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Preface

Which philosophy for which mathematics? This is a fundamental question, which the philosophy of mathematics must face in ever new ways. The various answers that have been given to this question have resulted in a variety of approaches, often competing, during the second half of the 20th century. Some of them were oriented towards language and logical or syntactic questions. Others focused on ontology. Others have pursued a dialogue with philosophical theories, and sometimes embedded their development in national traditions. More recently, the philosophy of mathematical practice has proposed to reorient the philosophy of mathematics towards the interests and methods of mathematicians. The history of philosophy and mathematics remains an inexhaustible source of developments, which can be combined with any of the philosophical strategies mentioned above.

We could multiply the examples and go into more detail about each of these approaches: the fact remains that the philosophy of mathematics has largely detached itself from mathematical *contents and advances*; from mathematics "in the making", its issues, its progress, and what they imply for general philosophy, the theory of knowledge, or mathematical philosophy proper.

This situation has not always been the rule: without going back to the golden age of Descartes and Leibniz in the 17th century, the first half of the 20th century was marked by a strong presence of mathematicians in philosophical debates with Enriques, Hilbert, Poincaré, Brouwer or Weyl, and their dialogue with the proponents of more logical and foundational approaches — Couturat, Frege, Russell... The activity at the interface of physics was equally dynamic with the birth of relativity and quantum mechanics, and it is known that the same Hilbert, Poincaré and Weyl were involved in and contributed to these discoveries.

From this point of view, the contemporary period is very disappointing. There are, of course, exceptions, and there is a basic

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tendency for the philosophy of mathematics to try to take hold of current mathematical events, but these attempts remain timid, rather isolated, and of limited impact.

It was this observation, shared by all of its founders, which led to the creation of the *Annals of Mathematics and Philosophy*. We wanted to create a space for debate between philosophers and mathematicians, similar to what the *Revue de Métaphysique et de Morale* had achieved in its early days: a journal that meets all the criteria of academic rigour, that is very selective about the quality of its publications, but that encourages collaboration between philosophers and mathematicians, in particular around all the developments in contemporary mathematics and the questions that agitate the mathematical community and structure its activity.

For all that, and it is important to specify this immediately, this approach is not exclusive. Intellectual progress always takes place in a double movement: that of the conquest of new theoretical fields, and that of the reappropriation and rereading of the past. We also expect a lot from contributions related to the conceptual history of mathematics, as long as they enrich our view of mathematics or contribute to renewing our understanding of what mathematics can contribute to contemporary philosophical debates.

It is in this spirit that we also intend to work on the collective reappropriation of the contributions of important authors, but little known beyond a geographical or linguistic area, by publishing, for example, translations with commentary or accompanied by articles contextualising them of texts written in languages other than English and therefore inaccessible to the whole of the international community for which English remains, in fact, the only shared language of reference.

The same interest in questions of language, but also the concern for style, which is for us an essential component of philosophical activity, has led us to adopt the choice of multilingualism, tempered by the need to be able to reach out to the whole philosophy of mathematics community — and beyond. The authors of the *Annals of Mathematics and Philosophy* are therefore encouraged to write in their mother tongue, with all the sophistication and quality of writing that this allows. We do, however, ask them to translate their work into English. This process is, in our experience, facilitated by the existence of excellent translation softwares. We hope that it will help to combat the impoverishment and standardisation of philosophical language that can be observed in recent production, including in English. Editorial management constraints require us

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to restrict the set of vernacular languages for which dual publication will be possible. At the moment, these are German, Spanish, French and Italian. We do not exclude the possibility of developing this offer in the future.

As far as the project of promoting an *effective* dialogue between philosophy and mathematics is concerned, we are aware of the difficulties it presents. The time of Poincaré, Enriques, Hilbert, Brouwer and Weyl has passed, in a context where specialisation within a given discipline is inevitable, sometimes to excess. It would be absurd to expect a professional philosopher to master the technical contents of contemporary mathematics, just as it would be vain to expect a mathematician interested in the general conceptual stakes of his/her work and his/her discipline to master the references and codes of philosophical writing. However, we do not intend to sacrifice the academic standards of the journal on the altar of pragmatism. In particular, all accepted articles must have a significant philosophical content. We therefore expect authors to accept, with the necessary humility, to undergo the scrutiny of experts from the other discipline, in order to enrich their discourse and thoughts with elements and references from other fields of knowledge.

Two phenomena have finally caught our attention and contributed to structuring the project of this journal. First, the philosophy of mathematics is evolving. There is a movement of internationalisation that goes hand in hand with an increase and a reciprocal fertilisation of traditions. The *Annals of Mathematics and Philosophy* will therefore be deliberately ecumenical and open to *all* philosophical approaches, without exclusivity, but with the constraint of focusing on mathematical issues or concepts, and with the idea that we intend to privilege the work of concepts for its own sake to the often sterile debates internal to this or that current.

Another phenomenon that has caught our attention is the profound transformation of mathematics, with the emergence of new fields, new practices, and new challenges. We can cite, without any concern for exhaustiveness, a growing interdisciplinarity *within* mathematics — to take one example among many others, contemporary group theory is structurally nourished and profoundly transformed by interactions with several forms of geometry, with arithmetic, but also with probability, dynamical systems, analysis — ; a transformation of the relationship between "pure mathematics" and "applied mathematics"; an opening to the human and

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natural sciences that goes beyond the case of physics and chemistry; finally, the presence of numerous issues in theoretical computer science, logic (complexity theory, proof theory...), statistical learning and machine learning. The field to be explored is immense, and we hope that the journal will contribute to its exploration.

In conclusion, we hope, with the entire editorial board, to offer with these *Annals* a place of renewal for the interactions between philosophy and mathematics, and a journal combining rigour and the will to be read by a public of philosophers, mathematicians, logicians, historians and didacticians of mathematics.

The first issue, which you are about to discover, has been conceived in an atypical mode, reflecting the different issues of this new publication. We have given carte blanche to a whole group of authors, emblematic of contemporary mathematical philosophy, to address subjects close to their hearts, whether in the form of testimonials or programmatic texts. The exercise was difficult and unconventional, and we thank them warmly for having complied with the rules of the game. The style and tone vary from one contribution to another; we see in this variety and in the variety of their theoretical interests, the exact reflection of the project of the journal: that of a profound renewal and a revitalization, where the form itself adapts to the contents and the requirements of the thought.

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Jean-Pierre Marquis, Université de Montréal ⋆
Frédéric Patras, Université Côte d'Azur ⋆ ⋆

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What is Axiomatics?

Paola Cantú[1]

Abstract. The article investigates axiomatics as a complex mathematical practice whose inquiry, while taking its cue from the analysis of some specific mathematical theories, requires an interdisciplinary approach. Axiomatics, if analyzed in detail through a study of its foundational component, of the styles with which it is associated and of the rules that govern it, performs a plurality of functions. It serves heuristic, descriptive, genetic-historical, pedagogical and architectural aims. But it can also play the role of conceptual analysis, modular analysis and coordination tool, soliciting a quest for rigor. An example taken from Peano's investigation of axiomatic systems illustrates the kind of results that this interdisciplinary approach to mathematical practice might produce, showing what can be achieved by considering axiomatics as research on the foundations of mathematics, or as a mathematical style, or as a social institution.

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§ 1. — Introduction.

What the relation between mathematics and philosophy should be is a delicate question. Stewart Shapiro has distinguished two different modes of relationship between philosophy and mathematics, described respectively by the methodological principles *philosophy first* and *philosophy last-if-at-all*: in the first case the philosophical reasons in favor of a given mathematical ontology determine normatively how mathematics should be done; in the second case

[1] I would like to express thanks to Andrew Haigh, who helped me to improve the English version.

philosophy is a mere epiphenomenon that exerts no influence on the development of mathematics.[2]

The philosophy-oriented approach — to which Shapiro aligns himself, while insisting on the need to break down unnecessary walls between philosophy and mathematics and on the desirability of considering philosophical normativity defeasible — assumes that philosophy is an activity carried out exclusively by professional philosophers (who may possibly also be competent in mathematics) and that it influences mathematics without being in turn significantly influenced by it. To this approach we ascribe the investigations related to the development of the twentieth-century 'isms' (logicism, pragmatism, structuralism, etc.), the research related to the well-known dilemma of Benacerraf on the indispensability of mathematics, and more generally all the contributions to the problem of the existence of abstract objects, their cognitive access and the choice of a first or second order logic.

The practice-oriented approach to mathematics, on the other hand, is characterized by the idea that the search for solutions to open problems in mathematics as well as the search for definitions and arguments are essentially philosophical activities — regardless of who is in charge of them — that can have repercussions as much on the development of mathematical theories themselves as on the transformations of the conceptual tools that philosophy uses. In the practice-oriented approach we include investigations that show the intersection of philosophy and history of mathematics, epistemological research related to visualization and explanation in mathematics, investigations of computer-assisted demonstrations, and analysis of certain properties of mathematical arguments, such as purity, evidence, and fruitfulness.[3]

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[2] See [Shapiro 1997, pp. 25ff]. The *philosophy-driven* and *practice-driven* distinction partly traces the opposition drawn by Paolo Mancosu between analytic philosophy and the maverick tradition, which includes [Lakatos 1976] and [Corfield 2003]. See [Mancosu 2008, p. 3].

[3] The practice-driven approach as it is understood here includes, for example, [Lakatos 1976] and [Corfield 2003], but also the new epistemology ([Mancosu 2008]), the interactions between philosophy and history ([Ferreirós and Gray 2006]; [Kerkove, De Vuyst, and Bendegem 2010]), recent investigations of argumentation in logic and mathematics ([Gabbay et al. 2002], [Aberdein and Dove 2013]), research on set theory, probability, computability, applications and open problems in mathematics ([Irvine 2009, pp. 461ff.] [Colyvan 2012]). This distinction intertwines but does not coincide with the distinctions drawn by Cellucci (2013, pp. 93-96) between static (aimed at the justification of an established body of

A relevant area of research that requires a back and forth between philosophy first and philosophy last-if-at-all approaches is the investigation of axiomatic theories and methods. Philosophy offers relevant tools for the conceptual analysis, but mathematics is the starting point since the object of the inquiry are given mathematical theories.

§ 2. — What is axiomatics?

'Axiomatics' is used in the following as a general term that stands for an inquiry into axiomatic theories and methods, including specific epistemological views and technical solutions, but also meta-theoretical considerations, and different objectives that the axiomatic method and theories might serve. Differences might concern also the purposes that the axiomatic method or the axiomatic formulation of a theory serve in a specific case (section 3). This paper will list different purposes that the axiomatic formulation of a theory might serve and illustrate them with an example taken from the investigation of the Peano School (section 5).

Let us begin with a preliminary conceptual clarification of the terms 'axiomatic method' and 'axiomatic theory'.

The 'axiomatic method' used for the formulation of mathematical theories comes in many forms, which differ by the language used: formal, informal or semi-formal. Even when the language is formal, there are several possibilities of reconstruction, for example by using first or second order logic, classical or intuitionistic logic, etc. Variations in the axiomatic method over time are well known in the literature, and a radical distinction is generally drawn between the classical Euclidean method and the modern hypothetico-deductive method. The former is considered as a content-oriented approach (*inhaltlich*), in which the axioms are considered as self-evident truths and the fundamental concepts are explicitly defined and known by intuition. The latter is presented as a formal approach, in which the axioms are hypotheses defining the primitive concepts and their mutual relations.

knowledge) and dynamic (interested rather in the growth of mathematical knowledge) philosophy of mathematics and between top-down (starting from some general unproven assumption) and bottom-up (starting from the activity of individuals) approaches. While it shares with the dynamic approach an interest in inductive arguments alongside deductive ones, it is bottom-up in the sense that it emerges in the activity of individual mathematicians.

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More recent research has shown that the change in the axioms chosen to underpin a theory often reflected a profound methodological and epistemological change.[4] Besides, the very notions of axiom and definition changed meaning several times,[5] making it difficult to compare different axiomatic systems without taking into account precise historical and conceptual differences.

The notions of logical consequence and more generally of logical rule are also closely linked to the axiomatic method and have undergone profound variations not only in the transition from syllogistic to twentieth-century symbolic logic (a transition that was by no means linear, as is still evident in Peano's school),[6] but also in the transformation of the role of propositions assumed as primitives and their relation to theorems. The study of various instances of axiomatic method suggests the existence of multiple variants which are in some way intermediate between the classical (Euclidean) axiomatic method and the modern hypothetico-deductive method.

An 'axiomatic theory' is composed of a language, a logical theory — including primitive logical terms, logical axioms and logical rules of inference — a set of primitive or undefined terms, a set of statements considered as axioms, a set of defined terms and a set of theorems that can be derived from axioms by means of the logical rules of inference (often, specific rules of derivation are allowed beside the fundamental logical rules). Set theory comes in a variety of different formulations, which are related to the technical objective of avoiding paradoxes (e.g., modifications to the comprehension axioms as made in type-theory) but also to the epistemological objective of offering an adequate account of the infinite. If the different formulations of set theory reflect different philosophical viewpoints (predicativism, finitism, and so on), one can find alternative axiomatic formulations of a theory even inside a single research group: for example, in the Peano School many different formulations have been offered of the theory of natural numbers, rational numbers, real numbers, complex numbers, vector spaces, projective geometry, plane and solid geometry.

The same axiomatic theory can be analyzed from a syntactic point of view as a set of statements, from a semantic point of view as a set of non-linguistic models that satisfy the statements,

[4] See e.g., [De Risi 2016] on the role played by the parallel postulate in geometry.

[5] See e.g., [Cantù 2018] on the deductive role played by definitions in Wolff's mathematical method.

[6] [Cantù 2022b].

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and finally, from a pragmatic perspective, as a set of examples, problems, norms, skills and practices. Yet, the general objective of axiomatics as a philosophical investigation of mathematical practices is not limited to the axiomatic method and the type of language and logic used, nor to axiomatic theories understood as sets of axioms and theorems or as theories composed of language, semantics, syntax and pragmatics. For example[7], axiomatics also questions the historical reasons why a certain way of conceiving the method and the axiomatic theories has been privileged in a given conceptual framework; it questions the role that axioms play in the foundations of mathematics, in the development of a mathematical style and in the institutionalization of a given social mathematical practice. The paper will thus consider three complementary ways to investigate 'axiomatics' as a whole: foundational, stylistic and institutional (section 4).

The axiomatic treatment of scientific theories was central to the early twentieth-century philosophy of logic and mathematics program and was a fundamental feature of the philosophy of science until at least logical empiricism. In more recent times, the general philosophy of science has moved toward less systematic approaches, which by undermining the unity of science at various levels have also called into question the methodological unity based on axiomatization of theories.[8]

This is one possible reason for the lack of a systematic survey of the role of axiomatics not only in contemporary philosophy of science, where it seems to have fallen out of favor, but also in the period from the late nineteenth to the mid-twentieth century. The temporal distance allows us to observe that in the first half of the twentieth century many axiomatic methods, styles, and theories were indeed developed that do not necessarily constitute a unity. The question is then whether it is possible to speak of axiomatics as a variegated complex of mathematical research and practices that can be characterized either on the basis of the common goal of investigating the foundations and the unity of science, or on the basis of some peculiar stylistic features, or even on the basis of some institutional characteristics.

Having carried out research on the axiomatization of geometry, extensive magnitudes and arithmetic, I got interested in the study of various axiomatic approaches to mathematical theories. In the

[7] [Savage 1990] ; [Winther 2016].

[8] See e.g., [Cartwright 1999].

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literature devoted to the history of analytic philosophy, the role of the axiomatic method is generally associated with the study of the foundations of mathematics, the philosophical analysis of mathematical principles and rules, the discussion on the possibility of reducing mathematics to logic, and questions of purity in the relation between arithmetic and geometry. The objective here is to plea for a broader vision of what I will call 'axiomatics', i.e., a variety of theoretical inquiries and practical activities related to mathematics and that present different forms, objectives, and methods. Even limiting attention to the period from the late nineteenth century to the first half of the twentieth century, the variety of methods, languages, styles, practices and rules at stake in axiomatized theories is such that it suggests there is the opportunity for a wider and deeper study of the subject — an approach that is not common in the literature.[9]

§ 3. — The objectives of axiomatics.

To investigate axiomatics one needs to understand the relation between the different methods and the objectives pursued by researchers who provide axiomatic formulations of mathematical theories. In this section we will offer a (non-exhaustive) list of roles which axiomatics has played.

Heuristics. Axiomatics is useful to formulate new conjectures, to understand why some hypotheses are more important than others, to provide new demonstrations, to simplify and speed up proofs, and to discover errors.

As Hilbert says:

> However, as I have already remarked, the present work is rather a critical investigation of the principles of the euclidean geometry. In this investigation, we have taken as a guide the following fundamental principle; viz., to make the discussion of each question of such a character as to examine at the same time whether or not it is possible to answer this question by following out a previously determined method and by employing certain limited means. This fundamental rule seems to me to contain a general law and to conform to the nature of things.[10]

[9] [Schlimm 2006, pp. 2–3].
[10] [Hilbert 1902, pp. 130–131].

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Freudenthal as a mathematician considers the heuristic objective of axiomatics as its main trait, and thus considers the *Grundlagen der Geometrie* as the best example of axiomatics, even if he acknowledges that an earlier formulation can be found in Padoa, and that the elimination of the link with intuition was already stated by Pasch and Fano (I would also add Bettazzi).[11] The cited example explains very well why, according to the objective considered as primary in axiomatics, one author or the other becomes the founding father.

Conceptual Analysis. Axiomatics is also a tool for conceptual analysis, i.e., a tool for understanding mathematics, and determining which concepts presuppose which, verifying whether a set of concepts are independent, and distinguishing notions expressed by a single term but having distinct logical or mathematical functions. Peano's ideography, for example, is a form of conceptual analysis[12] of the definitions occurring in mathematical textbooks. The analysis takes place in two stages that influence each other. On the one hand, the analysis of the language and of the structure of proofs allows us to highlight the primitive logical terms, for example by distinguishing the notions of membership and inclusion, often designated by the same term (the copula *est*), or by identifying the primitive logical rules (e.g., syllogism and *modus ponendo ponens*). On the other hand, the study of the mathematical definitions provided in the textbooks of analysis and geometry constitutes the starting point of the search for the fundamental concepts of mathematics and the axiomatic formulation of arithmetic, geometry, the theory of vector spaces, etc. As Peano and his collaborators clearly acknowledge, a back and forth between mathematics and logic is induced by the axiomatic treatment, because the axiomatic formulation of the mathematical theories induces a modification of the axiomatic formulation of the logical theory, and in its turn the modification of the latter requires an adjustment of the formulation of logic:

> Ideographic logic, in addition to being the most appropriate tool for a non-superficial study of logic, by abolishing any insidious promiscuity of its vocabulary with that of other sciences which presuppose only logic, like arithmetic and geometry, has obliged each of these to an equally diligent revision of their own vocabulary.[13]

[11] See [Freudenthal 1957, p. 153], [Padoa 1901], [Pasch 1884], [Fano 1958] and [Bettazzi 1890].

[12] [Schlimm 2021].

[13] See [Padoa 1933, pp. 75–77].

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Quest for rigor. Axiomatics serves to eliminate the risk of internal contradictions in a theory and to eliminate the errors which could result from an excessive recourse to intuitive ideas, to the assumption of contradictory axioms, the choice of a wrong definition, the application of an incorrect rule in a derivation, or the making of a mistake in a proof. The quest for rigor can be expressed in various ways in mathematics, and it is important to distinguish between mathematical and philosophical needs. For Peano, although his position on this point differs significantly from Pieri and Padoa, the search for rigor is dictated by a conceptual analysis that is intrinsic to mathematics and not by the effort to provide a philosophical foundation for mathematics. For Bolzano, the search for rigor is associated with the task of providing the right order of presentation of the concepts and propositions of a scientific theory.(14)

The search for rigor cannot be identified neither with the conceptual analysis which allows us to identify the elements of a theory, nor with the descriptive objective consisting in the production of an error-free presentation of a theory. Indeed, there are a variety of methods to increase rigor in mathematics. In Peano's case, the method by counterexamples is the most important and is mainly applied to mathematical definitions, whose inadequacy is shown by finding examples that satisfy the general definition but should be excluded, or examples that do not satisfy the definition but are intuitively taken to fall under the given concept. For example, Peano criticizes the definition of Peano-Schwartz because it holds for the length of a curve arc but not for all concave surfaces.(15)

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(14) See [Bolzano 1804] and [Cantù 2014].

(15) Serret had defined the length of a curve arc by analogy to the length of a surface: the former being defined as the common value to the upper bound of inscribed polygons and the lower bound of circumscribed polygons, the latter was defined as the limit of an inscribed polyhedric surface. But Serret's procedure was flawed, as both Peano and Schwarz acknowledged ([Peano 1890, p. 55]). Hermite offered an alternative definition, based on the limit of a series of non contiguous polygons that are tangent to the surface, but he thus lost the analogy between the inscribed polygons and the inscribed polyhedric surfaces. Peano in 1890 suggested a new definition based on the notions of vector and bivector, which respects the analogy: the length of the curve arc is the superior limit of the sum of the vectors of its parts; the area of a surface is the superior limit of the sum of the bivectors of its parts. [Peano 1890, p. 56]. For an exhaustive analysis of the problem of the definition of the surface area, see [Gandon and Perrin 2009].

Even if we limit ourselves to the logico-philosophical field, it is worth noting that the search for rigor does not necessarily presuppose a formal conception of axiomatics, but is perfectly compatible with a content-oriented vision, as well as with a positive evaluation of the role of intuition in mathematics, which is often taken to be necessary in order to justify the applicability of mathematics.(16)

Descriptive aim. Axiomatics is used to present a theory once it has already been developed. This objective is often considered by philosophers as purely accessory, or at least secondary to conceptual analysis or search for rigor, which are usually taken as the two pillars of the axiomatic method in the philosophical investigation of mathematical foundations. However, in the history of logic, long considered as a general theory of concepts, the primary objective of the axiomatic method was that of providing a good presentation of a given topic in a treatise. The right order of concepts was thus mainly understood as the better way to introduce a topic. For example, Bolzano defined logic as the art of presenting a theory in an appropriate textbook (*Wissenschaftslehre*), which explains the kind of criticism he made to the order of the geometrical concepts as exposed in Euclid's *Elements*.(17) Often, various orders of concepts were considered, and the order of concepts present in the divine mind (*ordo essendi*) was contrasted with the order reconstructed by humans and necessary to their understanding (*ordo cognoscendi*). The difficulty of providing an exhaustive list of the fundamental concepts of a discipline was precisely linked to human finitude, a theme that we also find in Gödel, for example when he observes that the paradoxes of set theory are due to a bad choice of the axioms and not to a problem concerning the existence of sets.(18)

For philosophers of science, the descriptive objective attests to the value of axiomatics in justification contexts, and explains why it does not allow new discoveries. Hempel observes for example that axiomatics, being a 'device of exposition' of theories, is used to compare and justify them, but not to discover new facts.(19) The descriptive objective is here opposed to the heuristic objective.

(16) See for example [Gödel 1953/9, pp. 348–9].
(17) See [Bolzano 1837], [Bolzano 1804] and [Bolzano 1810].
(18) [Crocco et al. 2020, pp. 48, 73, 86].
(19) [Hempel 1970].

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Genetic-historical aim. Axiomatics is sometimes used to compare different theories, to explain the transformation or the evolution of a theory into another and possibly the ensuing cognitive progress. A well-known example is Hilbert's *Grundlagen der Geometrie*, in which the distinction between groups of axioms is also used to compare different geometrical theories.(20) But I would also like to mention the case of Giuseppe Veronese, because his axiomatic formulation of infinitesimal geometry was from the beginning linked to the comparison between the non-Archimedean continuum and the continuum of Dedekind, and prompted him to write a long appendix to his book *Fondamenti della geometria* which constitutes one of the first examples of the history of mathematics being deeply influenced by axiomatics.(21)

Other interesting examples are Peano's *Formulario* and Bourbaki's *Éléments de mathématiques*, both of which are extremely attentive to the axiomatic formulations and their history, explaining the first occurrence of a certain formula or the evolution of mathematical ideas.(22)

Architectural aim. Axiomatics can be used to restructure and better understand the global edifice of a science, which evolves according to the individual axiomatic theories of which it is composed, and is renewed thanks to new structural analogies allowed by the development of axiomatics itself (think for instance of the mother structures in [Bourbaki 1950] or the disciplinary reorganization that transformed geometry from a mathematical science to a physical science in the 19th century).(23)

The reorganization of the architecture of mathematics can also influence its position in the classification of sciences, a problem that was crucial at the end of the 19th and the beginning of the 20th century, because of the multiplication of mathematical disciplines, including arithmetic, geometry and analysis, but also probabilities, combinatorics vector spaces and hypercomplex systems, and the fragmentation of other scientific disciplines, which increased the number of possible applications.

(20) [Hilbert 1899].
(21) [Veronese 1891].
(22) See [Peano 1901], [Bourbaki 1939-1984] and [Dieudonné 1978].
(23) [Torretti 1978].

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Modular analysis. In some cases, the interest in a mathematical problem or theorem is not only related to the understanding of the role it plays in a given axiomatic system. What one wants to study is the amount of mathematics needed to derive the theorem or to solve the problem. An example of this is the interest for axiomatics developed by Hilbert in the *Grundlagen der Geometrie*, as he investigated what can be proved using only one or some of the five groups of axioms he isolated (incidence, order, congruence, parallelism and continuity).[(24)] This is the objective of reverse mathematics, to determine which axioms are necessary to prove a certain set of theorems or which set of theorems or which formal systems isolate the principles necessary to prove them.[(25)]

Pedagogical aim. Axiomatics can also be a pedagogical tool, not only because it allows a clear exposition of theories, but also because it develops students' abstraction skills.[(26)] Asking students to define a concept or prove a mathematical theorem leads to the study of several possible definitions and proofs, each based on distinct notions and rules.[(27)]

The effort to check that the mathematical content presented by the teacher is actually accessible to students at a certain level of study requires a distinction between elementary and non-elementary parts of a theory, and sometimes suggested alternative axiomatic formulations of a theory using only the axioms that are considered to be elementary. For example, Giuseppe Veronese distinguished an intuitive, experimental and practical method of teaching, which he considered adequate for young children, from a rational and rigorous axiomatic method, which he recommended not to introduce before the age of 14. The geometrical propositions taught in the two cases are not the same, because material or practical geometry is limited to the field of the observable: instead of stating geometric propositions about unlimited lines, he referred only to line segments, so as to include only propositions that can be experimentally verified.[(28)]

The pedagogical objective is often associated with a descriptive objective, because in the writing of a textbook the right order of

[(24)] [Hilbert 1899].
[(25)] [Friedman 1975, p. 235].
[(26)] [Piaget 1968].
[(27)] [Peano 1921].
[(28)] [Veronese 1909].

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concepts is the one that facilitates understanding without requiring concepts that are foreign to the learners' field of knowledge. The problem of purity — i.e., the search for a proof of a theorem that does not call upon notions foreign to the theory in question — originated in the philosophical concern to adhere to the Aristotelian prohibition rule on kind crossing: e.g., one can forbid the use of arithmetical tools in a geometrical proof or theorems of spherical geometry in plane geometry. But it also emerged in the didactic concern of presenting a proof that could also be made by students fully autonomously.

Coordination tool. If mathematics is considered as a social activity carried out by actual human agents or multi-agent systems that find themselves with the practical necessity to regulate their interactions, an axiomatic theory can be used to solve the problems of coordination.

This problem did not originate neither in the recent tendency of the philosophy of mathematics to deal with concrete scientific practices, nor in computer-assisted proofs. Peano already envisaged the urge to coordinate mathematical activity, as he justified the wish to build a collection of mathematical formulas by the need of a practical tool to distinguish what has already been demonstrated from what is still in need of a proof.[29]

Coordination problems can also be solved through a genetic-historical approach, which allows us to compare theories developed at different times and with different tools, and thus also to understand to what extent a new proof provided with different techniques can be more or less adequate or general than a pre-existing proof.

From the perspective of the external history of mathematics, which focuses on concrete institutions in which mathematical agents operate (universities, journals, research institutions, schools, etc.), the objective of coordination becomes essential to understanding the dynamics of collective endeavors, peer review and scientific controversies.

To the extent that the unification of mathematics or science is also part of the instruments that promote the coordination of mathematical agents, this objective also intersects the architectural goal mentioned above.

[29] [Peano 1896].

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§ 4. — Three conceptions of axiomatics in mathematics.

Axiomatics is generally investigated as a method that facilitates research on the foundations of mathematics. But recent developments in historical epistemology and social ontology might suggest alternative or complementary viewpoints: axiomatics can be considered as research on the foundations of mathematics, as a mathematical style, or as a social institution.

Axiomatics as research on the foundations of mathematics. Axiomatic systems and methods have often been introduced to deal with the question of the foundations of mathematics, that is to say, to determine what the principles of science are, by possibly distinguishing *the ordo essendi* from *the ordo cognoscendi*, i.e., what is first in itself from what is first for us. In this classical sense, originated in Aristotle's *Prior Analytics*, axiomatics has an ontological and cognitive scope. Key questions posed by axiomatics are reductionism (which principles are really independent of each other and which are those that can be reduced to principles of other disciplines or to logical principles?) and purity (is it possible to formulate an axiomatic theory without incorporating principles from other disciplines or sciences?). Within this general framework, different interpretations of the question of foundations (relating, for example, to rigor and the elimination of errors in mathematical definitions and proofs, or to the philosophical clarification of the nature of fundamental elements) and of formal systems as closed or open, lead to an enormous variety of axiomatic formulations of a theory.

Actually, there might be two distinct ways to approach the foundations of mathematics. By mathematical problem of the foundations, I mean here the objective to give a rigorous organization to the whole of mathematics, by providing a precise and unambiguous characterization of its key concepts (e.g., limit and continuity in the case of analysis).[30] Among the authors who have contributed to the renewal of mathematics in this sense I include Gauss, Abel, Cauchy, Bolzano, and in the historical phase of the arithmetization of the analysis, Weierstrass, Cantor and Dedekind (but also Kossak, Meray, Heine, Lipschitz and Tannery), who tried to avoid the use of the notion of geometric continuity in the definition of the properties of real numbers.

By logico-philosophical foundation of mathematics, I rather mean a reflection on the nature of symbolism and abstraction used in mathematical practices, on the conditions that legitimize their

[30] [Mangione and Bozzi 1993, p. 269].

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application and justification.[31] The fundamental question is associated with a hypothetico-deductive view of axiomatic theories and to the discussion of alternative philosophical points of view on mathematics: logicism, intuitionism and formalism.[32]

A similar distinction is made by Leo Horsten: the inquiry is called *foundational research* when the mathematicians themselves are "concerned with the foundations of their subject" and *philosophy of mathematics*, when "philosophers investigate philosophical questions concerning mathematics".[33]

Axiomatics is one of those areas that requires a joint interaction of the two approaches, so as to avoid philosophy of mathematics being reduced to a list of 'isms' that investigate the nature of mathematical entities or how we can have knowledge of them. Axiomatics (generally a Hilbert-style axiomatics) is often taken to be the general framework in which these discussions and analyses are made. It is seldom questioned in the philosophy of mathematics itself, but rather delegated to philosophy of logic.[34]

Axiomatics is also a general topic of philosophy of science: a unitarian view, guided by a predominantly ontological interest, is the Classical Model of Science, which presents a list of relevant features of the method that are considered as still influential in contemporary science.[35] The consideration of the mode of presentation of a set of propositions and how this architecture might be influential in forming the epistemic attitude of the reader is seldom discussed, but this was a central feature of logic in Bolzano and more generally in the 19th century. An exception is constituted by researchers who looked for a joint answer to philosophical and historical questions, thereby investigating axiomatics both as a cultural phenomenon and a foundational issue.[36]

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[31] [Mangione and Bozzi 1993, p. 262].

[32] [Shapiro 2005].

[33] See [Horsten 2022]. Note that philosophy of mathematics need not be exclusively identified with the investigation on the foundations of mathematics, as has been argued by philosophers of mathematical practices as well as by historians of mathematics. I will not enter in the discussion here, but the reader might find surveys of these approaches in [Carter 2019; Ferreirós and Gray 2006; Giardino 2017; Kerkove, De Vuyst, and Bendegem 2010; Mancosu 2008].

[34] See e.g., [Jacquette 2002]. But note that [Quine 1970] claimed that axiomatics does not have an impact on logical theory, because the latter is a set of logical truths, no matter which are chosen as axioms and which are derived from them.

[35] [Jong and Betti 2010].

[36] See e.g., [Ferreirós and Gray 2006].

Axiomatics as a mathematical style. When axiomatics is considered as a cultural feature that is embodied in a particular mathematical theory, it is legitimate to wonder whether it could be described as a style.[37] The term 'style' has been introduced in mathematics by analogy with artistic or literary styles, and has been brought into the limelight by different traditions. Chevalley, a member of the Bourbaki group, remarked that axiomatics has deeply modified the style of contemporary mathematical writings.[38] Granger developed a general study of style in mathematics, philosophy of language and human sciences.[39]

Crombie gave an ostensive explanation, individuating six fundamental styles that characterize the scientific enterprise: postulation (Greek mathematics), experiment, hypothetical construction of analogical models, ordering by composition and taxonomy, statistical analysis and probability, and historical derivation of genetic development. Hacking required two further conditions for the identification of a new style: novelty and persistence through self-stabilization. Historical epistemology also insisted on the cultural features of styles, as results of the activity of a specific school, nation or tradition.

Paolo Mancosu, comparing the use of the term made in history of mathematics and in mathematics, distinguished several kinds of style: individual, methodological, epistemic, national, cultural, writing, thinking or cognitive style.[40]

Nowhere is axiomatics characterized as a style, even if Crombie's characterization of the postulationist style of Greek mathematics goes in this direction and Chevalley highlighted the relation between axiomatics and style modifications.

The question here is how to characterize axiomatics as a style. The term might refer to the writing style of a certain mathematician, as when one speaks of axiomatics à la Dedekind or à la Peano, or, as is often done, of Hilbert-style axiomatics. In the latter case, an individual axiomatic style is also a methodological style originating in a research tradition: as Chevalley remarks, the ϵ-style

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[37] This idea first came to me while reading [Mancosu 2017], [Marquis 2022], [Rabouin 2017], and was supported by fruitful discussions with Frédéric Patras and Sébastien Maronne on Bourbaki.

[38] [Chevalley 1935, p. 375].

[39] [Granger 1988].

[40] [Mancosu 2017].

was an author's style that became specific to a mathematical era.[41] The same holds for Buss's distinction between proof theory in the Hilbert style, in which each step of a derivation is a formula, and proof theory in the Gentzen style, in which each step of a derivation is a sequent.[42]

In another sense, axiomatics can be considered as an element of a style of thinking, i.e., as a component of a paradigm or as an epistemic concept.[43] Axiomatics can still be interpreted as a cognitive style, be it the structure of human psychological development[44] or one of the many cognitive styles preferred by agents in teaching and learning.[45]

In a historical context, it is possible to consider the axiomatic style as a historiographical or mathematical category — think of the contrast between the deductive Aristotelian conception of mathematics, in which axioms are true and self-evident, and the Hilbertian hypothetico-deductive conception[46] — or as an individual style, illustrated for example by Zermelo in the case of set theory.[47]

The axiomatic style could then give rise to sub-styles. The distinction between (a) a hypothetico-deductive approach, which considers a theory as a closed system, (b) a semantic approach, which sees a theory as a set of models, and (c) an analytical approach — compatible with the understanding of a theory as an open system[48] — could be seen as a difference in axiomatic style.

Two other opposite forms of axiomatic style would be content-oriented axiomatics, which includes Euclid's geometry, Newton's mechanics and Clausius' thermodynamics, and formal or existential axiomatics.[49] Similarly, one could compare the style of the modernists, for whom mathematics has only to do with words, with the style of the anti-modernists, for whom mathematics has to do with objects.[50]

[41] [Chevalley 1935].
[42] [Buss 1998].
[43] See respectively [Kuhn 1962] and [Hacking 1999].
[44] For [Piaget 1947] the mental development of the child is associated with the progressive unraveling of logical-mathematical axioms.
[45] See [Borromeo Ferri 2005] and [Lerman 1990].
[46] [Kline 1990, vol. 3]
[47] See [Zermelo 1908] and [Gray 2008, p. 260]. See also [Lorenzo 1971].
[48] [Cellucci 2017].
[49] See [Hilbert and Bernays 1934, p. 2] and [Sieg and Ravaglia 2005, p. 987].
[50] See [Mehrtens 1990] and [Gray 2008, p. 9].

