Some Thoughts and a Proposal in the Philosophy of Mathematics

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Abstract. The paper outlines a project in the philosophy of mathematics based on a proposed view of the nature of mathematical reasoning. It also contains a brief evaluative overview of the discipline and some historical observations; here it points out and illustrates the division between the philosophical dimension, where questions of realism and the status of mathematics are treated, and the more descriptive and looser dimension of epistemic efficiency, which has to do with ways of organizing the mathematical material. The paper's concern is with the first. The grand tradition in the philosophy of mathematics goes back to the foundational debates at the end of the 19^{th} and the first decades of the 20^{th} century. Logicism went together with a realistic view of actual infinities; rejection of, or skepticism about actual infinities derived from conceptions that were Kantian in spirit. Yet questions about the nature of mathematical reasoning should be distinguished from questions about realism (the extent of objective knowledge–independent mathematical truth). Logicism is now dead. Recent attempts to revive it are based on a redefinition of "logic", which exploits the flexibility of the concept; they yield no interesting insight into the nature of mathematics. A conception of mathematical reasoning, broadly speaking along Kantian lines, need not imply anti–realism and can be pursued and investigated, leaving questions of realism open. Using some concrete examples of non–formal mathematical proofs, the paper proposes that mathematics is the study of forms of organization—-a concept that should be taken as primitive, rather than interpreted in terms of set–theoretic structures. For set theory itself is a study of a particular form of organization, albeit one that provides a modeling for the other known mathematical systems. In a nutshell: "*We come to know mathematical truths through becoming aware of the properties of some of the organizational forms that underlie our world. This is possible, due to a capacity we have: to reflect on some of our own practices and the ways of organizing our world, and to realize what they imply. In this respect all mathematical knowledge is meta-knowledge; mathematics is a meta-activity par excellence.*" This of course requires analysis and development, hence the project. The paper also discusses briefly the axiomatic method and formalized proofs in light of the proposed view.

1. Different Dimensions in the Philosophy of Mathematics

There is a grand tradition in the philosophy of mathematics, stemming from the first three or four decades of the last century. At that time key figures, such as Frege, Russell, Whitehead, Hilbert, Poincaré, Weyl, Brouwer, to mention some, worked out and debated major foundational projects in mathematics. The competing positions are often characterized by the terms 'logicism', 'intuitionism' (or 'constructivism') and 'formalism', a somewhat misleading division, as I shall later explain. The classification has become a cliché, and as the debates have lost much of their original vitality, resistance has arisen to conceiving the discipline along these lines. There have been calls for more "down to earth" approaches, which focus on the way mathematics is actually practiced, and on historical and sociological aspects. Lakatos – who treats mathematics within the general framework of scientific research-programs – has been a forerunner in this "non-metaphysical" trend, which has gathered some momentum in the last ten years. To the extent that this tendency has broadened the perspective in the philosophy of mathematics – serving as a corrective to the neglect of epistemic, historical and sociological aspects – it should be welcome. But it cannot be taken seriously as a philosophical position, when it marches under the "anti-foundational" banner, that is, as a rejection of direct philosophical questions about the nature mathematics and mathematical truth. By 'foundational', I do not mean an approach whose goal is to provide mathematics with "safe foundations", but an inquiry into the nature of mathematical reasoning, its validity and the kind of truth it yields. The positions mentioned above are foundational in this respect; but so is Mill who construed mathematical truth as empirical (in the same sense that physics is empirical), and so is Wittgenstein, who denied the factual status of mathematical statements. Paying attention to epistemic, historical and sociological aspects in the practice of mathematics is complementary to, not a rival of foundational approaches. When it aspires to become a rival it results in bad philosophy, as well as a wrong phenomenal picture of the very practices it aims to describe. It leads to superficial positions that dismiss basic questions about the nature of mathematics, in favor of certain external descriptions of the activity.

Informative and interesting accounts can be provided by intelligent external observers; an atheist may comment knowledgeably on disputes concerning the divinity of Christ. But the analogy, for philosophy of mathematics, is misleading. What does "atheism" here mean? Anti-foundationalism is not a position that denies existence of mathematical facts; or, to put it in a different way, denies that mathematical statements are, independently of our knowledge, true or false. For such a denial is by itself a substantial foundational position, and its adherents are drawn into foundational debates when they are called upon to provide satisfactory accounts of their own. Only Wittgenstein, as far as I can tell, accepted the full implications of such a denial, across the board; he ended with an interesting, provocative, though untenable picture of mathematics. But Wittgenstein is a singularity in our story, and is not my present concern.

"Anti-foundational" positions merely reflect a certain tiredness of the old debates, a reaction to the overemphasis on formal logic, and an awareness (in itself true and valuable) of the gap between the formalized systems and the actual practice. It is therefore useful to remind ourselves how direct and natural basic questions are in the philosophy of mathematics. Quite aside from choosing a program or following a trend, these questions face us *qua* philosophers. Elementary questions about realism are a good example:

7 is a prime number, 6 is not; these are such trivialities that one is tempted to regard them as tautological consequences of a convention, rather than "facts". That 113 is a prime and that 111 is not is less obvious, but still trivial. Mersenne believed, in 1644, that $2^{67} - 1$ is prime, but he was proven wrong by Cole in 1903. It has been now verified

that $2^{24,036,583}$-1 (a number with about 724,000 digits) is prime. What kind of truths are these? It is remarkable that a claim such as the last can be easily understood by anyone with the most elementary arithmetical knowledge (a seventh-grader say), who has no idea how it was proved.[1] The gap between grasping the claim and understanding anything about its proof is even more striking in the case of Fermat's last theorem. "Everyone" can understand what the theorem says, but only an expert in this particular area can access the proof. "Everyone" moreover will initially find no wiggle place: either there are four non-zero numbers, x, y, z, n such that $n > 2$ and $x^n + y^n = z^n$, or there are none. A host of extremely simple questions, accessible to anyone with elementary knowledge, constitute open problems. It is not known, as I write this, whether every even number greater than 2 is a sum of two primes (Goldbach's conjecture); or whether there are odd perfect numbers (a perfect number is a natural number that is equal to the sum of its proper divisors, e.g., 6=1+2+3, or 28 = 1+ 2 + 4 + 7 + 14); or whether there is an infinite number of pairs of twin primes (a pair of twin primes is a pair of primes that differ by 2, such as 11, 13, or 17, 19; the conjecture that there is an infinite number of such pairs is the twin-prime conjecture). Do these mathematical questions have true answers, independently of our state of knowledge and methods of proof?

Consider, for comparison, a toss of a fair coin, whose outcome is unobserved and will never be known, because it is followed immediately by a second toss, or because the coin is destroyed. Did the coin land heads or tails? The answer is a hard fact beyond our knowledge. One might try to mitigate the gap between the fact and its knowledge by invoking a counterfactual: we *could have* observed the outcome and we *would have* then known the answer. But it is not clear that our belief in the objective evidence-independent outcome of the toss derives from our belief in the counterfactual, or vice versa: we accept the counterfactual because we consider the outcome an objective fact. In any case, the counterfactual and the fact-of-the-matter view reinforce each other. (I remark in passing that this connection has been utilized in modal interpretations of the theory of natural numbers.) As we move away from everyday scenarios, the appeal to counterfactuals becomes more problematic. Physical theory itself may imply the existence of certain events, which, in principle, cannot be known by us. Without going into this any further I can only say that our realistic conception of various parts of our world – from everyday events to events in the centers of stars – rests on a far-reaching web of theories, practices and beliefs. And at least in certain sectors of our framework the realistic conception is constitutive of the meaning of the concepts in question.[2]

In the case of mathematics – as exemplified by arithmetic – the situation differs in a crucial respect. The framework is, so to speak, transparent, and our thorough understanding of it does not seem to leave place for contingency: we may not know how things are, but whatever they are, they cannot be otherwise. The empirical factor, the opacity that endows everyday truths and the truths of science with the nature of hard

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[1]In principle, we can check every smaller number greater than 1, whether it is a divisor of this number. This, and similar brute-force methods (the sieve of Eratosthenes) would have taken more time than the life span of our sun.

