Mathematical Explanation: Problems and Prospects

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Introduction

Since this issue is devoted to the interaction between philosophy of mathematics and mathematical practice, I would like to begin with an introductory reflection on this topic, before I enter the specifics of my contribution. In the past thirty years there has been a marked shift in philosophy of mathematics, due to the appearance of research on aspects of mathematics that were previously ignored by philosophers of mathematics. A short, and very incomplete, list includes work on the dynamics of mathematical growth,[1] the debate on computer proofs,[2] the role of diagrammatic reasoning in mathematics,[3] induction and conjecture in mathematics,[4] problems at the interface of theoretical physics and mathematics.[5] In some cases, these contributions have been accompanied by much fanfare about the need to pay attention to mathematical practice and by an attack on philosophy of mathematics as "foundations of mathematics", variously called, "formalism", "foundationalism", "justificationism."[6] Whereas some of the polemical tone might have served to bring attention to new and exciting developments, I find that overall it is unwarranted and tends to muddle the issues. First of all, the characterization of the foundational programs, which are being attacked, is often one-sided at best and patently false in the worst cases. But even leaving questions of historical accuracy aside, all the programs in foundations of mathematics in this century have, in my opinion, been concerned with mathematical practice. In the grand foundational programs, say Hilbert's, attention to practice was necessary to insure that the consistency program be able to account for all of mathematics, as opposed to a small part of it.[7] And setting up the formalisms does require a very good sense of how much you need for various parts of mathematical practice. In this sense, many programs in contemporary logical foundations, such as reverse mathematics or predicative mathematics, are extremely sensitive to issues of mathematical practice. Moreover, the distinction between elementary and non-elementary methods, which was one of the cornerstones of Hilbert's program, is a typical issue emerging from mathematical practice. However, it is true that many of the classical foundational programs "filter out" many aspects of mathematical practice which are irrelevant to their goals. Hence, there is a kernel of truth in the above mentioned criticisms. There are many aspects of mathematical practice that are irrelevant for some of the classical foundational programs but nonetheless worthy of philosophical attention. Thus, for instance, while a study of mathematical heuristics is not relevant to Hilbert's program, it has much to offer to the philosophers and mathematicians who are interested in aspects of mathematics which go beyond the specific aims set by Hilbert for his task. But this, contrary to some of the polemical claims I referred to above, in itself does not invalidate Hilbert's program (other considerations do!). It only calls for a liberalization concerning what aspects of mathematics should be objects of philosophical interest. I think that much of the alleged opposition between these developments can be deflated if one keeps in mind that the aims of both traditions are legitimate and all provide essential information about the complex reality we are interested in, i.e. mathematics.

The topic of my paper, mathematical explanation, also escapes traditional foundational work. Part of the reason is that the subject area is admittedly vague, and consequently difficult to treat with precise mathematical or logical tools. Moreover, it does not bear directly upon some of the traditional foundational concerns, such as certainty, which have dominated much of philosophy of mathematics. It is nonetheless a subject of great philosophical interest. Consider, for instance, the situation in philosophy of science. There the topic of scientific explanation has received much attention. In this

Topoi **20**: 97–117, 2001.

area researchers have focussed on two sets of concerns. The first has been the proposal and analysis of several competing models of scientific explanation. In addition, there has been also much interest in what role explanatory concerns play in scientific practice. This can of course shed a lot of light on scientific controversies and styles of doing science.[8] In contrast to these rich developments very little attention has been devoted to the topic of mathematical explanation. In diagnosing the difficulty of providing an account of mathematical explanation Resnik and Kushner have claimed the following:

> Although from Aristotle onwards empirical science has acknowledged the production of explanations as one of its major goals and accomplishments, this is not an acknowledged goal of mathematical research. Mathematicians rarely describe themselves as explaining and Steiner's work (Steiner, 1978) is one of the few philosophical accounts of mathematical explanation. Given such evidence that the *practice* of explaining mathematical phenomena has been barely acknowledged, one could hardly expect that testing descriptive or normative accounts of it would be an easy task (1987, p. 151).

Since Resnik and Kushner wrote their piece, new work on mathematical explanation, to be discussed below, has deepened our understanding of the topic.

In this paper I have three major aims. The first is to introduce the topic of mathematical explanation by listing a number of problems followed by a reflection on the status of research and prospects for further development. The second is to draw attention to an important tradition in philosophy of mathematics for which explanation is a concern. For want of a better term I will call this tradition "hypothetico-inductivist" (henceforth h-inductivist; I would have used "inductivist" had this not been in conflict with Lakatos' use of the word "inductivism"). Thirdly, I will present a case study of a development in mathematical practice which originates from explanatory concerns. Hopefully, this material will contribute to the "evidence" we need in order to propose, and evaluate, descriptive or "normative" accounts of mathematical explanation.

Accordingly, the paper is divided into three parts. The first part introduces the general topic of mathematical explanation and provides important conceptual distinctions and clarifications. The second part discusses Mill, Russell, Gödel, Lakatos and other philosophers of mathematics on mathematical explanation by emphasizing the central "h-inductivist" intuition. What joins these philosophers is a view of mathematics that renders the boundaries with physics less sharp than the tradition would want us to believe. The third part presents Alfred Pringsheim's "explanatory" approach to the foundations of complex analysis.

Part I. Five questions on mathematical explanation

The following five questions will serve as an introductory guide to the topic of mathematical explanation:

1. Are there explanations in mathematics?
2. What form do they take?
3. Is mathematical explanation a novelty in philosophy of mathematics?
4. What are the philosophical accounts of mathematical explanation?
5. What is the relationship between mathematical explanation and theories of scientific explanation?

I.1. *Are there explanations in mathematics?*

Many mathematicians find the distinction between proofs that explain and proofs that convince but do not explain to be important (I will treat the philosophers in section I.3). For instance, Georges Bouligand presents that distinction under the terminology of causal and non-causal proofs. Thus in his *Premières leçons sur la théorie générale des groupes*, we read:

> Causal demonstrations. Many theorems can be given different demonstrations. The most instructive are of course those that let one understand the deep reasons of the results that one is establishing. On this matter the notion of domain of causality gives us a guide. (Bouligand, 1932, p. 6)[9]

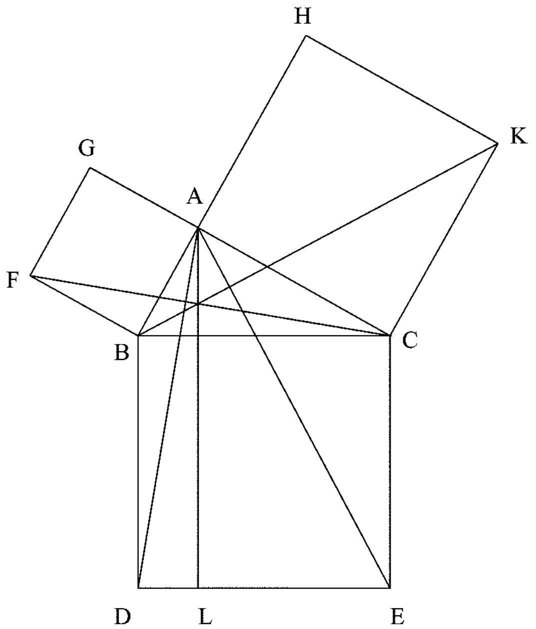
Bouligand identified the causal/non-causal opposition with that of explanatory/non-explanatory proofs. This is clear from several passages of his work of which the following, concerning a concrete example on homographic transformations, is representative:

> One sees clearly the difference between these two ways of treating the same problem. Only the second gives a satisfactory explanation, precisely because it takes place within the domain of causality of the propositions that is to be established. (Bouligand, 1932, p. 7)[10]

Let us consider a concrete example, given by Bouligand (1933, p. 258). I will give three proofs of Pythagoras' theorem (Euclid, *Elements*, I.47). According to Bouligand, the first proof convinces but does not explain; the second is explanatory; the third is intuitive but not explanatory.

Pythagoras' theorem: In right-angled triangles the square on the side sub-tending the right angle is equal to the sum of the squares on the sides containing the right angle.

Euclid's proof:
Let *ABC* be a right-angled triangle. Consider the squares on the sides *AB*, *AC*, and *BC*. Through *A* draw *AL* parallel to *BD*. Join *AD* and *FC*.



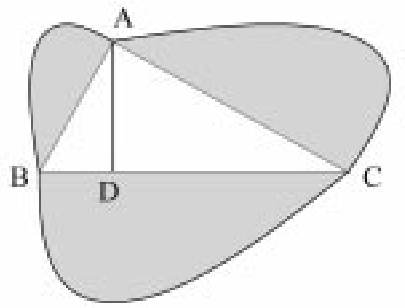
Since the angles *BAC* and *BAG* are right it follows that *CA* is in a straight line with *AG* (by proposition I.14). For the same reason *BA* is also in a straight line with *AH*. Now, since the angles *DBC* and *FBA* are both right, adding the same angle *ABC* to both of them, we obtain two equal angles, *DBA* and *FBC* (by common notion 2).

Consider now the triangles *ABD* and *FBC*. Since *FB* = *AB*, *BD* = *BC* and the angles *DBA* and *FBC* are equal we obtain, by I.4, that the triangles *ABD* and *FBC* are equal.

Consider now the parallelogram *BL*, with base *BD* and height *DL*. By I.41 the parallelogram *BL* is double the triangle *ABD*, for they have the same base *BD* and are constructed between the same parallels *BD* and *AL*. By appealing again to I.41 one can also see that the square *ABFG* is double the triangle *FBC*, for they have the same base *FB* and are constructed between the same parallels *FB*, *GC*. Thus *BL* = 2*ABD* and *ABFG* = 2*FBC* = 2*ABD*. Thus, the parallelogram *BL* is equal to the square *ABFG*.

By an analogous argument one now proves that the parallelogram *CL* is equal to the square *ACKH*. Thus the square *BDEC*, which is the sum of the parallelograms *BL* and *CL*, is equal to the squares *ABFG* and *ACKH*.

Explanatory proof:
Consider the triangle *ABC*. Draw *AD* perpendicular to *BC*.