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Axiomatics as an institution. A third way of approaching axiomatics is to consider its persistence in time, although in changing forms, as a cognitive enterprise realized by human agents to regulate complex interactions. The possibility to distinguish axiomatics as a *type* from its instantiations (*tokens*) based on different methods and different formulations of theories, and the presence of rules, functions agents and roles, legitimates the question of whether axiomatics can be considered as a social institution. In social ontology, the philosophical discipline that studies social institutions and their components, an institution is characterized by rules, obligations, coordination problems, agents and roles.[51]

Formal axiomatics could be associated with the following set of rules: 1) determine the primitive terms of a theory and the axioms that implicitly define them; 2) make the legitimate rules of derivation explicit; and 3) specify which rule is applied at each stage of a proof. In this approach, some of the classical meta-theoretical problems associated with axiomatics could be considered as specific *obligations*. For example, consistency would be an obligation justified by the fact that it is undesirable that a statement and its negation are both axioms or theorems of the same system. Completeness in the Hilbertian (geometrical) sense could be considered as an obligation dictated by the desire that the axiomatic system has an adequate level of generality. Agents include human beings, computers and multi-agent systems. Roles include researcher, teacher, student, newspaper reporter, etc. Many coordination problems could be facilitated by axiomatics: *a*) avoid proving a theorem that has already been satisfactorily proven; *b*) provide a common conceptual framework for comparing different proofs of the same theorem and evaluate which one is the most adequate; *c*) facilitate the interaction and coordination between different researchers, groups of researchers and research projects in proving new results or formulating new conjectures; *d*) facilitate the unification of fragmented results into an organic unity.[52]

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[51] [Epstein 2016; Guala 2016].

[52] This interpretation of axiomatics as a social institution was the subject of a presentation at the ENPOSS 2020 international symposium.

§ 5. — Peano's example.

Peano developed axiomatics as a tool for the scientific study of the foundations of mathematics, i.e., for organizing the corpus of mathematical knowledge and providing a precise and adequate characterization of its key terms. Peano also dealt with the logico-philosophical foundations of mathematics, especially in his reflections on the nature of symbolism and abstraction used in mathematical practice, but without adhering to any form of philosophical foundationalism, since the search for rigor is based neither on rigid deductivism nor on any form of reductionism.

Peano developed a specific axiomatic style, based on the method of counterexamples applied to definitions in order to find the right level of generality (for example, providing a definition of the length of the curvilinear arc applicable to all concave surfaces), a style based on the refusal to lean clearly towards an exclusively extensional or exclusively intensional interpretation of logic and mathematics, a style based on a particular way of associating syntax, semantics, and pragmatics through the grammar of language.[53]

Finally, Peano institutionalized axiomatics through collaborative projects: the *Formulario*, the *Dizionario*, the *Revue des Mathématiques* and the publications of the *Academia pro Interlingua*. The journals were the instruments for coordinating the work, but participation in national and international conferences also provided an opportunity to invite contributions, critiques, and corrections to collaborative projects: e.g., the conference held in Livorno in 1901 launched the project for a dictionary of mathematics.

The institutional nature of axiomatics is not only the result of an intersubjective process, but is also shaped by the way interactions are regulated and certain tasks are assigned to agents. As can be seen from Enriques' reading of Peano, axiomatics can be seen as a set of four rules: 1) all concepts must be taken as primitives or logically derived from primitives; 2) primitive concepts must be independent; 3) all propositions must be taken as axioms or derived from axioms; 4) axioms must be independent.[54]

Each of these perspectives on axiomatics (inquiry into foundations, mathematical style, social institution) can be characterized

[53] See [Cantù 2022a] and [Luciano 2017].
[54] [Enriques 1924–27, pp. 11-12].

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by the study of the objectives it is called upon to fulfill, which, in Peano's case, are indeed multiple.

1. Axiomatics is essential to conceptual analysis, understood as a preliminary operation to the introduction of an ideography.
2. It serves a heuristic objective, facilitating the search for errors in the definitions, but also the search for new examples and problems.
3. It induces a quest for rigor, notably elimination of inadequate definitions and sophisms.
4. It has the descriptive capacity to represent contemporary mathematical practices.
5. Axiomatics also produces a history of notations and theorems.
6. It allows a comparison of alternative theories on the basis of criteria of simplicity.
7. It facilitates an architectural organization of mathematical knowledge.
8. Axiomatics also plays the role of a modular analysis, either of the theorems derivable from a single axiom, or of the axioms necessary to derive a given theorem;
9. It also has a didactic objective, aimed at clearly explaining the theories and developing the students' abstraction capacities;
10. Finally, axiomatics also plays the role of a coordination tool allowing mathematicians to quickly discover whether a certain proposition has already been proved and with what resources.

§ 6. — Conclusion.

This article has attempted to show how philosophy, even if not taken as a normative starting point of the inquiry, can contribute to a reflection on the axiomatic activity of mathematicians. It is often said that mathematicians do not work axiomatically, and that this method is mainly concerned with organization rather than scientific discovery. Here we have tried to show that axiomatics, if

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analyzed in detail through a study of its foundational component, of the styles with which it is associated and of the rules that govern it, performs a plurality of functions. But this analysis requires an interdisciplinary approach in which mathematics, philosophy and history of logic and mathematics are involved: conceptual analysis cannot be dissociated from the punctual study of mathematical theories and their historical development. The analysis of Peano's example offers insight into the kind of results that this interdisciplinary approach to mathematical practice might produce.

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Paola Cantú, Aix Marseille Université, ⋆
CNRS, Centre Gilles Gaston Granger. ⋆ ⋆

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Introducing Heuristic Philosophy of Mathematics

Carlo Cellucci

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Abstract. Mainstream philosophy of mathematics, namely the philosophy of mathematics that has prevailed for the past century, holds that mathematics is theorem proving by the axiomatic method. But this is incompatible with Gödel's incompleteness theorems, and cannot account for many features of mathematics. This article proposes an alternative approach, heuristic philosophy of mathematics, according to which mathematics is problem-solving by the analytic method. The article argues that this is compatible with Gödel's incompleteness theorems, and can account for the features of mathematics not accounted for by mainstream philosophy of mathematics, such as the nature of mathematical objects and mathematical definitions.

Keywords. Mainstream philosophy of mathematics • Mathematics as theorem proving • Heuristic philosophy of mathematics • Mathematics as problem-solving • Philosophy of mathematical practice • Mathematical objects • Mathematical definitions

§ 1. — Introduction.

The purpose of this article is to outline the main aspects of heuristic philosophy of mathematics.

Heuristic philosophy of mathematics is an approach to the philosophy of mathematics alternative to mainstream philosophy of mathematics, the philosophy of mathematics that has prevailed for the past century

Mainstream philosophy of mathematics consists of the three big foundational schools (logicism, formalism, intuitionism), their direct descendants (neo-logicism, neo-formalism, neo-intuitionism), and their indirect descendants (platonism, structuralism, fictionalism, nominalism, etc.).

The three big foundational schools and their direct or indirect descendants are different in several respects but have certain characteristics in common. These characteristics may serve to characterize mainstream philosophy of mathematics.

§ 2. — Mainstream Philosophy of Mathematics.

The main characteristics of mainstream philosophy of mathematics are the following.

1. The philosophy of mathematics cannot concern itself with the making of mathematics, in particular discovery, because discovery is a subjective process, so it cannot be justified.

2. The philosophy of mathematics can concern itself only with finished mathematics, namely mathematics as presented in finished form in journals, books, or lectures, because only finished mathematics is objective, so it can be justified.

3. Since the philosophy of mathematics cannot concern itself with the making of mathematics, it cannot contribute to the advancement of mathematics.

4. The task of the philosophy of mathematics is primarily to give an answer to the question: How do mathematical propositions come to be completely justified? And, subordinately to it, to the question: Do objects exist in virtue of which mathematical propositions are true (in some sense of 'true')? And, if such objects exist, what is their nature?

5. The method of mathematics is the axiomatic method. The latter is the method according to which, to obtain a demonstration

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of a proposition, one starts from given axioms, which are true (in some sense of 'true'), and deduces the proposition from them.

6. The role of demonstration is to guarantee the truth of a proposition.

7. Mathematics is a body of truths, and indeed truths that are certain. Therefore, mathematics is about truth and certainty.

§ 3. — Mainstream Philosophy of Mathematics and Closed Systems.

From the above description of the main characteristics of mainstream philosophy of mathematics, it appears that a basic assumption of mainstream philosophy of mathematics is that mathematics is theorem proving by the axiomatic method.

The basic assumption implies that mathematical theories are closed systems. A closed system is a system whose development does not involve receiving inputs from and delivering outputs to the outside. Its development remains completely internal to the system, so the system is a self-sufficient totality.

The basic assumption implies that mathematical theories are closed systems because a mathematical theory is based on axioms that are given once for all, and its development consists entirely in deducing propositions from the axioms.

Now, deduction is non-ampliative, namely the conclusion contains nothing essentially new with respect to the premisses. For, as Russell says, a deductive inference "consists merely in saying in other words part or the whole of what is said in the premisses" (Russell 1997, 360).

That deduction is non-ampliative has been repeatedly pointed out from antiquity. Thus, Epicurus says that "we will not say that syllogism can make us know anything new" (Epicurus, *De natura*, XXVIII, 17, 1, ed. Sedley). Descartes says that logicians cannot "form any syllogism leading to a true conclusion unless" they "already knew the very same truth which is deduced in the syllogism," so they "can learn nothing new from such form of reasoning" (Descartes 1996, X, 406). Kant says that "using deduction as a tool for an expansion of information comes down to nothing but idle chatter" (Kant 1998, A61/B86). De Morgan says that deduction is subject "to the great rule of all search after truth, that nothing is to be asserted as a conclusion, more than is actually contained in the

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premises" (De Morgan 1835, 99). Mill says that, in the conclusion of a deduction, there is "nothing but what was already asserted in the premisses" (Mill 1963–1986, VII, 160). Peirce says that deduction "is evidently entirely inadequate to" go "beyond the facts given in the premisses" (Peirce 1931–1958, 2.681). Poincaré says that deduction is "incapable of adding anything to the data" which are "given it; these data reduce themselves" to "axioms, and we should find nothing else in the conclusions" (Poincaré 2015, 31). Wittgenstein says that "if one proposition follows from another, then the latter says more than the former, and the former says less than the latter" (Wittgenstein 2002, 5.14).

Since deduction is non-ampliative, the propositions deduced from the axioms are already implicitly contained in the axioms. Therefore, the development of a mathematical theory remains completely internal to the theory, it does not involve interactions with other mathematical theories.

As Hintikka says, in the axiomatic method you compress "all the truths about" a given "subject matter into" a "set of axioms," which "are supposed to tell you everything there is to be told about this subject matter" (Hintikka 1998, 1). Then "the rest of your work will consist in merely teasing out the logical consequences of the axioms. It is sufficient to study the axioms" (ibid., 2).

§ 4. — Mainstream Philosophy of Mathematics and Gödel's Incompleteness Theorems.

The basic assumption of mainstream philosophy of mathematics, that mathematics is theorem proving by the axiomatic method, is refuted by Gödel's incompleteness theorems.

The basic assumption is refuted by Gödel's first incompleteness theorem because, by the latter, for any consistent, sufficiently strong, formal system, there are propositions of the system that are true but cannot be deduced from the axioms of the system. This implies that mathematics cannot consist in the deduction of propositions from given axioms because, for any choice of axioms for a given part of mathematics, there will always be true propositions of that part which cannot be deduced from the axioms.

The basic assumption is also refuted by Gödel's second incompleteness theorem because, by the latter, for any consistent, sufficiently strong, formal system, it is impossible to demonstrate, by

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absolutely reliable means, that the axioms of the system are consistent. So, there is no guarantee that the propositions deduced from the axioms are justified knowledge, because from inconsistent axioms all propositions, even logically contradictory ones, can be deduced.

§ 5. — Other Shortcomings of Mainstream Philosophy of Mathematics.

In addition to the shortcoming shown by Gödel's incompleteness theorems, mainstream philosophy of mathematics has other shortcomings.

1. Mainstream philosophy of mathematics does not account for the fact that solving a problem of a given part of mathematics may require hypotheses from other parts of mathematics.

For example, to solve the problem posed by Fermat's conjecture, which is a problem about the integers, Ribet used a hypothesis about modular forms in hyperbolic space, the Taniyama-Shimura conjecture: Every elliptic curve over the rational numbers is modular. Then, to solve the problem posed by the Taniyama-Shimura conjecture, Wiles and Taylor used hypotheses from various parts of mathematics, from differential geometry to complex analysis.

Mainstream philosophy of mathematics does not account for this fact because, according to it, a solution to a problem of a given part of mathematics should be deduced from the axioms for that part.

2. Mainstream philosophy of mathematics does not account for the fact that a demonstration yields something new.

For, according to it, a solution to a problem is deduced from the axioms. Now, a deduction from the axioms cannot yield anything essentially new with respect to them because, as already said, deductive rules are non-ampliative.

3. Mainstream philosophy of mathematics does not account for the fact that new solutions, even hundreds of them, are often sought for problems for which a solution is already known.

In fact, giving new demonstrations of result already demonstrated has been a salient feature of mathematics since antiquity. As Knorr says, "multiple proofs were frequently characteristic of pre-Euclidean studies" (Knorr 1975, 9). But they have been frequently characteristic of post-Euclidean studies as well. For example, for the Pythagorean theorem over four hundred demonstrations are

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known, and their number is still growing. As another example, a Fields Medal was awarded to Selberg (among other important results) for producing a new demonstration of a theorem, the prime-number theorem, for which a demonstration was already known. For several examples of theorems with multiple demonstrations, see Dawson (2015).

Mainstream philosophy of mathematics does not account for this fact because, according to it, a demonstration establishes the truth of a theorem, and this is its function. Then, once the truth of a theorem has been established, there is no point in establishing it once again by another demonstration, let alone by hundreds of them.

Wittgenstein says: "Every proof, even of a proposition which has already been proved, is a contribution to mathematics". But "why is it a contribution if its only point was to prove the proposition?" (Wittgenstein 1978, III, § 60). Mainstream philosophy of mathematics cannot answer this question.

Heuristic philosophy of mathematics aims to remedy the shortcomings of mainstream philosophy of mathematics.

§ 6. — Heuristic Philosophy of Mathematics.

The main characters of heuristic philosophy of mathematics are the following.

1. The philosophy of mathematics can concern itself with the making of mathematics, in particular discovery, because discovery is an objective process, so it can be accounted for.

2. The philosophy of mathematics can concern itself also with finished mathematics. But finished mathematics is never really finished, because every mathematical concept or hypothesis can always be called into question, modified, or reinterpreted.

3. Since the philosophy of mathematics can concern itself with the making of mathematics, it can possibly contribute to the advancement of mathematics. In fact, from antiquity, the philosophy of mathematics has repeatedly contributed to it. For example, as Grabiner says, Berkeley's attack on the calculus "pointed out real deficiencies" and indicated "the questions which had to be answered if a successful foundation were to be given" (Grabiner 2005, 27).

4. The task of the philosophy of mathematics is primarily to give an answer to the question: How is mathematics made? And,

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subordinately to it, to the questions: What is the nature of mathematical objects, demonstrations, definitions, diagrams, notations, explanations, beauty, applicability, and knowledge?

5. The method of mathematics is the analytic method. The latter is the method according to which, to solve a problem, one looks for some hypothesis that is a sufficient condition for solving the problem, namely such that a solution to the problem can be deduced from the hypothesis. The hypothesis is obtained from the problem, and possibly other data, by some non-deductive rule. The hypothesis must be plausible, namely the arguments for the hypothesis must be stronger than the arguments against it, on the basis of experience. But the hypothesis is in turn a problem that must be solved, and is solved in the same way. Namely, one looks for another hypothesis that is a sufficient condition for solving the problem posed by the previous hypothesis. The new hypothesis is obtained from the previous hypothesis, and possibly other data, by some non-deductive rule, and must be plausible. And so on. Thus, solving a problem is an ongoing process. The analytic method dates back to the beginnings of mathematics. Its first documented uses are the solutions of Hippocrates of Chios to the problems of doubling the cube and the quadrature of certain lunules. For them, and the analytic method generally, I refer the interested reader to Cellucci 2022, chap. 5.

6. The role of demonstration is to discover a plausible solution to a problem.

7. Mathematics is a body of problems and plausible solutions to them. Therefore, mathematics is not about truth and certainty, but about plausibility.

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§ 7. — Origin of Heuristic Philosophy of Mathematics.

The origin of heuristic philosophy of mathematics can be credited to Lakatos.

Indeed, Lakatos criticizes mainstream philosophy of mathematics because in it "there is no proper place for methodology qua logic of discovery" (Lakatos 1976, 3). Mainstream philosophy of mathematics "denies the status of mathematics to most of what has been commonly understood to be mathematics," in particular it can say nothing about "the 'creative' periods" and "the 'critical' periods

of mathematical theories" (ibid., 2). On the contrary, the philosophy of mathematics must be concerned with methodology qua logic of discovery. Although there is no infallibilist logic of discovery, namely "one which would infallibly lead to results, there is a fallibilist logic of discovery" (ibid., 143–144, footnote 2). The latter consists in "the method of proof and refutations" (ibid., 50). According to it, a mathematician discovers solution to problems "by trial and error" (ibid., 73).

Lakatos's approach, however, has an important shortcoming. Lakatos assumes that the logic of discovery consists in the method of proof and refutations, according to which a mathematician discovers solutions to problems by trial and error. The assumption is untenable, because the number of trials a mathematician can make is very small with respect to all possible ones, so the probability that a mathematician may discover solutions to problems by trial and error is very low. This contrasts with the fact that over 100,000 research papers in mathematics are published every year.

Lakatos himself ends up admitting that the method of proof and refutations does not provide a basis for methodology qua logic of discovery. Indeed, he says that "modern methodologies or 'logics of discovery' consist merely of a set" of "rules for the appraisal of ready, articulated theories" (Lakatos 1978, I, 103). These rules do not "give advice" as to "how to arrive at good theories" (Lakatos 1971, 174). They only give "directions for the appraisal of solutions already there" (Lakatos 1978, I, 103, footnote 1).

Now, as Nickles says, it "is astonishing" that "Lakatos's methodology provides ways to appraise" solutions already there, "but stops short of giving advice" (Nickles 1987, 119). For, "the idea of a heuristic methodology which gives no advice is a contradiction in terms. Bluntly stated, Lakatos has no methodology" (ibid.,120).

This does not invalidate the claim that the origin of heuristic philosophy of mathematics can be credited to Lakatos. But it means that, with respect to heuristic philosophy of mathematics, Lakatos is a sort of 'non-playing captain', namely a captain who is not in the field when the game takes place.

Heuristic philosophy of mathematics, as formulated here, is not subject to the shortcoming of Lakatos's approach concerning the method of proof and refutations. For, it replaces the method of proof and refutations with the analytic method, in which hypotheses are discovered, not by trial and error, but by non-deductive inferences.

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§ 8. — Other Objections to Heuristic Philosophy of Mathematics.

In addition to the objection concerning Lakatos's method of proof and refutations, other objections have been raised against Lakatos. They, however, are invalid. I will consider them because such objections could also be raised against heuristic philosophy of mathematics as formulated here, therefore, it is important to show that they are invalid.

1. Feferman says: "Lakatos' fireworks briefly illuminate limited portions of mathematics," but only deductive "logic gives us a coherent picture of mathematics," it "alone throws light on what is distinctive about mathematics, its concepts and methods" (Feferman 1998, 93). The "logical analysis of the structure of mathematics has been especially successful," one can use formal systems also "to model the growth of mathematics" (ibid., 92).

But this objection is invalid. By the strong incompleteness theorem for second-order logic, there is no consistent formal system for second-order logic capable of deducing all second-order logical consequences of any given set of propositions. Now, much of mathematics requires second-order logic or beyond. Therefore, deductive logic cannot be said to give us a coherent picture of mathematics, nor to throw light on what is distinctive about mathematics, its concepts and methods. Moreover, deductive logic is non-ampliative, so it cannot explain why a demonstration may yield something new. Therefore, formal systems cannot be used to model the growth of mathematical knowledge.

2. Smoryński says: "Lakatos firmly denies the distinction between mathematics, on the one hand, and the "sciences on the other," but "I cannot accept such a denial" (Smoryński 1983, 11). Mathematics and the sciences are essentially different because, in the sciences there are revolutions, while mathematics "is a cumulative body of knowledge" (ibid.).

But this objection is invalid. Mathematics is not a cumulative body of knowledge, there are revolutions also in mathematics. This depends on the fact that in mathematics we start from problems, we formulate hypotheses for their solution by non-deductive inferences, and we establish the plausibility of hypotheses through a comparison with experience.

Now, for their solution, certain problems require hypotheses that cannot be deduced from the existing mathematics. Such

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hypotheses change mathematics in a profound and far-reaching way. For example, the problems of the infinitesimal calculus required hypotheses that could not be deduced from the (then) existing mathematics. They changed mathematics in a profound and far-reaching way. Hypotheses that cannot be deduced from the existing mathematics give rise to revolutions in mathematics. For more on revolutions in mathematics, I refer the interested reader to Cellucci 2022, chap. 10.

3. Mancosu says: "The predominance" of mainstream "approaches to the philosophy of mathematics in the last twenty years proves" that "the 'maverick tradition'," namely the tradition originating from Lakatos, "did not manage to bring about a major reorientation of the field" (Mancosu 2008, 5). Indeed, "logically trained" mainstream philosophers of mathematics "felt that the 'mavericks' were throwing away the baby with the bathwater" (ibid., 5–6).

But this objection is invalid. The 'baby' thrown away is the assumption that mathematical reasoning consists of deductive reasoning. This assumption is untenable because it is refuted by Gödel's first incompleteness theorem. Moreover, the predominance of mainstream approaches to the philosophy of mathematics in the last twenty years does not prove that the maverick camp has failed. As Dewey says, "old ideas give way slowly" because they are "deeply engrained," but eventually they are abandoned because of "their decreasing vitality" (Dewey 1910, 19). Now, there are clear signs that the ideas of mainstream philosophy of mathematics have shown a decreasing vitality.

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§ 9. — Heuristic Philosophy of Mathematics and Open Systems.

From the description of the main characteristics of heuristic philosophy of mathematics given above, it is clear that the basic assumption of heuristic philosophy of mathematics is that mathematics is problem-solving by the analytic method.

The basic assumption implies that mathematical theories are open systems. An open system is a system whose development involves receiving inputs from and delivering outputs to the outside. Thus, its development does not remain internal to the system, so the system is not a self-sufficient totality.

The basic assumption implies that mathematical theories are open systems because a mathematical theory initially consists only of problems to be solved and possibly other data already available. Its development consists in obtaining more and more hypotheses to solve the problems by non-deductive rules, and in checking that the hypotheses are plausible.

Since the hypotheses are obtained by non-deductive rules and non-deductive rules are ampliative, namely the conclusion contains something essentially new with respect to the premisses, the hypotheses are not implicitly contained in the problems and the other data already available. Moreover, the hypotheses need not belong to the same part of mathematics as the problems. They may belong to other parts of mathematics. So, the development of a mathematical theory may involve interactions with other mathematical theories.

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§ 10. — Heuristic Philosophy of Mathematics and Gödel's Incompleteness Theorems.

The basic assumption, that mathematics is problem-solving by the analytic method, is unaffected and even confirmed by Gödel's incompleteness theorems.

The basic assumption is unaffected and even confirmed by Gödel's first incompleteness theorem, because the analytic method does not confine mathematics within the closed space of an axiomatic system. It lets mathematics develop in an open space, making use of interactions with other systems of knowledge. Indeed, in the analytic method, the solution to a problem is obtained from the problem, and possibly other data already available, by means of hypotheses not necessarily belonging to the same part of mathematics as the problem. Now, by Gödel's first incompleteness theorem, for any consistent, sufficiently strong, formal system, there are propositions of the system that are true but cannot be deduced from the axioms of the system. So, solving a problem of a given part of mathematics may require hypotheses from other parts.

The basic assumption is also unaffected and even confirmed by Gödel's second incompleteness theorem, because the analytic method does not assume that the solution to a problem is certain. Indeed, in the analytic method, the hypotheses for the solution to

a problem are only plausible, therefore no solution to a problem can be certain. Now, by Gödel's second incompleteness theorem, for any consistent, sufficiently strong, formal system, it is impossible to demonstrate, by absolutely reliable means, that the axioms of the system are consistent. So, since we cannot know whether the axioms are consistent, no solution to a problem can be certain.

§ 11. — Other Advantages of Heuristic Philosophy of Mathematics.

In addition to being unaffected and even confirmed by Gödel's incompleteness theorems, heuristic philosophy of mathematics does not have the other shortcomings of mainstream philosophy of mathematics.

1. Heuristic philosophy of mathematics accounts for the fact that solving a problem of a given part of mathematics may require hypotheses from other parts of mathematics.

For, according to the analytic method, the hypotheses to solve a problem need not belong to the same part of mathematics as the problem, they may belong to other parts of mathematics.

2. Heuristic philosophy of mathematics accounts for the fact that a demonstration may yield something new.

For, according to the analytic method, the hypotheses for the solution to a problem are obtained from the problem, and possibly other data, by some non-deductive rule. So, they contain something essentially new with respect to them, because non-deductive rules are ampliative.

3. Heuristic philosophy of mathematics accounts for the fact that new solutions, even hundreds of them, are often sought for problems for which a solution is already known.

For, according to the analytic method, a mathematical problem can be seen from different perspectives, each of which may suggest different hypotheses that may lead to different solutions to the problem. Each solution establishes new relations between the problem and other parts of mathematics, showing the problem in a new light. Therefore, a new solution to a problem is a contribution to mathematics.

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§ 12. — Difference from the Philosophy of Mathematical Practice.

Heuristic philosophy of mathematics must not be confused with the philosophy of mathematical practice.

In recent years, the philosophy of mathematical practice has turned into a variety of different approaches, sometimes even in contrast with each other. Attempts to classify them have been made (see Van Bendegem 2014, Carter 2019), but they do not reveal a unity of the different approaches. A unitarian approach results only from the papers in Mancosu (2008), the first collection of essays on the philosophy of mathematical practice. Therefore, in what follows, by 'the philosophy of mathematical practice' I will exclusively mean that approach of the papers in Mancosu (2008), as presented in the Editor's Introduction.

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Mancosu says that the philosophers of mathematical practice "do not engage in polemic with the foundationalist tradition" (Mancosu 2008, 18). In particular, they reject "the polemic against the ambitions of mathematical logic as a canon for philosophy of mathematics," and do not consider mathematical logic to be "ineffective in dealing with the questions concerning the dynamics of mathematical discovery" (ibid., 4).

The philosophers of mathematical practice are only "calling for an extension" of the foundationalist tradition to "topics that the foundationalist tradition has ignored," namely topics concerning "aspects of mathematical practice" (ibid., 18).

That does not mean that the three big foundationalist schools were "removed from such concerns" (ibid., 6–7). Thus, Frege's development of a formal language aimed at capturing all valid forms of reasoning occurring in mathematics "required a keen understanding of the reasoning patterns to be found in mathematical practice" (ibid., 7). Hilbert's "distinction between real and ideal elements" also "originates in mathematical practice" (ibid.). Brouwer's intuitionism originated "from the distinction between constructive vs. non-constructive procedures" which was prominent "in algebraic number theory in the late 19th century" (ibid.). The direct and indirect descendants of the three big foundationalist schools "are also, to various extents, concerned with certain aspects of mathematical practice" (ibid.).

However, the three big foundationalist schools and their direct or indirect descendants "were limited to a central, but ultimately

narrow, aspect of the variety of activities in which mathematicians engage" (ibid.). Conversely, the philosophers of mathematical practice "cover a broad spectrum of case studies arising from mathematical practice" (ibid.,18). They extend the investigation "to a variety of areas that have been, by and large, ignored" but "are absolutely vital to an understanding of mathematics" (ibid.).

This, however, is a difference in quantity, not in quality between the philosophy of mathematical practice and mainstream philosophy of mathematics, therefore the former is continuous with the latter. As a result, the philosophy of mathematical practice inherits all the shortcomings of mainstream philosophy of mathematics.

§ 13. — Different Answers to Basic Questions.

As I have already said, according to heuristic philosophy of mathematics, the task of the philosophy of mathematics is primarily to give an answer to the question: How is mathematics made? And, subordinately to it, to the questions: What is the nature of mathematical objects, demonstrations, definitions, diagrams, notations, explanations, beauty, applicability, and knowledge?

Heuristic philosophy of mathematics and mainstream philosophy of mathematics give different answers to these questions. It would require much space to consider all of them, so I will only consider two of them, the nature of mathematical objects and the nature of mathematical definitions, which are strictly related.

§ 14. — Mathematical Objects.

First, I consider the nature of mathematical objects.

Mainstream philosophy of mathematics introduces and justifies mathematical objects on metaphysical grounds.

For example, Gödel introduces and justifies sets by saying that they belong to "a non-sensual reality, which exists independently both of the acts and of the dispositions of the human mind" (Gödel 1986–2002, III, 323). Yet, "despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory" (ibid., II, 268). Through it, sets "are known precisely, and general laws" about them "can be recognized with certainty" (ibid., III, 312, footnote 18).

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On the contrary, heuristic philosophy of mathematics does not introduce and justify mathematical objects on metaphysical grounds. According to it, mathematical objects are hypotheses mathematicians introduce to solve mathematical problems by the analytic method. Like all hypotheses in the analytic method, these hypotheses must be plausible.

When mathematical objects, introduced as hypotheses to solve a problem by the analytic method, turn out to be useful also to solve other problems, they consolidate and acquire a stability that makes them independent of the problem for which they were originally introduced, and become subjects of study themselves.

Therefore, according to heuristic philosophy of mathematics, mathematicians do not accept or reject mathematical objects on metaphysical grounds, but because they are, or are not, functional to the advancement of mathematics.

For example, Bombelli introduced imaginary numbers as a means to solve cubic equations. At first, imaginary numbers encountered strong resistance. Like Musil's young Törless, several mathematicians objected that "the square of every number, whether it's positive or negative, produces a positive quantity. So there can't be any real number that could be the square root of a minus quantity" (Musil 1986, 76). Eventually, however, mathematicians accepted imaginary numbers, not on metaphysical grounds, but because they were functional to the advancement of mathematics. Thus, Gauss said "If these imaginary quantities were to be neglected", the infinitesimal calculus "would lose immensely in beauty and roundness", and we "would be forced to add very hampering restrictions to truths which otherwise would hold generally" (Gauss 1880, 156).

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§ 15. — The Stipulative View of Mathematical Definition.

Now I consider the nature of mathematical definitions.

According to mainstream philosophy of mathematics, a definition merely stipulates the meaning of a term, it is only an abbreviation, so it is always correct, and can be eliminated.

This view of definition, which is known as 'the stipulative view of definition', goes back to ancient Greece. Thus, Aristotle says: "One type of definition will be the statement of what a name" means, "for example, what triangle means" (Aristotle, *Analytica Posteriora*, B 10, 93 b 30–32).

But Aristotle considers this kind of definition only to criticize it. Indeed, he argues that, if a definition were simply a statement of what a name means, this would have the following undesirable consequences.

1. "There would be definitions of non-substances and of things which do not exist, because it is possible to mean also things which do not exist" (Aristotle, *Analytica Posteriora*, B 7, 92 b 27–30).

2. "All expressions would be definitions. For, it is possible to assign a name to any expression whatever, so all our discourses would be definitions," for example, the entire poem "*Iliad* would be a definition" (ibid., B 7, 92 b 30–32).

3. "No demonstration could demonstrate that this name makes known this thing; therefore, definitions could not make known this thing either" (ibid., B 7, 92 b 32–34).

To the stipulative view of definition, Aristotle opposes the essentialist view of definition, which is based on Aristotle's fundamental assumption that "to know a thing is to know its essence," since "any single thing and its pure essence coincide" (ibid., Z 6, 1031 b 19–21). So, the object of science is to know the essence of things. Then, in particular, "a definition is said to be a statement of the essence of a thing" (ibid., B 10, 93 b 29).

Then, Galileo replaced Aristotle's fundamental assumption that the object of science is to know the essence of things by the fundamental assumption that gave rise to modern science: the object of science is to know phenomenal properties of things, mathematical in kind. Therefore, "the definitions of mathematicians" do not state the essence of things, they are only "an imposition of names, or we might say abbreviations of speech" (Galilei 1968, VIII, 74). So, they are arbitrary stipulations and, "being arbitrary, can never be bad" (ibid., IV, 700). Thus, Galileo revived the stipulative view of definition.

The stipulative view has prevailed in the past century especially as a result of Hilbert's replacement of the material axiomatic method with the formal axiomatic method.

Indeed, according to Hilbert, a "theory has nothing to do with the real objects and with the intuitive content of knowledge; it is a pure thought construct, of which one cannot say that is true or false" (Hilbert 2013, 435). So, "the axioms can be taken quite arbitrarily" (Hilbert 2004, 563). And a definition is "a mere explanation of signs" (Hilbert and Bernays 1968–1970, I, 292). Thus, definitions are arbitrary stipulations.

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§ 16. — Shortcomings of the Stipulative View of Mathematical Definition.

Despite its prevailing in the past century, the stipulative view of mathematical definition has important shortcomings.

1. The stipulative view does not account for the fact that finding an adequate definition can make the difference in discovering a solution to a problem.

For example, let us consider the definition of a sphere. There are two historically important definitions of a sphere, one by Plato, Aristotle, and Theodosios, the other by Euclid.

Plato, Aristotle, and Theodosios define a sphere as a solid figure with every point on its surface equidistant from its centre.

For example, Theodosios defines a sphere as "a solid figure contained by one surface, such that all the straight lines falling upon it from one point among those lying inside the figure are equal to each other" (Theodosios, *Sphaerica*, I, Def. 1, ed. Heiberg).

On the contrary, Euclid defines a sphere as a solid figure generated by a semicircle revolving about its diameter.

Indeed, Euclid defines a sphere as "the figure comprehended when, the diameter of a semicircle remaining fixed, the semicircle is carried around and restored again to the same position from which it began to be moved" (Euclid, *Elementa*, XI, Definition 14).

On the other hand, while Euclid defines a sphere by referring to motion, he defines a circle without referring to motion.

Indeed, Euclid defines a circle as "a plane figure contained by one line, such that all the straight lines falling upon it from one point among those lying inside the figure are equal to each other" (Euclid, *Elementa*, I, Definition 15).

Why does Euclid define a sphere and a circle in different ways? This is because Euclid's definition of a sphere plays an important heuristic role in Euclid's solution to the problem about the five Platonic figures.

Finding a solution to that problem was very important to Euclid because, as Thomas says, the solution to the problem about the five Platonic figures is "the culmination of the work" of Euclid's *Elements* "as a whole" (Thomas 2014, 234).

By referring to motion, Euclid's definition of a sphere guided him to find a solution. Thus, such definition made the difference in discovering a solution to the problem. This explains why Euclid defines a sphere and a circle in different ways.

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This cannot be accounted for, as the stipulative view claims, a definition is only an abbreviation.

2. The stipulative view does not account for the fact that mathematicians often use concepts for a long time, even centuries, before they can find a suitable definition for them.

For example, as Grabiner says, "Fermat implicitly used" the derivative; "Newton and Leibniz discovered it; Taylor, Euler, Maclaurin developed it; Lagrange named and characterized it; and only at the end of this long period of development did Cauchy and Weierstrass define it" (Grabiner 2010, 159–160). So, "the historical order of development of the derivative is the reverse of the usual order of textbook exposition," in which "one starts with a definition, then explores some results," thus "a definition is often the end, rather than the beginning, of a subject" (ibid., 160).

This cannot be accounted for if, as the stipulative view claims, a definition is only an abbreviation.

3. The stipulative view does not account for the fact that mathematicians often give definitions that afterwards turn out to be incorrect.

For example, Jordan defines a curve in a way that corresponds to the idea that a curve is what is generated if a point runs along in continuous motion. The motion of the point will be completely described by stating how the two coordinates x and y of the point depend on time t.

Indeed, Jordan defines a curve as "the succession of points represented by the equations $x = f(t), y = \phi(t)$, where f, ϕ are functions of an independent variable t. If these functions are continuous, the curve will be called continuous" (Jordan 1887, 587).

However, Peano defines something that is a continuous curve according to Jordan's definition, but "goes through every point of a square" (Peano 1973, 144).