[2]It is constitutive of the meaning of 'tree' that the proverbial tree that falls in the forest, falls independently of being observed. This by itself is not an answer to a radical skeptic who might argue that, in principle, we might be misguided in applying 'tree' in its ordinary meaning.

facts, is lacking. The mathematical setup seems to be a creation of the human mind, or a game whose possibilities have been determined by certain rules. This picture might encourage an attempt to reduce questions of truth to questions of provability, or to deny that 'truth' applies here at all. Yet the statement that the twin-prime conjecture is true and the statement that it is provable have altogether different meanings. The first is a clear sharp mathematical statement. The second depends on how we read 'provable'; if it means having a proof like those accepted in current mathematical literature, then the second statement is not a mathematical one. It is a clear mathematical statement only if 'provable' means provable in a given formal deductive system. In any case, our seven grader, who understands perfectly what the twin-prime conjecture is, has hardly a clear idea what it means for it to be provable (surely, one can understand the twin-prime conjecture without knowing anything about formal deductive systems). This still leaves open the possibility that, the difference of meaning notwithstanding, there is some deductive system, such that simple arithmetical statements of the kinds exemplified above are true just when they are provable. But this possibility is ruled out by the incompleteness and undecidability results. These results have revealed an unbridgeable gap between provability and truth. They make it extremely plausible that any system of mathematical reasoning accessible to humans, will fail to decide certain simple elementary questions. Further results (lower bounds in complexity theory) show that something like this holds even for simple statements about particular large numbers – statements which can, in principle, be decided, but whose minimal verification will necessitate a number of steps that puts it beyond human reach (say, more than the estimated life, in nanoseconds, of the sun, or of the galaxy, or of the universe). We cannot, of course, say of a particular problem that humans will not be able to solve it, because we do not have foreknowledge of what future deductive systems – in particular, what kind of axioms – will be used. But any formal system whose proofs can be effectively recognized will fail to decide some elementary statements, like the existence of a solution to a given system of diophantine equations.

Moreover, if a formalist appeals to a well-defined notion of 'proof', then the question, is there a proof for such and such a sentence? is itself a mathematical question, which, in principle, is not different from the question about the existence of a number with certain properties. For proofs can be encoded into natural numbers.

These observations are no more than opening moves in the lengthy analysis of realism in mathematics, moves that can be followed and responded to according to different strategies. In general, anti-realism with regard to actual infinities is expressed by rejecting the appeal to "either A or not-A", when this involves quantification over an infinite domain (e.g., either there is an odd perfect number, or there is no odd perfect number). An intuitionist will base the rejection of the excluded middle on a non-standard interpretation of the logical particles ('not', 'or', 'if…then…'), which leads to a different logic. The resulting theory will be weaker than the classical one, in the case of first-order arithmetic, but incomparable with the classical system in the case of real-number theory. A host of theories, motivated by different conceptions of real numbers and other higher-order entities, have been looked into.

In general, the extent of realism that one subscribes to is indicated by those statements one considers objectively true or false, independently of our state of knowledge. Anti-realism is therefore indicated by excluding certain statements from this class. It may or may not go with a different logic. For example, one can be a realist with regard to natural numbers, but reject the realistic picture for second-order logic on grounds of predicativity, as Feferman does. This position is expressed by replacing set theory with the weaker predicative version; it does not involve a change in classical logic.

A realistic conception rests, of course, on the *intended interpretation* (or intended model) of the language in question. It signifies an attitude that treats that language as sufficiently clear and univocal, so that questions of truth and falsity are completely settled. Thus, our understanding of the natural numbers seems quite clear, sufficient for convincing the great majority of mathematicians (and seven-graders) that the truth or falsity of the twin-prime conjecture is objectively determined by the interpretation of the numeric terms – the so-called standard model. Much more should and can be said, concerning the grounds for this conviction and concerning the fact that the situation is quite different with regard to set theory, where there is a broad spectrum of positions: from skepticism about the set of all reals, to full scale Platonism with respect to Cantor's universe. But this is not my subject here.

I conclude these brief observations about realism by pointing out that even constructivists must adopt a certain realistic attitude when it comes to quantification over finite but very large domains. Statements about finite domains can be decided, in principle, through finite checking; on the usual constructivist views, the excluded middle applies in these cases. But the number of steps can be so large as to render the checking practically unfeasible. Gödel's construction of a sentence that "says of itself" that it is not provable can be modified, so that it yields a sentence that "says of itself" that it is not provable in less than k steps, where k is a very large number. Using this technique we can get relatively short sentences whose minimal proofs are extremely long. Underlying this phenomenon is the fact that short names can denote very large numbers. Already in decimal notation numbers can be exponentially related to the length of their names. Using an exponentiation symbol, we can, furthermore, have numerical terms such as '$10^{100,000}$' or '$10^{10^{100,000}}$'. The exponentiation symbol can be eliminated by using the inductive definition of exponentiation in terms of addition and multiplication; the elimination of each occurrence will increase the length of the name by some constant but not more. Let t_n be a term of this kind, denoting the number n. Given a deductive system, e.g., ZFC (Zermelo-Fraenkel set theory with the axiom of choice), there is an arithmetical wff, $\varphi(x)$, involving only bounded quantifiers, such that: for all n, $\forall x < 2^{t_n}\varphi(x)$, is provable in the system, but every proof contains no less than $2^n/c \cdot |t_n|$, steps, where c is some constant and $|t_n|$ is the length of t_n. Since all quantifiers in the sentence are bounded, the excluded middle applies to it in intuitionistic arithmetic. There is no practical way to prove the sentence in the given system (unless the system is inconsistent).

Realism is not the subject of this paper; I focused on it, since it provides an example of an elementary question that calls for philosophical analysis. It can also provide an illuminating contrast to another dimension in the philosophy of mathematics, the dimension of efficiency and epistemic fruitfulness. Consider: (i) The extension of the system of positive numbers, 1,2,..., to the system 0,1,2, ..., of natural numbers obtained by adding 0, (ii) The extension of the system of natural numbers to the system of all integers, obtained by adding negative numbers, (iii) The extension of the system of integers to that of rational numbers, (iv) The extension of the system of real numbers to the system of complex numbers.[3] All of these might appear as "ontological enrichments", since they consist in "adding objects". But this conception is wrong. As far mathematical reality is concerned (the statements that have objective truth-values) there is no more to the natural numbers with 0 than there is to the non-zero ones, no more to the integers than to the natural numbers, and so on. Whatever is described in the system of natural numbers can be also described in terms of the non-zero ones. The addition of 0 is a formal move that rounds up the system and makes for a very efficient notation, but the enlarged system can be reinterpreted in terms of the original system: Let 1 play the role of 0, 2 – the role of 1, and so on; define addition, multiplication and any other function accordingly (i.e., for non-zero m, n, put $m +' n = ((m-1)+(n-1))+1$, $m \cdot' n = (m-1)\cdot(n-1)+1$). Statements about the larger system can be then rephrased as statements about the smaller one. The same goes for the addition of negatives, which can be accomplished by considering all pairs (i, n), where i is either 0 or 1, and n is a natural number that is strictly positive if $i>0$; our previous natural numbers are now identified with the pairs $(0,n)$ and the negative numbers with the pairs $(1, n)$. Rational numbers can be defined in the well-known way as pairs of integers (under the congruence that identifies fractions of equal value), and complex numbers as pairs of reals. These easy reductions lead to obvious translations of statements from one system into the other.[4] (The move from the rationals to the reals is, of course, quite another matter; there is all the difference in the world between the two.)