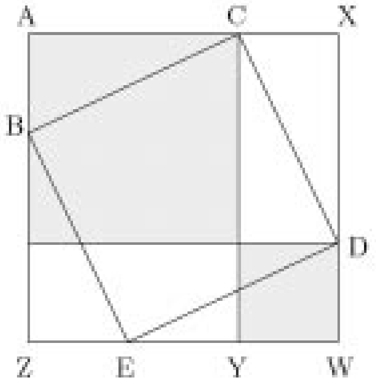


We obtain two triangles *DAB* and *DAC*, which are similar to *ABC*. Indeed, *ABC* and *DAC* are similar since they have equal angles (*ADC* and *BAC* are both right; *ACB* is in common). An analogous reasoning establishes that *ABD* and *ABC* are similar. Now the areas of similar plane figures (not only triangles!) are to each other as the squares of the corresponding sides. Thus the areas of *DAC* and *ABC* are to each other as the squares of *AC* and *BC*. Similarly the areas of *ABD* and *ABC* are to each other as the squares of *AB* and *BC*. Thus, $ADC/ABC = AC^2/BC^2$ and $ADB/ABC = AB^2/BC^2$, and hence $ADC + ADB/ABC = AC^2 + AB^2/BC^2$. But $ADC + ADB = ABC$, which implies that $AB^2 + AC^2 = BC^2$.

It is easily seen that the Pythagorean theorem is a special case of the fact that the area of a figure F constructed upon the hypothenuse is equal to the sum of the areas of figures similar to F constructed upon the other sides. Indeed, the areas of similar plane figures are to each other as the square of their corresponding sides. Thus we can write the shaded areas as λAC^2, λBC^2, and λAB^2. All we need to do now is to find three similar figures satisfying $\lambda AC^2 + \lambda AB^2 = \lambda BC^2$. But we already have those since, by the above, $ADC + ADB = ABC$. Hence $AC^2 + AB^2 = BC^2$.

Finally, we give a proof that is intuitive but not explanatory.

Intuitive proof:



The shaded areas correspond to the squares constructed on *AC* and *AB* respectively. The square built on the hypothenuse is *CBED*. The proof consists in noticing that in order to obtain *AXWZ* from *CBED* one would need to add the same area (four times the triangle *CXD*) that is needed to go from the shaded areas to *AXWZ*.[11]

It is worthwhile to point out that Steiner, independently of Bouligand, also focussed on the second proof of the Pythagorean theorem as an example of explanatory proof (1978, p. 138). Moreover, both of them claim that the "explanatoriness" of the second proof is due to its more general nature (although Steiner is tentative on this).

Another mathematician who has recently emphasized the role that the search for explanatory proofs plays in mathematics is Rota. In connection to the computer proof of the four color theorem he says: "Not all proofs give satisfying reasons why a conjecture should be true. Verification is proof, but verification might not give reason" (1997, pp. 186–187). Moreover, after having discussed the history of the classification theorem for Lie groups, he concluded: "We are led to the conclusion that mathematicians are not satisfied with proving conjectures. They want the reason" (p. 187). This search for reasons, or "explanations", seems to play an important role in mathematical practice.[12] Moreover, it is important to point out that many examples of explanation in mathematics are not of the sort mentioned above (explanatory vs. non-explanatory proofs). For instance, we are often faced with successful analogies between different areas of mathematics that remain mysterious until a new theory comes around to forge the right connections thereby providing an explanation for the successful analogy between the two areas. A good example here is how admissible set theory gave an explanation for a number of surprising analogies (and not less surprising disanalogies) that had been noticed between different classes of the arithmetical and analytical hierarchies. Examples could easily be added. Indeed, in his dissertation of 1997 Sandborg has shown how widespread informal talk of explanation in mathematics is by running a CD-ROM search of *Mathematical Reviews*, which provided several interesting examples.

It is my impression that the question I raised in this section is to be answered unequivocally in the positive. However, much more needs to be done here. The goal, in my opinion, is to focus on examples that mathematicians will recognize as central to the discipline and that will carry a force of persuasion similar to the standard stock of examples available in philosophy of science. Only detailed case studies will eventually provide us with what is needed. Part three of this paper is a contribution in this direction.

Having established that mathematical explanations exist we now need to ask what form they take.

I.2. *What form do mathematical explanations take?*

"Explanation" is a notoriously ambiguous word. Its meanings range from explaining the meaning of a concept to explaining how to fix a bike. In connection to mathematics talk of explanation, in addition to all its standard informal meanings, occurs in different contexts. In order to clarify what meaning of mathematical explanation I am primarily after, it is important to remark that the focus is on pure mathematics and that I am not, at least not immediately, after the following contexts:

1. One often speaks of the explanatory role played by mathematics in explaining physical phenomena. In this sense Poincaré claims that the explanation why space has three dimensions (a physical fact) is to be found in a theorem about group theory (a mathematical fact).[13]
2. Appealing to the distinction between context of discovery/context of justification, one often encounters cases where the context of discovery provides an informal explanation for a result which is later justified by rigorous mathematical means. In this sense Dedekind speaks of the intuitive notion of measuring one extensive magnitude by another as an

explanation of irrational numbers (something he disagrees with). Other examples of this sort abound in areas where physical intuitions are at the source of a formal mathematical development.[14]

3. In the classroom we often provide, or are presented with, explanations. There is some literature on mathematical explanation from the point of view of mathematical education (see Hanna, 1990, 1997; Sierpinska, 1994; Hersh, 1993, 1997, for discussion of explanatory proofs in the classroom). There are however good reasons to keep separate what might be explanatory in the classroom and what is explanatory for the community of mathematicians (see my comments on Hersh in section II.5).

Eventually, it would be good to be able to account for all these different uses of the word "explanation" in mathematical contexts. But at the moment it is important at least to draw the basic conceptual distinctions, while keeping in mind that there are often borderline examples and possible overlaps between the categories.

In recent contributions Sandborg (1997, 1998) has emphasized the *mathematical practice* of explaining mathematical phenomena by reference to case studies from contemporary mathematics. In particular, he has given a lengthy analysis of several proofs of Pick's theorem and of a theorem by Polya on inequalities. Sandborg has pointed out that mathematical explanations come in several varieties (he gave a classification with seven main patterns)[15] but his main focus is on the opposition between proofs that explain and proofs that do not explain. To complement his work, I would like here to pay attention to forms of explanation in mathematics in which a particular presentation of a theory provides the natural explanation for its results. The test case I present in the third part offers an example from the theory of functions of a complex variable.

In Mancosu, 2000 I also emphasized cases of explanations as "reduction to the familiar", such as in the translation given by Menger of Brouwerian set theory into concepts of the classical theory of real functions.[16]

The relevance of the distinction between explanatory and non-explanatory proofs for the mathematical practice of the past has been raised in Chemla, 1997 for third-century Chinese mathematics, in Mancosu, 2000 for seventeenth century mathematics, in Desanti, 1975 for Greek mathematics and later developments, and in Bouligand, 1933 for nineteenth century developments in geometry and group theory.[17]

Once again, I think there is much to be done in this area by means of classifying patterns of mathematical explanation and isolating perspicuous examples, both in contemporary mathematics and in the history of mathematics.

I.3. *Is explanation a novelty in philosophy of mathematics?*

In two recent articles (Mancosu, 1999 and 2000) I have argued that there is a long tradition in the philosophy of mathematics that considers the problem of explanation central to the discipline. The main intuitions of this tradition go back to Aristotle. In *Posterior Analytics* I.13 Aristotle distinguished between demonstrations "of the fact" and demonstrations "of the reasoned fact". The former do not provide scientific reasoning because they proceed from the effect ("the explanandum") to the cause ("the explanans"), whereas the latter proceed from causes to effects. "Cause" here translates *aitia* which, as a matter of fact, is most of the time translated as explanation. Thus, under this reading of Aristotle's theory we can argue that Aristotle is here characterizing a distinction between explanatory proofs and non-explanatory ones. According to Aristotle, the best examples of explanatory proofs are to be found in the mathematical sciences. This Aristotelian distinction is at the source of the Renaissance and seventeenth-century debates on mathematical explanation (see Mancosu, 1996 for an extended treatment) and of two major accounts of mathematical explanation in the nineteenth century, those of Bolzano and Cournot (see Kitcher, 1975 and Mancosu, 1999). Bolzano and Cournot clearly distinguish between explanatory proofs and non-explanatory proofs and see the role of philosophy of mathematics primarily as that of accounting for the distinction. In this paper I add something new to the story. I will show in part two that a number of important philosophers of mathematics, sharing an "hypothetico-inductivist" intuition, find it very natural to talk about mathematics in explanatory terms.

I.4. *Contemporary philosophical accounts of mathematical explanation*

There are mainly two philosophical accounts of mathematical explanation available. The first is due to

Steiner (1978) and has been discussed by Resnik and Kushner (1987). Whereas Steiner attempts to single out certain proofs as tout court explanatory, Resnik and Kushner argue, following van Fraassen, that explanation is a context-dependent feature of proofs: "nothing is an explanation simpliciter but only relative to the context dependent why-question(s) that it answers" (p. 153) (see Sandborg, 1997, ch. 2 and Mancosu, 2000 for a summary of the discussion). The second is due to Kitcher, who has discussed mathematical explanation in a number of publications (1975, 1984, 1989). Kitcher is a well known proponent of an account of scientific explanation as theoretical unification (see also Friedman, 1974). His account is meant to encompass mathematics and empirical science as well:

> And as in other sciences, explanation can be extended by absorbing one theory within another. It is customary to praise scientific theories for their explanatory power when they forge connections between phenomena which were previously regarded as unrelated. Within mathematics the same is true and it has become usual to defend the 'abstract' approach to mathematics by appealing to the connections which are revealed by studying familiar disciplines as instantiations of general algebraic structures. (Kitcher, 1975, pp. 259–260)

It is not my intention here to discuss the virtues or faults of the above accounts. I think the general feeling is that we are still very far from having a satisfactory philosophical account of mathematical explanation. But, given that work in this area has picked up momentum only recently, this is only to be expected.

In philosophy of science one distinguishes between epistemic and ontological accounts of scientific explanation (Ruben, 1993). Epistemic accounts often put emphasis on the relationship between understanding and explanation. Understanding in mathematics is obviously a very important topic in connection to explanation. Recent contributions to understanding in mathematics from a philosophical point of view include Thurston, 1994 and Manders, 1989 (for mathematical understanding from a pedagogical point of view see Sierpinska, 1994).

Another issue of great potential interest is how far explanations can be pushed. Are there mathematical facts that are unexplainable? Recent investigations by Chaitin on algorithmic information theory seem to point to an affirmative answer.[18]

I.5. *What is the relationship between mathematical explanation and theories of scientific explanation?*

The topic of mathematical explanation seems to me a central one for an accurate understanding of mathematics. Mathematicians, as we have seen, do not simply struggle to obtain rigorous and compelling proofs. They often look for new proofs or consider old proofs unsatisfactory on account of their lack of "explanatoriness". This aspect of mathematical activity must be accounted for by a philosophy of mathematics that is faithful to mathematical practice.