Now, a curve is a one-dimensional object, while a square is a two-dimensional object. Since, on Jordan's definition, there is a curve that is a square, then Jordan's definition of curve or continuous curve is incorrect.

This cannot be accounted for if, as the stipulative view claims, a definition is always correct.

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§ 17. — The Heuristic View of Mathematical Definition.

An alternative view of mathematical definition is the heuristic view. It derives from the heuristic view of mathematical objects.

Indeed, if mathematical objects are hypotheses human beings make to solve mathematical problems by the analytic method, then so are the mathematical definitions that introduce them.

Thus, mathematical definitions are hypotheses that are made to solve mathematical problems by the analytic method. Like all hypotheses in the analytic method, such hypotheses must be plausible.

§ 18. — Advantages of the Heuristic View of Mathematical Definition.

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The heuristic view does not have the shortcomings of the stipulative view.

1. The heuristic view accounts for the fact that finding a suitable mathematical definition can make the difference in discovering a solution to a problem.

For, a mathematical definition is a hypothesis, and finding a suitable hypothesis is a crucial step towards finding a solution to a problem.

2. The heuristic view accounts for the fact that mathematicians often use concepts for a long time, even centuries, before they can find a suitable definition for them.

For, finding an adequate definition can make the difference in discovering a solution to a problem. So, finding an adequate definition may require as much effort and time as finding any hypothesis capable of yielding a solution to a problem.

3. The heuristic view accounts for the fact that mathematicians often give definitions that afterwards turn out to be incorrect.

For, a mathematical definition is a hypothesis that is plausible, and a hypothesis that is plausible at one stage may become implausible at a later stage, when the arguments against the hypothesis become stronger than those for it.

Thus, mathematicians thought that Jordan's definition of a curve was plausible, but then Peano gave an argument that made it implausible.

§ 19. — Conclusion.

From what I have said, it seems fair to conclude that heuristic philosophy of mathematics is more adequate than mainstream philosophy of mathematics.

I have argued for this by considering the nature of mathematical objects and definitions. But I could have argued for it equally well by considering the nature of mathematical demonstrations, diagrams, notations, explanations, beauty, applicability, and knowledge. For them, I refer the interested reader to Cellucci 2022, chap. 10–17.

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Carlo Cellucci, Università degli Studi "La Sapienza" di Roma.

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Some Personal Remarks

José Ferreirós

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Philosophy is interested in the big question of human knowledge, its sources and limits; and, mathematics being a central ingredient in the elaboration of human knowledge, a philosopher can hardly avoid questions about mathematical concepts and methods (and objects too). The traditional debates surrounding this issue have been trapped between the extreme positions of platonism (math depicting, or trying to depict, a fully independent realm of objects and their interrelations) and mentalism (math as a human creation, its objects mere products of the mind). Yet I believe that none of these extremes is true to the topic: math originates in our experience, both passive ("outer") and active experience, which leads to discoveries about patterns in the world; but math grows through the assumption of hypothetical states of affairs, and it is devoted to exploring the properties of such hypothetical 'world pictures', developing methods that help in the exploration.[(1)] Thus mathematics is neither just discovered nor merely invented. The mathematical experience presents us with the phenomenon of discovering features of structures that we have partly constructed.

[(1)] See Feferman 2014, Ferreirós 2022.

§ 1. — Mathematics and nature.

About a century ago, there were attempts to establish that all human knowledge that is not empirical, can be reduced to logical laws and definitions (expressing the "conceptual analysis" of the meanings of crucial words), i.e. consists of analytic truths. But the underlying idea of a pure and transparent logico-linguistic rendition of empirical states of affairs (a kind of mirroring without distortion) is very likely to be unrealistic. Human knowledge seems to be more a matter of *synthesizing* models of the phenomena, models where we employ hypothetical assumptions (distortions, perhaps) in the effort to capture basic traits of the empirical or the given. This was, after all, the message that Poincaré tried to convey 120 years ago in *Science and Hypothesis* (Poincaré 1902): Synthesis, not mere analysis; conventions, not merely the empirically given and the a priori.

The distinction between pure and applied math is in large measure a product of recent institutional developments, not fully separable from some ideological assumptions. As 'purely' mathematical journals, departments, and congresses rose and grew from the 1830s onward, stably supported by our industrial societies, the ideology of pure math became dominant. But no area of human knowledge is fully separable from the rest, and it's impossible to make sense of mathematical knowledge around the year 2,000 without considering its history — a history that cannot be understood without the intricate links between math and knowledge of nature.(2) Thus, defending the honor of the human mind is fully compatible with partaking in the exploration of natural phenomena: Fourier and Jacobi are doomed to understand each other, in the end.

One could of course establish an artificial division of topics, regarding as "mathematical" only what has to do with logical deduction of consequences of established axioms, and considering non-mathematical, "scientific," what relates to the creation of models, the introduction of hypotheses eventually solidified in axioms. But imposing such a division onto real historical figures (Descartes, Newton, Gauss, Riemann), we would be cutting them in halves. By this criterion, Riemann was a 'scientist' most of the time, not a mathematician; Zermelo would have been a 'scientist' when he

(2) Again a message of Poincaré and Weyl (1951) — for examples, just consider the number systems (real or complex), the continuum, the function concept, or Fourier series.

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realized the important role played by the Axiom of Choice, and a mathematician when he worked on his systematic presentation of an axiom system. The two moments can be distinguished, indeed, but the reality of mathematical (and scientific) practices is that both are interconnected.

§ 2. — The problems of growth and objectivity.

The growth of mathematical knowledge is for me one of the big questions, in many directions. One aspect is the problem of cognition and mathematics, which of course is well known in connection with numerical cognition (studies of Dehaene, Butterworth, Carey, Overmann and others), but where I am particularly interested in the emergence and configuration of geometric thinking. This is a most traditional philosophical topic, but I do believe that it can be advanced in interesting ways by means of an interdisciplinary approach that integrates new historical insights with cognitive archaeology, new philosophical orientations with detailed work in cognitive science. Another aspect of the question is the relation of mathematical ideas to semiotic systems (forms of representation) and to human action; mathematical practices cannot exist without the semiotic component — diagrams, notations, formulas — and so the cognitive basis of math is not limited to the brain. Yet another is the historical study of the web of practices, how mathematical work is interlinked with basic human practices, with scientific work, with techniques. This includes the study of the interactions math-science, which for some time had been left out of the philosopher's agenda, and subsequently was posed in a biased way in terms of the 'problem of the applicability' of math. As I suggested above, and explained in more detail elsewhere, this formulation is ideologically laden.

Also central is the question of the objectivity of mathematical work. I agree with those who think that the main issue is the objectivity of mathematical discourse (or practices, or knowledge), not the problem of mathematical objects. How the assumption of mathematical objects derives from objective mathematical discourse, is rather well understood (Tait 2005, Parsons 2009), although more should be made to summarize and popularize this body of work.[(3)]

[(3)] I try to do such a thing in the introduction to a forthcoming paper (*Topoi*, special issue on 'Mathematical Practice and Social Ontology').

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But the question how conceptual products can be objective, has not been explored in sufficient depth; an important part of my work focuses on this issue. My standpoint assumes that mathematics is first and foremost conceptual work, conceptual development of models and methods; it emphasizes the cognitive and pragmatist roots of mathematical knowledge; and it fully assumes its historicity, arguing that it doesn't conflict with intersubjective objectivity (see my 2016 book).

§ 3. — Meaning in mathematics.

Another big question is the problem of meaning in mathematics, a difficult question intimately connected with the issue of conceptual development and conceptual understanding.[4] Perhaps one should begin here remembering that, early in the 20th century, in the wake of new ideas about logic and empiricism, there was great distrust of notions such as 'meaning' and 'truth';[5] the idea was to replace such unclear concepts with a fully alternative account in terms of logical laws, formal languages and empirical results. This was reinforced by then-recent developments in math, which promoted modern axiomatics and the free interpretation of symbols (Hilbert's 'point' can be a number, or a beer mug). After all, the phenomenon of meaning was regarded as a 'subjective' one, which implied that it should be eliminated from a scientific, objective way of thinking. In direct contraposition to this modernist ideal, I claim that the phenomenon of meaning is essential to mathematics.

To raise the question, let me employ a quotation from Hermann Grassmann: "Mathematics in its most rigorous form, in its inexorable consistency, is in a position to preserve the students from the fashionable rule of ingenious phrases, and to turn them to the practice of rigorous logical thinking. This aim would not be attained, however, if one wanted to present just formula after formula, without conceptual development. One must rather have both — formal development and conceptual development, going hand in hand."[6] Grassmann might be reflecting here not only his philosophical ideas (reminiscent of Leibniz at this point), but perhaps

[4] Also linked with the question of depth, discussed in *Philosophia Mathematica*, Vol. 23:2, June 2015.

[5] See Gödel's reminiscences; a relevant work is Carnap's *Logical Syntax of Language*, 1934.

[6] The quote is from H. G. Grassmann's *Lehrbuch der Arithmetik*, Berlin, Verlag von T.C.F. Enslin, 1861, vi.

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also his practical experience as a secondary-school teacher; many disciples fail to see any relevance or sense in mathematical formulas, as a result of their inability to join the formal manipulations with conceptual counterparts. This is often the source of failures in the formal manipulations themselves.

We make sense of mathematical symbols by associating meanings to them, but we are also able to disassociate senses from symbols, and find new counterparts, new senses. Notice however that reinterpretation only comes in after a long process of development, whether you think of it in historical terms or ontogenetically, as a matter of individual development.(7) One should not try to go too fast, presenting things "which have often cost several thousand years' labor ... as self-evident"; one should employ historical studies to promote "enlightenment" (Mach 1872, 1-2).

Meaning in math is to a large extent given by *use*, which explains why an epistemology of math will probably have to consider *practices*. But meaning is not simply identical with use, otherwise it would be impossible to reinterpret (and also to enrich the sense of some symbols). To describe the situation in just a few words, one might say that the symbolic plane incorporates enough freedom that meaning is intimately linked with use, and at the same time it can embed the uses into a richer context. This is connected with the fact that mathematical systems are not simply an abstraction from physical (or mental) reality, but in effect incorporate hypothetical assumptions that create or constitute richer structures (that may well be that the case with structures endowed with continuity, see below). The bottom-up movement of reflections on concrete action and practices, is joined with the top-down ingredient of hypothetical postulates. It is thus that mathematicians can consider 'hypothetical states of affairs' and study what necessary conclusions follow from the assumptions (axioms) characterizing such structures.(8)

Many philosophers have recurred to model theory (formal semantics) in their attempts to get a solid grip on the slippery issue of meaning. Let me confess frankly that I don't trust in mathematical solutions to this, I don't look for the answer in formal semantics;

(7) It's also a familiar phenomenon that people often feel uncomfortable if something other than the customary symbol is employed in formulas (e.g., Dedekind's $\sqsubset$ instead of $\subseteq$ for inclusion, or Peano's $\supset$ instead of $\rightarrow$ for the conditional).

(8) This is C.S. Peirce's apt way of characterizing his (Riemannian) understanding of the conceptual work of mathematics. See Carter 2014.

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it should not be found by developing *more* mathematics, but by analyzing what *underlies* math. I believe that key insights have to be obtained at a basic level, studying basic cognition, language and semiotics, and various kinds of practices, among which are mathematical practices — but also 'technical' practices and scientific practices.

I have proposed a thesis of the *complementarity* of formula and meaning in the practice of mathematics, none of the two poles being reducible to the other (see Ferreirós 2016, chap. 4). Admittedly, all of this is rather obscure, and my own work so far is only exploratory.

§ 4. — A hotchpotch of further remarks.

Of course, there have been other topics that guided some of my research. Initially e.g. the question of logic and its relations to math was quite relevant to me,[9] and in connection with it the issue of how properly to understand second-order logic. Also connected with this line of work is the basic notion of arbitrary infinite sets (and functions), which I have tried to clarify and bring to the fore. Modern set theory involves not only the study of infinite sets, but the emphatic consideration of *arbitrary* infinite sets (it rejects any constructivist or predicativist restriction on sets), which is essential to the usual understanding of power sets. That's often called the *quasi-combinatorial* viewpoint, and I argue that such ideas and assumptions properly belong to mathematics, not to logic.[10]

Some of the questions I have tried to study are quite inevitable for a philosopher who takes math seriously, like the conundrum of continuity: as Riemann called it, the "antinomy of the discrete and the continuous." This includes the question whether natural phenomena and their motions can be understood without continuity assumptions.

I should also acknowledge that, ever since I started my Ph.D. guided by Javier Ordóñez (Madrid), the conviction that history of science is indispensable for the philosophy of science grew stronger

[9] Logicism and related matters, including what I recently called the "dark heritage" of logicism (see *Metatheoria* Vol. 10, 2020, Special Issue - Foundations of Mathematics, pp. 19-30).

[10] Nevertheless, many logicians, being used to the idea by long years of training (as a result of widespread promotion by many relevant names), want to include them under second-order logic, or even plural logic.

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and stronger. Ordóñez convinced me to work on the history of math, rather than on a more purely philosophical or foundational topic, and the effect upon my way of thinking and reflecting was noteworthy. I am convinced that the HPS approach is indispensable for attempts to deeply reflect on mathematics (see the introduction to Ferreirós & Gray 2006), and we should also bring mathematical issues to the debates about history of science and philosophy of science.

Let me mention, too, that over the years a number of topics in the connections between math and culture have occupied me quite a lot: for instance the Enlightenment, neohumanism in the German-speaking area during the nineteenth century, or the influence of philosophers on some key figures in the history of math.

Thinking more about questions that guide other researchers, and topics for the future, one may expect that the question of reorienting math's role in human practical life and technology may become prominent. Along the twentieth century, the spirit of mathematical research has been increasingly affected by technological orientations and economic life. As everyone knows, our culture measures everything by its economic impact — sadly, I add. The high spirit of pure math that was so prominent in the first part of the twentieth century, is different from the reigning spirit in the 21st century. Could math help advance towards global cooling and adaptation to climate change? Could there develop a movement of mathematics for peace and equality? In principle yes, but one remains skeptical thinking that the main influences come from practical life, from the economy and 'the markets'.

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§ 5. — Back to mathematics and philosophy.

Mathematics does not bring to philosophy a special method that may solve its problems — I believe Kant was right in that. And even less does philosophy to math; it may bring some enlightenment, helping dispel some fogs, but not tools to help in concrete investigations. Nevertheless mathematics does have its philosophical conundrums, and so a conscientious mathematician will end up pondering philosophical questions. An example: what is the best balance between constructive methods and abstract-structural ones? how does it depend on the problem under study?

Is the current default foundation the most adequate one? In particular, does full-blown set theory create pseudoproblems that one would rather avoid? This writer, for one, believes that the Continuum Problem is not a well-defined mathematical question,[11] i.e., it's asking for full precision and control at a level where that cannot to be had.[12] Perhaps it would be wiser to regard the 'class' of real numbers as a virtual totality, not a fully determinate set. Perhaps one should devote one's efforts to elaborating mathematical tools to deal with systems that may or may not be continuous, instead of assuming that the continuum is fully understood and trying to build a most complex superstructure on top of it. Perhaps this is, after all, something that many scientists-mathematicians have been trying to do in the past and today.[13]

As the reader may intuit from those remarks, my position concerning mathematics and its foundations is pluralistic. If math is mostly conceptual work, as I believe it is; if it's partially dependent on hypothetical assumptions; then one should not aim at having a single all-encompassing framework. Let's consider instead a plurality of frameworks or theoretical systems, and let's employ them as best we can in the (piecemeal) exploration of reality that we call science.

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[11] Even accepting Gödel's argument that proofs of independence from ZFC do not have to constitute a solution of the question. See Feferman's 'The Continuum Hypothesis is neither a definite mathematical problem nor a definite logical problem' (still unpublished), a revised version of his 2011 EFI lecture at Harvard.

[12] In the question whether $2^{\aleph_0}$ is or is not $\aleph_1$, we are asking whether one partially defined domain is bijectable with another partially defined domain (the powerset operation is eminently "amorphous", as prominent set theorist R. Jensen said). Yes or no? — Indefinite: we should allow truth-value gaps.

[13] See e.g. the recent lecture by M. Atiyah at the 5th Heidelberg Laureate Forum, 2017, 'The discrete and the continuous from James Clerk Maxwell to Alan Turing'; or consider the ideas of R. Penrose.

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José Ferreirós, Departamento de Filosofía y Lógica
Facultad de Filosofía, Universidad de Sevilla.

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What Makes Mathematicians Believe Unproved Mathematical Statements?

Timothy Gowers[1]

Abstract. This paper considers the reasons mathematicians give for making probabilistic judgments about unproved mathematical statements, and discusses how one might interpret and justify such judgments more formally. Following Pólya, I argue that we update our probabilistic judgments in a broadly Bayesian way, while to explain what they mean in the first place, I argue that they are referring not so much to the truth of the statements as to the likely existence or otherwise of reasons for them. The link between the two is provided by a "no-miracle" principle, which says that a surprising mathematical statement will not be true unless it is true for a reason. This principle applies only to statements that are sufficiently natural, so the paper also sets out criteria for a statement to be more or less natural.

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§ 1. — Introduction.

A feature of mathematics that marks it out from all other disciplines is that it has a formal notion of proof, which makes it possible, at least in principle, to justify a mathematical statement not just beyond all reasonable doubt, but beyond any doubt at all.[2] As many people have commented, justifications as they appear in typical research articles are not formal proofs but more like blueprints that are sufficiently detailed to convince experts that formal proofs exist. Some of these blueprints are very complicated and use ideas

[1] This article is a written version of two seminars I gave at the Collège de France.

[2] Unless one wishes to take the drastic step of doubting either some very basic axioms or some very simple rules of deduction.

that are understood by only very few mathematicians, so it can be reasonable to be uncertain about whether they are in fact correct. Nevertheless, there is a large body of mathematical knowledge that is sufficiently simple and widely understood that we can be sure that it will not be overthrown in the way that, say, Newton's theory of gravitation was overthrown by Einstein's. There is also a smaller but rapidly growing body of knowledge that *has* now been completely formalized with the help of computers.

Because proofs have become the gold standard, at least for pure mathematicians, in the sense that a statement is "accepted" if and only if somebody has proved it, it is tempting to divide mathematical statements into three classes: definitely true, definitely false, and don't know. (One might also wish to divide the last class further, into statements with proofs or disproofs that have not yet been discovered, and statements that are undecidable.) However, this is a very crude classification that does not give a complete picture of mathematical practice, since mathematicians have varying degrees of belief in different unproved statements. For example, they are extremely confident that Goldbach's conjecture is true and that π is a normal number, they are quietly confident but with not quite 100% certainty that the Riemann hypothesis is true, they think that almost certainly P$\neq$NP (though a few outliers think that this confidence is misplaced), and think it is probably not possible to factorize a large integer in subexponential time but would not be unduly surprised if it turned out to be possible. (I shall discuss all these examples in more detail later.)

It is a simple empirical fact that mathematicians make probabilistic judgments about mathematical statements. They do not actually assign precise probabilities to them, but they use words and phrases like "probably", "almost certainly", "unlikely", and so on, and if pressed might even be ready to translate those into odds that they would be prepared to accept in a bet. And yet this fact raises many questions. A particularly basic one is what it even means to say something like that a statement is "probably true". A second is a descriptive one: what causes mathematicians to be more confident in the truth of some unproved statements than others? A third is whether mathematicians are behaving rationally when they make the judgments they make. And a fourth is *why* mathematicians make these judgments.

My focus in this article will be the second question, though it is related to all the others. But before I get on to it, I would briefly

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like to tackle the fourth, since I believe that it has an easy answer, though one that is also easy to overlook.

Contrary to what one might at first think, assessing the likelihood of as yet unproved statements is not just something that mathematicians do for amusement when they are talking shop with colleagues. Rather, it is a fundamental part of the research process. Solving a mathematics problem is extremely difficult and requires a big investment of time. In order to use one's time wisely, one has to make judgments about which statements are likely to be true, which are likely to have easy proofs and which not, which are likely to have interesting consequences, and so on.

That is not to say that trying to prove a false statement is always a complete waste of time. For some problems there is virtually no difference between trying to find a proof and trying to find a counterexample, since a good way to find a proof of a true statement is to try to disprove it and to examine why one's attempts fail. But for some problems that approach gets you nowhere. Even then it may be useful to try to prove a false statement. For example, early in my career I spent a long time trying to prove a theorem about Banach spaces that would have solved a famous open problem. I failed in my attempt, and later a highly ingenious counterexample was discovered. However, my time was not at all wasted: some of the ideas I had were extremely useful to me for solving other problems.

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Nevertheless, the point remains that judging the likely truth of a statement is a very important skill for a mathematician. So far I have discussed entire research problems, but probabilistic judgments become even more important when they are part of the *process of solving* such problems. Typically, in order to solve a problem, one must use a top-down approach, which will involve a lot of guesswork. For instance, if one is trying to prove that $P \implies Q$, one often tries to identify a statement R for which one can prove that $P \implies R$ and that $R \implies Q$. There may be several candidates, in which case one must decide which one to try first, and having made the decision one must continually think about whether and at what point to abandon that line of attack and try another. In general, to be successful at research one must search efficiently for proofs, and that requires a sophisticated search strategy in which probabilistic judgments, whatever they might be, play an essential role. For this reason, understanding how such judgments are made and to what extent they are justified is not merely of philosophical interest: it is also of practical interest to mathematicians.

The rest of the paper is organized as follows. I begin with an informal discussion of several major open problems and some of the reasons mathematicians have given for what they believe the answers to be. I follow this with a discussion of how mathematicians update their beliefs in the light of changes to what they know: some basic rules for this were put forward by Pólya, which, as David Corfield has pointed out, it is natural to regard today as a form of Bayesian reasoning. In the following section I try to explain what probabilistic judgments about mathematical statements could possibly mean given that their truth values (in so far as they have truth values) are fixed. For this purpose I formulate two closely related principles that I call the "no-coincidence principle" and the "no-miracle principle". This is followed by a discussion of naturalness of mathematical statements, which I argue is central to any understanding of how we make probabilistic judgments. Issues arise here that are very similar to those raised by Goodman in his famous "new riddle of induction". In order to deal with Goodman-type paradoxes I formulate another basic principle that I call the "nice-formula principle". In the next section I argue that naturalness in mathematics is closely related with levels of abstraction — roughly speaking, the more it is possible to formulate a statement or definition without resorting to constants (as opposed to parameters that vary according to context), the more natural it is. I finish with a brief summary of the argument put forward over the course of the preceding sections.

§ 2. — Some examples of unproved statements.

In this section I shall look at several major unsolved mathematical problems. For some of them there are answers that are widely believed to be true, while for others there is much greater uncertainty. I shall discuss the kinds of reasons that mathematicians give, or in some cases actually have given, for their assessments of these problems. In later sections I shall try to draw some general conclusions from these and other examples.

2.1. Goldbach's conjecture. Goldbach's conjecture is the assertion that every even number greater than 4 is the sum of two odd primes. For example, 1984=1979+5, and both 1979 and 5 are prime numbers. Goldbach's conjecture is one of the most famous open

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problems in number theory, but most mathematicians, at least if they have thought about the problem for a bit, are extremely confident that it is a true statement. What explains this confidence?

This question was considered in detail in an article by Alan Baker[3] on the use of scientific induction in mathematics. Goldbach's conjecture has been verified for all even numbers up to 4,000,000,000,000,000,000. Baker asked to what extent this kind of verification increased mathematicians' confidence in a statement, and to what extent it *should* increase their confidence, and concluded that in both cases the answer was not much.

One of the reasons he gave for this conclusion was that for any fixed n, the integers smaller than n are "minuscule", in the sense that almost all integers are far bigger. To a mathematician, this objection has force only if one has reason to suspect that the minuscule (in this sense) integers are not representative, and that varies from problem to problem. For instance, if I were to learn that the maximum possible value of a simple parameter associated with n-vertex graphs was $n-1$ for all n up to 10, then I might well regard that as very convincing evidence that it was always $n-1$, reasoning that any behaviour that would lead to a more complicated function would almost certainly have shown up by then. But there are examples in number theory of phenomena that show up only for very large numbers, so in general I would want to check further for a problem like Goldbach's conjecture.

Probably the reaction of most mathematicians, if all they knew about Goldbach's conjecture was that it was true up to 4,000,000,000,000,000,000, would be to think that it is probably true, but that it might be one of those funny problems, which are quite rare but which definitely exist, where the smallest counterexample is very large. However, as Baker pointed out, we actually know a lot more than this. Confidence in the truth of Goldbach's conjecture was greatly increased when tables were produced that showed not just *that* every even number up to a certain point was a sum of two odd primes, but also *in how many ways* that was the case. This revealed that, despite some fluctuation, on average the number of ways steadily increased as the even number increased. Therefore, small even numbers, to the extent that they are unrepresentative, are unrepresentative in the right direction: the evidence suggests that if you take a very large even number, then not only will it be

[3] *Is there a problem of induction for Mathematics?* in M. Potter (ed.), Mathematical Knowledge, Oxford University Press, pp. 57-71 (2007)

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expressible as the sum of two primes, but it will be expressible in a huge number of ways.

However, Baker did not give the whole story, as there is an even stronger reason to be impressed by these tables, one that is so compelling as to leave virtually no room for doubt in the truth of Goldbach's conjecture.

A striking fact about the primes is that they occur somewhat sporadically, behaving in many ways as though they have been chosen randomly, subject to certain constraints. A sign of this is that all attempts to produce a non-artificial formula for the nth prime have failed, to the point where no mathematician seriously believes that such a formula exists (which is of course another example of a probabilistic judgment).

What does "as though they were chosen randomly" mean? One of the most famous theorems in mathematics, the prime number theorem proved independently by Hadamard and de la Vallée Poussin, states that the number of primes up to n is approximately $n/\log n$, so to a first approximation one might say that the primes look like a set that you would obtain if for each n you were to choose it with probability $1/\log n$, making all choices independently. However, such a set would contain a roughly equal number of even and odd numbers, which is certainly not the case for the primes. A more accurate random model of the primes is to choose each n as follows. If it has a small divisor such as 5 (the limit of what counts as small grows slowly with n), then do not choose n. Otherwise, choose n with a probability somewhat larger than $1/\log n$ (which can be calculated precisely) to compensate for the numbers that have been discarded.

This model can be used to predict, for a given large even number n, the approximate number of ways of writing n as a sum of two primes. For instance, if n is a multiple of 3 and p is a prime not equal to 3, then p is not a multiple of 3, which guarantees that $n-p$ is also not a multiple of 3, so p's being prime is positively correlated with $(n-p)$'s being prime. By contrast, if n is of the form $3m+1$, then the only way for p and $n-p$ both to avoid being multiples of 3 is if p is of the form $3m+2$, which is true for only about half of all primes. With this kind of reasoning one ends up hypothesizing that on average multiples of 3 can be written as a sum of two primes in about twice as many ways as non-multiples of 3. More generally, one can write down an approximate formula for the number of ways that n should be representable as a sum of two primes,

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in terms of which small factors it has. And this formula turns out to be remarkably accurate. The tables show not just that large even numbers can be written in *many* ways as a sum of two primes, but that they can be written *in almost exactly the number of ways one would expect* if the random model of primes is a good one.

Why should this increase our confidence? The answer is twofold. First, the experimental evidence is now confirming a significantly more general statement (namely that the random model of the primes makes accurate predictions). The more general a statement is, the more diverse opportunities it has to be false, and therefore the more impressed one is when it is confirmed. Secondly, the predictions made by this more general statement are much more precise, and therefore again it is correspondingly easier for the predictions to fail, and therefore more impressive when they turn out to be correct.

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2.2. The normality of π. A positive real number x is said to be *normal* if its decimal expansion looks random in the sense that every sequence of digits occurs with the frequency one would expect if the digits had been chosen at random. More precisely, suppose that the decimal expansion of x is $x_0.x_1x_2x_3x_4\dots$, where x_0 is a non-negative integer and each of $x_1, x_2, \dots$ is an integer between 0 and 9. Then given any sequence of digits, such as 137, and any positive integer n, one can look at all the sequences $x_ix_{i+1}x_{i+2}$ with $1 \leqslant i \leqslant n$ and count how many of them are the sequence 137. If the x_i have been chosen randomly, then each triple $x_ix_{i+1}x_{i+2}$ has a $1/1000$ chance of being 137, so one would expect the proportion of triples that give 137 to converge to $1/1000$ as n tends to infinity. The number x is said to be normal if that happens for every small sequence: that is, each digit occurs with a frequency that converges to $1/10$, each pair of digits with a frequency that converges to $1/100$, and so on.

It is widely believed that π is a normal number, which would have amusing consequences such as that your date of birth must occur somewhere in its decimal expansion. However, proving the normality of π is a wide open problem — it may even be an unprovable statement. To give an idea of our level of ignorance, it is not even known that there is not some point beyond which all digits are equal to 0 or 1, which would be an extremely strong refutation of the conjecture that π is normal.

So why do we believe that π is normal? Certainly the experimental evidence is quite convincing: if one examines a table of the first thousand digits of π, say, they really do look pretty random, and statistical tests have been carried out much further than this without throwing up any anomalies. Furthermore, there is no particular reason to think in this context that the "minuscule" number of digits we can examine form an unrepresentative sample.

If pressed further, I myself would use the following two-step argument.

1. If π is not normal, then there will have to be some *reason* for any bias that might occur.
2. The kinds of reasons I can imagine are almost all ones that would lead to a bias showing up very clearly within the first million digits.

This argument falls far short of a proof, of course. When I said that there would have to be a reason for any bias, I did not rigorously rule out the possibility that π might "just happen" to have only finitely many 7s in its decimal expansion. But all my mathematical experience tells me that if such a remarkable phenomenon were to occur, there would in fact be an explanation for it.

The second part of the argument is slightly less convincing, since there are some notorious examples of phenomena that show up only for surprisingly large integers, despite having very natural explanations. One is the remarkable fact that

$$e^{\pi\sqrt{163}} = 262537412640768743.99999999999925\ldots.$$

It would be very reasonable to suppose, after calculating the expansion of $e^{\pi\sqrt{163}}$ up to twelve decimal places and obtaining the answer 262537412640768743.999999999999, that in fact it is equal to the integer 262537412640768744, and the justification would be very similar: it is hard to imagine a reason for a pattern like this that continues for twelve decimal places without continuing for ever.

However, there is such a reason, and in fact it is not all that strange, though it depends on some advanced number theory. It turns out that one can expand $e^{\pi\sqrt{163}}$ in a natural way as a series where the first two terms are $(640320)^3$ and 744, and all remaining terms are extremely small, and that is why one obtains a number that is very close to an integer without actually being an integer.

There are many examples throughout mathematics of simply defined numbers that turn out to be very large (and there are even

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theoretical reasons, related to Gödel's theorem, to expect this to be the case), so the mere fact that a pattern has been observed to continue for a long time is not always overwhelming evidence in favour of the hypothesis that it continues for ever. However, there is a difference between the example of $e^{\pi\sqrt{163}}$ and the hypothetical example of a failure of π to be normal. In the first case, there is a pattern that lasts for a long time before coming to an end. But for π to fail to be normal, one would require the opposite phenomenon: a pattern that only *starts* after a long time. The decimal expansion of π has been calculated up to 62.8 trillion digits without showing any sign of a pattern, and it is very hard to imagine what a proof could conceivably look like that would establish that some bias crept in after that point.[(4)]

2.3. $R(k,k)^{1/k}$ **tends to a limit.** A central theorem in combinatorics, Ramsey's theorem, asserts that for every k there exists an n such that if all the edges of a complete graph with n vertices are coloured either red or blue, then there must be k vertices entirely linked by red edges or k vertices entirely linked by blue vertices. The smallest such n is denoted $R(k,k)$. It is known that $2^{k/2} \leqslant R(k,k) \leqslant 4^k$. These upper bounds are very far apart, but it is a major open problem to improve substantially on either of them.

It is tempting to assume that there must be some constant C, lying between $\sqrt{2}$ and 2, such that $R(k,k)$ is roughly equal to C^k for all k. Plausible candidates for C are $\sqrt{2}, 2$, and 4. To be more precise about this assumption, what seems very likely to be true is that there is some C such that for any $\alpha < C$ and $\beta > C$, $R(k,k)$ lies between α^k and β^k when k is sufficiently large. A more concise way of saying this is to say that the quantity $R(k,k)^{1/k}$ converges to a limit C as k tends to infinity.

Almost all combinatorialists are (if my feelings are anything to go by) confident that this is true. The alternative is that as k gets larger and larger, the Ramsey number $R(k,k)$ "oscillates", in the sense that there are two constants $A < B$ such that infinitely often $R(k,k) \leqslant A^k$ and infinitely often $R(k,k) \geqslant B^k$.

[(4)] Strangely enough, there might be more hope of proving that the base-16 expansion of π is not normal, owing to a remarkable formula of Bailey, Borwein and Plouffe that enables that expansion to be calculated extremely rapidly. However, this possibility has been investigated and the best one can say at the moment is that their formula reduces the question of the normality of π to a simpler-looking but still wide open question, the probable answer to which would imply that π is normal even in base 16.

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Why does this alternative possibility seem so unlikely? One reason is that there are many examples of exponential growth rates that occur naturally and are known not to oscillate in this sense, and none (or none that I can think of) that do oscillate. However, this is not a completely convincing argument, because the examples in question, of functions f such that $f(k)^{1/k}$ converges, are often examples for the same reason. A function is called *submultiplicative* if $f(ab) \leqslant f(a)f(b)$ for every a and b, and *supermultiplicative* if $f(ab) \geqslant f(a)f(b)$ for every a and b. It is not too hard to prove that if a function f is either submultiplicative or supermultiplicative, then $f(k)^{1/k}$ converges to a limit, and this is the reason that many functions that occur naturally have this property. However, the function $f(k) = R(k,k)$ is not submultiplicative, and it is not obviously supermultiplicative either (though so few Ramsey numbers are known that one cannot rule out the possibility), so if $R(k,k)^{1/k}$ converges it will probably have to be for a different reason.

So why, despite there being no particular reason to suppose that $R(k,k)$ is submultiplicative or supermultiplicative, are mathematicians so confident that $R(k,k)^{1/k}$ converges? This seems to be another case where, *pace* Alan Baker, there is no reason to suppose that minuscule natural numbers are unrepresentative, so if $R(k,k)$ is about C^k for some very large (by human standards) k, then probably $R(k,k)$ is about C^k for all larger k as well.

This argument raises questions in its turn, since one could replace C^k by another function $D(k)$ defined by a formula such as $(2+\cos(\pi\log_2 k)/10)^k$, which I have designed so that $D(k) = (2.1)^k$ when k is equal to an even power of 2 and $D(k) = (1.9)^k$ when k is equal to an odd power of 2. If it could be established that $R(k,k)$ was approximately equal to $D(k)$ for some very large k, would that suggest that $R(k,k)$ was approximately $D(k)$ for all larger k? Clearly not, but why not?

The answer must be that in some way the hypothesis that $R(k,k) \approx D(k)$ is an artificial one, which leads to the question of how to distinguish between natural and artificial hypotheses. This is a mathematical version of Goodman's new riddle of induction, to which I shall return later.

2.4. The average end-to-end distance of a two-dimensional self-avoiding walk. A two-dimensional self-avoiding walk of length n is a path of length n in the two-dimensional integer grid $\mathbb{Z}^2$ that starts at $(0,0)$, moves at each step to one of the four neighbouring points,

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where the neighbours of (x, y) are $(x+1, y), (x-1, y), (x, y+1)$ and $(x, y-1)$, and do not visit any point more than once. Despite this simple definition, self-avoiding walks are very hard to analyse, and many basic questions about them are still unanswered.

One of these is to determine the average end-to-end distance of an n-step self-avoiding walk chosen at random from all such walks — that is, to work out how far, on average, the last point of the walk is from $(0, 0)$. It is known to be at least $n^{1/4}$ and a slight improvement to the trivial upper bound of n is also known. But that leaves a very large gap.

However, mathematicians are extremely confident that the average is in fact around $n^{3/4}$, and have been so for a long time. Why is this?

The reason is that there are arguments in favour of this conclusion that fall short of being rigorous proofs but that are nevertheless convincing. Pure mathematicians might call them heuristic arguments, but for many physicists they are completely satisfactory. They belong to a class of arguments where certain mathematical operations are carried out that are "invalid" (one famous example being to establish a formula that depends on dimension, which has to be an integer, and then to let the dimension "converge to zero"), despite which, they give precise predictions that are then confirmed by experimental evidence. This evidence can then be regarded as confirming not just the particular estimate of the average end-to-end distance of a self-avoiding walk, but also the more general hypothesis that there is some explanation, yet to be uncovered, for why physicists' methods seem to work so well.