Yet, these extensions constitute momentous developments in the history of mathematics. It is only with the hindsight of the 19^{th} and the 20^{th} centuries that we can describe them as we just did. They constitute advances in several respects: notational, algorithmic, and epistemic. The so-called "discovery of 0" and the importance of '0' for arithmetical notation is a well-researched story that needs no further comment. But the inclusion of 0 as a natural number was far from obvious even at the end of the 19^{th} century. In Dedekind's system (from 1888), the natural numbers start with 1, as they do in Peano's earlier work (from 1890); Cantor begins his ordinals with 1, though he sometimes recognizes the technical advantage of adding an additional zero-element. Negative and imaginary numbers have made their way into mathematical practice gradually during hundreds of years. First they were used as computational props – auxiliary symbols, which do not stand for numbers but are treated *as if* they did. They were therefore regarded with suspicion and were granted first-class citizenship only in the 19^{th} century, when it was realized that they can be modeled as pairs of *bona fide* numbers, as indicated above. Their incorporation

[3]I use 'system' in a broad sense: an underlying class of objects with various mathematical concepts that are associated with them. In the case of numbers, these will include various functions and relations. I assume that the ordering is included so that we can recover the non-negative integers from the integers, via '$x \geq 0$', without appeal to non-trivial theorems. Similarly, I assume that we have in the system of rational numbers a predicate 'x is an integer' and that in the system of complex numbers we have 'x is real'.

[4]This type of semantically-based translation leaves the logic intact. It should be distinguished from the syntactic type that preserve provability relations, exemplified by the translations between classical and intuitionistic first-order arithmetic. The second type does not amount to an "ontological reduction". If we want to get an "ontological reduction" using a syntactic approach we should also require that the translation should commute with the logical connectives. This blocks the translation from classical to intuitionistic systems.

had far reaching consequences, far beyond the computational aspect. It amounted to a restructuring of the mathematical space: the way a mathematician organizes his or her material. Although there is no more to the integers than there is to the natural numbers, viewing the natural numbers as part of the integers is altogether different from viewing them by themselves. The difference in epistemic organization is even more striking in the case of complex numbers. Although complex numbers are no more than pairs of reals, complex analysis – with its underlying two-dimensional geometric interpretation – is a different subject than real analysis, with different heuristics, different natural questions, and different techniques.

Philosophy of mathematics should address both of these dimensions: the ontological – as it is revealed by questions about realism, the reductions of systems, and the relative strength of various theories – and the epistemic, which has to do with ways of organizing the mathematical space. The first can be handled with the help of precise technical tools, and here mathematical logic is indispensable. This is a kind of philosophy that merges with its subject matter, since metamathematics itself is being treated mathematically. A realistic view can sometimes be stated with precision that is impossible in any other philosophical investigation, e.g., "I think that all first-order statements about natural numbers have objective truth-values, but not all higher-order statements do." The second dimension is far less technical and far less precise. Naturally, it helps itself more to phenomenal descriptions. The great difficulty here is in finding the right questions and the right parameters that will make possible a systematic approach.

The philosophical question about the nature of mathematics goes back at least to Kant, who posed it explicitly and gave an elaborate account. The question of realism, on the other hand, is relatively a new comer, an outcome of the foundational debates at the turn of the last century.[5] The two questions are, of course, related, but not to the extent that an answer to one must always determine an answer to the other. For example, Frege was a realist with regard to arithmetic as well as geometry; but he thought that arithmetical truth is logical and geometrical truth is not.[6] In the other direction, one's view regarding the nature of mathematics may leave undetermined which parts of set theory one conceives realistically. On the whole, the question about the nature of mathematical truth and the source of its validity is looser and less direct than the question of realism, since it depends more on other presupposed debatable categories. Thus, Kant used the double distinction of *a priori* versus *a posteriori* and analytic versus synthetic, as well as other conceptual tools in his philosophical arsenal.

[5]Questions about actual, versus potential infinity and skepticism with regard to the former are at least as old as Galileo. But only at the turn of the last century did the debates take place, which later crystallize into well-defined positions in terms of realism. The present day use of 'Platonism' in the philosophy of mathematics stems from these debates, whatever its affinity is with Plato's original philosophy. Finally, mathematical realism has to be distinguished from the subject of the old Newton-Leibniz debate regarding the reality of space. The question whether statements in pure geometry have objectively determined truth-values did not arise. I think both Newton and Leibniz would have answered it positively. The debate was rather about the physical interpretation of geometry. With hindsight, it can be rephrased as being about the factual status of certain statements; but these statements involve reference to physical bodies.

[6]He declared himself a Kantian with respect to geometry, though it is not clear that this was an accurate description of his view of geometry, as revealed in his debates with Hilbert.

2. The Grand Tradition

In Kant's account mathematics derives from a sort of intuition, a form of grasping that cannot be reduced to logic. At the turn of the last century the main opposite view was logicism, which claimed that mathematics (or, at least, arithmetic) *is* reducible to logic. This was one of the main points of contention, and was viewed as such by the people involved.[7] The other, more obvious issue of contention was infinity. Those who, broadly speaking, toed the Kantian line accepted potential infinity as legitimate in mathematics, but rejected, or were suspicious of actual infinities; to this group belonged, among others, Hilbert, Brouwer and Poincaré. Logicists, on the other hand, accepted the infinities provided by Cantorian set theory (in this or that version of it) as fully meaningful and legitimate. They thought, wrongly as it turned out, that set theory can be reduced to pure logic; their acceptance of actual infinity fitted nicely within the logicist view.

Subsequently, the issue was given a precise form: Can we use quantification over the natural numbers in forming statements that are objectively true or false? The first point of contention was thus about the nature of mathematics, the second was about realism.

On both issues Hilbert should be grouped with Poincaré and Brouwer. But unlike the intuitionists he did not propose a revision of logic but a different kind of restriction: finitism. Hilbert did not present his conception formally, but it is not difficult to see what he was aiming at. PRA (Primitive Recursive Arithmetic), or something like it, is a plausible candidate; this is a very simple system, more restrictive than intuitionistic arithmetic, which uses classical logic but disallows the usual quantification over the natural numbers. Hilbert is often associated with "formalism". But the upshot of his position was that his proposed formalization was a tool in the service of finitism. His idea was ingenious in its simplicity: Given a mathematical system that uses actual infinities, formalize it. If the resulting formal system is consistent, then general finitistic equalities that are provable in it, $f(x, y,\dots) = g(x, y,\dots)$ – where f and g are say, primitive recursive and 'x', 'y'... range over the natural numbers – must be valid; it cannot have a counterexample, $f(m, n,\dots) \neq g(m, n,\dots)$, because any particular inequality can be verified and this would lead to contradiction. Hence the consistency of the formal system means that we can use it safely to derive valid finitistic results. If, moreover, we have a consistency proof that satisfies finitistic standards, then we have also an effective way of transforming non-finitistic proofs of general finitistic equalities into finitistic ones. This will show that, in principle, actual infinities can be eliminated; their value is instrumental.

