the hostility by British philosophers is owing to a complete misunderstanding of Hegel's idealism, and their ignoring of the *Naturphilosophie*. "Hegel's grasp of contemporary science was, moreover, informed and accurate," Findlay argues and sums up that "Hegel gives one the science of his own day, together with the interpretations he puts on them" [3, p. 168]. Findlay has been supported by Henry Paolucci who, while referring to the many pages Hegel wrote on this subject [2] emphasizes that "Hegel's criticism was well informed" [9, p. 55].

Contrary to Findlay and Paolucci, Capek has convincingly demonstrated that Hegel's philosophy of nature was far behind the science of his own time. Either Hegel plainly and arrogantly denied those scientific discoveries which were generally accepted by the scientific community, or he ventured onto "peculiar, artificial, and often fantastic interpretations of some of the facts which even Hegel could not deny" [2, p. 109]. At any rate we have to take the Hegelian arguments seriously if we want to write this chapter of the history of mathematics. Although Hegel might be mistaken or badly informed about contemporary science and mathematics his philosophical arguments did have an impact. This necessitates studying, in this case, the arguments of metaphyscial philosophy while writing the history of mathematics.

Disregarding Hegel's somewhat illiterate knowledge of his contemporary science, it clearly did not match the scientific ideology of the leading Victorian mathematicians and men of science. Consequently Smith could self–confidently conclude that a Hegelian calculus "would certainly have been of little service to physics; but the doctrine of fluxions is itself a part of physics, and absolutely indispensable in some form or other to the right understanding of physical problems" [11, p. 497]. There were never any doubts, that there was a right understanding of physical problems. This was identical with that of the scientific establishment, and that no one should try to prove otherwise. Smith further estranged Hegel and his followers by showing that they did not share the intellectual and moral standards of the hard–working scrupulous scientists. HEGEL — and STIRLING's defence of him — confirmed SMITH's suspicion that it was allowed from a metaphysical point of view that someone wrote about things he had not studied but superficially. Even worse, though, was that people like STIRLING dared to implore mathematicians to come and read the results of such attempts, and hence waste their valuable time. To see that this did not happen more often, it was necessary and for the benefit of the scientific community to refute HEGEL's metaphysical speculations on the calculus and the derivation of the law of gravitation once and for all.

The publication of SMITH's paper was followed by a brief newspaper controversy on a very personal note where neither SMITH nor STIRLING gave way. Only interrupted by the controversy–offspring in INGLEBY and TAIT's exchange in *Nature*, the discussion slumbered for about three years only to break out again with the publication of STIRLING's *Lectures on the Philosophy of Law* in 1873. STIRLING had used the meantime to work on a new defence of Hegelian philosophy and on two elaborated responses to both WHEWELL and SMITH. About forty pages were devoted to vindicating Hegel in the mathematical reference, partly commenting SMITH's paper and partly giving new statements of HEGEL's doctrines on this lead. SMITH replied in *The Fortnightly Review* that if it had just been a personal matter between him and STIRLING, he would have stopped, claiming that it was not to be in the interest of the literary public. However, after sketching the history of the debate, SMITH hoped that he had convinced the reader that he did not appear as an advocate for himself, but instead for the noble cause of mathematics.

But SMITH did not stop at appealing to the readers' sense of protecting science from personal sneak–attacks. He went on and made it a matter of national pride by emphasizing NEWTON as a *British* scientist working in the interest of mathematical science, and "especially of that physico–mathematical school which is the heir to Newton's methods and ideas." This was, of course, the Victorian scientific ideology celebrated by its practitioners [13, p. 496]. However, SMITH argued, STIRLING was too little a mathematician to really understand the problems involved, since it was "quite evident" that he was not able to follow SMITH's "symbolic statements" [13, p. 501]. By calling attention to the non–personal character of the scientific interests at stake, SMITH tried to create a picture of an idiosyncratic and personally biassed challenge to the Victorian values of the intellectual establishment. SMITH received quite a good help from STIRLING himself by quoting the latter's polemic way of arguing verbatim. But put in a context of a man opposed to the disinterested scientific community, Smith managed to conceal what was in fact a highly personal attack, effectively sidelining Stirling and metaphysical philosophy in this discussion.