Over the years, this confidence, which was already high, has become even higher after remarkable work of pure mathematicians that proves the $n^{3/4}$ estimate, as well as many related estimates that had been predicted by physicists, subject to a general hypothesis known as conformal invariance — a plausible hypothesis that asserts that the macroscopic behaviour of a self-avoiding walk (and several other related models) has a certain kind of symmetry. The conformal invariance hypothesis plays a role here that is somewhat similar to the role played for Goldbach's conjecture that the random model of the primes makes accurate predictions, with two notable differences: the conformal invariance hypothesis is a more precise statement, and the fact that one can deduce from it precise information about the behaviour of self-avoiding walks is far from obvious.

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2.5. P versus NP. The P versus NP problem was formulated in 1971 by Stephen Cook, and independently in 1973 by Leonid Levin, and rapidly gained the status of being one of the most fundamental and important unsolved problems in mathematics and theoretical computer science. The basic object of study in theoretical computer science is the notion of an algorithm, and a basic question is whether, for a given computational task, there is an algorithm that can perform the task efficiently. A useful definition of "efficiently" is that of a polynomial-time algorithm, which means an algorithm for which there exists a polynomial P such that if the input to the algorithm has size n, then the time taken by the algorithm is at most $P(n)$. For example, standard long multiplication of two n-digit numbers is efficient in this sense, as you have to do roughly n^2 simple arithmetical operations. (There are also cleverer methods that are significantly more efficient, the current record being an algorithm that takes time closer to $n \log n$.)

To prove that there is an efficient algorithm for a given task, all you have to do is find such an algorithm. What appears to be far harder is to prove that there is *not* an efficient algorithm for some task. This general problem was brought into sharp focus by Cook and Levin, who observed that there is a large class of important computational problems that are all of equivalent difficulty in the following sense: if you can find a polynomial-time algorithm for one of them, then you can use it to create a polynomial-time algorithm for any other one.

This class of problems is closely related to a class of problems called NP (which stands for "non-deterministic polynomial time"). Loosely speaking, a problem belongs to NP if there is a polynomial-time algorithm for checking whether a proposed answer is correct. For example, an important problem in cryptography is the following: you are given a product n of two large prime numbers p and q and your task is to determine what p and q are. Nobody knows of an efficient way to do this, but if you are given two numbers p and q, then you can use long multiplication to check efficiently whether pq really does equal n.

The big unsolved problem is whether P equals NP. That is, if a computational task has the property that checking whether an answer is correct can be done efficiently, does that mean that finding the answer can also be done efficiently? The factorization example suggests that the answer is probably no: there just seems to be no reason for finding an answer to be anything like as easy as checking that an answer is correct once found.

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Cook and Levin observed the remarkable fact that many natural problems in NP are what we call *NP-complete*, which means that if you can solve one of these problems then you can solve all problems in NP. One of the most famous such problems is the travelling salesman problem: you are given a collection of towns, and roads linking those towns, and the problem is to determine whether there is a route that visits each town exactly once before returning to its starting point. This belongs to NP, since if somebody shows you a route, you can easily check whether it visits each town exactly once. Much less obviously, it is an NP-complete problem, which means that if there is an efficient method for determining whether such a route exists, then that method can be converted into an efficient method for any other problem in NP. For example, it could be converted into a method for factorizing products of two large prime numbers. (By contrast, the factorization problem itself, though in NP, is almost certainly not NP-complete.)

The fact that there are many NP-complete problems and that finding an efficient algorithm for just one of them would prove that P=NP might seem to tip the balance back in favour of P and NP being equal after all. But that is not how mathematicians see it. A typical view, which I share, is that P and NP are almost certainly not equal, but that the level of certainty is not quite as high as it is for the other problems I have discussed so far. Rather than formulating my own reasons for this view, I shall leave that task to the well-known and philosophically inclined theoretical computer scientist Scott Aaronson. In an article entitled "$P \stackrel{?}{=} NP$" he writes the following.

> To my mind, however, the strongest argument for P≠NP involves the thousands of problems that have been shown to be NP-complete, and the thousands of other problems that have been shown to be in P. If just one of these problems had turned out to be both NP-complete and in P, that would've immediately implied P = NP. Thus, we could argue, the P≠NP hypothesis has had thousands of chances to be "falsified by observation." Yet somehow, in every case, the NP-completeness reductions and the polynomial-time algorithms "miraculously" avoid meeting each other — a phenomenon that I once described as the "invisible fence".

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He then goes on to mention a particularly striking example of this invisible fence in action, related to a central example of an NP-complete problem, called 3-SAT. An instance of 3-SAT is a collection of variables $x_1, \dots, x_n$, each of which can take the value True or False, and also a collection of "clauses", each of which is the OR of three variables or their negations. For example, a possible clause is $x_2 \vee \neg x_5 \vee x_8$, which is true if and only if either x_2 is true or x_5 is false or x_8 is true. The problem is to determine whether there is a choice of values for the variables that makes all the clauses in the given collection true. This problem is a central example, because many proofs of NP-completeness proceed by showing that a problem can be reduced to 3-SAT, and since 3-SAT is already known to be NP-complete, that shows that the given problem is also NP-complete.

In the absence of an efficient algorithm for solving 3-SAT, one can set one's sights a little lower and try to find an efficient algorithm that will find an assignment of values to the variables that makes as many of the clauses true as possible. And an algorithm is known that will find values that make approximately 7/8 of the clauses true.[(5)]

A deep result of Johan Haståd shows that unless P=NP, one cannot do better than this. In other words, for any fraction α greater than 7/8, if there were an efficient algorithm for finding an assignment that satisfies at least a proportion α of the clauses, then P would equal NP. In other words again, the problem of finding a satisfying assignment for a proportion α of the clauses is NP-complete: the invisible fence appears at exactly 7/8.

2.6. The Riemann hypothesis. The Riemann hypothesis is regarded by many as the single most important unsolved problem in mathematics (though experts in number theory will often qualify this by pointing out that for many applications the Riemann hypothesis on its own is insufficient and what is needed is a more general form of it). Like the P versus NP problem, it is one of the Clay Millennium Problems, for which a million dollars is offered for a solution. It states that the zeros of the Riemann zeta function, which is defined for complex numbers with real part greater than 1 by the formula $\zeta(s) = \sum_{n=1}^{\infty} n^{-s}$ and on the rest of the complex plane apart from 1 by analytic continuation, are all either "trivial zeros" that occur at negative even integers or have real part equal to 1/2.

(5) It is even possible to say very roughly how it works. First note that if you choose the values at random, then each clause has a 7/8 probability of being true. But there is also a known derandomization procedure that can convert this simple observation into a deterministic algorithm.

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The general attitude amongst experts towards the Riemann hypothesis is that there are certainly arguments that support the belief that the Riemann hypothesis is true, but there are also arguments against it, with the result that opinions vary, and while most mathematicians (or at least this is my impression) think it is probably true, they certainly do not rule out the possibility of its being false.

One argument in favour of it is that it has been checked with the help of computers that the imaginary part of any counterexample would have to be larger than a very large number that has steadily increased over the years and currently stands at about 3×10^{12}.

A second argument is that there is something very natural about the statement. A sign of this is that it has many equivalent formulations that look very different. One of these is related to the heuristic principle mentioned earlier in connection with Goldbach's conjecture, namely that the primes are distributed as randomly as they can be given certain obvious constraints. As mentioned earlier, the prime number theorem asserts that the density of primes close to n is approximately $1/\log n$. The Riemann hypothesis is (as Riemann himself showed) equivalent to the statement that the error term in this approximation is not much bigger than $\log n/\sqrt{n}$, which is the sort of error term one would expect if one were choosing the primes according to the random model mentioned before.

In an introduction to the Riemann hypothesis for the Clay Millennium Prizes, the famous analytic number theorist Enrico Bombieri, wrote a section entitled "Evidence for the Riemann hypothesis". He mentioned the computational evidence (which at the time was less advanced than it is today), but also a subtler argument, which is that there are reasons to believe that a counterexample, if it exists, should be in the vicinity of a number $1/2 + it$ for which $|\zeta(1/2 + it)|$ is particularly large. Such peaks are known to exist, but are also very infrequent, which raises the possibility that there is very large counterexample but no smaller one. The fact that this possibility is at least imaginable would seem to make the computational evidence mentioned above less persuasive. However, Bombieri went on to mention that Andrew Odlyzko, one of the leading experts on computation of zeros of the zeta function, had looked at zeros in regions where counterexamples would be more likely to be found (for instance, he had found 175 million consecutive zeros with imaginary parts around 10^{20}) and not found any. Thus, the Riemann hypothesis had in fact passed more stringent tests.

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Other arguments given by Bombieri are that there are results showing that zeros with real parts that are not close to 1/2 are rare, and also results showing that at least 40% of the zeros have real part equal to 1/2. At first sight, this may not seem like very convincing evidence: why could it not be the case that a certain proportion of zeros have real part 1/2 but not all of them? The answer to this is quite interesting. The zeta function arises very naturally, as I have already stressed, so one would expect that the set of zeros of the zeta function would also look natural. But in that case one would expect either that they would all be on the critical line (that is, the vertical line of complex numbers of real part 1/2) or that they would have real parts that were scattered around 1/2 in a somewhat random-like way. It is much harder to conceive of a natural set that would be largely, but not completely, confined to the critical line.[6]

A final and important reason is that the Riemann hypothesis is now just one of a large number of analogous statements about different kinds of "zeta function", some of which are not open problems but major theorems. This raises the possibility of proving the Riemann hypothesis by considering classes of functions and not just one isolated function — a technique that has been very fruitful in much of mathematics. (There are also some analogues that turn out to be false, but they are not sufficiently closely related to the Riemann zeta function to dash this hope.) It also raises the possibility of using another proof-finding technique, namely analogy construction: one looks at the proofs for the other zeta functions and tries to solve problems of the form "This zeta function is to the Riemann zeta function as this element of the proof is to what?" and in that way one hopes to build up a proof. Bombieri writes, somewhat cautiously,

> In our opinion, these results in the geometric setting cannot be ignored as not relevant to the understanding of the classical Riemann hypothesis; the analogies are too compelling to be dismissed outright.

Another mathematician who is on record as saying that he believes strongly in the truth of the Riemann hypothesis is Peter

[6] I did not fully appreciate this point until, quite by chance and after having written a first draft of this article, I overheard my colleague Aled Walker explaining over lunch to a non-mathematician why he believed the Riemann hypothesis. He gave this reason as the one he found most convincing, so I asked him why, and he responded with something like the argument above.

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Sarnak. In an interview in 2012 he mentions two points that back up his view (though he doesn't say that explicitly). One is that of the huge number of interesting consequences that have been discovered of the Riemann hypothesis, a significant number have subsequently been proved by methods that avoid appealing to it. Thus, rather like the statement that P$\neq$NP, it makes non-obvious predictions, several of which have now been rigorously confirmed and none of which have been disproved. His second reason (also alluded to by Bombieri) is that there is a fascinating connection between the distribution of the zeros of the Riemann zeta function and the distribution of the eigenvalues of a random matrix. The latter is quite well understood, and has led to predictions about the former that would have been impossible to guess, and which have been strongly supported by computational evidence. Thus, as with non-rigorous arguments about self-avoiding walks, one has the feeling that "something is going on" even if it is not yet fully understood.

Andrew Odlyzko's view after his computational work is completely agnostic: he thinks that the Riemann hypothesis could be true and recognises that there is evidence in favour of it, but would not be especially surprised if there turned out to be a large counterexample (which Bombieri seems far less willing to countenance, saying that it "would create havoc in the distribution of prime numbers"). Some clue as to the reason for his attitude can be found in an unpublished paper where he writes,

> The main conclusion that can be drawn from the data in this paper is that in many respects the zeta function reaches its asymptotic behavior slowly, so that even the neighbourhood of the 10^{20}th zero does not represent what happens much higher.

He then goes on to give some idea of why this slow convergence occurs.

2.7. The difficulty of factorizing large integers. Modern cryptography, and in particular internet security, rely heavily on protocols that would be insecure if integer factorization turned out not to be hard. And while it is widely believed that the problem is indeed hard, that belief is nowhere near as strong as our belief that P$\neq$NP. The reason for this is connected with a fact that I mentioned earlier, which is that although the problem belongs to NP — if you give

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me a factorization, I can check easily whether it is correct — it is not thought to be NP-complete. Let me briefly explain the reason for this.

First, one needs to know that search problems, where one is asked to find an object with certain properties, are closely related to corresponding decision problems, where one is merely asked to determine whether the answer to a certain question is yes or no. In our case, the search problem is to find a factorization of a given large integer n. The corresponding decision problem is to determine whether n has a factor less than some given k.

Clearly if one can solve the search problem, one can solve the decision problem. But the reverse is also true: if one has an efficient algorithm for determining whether n has a factor less than any given k, one can play a game of twenty questions, rapidly narrowing down the range in which there must be a factor until one finds it exactly.

As with the search problem, the decision problem is said to belong to NP if the answer can be demonstrated to be correct in polynomial time. That is clearly the case here: if you want to convince me that n has a factor m that is less than k, you just have to exhibit m and do a routine calculation to show me that n really is a multiple of m (as well as demonstrating that $m < k$, which is even easier).

However, something else happens here that is much rarer: if n does *not* have a factor less than k, you can also convince me of that in polynomial time. The basic idea is that you show me a complete prime factorization of n: that is, you tell me that $n = p_1 p_2 \dots p_r$ for some prime numbers $p_1, \dots, p_r$, and point out that all the p_i are at least as big as k. Of course, that does not convince me unless I am sure that the p_i are indeed prime numbers. It is not at all obvious that there is a quick way of convincing me that a given number is prime, but it turns out that such methods do exist. One method is to use a breakthrough result of Manindra Agrawal, Neeraj Kayal, and Nitin Saxena, who showed that one could determine in polynomial time whether a number n is prime. (Here "polynomial time" means that the time taken is bounded above by a polynomial function of the number of digits of n.) It was previously known that determining primality was in NP — the proof depended on the number-theoretic fact that if p is prime, then there will be a number a such that a^{p-1} is congruent to 1 mod p and no smaller power of a is congruent to 1 mod p.

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A decision problem of the form "Does there exist x such that $P(x)$?" is said to belong to co-NP if a negative answer can be checked in polynomial time. So the decision version of the integer factorization problem belongs both to NP and to co-NP, in strong contrast to most natural NP problems and in particular to all known NP-complete problems (at least as far as we can tell — if P=NP then all these distinctions collapse).

Why does this cause us to believe that integer factorization is not NP-complete? Because that would have the extraordinary consequence that NP=co-NP. For example, given an instance of 3-SAT — that is, a collection of clauses of size 3 — we would be able to find a pair of integers n and k such that n has a factor less than k if and only if the collection of clauses can be simultaneously satisfied. Since integer factorization is in co-NP, that would mean that if the collection of clauses *cannot* be simultaneously satisfied, there would be a way of demonstrating that fact, by demonstrating that n does not have a factor less than k. So 3-SAT would also belong to co-NP (as would all other problems in NP by a similar argument).

Thus, problems that belong both to NP and to co-NP are believed to be "easier" than NP-complete problems. This reduces our confidence that integer factorization is hard.

There are three more facts that reduce our confidence still further. One is that although one might think that to factorize a product n of two large primes there is not much option beyond a brute-force search for the smaller of the two primes, which would take time roughly exponential in the number of digits of n, that is not in fact true: there are methods that are far from obvious that use advanced number theory and take a time that is exponential in something closer to the cube root of the number of digits. Though that is still a rapid growth rate, which makes the methods impractical for numbers with more than a few hundred digits, it is still a big improvement on the naive approach, and demonstrates that there can be algorithms that work in clever and unexpected ways. The second fact is that Peter Shor famously showed that a quantum computer *can* factorize large integers in polynomial time. While that is not strong evidence that a classical computer can do the same (in fact, it is normally taken as evidence of the additional power of quantum computers), some of the number-theoretic tricks that are used to prove the result are further demonstrations of the fact that this is a field where there are unexpected ideas to be found. (Another example of this is the astonishing primality

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test mentioned earlier.) The third fact is that another problem that famously belongs to NP but does not appear to be NP-complete, the so-called graph isomorphism problem, was shown by László Babai in 2017, by a remarkable and unexpected argument, to be soluble in "quasipolynomial" time — that is, much faster than a typical NP-complete problem.[7] This demonstrates that at least some natural NP problems that seem hard can turn out, for highly non-obvious reasons, to be very much easier than NP-complete problems. This increases our perception of the likelihood that factorizing could be such a problem.

§ 3. — Some general reasons for finding mathematical statements plausible.

A common thread that runs through many of the justifications discussed in the previous section is what I think of as *pseudo-Bayesian* arguments. That is, one starts with some prior belief about the likelihood of a mathematical statement being true, does a suitable experiment, and updates one's beliefs accordingly. What makes this process pseudo-Bayesian rather than genuinely Bayesian is that, as mentioned earlier, the probabilities in question do not take numerical values — rather, they are vaguer notions such as "extremely likely" or "certainly possible", which mathematicians might or might not be willing to convert into very rough estimates such as "with probability at least 70%". (For example, I am ready to stick my neck out and say, very much as a non-expert, that I would give the Riemann hypothesis at least a 90% chance of being true.)

This kind of reasoning is discussed in detail by George Pólya in his 1954 book Mathematics and Plausible Reasoning Volume II: Patterns of Plausible Inference and more recently taken up again by David Corfield in his article Bayesianism in Mathematics, from the book Foundations of Bayesianism, edited by David Corfield and Jon Williamson. One pattern of reasoning Pólya mentions is, for example, the following. Suppose you wish to assess whether a statement A is true, and for the moment your level of confidence in A is medium. Now suppose you spot that A has a consequence B that

[7] A function $f(n)$ is said to be quasipolynomial if it is bounded above by $A\exp(C(\log n)^r)$ for some constants A, C and r. (If $r = 1$ then $A\exp(C(\log n)^r) = An^C$ and f is bounded above by a polynomial.)

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would be extremely unlikely to be true "just by chance", and you then discover that B is true. This will greatly increase your confidence in A. Pólya presents this pattern as follows.

A implies B B very improbable in itself B true
A very much more credible

Pólya does not set out explicitly *why* A becomes very much more credible (though he provides persuasive examples), but it can be explained quite naturally in a pseudo-Bayesian way as follows. There are two possible explanations for the truth of B. One is that A is true, which happens with probability $\mathbb{P}[A]$. The other is that A is false and that B just happens to be true, which occurs with probability $\mathbb{P}[\neg A]\mathbb{P}[B|\neg A]$ (the second term stands for the conditional probability that B is true given that A is false), which is small. Putting this slightly more formally, Bayes's formula tells us that

$$\mathbb{P}[A|B] = \frac{\mathbb{P}[A\wedge B]}{\mathbb{P}[B]} = \frac{\mathbb{P}[B|A]\mathbb{P}[A]}{\mathbb{P}[B]} = \frac{\mathbb{P}[B|A]\mathbb{P}[A]}{\mathbb{P}[B|A]\mathbb{P}[A]+\mathbb{P}[B|\neg A]\mathbb{P}[\neg A]}$$
$$= \frac{\mathbb{P}[A]}{\mathbb{P}[A]+\mathbb{P}[B|\neg A]\mathbb{P}[\neg A]},$$

where the first three equalities constitute a proof of Bayes's formula and the last equality follows from the fact that B is a consequence of A, which implies that $\mathbb{P}[B|A] = 1$. Because $\mathbb{P}[B|\neg A]$ is small and $\mathbb{P}[A]$ is not particularly small, the bottom of the last fraction is only slightly bigger than the top, which means that the probability $\mathbb{P}[A|B]$, that is, the probability we should now assign to A given the evidence B, is close to 1.

I have still not made any attempt to make sense of the notion of the probability that a mathematical statement is true (or of the related idea that B might "just happen to be true"), but let us postpone thinking about this problem for the moment, and instead look back at some of the arguments in the previous section and try to understand them from a pseudo-Bayesian point of view.

Goldbach's conjecture provides a very good illustration of more than one of Pólya's patterns of plausible inference. A second principle, which is closely related to the first, he presents as follows.

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A implies B B quite probable in itself B true
A just a little more credible

It too can be justified using Bayes's formula. Again we have that

$$\mathbb{P}[A|B] = \frac{\mathbb{P}[A]}{\mathbb{P}[A] + \mathbb{P}[B|\neg A]\mathbb{P}[\neg A]},$$

but this time $\mathbb{P}[B|\neg A]$, rather than being small, is fairly close to 1, which means that the bottom of the fraction on the right is only a little smaller than $\mathbb{P}[A] + \mathbb{P}[\neg A] = 1$, and therefore the entire fraction is only a little larger than $\mathbb{P}[A]$.

Now let A be Goldbach's conjecture and let B be a statement such as that 10,000,000 can be written as the sum of two primes. We certainly know that A implies B, but in order to apply one of Pólya's patterns of reasoning we need to decide how probable we think that B is "in itself". That is, how likely would it be that 10,000,000 could be written as a sum of two primes if Goldbach's conjecture is not true?

The answer to this question seems to depend quite a lot on how hard one thinks about it. Somebody with no mathematical experience at all might think "It could go either way" or even "It's quite hard to find primes, so it's unlikely that there will be two of them that add up to a large number like 10,000,000 just by chance." Such a person might well have their confidence in Goldbach's conjecture significantly increased by the information that it is true for the number 10,000,000.

But somebody with more mathematical background will realize that the second piece of reasoning above is quite wrong: it may be hard to find large prime numbers, but that does not mean that they do not exist, and in fact the prime number theorem implies that they exist in some abundance. Therefore, for any given large even number it is highly probable that there will be two primes that add up to it "just by chance", and therefore knowing that it is true for just one such number has almost no effect on our confidence in Goldbach's conjecture.

However, in a sense that is true only because our confidence in Goldbach's conjecture has been greatly increased in another way. Let R be the (not completely precise) statement that the primes are distributed in a random-like way. If R is true, then given our knowledge that there are many primes less than n, the probability that

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any given large even number can be written as a sum of two odd primes is extremely close to 1. One can even quantify this probability (assuming a suitable random model) and reach the conclusion that the sum over all large m of the probability that $2m$ is not a sum of two primes is very small, and since Goldbach's conjecture has been checked for small m, this means that Goldbach's conjecture is very likely to be true.

What has happened here? We have established that if the random-distribution principle R is true, then almost certainly Goldbach's conjecture is true. But why should that increase our confidence in Goldbach's conjecture itself? On the face of it, since R more or less implies Goldbach's conjecture, it cannot be more likely to hold than Goldbach's conjecture itself, so should not increase our confidence in Goldbach's conjecture.

However, this objection fails: the point is the very simple one that if we observe that a statement A follows from a statement B that is very likely to be true, then it follows that A is very likely to be true, so our confidence in A may well become significantly higher than it was before we made the observation. An extreme example of this is if B is the final observation that completes a proof of A, but even if we do not know for sure that B is true, it may well be that for pseudo-Bayesian reasons we are more confident in B than we previously were in A.

In the case of R, one reason we can be more confident in it than we might have been in Goldbach's conjecture in isolation is that it makes stronger predictions: it implies for instance that there will be many ways of writing 10,000,000 as a sum of two primes and not just at least one. Those predictions are less likely to be true just by chance, so our confidence in R is increased by correspondingly more.

As I mentioned earlier, this is even more clear when we use R to make predictions about approximately how many ways there should be of writing 10,000,000 as a sum of two primes. Predictions of this kind turn out to be remarkably accurate, a fact that would be hard to explain without R, so this is another example of the first of Pólya's patterns mentioned above.

As I also mentioned, the extra generality of R allows one to perform a much wider diversity of tests. For example, one can use it to guess roughly how many primes p less than n there should be such that $p+2$ is also a prime. Such pairs $(p, p+2)$ are called *twin primes*, and the famous twin prime conjecture states that there are infinitely many of them. Although the twin-prime conjecture is

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still open, the number of twin primes up to n closely matches what R suggests it ought to be, at least for the range of n we have managed to test experimentally. This provides what one might think of as an independent test of R, since instead of counting solutions to equations of the form $p+q=2m$ (where p and q are required to be prime) we are counting solutions to the equation $p+2=q$ in the range $q\leqslant n$ for varying n. There are many such tests, and R seems to pass all of them (with one or two subtle exceptions that are well enough understood that they do not shake our confidence in a suitably adjusted principle).

This is an important observation. As we shall see, for several of the examples considered earlier, our confidence in a statement increases if that statement is shown to follow from a more general statement that we have been unable to disprove. Since there are many more potential ways of disproving a more general statement, we are more confident in the general statement than we were in its consequence when the consequence was considered in isolation.

Here is a list of general statements, not all of them precise, that played this kind of role in the discussions of specific problems earlier.

1. The normality of π follows from a more general principle that biases in decimal expansions do not occur without a reason, and that the reason is usually fairly simple. This more general principle is confirmed by many other examples: for instance, there is no discernible pattern to the decimal expansions of $\sqrt{2}$, e, π^2, and so on.

2. The statement that $R(k,k)^{1/k}$ converges to a limit follows from a general principle that the behaviour of large graphs is not very sensitive to the number of vertices. So if, for instance, $R(100,100)$ is close to 2^{100} and $R(150,150)$ is close to 2^{150}, then we would expect $R(200,200)$ to be close to 2^{200}. Again, there are many examples, and in some contexts even theoretical arguments, that support this general principle.

3. The statement that the average end-to-end distance of an n-step self-avoiding walk is roughly $n^{3/4}$ follows from the more general principle that the heuristic arguments of physicists are a good guide to the truth, even if they are not rigorous proofs. Once again, this general principle is confirmed by many other examples (though there have been occasional cases where pure mathematicians have disproved statements that physicists had confidently asserted).

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4. Like Goldbach's conjecture, the Riemann hypothesis can be justified by the more general principle that the primes behave as a natural random model would predict. (Recall Bombieri's statement that the failure of the Riemann hypothesis would create havoc in the distribution of prime numbers.) It also follows from the generalized Riemann hypothesis, which is a precise mathematical statement. It also follows from the even more general, but less precise, statement that there is a large family of zeta functions that all satisfy appropriate versions of the Riemann hypothesis. (The lack of precision is that, as far as I know, there is not a clear conjecture about what the family is, though there are many important zeta functions that would belong to it.)

A second reason for increased confidence in a conjecture is if it can be shown to have an "otherwise unlikely" consequence that we can prove, or at least confirm experimentally. Often the reason the consequence is unlikely is simply that it is very precise. (Of course, we see this in science too, such as Eddington's famous measurement of the gravitational deflection of starlight, which provided a dramatic confirmation of Einstein's theory of general relativity.) Some examples of this related to the problems that we saw earlier are the following.

1. The general principle that the primes are distributed as if they are random makes precise predictions about the approximate number of ways of writing a large even number as the sum of two primes.

2. The arguments of statistical physicists concerning self-avoiding walks predict not just the average end-to-end distance but also a great deal more detailed information about what a typical self-avoiding walk looks like, all of which is confirmed by experiment.

3. A consequence of P not equalling NP is that even though it is easy to find an efficient algorithm that solves 7/8 of the clauses in an instance of 3-SAT, there is no such algorithm that will solve a very slightly larger fraction of the clauses. And indeed, no such algorithm has been found, and there is no serious hope that one will be found.

4. It is conjectured that the zeros of the Riemann zeta function do not just all have real part equal to 1/2 but that they are

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distributed in a way that closely resembles the distribution of eigenvalues of random Hermitian matrices (chosen in a natural way). This basic idea has led to many remarkably precise conjectures about the behaviour of the zeros that agree impressively with experimental evidence.

It is also worth reflecting on reasons that our confidence may be decreased. One example we have seen was Odlyzko's point that the Riemann zeta function "reaches its asymptotic behaviour slowly". Although this does not alter the fact that the experimental evidence concerning the zeros agrees extremely well with precise theoretical predictions, it raises the possibility that there might be some phenomenon that shows up only for zeros with very large imaginary parts, which would make that phenomenon difficult to detect experimentally.

Another example was the probable difficulty of integer factorization, where there were two main reasons that mathematicians are inclined to be a little cautious in their assessment. One was that there are known algorithms that perform related tasks efficiently in very unexpected ways, so the mere fact that there is no *obvious* way of factorizing efficiently is not completely compelling evidence: it is at least conceivable that there is a non-obvious way of doing it. The other was that factorization is a central example of a problem that is in NP but that is not believed to be NP-complete, and another prominent example of such a problem was recently shown to be soluble in quasi-polynomial time.

The second of these reasons is an example of a further general principle that mathematicians use when making probabilistic judgments about unproved statements, which is that *similar statements may well behave similarly*. We often make guesses about problems that are based on experience with similar problems, though obviously there is work to do in deciding what "similar" means here, as some similarities will be more relevant than others. The integer factorization problem and the graph isomorphism problem are superficially quite different: the relevant similarity is that they both belong to NP and to co-NP, which is a rather deep phenomenon.

Once again, Pólya has identified this pattern of reasoning. He presents it as follows.

A analogous to B B true
A more credible

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He tentatively justifies it by saying that because the statements are analogous, one can imagine that they are both consequences of some more general statement H, and, as we have seen, generalizing a statement often makes it more plausible. Of course, if a third statement C analogous to A and B turns out to be false, this will reduce our confidence again, unless on closer inspection we find that C has some property that might lead us to expect it to behave differently, in which case we have the option of replacing the generalization H by a more specific generalization H' that includes A and B and rules out C.

§ 4. — What do probabilistic judgments about mathematical statements even mean?

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The pseudo-Bayesian account cannot fully answer the question of why mathematicians have differing levels of confidence in different unproved statements, because Bayes's formula contains probabilities on both sides of the equals sign. So while it may give a good account of how we update our beliefs given our priors and our assessments of certain conditional probabilities, it does not explain how we arrive at the priors and conditional probabilities in the first place (except when they themselves are updated beliefs, but that just pushes the problem further back). At some point we need to grapple with the question of what is going on when we use probabilistic language in this very deterministic-seeming context.

At first glance it seems to make no sense at all to say of a specific (decidable) mathematical statement S that S is "probably true" since it is either definitely true or definitely false. It is not as though we repeatedly try to prove it, usually succeeding but occasionally finding a counterexample.

However, this objection is not very strong, as easy examples from outside mathematics show. If you toss a coin and keep it covered with your hand, then it is either definitely heads or definitely tails, but it still makes perfect sense for me to say that the probability that it is heads is $1/2$. Indeed, that makes sense even if you have surreptitiously looked at the coin to see how it landed.

The judgment in the case of the coin can be justified with a very basic frequentist interpretation of probability: all one has to note is that the event is one that could in principle be repeated many times, and one would expect the coin to come up heads half the time and

tails half the time. So even though the coin has now been tossed, before it was tossed it was reasonable to assign a probability of $1/2$ to its coming up heads, and since I do not yet know the result it is still reasonable.

If we try to use reasoning of this kind for our judgment about S, we run into a problem, which is that S is not obviously an instance of a repeated event that has varying outcomes: it is just a fixed mathematical statement. However, one should not give up too quickly, as there are certainly *some* probabilistic judgments about mathematical statements that are similar to our judgment about the coin toss. For instance, suppose I choose a random integer n between 1,000,000 and 1,100,000. The prime number theorem tells me that about 7% of the numbers in that interval are prime, so I have no difficulty in saying that the probability that n is prime is about 7%.

This might be regarded as a cheat, however, since I chose n randomly, and if I put randomness in, then it is no surprise if I can get randomness out. So let us instead make it deterministic. I shall define a number n as follows. The number 2^{1200} has around 400 digits. Take the sequence of five consecutive digits of 2^{1200} that start at the 201st digit, add the corresponding five-digit number (which might start with some zeros) to 1,000,000, and let n be the result of this calculation. I would like to argue that it is reasonable to say that the probability that n is prime is about 7% — until, that is, I go ahead and check, which I shall now do. The sequence of five digits starting at the 201st is 06441 (if I didn't miscount), so n =1,006,441, which turns out to be prime, which is quite surprising given that it there was only a 7% chance of this happening.

But what does it mean to say that n had only a 7% chance of being prime? It is definitely prime, so does that not mean that it had a 100% chance of being prime? In this case, the answer seems to be something like the following. Although n is a single fixed number, the way it was chosen was rather arbitrary. In fact, what I did was to devise a simple *pseudorandom generator* — that is, a procedure that was likely to produce for me a sequence of five digits with no discernible pattern. I could just as easily have started the sequence at the 130th digit, or taken the digits in places 100, 200, 300, 400, 500 from the decimal expansion of $\sqrt{2}$, or used any number of other deterministic sequences defined in ways that would lead me to expect that they will not have any properties that would affect the propensity of n to be prime. (I could also have defined it in ways that *would* have had such an effect: for example, had I

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taken the last five digits of 2^{1200} then n would have been guaranteed to be even, or if I had taken five consecutive digits from the decimal expansion of 1/7, then there are only six numbers I could have obtained, none of which, it turns out, yield a prime when added to 1,000,000.)

Thus, my judgment that n had a 7% chance of being prime is based on regarding the deterministic choice of n as just one example of a whole class of choices — roughly speaking, numbers obtained from five-digit sequences that experience suggests will be typical. And the role of the calculation I did (with the help of Wolfram Alpha[8]) was very similar to the role you play when you lift your hand and show me the coin. It may be that the fact that n is composite follows from axioms and rules of deduction that I fully accept, but that does not imply that I know that n is composite: to gain such knowledge I need to do the calculation, which takes effort. Moreover, this last point can be made quite formal using the theory of computational complexity, which actually studies the amount of effort needed to determine whether certain mathematical statements are true.

The basic idea here is that a mathematical statement such as "This n is prime" can often be seen as a representative of a much wider class of mathematical statements, and once one has a big class of statements it makes perfect sense to ask what percentage of them are true. But while that kind of analysis shows that the notion of probability can sometimes make good sense, can we apply it to statements such as Goldbach's conjecture, the Riemann hypothesis, or the normality of π?

I believe that it is indeed possible, and that some of our reasons for judging these statements to be likely or unlikely suggest that we do indeed regard them as representative of larger classes of statements.

This is quite easy to see in the case of the normality of π. Let us think about how a typical mathematician's understanding of this problem might develop. As a child, one learns about decimals, and after a while one learns to calculate the decimal expansions of fractions such as 1/2, 1/5 and 1/10, followed by more interesting examples such as 1/3, 1/9 and 1/6, followed by more interesting examples still such as 1/7. In all these cases the decimal expansion either terminates or eventually recurs, so one might come to the

[8] Wolfram Alpha is a website that computes answers to questions from several different domains, including mathematics.

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view that non-whole numbers typically have terminating or recurring decimal expansions, which would lead one to expect, wrongly as it turns out, that π has such an expansion and is therefore very far from being normal. Note that π is being seen here not just as an individual number but as an example taken from the set of all numbers. One might realize on reflection that it is certainly not true that all numbers have recurring expansions, since one can just write down an infinite decimal expansion and make sure it doesn't recur. But one could still take the attitude that such numbers were artificial, whereas π arises naturally and is therefore likely to have a more regular decimal expansion.

A little later one's perspective shifts completely: one observes that numbers that arise naturally tend to have very *irregular* decimal expansions unless they are rational numbers. That is, terminating or recurring decimal expansions are very much the exception rather than the rule.[(9)] At that point, why is it natural to expect that π is a normal number? An obvious answer that our budding mathematician might give is, "We know the decimal expansion of π to many millions of decimal places: if there were some kind of bias, one would expect it to have shown up by now." This can be seen as appealing to the following general principle: *if the decimal expansion of an irrational number that arises naturally or is built in a simple way out of numbers that arise naturally does not show any sign of bias in the first million digits, then probably it will never show any sign of bias*. This principle allows for the possibility that there may be exceptions, but there are many numbers to choose from, so π probably isn't an exception.

Suppose we pursue the question a little further, raising the possibility that since π is a very special number, it might conceivably have a property that leads to an unexpected but very small bias in its decimal expansion. Why does this seem so unlikely? Again it is natural to appeal to more general principles: it is hard to imagine what a proof could possibly look like — for one thing, decimal expansions are closely tied to the number 10, which seems to have nothing to do with π — and we know of no such argument for *any* irrational number built in a simple way out of naturally occurring irrational numbers.

The absence of the merest hint of a technique that might be useful for showing that π is not normal is not in itself evidence that π is

[(9)] Of course, for this to happen one must first become familiar with facts such as that π is irrational and that there are a lot of irrational numbers.