Roughly speaking, the constructivist views (among which I include Hilbert's position) viewed infinity as reflecting an unbounded process of iterative constructions, grounded in an intuitive grasp à la Kant. The principle of predicativity belongs here as well. Predicative set theory uses classical two-valued logic and is realistic about the natural numbers: it accepts the standard model. But it conceives the subsets of the natural numbers not as pre-existing things, but as entities that are constructed in an ongoing process of definition. The constructive process proceeds bottom-up, in a well-ordered sequence, in which earlier sets can be used in constructing later ones. Russell subscribed to predicativity and incorporated it into the basic setup of the *Principia*; but this undermined logicism, since

[7]In particular, Poincaré, in his *Science et méthode* represents the dispute as a momentous struggle between Kantians (in a broad sense of the term) and logicists.

the system is then insufficient for the purpose of reconstructing arithmetic. Therefore an additional axiomatic scheme, the so called reducibility axiom, was included to undo the limitations imposed by predicativity. Russell frankly admitted that the axiom was not logical and hoped to eliminate it.

The views characterized by the three standard terms mentioned at the beginning of the paper had different fates. Russell's attempt to eliminate his reducibility axiom failed; the axiom cannot be dropped (this was first proved by Myhill). More generally, one can also see that Gödel's results undermine logicism, but I shall not go into this here. In recent years there have been attempts to resuscitate defunct logicism through artificial respiration – a broadening of what was originally conceived as logic. An assortment of arguments – some bad, others with some appeal perhaps – has been invoked in order to classify under "logic" substantial portions of set-theory. This line provides no insight about mathematics, but is rather a thesis about logic, which arguably redefines "logic" in a rather uninteresting way. If anything, the attempt only shows how pliant a concept can become under the expert massaging of some skilful philosophers.

While Hilbert's project has been proven unfeasible by Gödel's results, finitism remains a philosophical option. This is the view that actual infinities are no more than useful fiction for deriving valid finitistic theorems. The thesis that the use of actual infinities leads to valid finitistic theorems is equivalent to the claim that the use of actual infinities produces no contradiction, and this, by Gödel's results, must remain an unprovable belief. Finitists are called upon to give some account for the prevalence of this belief – the belief, which presumably they share, that systems used in current mathematics, such as Peano arithmetic, or analysis, or set theory are consistent. The obvious gesture at "inductive confirmation" (no contradiction found so far) is not convincing. Past experience is, to be sure, crucially relevant, but is not sufficient by itself. We are not concerned here with a black box whose output, so far, has been consistent. We know how this "black box" functions. Whatever philosophical view we espouse, we understand quite well the reasoning used in the standard systems and our convictions about consistency stem from this understanding.

The various positions that come under "constructivism" remain, on the whole, viable positions. I do not intend to discuss them at any detail. The problem of accounting for the general belief in the consistency of classical systems arises here, as it arises for finitism, but is less of a challenge – since these systems are richer. In certain cases the belief can be fully accounted for by relative consistency proofs; e.g., any proof of a contradiction in classical first-order arithmetic can be effectively converted to a proof of a contradiction in the corresponding intuitionistic system. An intuitionist can therefore justify, from his own point of view, the belief that the classical system is consistent.

In the first two decades of the last century, constructivists had extremely high expectations of transforming mathematical practice. These hopes petered out. Some constructivist programs continue to produce active research; also intuitionistic mathematics has found new uses in the context of theoretical computer science. But the impact of constructivism on mathematical practice as a whole is hardly noticeable. The reason for this is not hard to discover: the use of actual infinities, within the framework of classical logic, makes for a simpler more convenient organization of the mathematical space, and is therefore more effective. When classical results can be reproduced within constructivist systems the proofs are usually less transparent, and when the systems diverge, as in the case of intuitionistic analysis, the intuitionistic one is far more complicated. Abraham Robinson, who, as a philosopher, rejected actual infinities altogether, had a considerable corpus of mathematical works in diverse fields, all within the classical systems. He regarded actual infinities as useful fiction, but was not willing to give the fiction up. Other, less philosophically inclined mathematicians do not bother with the problem.

In its grand tradition period, philosophy of mathematics was produced by researchers with comprehensive foundational projects, who saw themselves responding to the discipline's needs, continuing the overhaul that took place in the 19^{th} century. Most were active mathematicians whose philosophical insights derived from an intimate acquaintance with their subject. The combination of philosophy and mathematical technique is also represented by the more philosophical figures of Frege and Russell, who produced ground breaking technical innovations. In the second half of the last century this is no longer the case. There are mathematical logicians, who produce technical work – either motivated by philosophical questions, or with clear philosophical implications – and there is a community of less-technical, or non-technical philosophers who address their subject from some general philosophical perspective. Works that impinge on the question of realism – and here I include the various constructivists positions – belong mostly to the first group. To this group belong also works that measure mathematical theorems by the logical theories within which they can be derived, this is the Friedman-Simpson project of reverse mathematics. In the second group we find works that explain and clarify historical positions, as well as works that try to tell some sort of philosophical story about current mathematics. Many of the latter adopt a deliberate non-critical position, intending to give a philosophically palatable description of the reigning practice. We get valuable insights, as in some versions of structuralism, or useful proposals, such as the modal approach (which can also involve technical work). But sometimes one cannot avoid the impression of philosophical decorative annotations, or, in other cases, of ideas moving in a closed circle of a philosophical game that adds little to our understanding. This danger is hardly avoidable in philosophy, where opinions might vary almost as they vary in art criticism.

3. The Proposal

I propose that, using concrete simple examples, we take a hard look at the nature of mathematical reasoning and the source of its validity. At least provisionally, the subject can be treated separately from questions concerning realism. At the turn of the last century, those who conceived mathematics on, broadly speaking, Kantian lines were also those who adopted an anti-realist position with regard to actual infinities. But this is not at all necessary. Suppose that our reasoning in arithmetic derives from an intuitive grasp of finite strings of abstract strokes, arranged in an unending sequence (Hilbert's position). Does this imply that statements in first-order classical logic, with quantifiers ranging over the natural numbers, do not have objective truth-values? It does not. It also does not imply that they do. Either decision is an additional step. Gödel explained our mathematical knowledge by appealing to some sort of intuition, or perception. This did not prevent him from being an extreme realist with regard to the set-theoretical universe. One's position regarding realism will, as a rule, be sensitive to developments in mathematical logic, and, possibly, even to physical theory. There are two kinds of possible investigations. One can have a definite conviction about what should and should not be conceived realistically, that is, about the sentences that have objective truth values and those that do not; one can develop a line of argument for this view, which conceivably can also involve some technical work. Or one may suspend judgment and contend oneself with a systematic analysis of possible positions. I myself find questions of realism extremely hard and I opt for the second approach. Realism however is not the subject of the paper.

I shall proceed with some simple examples of mathematical reasoning, which come mostly in the form of mathematical puzzles. They are easily understood by "everyone" (our proverbial seventh-grader); their solutions are difficult, but, once given, are obvious and utterly convincing ("how neat, why didn't I think of it before?"). They can teach us something significant about mathematical reasoning. There are scores of such problems. Here are three.