In addition to the relevance to philosophy of mathematics, the issue of mathematical explanation is also important for the testing of theories of scientific explanation. Theories of scientific explanation often attempt to characterize the "scientific" aspect of an explanation, independently of the subject area in which it might be offered. These accounts should thus be able to capture mathematical explanations. If they are unable to do so this will reveal a serious limitation of the theories. If, on the other hand, it turns out that there is no single theory that can account for explanations as given in empirical science and mathematics, this will reveal a very interesting difference between the two domains. It is thus clear that mathematical explanations can be used to test theories of scientific explanation and that an account of mathematical explanation might have important consequences for the philosophy of science. Sandborg, 1998 has used his case studies to "test" the viability of van Fraassen's theory of scientific explanation (1990) as a theory of mathematical explanation. Once again, there is much room here for further contributions.

Part II. The "hypothetico-inductivist" tradition on explanation in mathematics

II.1. *Explanation and hypothesis in science and mathematics*

Talk of explanation is ubiquitous in the natural sciences. So much so that many philosophers of science, as well as many scientists, have claimed that explanation is the principal aim of science. For instance, E. Meyerson in *De l'explication dans les sciences* (1921) writes:

> The concern for explanation and the tenacious will to extend its domain at any cost so far outweigh any other consideration in the march of science that the truths that initially seemed the most plausible, the most well-established facts, are set aside, intentionally forgotten as it were, upon the appearance of a more comprehensive theory allowing a much greater number of phenomena to be reduced to a system, to be connected by deduction (p. 78; English translation, p. 63).[19]

However, some have even denied that explanation has any place in science. Duhem epitomizes the distinction between the two points of view as follows:

> The first question we should face is: What is the aim of a physical theory? To this question diverse answers have been made, but all of them may be reduced to two main principles:
>
> "A physical theory," certain logicians have replied, "has for its object the *explanation* of a group of laws experimentally established."
>
> "A physical theory," other thinkers have said, "is an abstract system whose aim is to *summarize* and *classify logically* a group of experimental laws without claiming to explain these laws." (Duhem, 1906, Engl. Transl. p. 7)

It is well known that Duhem was, together with Comte and Mach, an ardent supporter of the second view. One recognizes in this description of the two alternative views of the goal of physical theories a central aspect of the debate on explanation and hypothesis in the natural sciences, which was raging in the nineteenth and early twentieth century. As John Stuart Mill noted, the connection is fundamental:

> A hypothesis is any supposition which we make (either without actual evidence, or on evidence avowedly insufficient) in order to endeavor to deduce from it conclusions in accordance with facts which are known to be real; under the idea that if the conclusions to which the hypothesis leads are known truths, the hypothesis itself either must be, or at least is likely to be, true. If the hypothesis relates to the cause or mode of production of a phenomenon, it will serve, if admitted, to explain the facts as are found capable of being deduced from it. And this explanation is the purpose of many, if not most, hypotheses. (Mill, 1843, Book 3, Ch. XIV §4)[20]

The issue of the role of hypotheses in science was a burning one in nineteenth-century physics. A strong reaction against the use of explanatory models led many to consider explanations as metaphysical extra baggage. The book by Meyerson quoted above should in fact be seen as an extended polemic against positivist and idealist theories of science. Both are guilty, according to Meyerson, of reducing arbitrarily the explanatory power of science.[21]

Much of contemporary philosophy of science has been deeply engaged in the attempt to explicate the concept of scientific explanation. These attempts are not concerned directly with the debates mentioned above and often assume that despite the variability of the criteria of what is accepted as explanatory, explanations can always be captured as belonging to specific types.

By contrast, mathematics looks remarkably free from issues related to hypotheses and explanatory concerns as they appear in physical theories. However, I will try to show that some very important philosophers of mathematics have forged a connection between mathematics, explanations and hypotheses.

Perhaps the easiest place to begin is with Mill.

II.2. *John Stuart Mill on mathematical explanation*

We have seen the close connection that Mill drew between hypotheses and explanations in science. However, according to Mill, the deductive sciences – including arithmetic and geometry – are not essentially different from the empirical sciences:

> From these considerations it would appear that Deductive or Demonstrative Sciences are all, without exception, Inductive Sciences; that their evidence is that of experience; but that they are also, in virtue of the peculiar character of one indispensable portion of the general formulae according to which their inductions are made, Hypothetical Sciences. Their conclusions are only true on certain suppositions, which are, or ought to be, approximations to the truth, but are seldom, if ever, exactly true; and to this hypothetical character is to be ascribed the peculiar certainty which is supposed to be inherent in demonstration. (Mill, 1843, Book 2, Ch. VI, §1)

It thus follows that for Mill mathematics presents explanations just as any empirical science does, for the hypotheses of arithmetic and geometry are introduced to explain the regularities found in the phenomena. In my opinion Mill is one of the most important defenders of the "h-inductivist" tradition in philosophy of mathematics. Although in Mill the "h-inductivism" is a consequence of his empiricism the two positions should not be conceptually confused. What is meant here by "h-inductivism"? H-inductivism is, roughly, a conception of mathematics which asserts that the acceptance of axioms for a mathematical discipline might be motivated not by criteria of evidence and certainty but rather, like hypotheses in physics, by their success in deriving and systematizing a certain number of familiar consequences. In this sense the consequences are often more evident than the axioms we are appealing to in deriving them. Eminent philosophers and mathematicians such as Russell and Gödel defended an h-inductivist view and remarked upon the relationship between h-inductivism and explanation.

II.3. *Russell and Gödel on hypothetico-inductivism in the foundations of mathematics*

Let us look at Russell's position on h-inductivism in mathematics. Particularly revealing in this connection is a paper entitled "The Regressive Method of Discovering the Premises of Mathematics" read before the Cambridge Mathematical Club on March 9, 1907 and published in the collection *Essays in Analysis* (1973). The analogy between mathematics and the experimental sciences is stated at the very beginning of the article:

> My object in this paper is to explain in what sense a comparatively obscure and difficult proposition may be said to be a premise for a comparatively obvious proposition, to consider how premises in this sense may be discovered, and to emphasize the close analogy between the methods of pure mathematics and the methods of the sciences of observation (p. 272).

The problem was pressing for Russell. Indeed, according to his logicist project an evident truth like $2 + 2 = 4$ can be proved from more basic propositions of logic, which are however more recondite and obscure (see also Couturat, 1905, pp. 7–8). For Russell this is consistent with claiming that the logical truths are simpler than the mathematical ones. This is because "it is a mistake to suppose that a simpler idea or proposition is always easier to apprehend than a more complicated one" (p. 273). According to this criterion of logical complexity Russell goes on to say that the propositions more easily apprehended are those which are neither too simple nor too complex. He then adds:

> In mathematics, except in the earliest parts, the propositions from which a given proposition is deduced generally give the reason why we believe the given proposition. But in dealing with the principles of mathematics, this relation is reversed. Our propositions are too simple to be easy, and thus their consequences are generally easier than they are. Hence we tend to believe the premises because we can see that their consequences are true, instead of believing the consequences because we know the premises to be true. But inferring the premises from consequences is the essence of induction; thus the method of investigating the principles of mathematics is really an inductive method, and is substantially the same as the method of discovering general laws in any other science (pp. 273–274).

The above passage justifies our use of h-inductivism for characterizing the position outlined in the previous section. One important problem to keep in mind is that Russell seems to apply the h-inductivist conception only to the regress involved in the discovery of the basic laws from which mathematics can be derived. He seems to think that most theorems of ordinary mathematics are not justified inductively, i.e. we believe them because we believe the premises to be true. Russell concluded his paper remarking that the traditional conception of mathematics only gave us the right order of exposition but not the right order of knowledge, which goes from what is most evident to what is not as evident:

> But when we push analysis farther, and get to more ultimate premises, the obviousness becomes less, and the analogy with the procedure of other sciences becomes more visible. The various sciences are distinguished in their subject matter, but as regards method, they seem to differ only in the proportions between the three parts of which every science consists, namely (1) the registration of 'facts', which are what I have called empirical premises; (2) the inductive discovery of hypotheses, or logical premises, to fit the facts; (3) the deduction of new propositions from facts and hypotheses (p. 282).

I find this paper by Russell extremely revealing. Surely, one can find here and there in Russell's writings statements pointing at his h-inductivist position. For instance the h-inductivist position is defended in the Preface to *Principia Mathematica* (1910).[22] Moreover, it is found in the paper "Logical Atomism" where we read:

> When pure mathematics is organized as a deductive system – i.e. as the set of all those propositions that can be deduced from an assigned set of premises – it becomes obvious that, if we are to believe in the truth of pure mathematics, it cannot be solely because we believe in the truth of the set of premises. Some of the premises are much less obvious than some of their consequences and are believed chiefly because of their consequences. This will be found to be always the case when a science is arranged as a deductive system. It is not the logically simplest propositions of the system that are the most obvious, or that provide the chief part of our reasons for believing in the system. With the empirical sciences this is evident. Electro-dynamics, for example, can be concentrated into Maxwell's equations, but these equations are believed because of the observed truth of certain of their logical consequences. Exactly the same happens in the realm of pure logic [. . .]. Our reasons for believing logic and pure mathematics are, in part, only inductive and probable [. . .]. (Russell, 1924, pp. 325–326)

However, the 1907 paper is the clearest statement of Russell's h-inductivism.[23] For my goals the article is important in connection to the issue of explanation. If work in the foundations of mathematics is seen in analogy with the postulation of hypotheses in physical theory then it should follow almost of necessity that the organization of the body of mathematics provided by the foundational apparatus can be seen in terms of explanatory hypotheses on a par with the explanatory hypotheses in the natural sciences. Russell does not use these terms although he speaks of the foundations as providing an "organization of our knowledge" (p. 282). Only in "La théorie des types logique" the connection to explanation becomes explicit:

> Les raisons d'accepter un axiome, comme tout autre proposition sont toujours, en grande partie, inductives: c'est par exemple, le fait qu'on peut déduire nombres de propositions, qui sont de leur côté à peu près hors de doute; et qu'*on ne connait aucune manière aussi plausible d'expliquer la vérité de ces propositions*, si l'axiome était faux; et, enfin, qu'on n'en peut déduire aucune proposition qui soit probablement fausse. (1911, p. 300; my emphasis; the English manuscript, published in Russell, 1973 reads differently)

In any case, the connection is obvious and indeed it did not escape Gödel.