Thomson's and Tait's remarks from the meeting of the Royal Society of Edinburgh in January 1869 had been translated into French in the meantime which made Stirling regret all the "rabid nonsense" that had been directed against him. As a response, Stirling attempted to patronize Smith, playing on his academic insignificance compared to Thomson and Tait, suggesting that these two leaders of physical science had been misled by an error in Smith's paper. The problem about this argument, as Smith rightly pointed out, was that Thomson and Tait made their comments several months before Smith's paper was presented for the Royal Society of Edinburgh, and more than a year before it was published. Stirling had not payed attention to these facts in plotting Smith as "the evil genius of Scottish physicists" [13, p. 497]. But at this point of the discussion the aim was to disrepute Smith rather than solve the problems involved with the Hegelian calculus. Curiously enough, Stirling accused the baffled Smith of having asserted his malign influence on Tait with regard to Leibniz as it came forth in the latter's exchange with Ingleby (Tait had called Leibniz a mere thief with respect to the calculus.) Smith never mentioned Leibniz, but it indicates how these two discussions were connected — also in the eyes of their contemporaries — in a conflict between the natural sciences and metaphysical philosophy.

Stirling responded to the charges of his mathematical incompetence in a contemptuous note appearing in the following issue of *Fortnightly Review.* He very cooly explained why he thought Hegel was right in technical terms and what his aim was in pressing charges against the Newtonian calculus. This time Stirling tried to avoid personal comments and keep the discussion at a purely professional level, but failed in holding himself back from poisoning his pen. Hegel, Stirling argued, when he took up the study of mathematics, wanted to see what all this meant, but got no intelligible explanation. As a result he started his metaphysical investigation of the question of "what was this differential?" [16, p. 513]. This gave Stirling a chance once more to emphasize that Hegel's interests were not mathematical, but strictly philosophical. Smith's paper, on the other hand, was viewed as being of "such a character as not to demand any further answer from me" [16, p. 514]. Nevertheless, did Smith's response to his alleged bad influence on Thomson and Tait do their work on Stirling who went through great pain in emphasizing his respect for the two "leaders of physical science". He concluded by addressing Thomson and Tait directly in order to clear his name from Smith's indirect charges. At the same time he tried to smear Smith by making him look like he was degrading the discussion, whereas Stirling himself obviously resided in the same superior class as Thomson and Tait.

To receive the full attention and sympathy of the entire scientific community, SMITH now directed the locus of discussion from *The Fortnightly Review* to the columns of *Nature*. STIRLING thought to have caught SMITH in admitting that a Hegelian calculus could actually be a possibility, but in his uncompromising response SMITH answered that the phrase "Hegelian calculus" was used in irony, and stated that what

> *Hegel has given us on the subject of the calculus is, strictly speaking, nonsense. But, as I have shown, this nonsense is not mere metaphysic, but involves mathematical absurdity. It is of course only in irony that one can dignify the paradoxes of mathematical ignorance with the title of a Calculus; and if this admission satisfies Dr. Stirling, then our controversy is at an end.* [14, p. 443]

SMITH had, in other words, nothing further to add. He felt that he had successfully demonstrated the untenability of the Hegelian doctrines, and since there had not been any arguments against SMITH's mathematical proofs, STIRLING ought to accept that. One curious, but important thing is, that SMITH indirectly admitted a more harmless form of metaphysical speculation. The problems did not become intolerable for the scientist until the arguments stopped relating only to metaphysical speculation and began to impose itself on scientific subject matters, claiming knowledge in these fields that contradicted the consensus of the scientific community. Metaphysics had no right to overrule the authority of science, but if it eventually did happen, SMITH demonstrated that the scientific community reacted instantly and effectively. STIRLING tried to fence off the charges repeating himself from *The Fortnightly Review* by calling attention to the tone of SMITH's reply as being "on the level of a business transaction." Believing that such had been hitherto unexampled in literary controversy, he denied to answer. "I cannot with any respect to myself," he concluded, "enter into further direct relations with Mr. Smith" [17, p. 27]. Thereby he avoided entering the technical discussion of the calculus being the substance of SMITH's response, and at the same time fulfilled the latter's wish to end the controversy. However, things did not end for Stirling by that, although Smith held back.