1. A Tiling Problem

A domino-tiling of an area, made up of non-overlapping squares of equal size, is a covering of the area by rectangular domino pieces, in which every domino covers two adjacent squares; there are no overlapping dominos, the whole area is covered and nothing else. From now on 'tiling' refers to domino-tilings. Consider a division of a square by a grid, into 8×8 squares: 8 rows and 8 columns; call this a "standard board" (fig. 1). Trivially, it can be tiled by using 4 dominos to tile each row. An example of a more complicated tiling is given in fig. 2.

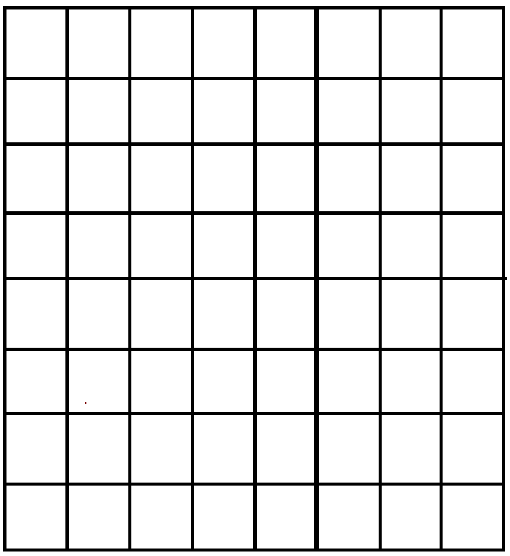

Figure 1

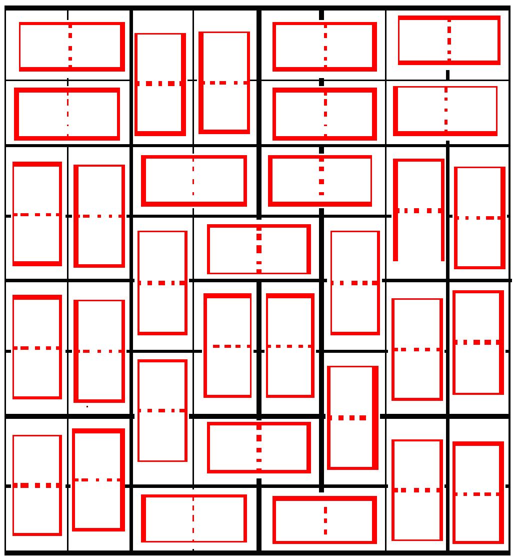

Figure 2

Consider now boards obtained by removing squares from the standard board. Since each domino covers two squares, a board that can be tiled must have an even number of squares. The removal of a single square yields a board with 63 squares, which cannot be tiled. Suppose we remove two: the bottom-left corner and the top-right corner (fig. 3) can the remaining board, which consists of 62 squares, be tiled? This is the puzzle.

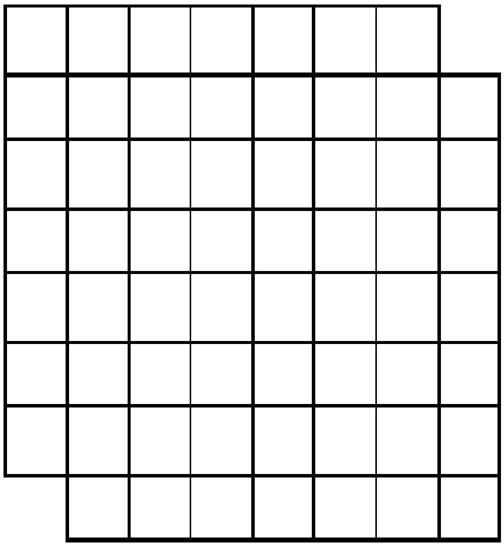

Figure 3

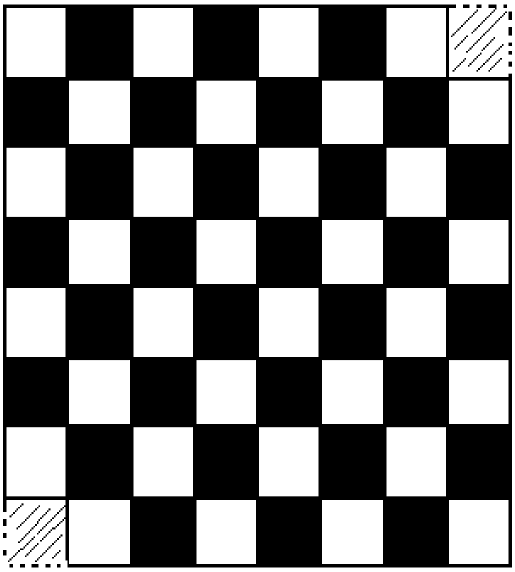

Figure 4

The solution uses an ingenious idea. Color the squares of the standard board black and white, alternating the colors as in a board of chess. Our removed squares are the endpoints of a main diagonal; they have the same color, say they are black (fig. 4). Then the mutilated board consists of more white squares than black. Since each domino covers adjacent squares, and adjacent squares have different colors, the covered area must have equal numbers of white and black squares. Hence the mutilated board cannot be tiled.

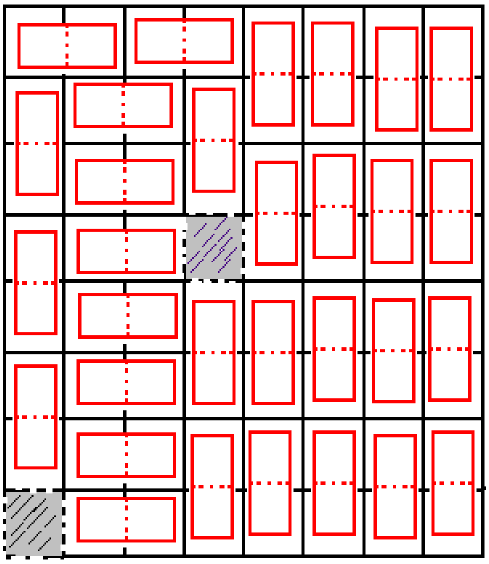

Figure 5

As fig. 5 shows, there are boards obtained by removal of two squares, quite apart from each other, which can be tiled. The removed squares must of course have different colors. Is this condition sufficient for the existence of a tiling? It can be shown that the answer is yes (it holds moreover for any standard board with an even number of rows).

Our ingenious trick is therefore more than a trick; it gives us a necessary and sufficient condition for the existence of a tiling after a removal of two squares. I note this in order to put our trick in wider perspective; it is not part of the example and is not needed for the philosophical points I am going to make, but it shows something about the way mathematics poses questions and progresses.

I do not know the history of this puzzle; some books present it as a question about chess boards, providing thus the major step in the solution and making it easy and not very interesting (an 8×8 board may give a hint already, a 10×10 board is preferable). For the purpose of the discussion let us ignore the history and treat it as a mathematical question. The following are noteworthy features:

1. It is an elementary problem whose understanding hardly requires mathematical training. Tiling games are easily taught to first-graders (and below) and it does not take much to understand the goal of tiling a given board. Understanding the impossibility proof requires more, but should pose no problem to anyone who has minimal grasp of chess-coloring and of concepts such as *equal numbers of black and white squares.*

2. The proof exemplifies the methodology of solving a problem by adding structural elements not found in the given description. In this it resembles some famous impossibility proofs, e.g., the impossibility of a ruler-and-compass trisection of an angle. There, the added structure consists in automorphism groups, here it consists in the coloring.