Gödel's writings on philosophy of mathematics often call attention to the analogy between mathematics and the natural sciences. In "Russell's mathematical logic" Gödel wrote:

> The analogy between mathematics and a natural science is enlarged upon by Russell also in another respect (in one of his earlier writings). He compares the axioms of logic and mathematics with the laws of nature and logical evidence with sense perception, so that the axioms need not necessarily be evident in themselves, but rather their justification lies (exactly as in physics) in the fact that they make it possible for these "sense perceptions" to be deduced; [. . .] I think that [. . .] this view has been largely justified by subsequent developments. (Gödel, 1944, p. 121 of CW)

Also well known are Gödel's analogies between empirical intuition and intuition in mathematics (see Gödel, 1947). But for my purposes the most interesting fact is the explicit connection Gödel drew between h-inductivism and explanation.[24] In his commentary on Gödel, 1944, Parsons remarks on the fact that although Gödel "does not use the language of explanation in the two passages, where he is most explicit about the justification of mathematical axioms through their consequence", it is nonetheless the case that "in spite of its lack of direct support, the interpretation in terms of explanation is difficult to refute" (Parsons, 1990, p. 108). Parsons was correct in stressing the importance of explanation and more evidence can be given to support his interpretation. In Mehlberg, 1960 we find mentioned a conversation between Mehlberg and Gödel on the issue of the role of foundations:

> The limited effect of the failure of Hilbert's program upon the dependability of the impressive cluster of mathematical theories which he tried to place on a common 'foundation' can be clarified by reference to certain relevant views of Gödel which he informally conveyed to me, some years ago, during a discussion we had at Princeton, N.J. According to Gödel, an axiomatization of classical mathematics on a logical basis or in terms of set theory is not literally a foundation of the relevant mathematics, i.e., a procedure aiming at establishing the truth of the relevant mathematical statements and at clarifying the meaning of the mathematical concepts involved in these theories. In Gödel's view, the role of these alleged 'foundations' is rather comparable to the function discharged, in physical theory, by explanatory hypotheses. Thus, in the physical theory of electromagnetic phenomena, we can explain why the sky looks blue to us under normal circumstances, and we are even able to produce the same phenomenon in the laboratory. Both the explanation of the physical phenomenon under consideration and its production under laboratory conditions are due to the logical fact that the statements describing the blue of the sky or that of an artificially produced area in the laboratory are theorems provable within an axiomatic system the postulates of which are concerned with hypothetical laws governing electro-magnetic phenomena, the composition of the atmosphere, etc. It would not occur to a physicist that these electro-magnetic assumptions which enjoy the role of postulates in an axiomatized, or axiomatizable physical theory, are more dependably known to be true than the pre-scientific phenomena (like the blue of the sky) which are being explained by being shown to be provable theorems in the aforementioned physical theory. Thus, the actual function of postulates or axioms occurring in a physical theory is to *explain* the phenomena described by the theorems of this system rather than to provide a genuine 'foundation' for such theorems. Professor Gödel suggests that so-called logical or set-theoretical 'foundations' for number-theory, or any other well established mathematical theory, is explanatory, rather than really foundational, exactly as in physics (pp. 86–87).

The analogy mentioned by Mehlberg is, as Parsons had already remarked, presupposed in what Gödel said in his philosophical papers although in the published work we never encounter such explicit mention of explanation. Remarkable is also the similarity to Russell's view, even in the choice of electro-magnetism as the example for an explanatory theory in physics.

II.4. *Lakatos on mathematical explanation*

The h-inductivist conception of mathematics was emphasized in a number of publications by Lakatos in the sixties (he calls it quasi-empirical). Lakatos tried to extend as much as possible these h-inductivist tendencies by proposing a view of mathematics as a quasi-empirical science, modelled upon the Popperian scheme of conjectures and refutations. In his papers Lakatos often speaks of explanation in mathematics. Lakatos emphasizes the opposition between the Euclidean model of science and mathematics and what he calls the quasi-empirical conception.[25] Whereas the former aims at reaching self-evident axioms from which to prove the remaining part of the system, the quasi-empirical methodology is fashioned according to a heuristic model of bold hypotheses which aim at "explanation":

> The methodology of a science is heavily dependent on whether it aims at a Euclidean or at a quasi-empirical ideal. The basic rule in a science which adopts the former aim is to search for self-evident axioms – Euclidean methodology is puritanical, antispeculative. The basic rule of the latter is to search for bold, imaginative hypotheses with high explanatory and 'heuristic' power, indeed, it advocates a proliferation of alternative hypotheses to be weeded out by severe criticism – quasi-empirical methodology is uninhibitedly speculative. (Lakatos, 1967, p. 29)

Lakatos argued throughout his work that the proper methodology of science and mathematics is quasi-empirical. Thus, according to him, in mathematics we also witness the development of theories which will put forward hypotheses to explain mathematical phenomena. There are at least three different contexts in which Lakatos raises the issue of mathematical explanation.

The first context concerns examples of global foundational activity. In connection with the foundations of the calculus he says in "Cauchy and the Continuum":

> What was revolutionary about Weierstrass' theory was that the known calculus could be fully explained, and even further developed, with Weierstrassian real numbers only [. . .]. It was the heuristic potential of growth – and explanatory power – of Weierstrass' theory that brought about the downfall of infinitesimals. (Lakatos, 1978a, pp. 48 and 54)

Another example often discussed by Lakatos is the Frege-Russell program in the foundations of mathematics:

> The Frege-Russell approach aimed to deduce all mathematical truths – with the help of ingenious definitions – from indubitably true logical axioms. It turned out that some of the logical (or rather set-theoretical) axioms were not only not indubitably true but not even consistent. It turned out that the sophisticated second (and further) generations of logical (or set-theoretical) axioms – devised to avoid the known paradoxes – even if true, were not indubitably true (and not even indubitably consistent), and that the crucial evidence for them was that classical mathematics might be *explained* – but certainly not *proved* – by them. (Lakatos, 1967, p. 30)[26]

The last distinction between explaining and proving might seem puzzling at first sight. According to Lakatos, in a Euclidean theory the indubitable axioms at the top of the deductive system prove the rest of the system, whereas in a quasi-empirical conception the true basic statements are explained by the hypotheses organizing the system (see Lakatos, 1967, p. 29). And in *Proofs and Refutations* he also remarks:

> It is well known that criticism may cast doubt on, and eventually refute, 'a priori truths' and so turn proofs into mere explanations. That the lack of criticism or of refutation may turn implausible conjectures into 'a priori truths' and so tentative explanations into proofs is not so well known but just as important. (Lakatos, 1976, p. 49, note 1)

In short, for Lakatos quasi-empirical theories are always conjectural, since the hypotheses which organize the system can only be conjectural and thus will at best provide an explanation for the set of basic facts which we hold as true and as the explananda of the system. These basic facts might even be derivable from the hypotheses but they cannot be proved in the Euclidean sense of the term (i.e. shown to be true from indubitable premises).

The second context in which Lakatos talks about explanations in mathematics concerns the idea of formal theories as explanatory accounts of informal mathematical theories. For instance, addressing a hypothetical case wherein some formal set theory proves a statement whose intended meaning is that there exists a non-Goldbachian even number while a number theorist provides an informal proof that all even numbers are Goldbachian, Lakatos says:

> If it [the informal proof] cannot be thus formalized in the formal set theory, the formal set theory will not [have been shown to] be inconsistent, but only to be a *false* theory of arithmetic [. . .]. Then we may call the informally proved Goldbach theorem a *heuristic falsifier*, or more specifically, an *arithmetical falsifier* of our formal set theory. The formal theory is false in respect of the informal explanandum that it had set out to explain; we have to replace it by a better one. (Lakatos, 1967, p. 36)[27]

Finally, we also encounter in Lakatos a traditional distinction between proofs that convince and proofs that explain. For instance, in *Proofs and Refutations* we read:

> Teacher: Do you then reject our proof?
> Omega: I do. The satisfactory proof has to explain the Eulerianness also of the 'great stellated dodecahedron'. (Lakatos, 1976, p. 62)

In short, the importance of explanations for the development of mathematical theories for Lakatos cannot be overestimated.[28] Indeed, he sees in explanation the key to success in the mathematical arena: "The battle between rival mathematical theories is most frequently decided also by their relative explanatory power" (Lakatos, 1967, p. 40).

II.5. *Hersh on proving and explaining*

In the wake of Lakatos' philosophy of mathematics (and a good dose of Quine)[29] several influential philosophers of mathematics have raised the issue of mathematical explanation. I have already mentioned in part I, Steiner's[30] and Kitcher's contributions.

I will conclude my survey of the h-inductivist tradition discussing a contribution on mathematical proof by Reuben Hersh. Hersh has defended, by building on Lakatos' insights, a fallibilist approach to the philosophy of mathematics (see Davis and Hersh, 1981, Hersh, 1997a; also Burgess and Ernest, 1997 for an appraisal). In two recent articles, Hersh (1993 and 1997) emphasizes the different role proofs play in research and in the classroom:

> The role of proof in class is not the same as in research. In research, it's to *convince*. In class, students are all too easily convinced! [. . .] The student needs proofs to *explain*, to give insight why a theorem is true. Not proof in the sense of formal logic (1997, p. 162).

An then again:

> Proof can convince, and it can explain. In research convincing is primary. In high-school or undergraduate class, explaining is primary (1997, p. 164).