A couple of months later *Nature*'s readers had Smith's views confirmed by William Stanley Jevons in his review of *Lectures on the Philosophy of Law.* This only demonstrates Smith's smart move of redirecting the discussion to the voice of Victorian scientific ideology. Jevons immediately took Smith's side in the controversy against "Hegel and his satellite Stirling". While reading the first fifteen pages of Stirling's book, he "did not enjoy for a single moment the feeling of solid ground." Subsequently Jevons felt that Hegel "must suffer both in his metaphysics and his physics". There could be no doubt for *Nature*'s readers about the poor value ascribed to Stirling's defence of metaphysics against the accepted scientific doctrines, and Jevons could conclude, confident on behalf of science, by asking rhetorically:

> *When Hegel's philosophy breaks down so sadly at the slightest touch of fact, can we waste our time, or that of our readers, with endeavouring to attach a meaning to pages of this kind of philosophy?* [4, p. 241]

Two months later Stirling ascribed to some of the disagreements there had been to a misunderstanding of Hegel, who now had nothing against neither gravitation as a fact, nor the differential calculus as an established method of indubitable scientific calculation. The thing was rather, Stirling argued, that Hegel "would only attempt to philosophise both by placing metaphysical principles under them" [18, p. 382]. While insisting upon the value of Hegel's approach to physics or mathematics as a metaphysician rather than a physicist or a mathematician, Stirling repeated an argument already tested eight years earlier [15, Vol. II, p. 380]. He did not have much success with it then, but now he was played the opportunity of reusing it on the view put forth by Jevons — whom Stirling by the way failed to identify — by characterising it as an example of "only once again the blind rush of prejudice from its usual dark corner of relative ignorance — an ignorance which it will persist in, and not (through study) convert into the light of day" [18, p. 382]. In other words, Stirling tried to use his antagonists' own weapons by turning the argument of ignorance — as used by Smith — around. Both Smith and Jevons remained silent, assured of having convinced the scientific community of the untenability of Hegel's metaphysics in questions of scientific methods and natural knowledge. Who could take seriously a man who tried to prove that NEWTON understood neither fluxions, nor the law of gravitation?

Nevertheless, members of the Victorian scientific elite put an extraordinary effort into refuting HEGEL's philosophical challenge to mathematics. It was debated vigorously at meetings, at public lectures, in newspapers, and in several professional journals. This was the case even though the defenders of Newtonian calculus (and thereby of the Victorian scientific ideology) explicitly said that it was not worth wasting any time on. The irony of this speaks for itself and serves as a good example of how careful we should be not taking bold statements at face value. The intellectual effort and investment of time alone show us that the Victorian mathematicians certainly did take this philosophical challenge seriously. Hence, we as historians have to do so as well.

* * *

From an historiographical perspective, historians of mathematics might find the perspective of "informed frameworks" most useful. Certainly there are many tacit assumptions made when writing even what is considered as straightforward history of mathematics. More or less explicitly, they all count as informed frameworks. "Spontaneous philosophy" seems to be more of an exclusively philosophical interest. Yet, as argued above, such instances have to be taken seriously by the historian of mathematics when they form part of the history and times in question. The same goes for the topic of "philosophical challenges to mathematics." In this paper I have given the latter perspective more attention than the former two. In part, this is because such examples provide us with excellent insights into what mathematicians were thinking about what they were doing, how they viewed themselves, their (often tacit) ideology, their nationalistic commitments, etc., and how all that have influenced their work and the choices they have made in that regard. Taking up a discussion like the Victorian counter–attack on the alleged Hegelian calculus makes a good example of the importance of studying the connections between history and philosophy of mathematics. When seen from an internalist point of view it may seem to say less of how the mathematics we know today has evolved. On the other hand it tells us a lot about what it really meant to be a mathematician in mid–Victorian Britain. To my mind that is a far more interesting question in the history of mathematics today.

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Peter C. Kjærgaard
History of Ideas
University of Aarhus
Ndr. Ringgade 1
DK-8000 Aarhus C
Denmark
e-mail: idepck@human.au.dk