3. The trick works like magic. The problem now seems easy, but make no mistake , this is a hard problem! It takes considerable ingenuity to invent such a trick (unless one has seen already similar devices).

I think that the problem is as clear, and the proof as certain as we can ever get in mathematics. As far as clarity and validity are concerned we have hit rock bottom. A more formal proof, which requires a more formal rephrasing of the question, will add nothing in this respect. Although "everything" can, in principle, be recast in set theoretic terms, the route from the question, as stated above, to some formal rephrasing is far from clear. Let us try. The first suggestion is to represent boards as relational structures consisting of (i) a finite set (whose members are the squares), (ii) the adjacency relation, consisting of all pairs of adjacent squares. A tiling is then a partition of this set into a family of disjoint sets, each of which consists of two adjacent members. When the standard board is thus viewed, we loose any insight into the situation; even the existence of trivial tilings becomes a fact in need of careful checking. The impossibility proof will then involve the following steps: We divide the given set into two disjoint subsets (the "white squares" and the "black squares"), for which we show that adjacent elements cannot belong to the same division member. We also show that the two deleted squares belong to the same division member. From this we can derive in set theory the impossibility claim. But the definition of the two-fold division and the alleged truth of the claims are complex affairs into which we have little insight. Note that our non-formal proof generalizes trivially to any even-sized board and yields a general statement for all boards of $2n\times 2n$. In the suggested formal version this is altogether obscure.

We can do better by representing our standard boards not as arbitrary sets but as Cartesian products, $A \times A$, where the set A is also provided with a "neighboring relation". Adjacent members of $A \times A$ are pairs (a, b), (a', b'), such that either $a = a'$ and b and b' are neighbors, or vice versa. We can improve further by letting the set A be ordered; yet the coloring remains a complicated affair. The way that leads to a comprehensible set-theoretic proof (where "set-theory" includes also arithmetic) is the following. The board is $A \times A$, where $A = \{1,\ldots,8\}$, or in general $\{1,\ldots,2n\}$. Two elements, (i, j), (i', j'), are adjacent if $i = i'$ and $|j - j'| = 1$, or vice versa. We partition the squares, (i, j), into two subsets, the odd ones and the even ones, according as $i + j$ is odd or even. Modeled thus, the original proof can be recast as a formal one, which is comprehensible.

Even the comprehensible formal version may give pause to non-mathematicians, who have no problem with our original solution. It also takes some mathematical training to realize that the formal version is a faithful translation of the original into a different setting. Yet, our original argument needs no translation. It is as valid a piece of mathematical reasoning as any.

What is then the value of a translation into set theory, or into some other accepted mathematical framework? Before we consider this, let us consider a more fundamental question: From where does our original grasp of the mathematical problem derive? The required understanding of concepts such as *area made of non-overlapping squares,* and *tiling* can be acquired through simple games, or – in the case of grown ups – through explanations backed by a couple of examples. The number of games and examples is finite, yet we find no difficulty in generalizing definite features, from very few cases to a potentially infinite collection; this makes the reasoning that proves theorems possible. Wittgenstein characterizes the phenomenon in terms of rule following: somehow we acquire the ability to follow the "right" rule. On his view it would be wrong to say that we recognize certain patterns, since rule-following generates the very patterns we come to recognize; rule following is an irreducible primitive that determines arbitrarily what we consider as "being of the same kind". I have no objection to speaking in terms of rule following, as long as this leaves place for genuine discoveries, the discovery of non-trivial facts that are implied by the rules we follow. The way we organize our conceptual space – having a world that is informed, among other things, by areas, squares, and tilings – may be arbitrary (let us grant this); yet, *in a world thus organized* the mutilated board has no tiling, and *that* is not arbitrary, but a substantial truth. Wittgenstein denies this possibility because he denies that we can discover truths by reflecting on some of our rules; this kind of meta-level perspective has no place in his account. As he sees it, the impossibility of a ruler-and-compass trisection is not a discovery of a truth, which is implied by our geometric conception, but a kind of choice that we made after seeing the impossibility proof.[8] The choice can be "good" or "natural", but it is still a choice. The non-existence of a tiling is a miniature example of the same type, which shares the important feature (ii); I do not see how one can accept it as a "good choice" rather than a necessary truth. Consider, for illustration the game of chess. The rules that define it are perhaps a "good choice", since they yield an enjoyable intriguing game. They are nonetheless arbitrary.

[8] *Lectures on the Foundations of Mathematics, Cambridge 1939*, edited by C. Diamond, University of Chicago Press, p. 56 (paperback edition). Cf. also the discussion in lectures V and VI. Wittgenstein's paradigmatic example is the impossibility of a ruler-and-compass construction of a heptagon.

But, *given that these are the rules*, some far from obvious statements follow, which are not arbitrary at all; e.g., generally, it is impossible to mate with a king and two knights against a king. Some claims of this type are known to be true only through the use of computers. Sometimes we know that one of several alternatives holds, without knowing which: either white has a winning strategy, or black has one, or both have strategies that guarantee at least a draw. It would be a major discovery to find which of these is the case. In this respect mathematics is like chess. Wittgenstein was right in seeing the rules of mathematics as rules that determine meaning. He was wrong in denying the factual character of mathematical truths.

I am going to suggest that mathematics is the study of forms of organization (or patterns, or structures, or kinds of configurations), and what they imply. Here 'implication' marks a semantic notion that goes with a notion of truth, which cannot be reduced to provability in some deductive system. Let me consider first two other examples.

2. Euler Graphs

This is a famous historical case, known as the problem of the seven bridges of Königsberg. It concerns seven bridges connecting four land areas that are separated by a river (Pregel). A known puzzle of the time asked whether it is possible to cross in a single walk all the bridges, so that every bridge is crossed no more than once. Figure 6 represents the configuration of the areas and bridges as a graph, in which the areas appear as vertices, marked with capital letters, and the bridges as edges (connecting lines). In 1735 Euler gave a lecture to the Russian Academy in St. Petersburg in which he proved that this is impossible. His proof established a necessary condition for the existence of such a walk in similar configurations, a condition that can be easily checked and which obviously fails in the case of the Königsberg bridges. In a paper published a year later, he noted that some people thought that the walk was impossible, others were doubtful, and none claimed that it can be done. His proof thus confirmed a general suspicion. He also notes that, in principle, the answer can be found by checking all possible combinations, but that this is an involved task and when there are more bridges it is unfeasible.

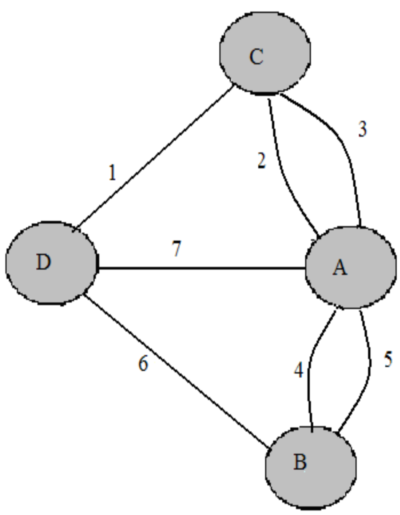

Figure 6

The idea of a graph – a system of vertices and edges, where each edge has two vertices, pictured as its "end points" – is easily explained with the help of a few examples (such as fig. 6). An edge is said to be *incident* on each of its vertices, and each of its vertices is *incident* on it. The *degree* of a given vertex is defined as the number of edges incident on it. In fig. 6, the degree of A is 5, and of each of C, D, B has degree 3.