Given Hersh's admiration for Lakatos' philosophy of mathematics the emphasis on proofs that explain was to be expected. However, he draws the line between convincing and explaining as having to do primarily with the distinction between research and pedagogical concerns. Lest we be misled by Hersh's characterization, I hasten to add that we have to take into account the fact that mathematical research itself is often driven by explanatory concerns. It is not my desire here to claim that these concerns are primary. I am happy to grant that explanatory concerns might emerge only after a "convincing" number of results have already been obtained. However, mathematicians do not stop there but aim, like all good scientists, to obtain proofs or theories that organize the facts in an intellectually satisfactory way. Very often this takes the form of an appeal to explanatory proofs or theories. So far, Hersh would probably be in agreement. However, and this is what I find misleading in Hersh's characterization, the "explanatory" concerns to be found in mathematical practice should not be confused with what might count as "explanatory" in the classroom. At the very least, an argument would have to be given to show that the "explanatory" concerns emerging in mathematical research always coincide with those needed in the classroom. I personally have serious doubt that the call for "explanation" in these two contexts is to be identified. For instance in the classroom we might want to explain by means of a picture-proof a result whose "deep" explanation might require some extremely complex mathematics. The latter might turn out to be a perfect explanation for the professional mathematician but not for the student. Another aspect of the same conflation occurs in Hersh, 1993:

> In my opinion, the main purpose of an upper division course [. . .] is to introduce new concepts to the student, and to explain them. A proof is a complete explanation. Sometimes a partial explanation suffices. (Hersh, 1993, p. 397)

According to the above, every (complete) proof counts as an explanation. But I think this is false both for the research environment as well as for the classroom. It also seems to me that the claim goes against both the spirit and the word of Hersh's own approach. In research, as we have seen, a proof might leave us so unsatisfied that it leaves us wondering why the result is true. Talking about the four color theorem Hersh himself says

> More than *whether* a conjecture is correct, we ask why it is correct. We want to understand the proof, not just be told it exists. (Hersh, 1997, p. 161; 1993, p. 390)

Moreover, it could be reasonably argued that a full proof might not serve as an explanation in the classroom. Indeed, often pictures or informal arguments will play an ideal "explanatory" role, whereas a full proof will be no explanation at all in that context.[31] After all, consider the discussion of the solutions to the two-pancake problem (the area of two plane pancakes of arbitrary shape can be simultaneously bisected by a single straight line cut of the knife) given in Davis, Hersh, Marchisotto, pp. 306–316. It shows how the teacher gave a first proof, which led to a crisis in the classroom, and then a second proof, which resolved the crisis. Thus, the first proof (although complete) could not function as an explanation, which shows that (complete) proofs are not always explanations. The example of the two proofs of the irrationality of $\sqrt{2}$ (pp. 331–333) also leads to the same conclusion ("Proof II seems to reveal the heart of the matter" [. . .] "Proof II exposes the 'real' reason"). The role of explanations in the classroom and in research, and their comparison, seems to me to be an important topic to pursue.

The following case study is intended to bring attention to a specific mathematical research program emerging from "explanatory" concerns in mathematical practice.

Part III. Pringsheim's *Vorlesungen über Funktionenlehre*

In the following I will present the approach to the theory of analytic functions propounded by Alfred Pringsheim in his *Vorlesungen über Funktionenlehre* (1925). The original approach to complex analysis defended by Pringsheim is based on the claim that only according to his method it is possible to "explain" a great number of results, which in previous approaches, in particular Cauchy's, remain mysterious and unexplained. However, in order to fully understand Pringsheim's contribution it is important to provide some background.

III.1. *Historical background on the theory of analytic functions*

Complex analysis was developed in the 19th century within the schools founded by Cauchy, Riemann, and Weierstrass respectively. To some extent there certainly were mutual influences between these schools in that they did not work in complete isolation and independently from each other.[32] At the same time, one can observe critical distance and rivalries between them. After Riemann's death, for example, his methods were repeatedly attacked by Weierstrass, which led to many mathematicians abandoning Riemann's methods (see Neuenschwander, 1981, pp. 96ff.). Weierstrass also had reservations concerning the ideas and results of Cauchy. He hardly ever quoted Cauchy in his courses at the University of Berlin, "and when the Paris Academy presented him with the first volume of the recently-published collected works of Cauchy, Weierstrass wrote not a line in response" (Kolmogorov and Yushkevich, 1996, p. 186). These tensions are noticeable in Pringsheim's work, who adopts the Weierstrassian approach sharing certain lines of criticisms vis-à-vis the alternative approaches to the theory of analytic functions. But he also put forward an attempt at bridging the three different methodologies. Before considering his position in more detail let us first set the stage by briefly sketching these three approaches.

One can trace the emergence of modern complex function theory to C. F. Gauss; this is sometimes done with particular reference to his well known letter to Bessel in 1811, which is a testimony to his knowledge of central results in complex analysis. However, since he did not contribute anything in print to the development of this new field, it is Cauchy who is usually credited with "officially" originating complex function theory, starting with his *Mémoire sur les intégrales définies* in 1814 (published only in 1827). The object of study are "monogène" functions, i.e. functions $f: C \to C$ that possess a complex derivative. The notion of differentiability is taken over from real analysis without great methodological worries. Cauchy showed that monogène functions satisfy the so-called Cauchy-Riemann differential equations.[33] Central to Cauchy's development is the notion of residues and the celebrated integral theorem. The special version of this theorem for disks is formulated as follows:

Let G be an open set and f (complex) differentiable in G. If γ describes a circle[34] ∂K such that the disk K enclosed by γ is contained in G, then we have

$$\int_\gamma f(z)\,\mathrm{d}z = 0 \qquad (1)$$

Among the French mathematicians that worked within the basic framework created by Cauchy, and further developed it, are P. A. Laurent, J. Liouville and V. Puiseux.

In Riemann's theory the geometrical point of view is essential – already in his groundbreaking dissertation of 1851. He defines "analytic function" like Cauchy, via the Cauchy-Riemann differential equations. The deeper motivation for studying analytic functions is the idea of "similarity in the smallest parts". Conceiving of functions as mappings between regions in the complex plane, or, more generally, between Riemann surfaces, Riemann showed that analytic functions transform small pieces of one region into similar small pieces of the other region, i.e. in modern terminology that analytic functions are *conformal*. The geometrical conception of complex functions was a new step in the history of complex analysis. "It can be compared in significance with the introduction of the graphs of functions originally represented by formulas" (Kolmogorov and Yushkevich, 1996, p. 207). Riemann surfaces together with the so-called Dirichlet Principle[35] are the basic tools in Riemann's approach.

Weierstrass puts the concept of power series at the center of complex function theory, which he developed in an algebraic manner. An "analytic" function is defined as a function that can be locally expanded into a convergent power series. The beginnings of this conception can be found in J. L. Lagrange but in contrast to earlier approaches which considered in general just one power series for each function, Weierstrass represented functions by whole systems of power series, organically interconnected by the process of analytic continuation. The methods of Weierstrass introduced a new standard of rigor to the study of complex functions. "In comparison with the Weiertrassian function theory, built on strictly arithmetical foundations, the Riemannian theory, still operating in part with intuition and unproven limiting procedures, was in a truly difficult position" (Neuenschwander, 1981, p. 97). Poincaré captured the essence of the opposition by saying that Weierstrass and the Berlin school "does not try to see but to understand" (Poincaré, 1898, p. 16).

The three methodologically quite distinct approaches turn out to be equivalent in the sense that the class of functions satisfying the Cauchy-Riemann equations coincides with the class of analytic functions in Weierstrass' sense. This equivalence was proved by Goursat in 1900, and after that the three theories were gradually unified.[36]

III.2. *Pringsheim's program*

Pringsheim begins his *Funktionenlehre* by positioning himself in the context of the different schools in function theory described above:

> The present account of the "foundations of the theory of analytic functions" published as the first series of my lectures on complex analysis [Funktionenlehre], differs essentially from all accounts I know in so far as it aims at an organic merger of *Weierstrass*' definition of an analytic function as a system of interconnected *power series* – which forms the basis of the present account – with the *Cauchy-Riemann* theory presupposing only *differentiability*. We achieve this by applying a *method of mean values*, used occasionally by *Cauchy* already and perfected by the author. This method which is, by the way, not at all alien to *Weierstrass'* approach either, allows one to prove, immediately upon introducing *Weierstrass*' concept of function, that *Cauchy's* "*monogen*", i.e. complex differentiable functions (which *Riemann* called just *functions* of one complex variable) coincide with those functions that can be expanded into a power series, i.e. the "*analytic*" functions in *Weierstrass'* sense. (Pringsheim, 1925, p. v)[37]

The extensional equivalence of the three classes had already been proved by Goursat in 1900. Accordingly, the advantage of Pringsheim's new approach does not lie in the mathematical result itself but rather in the fact that the new approach systematizes and "explains" a good number of results in an organic way. Pringsheim says:

> If we thus, from the very start provide a common basis for both concepts of function, which, according to their definition appear radically distinct, then we gain an advantage not to be underestimated over the common approach within Cauchy's theory involving complex integration right from the beginning – namely that basic insights which appear in Cauchy's theory as sensational results of a mysterious mechanism performing miracles as it were, *receive within our approach their natural explanation* [my emphasis, P.M.] by means of a reduction to the humbler efficacy of the four species [= arithmetical operations]. That's not to say that this great analytic tool should be excluded once and for all; rather we only claim here that if we don't use it prematurely, then the construction of the whole theory gains essentially in perspicuity. (Pringsheim, 1925, p. v)[38]

The treatment presented in the *Funktionenlehre* originated from a series of previous investigations dating back to the late 1890s. These investigations show that Pringsheim's approach was from the start motivated by a desire to achieve a certain organic development in the theory of functions of a complex variable. His methodological motivation is also very much in evidence in his *Vorlesungen über Zahlenlehre* (1916) with his emphasis on the use of "elementary methods". Hans Hahn, who reviewed the book for the *Göttingische gelehrte Anzeigen* (1919, pp. 321–347) challenged Pringsheim to provide a characterization of the notion of "elementary". Pringsheim came back to the issue in his paper "Elementare Funktionentheorie und komplexe Integration" (1920). Although he claimed that it would be hopeless to try to specify what counts as elementary at the outset he insisted on the advantages of an "elementary" treatment:

> Moreover, in my opinion there is more to the advantages of an "elementary" account, as the one we are concerned with here, than just the "aesthetic" effect. Rather, I believe that operating with *more elementary* and hence more perspicuous methods often yields a clearer understanding of the essence of the matter than the more mechanical methods of the infinitesimal calculus which work in a more obscure way. (Pringsheim, 1920, p. 149)[39]

And in the same paper he adds,

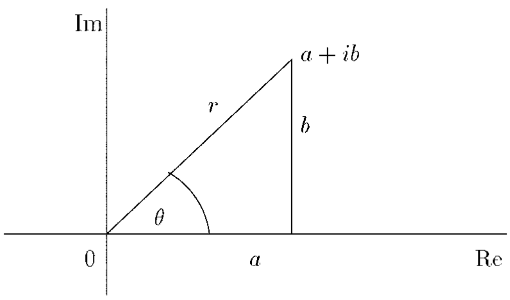
> I am of the opinion that [elementary complex analysis] has also additional important advantages: not only does the choice of the starting point and, in turn, the structure of the whole development seem to be much more natural and self-evident, as it were, than Cauchy's theory, but here it also becomes apparent again that the application of more elementary methods gives a clearer insight into the working of the fundamental results and how they are related to arithmetic, which usually gets completely obscured by the proof-shortening mechanism of complex integration. (Pringsheim, 1920, p. 152)[40]

I will try to convey the essence of Pringsheim's approach to complex analysis by developing some of the basic notions and treating a single example in detail. We need first of all to introduce the concept of "mean value of a function".