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normal, but it becomes convincing thanks to an even more general principle that is confirmed by a great deal of mathematical experience.

No-coincidence principle. *If an apparently outrageous coincidence happens in mathematics, then there is a reason for it.*

We have arrived at a possible interpretation of the statement "π is almost certainly normal". Suppose we accept the no-coincidence principle on the grounds that it is amply backed up empirically. From it we can deduce that if there is no good reason for π not to be normal, then π is normal. So now our attention turns to the probability that there is a good reason for π not to be normal. Experience with good reasons suggests that they usually, though not always, kick in reasonably soon, so given that there is no discernible pattern in the digits of π that have so far been calculated, of which there are a huge number, any reason for the non-normality of π would have to be very strange and unusual. So now we wish to assess the probability that there is a good, but strange and unusual, reason for π not being normal. At this point we can perhaps appeal to a more precise variant of no-coincidence principle, namely that, on average at least, the greater the coincidence, the easier it is to find an explanation for it.[(10)] If, for example, there were only finitely many 7s in the decimal expansion of π, that would *a priori* be such an extraordinarily improbable event that we would expect to be able to uncover the reason for it. The fact that we have not been able to find any such argument, for π or for any other number, is therefore quite strong evidence that no such argument exists. Thus, the interpretation of what it means to say that π is almost certainly normal is that while unexpected reasons do exist and therefore strange things do happen, the chances that any such reason applies to any given statement (of a kind that asserts that there is no departure from randomness, and where this is backed up by experimental evidence and a complete failure to find anything resembling a plausible reason that there would be such a departure) are small.

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[(10)] I first heard something like this principle from Don Zagier, who argued that many problems in number theory are hard precisely because they are asking you to prove that something happens that is *exactly what you would expect*. If a phenomenon comes into this category, it is not clear that it demands an explanation, which raises the likelihood that there is no easy proof of it. Goldbach's conjecture is a case in point — as already discussed, it would be a big surprise if some large even integer were *not* a sum of two primes.

The example of $e^{\pi\sqrt{163}}$ discussed earlier can also be analysed in this way. To somebody who does not know the relevant number theory and has not heard that $e^{\pi\sqrt{163}}$ is extremely close to an integer, it is just a fairly arbitrary number one can build, not interestingly different from $\pi^{\sqrt{2}}$ or e^{π^2} or $e^{\pi+\sqrt{163}}$. And if you build numbers that are not obviously rational out of numbers like π, e and square roots of positive integers, then they will tend to have fractional parts that are pretty uniformly distributed in the interval $[0,1)$, so although some of them will be fairly close to integers just by chance, the probability of stumbling on one that just happens to be within a million millionth of an integer will be astronomically small.

One is therefore justified in being very surprised that $e^{\pi\sqrt{163}}$ is indeed within a million millionth of an integer: again the point is that we are seeing $e^{\pi\sqrt{163}}$ as a member of a more general class. Of course, that does not mean that an event with a probability of order of magnitude 10^{-12} has just occurred, because we should at least entertain the possibility that some of the numbers in the class are close to integers for special reasons. For example, this is true of the number $\phi^{100}/\sqrt{5}$, where ϕ is the golden ratio $\frac{\sqrt{5}+1}{2}$. The 100th Fibonacci number is given by the formula $\frac{\phi^{100}-(-\phi)^{-100}}{\sqrt{5}}$. Since $\phi>1$, ϕ^{-100} is tiny, so $\phi^{100}/\sqrt{5}$ is extremely close to the 100th Fibonacci number, which is an integer. Thus, some simple expressions do turn out, for good reasons, to define irrational numbers that are very close to integers, and this affects one's judgment of the probability that it will happen in any particular case. That contrasts with the case of the normality of π, where we completely lack arguments that might be used to prove non-normality, and therefore feel very confident that π is normal. Note also that additional mathematical knowledge can affect one's judgment of the likelihood of an argument of a certain kind existing. If, for instance, one is told that 163 is the largest positive integer k such that the field $\mathbb{Q}(\sqrt{-k})$ has class number 1, then one has the information that the number 163 has a very special number-theoretic property. This could be completely irrelevant, but it at least increases the probability that an argument exists that would show that $e^{\pi\sqrt{163}}$ is very close to an integer. And as it happens, it turns out to be highly relevant.

We can interpret the assertion that Goldbach's conjecture is almost certainly true in a similar way, but only after a certain amount of mathematical thought, because while the failure of some

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very large even integer n to be the sum of two primes would be a remarkably improbable event if we assume that the primes are "distributed randomly", the primes are in fact created in a rather systematic way — they are what is left over when we remove from the positive integers the number 1, then all multiples of 2 (not including 2 itself), then all multiples of 3, then all multiples of 4 (though they have already been removed), and so on. The sets we are removing have a lot of structure — they are arithmetic progressions — and it is not obvious that we can't exploit that structure to show that the set that is left over has interesting structure as well, over and above simple facts such as that only one prime is even. However, we now have a great deal of experimental evidence that the primes are indeed about as random as they can be, and we also have enough theoretical understanding of this quasirandomness to have been able to prove that every odd integer from 9 onwards is a sum of three odd primes, as well as several other results in a similar vein. This provides strong evidence that there is unlikely to exist a surprising reason for some large even integer *not* to be a sum of two primes, evidence that is all the more compelling given the tables that show a very close agreement between how many ways there are of writing large even numbers as a sum of two primes, and how many ways we would expect there to be.

Again, thanks to the no-coincidence principle, the focus has shifted from the probability that Goldbach's conjecture is true to the probability of the existence of a certain kind of argument. That we judge to be unlikely for various reasons: not only have we failed, after centuries of thinking about the primes, to find any arguments that would lead us to expect departure from randomness (if "randomness" is suitably interpreted), but the fact that we have also not observed any such departure empirically, either with Goldbach's conjecture or with a large number of other conjectures that assert that the primes behave roughly as one would expect them to, makes it unlikely that any such arguments exist, since if they did exist then we would probably have observed their consequences.

This kind of analysis can be used to understand both our confidence in the Riemann hypothesis, such as it is, and the fact that that confidence is not total. For there to be a counterexample to the Riemann hypothesis, there would have to be a very unexpected bias in the distribution of the prime numbers, so there would need to be a reason, and that reason would have to be somewhat strange, since the bias has not shown up despite a great deal of computational

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evidence being collected. However, as one of the main collectors of that computational evidence has pointed out, while it is difficult to imagine an explanation for the existence of a counterexample with what would have to be a very large imaginary part, it is not impossible, since there are reasons to believe that the typical behaviour of the Riemann zeta function manifests itself extremely slowly. That reduces our confidence that no argument of the required kind exists.

When it comes to the problem of whether P=NP, the no-coincidence principle does not at first seem to help in quite the same way. What might a reason for P not equalling NP be like? An obvious answer is that it might be an efficient algorithm for solving an NP-complete problem. But such an algorithm does not seem as though it would be an explanation for an outrageous coincidence: it would just be a straightforward counterexample to the assertion that no such algorithm exists.

Nevertheless, a slight modification of the no-coincidence principle does apply here reasonably well. Consider again the NP problem where one is presented with a number n (which one should think of as having a large number of digits), told that it is the product of two primes, and asked to work out what those two primes are. As mentioned before, it is relatively easy to program a computer to check whether a proposed answer is correct: if somebody suggests the two numbers p and q, then all one has to do is multiply p and q together using a standard procedure such as long multiplication. If P=NP, then this fact is enough to guarantee that one can actually *find* p and q, which on the face of it seems unlikely. (If you think it is not unlikely, see if you can find the two prime factors of 35873023. In case it helps, I obtained the numbers in question from a Wikipedia page entitled "List of prime numbers".)

But why should ease of checking have anything to do with ease of finding? If I'm looking for my telephone, I'll recognise it very easily when I see it, but that doesn't mean it is easy to find. Similarly, my ability to do long multiplication seems to have almost nothing to do with factorizing.[(11)] So although the statement that P=NP is not obviously a claim that an outrageous coincidence occurs, it is still claiming something pretty outrageous that would demand an explanation. Therefore, we can maintain the spirit of the no-coincidence principle by generalizing it slightly as follows.

[(11)] As I mentioned earlier, it is not impossible that there is some extremely clever method of factorizing efficiently. But if P=NP it would be possible to factorize efficiently by virtue of the fact that we can multiply efficiently — that is what seems unlikely.

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No-miracle principle. *If an apparent miracle happens in mathematics, then there is a reason for it.*

The difference between this and the no-coincidence principle is that we allow miracles that do not have natural probabilistic descriptions: it would be miraculous if the ability to carry out long multiplication guaranteed the ability to factorize efficiently, but the word "coincidence" does not seem appropriate for that particular miracle.

That said, with a little more effort it is possible to use the no-coincidence principle for the P versus NP problem as well. Indeed, one of Scott Aaronson's arguments has that flavour: if P did equal NP then it would be quite a coincidence that we find it easy to satisfy 87.5% of the clauses in a 3-SAT instance but extremely hard to satisfy 87.6% of them. There is also a concept of *pseudorandom generators*, which are deterministic methods of generating sequences that appear to all intents and purposes to be random. (Earlier in this article I defined a baby pseudorandom generator when I wanted to create a random-looking five-digit sequence and took a string of digits from the middle of the decimal expansion of 2^{100}.) There are several such methods, and the evidence is that they work, in the sense that they have no features that would allow one to detect efficiently how they were created or make any guesses about how they will continue. However, if somebody explains how they generated a pseudorandom sequence, it is easy to check that their method does indeed generate the given sequence, so if P=NP then there is some outrageous coincidence hidden in the behaviour of all these apparently random sequences that allows one to efficiently work out how they were generated (or at least how each one could have been generated).

Whichever version of the principle we decide to use, the conclusion is the same: for P to equal NP something outrageous would have to happen; that would demand an explanation; and the evidence leads us to believe with very high confidence that no such explanation exists. (Our confidence is perhaps not total, because over the years some very surprising efficient algorithms have been discovered, but there are additional reasons to believe that an algorithm for efficiently solving an NP-complete problem would be a step too far. That said, the existence of surprising algorithms is commonly taken as a reason to expect it to be very hard to *prove* that P does not equal NP.) So once again the focus shifts from the statement itself to the existence of a proof of a certain kind.

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Let me try to summarize the proposal I am making for how to interpret probabilistic judgments concerning mathematical statements. Perhaps the most important point is the suggestion that they are not really probabilistic judgments concerning the *truth values* of statements, but about the existence of *arguments* (not necessarily rigorous) in support either of those statements or of their negations. In particular, many probabilistic judgments appear to have justifications of the following general kind.

1. If statement S were false, a miracle would have to occur, and there would have to be an explanation for why it occurs.
2. There is probably no explanation of the kind that would be needed to explain such a miracle.

However, so far this replaces one probabilistic judgment of a deterministic event by another. What does it mean to say that there is probably no explanation of a certain kind, when either there is one or there isn't?

Here the proposal would be to interpret the probabilistic judgment as standing for a statement of the form that statements of a certain kind tend not to be backed up by arguments. That is, we look at the miracle not in isolation but as representative of a certain class of miracles. Our judgment is then a genuine probabilistic statement: that a random miracle chosen from that class has only a small chance of being explainable (and therefore only a small chance of actually occurring).

We have seen an example of this: somebody who did not know about $e^{\pi\sqrt{163}}$ would argue that it would take a miracle for such a number to be within 10^{-12} of an integer, and therefore that it could not be so close to an integer without some very good reason, and that such reasons, though they do exist, are few and far between. Therefore, $e^{\pi\sqrt{163}}$ is very unlikely to be within 10^{-12} of an integer. That would be a perfectly reasonable judgment, even though it happens to be mistaken.

Note that the more one knows about a problem, the more features of any given miracle one will be aware of, which affects what class of miracles one regards the particular miracle as belonging to. For instance, if you are asked whether $(\sqrt{2}+1)^{30}$ is likely to be very close to an integer after reading what I wrote earlier about the golden ratio, you will probably regard $(\sqrt{2}+1)^{30}$ not just as some random strange number that can be built out of well known irrational numbers, but as a power of a root of a fairly simple quadratic equation, which might well have a reason to be close to an integer — as indeed it turns out to be.

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§ 5. — Goodman-type paradoxes.

Our discussion has taken us quite a long way from *experimental* evidence, so let us briefly consider what part it has to play in the above account. I have suggested that what is meant by the judgment that S is probably true is that the miracle that would be required for S to be false belongs to a class of miracles of which only very few actually occur, and that is because miracles do not occur without explanations, and explanations of the kind that would be needed to explain miracles in the given class are very rare.

If that is a correct interpretation, what is the role of experiment in increasing our confidence in a mathematical statement? More generally, how does that interpretation fit with the pseudo-Bayesian account of how we update our perceptions of probability in the light of new evidence?

The answer to the second question is quite easy to give in connection with specific examples. For instance, let us consider again the fact that our confidence in Goldbach's conjecture increases somewhat when we find that it holds for all even integers up to 10^{10} (say), quite a lot more when we find that the number of ways of writing an even integer n up to 10^{10} as a sum of two primes is not just non-zero but large whenever n is large, and even more when we find that the number of ways of doing it is not just large but a good approximation to what a natural random model of the primes would predict.

The fact that Goldbach's conjecture is true up to 10^{10} means that if it is false, then it has to be true up to some large integer and then false. That would be quite strange, so it counts as a miracle that would need an explanation. However, there are quite a lot of explanations around that have the right form: that is, they explain why some statement of the form $\forall n\, P(n)$ is false but also explain why the smallest counterexample is very large. That is not to say that such explanations are commonplace, but it says that in the absence of any further considerations, the miracle would not be all *that* miraculous, so we do not want to assign too small a probability to Goldbach's conjecture being false.

If we now find that the large even integers we look at can all be written in many ways as a sum of two primes, then we start to get the impression that writing an even integer as a sum of two primes tends to be not just possible but actually quite easy. It now becomes

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significantly harder to imagine what an explanation for the conjecture being false could look like. There would presumably have to be something special about some particular very large even integer n that caused a "conspiracy" between the primes — a deal that each time p is prime, $n-p$ will make sure it has a non-trivial factor. There is no hint of any such conspiracy occurring, or even getting close to occurring, for any of the even integers we have looked at so far, and it is very difficult to think of a property of an extremely large n that would not hold for any smaller n and would explain why the primes suddenly aligned themselves for that n.

Finally, if we go on to observe that the number of ways of writing a large integer n as a sum of two primes is well predicted by a random model, then what is demanded of a putative explanation for the failure of Goldbach's conjecture is even stronger: we would need to know why the number of ways of doing it behaves exactly as a random model would predict for all the even integers we have looked at, but then for some very large even integer not only fails to behave as predicted but fails in a very radical way. Since the random model makes good predictions not just about Goldbach's conjecture but about many other statements to do with the primes, the evidence strongly suggests that no such explanations exist (since if they existed, we would expect to have observed their consequences by now). This contrasts with the situation we were in when all we knew was that Goldbach's conjecture itself was true up to 10^{10}, when we were considering a different class of explanations — broadly speaking, explanations of why some natural property is true for all positive integers up to some very large n but not true for all positive integers — of which there are some notable examples.

Let us now consider how a famous paradox of Goodman plays out in the context of computational evidence for mathematical statements. Goodman pointed out that the traditional problem of induction — why should we believe that a certain statement will continue to be true just because it has been observed to be true up to now? — is not adequately formulated, because there are many statements for which we do not have such a belief. To make his point, he defined a predicate "grue", which means "green up to time t and blue thereafter". If we set t to be the beginning of 2050, say, then all experimental confirmation of the statement "all emeralds are green" also confirms the statement "all emeralds are grue". And yet we do not believe the latter statement — we believe that at

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the beginning of 2050, emeralds will remain green. It is clear that the reason for this has something to do with the fact that "grue" is a very unnatural predicate, but it is quite hard to specify which predicates should count as natural.

Here is a statement that relates in a Goodman-like way to Goldbach's conjecture.

Conjecture. *No power of* $2^{1,000,000,000}$ *is a sum of two primes, but all other even integers greater than 2 are sums of two primes.*

All the computational evidence we have obtained is consistent with this modified conjecture — indeed, the largest known prime is far smaller than $2^{1,000,000,000}$. So why do we not regard the evidence as supporting the modified conjecture?

If one uses the framework suggested in the previous section, then the answer is surprisingly easy: for the modified conjecture to be false, a miracle would not have to occur. Indeed, all that would be needed for the modified conjecture to be false is for the primes to behave exactly as expected, since that would guarantee that $2^{1,000,000,000}$ could be written in many ways as the sum of two primes. In the case of Goldbach's conjecture itself, a miracle needs to occur for it to be false, and the experimental evidence, which has failed to find even a hint of such a miracle, strongly supports the hypothesis that there is no reason for a miracle to occur, which in turn (thanks to the no-miracle principle) strongly supports the hypothesis that no miracle in fact occurs. For the modified conjecture, a miracle doesn't need to occur, so the experimental evidence no longer plays this role.

A very interesting contribution to the large literature about Goodman's paradox was made by Rosemarie Rheinwald in 1993. She points out that one's starting beliefs, or what she calls our *epistemic situation*, have a critical effect on whether a predicate seems natural or not. She illustrates this with the following beautiful example. Apparently, there are two kinds of hares, field hares and Alpine hares. Field hares are always brown. As for Alpine hares, they themselves come in two kinds: one kind is always white, while the other kind is brown in the summer and white in the winter. She invents a Goodman-like predicate "su-wi-brote" that allows one to express this situation by saying, "All hares are su-wi-brote". She then argues that "su-wi-brote" is a *projectible* predicate — this means that it is suitable for the purposes of inductive inference — but only to somebody with the right set of background beliefs. To

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someone like me before I read her article, the fact that all hares I had observed up to now (not just in real life but in books, photographs, etc.) were su-wi-brote would not count as evidence in favour of the hypothesis that all hares are su-wi-brote, since they had also been brown and I had not heard of Alpine hares. But if somebody who *had* heard of Alpine hares and knew a bit about them were to obtain appropriate confirming instances of the hypothesis "All hares are su-wi-brote" — and for the confirmation to be appropriate it would of course be necessary to look at field hares and Alpine hares, and to look at the latter both in summer and winter — then their confidence in the statement "All hares are su-wi-brote" would be increased.

We have already seen examples of how background beliefs have an important effect on one's perception of how likely mathematical statements are to be true. They can also have an effect on which predicates are projectible, as the following example, which is well known to mathematicians, illustrates nicely.

The example concerns the following question. Suppose you draw n points round the circumference of a circle, and you join each pair of points with a line segment. Assuming that the points are in general position (in particular, no three of the lines meet at a point that isn't one of the points on the circumference), how many regions is the circle divided into by the lines?

If one draws one point, then there are no lines and the circle is "divided" into one region. If one draws two points, then there is one line, which divides the circle into two regions. With three points, the lines form a triangle, and there are therefore four regions — three outside the triangle and one inside. With four points, the lines form a quadrilateral with its two diagonals, making eight regions — four outside the quadrilateral and four inside. With five points there are sixteen regions, as you will readily see if you draw a picture. (There is a pentagon with five regions outside it and a five-pointed star inside. Inside the pentagon but outside the star there are five triangular regions. The star itself consists of an inner pentagonal region and five triangles attached to it. This makes 5+5+5+1=16 regions.)

Writing down the numbers we have obtained so far, we obtain the sequence 1, 2, 4, 8, 16. It would seem that the only natural hypothesis we can form is that the number of regions doubles each time one adds a point, so that with n points there are 2^{n-1} regions.

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However, as is often pointed out, *any* continuation of *any* short sequence can be justified with the help of the fact that if d terms $a_1,\dots,a_d$ of a sequence are given, then there is a polynomial P of degree at most $d-1$ that $P(1)=a_1, P(2)=a_2,\dots,P(d)=a_d$. For example, suppose I wish to create a cubic polynomial P such that $P(1)=1, P(2)=1, P(3)=2$ and $P(4)=3$. I first note that the polynomial $(x-2)(x-3)(x-4)$ is a cubic polynomial that takes the value -6 at 1 and 0 at 2, 3 and 4, and in a similar way I can create cubic polynomials that vanish at any three of the numbers $1,2,3,4$ and not the fourth. Then a suitable combination of these cubic polynomials can be used to give me one that takes the values I wish. In this case, we will end up with the polynomial

$$P(x)=-\frac{(x-2)(x-3)(x-4)}{6}+\frac{(x-1)(x-3)(x-4)}{2}-\frac{(x-1)(x-2)(x-4)}{2}+\frac{(x-1)(x-2)(x-3)}{6},$$

the origin of which I could disguise if I simplified it by multiplying out all the brackets and collecting terms.

Returning to the sequence 1, 2, 4, 8, 16, we therefore see two things: first, given any continuation of this sequence, there is a polynomial of degree at most 5 that will give that continuation, and secondly, there is a polynomial of degree at most 4 that agrees with the sequence so far. In the light of that, should we revise our assessment that 32 is the obvious continuation of the sequence?

If one is given the sequence 1, 2, 4, 8, 16 in isolation and asked to continue it, then 32 is unquestionably the most natural answer: it is unusual for each term of a sequence to be obtained from the previous one by a process as simple as doubling, and this has now happened four times. (I suppose I must acknowledge that it is also the case that we have *troubled* the previous number four times, where that means doubling it if the number is at most 10 and trebling it if it is greater than 10, but troubling is quite clearly a more complicated operation to define than doubling.) By contrast, it is not at all unusual for a sequence of five numbers to be given by the values of a quartic polynomial, since that is true of *all* sequences of five numbers.

Interestingly, one can say the same if one has a little more information about where the sequence comes from. Suppose one knows that it comes from a parameter associated with a natural sequence of combinatorial structures. Experience shows that something like the following principle is quite reliable.

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Nice-formula principle. *Given a sequence that arises naturally in mathematics, either it is given by a nice formula, or there is no hope of expressing it by any exact formula but it can at least be approximated by a nice formula.*[12]

I would not wish to claim that the above principle holds universally — indeed, I can think of examples of combinatorial problems that give rise to bizarre formulae — but exceptions to it appear to be quite rare. (Note that this probabilistic statement is quite straightforward to interpret: it is simply saying that the proportion of naturally occurring sequences that are exceptions to the rule is small.) Nevertheless, if one knows that a sequence that begins 1,2,4,8,16 has arisen naturally in a mathematical context, there is a very good chance that it is the sequence of powers of 2.[13]

Now let us change our mathematical epistemic situation by reflecting a little more on how the number of regions the circle is divided into behaves as the number of points increases. Each region contains on its boundary either one of the n points on the circle or a point where two of the connecting lines intersect. Each of the n points on the circle belongs to n regions, and since each intersection of two of the lines can be specified by four of the points (the end points of the two lines in question), the number of intersections of two of the lines is at most $\binom{n}{4}$. Moreover, each such intersection belongs to four regions. Therefore, the number of regions is certainly less than $n^2+4\binom{n}{4}$, which is easily checked to be at most n^4 for all positive integers n.

This shows that the formula 2^{n-1} cannot be correct — it grows exponentially quickly, which, as any mathematician knows, is a far quicker rate of growth than any polynomial, and in particular far quicker than n^4. Indeed, we can say more: if there is a polynomial formula for the number of regions, then that polynomial will have to be of degree 4 at most, since polynomials of higher degree

[12] An example of the second situation is the sequence of primes: there is no nice formula for the nth prime, and almost certainly no sensible formula at all, but it follows from the prime number theorem that the nth prime is approximately equal to $n\log n$. For the first situation, I am taking the word "formula" in a fairly broad sense, and would include sequences given by simple recurrence relations or as the coefficients of the power series of some nice function, for instance.

[13] I implicitly appealed to the nice-formula principle when discussing the Riemann hypothesis earlier. If one forms the sequence of real parts of the zeros of the zeta function, in increasing order of their imaginary parts, then it must equal $1/2$ for all the many terms we know so far, and must equal $1/2$ at least 40% of the time. The nice-formula principle then tells us that it should be a constant sequence.

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grow faster than polynomials of lower degree and we know that the growth rate is not faster than that of n^4.[14]

With this new perspective, we find ourselves asking the following question: what is the nicest formula that would yield the values 1,2,4,8,16 but grow at most as fast as a quartic polynomial? And the answer is probably that one should actually take a polynomial. There is only one that can work, and if it gives the correct formula, then the number of regions we obtain with six points round the circle should be 31. And that turns out to be the case.[15]

The main point I am making here is that the hypothesis that the sequence 1,2,4,8,16 is given by the unique quartic polynomial that takes those values is a bit like the hypothesis that all hares are suwi-brote: at first glance it seems very unnatural, but if one knows a bit more, then that perception changes substantially.

This is also another nice example to test out the analysis of probabilistic judgments. It now seems highly probable that the quartic polynomial is indeed the correct formula for the number of regions, but why?

Before we did the experiment of drawing a sixth point and finding that the number of regions was 31, our two likely hypotheses, given the nice-formula principle, were these.

1. The number of regions is given by a nice formula.
2. The number of regions is not given by a nice formula but can be approximated by a nice formula.

The fact that the obvious nice formula 2^{n-1} was doomed to fail meant that the nicest formula by some way was given by the one quartic polynomial that agrees with the sequence 1,2,4,8,16. Therefore, if the first hypothesis is true, then probably the number of regions is given by this formula. If the second hypothesis is true, then there is some formula that will be a good approximation to the number of regions when n is large, and may or may not be a good approximation when n is small. With a bit of experience, we might also judge that the chances of the number of regions having a polynomial dependence on n are reasonably high — something

[14] A less conclusive but still quite convincing argument that 2^{n-1} is unlikely to be the correct formula is that it fails when $n = 0$: if you draw no points, then the number of regions will be 1 and not 1/2. This is less conclusive because sometimes zero behaves differently from other numbers, but it is a disturbing observation nevertheless.

[15] The polynomial in question can be written as $\binom{n-1}{0}+\binom{n-1}{1}+\binom{n-1}{2}+\binom{n-1}{3}+\binom{n-1}{4}$. The advantage of writing it this way is that it makes it clear why it gives powers of 2 for small n and not for large n. Note also that if we substitute in $n = 0$ we obtain the answer 1, so the troubling anomaly at zero is no longer present.

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like 75%. (This judgment is based on having seen a multitude of problems, some of which lead to nice exact formulae and others of which don't, and feeling that this one could go either way but is probably the right sort of problem for a polynomial dependence.)

If the first hypothesis is correct, then the next term of the sequence will be 31. If it is incorrect, then the next term might be 31 just by chance, but that is fairly unlikely — there are probably around half a dozen numbers it might be. If we take this probability to be 1/6, then Bayes's formula tells us that the probability that the first hypothesis is true given that the number of regions is indeed 31 when there are six points is

$$\frac{3/4}{\frac{3}{4}+\frac{1}{6}\times\frac{1}{4}}=\frac{18}{19}.$$

Of course, the above calculation depended very much on the prior: if we had started out with a greater faith in the formula turning out to be nice, then our confidence after the experiment would be that much higher.

If we work out the next value of the polynomial, we find that it is 57, and this agrees with the number of regions when there are seven points round the circle. A Bayesian calculation similar to the above, but with 1/6 replaced by a smaller fraction (because now the range of plausible numbers if there is not an exact formula is larger) increases the probability to one that is quite a bit closer to 1 — something like 199/200.

It is worth reiterating that this sort of reasoning is of more than merely philosophical interest, and plays an extremely important role in the choices mathematicians make when deciding what to think about next when they are searching for proofs. This can be seen very directly if one looks at a famous database of integer sequences created by Neil Sloane. Sloane's database is a huge collection of beginnings of sequences obtained from all over mathematics, and it has led to many discoveries in the following way. A mathematician thinks about a problem that leads naturally to the definition of an integer sequence, calculates the first few terms of that sequence, searches for a sequence in Sloane's database that agrees with this sequence as far as the calculations have been carried out, and then guesses that the two sequences are equal. This often turns out to be the case even if the sequences have been obtained in very different ways. There follows the fascinating process of trying to explain *why* the sequences are the same — the no-coincidence principle gives one considerable confidence that such an explanation exists.

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The two conclusions we can draw about Goodman-like mathematical statements after the discussion above are these.

1. Some such statements are not backed up by experimental evidence because they do not require a miracle to be false, so the analysis of why we judge some statements to be likely, and of the role that experimental evidence can play in that judgment, does not apply.
2. What looks like a natural hypothesis can change dramatically when one knows more about a question.

This does not fully solve Goodman's paradox in a mathematical context, however. For instance, I have not given a Goodman-proof account of what a "miracle" is or of what a "nice formula" is.

These two questions are closely related. For instance, we do not say that it is a miracle that of all the million possibilities that it could be, the sequence of six consecutive digits of π that starts at the 20th digit after the decimal point should turn out to be 626433, but had it turned out to be 000000 then we would have been surprised. Had it been 333333 or 123456 we would have still been surprised but perhaps slightly less so, and if it had been 345678 or 246810 we would have been slightly less surprised still. And the reason for that seems to be tied to how easy it is to describe the sequences.[16] [17]

So a rough definition of "miracle" is something like this. Before we do an experiment (which might take the form of a hand calculation) we have a probability distribution in mind of what the outcome is likely to be. If a certain property is, according to that probability distribution, very unlikely to occur *and also has a very simple description*, then that counts as a miracle. Of course, it is only a miracle from our prior perspective: the no-miracle principle tells us that there is probably an explanation for what we have just observed, and if we find one then we will no longer perceive the observation as miraculous.

To give an example, if we found a very large even number that could not be expressed as a sum of two primes, that would be an

[16] The reflex for many mathematicians would be to mention Kolmogorov complexity at this point, but while in many situations it would be very surprising if a random-looking sequence could in fact be generated by a short program, the miracles that occur in normal mathematics tend to involve sequences with patterns that are simpler than is guaranteed by their being the output of a short program.

[17] Only after writing this paragraph did I learn that starting at the 768th digit of π there is a sequence of six consecutive 9s. This is genuinely surprising, but not surprising enough to demand an explanation.

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extremely unlikely event according to a very natural random model of the primes, and it is also an easy event to describe, so we would count it as a miracle. The event "Can be written as a sum of two primes in under half the number of ways one would expect" would count as a miracle if it occurred — not quite as big a miracle, but still (to an expert) utterly astonishing. But the event "Can be written in r ways for some positive integer r that has four 5s among its last six digits" would not be a miracle — there are just too many descriptions of about that level of complexity, given the probability of the event that we are witnessing.

Thus, it remains to discuss concepts such as "nice formula" or "simple description" or "natural statement".

§ 6. — Naturalness and levels of abstraction.

If I am using LaTeX to typeset a document and want to emphasize a word, then I have two options. One is to enclose the word in curly brackets and write `\textit` outside it. This has the effect of putting the word into italics. The other is to use the command `\emph` instead, which stands for "emphasize". Here is a sample sentence, typeset once using `\textit` and once using `\emph`.

1. We have also *troubled* the previous number four times.
2. We have also *troubled* the previous number four times.

Which command I chose made no difference to the output, but there is nevertheless an important difference between the two commands, and in many contexts it is considered better style to use `\emph`. To see why, let us look at an example where the choice of command *does* make a difference. Again, I shall use `\textit` first and then `\emph`.

Definition. *A positive integer n is said to be troubled if $n \leqslant 10$ and it is doubled or $n > 10$ and it is trebled.*

Definition. *A positive integer n is said to be* troubled *if $n \leqslant 10$ and it is doubled or $n > 10$ and it is trebled.*

The advantage of `\emph` is now obvious: inside the particular LaTeX definition environment I chose, the non-mathematical text is italicized, so if I use `\textit` in order to stipulate that the word "troubled" should be italicized, the result is that it is not distinguished from its surroundings and is therefore not emphasized.

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But if I use the command `\emph`, then LaTeX knows that I want to emphasize the word, so it chooses a contrasting font *whatever environment it is in*. The command `\textit` is something like a rigid designator — it causes text to be in *that* font, regardless of the environment — whereas `\emph` is more like a definite description — it causes text to be in "the font that contrasts appropriately with the surrounding font", which varies from environment to environment.

A great advantage of "nonrigid" commands is that they make it much easier to change the look of a document. Suppose, for example, that a journal's house style was to use boldface to emphasize words in definition environments. It could have a style file in which the `\emph` command was defined so as to ensure that that happened, and there would be no need to make any changes to the source file of the document itself.

Let me now jump to a different example. In 1988, Douglas Hofstadter, Melanie Mitchell and others developed a program called Copycat, with an extremely interesting architecture, which was designed to solve simple analogy problems concerning letter strings, of which a representative example is the problem `abc:abd::ijk:?`, which can be read as "`abc` is to `abd` as `ijk` is to what?" The class of problems of this kind is surprisingly rich, which makes solving them an interesting computational challenge. But here I just want to discuss what makes one solution more satisfying than another.

For that purpose, let us look at the following example: `abc` is to `abd` as `abbcccdddd` is to what? Here are several possible answers to that question, together with brief justifications for each.

1. `abd` (replace the string by `abd`)
2. `abdcccdddd` (replace the third letter in the string by `d`)
3. `abbcccdddd` (replace the last letter by a `d`)
4. `abcccdddd` (replace the third letter by its successor)
5. `abbddddddd` (replace all `c`'s by `d`'s)
6. `abbddddddd` (replace all `c`'s by their successors)
7. `abbcccddde` (replace the last letter by its successor)
8. `abbccceeee` (replace the letters in the last group by their successors)
9. `abbccceeeee` (replace the key parameters that define the last group by their successors)

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It is clear that the last answer is by far the most satisfying, and that amongst the others some are more satisfying than others, with some of the less satisfying ones being quite laughable. For instance, a natural reaction to the first proposed answer is "Why would one replace every string by `abd`?"

A good measure of what makes one answer better than another appears to be nonrigidity. In each case, the justification takes the form of a description of some process that can be applied to at least some letter strings. Some of these processes involve constants, such as particular letters of the alphabet or particular small numbers. To the extent that they do so, they are rigid. Others are defined in ways that vary in a simple manner according to the sequence, which makes them nonrigid. For example, the second answer makes use of the letter `d` and the number 3, so it is a highly rigid process, and that is why it seems ridiculous, and similar reasoning applies, to differing extents, to all of the first six proposals.

The analysis of the last three answers is slightly subtler, but still involves rigidity. If we look at the sequence `abbcccdddd`, we very easily spot that it can be described as one `a` followed by two `b`'s followed by three `c`'s followed by four `d`'s. And while we do not have a convenient language to express the thought concisely, we definitely also notice that the letters and numbers involved in that description were not arbitrary: we went through the first four letters of the alphabet in order and the first four natural numbers in order. We can even go up a level of abstraction and say that we performed the same process on the natural numbers as we did on the letters (thereby avoiding having to say twice that we chose the first four in order).

This description is greatly preferable to describing the sequence by simply listing it, because it is significantly less rigid. Instead of specifying a string of ten letters one by one (as we would have to do for a string such as `rjkepgjwrt`), we can regard the generation of the sequence as itself the solution of some smaller-scale analogy problems, such as `a:bb::bb:?`, together with an instruction to stop after four steps. (We would also think of `a` not as "the letter `a`" but as "the first letter of the alphabet", and so on.)

Once we think about the sequence in this way, a natural response to answer (7) is "Why do you want to do something to the last letter in the sequence?" The operation "pick the last term" is a bit too rigid, but it can be made less so if we replace it by "pick the last group", which yields answer (8). Similarly, the operation "replace

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all letters in the last group by their successors" can be made less rigid still if we go for "take the letters generated in the last stage of the generating process and replace them by the letters in what would have been the next stage of the generating process".

The suggestion I would like to make in this section is that the more abstract (that is, non-rigid) a statement is in mathematics, the more natural it is perceived as being, and the more likely it is that we will be convinced by confirming evidence. For example, the variant of Goldbach's conjecture that I suggested earlier, that every even number is a sum of two primes as long as it is not a power of $2^{1,000,000,000}$, is not a natural statement because it depends on the constant $2^{1,000,000,000}$ (though the fact that we can express it more concisely as $2^{10^{3^2}}$ makes it more natural than if we replaced it by a number that was much harder to specify).

For a more interesting example, consider the following two statements.

Statement 1. *Let G be a group. If $x^2 = e$ for every element x of G, then G is Abelian.*

Statement 2. *Let G be a group. If $x^5 = e$ for every element x of G, then G is Abelian.*

As it happens, the first of these statements is true and the second is false. However, I would like to argue also that the first statement is more natural than the second.

At first sight they seem very similar — all I have done is replace one constant, 2, by another. However, the first statement can be reformulated in a more abstract way as follows.