A general "walk" on a graph can be given as a sequence of alternating vertices and edges that starts and ends with vertices (the beginning and the end of the walk), v_1 , e_1 , v_2, ..., e_{k-1}, v_k, such that for any edge of the sequence, e_j , its two vertices are its two sequence-neighbors, v_j, v_{j+1}. Such a sequence is also referred to as a *path*. The condition that every bridge be crossed exactly once means that every edge of the graph occurs in the sequence one time exactly; such a path is called an Euler path, and a graph that has it is called an Euler graph. The argument in Euler's paper involves unnecessary details. The following is a proof that "everyone" can easily grasp.

Suppose we start the walk in vertex v' and end it in vertex v'', where the two may or may not be the same vertex. Consider any vertex, v, different from the end points. Each time we pass through v, there is an edge through which we arrive and an edge through which we depart. Since every edge incident on v is traversed exactly once, the number of edges incident on v is twice the number of times that we pass through it (which is the number of times v occurs in the sequence). This implies that except for the end points, v' , v'', every vertex has an even degree. The same counting applies to the end points, except that, for v', the first edge of our walk is unpaired, and, for v'', the last edge is unpaired. If $v' \neq v''$, this implies that the degrees of the end points are odd. But if $v' = v''$ then the first outgoing edge is paired with the last ingoing edge, hence all the degrees are even. Therefore a necessary condition for the existence of an Euler path is: either (i) all vertices have even degrees, or (ii) all vertices except two have even degrees. In fig. 6 all vertices have odd degrees. There is no Euler path. If we delete edge 7 between A and D, then A has degree 4, B and C have degree 3 and D has degree 2. In this case there is an Euler path:

B 4 A 5 B 6 D 1 C 2 A 3 C .

We can also see that (i) is necessary for the existence of an Euler *cycle* (an Euler path that starts and ends in the same vertex), while (ii) is necessary for the existence of an Euler path that is not a cycle.

Euler claimed that his condition is also sufficient; this claim is true, provided that the graph is connected. His argument for the sufficiency is incomplete; the full proof of sufficiency is more involved than the argument for necessity and I omit it. Figure 7 is based on an illustration given by Euler in the same paper, with 16 bridges (including the dotted bridge between C and D) and 6 land areas marked by capital letters. The degrees of all vertices except D and E are even; indeed, there is an Euler path. If we omit the dotted edge, then also C and F have odd degree, and there is no Euler path.

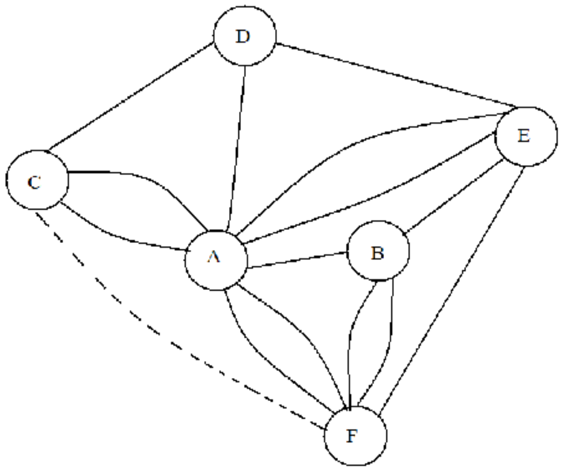

Figure 7

Unlike the tiling problem, the present example is based on a concept (that of a graph) which is not fully specified in the statement of the problem. The drawings used to explain the concept are bound to be *planar*: graphs that can be embedded in the plane, with vertices appearing as points and edges as continuous arcs that do not intersect except at their end points. But evidently the argument does not depend on this restriction. Although our explanations leave the concept somewhat open, I claim that the proof is as valid as it can be. It works in the more restrictive interpretation of 'graph'; and it works, exactly in the same way, under other less restrictive interpretations. Actually the proof depends only on an abstract conception, under which a graph consists of two finite disjoint sets – a set of vertices and a set of edges – and a correlation that associates with every edge two vertices.

The third example goes further in this direction: a valid argument that proves a claim that involves an open concept.

3. The Coin Placing Game

Two players take turns in placing coins of equal size on a perfectly round table. The coins can touch, but should not overlap and should not extend beyond the table's perimeter. The player who cannot place a coin loses. (The table is of course much larger than a coin, say its diameter is at least six times that of a coin.) Does player I (who goes first) have a winning strategy, or does player II have one? Since the game must terminate after finite number of moves (a number that is smaller than the ratio of areas of the table and the coin) and one of the players must lose, a well-known theorem says that one of the players has a winning strategy. 'Winning strategy' is a precise, well-defined term of game theory. Our riddle however is addressed to the mathematically non-sophisticated. 'Strategy' therefore means no more than a prescription (in some intuitive loosely understood sense) for playing the game, and a winning strategy is a prescription that leads to a win. No appeal is made to game theory. In any case, the theory does not help us towards a solution. Again, the solution is not easy, if you have not seen similar devices before; but, once given, is completely obvious.

Player I starts by placing his coin at the center of the table. Then, whatever player II does, player I responds symmetrically in the diametrical opposite place: if player II places her coin at point x, player I places his at x', which is on the diameter through x on the opposite side of, and at the same distance from the center; cf. figure 8.

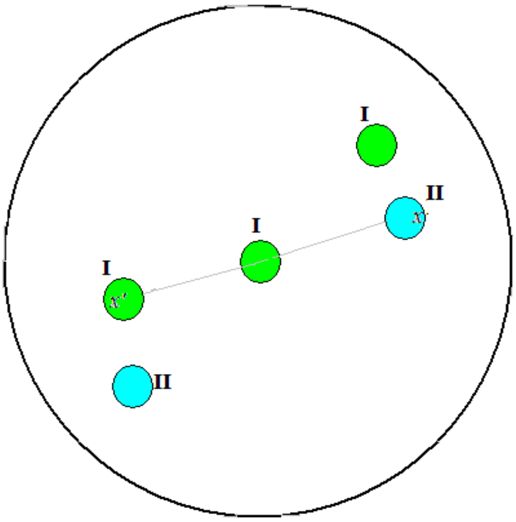

Figure 8

After each move of player I the coin configuration has radial symmetry. If player II has a place for her coin, player I has the diametrically opposite place for his. Hence, the first to run out of places must be player II.

Playing according to this prescription, player I ensures a win. Here again we have hit rock-bottom as far as validity is concerned. The solution is a well-defined particular prescription; it does not matter that a general definition of 'winning strategy' is still pending.

Each of our three examples involves geometry, but geometry plays in them different roles. The tiling problem is purely combinatorial, but it makes little sense without the geometric organization that helps to define it. The graph theoretic problem is introduced in a geometric setting, but the geometry can be easily dispensed with. The last example differs in that geometry enters essentially, since the metric is crucial. Hence the last example may give rise to the standard old observations about the imprecision of the real world: a table is never a perfect circle, a player can measure distances with limited accuracy only, etc. Evidently, the game is supposed to be *mathematical*: the table and coins are ideally circular and places are determined with perfect precision. It does not take mathematical sophistication to appreciate this aspect of the problem.