III.3. *The concept of mean value of a function*

Pringsheim introduces the mean value of a function in order to prove the mentioned equivalence between the Weierstrassian concept of analyticity and the Cauchy-Riemann concept of differentiability. Thereby he intends to provide right from the beginning a common basis for both theories and merge them in an organic way. Before giving the formal definition let us recall the geometric interpretation of complex numbers and their kth roots. This will aid the understanding of the intuition underlying this concept of mean value.

Every complex number $z = a + bi$ can be expressed uniquely in the form $z = r(cos\theta + isin\theta)$ where $\theta \in [0, 2\pi)$ and $r = \sqrt{a^2 + b^2}$.



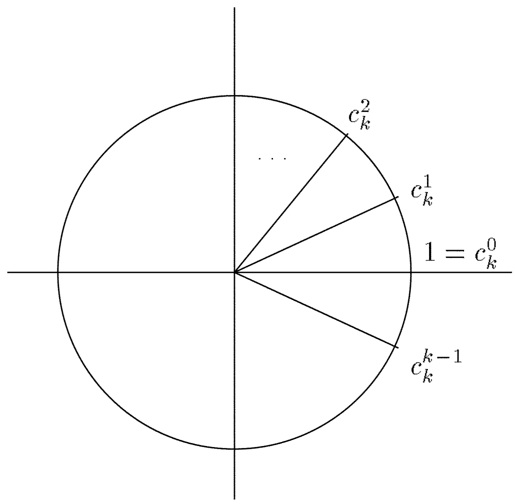
Let k be a positive integer. The k distinct kth roots of unity are given as follows:

$$1,\ cos\frac{2\pi}{k} + isin\frac{2\pi}{k},\ cos2\frac{2\pi}{k} + isin2\frac{2\pi}{k},\ \ldots,$$
$$cos(k-1)\frac{2\pi}{k} + isin(k-1)\frac{2\pi}{k}$$

One can easily verify that the kth power of each of these complex numbers equals 1 relying on De Moivre's Theorem, i.e. $[r(cos\theta + isin\theta)]^k = r^k(cosk\theta + isink\theta)$ and observing that $cos2n\pi = 1$ and $sin2n\pi = 0$ (for $n = 1, 2, \ldots$). The value $cos\ \frac{2\pi}{k} + isin\ \frac{2\pi}{k}$ is called the 'main kth root of unity', let's call it 'c_k'. Again using De Moivre's Theorem we see that the above kth roots of unity can be expressed as powers of c_k:

$$c_k^0,\ c_k^1,\ c_k^2,\ \ldots,\ c_k^{k-1},$$

All the kth roots of unity lie on the unit circle in the complex plane and since they are equally spaced on this circle one can conceive of them as the vertices of a regular polygon inscribed in the circle.



More generally, for circles with *arbitrary* radius r (and center at the origin) the vertices of an inscribed regular polygon of k sides are given by

$$rc_k^0,\ rc_k^1,\ rc_k^2,\ \ldots,\ rc_k^{k-1} \tag{2}$$

Our aim is to determine the average value of a (continuous) function f on a given circle Γ with radius r, and we proceed by considering a sequence of regular polygons, inscribed in Γ, of increasingly many vertices.

For each polygon the arithmetic mean of the values of f at its vertices $z_1, \ldots, z_N$ is just

$$\frac{1}{N}(f(z_1) + \ldots + f(z_N)) = \frac{1}{N}\sum_{j=1}^{N} f(z_j).$$

It can be shown that the sequence of these arithmetic means converges as N goes to infinity – which suggests strongly to take this limit as the *mean value of f on* Γ.

This is the main idea behind Pringsheim's definition of the concept of mean value of a function. However, instead of working with the *full* range of inscribed polygons (of vertices $N = 3, 4, 5, \ldots$), he restricts himself, for technical as well as methodological reasons, to those whose number of vertices is $N = 2^n$ (where $n = 2, 3, 4, \ldots$). Given this restriction and bearing in mind how vertices can be represented by roots of unity, as in (2), we finally arrive at the following definition.

Def. 1. Let $f: C \to C$ be a function bounded on the circle Γ_r with center at the origin and radius r. For $n = 1, 2, 3, \ldots$ let c_n denote the (2^n)th main root of unity. (This simplifies our notation, according to the outline above we would rather have to write, more clumsily, c_{2^n}.) Then we use the symbol '$\mathcal{M}_n(f, r)$' to refer to the arithmetic mean of the values $f(rc_n^0), f(rc_n^1), \ldots, f(rc_n^{2^n-1})$. Hence we set

$$\mathcal{M}_n(f, r) = \frac{1}{2^n}\sum_{j=0}^{2^n-1} f(rc_n^j).$$

Under the additional assumption that f is *continuous* on Γ_r (implying that f is even *uniformly continuous* on Γ_r), we can prove the existence of a limit of the sequence $\mathcal{M}_1(f, r), \mathcal{M}_2(f, r), \ldots$ for $n \to \infty$. For the details of this proof see Pringsheim 1896, p. 133ff. or 1925, p. 270ff. The proof is considerably simplified by the restriction to 2^n-gons in place of arbitrary polygons of n vertices which is exactly Pringsheim's technical reason for adopting it. Now we are in the position to define the *mean value* of a function.

Def. 2. Let f and Γ_r be as in Definition 1. Additionally, let f be continuous on Γ_r. Then we set

$$\mathcal{M}(f, r) = \lim_{n\to\infty} \mathcal{M}_n(f, r)$$

and we call $\mathcal{M}(f, r)$ *the mean value of f* on Γ_r.

The definitions of $\mathcal{M}_n(f, r)$ and $\mathcal{M}(f, r)$ can easily be generalized from circles around the origin to circles with arbitrary center (cf. Pringsheim, 1925, p. 274). Furthermore, by observing that for any constant q it holds that

$$\mathcal{M}_n(q\,f, r) = \frac{1}{2^n}\sum_{j=0}^{2^n-1} q\,f(rc_n^j).$$

$$= q\left(\frac{1}{2^n}\sum_{j=0}^{2^n-1} f(rc_n^j)\right) = q\mathcal{M}_n(f, r)$$

we infer as an almost immediate corollary of Definition 2 that for any constant q,

$$\mathcal{M}(q\,f, r) = q\mathcal{M}(f, r) \tag{3}$$

As another simple corollary concerning the *sum* of functions $f_0, f_1, \ldots, f_m$ we get $\mathcal{M}((f_0 + f_1 + \ldots + f_m), r) = \mathcal{M}(f_0, r) + \mathcal{M}(f_1, r) + \ldots + \mathcal{M}(f_m, r)$ provided all of the functions have the properties presupposed of f in Definition 1 and 2. This can be even further generalized to an *infinite series* $\sum_{m=0}^{\infty} f_m$ on the additional assumption that $\sum_{m=0}^{\infty} f_m$ converges uniformly on Γ_r. In that case we have

$$\mathcal{M}\left(\left(\sum_{m=0}^{\infty} f_m\right), r\right) = \sum_{m=0}^{\infty} \mathcal{M}(f_m, r) \tag{4}$$

This will already suffice to obtain an interesting result about mean values in the next section.

III.4. *The explanatory power of mean values. An example*

As an illustration of the claim that the use of elementary methods can serve to *explain in a natural way* "basic insights, which appear in *Cauchy*'s theory as sensational results of a mysterious mechanism" (Pringsheim, 1925, p. v), Pringsheim repeatedly discusses the use of mean values to represent the coefficients of power series.

In the context of this simple application we start with a function f given by a power series converging for $|z| < R$, i.e. inside the circle Γ_R, thus

$$f(z) = a_0 + a_1 z + a_2 z^2 + \ldots = \sum_{m=0}^{\infty} a_m z^m.$$

So on the one hand we clearly have $f(0) = a_0$. Let's now compute the mean value $\mathcal{M}(f, r)$ for some fixed r such that $0 < r < R$. We observe that the functions $a_m z^m$ satisfy all requirements for the applicability of (4). That's because every $a_m z^m$ is continuous and hence $\sum_{m=0}^{\infty} a_m z^m$

converges locally *uniformly* (e.g. on a circle Γ_r) inside of Γ_R. Thus according to (4) we get

$$\mathcal{M}(f, r) = \mathcal{M}\left(\left(\sum_{m=0}^{\infty} a_m z^m\right), r\right) = \sum_{m=0}^{\infty} \mathcal{M}(a_m z^m, r).$$

It remains to determine the values of $\mathcal{M}(a_m z^m, r)$, and since according to (3), $\mathcal{M}(a_m z^m, r) = a_m \mathcal{M}(z^m, r)$, we are left with the task of determining $\mathcal{M}(z^m, r)$ for $m = 0, 1, 2, \ldots$, but this can be done quite straightforwardly.

Let's first consider the case $m = 0$. By Definition 2, $\mathcal{M}(z^0, r) = \lim_{n\to\infty} \mathcal{M}_n(z^0, r)$ and by Definition 1 as well as by the fact that $z^0 = 1$ we have for all n, $\mathcal{M}_n(z^0, r) = \frac{1}{2^n} \sum_{j=0}^{2^n-1} (rc_n^j)^0 = \frac{1}{2^n} \sum_{j=0}^{2^n-1} 1 = \frac{1}{2^n} 2^n = 1$. So it follows that $\mathcal{M}(z^0, r) = 1$. Now let $m > 0$. Again, we have to consider $\mathcal{M}_n(z^m, r)$, i.e. $\frac{1}{2^n} \sum_{j=0}^{2^n-1} (rc_n^j)^m$. After simplifying the sum by pulling out the factor r^m, which yields $\frac{1}{2^n} r^m \sum_{j=0}^{2^n-1} (c_n^j)^m$, we observe that by switching the exponents the sum $\sum_{j=0}^{2^n-1} (c_n^j)^m$ can be written as a (finite) *geometric series*, namely as $\sum_{j=0}^{2^n-1} (c_n^m)^j$. Thus applying the general formula for geometric series

$$\sum_{j=0}^{n} q^j = \frac{1 - q^{n+1}}{1 - q}$$

to our series yields

$$\sum_{j=0}^{2^n-1} (c_n^m)^j = \frac{1 - (c_n^m)^{2^n}}{1 - c_n^m}.$$

But since c_n is the (2^n)th main root of unity, that's to say $c_n^{2^n} = 1$, we conclude that $(c_n^m)^{2^n} = (c_n^{2^n})^m = 1^m = 1$ and, in turn,

$$\frac{1 - (c_n^m)^{2^n}}{1 - c_n^m} = \frac{1 - 1}{1 - c_n^m} = 0.$$

Hence, putting everything together we have for $m > 0$

$$\mathcal{M}_n(z^m, r) = \frac{1}{2^n} \sum_{j=0}^{2^n-1} (rc_n^j)^m$$

$$= \frac{1}{2^n} r^m \frac{1 - (c_n^m)^{2^n}}{1 - c_n^m} = \frac{1}{2^n} r^m 0 = 0$$

and therefore also $\mathcal{M}(z^m, r) = 0$.