Statement 1 (Equivalent version)**.** *Let G be a group. If every element of G is equal to its own inverse, then G is Abelian.*

Once the statement is formulated like this, we see that 2 was not really a constant: rather, it was the only number that bore a particular relation to the notion of a group. (This is similar to our earlier recognition that the string `abbcccdddd` can be specified in a more abstract way.)

The fact that 5 does not have this property has important consequences for what my judgments about the second statement would have been had I not known whether it was true or false. I would have expected that *if* the second statement was true, then it would be a special case of a more general statement such as that for any

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prime p, a group G must be Abelian if x^p is always equal to the identity. That would be a more abstract statement and therefore more natural.

Similarly, the fact that 2 does have this property has a big influence on how I would set about proving Statement 1 if I did not already know the proof. I would try to resist thinking of 2 as "that number" and instead focus on its properties, so if I spotted that the equation $x^2 = e$ implies the equation $x = x^{-1}$, I would seize on that observation as being likely to help.[18]

For a third example, consider the following two theorems from linear algebra, and particularly their proofs.

Theorem. *Let V be a finite-dimensional vector space. Then V is isomorphic to its dual space V^*.*

Proof. Let $v_1, \dots, v_n$ be a basis of V. Then define a basis $v_1^*, \dots, v_n^*$ of V^* as follows: if v is a vector in V, then it can be written uniquely in the form $\sum_{j=1}^n \lambda_j v_j$. Let $v_i^*(v)$ be defined to be λ_i. (Thus, v_i^* picks out the ith coordinate of v with respect to the given basis.) It is easy to check that $v_1^*, \dots, v_n^*$ is a basis of V^*, and then the map $\sum_i \lambda_i v_i \mapsto \sum_i \lambda_i v_i^*$ defines an isomorphism between the two spaces. □

Theorem. *Let V be a vector space. Then V embeds isomorphically into its second dual space V^{**}.*

Proof. Let $\mathbb{F}$ be the field of scalars of V. For each $v \in V$ we can define a linear functional $\tau_v : V^* \to \mathbb{F}$ by $\tau_v(v^*) = v^*(v)$. If $\tau_v(v^*) = 0$ for all v^*, then $v^*(v) = 0$ for all v^*, so $v^* = 0$. Also, the map $v \mapsto \tau_v$ is easily checked to be linear. Therefore, that map is an isomorphic embedding. □

There is a very important distinction between the two proofs, which leads mathematicians to describe the isomorphism given in the first proof as unnatural and the isomorphic embedding given in the second proof as natural. Indeed, in contexts such as this one, the word "natural" has a formal meaning (the category-theoretic concept of a *natural transformation*) that captures the difference. The clearest sign of the difference is the first sentence of the first proof. One is asked to take a basis, but is not told how to do so. Indeed,

[18] And indeed it does. It is a standard fact in elementary group theory that $ab = b^{-1}a^{-1}$ and if every element equals its own inverse then we deduce immediately that $ab = ba$.

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given an arbitrary finite-dimensional vector space there is no single best method of picking a basis. (For example, consider the 2-dimensional vector space of all triples $(x, y, z) \in \mathbb{R}^3$ such that $x + y + z = 0$. It is easy to find a basis, but not easy to argue of any particular basis that it is the most natural and obvious basis to pick.) Thus, the basis feels like a rigid choice, because it does not relate to the space in a clear way. This is a slightly different kind of rigidity: it is not saying that the basis is always the same regardless of the space, but rather that each basis one chooses exists only in the space for which it was chosen. Mathematicians would say that the choice is *non-canonical*. By contrast, the choice of τ_v given v is entirely canonical — it is the obvious linear functional to build out of v.

Returning to the example of division of the circle, we can see why the sequences that played a role in that discussion were natural. We knew from experiment that we had a sequence that began 1,2,4,8,16. The sequence of powers of 2 can be thought of as the solution to the easy analogy problem `1:2::2:4::4:8::8:16::16:?` (and the reason we regard the operation performed at each step as doubling rather than troubling is that the definition of troubling involves additional constants such as 3 and 10), while the correct sequence 1,2,4,8,16,31,57,... can be defined as the sequence of values of the lowest-degree polynomial consistent with the values 1,2,4,8,16. (Another way to pick out the sequence of powers of 2 would be to say that $a_n = P(a_{n-1})$ for some polynomial P, and moreover P is the polynomial of smallest degree that is consistent with the values 1,2,4,8,16, which happens to be the polynomial $2x$.)

Often the naturalness of a mathematical statement is not immediately apparent. The Riemann hypothesis provides an example of this. The precise statement can be written as follows.

Conjecture (Riemann hypothesis)**.** *If $\zeta(s) = 0$, then either s is a negative even integer or the real part of s is equal to $1/2$.*

At first sight, the fact that this statement is suggesting that there are two different kinds of zeros makes it seem unnatural. However, as Riemann showed, the statement has another formulation that lacks this defect. The modern way to express it is to define a function ξ by the formula

$$\xi(s) = \frac{1}{2}s(s-1)\pi^{-s/2}\Gamma(s/2)\zeta(s),$$

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which I shall not try to explain here. Riemann showed that the xi-function satisfies the functional equation $\xi(s) = \xi(1-s)$. This property is not enjoyed by the Riemann zeta function, which makes the ξ function in some ways more natural (in a different sense of "natural" from the one I have been discussing). Better still, the zeros of the ξ function are the zeros of the ζ function with the zeros at negative integers excluded, so the Riemann hypothesis is equivalent to the following statement.

Conjecture (Reformulation of Riemann hypothesis)**.** *If $\xi(s) = 0$, then the real part of s is equal to $1/2$.*

But even this statement involves the constant $1/2$. Is that a sign of unnaturalness? For all sorts of reasons it isn't. One is that the vertical line through $1/2$ in the complex plane is the only one that is mapped to itself by the symmetry $s \mapsto 1-s$ of the ξ function — this gives a way of specifying the number $1/2$ in terms of the function rather than simply as the number $1/2$. Another is that, as I mentioned earlier, the Riemann hypothesis is equivalent to a statement about the distribution of the primes, which says, roughly speaking, that the error term in the prime number theorem is roughly what the natural random model would predict. The appearance of the number $1/2$ in this model is closely related to probabilistic phenomena such as that the expected end-to-end distance of a random walk of length n is around $n^{1/2}$. So again we can think of it not as "that number" but as "the exponent that comes up when you add together a number of independent random variables that satisfy some commonly occurring conditions".

As one final example, let me return to the question I left open at the end of my discussion in §2 of the behaviour of the Ramsey number $R(k,k)$. There I had argued that if $R(k,k)$ was approximately C^k for some very large k, then we would expect $R(k,k)$ to be approximately C^k for *all* very large k. I then invented an artificial function $D(k)$ given by the formula $(2+\cos(\pi\log_2 k)/10)^k$. What was unnatural about it?

One answer is that it involved various constants such as π and 10. One could argue that π relates so closely to the cosine function that it should not be regarded as problematic. (In fact, the *absence* of π here could be argued to be less natural than its presence.) But that still leaves 10, and the cosine function itself is also somewhat rigid because it does not relate in any obvious way to the graph-theoretic problem at hand, so has to be thought of as "that function".

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Recall that we know that $R(k,k)$ lies between $(\sqrt{2})^k$ and 4^k, or equivalently that $R(k,k)^{1/k}$ lies between $\sqrt{2}$ and 4. The simplest functions that are bounded above and below are constant functions, so the most natural hypothesis, given that small values show that $R(k,k)^{1/k}$ is not in fact constant, is that $R(k,k)$ at least *converges* to a constant. (Applying the nice-functions principle, I then conclude that this hypothesis is very likely to be true.) One could go further and argue that the most natural candidates for the value of C are $\sqrt{2}$ and 4, which would be saying that at least one of the arguments we have found so far cannot be substantially improved. The next most natural candidate is probably 2, since C has to be bigger than 1 and 2 is the simplest such number. (There are also reasons more connected with the problem itself to think that 2 might conceivably be correct.)

§ 7. — Conclusion.

The argument I have put forward can be summarized as follows.

1. Mathematicians update their beliefs using something like Bayes's formula, but with vague notions such as "probable" or "extremely unlikely" replacing actual probabilities.
2. The prior distributions and conditional probabilities are best thought of not as probabilities that certain statements are true, but as probabilities that certain kinds of arguments exist, which we estimate based on our experience with finding or not finding arguments of a similar kind in related contexts.
3. Those probabilities relate to our confidence in the original statements because of the no-miracle principle: miracles in mathematics have explanations, so if we have no evidence of a certain type of miracle existing and cannot conceive of what an explanation might be like if it did exist, then we become confident that it does not exist.
4. Miracles are situations where an event E with a natural description occurs, but according to our prior distribution E is very unlikely to occur. (Thus, whether or not E is miraculous depends heavily on our state of knowledge. In particular, as soon as an explanation is discovered it ceases to be a miracle.)
5. A description of an event is more natural the more abstract and less "rigid" it is. (This too can change as our state of knowledge changes, as additional knowledge can help us reformulate descriptions in more abstract ways.)

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There is plenty more one could say about all of these points, but I shall instead end with one more question. I stated the no-miracle principle and the nice-formula principle, which play a very useful guiding role for mathematicians when they are doing research, but gave no justification for either. That is because I find them rather mysterious. Why should it be that mathematics is so full of interesting patterns and that these patterns so often have explanations, some of which are remarkably deep and lead to whole new areas of study? There seems to be something extraordinarily "productive" about the particular set of axioms and deduction rules that we allow ourselves. With a nod to Eugene Wigner, one might call this the unreasonable coherence of mathematics, a miraculous-seeming phenomenon that itself demands an explanation.

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Timothy Gowers,
Collège de France and the University of Cambridge.

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Ways of Thinking in Mathematics

Javier de Lorenzo

Abstract. All mathematicians, whether they are engaged in teaching, in researching or in both at the same time, must master specific ways of thinking, of reasoning. These are ways of thinking that can be divided into three categories: reasoning and problem-solving; organizing already acquired knowledge; and characterizing, constructing or defining new elements and concepts. These are the modes that are presented in this essay, with a very brief indication of the historical moment in which they are constructed. The obligatory mastery of these modes of thinking, by itself, does not lead to mathematical creation; the imaginative and creative power of the mathematician is involved in this. Finally, we point out the appearance of an ontological problematic from the inversion that took place in the 19th century: are mathematical entities discovered or constructed? It is a problematic that has become a theme throughout the twentieth century about mathematical doing.

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Man

Western man, from birth to death, lives in a space built according to Euclidean metric geometry;
Man, from birth to death, lives in an arithmetical world of recurrences, counts, weights and measures;
Man, from birth to death, lives in a topological space of open and closed neighborhoods, of insides and outsides, of frontiers;
Man, from birth to death, lives in a space of structures, of groups and their relationships;
Man, from birth to death, lives in a world of statistics, of percentages, of sampling;
Man, from birth to death, is one more element of the big-data that is built by companies and governments;
Man, from birth to death...
.....
Western man, from birth to death, lives in a mathematical world.

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In the last third of the twentieth century there was a rupture in the way of thinking about mathematics. From the study of its foundations according to the formalist, intuitionist and logicist schools, mathematics came to be approached as a product of the mathematician, as a work or task within certain social frameworks. In 1972, at the zenith of a Bourbakist formalism, I devoted a paper to what I called mathematical styles. Since then, and so far in the 21st century, it has become a cliché to speak of styles, of the practice of mathematics.

From these positions a question arises: if it is stated that mathematics requires that someone practices it and, in this practice, he or she obtains a product, the mathematical content, is there a specific way of thinking, of making, in order to obtain this product? If so, what is it? I start from the conviction that every scientific discipline has its own way of making, of working, a characteristic way that must be mastered and managed by those who dedicate themselves to the cultivation of that discipline.

The aim of this essay is to briefly outline the ways of mathematics doing and its origins. With the precision that every mathematician, besides mastering these ways of thinking, has to study and know the content of his discipline with which he or she obtains what is considered to be mathematical experience. It is clear that the creative mathematician, from this experience and in order to construct

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new and beautiful mathematics, must possess a great creative imagination to pose new problems, questions and approaches, establish analogies between different fields and even provoke authentic epistemological ruptures.

This is what Abel did when he turned the question of quintics upside down, thus giving rise to the future theory of groups; or Dedekind, when he tried to construct mathematical analysis without resorting to geometry, giving rise on the one hand to the theory of sets with Cantor and on the other to the arithmetization of analysis in line with Weiertrass and Kronecker; or Grothendieck when, instead of defining an object and exploring an internal structure of that object, goes on to define the category of all interacting objects and studies the internal structure of that category. It is an epistemological inversion that implies moving from the object in itself to the multiple object through an associated representable functor. With this inversion Grothendieck constructs algebraic geometry in the categorical manner, as Fernando Zalamea has emphasized in his wonderful works.

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§ 1.

Plato, in the second mathematical passage of his dialogue *Menon*, takes for granted that geometricians reason in a specific way: by hypothesis. It is what today, with nuances slightly different from the original Greek language, we qualify as assumptions or postulates. It is the starting point of reasoning supported, implicitly, in the principles of contradiction and excluded middle, namely in the principles $\neg(A \wedge \neg A)$ and $A \vee \neg A$.

The geometrician establishes some assumptions or postulates of which he or she does not know beforehand whether they are true or not; from them, he or she reasons deductively and, having obtained a conclusion, whoever accepts the assumptions or postulates has to accept that conclusion. If he or she reaches an incorrect conclusion, he or she discards the hypothesis or hypotheses from which he or she started, adopts new ones and repeats the deductive, derivative process. This way of working can be described as trial and error.

What Plato has exposed is one of the key modes for the work of every mathematician: he imposes the condition that every mathematician must have a great capacity to raise hypotheses, assumptions, conjectures... from which to start and then maintain a rigorous derivative process.

There are several points in the above: first of all, accepting that the hypothetico-deductive mode of reasoning is that of the geometrician, of the mathematician. Moreover, if we associate the principle of contradiction to this mode of reasoning, we have one of the most powerful methods of mathematical reasoning: the reduction to the absurd; if one wants to prove A, one adopts $\neg A$ as a hypothesis; one derives with all possible rigor and arrives at a contradiction; consequently, one must accept its negation, that is to say, $\neg\neg A = A$. This is what Newton qualifies in *Principia mathematica* as "the tediousness of deducing perplexed demonstrations ad absurdum, according to the method of the ancient geometers". (Scholium to Lemma XI, Book I, section I)[(1)]. The method of the ancients is the other side of the hypothetico-deductive method: from the approach of problems, conjectures to be solved and propositions to be demonstrated, we move on to the attempt of solution or demonstration, using one of the best instruments that the mathematician has, that of reduction to the absurd.

It seems that by this method the Pythagoreans demonstrated the incommensurability of the diagonal of the pentagon with respect to the side, or the diagonal of the square with respect to its side. As an immediate problem, they must prove that $\sqrt{2}$, for example, is a number; making use of the reduction to the absurd, and by successive hypotheses, it is proved that if it is odd one arrives at contradiction; the same holds for the hypotheses of even or rational number; consequently, it is not a number and, hence, we deduce the incommensurability that goes against one of the essential Pythagorean principles; but reason imposes itself: $\sqrt{2}$ is not number, consequently there is no commensurability. However, with the diagonal of the square one can obtain a geometrical property with an accuracy that is impossible from the pure numerical: to obtain a square of an area that is double of a given one. This is what the first geometrical passage of Plato's *Menon* will show. New consequence: the Pythagoreans establish that it is possible to reason with two different approaches: in a pure geometrical way and in a pure arithmetical way, with two methods that much later Aristotle will qualify as two demonstrative genres: geometrical, arithmetical. As two different genres they are maintained until about the 16th century.

[(1)] Isaac Newton, Newton's Principia : *The mathematical principles of natural philosophy,* New York, Daniel Adee, 1846, p. 102.

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What I am interested in emphasizing is that in Greece conceptual mathematics arises related to two ways of thinking: the direct hypothetico-deductive and the reductio ad absurdum. And so, with two distinct genres: geometric and arithmetic, and a caveat: what reason imposes is accepted even if what is obtained goes against admitted principles. On the other hand, it is a way of reasoning that requires the figural diagram: one must start from the diagram, knowing that the figure — the triangle that is drawn and erased — is nothing more than a representation, a scheme, but it is indispensable for this way of reasoning.

Euclid, in the third century, turns this hypothetical-deductive method from a process or method of reasoning into an organizational process. Euclid starts from mathematical knowledge already acquired and organizes part of it for teaching in his book *Elements.* He chooses definitions, common notions and postulates for the geometrical part and gives definitions for the remaining parts of the work. Together with the diagram, he exposes, deductively, this knowledge. It is an organizing principle that in 1972 I described as geometric style. These *Elements* do not include all the mathematical knowledge known at the time, nor do they establish postulates for arithmetical reasoning.

It is an organizational approach that raises numerous problems such as, for example, whether the postulates chosen are sufficient to account for the sectoral knowledge adopted, whether these postulates are independent... On the other hand, there was a turning point started by Aristotle: scientific disciplines must be truthful and, therefore, they cannot start from postulates but from axioms, from self-evident truths. The hypothetical-deductive method becomes the axiomatic method. With this, those who follow the Aristotelian line accept an inflection: axioms cannot be denied because it would be foolish to deny the evidences. It is a line that became predominant and was even adopted by Kant.

The *Elements*, the work that Euclid composed for teaching in the third century, has been the most influential book in the Western world. Considered an example of the rigor of reasoning, of expository rigor, it has been used in many other disciplines and even in other non-scientific fields, and of course in teaching.

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§ 2.

A time jump and we reach the sixteenth and seventeenth centuries. Exceptional moments for the mathematical and scientific work that is being developed in these times. We must focus on René Descartes. Descartes creates another way of thinking and making mathematics, a way that in 1972 I called algebraic-Cartesian style. He breaks and at the same time unifies the two demonstrative genres established since the Greek world.

Without axioms, operational rules are established for the use and handling of the symbols of the alphabet, together with those of the decimal number system, in such a way that they represent geometric objects both in the plane and in space. An expression such as $3x + 4y + 5 = 0$ is that of a line in the plane, a line whose general expression is given by $ax + by + c = 0$. Here, x, y represent variables; a, b, c the associated parameters. Two expressions of this type are those of two straight lines whose relative positions will be studied according to the parameters, for the handling of which a new object, the determinant, will be constructed.

If the use of figurative diagrams — the triangle, the circumference... — were essential in the geometric style, now it is even more essential to use the first letters of the alphabet to represent parameters and the last letters for variables. It is necessary to reason on the basis of these symbolic diagrams.

An expression such as $ax + by + c = 0$, in the plane, represents a straight line. There is something more: to make that graphical representation it is written as $y = -\frac{a}{b}x - \frac{c}{b}$ and it is held that to each value of x corresponds a value of y; so the points $P(x, y)$ will be obtained in any given reference frame in the plane. The last expression goes beyond this geometric role because what it represents is a new concept, that of function, $y = f(x)$, with x as independent variable and y as dependent, in this case.

The function is a concept, an instrument conceptual that conditions a new way of thinking in both mathematical and scientific doing. This is what Newton will express in his *Principia*, where he shows, with the formulation of his laws as functions, that it is a question of mathematical doing. It is a scientific endeavor that historically has been approached as purely quantifying because it establishes as a dogma the division between subjective qualities — smell, taste, color, texture... — and objective magnitudes which are quantifiable and measurable — force, acceleration, quantity of matter or mass...

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Certainly it is a quantifiable activity, but its power goes beyond quantification because it is a functional activity. What matters are not the isolated concepts but the concepts in a network, in mathematical relations. Thus, the second law is established as the functional relation between force, mass, acceleration; what we have is a differential equation as an expression of a law of physis, of nature.

Thus, together with a way of thinking, of an algebraic making, we have the creation of another way of thinking, now a functional one. The trajectory of a mobile in the space will be nothing but the representation of a function, and that trajectory, that function, can be represented in an Euclidean metric geometric space with a given referential system. We have then two ways of thinking that are so linked to each other that sometimes we use the algebraic in the functional and vice versa.

The functional mode of thinking, on the other hand, is initially elaborated on the basis of another geometrical image: that of indivisibles. The surface that determines a curve in the plane is considered to be constituted by infinitesimal rectangles, increasingly of smaller area for a greater approximation to the given surface. With this idea, and that of rotation with respect to the abscissa axis, Blaise Pascal will outline the definite integral calculus, giving even the rules of integration by parts and by change of variable.

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Immediately, Leibniz makes an epistemological inversion by studying Pascal's writings and goes on to construct the differential calculus as the inverse of the integral. He does so with a writing that breaks with the style of indivisibles used by Pascal and Newton. Leibniz imposes the writing that we follow today for the infinitesimal analysis and that will have in the work of L'Hôpital the first reference book.

Handling the style of indivisibles, but in the language of fluxions for differential calculus, Newton, from his laws and definitions, will approach the motion of bodies in Euclidean geometric space as represented by functions, and for its study he will handle the serial development of these functions with a given remainder.

The sixteenth and seventeenth centuries were exceptional times. In addition to the ways of thinking coming from Greece, Blaise Pascal produces another specifically mathematical mechanism. In his *Treatises on Arithmetic,* in order to demonstrate properties of the sums of the series appearing in his arithmetical triangle, Pascal will handle, for the first time in history, the method of complete induction. This principle of complete induction is, for some later

mathematician, the authentic way of thinking, the most specific, of making mathematics.

There is one more element: in an epistolary exchange, Pascal and Fermat give way to the Calculus of Probabilities. Immediately, the *Ars conjectandi* is published, the use of statistics appears... Another way of approaching knowledge emerged.

From now on, the algebraic and functional modes of reasoning, the method of complete induction, the principles of the calculus of probabilities and statistics are added to the hypothetico-deductive method and to the method of reductio ad absurdum. In his or her starting point, in the search for assumptions or hypotheses, in the search for conjectures, the mathematician must now add the search for relations and functions between magnitudes that can be quantifiable or have a high degree of probability.

In this task, the mathematician has two conditioning factors: he or she must start from the diagram, whether figurative or symbolic, in order to carry out his or her work. In addition, and from his or her starting point, he or she will have a singular, concrete object, such as studying the behavior of a specific function, solving an algebraic or differential equation, trying to solve one of the millennium problems, studying a Fourier transform... It is a way of thinking or making, of working, that I have described elsewhere as figurative making.

I must observe that the mathematician, from his or her very starting point, no longer has a more or less delimited or unique field: from these moments his or her work will split into different fields. This division will become, in some moments, deeper in order to give way to different styles, to different ways of making mathematics.

§ 3.

The previous way of making mathematics was radically criticized during the 19th century. Richard Dedekind observes that in his lectures on differential and integral calculus, he resorts to graphical representation, an instrument that proves to be indispensable. Dedekind wonders if, in fact, this representation is so essential. His answer, as we all know, is that we must dispense with this representation: we must go to an expression proper to Analysis.

In this answer Dedekind provokes a profound epistemological rupture, which Weiertrass and Kronecker will also make on their side: the elimination of the geometric image leads to a process that

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has been called the arithmetization of analysis, but which is deeper than the simple process. In his work to characterize real numbers Dedekind introduces the cuts, the classes or sets of rational numbers. A process that, together with others coming also from the study of functions, leads Dedekind, Paul du Bois-Reymond... and, above all, Cantor to elaborate the theory of sets.

In these works, a new way of thinking is produced, which is no longer figurative, but global. Now it is necessary to start from sets, classes, extensions of concepts... and not from concrete objects as in previous ways of thinking. It is a global way of thinking in which objects become structures built on sets or classes. For some, it implies moving from using real numbers to thinking about structures.

On the other hand, another turning point takes place in terms of the mathematical content itself: by trying to clarify and define the concepts of Analysis without calling upon the geometric figurative image, a question of foundation arises. Mathematicians move on from attempting to found the Analysis to the attempt to found all mathematical work with a break with respect to the previous work; we no longer have recourse to the geometric figurative diagram, although we do have recourse to the symbolic diagram. In an intrinsic work of his own, some mathematicians stop at the natural number, like Kronecker, but others go further and look for a foundation for the natural numbers themselves. Frege thought he found that foundation in logic and in the extension of concepts, which is nothing other than logic and set theory. A new formal or mathematical logic that ends up becoming a new object of work for the mathematician. For his part, Cantor and his followers will try to find this foundation in set theory.

Both attempts end up in paradoxes or associated difficulties but, in any case, the question of foundations, in these first moments, is the subject and work of mathematicians.

But the mathematician, besides discussing and elaborating these processes of arithmetization, formal logic, set theory, and organizing this knowledge in logical or conjunctive axiomatized theories, is also concerned with the construction of new fields of work – topology, algebra... — in which different approaches, ways of doing things and styles will be manifested. All these fields are supported by a new way of making: a global way of making where what matters is not the set itself, but the set with a given structure together with the morphisms, the links between these structures. It is a way

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of making that also requires a new ideographic language: it is necessary to specify, with clarity, the membership of an element to a set, the existential and universal quantifiers, the conjunction and disjunction operators, the initial and final sets...

§ 4.

The ways of structuring a set imply new ways of characterizing, of defining objects, because in the epistemological inversion produced, not only new ways of reasoning are created, but also new ways of characterizing what is to be handled. The traditional modes of definition are the *nominal* or stipulative, which gives a name to what exists or is constructed; the *descriptive* or dictionary definition, which, in general, is never complete and can even be circular; and the definition considered as the authentic definition, which is the *essential* one: the one that establishes both the genus and the specific difference of the object defined by what, from the traditional logic, is intended to characterize the essence of that object. These are the definitions accepted in traditional logic, the one canonized since Aristotle and scholasticism.

But I insist that we no longer start from the singular entity, for which these types of definition are valid; now we start from sets or classes, and this forces us to construct new ways of defining or constructing the new mathematical entities. Thus, together with these characterizations, the following will be established in the new mathematical practice:

By *abstraction:* Given a set or class A, a relation $\mathscr{R}$ of equivalence is defined between its elements; this relation causes a partition of the set in equivalence classes $A/\mathscr{R}$ where each class is considered a new object. It is a mechanism by which Gauss had already established his congruence classes. It is the process that, for example, will be used to construct, from the set theory as a basis, the natural numbers, from the natural numbers, the integers, from these, the rational numbers and so on...

Definition by abstraction is a mechanism that involves the formation of new objects and structures, of new concepts. A mechanism that, from a global point of view, is linked to the notion of function: to every function is associated not only an equation, but also an equivalence relation in the domain of that function: $x\mathscr{R}y$ if and only if $f(x) = f(y)$.

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On the other hand, it could be considered as the proper mechanism for the elaboration of classificatory concepts and, thus, as a formal justification for the process considered as abstraction. Of course, it poses a problem for some realist philosopher: these objects are not singular entities but sets, classes; even more, classes of classes of classes...

By *recursion:* it is a key mechanism as an element of the definition for arithmetic where the operations of addition and multiplication are defined by recursion and their properties are proved by complete induction. I would refer to the work of Peano in his formalization of arithmetic, to Poincaré's work on mathematical reasoning...

By *postulates, implicit or axiomatic definition*: together with the organizational character established by Euclid for geometry, the hypothetico-deductive method becomes, from the 19th century onwards, and after the discussion of the role of axioms in the different geometries, a constructive method of concepts. The discussion of axioms in their role of conventions or disguised definitions makes them end up being approached as constitutive and, at the same time, regulative elements of some fields of play. Thus, to characterize a structure as that of a group is to give a set and, in it, an operation that satisfies certain conditions, postulates or axioms; from them, so to speak, begins the game of deriving properties in that field of play. The group structure has been characterized by means of an axiomatic definition; the equivalence relation that is imposed in the definition by abstraction is characterized according to some conditions or postulates, to an implicit definition.

It is a method of construction of structures, of concepts that has become one of the most important tools for mathematical work from the global approach. It is an approach that has been constituted as such since the appearance of non-Euclidean geometries in the 19th century and that has led to the discussion of the role of axioms. A discussion in which, depending on which postulates one defends, one will have affine, metric, projective geometries... This is what Hilbert will clearly show in his *Fundamentals of Geometry* with a beginning such as "Let us think of three different systems of entities..." to later establish the axioms corresponding to each of the previously established groups.

It is a line of work that moves to more algebraic fields from which these geometries will be characterized as groups of transformations, in such a way that it can be affirmed that a geometry is nothing other than a certain group of transformations. It is a process in which the algebraic approach to geometry is radicalized.

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The hypothetico-deductive method thus acquires a new dimension: it is reasoning, it is an organizational tool of already acquired knowledge, but it also becomes an instrument for the characterization of new entities, of new mathematical objects and concepts that are already geometries, groups of transformations...

§ 5.

The modes of thinking described above, structured in three sections, that of reasoning, demonstrating and solving problems, that of organizing already established fields of knowledge, and that of constructing and defining concepts, are modes of thinking proper to the mathematician. They must be handled by those who are dedicated to teaching, to educating citizens of whom some, very few, will be inclined to mathematics in the future; they must be assimilated by the creative mathematician who, in addition to learning the mathematics of his or her environment, of his or her time and circumstances, in addition to effort and work, must have a very strong creative imagination and, of course, luck in his work.

With a nuance: both the mathematician who focuses on teaching and the one who, in addition to teaching, engages in research, do so with different sensibilities: the split produced from the origin by the Pythagoreans between geometry and arithmetic reflects a very deep separation. It does not apply only to the modes of demonstration — or genres in Aristotelian terms —. There are mathematicians with a special sensitivity for the continuum — Riemann would be an example —; others for the algebraic — some members of Bourbaki would be models —; others for the arithmetical — I would cite Spanish number theorist Laureano Pérez-Cacho (1900-1957) —. There are different sensibilities, situated in social contexts that may also be different, to build, to materialize a common making for all, a common making that is mathematics.

§ 6.

The profound reversal that took place at the end of the 19th century and that led to globalization can be considered to be parallel to that of Western society. From a historical-sociological approach, it can be observed that throughout the 19th century, and as a consequence of techno-scientific advances, capitalist democracies

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developed in the Western world with their proposals for new nationalisms and states. This is the origin of political parties, trade unions, social classes... The individual loses prominence in the face of the structures of the new collectivities.

In this inversion, and in the mathematical field, in the global way of making things, the mathematicians' problem of establishing their own way of making things has arisen. A problem that did not exist until then, because formerly the starting point was the datum of the singular entity, whether arithmetic, geometric, analytical... or coming from fields such as physics, astronomy in the form of celestial mechanics... I have indicated that the classical definitions were valid for this approach. However, the inversion makes the mathematician have as a starting point the set or class and define in it an operation, a relation or some axioms to give it a certain structure. The datum of this set also supposes the adoption of the actual infinite as a given entity; moreover, different types and scales of infinite: the actual numerable infinite, the continuous, the transfinite cardinals and ordinals...

It is the process, as I have indicated, for the construction of the natural numbers; from the set of all the pairs of NxN and with a given equivalence relation, we have the integers that are shown as classes of classes of pairs of naturals; from the integers, and by the same procedure, we obtain the rationals... On his side, when Weiertrass characterizes a function with an infinity of points of discontinuity with impossibility of graphic representation and gives way to what with Hermite are considered teratological functions, this actual infinite is also handled. The immediate question arises: do these constructions make any sense, or are they what Goya wrote in one of his engravings "The sleep of reason produces monsters"?

The mathematician is confronted, in a profound way, with the existential, ontological problem of the entities he handles. In classical logic, definition has an Adamic role: Adam gave names to already existing beings. It is the mechanism that, for example, Linnaeus used in his botanical classification: genus-species now family-species... A basically descriptive mechanism of something that does not offer, in principle, difficulties because it is given beforehand. Now it seems that by means of definitions the mathematician constructs structures that are not given, that are not found in ordinary life, just as one does not find that actual infinite that one has at the starting point. The new entities are no longer, as Aristotle intended, and with him the later tradition, abstraction of objects of

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ordinary life, they are not the abstract ideas obtained by the creative mathematician from that ordinary life.

As early as 1882, Paul du Bois-Reymond constructs his work *General Theory of Functions* as a confrontation between two positions for the construction of analysis, which he calls idealist and empiricist. But it will be Poincaré, in the early years of the twentieth century and in his harsh criticism of Russell's and, above all, Cantor's positions, who will highlight the existential, ontological problem that the new way of doing implies.

For Poincaré, the Cantorians appear to be authentic realists because for them the mathematical entities, among which the actual infinite is necessarily found, have their own independent existence, in such a way that the "geometrician does not create them, he discovers them". Because they have this existence independent of the mathematician, these entities exist even if no mathematician exists. A realistic position that holds for the mathematical world as well as for the physical, material world. In the classical, traditional way, Poincaré will suggest that the world in which these entities exist is similar to that of the ideal forms of which Plato spoke. In this way, what will be called "mathematical Platonism" is introduced, a name that is not very fortunate in terms of its historical attribution.

To the Cantorian realist position Poincaré will oppose those he calls the Pragmatists. They are those who only admit objects and entities that can be defined by a finite number of words. For this reason, they consider that a (mathematical) object exists only when it is thought, and it would not be possible to conceive of an object thought independently of a subject that thinks it. Poincaré will affirm that this position is that of an idealist. As opposed to the Cantorians, with their existential realism, the pragmatist admits that the thinking subject is a human being — "or something resembling a human being", Poincaré will ironize — who is a finite being, and therefore "infinite can have no other meaning than the possibility of creating as many finite objects as one wishes".

In fact, Poincaré's criticisms go in parallel with the appearance of paradoxes both in set theory and in Frege's logicism, which give rise to various attempts to overcome them. What I am interested in emphasizing is that in Poincaré's critique two elements are raised that intermingle in these attempts to overcome the paradoxes, in the subsequent attempts to clarify the difficulties and in the mathematical work itself. On the one hand, there is an epistemological

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question, which is centered on determining which are the proper modes of mathematical thinking, which are the modes with which he carries out his work and with which he obtains a certain mathematical knowledge; it is the question that Poincaré would point out when he indicates that mathematical entities are those that can be obtained by definition by a finite number of words, those obtained by finite methods, since man is a finite being.

On the other hand, there is the purely existential, ontological question, which focuses on the type of existence of mathematical objects and entities, whether or not this existence is independent of the subject who thinks and handles them, whether or not they are independent of the mathematician; in the latter case, if this independence is admitted, the epistemological problem arises here: how this independent world is reached...

They are two intermingled questions because from the epistemological approach, by saying that mathematical entities are obtained through a certain process, one is affirming, at the same time, the constructive, existential role of the mathematician: one gets to know what one is constructing... With an added problem: among other things Poincaré will recall a very classical dictum: definition does not imply the existence of what is defined. And this will force to establish conditions to assure the existence that the definition implies. Conditions that go from obtaining the data of a model to trying to demonstrate consistency for the defined. These are issues that have been intermingled throughout the twentieth century in the different positions that have been established to overcome the initial paradoxes.

With Poincaré and in line with what I have maintained since 1972, I consider that mathematics involves the work of a thinking being, the mathematician. It is a work or making by which he or she obtains a product, mathematical knowledge, in a given historical context and circumstances. For his or her work he or she requires a previous mathematical experience achieved through an apprenticeship, although, with the French mathematician, "the mathematician is born, not made".

In his or her construction, the mathematician has to carry out a work, sometimes very hard work, to end up obtaining the creative idea. In this apprenticeship, with the experience that it entails, the mathematician learns to handle some regulating elements such as the ways of thinking presented here, or the actual infinite.

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In any case, and from the sensitivities of each mathematician, he or she has to start from that mathematical experience. This experience is obligatory in order to be able to carry out his or her work, be it teaching, solving a problem, obtaining a new theorem or making an epistemological turn with the construction of a new approach and new contents... but always in order to make mathematics.

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Javier de Lorenzo, Catedrático Emérito
de la Universidad de Valladolid.

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Logic, Mathematics, and Philosophy

Danielle Macbeth

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As is common in political science, physics, and many other disciplines in relation to the relevant subfields of philosophy (political theory, philosophy of physics and so on), the more reflective parts of mathematics overlap considerably with the subfield of philosophy that is concerned with mathematics, with the philosophy of mathematics. It can seem, then, that the relationship between philosophy and mathematics is the same as that between philosophy and other academic domains, the subfield of the one essentially like the very theoretical, abstract, and foundational end of the other. The discipline of logic in relation to mathematics and philosophy provides a different perspective. Although logic was traditionally a topic for philosophers, it is now also a field of mathematics: mathematical logic is a study of patterns of reasoning that uses (or at least can use) distinctively mathematical methods. Such study does not exhaust the purview of logic, but even logic more broadly conceived has a special relationship to mathematics. From its first beginnings in Aristotle and still today, logic looks to mathematics as a paradigm of human reasoning.