The examples illustrate and support my main point to which I now return: Mathematics studies patterns, structures, kinds of configurations, what I shall refer to as *forms of organization*. I take this notion as primitive. When a formal language is used in characterizing a system, the underlying form of organization is indicated by the structures that are considered as possible interpretations of the formal language.[9] But in this context

[9]As a rule, mathematical theories are considered in contexts that include at least arithmetic (if not analysis and more). Hence, as a rule, the structures that are considered as a possible interpretations should include the standard model of natural numbers as a component. For example, group theory can be characterized by first-order axioms; but when we speak of finite groups, or groups generated by a finite

'structure' is a technical, or semi technical term of set theory – a theory that presupposes already a kind of "structure" of a more basic, non-technical kind. This generic concept is marked by 'form of organization'.

The view advocated here has an obvious affinity with Kant, who, speaking broadly, saw mathematics as the discipline that studies time and space, where these are forms of perception – the way we organize sense data. But my proposal avoids the Kantian metaphysics of raw sense data that are organized into experience. It is concerned with general forms of organization that underlie our practices and views of the world. This is as broad a category as what comes under Wittgenstein's rule following. We come to know mathematical truths through becoming aware of the properties of some of the organizational forms that underlie our world. This is possible, due to a capacity we have: to reflect on some of our own practices and the ways of organizing our world, and to realize what they imply. In this respect all mathematical knowledge is meta-knowledge; mathematics is a meta-activity *par excellence*. The meta-reflection need not be explicit, or deliberate. The ancient Egyptians and Babylonians used particular examples, involving the adding, subtracting and dividing of quantities of merchandize, not for their own sake, but as *generic*: to illustrate how similar problems are to be solved; this marks already the move to the meta-level.

In mathematics the study of organizational forms yields clear–cut, necessary, and far from trivial truths. This is a special combination of features; when one of them is missing we do not have mathematics. The late Jerry Katz claimed that, underlying the semantics of natural languages, there is a theory of *sense*, which is a sort of mathematics. I do not know why there should not be such a system, but the brute fact is that (so far) there is not. The semantics of natural languages is an empirical discipline, which did not breed any new mathematical system.

Mathematics is continuously expanding by deriving new mathematical systems from areas of human cognition and practice. A domain may be thus "mathematized". The ongoing list is numerous. Graph theory, initiated by Euler's work (our example 2) is a nice illustration; game theory, which grew in the context of economics, is another. Many new systems originate within mathematical practice itself, or mathematical practice supplemented by concrete examples (e.g., the theory of knots). Probability theory and mathematical logic are two grand examples whose significance can be hardly overestimated. In mathematical logic, which includes the study of formal languages, aspects of mathematical activities become themselves the subject of mathematical investigations.

Each of the points made above needs elaboration and further philosophical work. In particular, further explanations are due concerning the key notion of "form of organization". I am arguing here for a line of investigation, rather than for a worked out account. Let me only note that a form of organization is related to what is known as a structure, but the link is not rigid. A form of organization need not correspond to a single standard interpretation of a given mathematical language, it can correspond to a family of interpretations, which share some basic features. Note also that this is not a mathematical concept. Philosophy must have recourse to a looser conceptual apparatus than the mathematics it studies.

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number of generators, etc., the structure involves, besides the group itself, the natural numbers.

The story I told so far is quite partial. The interconnections of mathematical disciplines, the reductions, the translations, the unifying grand systems, the axiomatic method, all these are missing. I focused on certain examples in order to bring to the fore essential features of mathematical reasoning. They should not mislead us into picturing mathematics as a dispersed collection of puzzle-solving techniques. How does the analysis offered so far fit into the bigger picture?

First, note that proofs of the kind exemplified above have limited use only. As a rule, difficult theorems are established through derivations from a relatively small number of axioms, because they cannot be proved by ingenious immediate devices. Even when a theorem seems evident (as is the case with quite a few geometrical claims), its derivation from a small number of axioms is useful in the interest of a streamlined system, one that gives us a good overview of the area. It is highly desirable to keep one's working desk uncluttered. The system serves also as a common framework for a wide community of researchers. It facilitates communication by establishing a common terminology, sets up accepted standards, and reduces the possibilities of misunderstanding.

There is also, as in any cognitive enterprise, the danger of error. In reasoning about our forms of organization, we can make mathematical errors. There is no lack of mistakes in the history of mathematics. The axiomatic method does not guarantee an error-free activity but it provides us with an error-correcting methodology. We can go again, and again, over the steps in a proof, and we can make it more formal and detailed as we need; faced with contradicting claims of different researchers, we can sort out the conflict and deliver a verdict. The uniqueness of mathematics does not consist in its being error-free – which it is not – but in its error -correcting mechanisms.

All this is compatible with my previous claim that the solutions of our three examples are as valid as they can be. Nothing will be added by recasting them in some acceptable system and deriving them formally from axioms. If I am asked, how I can be sure that I have not overlooked something in the tiling proof, I respond: how can we be sure that 59 is a prime number? we have checked and rechecked, but perhaps we have overlooked something? At a certain stage doubt must come to an end. In each of the above examples that stage is reached with the solution – the one given above, not some formalized version. Writing the tiling-proof formally and having a computer verify all the steps (every modus ponens) is of no help. First there is a possibility that we have not formalized the problem correctly, second, there might have been an error in the proof-checking program, third, the system's software, even its hardware, could have been faulty.

Finally, the axiomatic method is crucial in uncovering hidden assumptions. This is a process of making explicit various features of an organizational form, which, unaware, we took for granted. In geometry the process spanned long stretches of history, culminating in the full axiomatization, at end of the 19^{th} century, where everything was made explicit; it also inspired the development of pure formalisms. The uncovering of a hidden assumption opens the possibility of modifying the form of organization, either by omitting the assumption or by modifying it. This is not a mere syntactic replacement of an uninterpreted axiom by another, but a change of meaning, where different structures are considered as possible interpretations of the language.[10] Thus, the non-derivability of the

[10]In situations where the completeness theorem applies, the change of possible interpretations can be

parallel postulate from the other geometric axioms was not sufficient for the emergence of non-Euclidean geometry. Quite a few mathematicians were convinced that the postulate was not derivable, but Non-Euclidean geometry emerged when Gauss, Bolyai, and Lobachevsky realized the possibility of a different geometric structure; and the theory was developed before any consistency proof was considered.

What then of set-theory? Like other mathematical systems set theory studies a certain form of organization. It is a form that arose within mathematics itself, when Cantor saw how basic patterns of reasoning about collections of real numbers can be generalized, extended and made into a self-standing discipline. That form of organization was developed by focusing on some basic concepts and by establishing some non-trivial properties (e.g., the Cantor-Bernstein theorem); later it was recast axiomatically and, still later, was crystallized into what is now known as the iterative concept of sets. It is an elegant system, which can be grasped after some elementary training. Its unique position is due to the fact that other current mathematical systems can be modeled in it, and then the theorems of current mathematics can be derived from its axioms. The last feature underlies the significance of some set-theoretic independence results. What this tells us about the philosophical status of set theory is a difficult question and a matter of debate. It should be noted that, in principle, other systems can have this universal character. Indeed, category theory is a contender – though category theory, I am told, must help itself to a modicum of set theory, since it has to rely on the distinction between sets and proper classes. The view proposed in this paper and the line suggested by it, can be pursued independently of one's conclusion regarding set theory.

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fully characterized syntactically. The passage from commutative to non-commutative fields is captured in a purely formal way, as long as we are interested in the first-order theory of fields. But such theories are, as a rule, considered in wider contexts that include arithmetic (perhaps also analysis and more), cf. footnote 9. Once we throw in the standard model of natural numbers, we must appeal to a semantic, not a syntactic, consequence relation.