Relying on our results we are now finally able to determine $\mathcal{M}(f, r)$: $\mathcal{M}(f, r) = \mathcal{M}((\sum_{m=0}^{\infty} a_m z^m), r) = \sum_{m=0}^{\infty} a_m \mathcal{M}(z^m, r) = a_0 \mathcal{M}(z^0, r) + \sum_{m=1}^{\infty} a_m \mathcal{M}(z^m, r) = a_0 1 + 0 = a_0$.

This result has been derived by very elementary means and since, as already mentioned, $f(0) = a_0$ (and 0 being the center of Γ_r) our final equation $\mathcal{M}(f, r) = a_0$ amounts to "a significant statement about the course of values of the function $f(z)$: its values propagate in such a symmetrical way that the value at zero is retained again and again as the mean value (average value) of the different values of the function on an arbitrary circle $|z| = r$" (Pringsheim, 1925, p. vii).

Pringsheim's approach to the foundations of complex analysis shows one of the many ways in which explanatory concerns play a role in mathematical practice. Here, we are not investigating a "local" phenomenon, such as providing an "explanatory" proof for a theorem, but rather a "global" reorganization of a theory according to explanatory principles. This particular example reveals the role that unification and purity of methods might play in mathematical explanations. Pringsheim emphasized the unification of Cauchy's and Weierstrass' approaches that he obtained as a natural consequence of his approach. The issue of purity of methods arises in the restriction, as much as possible, of the development to "elementary" methods, so that the more analytical parts, prominent in Cauchy's approach, will not be applied prematurely. Pringsheim claims that the use of elementary methods corresponds to a gain in perspicuity and captures the essence of the matter. Finally, I should add that Pringsheim's approach did not have a large following. Contemporary textbooks in complex analysis do not follow his approach. It would be interesting to study why Pringsheim's approach did not catch on. However, this lack of success does not detract from the importance of Pringsheim's work as an expression of the urge, common to natural science and mathematics, to look for explanations of the facts.

Acknowledgements

I would like to thank my students Mark Goodwin, Johannes Hafner, Chris Pincock, and Richard Zach for many useful comments and help in tracking down interesting references. Johannes Hafner has been most helpful in helping me develop part III of the paper. His work and my work were funded by NSF Grant SES-9975628 (Science and Technology Studies Program; Scholar Award). Many thanks also to Richard Epstein, David Sandborg, Gila Hanna, Arnie Koslow, James Tappenden, and Ivor Grattan-Guinness for email correspondence on mathematical explanation. I am also very grateful to Philip Kitcher and Peter Achinstein for stimulating conversation on the topic of this paper.

Notes

[1] See Lakatos, 1976, 1978; Davis, Hersh and Marchisotto, 1995; Hersh, 1997a; Kitcher, 1984; Breger and Grosholz, 2000.

[2] See for instance the debate on the four-color theorem. Among others see Tymoczko, 1979; Detlefsen and Luker, 1980; Teller, 1980.

[3] See Brown, 1997, 1999 (especially chapters 3, 6, 9, 11), Barwise and Etchemendy, 1996; Allwein and Barwise, 1996; Giaquinto, 1992, 1993, 1994.

[4] See the general introduction in chapter 10 of Brown, 1999 and Mazur, 1997.

[5] Especially informative here is the recent debate that took place in the pages of *The Bulletin of the American Mathematical Society*. See Jaffe and Quinn, 1993; Jaffe and Quinn, 1994; Thurston, 1994; Atiyah et al. 1994. Of related interest is also the special issue of *Synthèse* in 1997 edited by Kanamori containing, among other things, Jaffe, 1997; MacLane, 1997; Rota, 1997; Mazur, 1997.

[6] Quine, Putnam, and Lakatos set the trend. For attacks on foundationism or foundationalism see Davis and Hersch, 1981 (2nd edition, with Marchisotto, 1995), Hersh, 1997a; Aspray and Kitcher, 1988; Tymoczko, 1986. On "justificationism" see Sandborg, 1997.

[7] For an introduction to Hilbert's program and further references see Mancosu, 1998. For a discussion that highlights some of the problems touched in the introduction see the review of Davis, Hersh and Marchisotto, 1995 written by Burgess and Ernest (Burgess and Ernest, 1997).

[8] See Salmon, 1990 for an extended bibliography and Ruben, 1994 and Pitt, 1988 for anthologies.

[9] Démonstrations causales. Bien des théorèmes sont susceptibles de différentes démonstrations. Les plus éducative sont naturellement celles qui font comprendre les raisons profondes des résultats qu'on se propose d'établir. En pareille matière la notion du domaine de causalité fournit une guide (Bouligand, 1932, p. 6).

Bouligand's account of the difference between explanatory and non-explanatory proofs is not always easy to follow and brings into play different intuitions. Central to his account are the notions of causality in mathematics, domain of causality, and the distinction between direct vs. indirect methods. Since Bouligand looked at several examples from analysis, algebra, and geometry, his work is a rich source of test cases for the topic of mathematical explanation.

In 1935 it was pointed out to Bouligand that his notion of causality was related to Cournot's distinction between *ordre logique* and *ordre rationnel*, which he acknowledged (see Mancosu, 1999 and Bouligand, 1935, p. 139). For Bouligand's methodological reflections see Bouligand, 1932, 1933, 1934, 1935, 1937.

[10] On voit nettement la différence entre ces deux manières de traiter une même question. La seconde seule nous donne une explication satisfaisante, précisément parce qu'elle s'exerce au sein même du domaine de causalité de la proposition à établir (Bouligand, 1932, p. 7).

In *La causalité des théories mathématiques* (1934) he also spoke of causal proofs as "compréhensive": "Nous avons vu l'idée de causalité mathématique s'introduire du fait qu'il y a des démonstrations causales (on pourrait dire aussi: compréhensives) et d'autres qui ne le sont guère, leur réussite semblant tenir à des hasards hereux" (Bouligand, 1934, p. 16).

[11] Whereas we have a stock example of scientific explanations, we cannot say the same about mathematical explanations. Another simple example that was used already in Steiner, 1978 is the following. Consider the usual proof by induction for the statement: $1 + 2 + \ldots + n = \frac{n(n+1)}{2}$.

Proof by mathematical induction:

For $n = 1$ the theorem is true.

Assume it is true for $n = k$.

Then $1 + 2 + \ldots + k + (k + 1) = \frac{[k(k+1)]}{2} + (k + 1) = \frac{(k+1)(k+2)}{2}$

This shows the theorem to hold for $k + 1 = n + 1$. Thus the theorem holds for all n.

Now compare that above proof with the proof Gauss gave.

Gauss' proof.

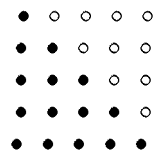
Consider $S(n) = 1 + 2 + \ldots + n$.

Then,

$$\begin{array}{l} S(n) = 1 + 2 + \ldots + n \\ S(n) = n + (n-1) + \ldots + 1 \\ \hline 2S(n) = (n+1) + (n+1) + \ldots + (n+1) \end{array}$$

Thus $S(n) = \frac{n(n+1)}{2}$

Finally, consider the following picture-proof:



By counting the points on the diagonals, starting from bottom left, the result is easily seen to hold. Many people find the second and third proofs explanatory but not the first. However, even on this simple example there is no unanimity. Hanna finds the Gaussian proof explanatory but not the first (1990, pp. 10–11). Brown says: "In the two number theory cases above [one of which is the theorem in question], a proof by induction is probably more insightful and explanatory than the picture-proofs. I suspect that induction – the passage from n to $n + 1$ – more than any other feature, best characterizes the natural numbers. That's why a standard proof by induction is in many ways better" (1999, p. 42).

[12] The geometer Thurston raises the same issue when he says: "There is a real joy in doing mathematics, in learning ways of thinking that explain and organize and simplify. One can feel this joy of discovering new mathematics, rediscovering old mathematics, learning a way of thinking from a person or text, or finding a new way to explain or to view a new mathematical structure" (1994, p. 11). Note that here the explanatory activity does not exclusively apply to proofs.

[13] L'ensemble de tous les déplacements constitute ce qu'on appelle un groupe; l'ensemble des déplacements qui laissent fixe un point de l'espace constituera un group partiel ou sous-groupe. C'est dans les rapports de ce groupe et de ce sous-groupe qu'il faut chercher l'explication de ce fait que l'espace a trois dimensions (Poincaré, 1895, p. 641). See also Nehrlich, 1979.

[14] A good case could be Witten's representation of the Jones invariant of knots using Chern-Simons field theory. See the remarks in Jaffe, 1997, pp. 139–140. For a treatment of the topic in the context of mathematical education see Hanna and Janke, 1999.

[15] The seven patterns he discerns by using the results of his CD-Rom search of *Mathematical Reviews* are:

Proven but unexplained results; counterexamples explained by restrictions placed on theorems; counterexamples explain restrictions put upon a theorem; explanations of "mathematical-empirical" data; physically applicable mathematical explanations; explanations of informal facts; explanations of theoretical analogies (see chapter 3 of Sandborg, 1997). Note that the case discussed in part three of this paper does not fall in any of the above classes.

[16] Richard Epstein rightly pointed out to me that the translation would not be an explanation for the intuitionist, for the translation in general does not preserve meaning. However, I think there is no contradiction here in claiming that the translation might function as an explanation for the classical mathematician.

[17] For instance, Karine Chemla in her work on Chinese mathematics says: "[Mathematicians] also prove in order to understand the statement proved, to know why it is true and not only that it is true" (1997, p. 229).

[18] "One normally thinks that everything that is true is true for a reason. I've found mathematical truths that are true for no reason at all. These mathematical truths are beyond the power of mathematical reasoning because they are accidental and random" (Chaitin, 1994, p. 4).