History reveals further connections. Not only is logic transformed over the course of history, and this in three stages, beginning with Aristotle's term logic, through Kant's quantificational logic, to Frege's *Begriffsschrift* or concept-script; the history and practice of mathematics likewise has progressed in three stages, beginning with ancient Greek diagrammatic practice, through the practice of constructive algebraic problem solving made possible by the symbolic language Descartes introduced in 1637, to the sort of deductive reasoning from defined concepts that has been the norm since the nineteenth century. And in every case, it is mathematics that leads the way: profound transformations in mathematical practice ground and catalyze equally profound transformations in logic.

Those developments in logic enable in turn radically new forms of philosophical practice. As I show in *Realizing Reason*, Descartes' new mathematical practice with symbols provides the foundation not only for Newton's transformations in fundamental physics but also for Kant's radically new critical philosophy, at the heart of which were profound advances in logic. Similarly, the image we inherit from Kant of logic as a purely formal discipline without content or truth is superseded with developments in logic due to Frege following developments in mathematical practice over the course of the nineteenth century. These logical developments due to Frege, although misunderstood for almost the whole of the twentieth century, ground in turn advances in core areas of philosophy. And they hold out promise of advancing work in mathematics as well. It is this history that is the focus here, in particular, the logical developments that follow the two revolutions in mathematical practice, the first in the seventeenth century and the second in the nineteenth century, in light of the philosophical problem of how knowledge in mathematics is possible at all—developments that have significant ramifications, in turn, for mathematical practice. The movement we trace, then, is from mathematics to logic, to philosophy, and then back again to mathematics, first, in the seventeenth and eighteenth centuries, and then again in the nineteenth century until today.[(1)]

Ancient Greek mathematics is a science of objects, geometric shapes such as circles and spheres, and numbers conceived as collections of units. The philosophical problem is to understand, first, the nature of these objects, and also how it is that we are able to discover the timeless necessary truths about them that we do in mathematics. Plato had a nice account of mathematical objects: they are purely intelligible entities that exist outside the ever-changing realm of becoming of everyday life. His problem was to account for our cognitive access to such objects. Aristotle denied that there are any such objects; according to him, there are only everyday perceptible objects that are regarded in a peculiar, distinctively mathematical way in the mathematician's practice. What Aristotle could not explain was the distinctive character of mathematical truth, the fact that it is, unlike empirical truth, strictly necessary and a priori. Plato had a nice account of the metaphysics of mathematics but one that made the epistemology wholly intractable. Aristotle had instead a plausible epistemology, but no

[(1)] Further elaboration and defense of many of the themes outlined here can be found in my *Realizing Reason*.

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adequate account of mathematical truth. This problem of truth and knowledge in mathematics, made famous for us by Paul Benacerraf in "Mathematical Truth", is as old as philosophy itself.

In 1637, the practice of mathematics was transformed with the introduction of Descartes' symbolic language within which to construct algebraically solutions to problems. What had seemed to the ancient Greeks to be truths about objects are now to be seen as directly about relations objects can stand in. Mathematics becomes the study of functions, curves in Cartesian space (another major conceptual advance) that can be expressed as equations in Descartes' newly devised written language with its two essentially different sorts of letters, those for unknowns and those for the known parameters of a problem. A century on, these two sorts of letters would come to be reflected in Kant's transformed understanding of logic as grounded in a logical distinction between referring (Kantian intuitions) and predication (Kantian concepts). Aristotle's logical notion of a term, which is at once referential, a name for things, and predicative, a means of characterizing things in categorical sentences, was now to be seen as a *confusion* of two essentially different logical functions. Because Kantian concepts are purely predicative, and because generalities—which in Aristotle's logic directly concern the objects that are referred to by the subject term—involve only concepts, Kant needed also to introduce an essentially new logical notion, that of a quantifier by which to relate the concepts in a general judgment to objects relative to which the judgment is true or false. Although only monadic, and not yet fully symbolic, Kant's logic is a quantificational logic, the first such logic in all history.[(2)]

Kant also had a nice account of truth and knowledge in the practice of mathematics—not only the diagrammatic practice of the ancient Greeks but also and most especially the constructive algebraic practices of seventeenth- and eighteenth-century mathematics—in terms of the idea of the construction of concepts in pure intuition. Mathematical truths, on Kant's account, are about empirical objects but only as to their a priori form, and we can know those truths because we construct a priori such objects in the course of our mathematical reasoning. No sooner had Kant

[(2)] That it is with Kant that central themes of quantificational logic make their first appearance in history is pointed out by Tiles (2004) in her discussion of Kant's Transcendental Logic. This is also a crucial lesson, independently arrived at and defended, of my *Realizing Reason*.

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formulated the account than mathematicians began to prove him wrong. Mathematics, at least as it came to be practiced over the course of the nineteenth century, is not grounded in pure intuition by way of the construction of concepts but directly in concepts. Mathematical truths concern (mathematical) concepts, and are known, as Bolzano illustrates in his 1817, purely conceptual, deductive proof of the intermediate-value theorem, not through constructions but through deductive reasoning from explicitly formulated definitions of concepts.

By the end of the nineteenth century, the deductive proof of theorems from defined concepts had become the norm in mathematical practice. Because this practice seemed to involve a discursive use of reason—as such a use contrasts, in Kant's philosophy, with an intuitive use through the construction of concepts, the use that Kant had held is that to which reason is put in mathematical practice—Kant's solution to the problem of truth and knowledge in mathematics came to seem utterly beside the point. The problem of truth and knowledge in mathematics had not after all been solved. Indeed, it now seemed more intractable than ever: how, by deductive reasoning from concepts, might one discover substantial mathematical truths? If Kant was right about the formality and sterility of purely logical, deductive reasoning, the idea of mathematical proof as at once ampliative and strictly deductive was simply, and obviously, incoherent. Frege determined that Kant was not right.

At the heart of standard quantificational logic, the fundamental idea of which was (again) developed by Kant, is the logical distinction of referring and predicative expressions. It is this distinction—together with the notion of a quantifier that is required for the completion of logic so conceived—that distinguishes modern logic from Aristotle's term logic. At the heart of Frege's logic is a further logical distinction. As from the perspective of quantificational logic, Aristotle conflates the logical functions of referring and predicating, so from the perspective of Frege's logic, the quantificational (Kantian) distinction of referring and predicative expressions conflates two logically different distinctions, that of *Sinn* and *Bedeutung* with that of (Fregean) concept and (Fregean) object. The Kantian conception of a concept confuses the notion of a Fregean concept, understood as a function mapping arguments onto truth values, with that of cognitive significance, that is, Fregean sense through which one thinks anything objective at all.

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Pace Kant, it is not through concepts, predicates of possible judgments, that objects are thought, but instead through Fregean senses. And the Kantian notion of an object similarly confuses objectivity with relation to an object. Although in Kant's logic, all content and all truth lie in relations to objects, so that without relation to any object there is simply no content, no truth, this, Frege shows, is wrong. Concepts, the *Bedeutungen* of predicates, are perfectly objective despite not being objects. What the mathematician discovers, on Frege's account of mathematical practice, are necessary relations among mathematical concepts. Truth, at least in mathematics, does not lie in relation to objects. There are no mathematical objects.[3]

Frege claims in his 1884 *Grundlagen*, section 91, that he had shown in Part III of his 1879 logic how a strictly deductive proof can be ampliative, that his proof of theorem 133 in *Begriffsschrift*, which is strictly deductive from explicitly defined concepts, extends our knowledge. As Frege (1884, 104) puts it, "from this proof it can be seen that propositions which extend our knowledge can have analytic judgements for their content." In Chapters Seven and Eight of *Realizing Reason* I aim to show in detail why we should take Frege at his word here. Fortunately, we do not need to study a long *Begriffsschrift* proof to get the essential idea as needed for purposes here. Although it could only be discovered after millennia of inquiry together with profound mathematical and logical advances, the phenomenon in question is manifest, in retrospect, even in the very simplest mathematics, and indeed is a variant of what Kant already understood about the constructive nature of mathematical practice.

Frege's discovery of the distinction, conflated in Kant's logic, between *Sinn* or cognitive significance and concepts, on the one hand, and objects and objectivity, on the other, is a discovery about how language, most obviously and immediately mathematical language, works. It applies to anything that is properly thought of as a language, but is clearest in a specially devised written system of signs suitable for reasoning in mathematics. Frege's discovery, as applied to the case of mathematical language in particular, is that

[3] Frege himself thought that there had to be mathematical objects for numbers to be. He was wrong about that. Mathematics needs only *concepts* of numbers. And in retrospect, this seems obvious given that it is on the basis of concepts that one reasons. Nothing follows from something's being the object it is, but only from the properties it has, that is, from concepts under which it falls. Any and all reference to the object simply falls away; objects are irrelevant to mathematical practice.

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the primitive signs of the language—which in the case of mathematics is (again) a specially devised system of written signs within which to reason in mathematics—function first and foremost to express Fregean sense, *Sinn*. Such signs do not designate, at least not as such. It is only when those signs are combined in some context of use that they designate or have *Bedeutung*. Imagine, then, an absurdly simple mathematical language, a stroke language with only one primitive sign, the stroke, which is to be used in the formation of complex signs that designate numbers and in (absurdly simple) reasoning about numbers. (Although we discover over the course of the history of mathematics that mathematics has no objects, we can assume for the purposes of this very simple example that there are mathematical objects, in particular, numbers.) We begin, then, by forming complex signs for, say, the numbers seven and five using our one primitive sign: /////// /////. These two complex signs are not to be read (seen) simply as collections of units. They are *meaningful* signs of a particular *mathematical language* and are to be read as such, the first as a sign designating the number seven, that particular unitary mathematical object, and the second designating the number five, a different mathematical object, both through the display of what it is to be such numbers, namely, on the relevant conception, certain multiplicities. Because the contents of these numbers as they matter to reasoning in the system of signs are displayed in the signs that designate those numbers, we can manipulate the signs according to antecedently stated rules to discover something new. In particular, in this case, we can construct from the two original signs a sign for the number twelve, demonstrating thereby that seven plus five equals twelve, a properly mathematical result. (Notice that simply combining the strokes and counting the resultant collection would *not* constitute a piece of mathematical reasoning.) Our stroke language, simply though it may be, is at once a Leibnizian lingua and a calculus ratiocinator. Complex signs in the language display the contents of the mathematical entities those signs designate; the language is a Leibnizian lingua. And they do so in a way enabling rigorous, rule-governed reasoning on the basis of those contents *in* the system of signs; the language is at the same time a calculus for reasoning.

Reasoning in Frege's mathematical language *Begriffsschrift*, although much more sophisticated, is essentially the same, at least in this respect. Using primitive signs that in themselves only

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express Fregean senses, one constructs complex signs for mathematical concepts of interest, signs that at once designate those concepts and display their contents as they matter to inference. Then, following antecedently stated rules, one manipulates those expressions, sometimes individually, more interestingly in combinations, to yield, ultimately, some new result, one that reveals a logical relation among the concepts with which one began. As Frege himself explicitly saw, such reasoning is, or at least can be, ampliative, a real extension of our knowledge, despite being strictly deductive. And this is possible, I argue in Chapter Eight of *Realizing Reason*, because not all deduction, that is to say, necessary, truth-preserving reasoning, is *logically* deductive. Although most steps in a chain of mathematical reasoning are purely logical, some can be—as they are in Frege's proof of theorem 133 in Part III of *Begriffsschrift*—licensed by rules of inference derived from definitions, in the case of Frege's proof of theorem 133, from Frege's definition of following in a sequence. Such steps, because they are deductive without being licensed by logic alone, are ampliative. They extend our knowledge.

Frege's advances in logic enable us to understand how a purely deductive proof such as, for example, Bolzano's 1817 proof of the intermediate-value theorem, or proofs of significant theorems in abstract algebra, can be ampliative. Indeed, they enable us to distinguish between *three* different degrees of ampliativity in a mathematical proof, and this along three different dimensions.

First, we know already from Kant that judgments that are ampliative in mathematics are synthetic a priori; they are necessary but not logically necessary. What we are interested in, then, is a distinctive sort of unity in mathematics, what I call an intelligible unity to distinguish it both from an essential unity, a real whole with no real parts, and from an accidental unity, which has real parts but is not a real whole. An essential unity in this context is to be understood as something that is logically necessary, as an analytic truth is on Kant's account. Because in an analytic judgment as Kant understands it, the predicate is contained already in the concept of the subject, the judgment itself is a real whole without any real parts. To try to separate out the parts, by denying the predicate of the subject concept, is to fall into a logical contradiction. At the other end of the spectrum are accidental unities. An a posteriori judgment is such a unity insofar as it has real parts, the subject concept and predicate, but because these two components of an a posteriori

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judgment stand in no real, or necessary, relation, the judgment as a whole lacks any real unity. The two concepts, subject concept and predicate, happen to belong together in virtue of certain contingent facts about empirical objects, but they just as easily could be without any relation at all. An a posteriori judgment is an accidental unity; it has real parts but is not itself a real whole. Synthetic a priori judgments are, of course, the interesting case. Because they are a priori, that is, necessary and strictly universal, such judgments are real wholes. But they also have real parts insofar as there is no contradiction in denying the predicate of the subject concept. The two concepts in a synthetic a priori judgment are logically distinct, and yet they are necessarily related. A synthetic a priori judgment is, then, an *intelligible* unity; it is a real whole, unlike an accidental unity such as an a posteriori judgment, but it also has real parts, unlike an essential unity or analytic judgment. Now we need to apply this idea of an intelligible unity, not only to judgments, that is, to the theorems of mathematics, but also to its defined concepts, and to its chains of reasoning.

Following Frege, we distinguish, first, between fruitful and other definitions, where (as Frege thinks of it) a fruitful definition draws new boundary lines. By contrast with a definition that does not draw new lines—such as Frege's definition of belonging to a sequence, which, like the definition of less-than-or-equal-to, is merely disjunctive, merely an accidental unity of parts introducing nothing new—definitions that draw new lines involve both the conditional stroke and Frege's concavity with widest scope.[(4)] Such definitions are intelligible unities insofar as they clearly have real parts, as indicated by the primitive signs they involve, but are also real wholes. Again, in Frege's imagery, they draw new boundary lines. (Primitive signs, because they are undefined, can be thought of as essential unities; they are wholes without parts.) Proofs that combine content from two or more fruitful definitions, for instance, those that show that one such defined concept is logically related to another by subordination, will have conclusions that are themselves intelligible unities. Such conclusions are, in Kant's terminology, synthetic a priori insofar as the predicate concept is not contained in the subject concept but can be shown through the course of reasoning to be necessarily related to it. Theorems that

[(4)] Contrary to the received view, Frege's concavity is not a notational variant of the universal quantifier of standard logic, as is indicated already by the fact that it serves in the formulation of laws. See my *Frege's Logic* for details.

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merely draw out some consequence of this or that definition, even one that is fruitful, are instead analytic. Nothing new emerges in that case.

We have just seen that the signs designating concepts in a mathematical language such as Frege's *Begriffsschrift* can be divided into three sorts. Primitive signs are essential unities that designate concepts directly; they have no real parts. Some defined signs, such as the sign for less-than-or-equal-to, function merely as abbreviations; they are accidental unities that introduce nothing new. The last and most interesting signs are those for concepts that have fruitful definitions, definitions that draw new lines. Fruitful definitions reveal the concepts they define as intelligible unities, real wholes of real parts: the concepts are definable but they are nonetheless significant concepts in their own right. They are not reducible to their parts in combination. Concepts that have fruitful definitions thus have a kind of unity and integrity that is lacking in concepts designated by mere abbreviations for complexes of signs. They constitute the subject matter of significant domains of mathematics and are the material basis for significant mathematical theorems.

Among mathematical signs, primitive and defined, there are essential unities, namely, primitive signs, accidental unities, defined signs that function merely as abbreviations, and intelligible unities, defined signs for the concepts about which one reasons in mathematics. And as we know, mathematical theorems likewise can be divided into sorts. As most would agree, there are no merely accidentally unified judgments in mathematics, no a posteriori judgments; all truths of mathematics are necessary. But among the necessary truths of mathematics there are both analytic judgments, that is, essential unities, the negations of which are logical contradictions, and synthetic a priori judgments, that is, intelligible unities, real wholes of real parts, judgments that are necessary but not logically necessary. Now we need to think about different sorts of mathematical reasoning, differences among chains of reasoning in mathematical proof.

First and most obviously, no mathematical chain of reasoning is accidental. Mathematical reasoning—by contrast with, say, reasoning (inductively) from cases to a generality that may or may not in fact be true—is necessary reasoning: if the premises are true then the conclusion must be true as well. And there are clearly proofs, chains of reasoning in mathematics that are essential unities, that are logically necessary throughout. In such a proof, each

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step is licensed by a rule of logic; and because it is, the conclusion is analytically contained in the definitions with which one begins, even in the case in which the conclusion itself is a synthetic a priori judgment, that is to say, an intelligible unity. But there are also mathematical proofs, chains of mathematical reasoning, that seem clearly to be not analytic in this way but are, it seems, ampliative, real extensions of our knowledge. (Again, Frege claimed that his proof of theorem 133 in Part III of *Begriffsschrift* is such a chain of mathematical reasoning.) And if there are such chains of reasoning in mathematics, they must have steps that are necessary but not logically necessary, not instances of purely logical inferences. Such steps must be instead materially valid, valid in light of the meanings of the non-logical concepts involved.[(5)]

Very often in mathematics, deductive proofs of theorems, that is, not only the conclusions but the chains of reasoning, seem clearly to constitute real extensions of our knowledge. We are trying to understand how they do; and for this, we need a distinction that Frege did not draw, though it is evident in his inferential practice. We need to distinguish between those fruitful definitions that enable proofs of theorems that are synthetic a priori but involve only purely logical reasoning, and those fruitful definitions that enable proofs not only of theorems that are synthetic a priori but that are themselves ampliative because involving those definitions *in* the steps of reasoning. In these latter sorts of cases, definitions provide not only premises from which to reason but also principles according to which to reason. The reasoning in such cases is deductive, necessary, since in such cases if the premises are true then the conclusion must also be true, but the reasoning is not logically deductive, not by logic alone. The definition is essential to the reasoning insofar as, assuming we have correctly defined the relevant mathematical concept, the step of reasoning is valid *as* licensed by that definition, despite not being logically valid. Because and insofar as without that definition there is no proof, no path of strictly deductive reasoning from the definitions to the theorem, not only the definition with which one begins, and the theorem that is proved, but even the reasoning itself constitutes an intelligible unity, a real whole of real parts.[(6)]

[(5)] On the notion of material valid inference as it contrasts with logically valid inference, see Sellars (1953).

[(6)] See Chapters Seven and Eight of *Realizing Reason* for further development and defense of this claim.

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We have just seen that mathematical definitions can be fruitful (in Frege's sense) in two essentially different ways. All fruitful definitions are fruitful in themselves insofar as they draw new lines; they are real unities of parts that can provide the grounds for synthetic a priori judgments of mathematics. But some fruitful definitions also enable fruitful, that is, ampliative, steps of reasoning within proofs. They enable deductive steps of reasoning that are not strictly logical but are nonetheless necessary, strictly deductive insofar as it is impossible for the premises to be true and the conclusion false. Such a definition thus can be used to license an inference that is not otherwise available to be drawn. Frege's definition of following in a sequence is just such a definition: assuming that it gets the content of the relevant concept right, the inferences it licenses are valid, but they are not logically value; they do not follow by logic alone. Chains of reasoning that depend on such steps of inference, such as that involved in Frege's proof of theorem 133, not only start from fruitful definitions and have conclusions that are synthetic a priori; the reasoning itself is, in its way, synthetic a priori. The relationship of the conclusion to the definitions with which the reasoning begins is necessary but not logically necessary, and hence the proof as a whole is not only a real whole, as is the case in any strictly logical reasoning, but also one that has real parts precisely because although the conclusion follows, it is not contained already in the premises, needing only strictly logical inferences to be make explicit. It is contained, to borrow once again imagery from Frege in *Grundlagen* (section 88), not as beams are contained in a house, that is, implicitly, already there only not obviously so, but as the plant is contained in the seed, that is, potentially. It is the course of reason, reasoning that uses not only rules of logic but also at least one rule of inference derived from a definition, that brings that potential to actuality.

Frege was a mathematician by training and by profession. He devised his logical language *Begriffsschrift* as a symbolic language within which to reason in mathematics. The immediate aim was to prove the truth of logicism, but the language he developed was, as he knew, of more general mathematical significance. As Descartes' symbolic language was a language within which to reason in the constructive algebraic tradition of eighteenth-century mathematics so Frege's language was to be a language within which to reason in the deduction-from-concepts tradition that emerged over the course of the nineteenth century. Because Frege's language was not

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understood, it was never used. Mathematicians' proofs as currently promulgated are reported in natural language, with a smattering here and there of logical signs serving as stand-ins for words of natural language, just as they were throughout the nineteenth century, that is, before the development of our modern, symbolic, quantificational logic. Unsurprisingly, at least in retrospect, the development of quantificational logic into a full logic of relations at the turn of the twentieth century did nothing to change the way mathematicians formulate and present their reasoning. Quantificational logic, which again, is merely Kantian, is utterly inadequate as a logic of mathematics as it has come to be practiced since the nineteenth century. What is needed is Frege's logic.

Frege's logic was enabled by nineteenth-century advances in mathematics as Kant's logic was enabled by seventeenth-century advances in mathematics. And as Kant's logic provided the foundation for Kant's radically new practice in philosophy, his critical turn, so Frege's logic provides the foundation for a radically new philosophical practice. But as indicated, Kant did not only revolutionized the practice of philosophy; he also, in so doing, provided the first significant solution to the problem of truth and knowledge in mathematics. Frege's logic as providing the foundations for a new philosophical practice likewise sheds new light on this venerable problem. Indeed, Frege's advances in logic enable fundamental advances in philosophy not only with respect to the problem of truth and knowledge in mathematics but, as I aim to show in some detail in *Realizing Reason*, as concerns reasoning, thought, and knowledge more generally. And such advances have the potential to advance in turn the practice of mathematics. Once having been apprised of how mathematics functions to extend our knowledge by deductive reasoning, and of how a specially devised written mathematical language can display such reasoning, mathematicians may well discover that this image of mathematics bequeathed to us by Frege not only advances the teaching and learning of mathematics, by putting the reasoning before students' eyes just as earlier mathematical languages did, but also alters assessments of what is and is not a tractable problem in mathematics. The great benefit of Frege's notation, to one who knows how properly to read it, is the way it enables setting out in a two-dimensional array the contents of mathematical concepts as they matter to inference. Who knows what new mathematics may be enabled by mathematicians being

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able finally to see, literally to see, the contents of the concepts with which they are concerned?

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Danielle Macbeth, Professor and Chair
of Philosophy, Haverford College

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A Philosophical Task in Our Times

David Rabouin

My rather grandiose title will have no other excuse than that it has been extracted from an article by Ian Hacking on the idea of styles of scientific reasoning[(1)]. According to Hacking, his reflection on style was part of a general problem that went far beyond the philosophy of science and that he did not hesitate to call "a philosophical task in our times". This task consisted, first of all, in making room for two apparently incompatible orientations in our vision of scientific knowledge. The first has emerged, since the 1960s, with the development of *Science Studies* and related approaches. It gives prominence to micro-historical events, controversies, local contexts (sociological, political, institutional, etc.), and "minor" authors. On the contrary, the other more classical approach underlines the strongly cumulative aspect of scientific knowledge, in a regime of historicity that Hacking qualified as "Braudelian". It is interested above all in the persistence, notwithstanding endless local variations, of global conceptual structures. Attempting to reconcile both approaches seemed to require, in Hacking's eyes, a revival of nothing less than "metaphysics" — by which he meant the revival of interest in this discipline that occurred in so-called "analytic" philosophy after Quine, and in particular from the work of Putnam and Kripke. To put it more simply, it was a matter of bringing together important questions about the status of "truth" and "reality" with the apparently contradictory images of science that emerge from the two previous orientations. The difficulty, Hacking emphasized, is not that we have to choose between one orientation or the other, as some have believed and as many still believe, but rather that they both appear to be correct even though we do not have a theory of knowledge that can accommodate them. The reference to Putnam was then natural since it

[(1)] I. Hacking, "Statistical language, statistical truth and statistical reason", in E. McMullen (ed.), *Social Dimensions of Sciences*, University Notre-Dame Press, 1991, pp. 130–157.

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was precisely one of the problems he had wanted to raise, under the heading of the question of the stability of reference — a question that emerges when we note both the cumulative character of scientific knowledge and the local variations to which its expression is subject.

Whatever one thinks of the fact that this is one of the philosophical tasks in our times, one can agree that there is a central problem that arises today in our image of the world. Moreover, it plays an important role in the difficult dialogue between history and philosophy of sciences[(2)]. However, this question has scarcely reached our vision of mathematics, where it is nevertheless particularly salient. In fact, if there is any knowledge that appears to us as strongly cumulative, it is indeed that of mathematicians. Whatever the legitimate mistrust we may have towards the idea of "progress", it seems difficult not to concede that we recognize some results found in Archimedes, Li Zhi (Li Ye) or Nilakantha Somayaji as "true" (perhaps at the cost of misunderstandings, it does not matter). We think, on the other hand, that we know "more" in mathematics than they did. At the same time, "local" approaches to mathematics have gained considerable momentum in recent decades. Even when they do not recognize themselves in the picture drawn by *Science studies*, even when they strongly claim to belong to "conceptual" history, most historical studies now set themselves the task of reconstructing the *specific way* in which mathematical rationality was able to unfold in such and such a context. This specificity is often expressed by difference, i.e. we see more and more clearly that while Euclid, Al-Haytham, Liu Hui, Baskhara I, Leibniz, Lagrange, Kronecker and Banach, put forward truths that we recognize as ours, they did not think them exactly as we do.

But there is more, and it is at this point that the question becomes more than simply methodological. Indeed, the variability of contexts brings with it the variability of descriptions and opens up a question for history as well as for philosophy: what does mathematics talk about in the variability of its historical and geographical forms? Can I be satisfied with the fact that Euclid and Grothendieck use comparable terms (translatable into each other) to designate natural numbers, and conclude that they are dealing with the "same" object? This is the question of reference and more precisely of its stability across historically and culturally situated expressions.

[(2)]See problems 7 and 8 ("Locality and globality") in Peter Galison "Ten Problems in History and Philosophy of Science", *Isis* 99 (2008), pp. 111–124.

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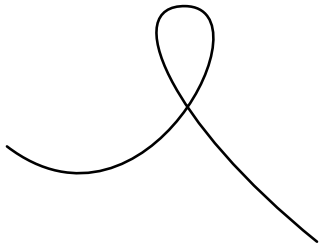
Now, whereas the solution to this problem envisaged by Putnam and Kripke consisted in grounding reference independently of descriptions (always revisable and variable) and anchoring it in an "external" reality, with which we interact causally (what has been called "semantic externalism"), this solution seems to be forbidden — or, at least, very costly — in mathematics. Except in some extreme forms of Platonism where the intuition of ideal objects is compared to a form of "perception", philosophers tend to agree on the idea that mathematical objects are not with us in forms of causal interactions. My aim in this article will be to convince the reader that this is a central, though neglected, problem in the dialogue between philosophy and mathematics, and then to sketch out a few ways to answer it. I will limit myself, for lack of space, to an essentially programmatic approach.

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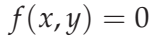
§ 1. — Position of the problem.

In order to make the problem from which I would like to start tangible, I will begin with an exchange between Claude Chevalley and Oscar Zariski reported by Serge Lang:

> Chevalley and Zariski (one of the luminaries, along with Weil, Serre and Grothendieck, in the revival of algebraic geometry) were having a discussion about curves, and neither seemed to understand the other. In desperation, Chevalley asked Zariski, "What do you call a curve?" They were in front of a blackboard, Zariski said, "Well! For me it is this", and he drew a curve:



A curve acording to Zariski



A curve acording to Chevalley

> And he continued: "And for you, what is a curve?" Chevalley answered: "It is not at all that for me, for me it is $f(x,y)=0$" ![(3)]

(3) Marcel Berger, *Cinq siècles de mathématiques en France*, Paris, apdf, 2005, p. 179 (my translation).

This dialogue will perhaps surprise an outside observer. Two mathematicians, who had a major influence on the development of their discipline, who used to read each other's papers and exchange ideas, finally realize that they do not understand each other. More intriguingly perhaps, it is not a discussion about a particularly complex object, some higher homotopy group residing at the outer reaches of their knowledge, but about the seemingly simplest and most common idea in geometry, that of a curve. Yet mathematicians know that this situation is common — even if their practices of communication tend to banish it from public exchanges. It is true that this is the kind of disagreement that one prefers not to bring to the forefront, especially in a science that wants to be marked by "certainty". This does not make the dialogue with philosophers and historians any easier: mathematicians sometimes seem to suffer from a curious schizophrenia when they turn to the outside world and suddenly forget all the misconceptions, differences of opinion, and incomprehension that they had in the corridors a few moments before. This situation is not new. If we open Euclid or Archimedes, we are struck by the fact that their treatises do not contain any elements of reflection on their objects and methods, let alone on polemics. However, we know from almost contemporary testimonies that mathematics was already full of controversies about the nature of objects, the strength of this or that proof, and the legitimacy of this or that approach[(4)]. This phenomenon is even more evident for periods and cultural contexts, such as the Renaissance and the European Early Modern period, where the regime of controversies and discussions was an integral part of scholarly sociability.

Note that the debates reported by Proclus and Eudemus are not quarrels instigated by philosophers ignorant of mathematics. They involve actors as important as Menelaus and Geminus, Euclid and Apollonius. The early modern age provides us, for the reasons I mentioned, with many other examples, from the quarrels between Fermat and Descartes over their respective methods of tangents to those of Pelletier, Viète and Clavius over the angle of contact, not to

[(4)] An interesting account of some of these controversies can be found in Proclus' commentary on the first book of Euclid's *Elements* (*Procli Diadochi in primum Euclidis elementorum librum commentarii ex recognitione Godofredi Friedlein*, Leipzig, Teubner, 1873 [henceforth *In Eucl.*]). Although Proclus lived in the 5th century CE, he claims to depend for part of his sources on Eudemus of Rhodes, a disciple of Aristotle who lived in the 4th century BCE.

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mention the clash over the methods of "indivisibles", and then over infinitesimals. Closer to us, the controversy between Kronecker and Jordan on the reduction of bilinear forms, Weierstrass' mockery of Riemann's "geometrical fantasies", Poincaré's anathema of set topology, Severi's disputes with the younger generation of algebraic geometers, Arnold's invectives against the Bourbaki style, to say nothing of the various "foundational" quarrels and the ongoing debates on the validity of the "inter-universal Teichmüller theory", etc. , should remind us that these discussions are, in fact, constant.

This is a field that has been of particular interest to the recent history of mathematics because it shows, if indeed it were necessary, that mathematics is no less subject to misunderstandings and disagreements than other disciplines. The cases we have mentioned can be described, at first glance, by the fact that the actors belong to different contexts (choose here the category that suits you best: tradition, style, school, paradigm, epistemic configuration, etc.). The phenomenon that emerges is then, according to the first branch of the problem raised by Hacking, that knowledge is strongly context-dependent, to the point that actors may not mean the same thing by the same term. This is the pole of instability of reference that all history of knowledge seems to manifest and from which mathematics does not seem to be able to escape.

Note that for that reason, it would be wrong to reduce the phenomenon in question to that of controversies, which form only a very particular case. Between Chevalley and Zariski, for example, even if one can relate them both to different "styles", there is no controversy. More generally, it is not difficult to find an equivalent of this type of exchange marked by misunderstanding or disagreement among actors belonging to the same school/tradition/epistemic configuration. The quarrel between Leibniz and Bernoulli over the logarithms of negative numbers is a case in point — all the more interesting, as Euler pointed out, because both had perfectly fair arguments. I will mention another, taken from the same context. When the quarrel between the supporters of differential calculus and the supporters of the "old style" (as Varignon called them) broke out in the Royal Academy of Sciences, the defenders of Leibnizian formalism naturally turned to their master for support. Here is the way the latter reports this episode:

> When they were disputing in France with the Abbé Gallois, Father Gouye and others, I told them that I did

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> not believe that there were truly infinite or truly infinitesimal magnitudes; that they were only fictions, but useful fictions, in order to abbreviate or to speak universally, like the imaginary roots of algebra $(\sqrt{-1})$... But as the Marquis de l'Hôpital thought that by this I was betraying the cause, they begged me to say nothing about it, except what I already had said in the Leipzig *Acta*, and it was easy for me to comply with their request. (à Pierre Dangicourt 11 Septembre, 1716, Dutens III, 500–501)

This situation is not at all exceptional. To take up examples we have already seen, Proclus, while arguing that Euclid had formulated his demonstrations in the best possible way, points out that some proofs would better express the reasons for the fact under consideration if they were expressed by means of motion, an approach that is however avoided in his master's treatise[(5)]. Van Schooten, the great promoter and editor of Descartes' mathematics presented his master's famous method of tangents on the same footing as that of... Fermat![(6)] Closer to us, Hermann Weyl, while claiming a Riemannian heritage in complex analysis, had no difficulty in integrating Weierstrass' approach, which Felix Klein had nevertheless presented as belonging to an irreducibly different "style"[(7)]. What is obviously troubling in these exchanges is that the actors sometimes seem to disagree not only on major epistemic values that could always be claimed to be external to their practice, but on the very characterization of the objects and the methods of reaching them[(8)].

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[(5)] *In Euclidem* 69. See Orna Harari (2008), "Proclus' account of explanatory demonstrations in mathematics and its context", *Archiv für Geschichte der Philosophie* 90 (2), pp. 137–164.

[(6)] *Geometria à Renato Descartes anno 1637 Gallice edita* [...]. *Operâ atque Studio Francisci a Schooten*, Amsterdam, Elsevier, 1659, pp. 252–253. On the method of normals in Descartes and his quarrel with Fermat, see V. Jullien, *Descartes. La 'Géométrie' de 1637*, Paris, PUF, 1996, pp. 101 sq.

[(7)] See H. Weyl, *Die Idee der riemannschen Fläche*, Berlin, Teubner, 1913 and *Oeuvres mathematiques de Riemann* trans. by L. Laugel; with a pref. by M. Hermite; and a discourse by M. F. Klein, Paris, Gauthier-Villars, 1898.

[(8)] With a few exceptions to which I will return, it is very significant that the so-called "philosophy of mathematical practice" has massively focused on the question of values: purity, fecundity, explanatory and aesthetic character of proofs, see Paolo Mancosu (ed.), *The Philosophy of Mathematical Practice*, Oxford, OUP, 2008.

§ 2. — The question of reference.

This problem is particularly important because it gives us a synchronic view of a phenomenon that unfolds diachronically as *the very fact of the history of science*. That science can evolve is indeed marked by the fact that the descriptions that one can give of the same object can change, that it advances, as Cavaillès said, "by deepening and erasing"[(9)]. When it is a question of proposing a more precise or better suited description of the objects, this does not really pose a problem. But the same cannot be said for correcting a description in a way that is not a simple improvement. In this case, in fact, a property that we thought belonged to an object will be rejected (or there will be no "correction") and the two descriptions, the old and the new, will therefore be incompatible.

It is this phenomenon that Putnam pointed out in order to contest the idea (of Russellian origin) according to which we access objects through definite descriptions, i.e. a set of statements that can be formulated with the help of a list of predicates and that, when the description is complete, would suffice to determine "what we are talking about"[(10)]. Among the arguments that he mobilized were famous cases of evolution of our scientific concepts, such as the different theories of the electron that followed one another from Rutherford to Schrödinger. We know, in fact, that the "planetary" model sees the electron as a particle endowed with a position and a velocity determined at each instant, while the "cloud" model sees only orbitals (regions determined by a probability of presence of the particle) and reject the idea that one can determine both the velocity and the position. These two descriptions are incompatible: either the electron has a determined position or it does not. The second interpretation is not simply a way of specifying the first. But supposing we accept the latter, how can we explain that we were "already" targeting the same object with a description *that did not correspond to it*?[(11)]

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[(9)] *Sur la logique et la théorie de la science*, Paris, Vrin, 1997, p. 78.