[19] Le souci de l'explication, la volonté tenace d'en étendre à tout prix le domaine, priment à tel point, dans la marche de la science, toute autre considération, que les vérités qui ont d'abord paru les plus plausibles, les faits les mieux acquis, sont mis de côté, intentionnellement oubliés en quelque sorte, quand surgit une théorie plus large, permettant de réduire en système, de relier par une déduction un nombre beaucoup plus considérable de phénomènes (Meyerson, 1921, p. 78; English translation, p. 63).

Of course, not everyone would agree. The physicist Ruhla writes: "It is often thought that science is an explanation of the world. Though this is an important feature, it is not the most characteristic: *the overriding priority in science is prediction*" (Ruhla, 1992, p. 1). On Meyerson's conception of explanation see Kelly, 1937.

[20] For Mill on explanation see Ruben, 1990.

[21] Were we to follow in detail the development of the above mentioned debates we would also be faced with one essential fact about explanation: what kinds of arguments scientists have been willing to accept as explanations have changed throughout time. This raises an important issue: Is it possible to formulate general criteria for scientific explanation that are independent of the historical shifts which have characterized the development of science?

[22] "The justification for this is that the chief reason in favour of any theory of the principles of mathematics must always be inductive, i.e. it must lie in the fact that the theory in question enables us to deduce ordinary mathematics. In mathematics, the greatest degree of self-evidence is usually not to be found quite at the beginning, but at some later point; hence the early deductions, until they reach this point, give reasons rather for believing the premisses because true consequences follow from them, than for believing the consequences because they follow from the premisses" (Whitehead and Russell, 1910, Preface, p. v).

In *Les Paradoxes de la Logique* (1906) we read: "En tout cela, la Logistique est exactement sur le même pied que l'astronomie par exemple, excepté que, en astronomie, la vérification s'effectue non par l'intuition mais par les sens. Les "propositions primitives" d'où partent les déductions de la Logistique doivent, si possible, être evidentes par l'intuition; mais ce n'est pas indispensable, et, en tout cas, ce n'est pas la raison unique de leur adoption. Cette raison est inductive, à savoir que, parmi leurs conséquences connues (y compris elle-mêmes), beaucoup paraissent à l'intuition être vraies, aucune ne parait fausse, et celles qui paraissent vraies ne peuvent pas se déduire (autant qu'on peut voir) de quelques système de propositions indémontrables avec le système en question" (Russell, 1906, p. 630).

[23] See Irvine, 1989 for an analysis of Russell's paper in the context of Russell's philosophy.

[24] The presence of explanatory issues in Russell and Gödel was first pointed out in Lakatos, 1967. However, whereas Lakatos was more interested in the "inductivism", and consequent "fallibilism", my emphasis is on the consequences of this position for explanation in mathematics. The parallel between Russell and Gödel is also discussed in Irvine, 1989.

[25] The literature on Lakatos is very extensive. For an informative discussion of Lakatos' philosophy of mathematics see Koetsier, 1991.

[26] "Let us draw some conclusions which Russell refused to draw. The infinite regress in proofs and definitions in mathematics cannot be stopped by a Euclidean logic. Logic may explain mathematics but cannot prove it. It leads to sophisticated speculation which is anything but trivially true" (Lakatos, 1962, p. 19). Compare this passage with Wittgenstein's *Lectures on the Foundations of mathematics* where Wittgenstein argues against the idea that Russell's logic can be seen as an explanation. "If Russell has connected mathematical procedures with logic, this might mean that he just translates them in a new language. But it is misleading to think this an explanation: to think that when we get down to predicates and predicative functions we see what mathematics is really about" (Wittgenstein, 1976, p. 271). Of course, the meanings of explanation in Lakatos and Wittgenstein appear to be different.

[27] On the heuristic pattern of deductive guessing, Lakatos says in the voice of Pi:

"The *first main pattern* is when naive concept-stretching outstrips the theory by far and produces a vast chaos of counterexamples: our naive concepts are loosened but no theoretical concepts replace them. In this case deductive guessing may catch up – piecemeal – with the backlog of counterexamples. This is, if you like, a continuous 'generalizing' pattern – but do not forget that it starts with refutations, that its continuity is the piecemeal explanation by a growing theory of the heuristic refutations of its first version [. . .] But it might happen that each single refutation or expansion of naive concepts is *immediately* followed by an expansion of the theory (and theoretical concepts) which explains the counterexample; 'continuity' then gives place to an exciting alternation of concept-stretching refutations and ever more powerful theories, of *naive concept-stretching* and explanatory *theoretical concept stretching* [. . .] in both of them [heuristic patterns] the power of the theory lies in its capacity to explain its refutations in the course of its growth" (Lakatos, 1976, pp. 93–94).

[28] Steiner, 1983 mentioned the second and third contexts of Lakatos' notion of explanation and relates it to his own theory of explanation (Steiner, 1978).

[29] "We may more reasonably view set theory, and mathematics generally, in much the same way in which we view theoretical portions of the natural sciences themselves; as comprising truths or hypotheses which are to be vindicated less by the pure light of reason than by the indirect systematic contribution which they make to the organizing of empirical data in the natural sciences" (Quine, 1958;

quoted in Lakatos, 1967). In this context it would be appropriate, of course, to also discuss indispensability arguments and their bearing on holistic views concerning mathematics and the natural sciences. But since our space is rather limited we have to leave that aside in this paper.

[30] "Philosophers have long pondered explanation in the natural sciences [. . .]. The growing acceptance, however, of continuity between the natural and mathematical sciences – urged by Quine, Putnam, and the present author – has prepared the way for what follows" (Steiner, 1978). In Steiner, 1983 the emphasis is on Lakatos.

[31] On pictures as explanations see Brown, 1999, p. 42.

[32] Cf. Neuenschwander, 1981 and further literature referred to therein on the interactions among the French school, Riemann, and Weierstrass.

[33] Letting $z = x + iy$ and $f(z) = u(x, y) + iv(x, y)$, the Cauchy-Riemann differential equations state: $\partial u/\partial x = \partial v/\partial y$; $\partial u/\partial y = -\partial v/\partial x$.

[34] More precisely, γ is a counterclockwise parametrization of the boundary of a disk K in G.

[35] According to this principle a function can be determined by a system of (presumed) necessary and sufficient conditions independently of an analytic expression. In 1870 Weierstrass pointed out that this principle can fail, the conditions used by Riemann do not always define a function.

[36] Modern textbooks in complex analysis usually proceed from the definitions of the line integral and the complex derivative of a function.

[37] Die vorliegende, als erste Abteilung meiner Vorlesungen über Funktionenlehre erscheinende Darstellung der "Grundlagen der Theorie der analytischen Funktionen" unterscheidet sich von allen mir bisher bekannt gewordenen wesentlich insofern, als sie, grundsätzlich aufgebaut auf der *Weierstraß*schen Definition einer analytischen Funktion als eines Systems ineinander greifender *Potenzreihen*, nichtsdestoweniger von vornherein eine organische Verschmelzung mit der *Cauchy-Riemann*schen, lediglich auf der Voraussetzung der *Differenzierbarkeit* beruhenden Theorie anstrebt. Dieses Ziel wird erreicht durch Anwendung einer gelegentlich schon von *Cauchy* benutzten und vom Verfasser vervollkommneten, übrigens auch dem *Weierstraß*schen Gedankenkreise keineswegs fernliegenden *Mittelwertmethode*, welche es ermöglicht, unmittelbar an die Einführung des *Weierstraß*schen Funktionsbegriffes den Nachweis zu knüpfen, daß die *Cauchy*schen "*monogenen*", d.h. im komplexen Sinne differenzierbaren Funktionen (von *Riemann* schlechthin und ausschließlich als *Funktionen* einer komplexen Veränderlichen bezeichnet) keine anderen sind, als die in Potenzreihen entwickelbaren, im *Weierstraß*schen Sinne "*analytischen*" (Pringsheim, 1925, p. v).

[38] Werden auf diese Weise die beiden definitionsgemäß gänzlich verschieden erscheinenden Funktionsbegriffe von vornherein auf eine gemeinsame Basis gestellt, so gewinnt man gegenüber der üblichen, von vornherein die komplexe Integration in Anspruch nehmenden Behandlungsweise der *Cauchy*schen Theorie den nicht zu unterschätzenden Vorteil, daß grundlegende Erkenntnisse, die dort als sensationelle Ergebnisse eines geheimnisvollen, gleichsam Wunder wirkenden Mechanismus erscheinen, hier ihre natürliche Erklärung durch Zurückführung auf die bescheidenere Wirksamkeit der vier Spezies finden. Damit soll jenes glänzende analytische Hilfsmittel keineswegs ein für allemal ausgeschalten werden, vielmehr wird hier nur die Ansicht verfochten, daß der Aufbau der ganzen Theorie durch den Verzicht auf dessen vorzeitige Anwendung wesentlich an Durchsichtigkeit gewinnt (Pringsheim, 1925, p. v).

[39] Im übrigen bin ich der Ansicht, daß die Vorzüge einer "*elementaren*" Darstellung, wie der in Rede stehenden, mit jener "ästhetischen" Wirkung keineswegs erschöpft sind. Vielmehr glaube ich, daß das Operieren mit den *elementareren* und darum durchsichtigeren Hilfsmitteln in vielen Fällen einen deutlicheren Einblick in das Wesen der Dinge gewährt, als die mechanischer und versteckter arbeitenden Infinitesimal-Methoden (Pringsheim, 1920, p. 149).

There are interesting parallels between Pringsheim's work and Bouligand's contraposition between algorithmic methods and direct methods (see for instance Bouligand, 1934). Exploring this topic would lead us too far from our present goals.

[40] Ich bin indessen der Meinung, daß sie [= die elementare Funktionentheorie] doch noch andere nicht unerhebliche Vorzüge besitzt: mir erscheint nicht nur die Wahl des Ausgangspunktes und die daraus sich ergebende Anordnung des ganzen Aufbaus viel natürlicher, ich möchte sagen selbstverständlicher, als bei der Cauchyschen Theorie, sondern es zeigt sich auch hier wieder, daß die Anwendung der *elementaren* Methoden zumeist eine klarere Einsicht in das Zustandekommender grundlegenden Ergebnisse und deren arithmetischen Zusammenhang ermöglicht, welcher durch den beweiskürzenden Mechanismus der komplexen Integration meist völlig verdeckt wird (Pringsheim, 1920, p. 152).

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