[(10)] Putnam's immediate target is of course Quine and his slogan that "to be is to be the value of a variable". For Quine, in fact, the philosopher has to evaluate ontological commitments on the basis of scientific discourses, all of which are assumed to be reformulated in first-order logic, and whose variable values constitute the entities we are dealing with in this or that domain.

[(11)] See, for example, H. Putnam, "Language and Reality" in *Philosophical Papers, Vol. 2: Mind, Language and Reality*, Cambridge, Cambridge University Press, pp. 272–290.

A natural solution, Putnam noted, is to accept that our ways of referring to objects are in fact *independent* of the descriptions we give them. This is ultimately rooted in interactions with real exemplars or paradigms (which can be grounded on direct or indirect perceptions, like observations on the screen of a cathode ray tube), whose extension has no reason to be perfectly fixed (our description can lead us to finally reject an exemplar as not being of the same type as the others). But this does not mean that we necessarily have to interact with scientific observations in order to be able to speak of an electron. The other crucial mechanism that Putnam was pointing to is deference to experts: we trust experts in the way they relate to different objects with which we have no interaction (except through them). In both cases, the reference is thus not fixed by the meaning of the terms alone, but by mechanisms external to the descriptions.

Before proposing some examples indicating that mathematics has no way of departing from this observation, I would like to make two more general remarks. First of all, even though the initial difficulties here come from a tradition which considers that the study of language is the gateway to philosophy, one should not believe that they depend on such a framework. Insofar as it is a question of refusing that signification can fix the reference, these difficulties immediately extend to all the traditions, of idealist or phenomenological origin, which would support comparable theses but by placing themselves at a level which would be that of thought (or of "concepts") or of consciousness. This is a point that Putnam himself has stressed, hammering home the point that meanings cannot be "in the head". Moreover, the mechanism of deference to experts is not the same as that of intersubjective agreement on which phenomenology has placed great emphasis. It is not a question of saying that my conceptions must agree with those of other subjects, but rather that I defer the explicitation of the reference of certain terms.

A second remark is that we will not be able to get away with a moderate relativism that would quickly concede that our actors do not speak of the same object when they provide incompatible descriptions of it. Contrary to the cases that have been put forward by authors such as Bachelard, Kuhn, Foucault, Lakatos, Kitcher, etc., we are not saying that the sciences evolve, from time to time, by changing major conceptual frameworks (which is *also* true). It is a question of pointing out the ubiquity of the mechanism of conceptual variability which can be located on a very small time-scale and

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sometimes in a single individual. We have already seen an example of this with Bohr, who was an important actor in the elaboration of the two incompatible conceptions of the electron that we have mentioned. There is therefore no need for several actors, the very fact of "better understanding" an object and that this better understanding leads us to revise our own judgments (and thus possibly to deny certain "essential" properties) is enough to open the question: how do we then relate to the "same" object? This is why the problem raised by Putnam is, in a sense, much more significant than the one raised by Kuhn and his successors: it is not a question of saying that different "paradigms" or "styles of thought" generate distinct objects and that incomprehension prevails between them. The phenomenon of conceptual change prevails within any framework of thought. To hold that each conception produces its own objects would be to support a regime of generalized misunderstanding, including with ourselves. There would be, in the end, only a dusting of meanings and objects, whose apparent relations of identity would never be more than illusory (a bit like the way the Humean self is constructed as a simple name for a bundle of experiences). One will certainly find some forms of Wittgensteinism ready to happily grant this pulverization of meaning into a myriad of language games, more or less well joined to each other (this is the slope towards which *Science Studies* naturally leads according to David Bloor)[(12)]. But, as Putnam points out, one wonders who can then put forward this description of meaning (which is true of *all* contexts)?[(13)] Relativism is here, as it often is, merely a hollow reproduction of the "divine point of view" that it intended to contest by arrogating to itself, it is not clear how, a point of view on *all meanings*.

But let us leave these metaphysical quarrels there and turn to mathematics which, as I have said, has remained relatively distant from these questions. The phenomena we have just encountered are however just as present in mathematics, and even more problematic, since descriptions are our privileged mode of access to mathematical objects and it does not seem that we can interact causally with an "exemplar" of a mathematical object. Let us look, for example, at the first propositions of Simon Stevin's *Arithmetique*,

[(12)] David Bloor, "Wittgenstein and Mannheim on the sociology of mathematics", *Studies in the History and Philosophy of Science*, vol. 4, n 2, 1973, pp. 173–191.

[(13)] See H. Putnam, *Realism with a Human face*, ed. By J. Conant, Cambridge, MA: Harvard University Press, 1990, chap. 1.

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one of the places where symbolic algebra was invented in the Renaissance[14]. Stevin reproduces a dialogue (perhaps fictitious) that he had with scholars of his time about what a number is, and takes issue with the fact that their definitions do not include the "unit" (in the tradition stemming from Euclid, the unit is the "principle" of numbers defined as "multiplicities of units"). Defining number by measure, he then points out that there is no need to even conceive of a number as a discrete entity (a unit being a number, there is no problem in dividing it and calling numbers rational or "broken", as well as irrational or "deaf"). More generally, he introduces a notion of "geometric number" and does not hesitate to advance, in a passage that has remained famous, "that there are no absurd, irrational, irregular, inexplicable or deaf numbers" (p. 33).

One could say that this is only a matter of *extending* the Euclidean notion in a conservative way, but this is not the case: Stevin's natural numbers are not Euclid's natural numbers, if only because the former must include *zero* and *one*. But the consequences of this first, apparently innocuous, extension are considerable. As Stevin forcefully points out, it authorizes a "community" of numbers and magnitudes that was *forbidden* for an ancient Greek mathematician (notably by making the geometric point correspond to the arithmetic zero). In fact, for Euclid, the world of numbers and the world of magnitudes behave in a closed way, and this is manifested by several surprising features in the *Elements*, such as the fact that no demonstration passes from the geometric books to the arithmetic books, or the fact that he finds himself having to formulate two theories of proportions (one for numbers and the other for magnitudes). The question is therefore not whether or not to add two elements to a set, it mobilizes different understandings of what a "number" is in depth. But how can Stevin and Euclid be said to relate to the "same" object "number"? Assuming that this is defended, a second, no less awkward question must be answered: how are they — and how are we ourselves — supposed to have access to this object *independently* of the (partly incompatible) descriptions that have just been given?

At this point, the example is so elementary that one might say that it is resolvable by some recourse to a supposedly universal structure of our mind that would give its "real" basis to all these descriptions, say "the human activity of counting" or "the pure intuition of time as succession". This hypothesis is not easy to

[14] S. Stevin, *L'Arithmétique*, Leyde, Plantin, 1585.

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defend, given the considerable historical and cultural variability that we know today in the different numbering systems. But we do not even need to engage in such discussions. For the phenomenon in question obviously also applies to a multitude of mathematical concepts created from scratch in more recent times. Let us take the example of the concept of "group" invented by Galois. Historical studies have first taught us that his original concept is not exactly the same as ours, so that the question arises as to what we mean when we say that Galois created "the" concept of group. And there is more. This concept was very quickly taken up in different meanings and practices by authors such as Cayley and Dedekind(15). This multiplicity of views has deeply nourished "our" concept of group, which can be presented either in an "abstract" way (à la Dedekind) or by "generators and relations" (à la Cayley). One might then be tempted to say that such terms function as proper names that are transmitted in the manner of Kripkean "rigid designators". But besides the fact that this view leaves the problem unresolved (what makes us believe that it is a matter of rigidly designating "the same thing" if we have no other access to mathematical objects than descriptions?), it is not supported by historical studies. Galois does not use the word "group" to designate what we isolate in his text as "group". Moreover, it is not uncommon, in fact, for something that appears to us at one time as the same object to have received several names ("differential"/"fluxion"; "table"/"matrix", etc.). Symmetrically, the same name can be used by different actors to designate different things ("structure" for the young Bourbakists/for Glivenko and Ore; "sheaf" in the sense of projective geometry/in the sense of Leray, etc.). More generally, the solution that would consist in basing semantic externalism on the mechanisms of historical transmissions (this was a path that Kripke favored for the transmission of proper names and that has been generalized by some for theoretical terms(16)) will come up against all the cases, now well documented, where no transmission is historically attested. Let us think of the presence of the "arithmetic triangle" or of the way in which systems of linear equations

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(15) See Caroline Ehrhardt : "Un concept mathématique, trois notions : Les groupes au XIXe siècle chez Galois, Cayley, Dedekind", *Images des Mathématiques*, CNRS, 2010, `http://images.math.cnrs.fr/Un-conceptmathematique-trois.html`.

(16) See P. Kitcher, "Theories, Theorists and Theoretical Change", *The Philosophical Review*, Vol. 87, No. 4, 1978, p. 519-547.

are already solved "by the Gauss pivot method" in Chinese mathematics, of the techniques of calculation on "series" developed in the Kerala school, of the "resolution of quadratic equations" on such and such a Babylonian tablet, etc. Finally, how can we not evoke more generally the cases where a mathematical fact is rediscovered or those of independent discoveries?

§ 3. — Intermezzo on Symbolic knowledge.

As is often the case, those in contemporary philosophy who think that the study of its history is only of interest to antiquarians often end up merely echoing a problem that has arisen before, albeit in a different guise. In fact, the question of whether a description can fix the reference of a concept was already at the heart of the quarrel of "ideas" that came about in the 17th century. Descartes, as we remember, proposed a criterion of truth very different from adequacy to reality, which had dominated until then in the Aristotelian tradition: "certainty" (not supposed to rid us of, but to guarantee "adequacy"). This certainty, of which mathematics was an example, was to be evaluated by the fact that it was based on "clear and distinct" ideas, characterized by their indubitability. But this opened up an obvious question that was to tear his successors apart: should we consider that an idea that is not "clear and distinct" refers to "nothing" or that it refers to something "improperly"? For Malebranche, inspired by Augustinian Platonism, the word "idea" should be reserved for what true knowledge targets. For Arnauld, one could and should speak of "idea" in all cases, under penalty of not being able to speak of a "false" idea. Leibniz intervened in this debate, in a famous text, by pointing out that the positions were vitiated by never producing a criterion of "clear and distinct"[(17)]. For the rest, the question seemed purely terminological. It was enough, for example, to use the term "notion" for the different conceptions that we have and reserve the term "idea" for the case where there would be adequacy (but this is only one choice among others).

[(17)] *Meditationes de cognitione, veritate, et ideis*, *Acta Eruditorum*, November 1684, transl. Ariew and Garber, *G.W. Leibniz: Philosophical Essays*, Indianapolis, Hackett, 1989, p. 25.

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In his argument in connection with mathematics, Leibniz made a remark of capital importance. If we characterize "clear and distinct" notions by the fact that we are able to provide a list of necessary and sufficient characteristics to acknowledge them (a path that was proposed by some Cartesians and that clearly anticipates complete definite descriptions), then this criterion will fail to guarantee the truth of the discourse in the sense of adequacy or correspondence to a "reality". In fact, I can give a list of criteria to "clearly and distinctly" identify an object like the regular decahedron: it is a convex polyhedron with ten identical faces, whose vertices have the same number of edges. But there is no such thing as a regular decahedron, as was demonstrated by Euclid, in a still unsatisfactory way, and then by Descartes by means of algebra (a demonstration that Leibniz knew because he had read it in Paris). Thus the philosopher underlines the fact that our knowledge can be "clear and distinct", but still contain hidden incompatibilities.

This defines a type of knowledge that Leibniz called "blind" or "symbolic". It is based on the fact that we use "symbols" in our reasoning, in the most general sense of the term (including words in everyday language, or diagrams), which we assume to have already been fully analyzed. "We use them", he says, "in algebra and arithmetic, and, indeed, almost everywhere"[(18)]. Let us note in passing that the mechanism of deference is therefore in no way limited to the deference to experts. We also apply it to ourselves insofar as we think that we have elucidated certain concepts at such and such a moment of our learning (but no less frequently we realize later that "we had not understood anything" about this concept). The key point of the argument is that, except in the extremely rare cases where we have an "intuitive" knowledge of the objects concerned, that is to say where we could extend the analysis until we reach notions known by themselves, the descriptions which would allow us to delimit clear and distinct ideas cannot by themselves guarantee an access to truth. They must therefore be accompanied by procedures attesting to the possibility of the objects. Leibniz then isolates three types of attestation to which I will return later: conceptual analysis (mostly imperfect), the construction of the object by means of a "genetic" or "causal" characterization, and experience.

[(18)] *art. cit.*, p. 19.

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§ 4. — The relationship to "*practice*".

If we return to the problem raised by Putnam, we see immediately that the road to semantic externalism seems difficult to follow in mathematics. Among the three strategies mentioned by Leibniz, this path would correspond to the one that allows us to stabilize the reference by resorting to a form of experience (in the form of a causal interaction with exemplars). However, there is a widely shared consensus that mathematics is not empirical in the sense that we do not develop an *a posteriori* knowledge of its objects and that we do not have causal interactions with them. Even authors who grant a primitive role to experience in mathematics, such as Brouwer and his followers, generally understand it as an experience *in thought*. There are certainly forms of "Platonism" (such as the one Gödel seems to have defended), which posits that we interact with mathematical objects that are "out there", independently of us, and that we "perceive" in a certain way. But this position is no less difficult to hold for reasons that Putnam immediately encountered in the case of sensible objects themselves. In fact, even for the latter, the idea that they can be "there, outside, independently of us" is full of mystery. This is the meaning of a long series of arguments he then deployed against "metaphysical realism".

Yet the road to semantic externalism is not entirely barred to us if we turn to another meaning of experience, one that is linked to our practices — what Putnam has called a "pragmatic" or "internal" realism. It is then a matter of pointing out that the stabilization of reference does not take place independently of certain practices of justification, including at the level of perception, and that it is in this way that the idea of things with which we interact independently of the descriptions we give of them is maintained[(19)]. Now, even if such a path will not bear on perception *stricto sensu* in the case of mathematics, it can at least be based on the set of the non-verbal practices that feed it. This is the path that Reviel Netz proposed to follow in order to overcome the fact that Greek mathematicians were, as we have already underlined, in a state of permanent disagreement as to the nature of the objects and the methods they were using:

[(19)] Putnam's argument, then, is that just because we only access "reality" through conceptual frameworks does not mean that we cannot accommodate, *from within such and such a framework*, the idea of entities independent of our descriptions.

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> what unites a scientific community need not be a set of *beliefs*. Shared beliefs are much less common than shared practices. This will tend to be the case in general, because shared beliefs require shared practices, but not vice versa. And this must be the case in cultural settings such as the Greek, where polemic is the rule, and consensus is the exception. Whatever is an object of belief, whatever is verbalisable, will become visible to the practitioners. What you believe, you will sooner or later discuss; and what you discuss, especially in a cultural setting similar to the Greek, you will sooner or later debate. But the real undebated, and in a sense undebatable, aspect of any scientific enterprise is its non-verbal practices[(20)].

This kind of strategy is at the heart of the program of a philosophy of mathematical practice and reproduces, with a certain delay, a "practical turn" which had already imposed itself, for the same reasons, in the study of experimental sciences. But it should be noted that, from a philosophical point of view, this solution will be of little value if it is not able to specify how one can delimit a "practice". If the analysis of the practice leads to saying that Rutherford and Bohr do not share the same practice, the problem of knowing how they could relate to the same objects will remain unresolved. If, on the other hand, we hold that they share the same practice, the problem will arise of knowing where the boundaries of such a "practice" pass if it encompasses incompatible views[(21)].

The problem is that "practice" here is a category introduced by the observer for the needs of the study and which, for this reason, has all the appearance of a pure and simple convention. One will thus find a number of historical studies showing the divergence of practices between actors relating to the "same" objects (in Putnam's sense). This will include all the cases already mentioned where conceptual disagreement is explained by a difference in practical

[(20)] R. Netz, *The shaping of deduction in Greek mathematics*, Cambridge, CUP, 1999, p. 2.

[(21)] This difficulty is underlined by Ferreiros when he studies Kitcher's proposal that a "practice" goes with a "language" seen as a conceptual framework (P. Kitcher, *The Nature of Mathematical Knowledge*, Oxford, OUP, 1983). Thus Kitcher, because he includes Newton and Leibniz in the same practice (that of the nascent differential calculus), must attribute to them a common "conceptual framework," which seems questionable, to say the least (see J. Ferreiros, *Mathematical knowledge and the interplay of practices*, Princeton University Press, 2015, p. 35).

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context. To return to the example mentioned above, it certainly makes sense to say that Euclid and Archimedes share the same practice (the one that Netz undertook to describe), but it is no less true to say, as Leibniz maintained, that there are here two great practices in geometry: one that, via Apollonius, leads to the Cartesian geometry of curves, and the other that leads to infinitesimal geometry in its various forms. Moreover, we can easily find different "practices" in the same author: thus, the reasoning by exhaustion that supports Archimedes' "infinitesimal" practice can be found on occasion in Euclid himself[22].

But "pragmatic realism" is more specific here than a mere incantatory reference to "practice". The point Putnam was highlighting was that the practices in question are characterized by forms of *justification*. If we take the example of ancient Greek geometry, the question is not so much to describe a certain way of reasoning with objects as to isolate a particular type of *justification*. This is what Ken Manders undertook to do in a famous study in which he succeeded in isolating a type of inference attached to diagrams (as opposed to that carried by the text) in the context of the plane geometry of Books I to IV of the *Elements*[23]. We are then in a position to explain both in what sense Euclid and Archimedes belong to the same practice — but also why the reasoning by exhaustion that plays a central role in Archimedean geometry can open up a *different* way of relating to objects. In both cases, the important thing is not the delimitation of a "practice", but our ability to identify stable types of inference.

The temptation is great then to say that we can completely dispense with semantic considerations and characterize objects only by their roles in inferences. This is the path that Manders clearly follows in relation to diagrams[24]. But this poses two problems that will prevent us from closing our initial questioning too quickly. On the one hand, it is not at all obvious that we can do without descriptions to *control* inferences, and in particular to block the way to certain illegitimate inferences. In this case, the practice of definitions/descriptions interacts with inferential practices in a

[22] The idea that mathematicians work at the crossroads of multiple "practices" is a point that Ferreiros insists on (*op. cit.*).

[23] K. Manders, "The Euclidean diagram", in P. Mancosu (ed.), *op. cit.*, pp. 80–133.

[24] See Manders, *art. cit.*, section 4.1.2 and Ferreiros, *op. cit.*, p. 9.

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complementary way to ensure a form of stabilization[25]. On the other hand, confining objects to their inferential role only makes the problem of transtheoretical identity all the more acute, i.e. the fact that certain objects, say the Euclidean circle, can be reinterpreted in *other* inferential practices (starting, in the Euclidean case, with Archimedes', but just as well with Cartesian algebraic geometry, complex analysis or modern algebraic topology). If the identity of objects is limited to their inferential role in a given practice, such phenomena of reinterpretation will simply not make sense. Finally, we should note that the stabilization by the "material" practice of the various symbolic systems which Manders highlights will be of no help in cases where these systems are different (typically in the case of different notational systems). And this was precisely the case in the two examples we came across (Stevin/Euclid and Cayley/Dedekind).

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§ 5. — By way of conclusion: towards a study of the modes of referential stabilization.

I hope to have indicated in the above that the general problem of the stabilization of reference would benefit from being posed within the framework of mathematics — if only because it presents specific difficulties which a general theory of knowledge should be able to account for. It seems to me that the most promising way out of the difficulties raised is to recognize that there can be several modalities of stabilization. From this point of view, Leibniz's suggestions seem particularly interesting, both because they rescue a form of scientific realism in a general context of epistemic opacity (for Leibniz, as we have seen, the vast majority of our knowledge is "blind"), but also because they propose a pluralist approach to the problem of fixing references. They thus open up a research program for the philosophy of mathematical practice, the main lines of which I would like to sketch in order to conclude this paper by returning to the difficulties raised.

So let's go back to the example of Euclid and Stevin. As I have pointed out, we will not be able to base a possible trans-theoretical

[25] For examples, I refer to D. Rabouin, "Proclus' conception of geometric space and its actuality", in V. De Risi (ed.), *Mathematizing Space. The Objects of Geometry from Antiquity to the Early Modern Period* (Basel: Birkhäuser, 2015), pp. 105–142.

identity in this case either on a definition or on a common symbolic practice. If we associate "conceptual analysis" with the first option and a certain relation to "experience" with the second (I will come back to this point shortly), two of the Leibnizian strategies are thus immediately barred. But what about the other way, based on a "genetic" characterization of objects? Here the divergence is greatly reduced. While Stevin explicitly departs from Euclid on the definition of "number", he does agree with him on the fact that a number is characterized by certain operations and, in particular, that it makes sense to look for the greatest common divisor of numbers, to study their decomposition into prime factors, etc. These constructions are at the heart of the extension he proposes in order to generalize the concept of number to what he calls "algebraic numbers" (in a different meaning from the one we give to this term today) and their "arithmetic" (which we would call an arithmetic of polynomials). More interestingly perhaps, the "genetic" approach allows us to isolate a practice over the long term, from the "arithmetizing" tradition of Arabic algebra to the contemporary notion of a polynomial ring (in this case a "Euclidean" ring). The key point is that the disagreement on the descriptions is resolved here in an agreement on another form of characterization of objects *by the way they are generated and manipulated*. This allows us to argue that in one sense our two authors are talking about the same object and in another sense they are not — and yet this relativity never lapses into relativism precisely because we are able to *specify* the modes of justification (construction vs. definition) that allow for such judgments — this is the basis of "internal" realism, as Putnam defines it.

Let us take another example. It is known that Cantor and Dedekind, while sharing many of the conceptions that gave shape to the emerging set theory, disagreed about the nature of the continuum. In *Grundlagen einer allgemeinen Mannigfaltigkeitslehre* (1883), Cantor criticized Dedekind for having characterized continuity by a property that applies to any perfect set. He then gives the well-known example of a set that satisfies this criterion, but that we would like to call "discontinuous" (the famous "triadic" set). This set is continuous in the sense of cardinality (it has the "power of the continuum"), it is also perfect, in the sense that it coincides with the set of its accumulation points, but all its points are nevertheless disconnected from each other (its connected components are reduced to singletons). In the latter sense, we would therefore like to call it "discontinuous" and not "continuous". We are here in a case where

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the actors are situated in a homogeneous symbolic practice (let us say, for the sake of brevity, the nascent "set theoretic" practice) and agree on the operations to be carried out with their objects. But they clearly separate themselves at a purely conceptual/descriptive level. It is the conceptual analysis, the first path traced by Leibniz, which allows then the reference to be fixed by separating the different meanings of the term "continuous". Thus, after Cantor, we can fix the reference of the term "continuous" not in a single object or a single property, but by distinguishing *different meanings* attached in the past to the same word "continuous" (and which we may designate by other terms today: completeness, connectedness). The fact that we are not necessarily dealing with the same entities under the same terms in earlier periods is then no longer particularly troublesome since we are able to distinguish meanings that were wrongly mobilized in an equivalent way. It is a simple (and relatively trivial) case of equivocity: what the ancient authors designated with a single term corresponds in fact to different, but well delimited meanings[(26)].

The last point I would like to emphasize is the question of recourse to "experience". As I have indicated, it does not seem to be possible to rely on experience in mathematics if one has to accept *a posteriori* modes of justification (which does not mean that there is no "experimental" dimension in mathematics). From this point of view, the third Leibnizian strategy seems forbidden. But Leibniz himself stressed several times that mathematics nevertheless rested on a singular regime of experience, that of the symbolic systems we work with[(27)]. We have seen, moreover, that this avenue has been explored in a very fruitful way by authors such as Netz and Manders concerning Euclidean diagrams (or notations). That these systems have a truly "material" component is a point which Manders particularly underlines in his study of Euclidean diagrams. Yet the anthropologist Ed Hutchins has rightly pointed

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[(26)] This phenomenon has been excellently analyzed by Kitcher (*art. cit.*), in response to Putnam's externalism, to argue that descriptions can fix the reference in certain situations (at least if we allow for the possibility of the coexistence of several descriptions that appear to us to be retrospectively incompatible in one and the same actor). This is what he called a "reference potential".

[(27)] "It is therefore necessary to notice that the proofs or experiences that one makes in mathematics to guarantee oneself from a false reasoning (...) are not made on the thing itself, but on the characters that we have substituted in the place of the thing" (A VI, 4, 5). The same text qualifies this reasoning explicitly as *a posteriori* (A VI, 4, 4).

out that the study of human reasoning reveals two major strategies for stabilizing our reasoning: one that relies on meaning (this was the case for the first two paths proposed by Leibniz), and the other on experience, and more precisely on what he called the "material anchoring" of reasoning[28]. In this case, we stabilize the reference by delegating part of our inferences to material devices that reason in a way "for us". This idea has also been put forward by some logicians as a way of accounting for "surrogative reasoning", sometimes in explicit reference to Leibniz, for whom it is believed — quite incorrectly — that this is the only "blind" mode of knowledge[29]. As Shimojima has argued, this makes it possible to characterize "diagrammatic" reasoning as ultimately relying on the interaction with material objects whose structures we exploit in order to make our inferences[30] — a description that is remarkably consistent with the analyses of both Netz and Manders. This is not the place to go into the details of this fascinating research, but I wanted to mention it in order to indicate both the fruitfulness of the Leibnizian diagnosis and the way in which it could be used as a basis for certain adapted forms of semantic externalism compatible with mathematical knowledge. It seems to me that there are all the necessary tools here to take up the challenge launched by Hacking for philosophy in our times.

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David Rabouin,
ERC Philiumm (AdG n° 101020985) &
Laboratoire SPHERE, CNRS,
Université Paris Cité

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⋆ ⋆

[28] E. Hutchins, "Material anchors for conceptual blends", *Journal of Pragmatics*, 37, 2005, pp. 1555–1577.

[29] C. Swoyer, "Structural Representation and Surrogative Reasoning", *Synthese*, Vol. 87, No. 3, 1991, p. 449-508.

[30] A. Shimojima, "Reasoning with diagrams and geometrical constraints", *Logic, Language and Computation*, 1, 1996, pp. 527–540.

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Towards Mathematical Criticism

Fernando Zalamea

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§ 1. — Introduction.

Often, in the so-called "Philosophy of Mathematics", Mathematics has been forgotten, and replaced by linguistic and speculative considerations, very distant from the original creations of the mathematicians. Some alternative perspectives have tried to come back to Mathematics (*e.g.* a "Synthetic" philosophy of mathematics, a philosophy of "Mathematical Practice"), but a fair assessment shows that a deeper understanding of *mathematical creativity*, looking with further precision into the very mathematical fabric, is still needed.

In that order, a simple Pascalian reason — "Mathematical Criticism should be the same to Mathematics as Literary / Artistic / Musical Criticism is to Literature / Art / Music" (see *Figure 1*) — drives our search. One of the main outputs of this approach is that what we may call (A) "Mathematical Criticism" turns out to be very far apart from the standard paths taken in normal (B) "Philosophy of Mathematics". In that distinction, we think that perspective (A) serves Mathematics in a more *full and faithful* way, which may be useful for the discipline.

Our article is divided in three sections. *Section 1* studies the establishment of a new field of inquiry, "Mathematical Criticism", akin to Literary Criticism, Art Criticism, or Musical Criticism, in which the main focuses of *critics* correspond to (*i*) precise descriptions of the *creative* works involved, (*ii*) thorough *internal and external* assessments of the works described, (*iii*) *calibrations* of their mathematical, philosophical, and cultural irradiations. *Section 3* offers a case study, around Galois and Riemann, comparing method and substance in the "Analytic Philosophy of Mathematics" with the "Mathematical Criticism" envisioned in the previous section. *Section 4* reviews some examples of "pioneers" of Mathematical

Criticism (*e.g.* Shaw, Lautman, De Lorenzo), often subsumed under a larger and more vague adscription to the "Philosophy of Mathematics", and presents our new RTSK models where Mathematical Criticism acquires a natural life.

$$\frac{\textbf{Mathematics}}{\textbf{Mathematical Criticism}} \equiv \frac{\textbf{Literature / Art / Music}}{\textbf{Literary / Artistic / Musical Criticism}}$$

Figure 1. — Towards Mathematical Criticism

§ 2. — Acknowledging the Need of Mathematical Criticism.

By their very practice, Literary Criticism, Art Criticism, and Musical Criticism, *delve into the creative works* of writers, artists, composers. In that sense, it is entirely impossible to understand Modern and Contemporary literature, art, or music, without entering in detail into the works of Proust, Manet, or Beethoven, just to mention some striking examples. In the same way, Literary Criticism stands on the towering figure of Walter Benjamin (and his Proust *critique*, for example), Art Criticism rests on Aby Warburg (and his Manet dissections), and Musical Criticism lies over Heinrich Schenker (and his Beethoven studies). Both (1) a fine attention to the works involved (*La Recherche*, *Le Dejeuner*, or the *Quartets*) and (2) their careful description-assessment-calibration are needed in order *to really sense and understand* literature, art, music. Following Pascal's *dictum* — "The heart has its reasons that reason knows nothing about" — we need to take into account both a "sensorial" aspect (heart, in Spanish "corazón" = "co-razón", the exact dual of reason) and a "rational" aspect, to understand a field of knowledge. Particularly, if that field tends to be very *creative*, we may need consistently a back-and-forth between perspectives (1) and (2) to capture *fully and faithfully* the given field (see *Figure 2*; the back-and-forth may be represented by a *functor*, which captures contexts, obstructions, and transferences).

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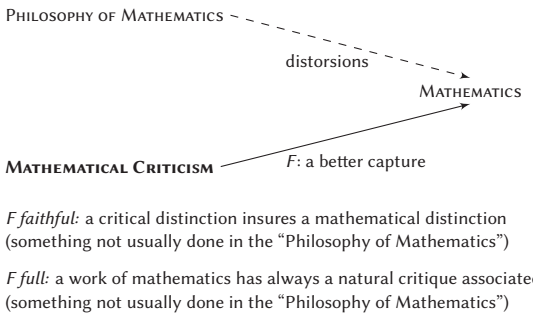


Figure 2. — A full and faithful functor F

If we follow [Francastel 1965], who reckoned that Art and Mathematics are the major poles of creativity in human thought, Mathematics would then be very much enriched by a Mathematical Criticism where the extrapolations of (1) and (2) to Mathematics would be mandatory.

The many *stratifications* of a creative work were beautifully captured in Gilles Deleuze's Foucault course (Vincennes, 1985-86). In a recent edited transcription of the course[(1)], Deleuze presents a deep understanding of cultural complexity, around Melvillean metaphors[(2)]. Building on *Moby-Dick; or, The Whale* (1851) and *Pierre; or, the Ambiguities* (1852), Deleuze constructs a "Whaling Line" diagram, in which he shows a profound understanding of *traversing culture*, both around natural and scientific languages. In a back-and-forth between sight and language, Deleuze describes "thick" strata where some statements are inscribed, to be further "opened" and studied through descents and ascents in an "oceanic zone" of knowledge. There, a "line of the outside" between the Self and the World is captured through its "velocity" and "foldings", producing for each of us a complex horizon of learning (see *Figure 3*)[(3)].

Deleuze evokes then "a mathematical genius, Galois, who made sorts of proofs with ellipses, deviations, precipitations, fulgurations"[(4)]. Galois is brought up as an example of "real mathematical

[(1)] See [Deleuze 1985-86]. The Spanish transcription has not yet even appeared in French.

[(2)] Lesson of 25 May 1986. *Ibidem*, pp. 173-204.

[(3)] The diagram is ours, based on the transcription of Deleuze's lesson, while he draws on the blackboard. The audio was taped, but we do not know of a video recording which captured the blackboard.

[(4)] *Ibidem*, p. 194.

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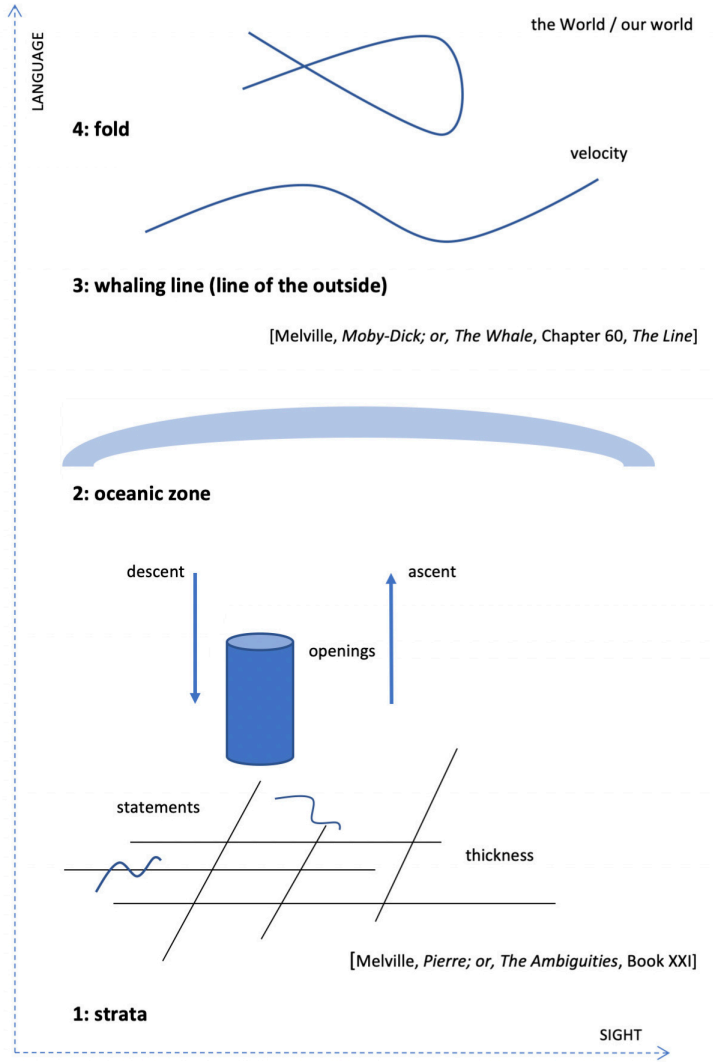


Figure 3. — Deleuze's Last Lesson – Foucault Course (20 May 1986)

creation", where the mathematician plunges in a series of "fulgurations, with blanks, deviations, etc."[(5)] Immersing in Black, Deep Waters — well beyond the futile "clearness" sought by Analytic Philosophy —, Deleuze studies the "velocity of thought" captured

[(5)] *Ibidem*, p. 193.

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by mediations, deviations, ramifications. In that *mud* of knowledge, mathematical creation responds naturally to literary, artistic, or musical creation, and many layers above deep bottoms represent the search for a dynamic, ever fluctuating knowledge.

In Deleuze's diagram, basic strata — either literary, artistic, musical, or mathematical — support ascents and descents in open, imaginary zones, where the lines of the Self and the World entangle, to introduce the fundamental dialectics which guide our understanding. The emerging creative Work — either literary, artistic, musical, or mathematical — is then looked at by the Critic, who can describe and uncover the raw forces which shape the written, plastic, or symbolic cultural production observed. But, contrary to Art Criticism, Musical Criticism, or Literary Criticism, well established cultural practices, *Mathematical Criticism* has never surfaced as a field on its own, and has usually been confused with fragments of the Philosophy of Mathematics (see *Figure 4*).

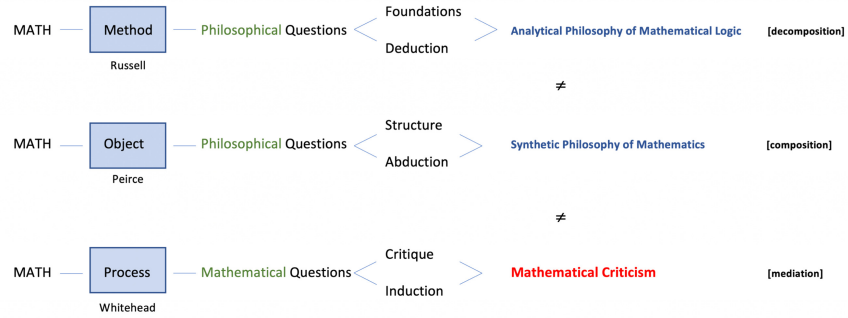


Figure 4. — Towards Mathematical Criticism

As the Art Critic deals with artistic questions, the Literary Critic deals with literary questions, and the Musical Critic deals with musical questions, the Mathematical Critic has to deal with *mathematical* questions. A *real knowledge of mathematics* is thus necessary, a knowledge *at large* which cannot be reduced, for instance, to philosophical questions about the language of logic and set theory (= "Analytic Philosophy of Mathematical Logic and Set Theory", an interesting but extremely restricted field, often confused with the "Philosophy of Mathematics").

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Mathematical Criticism can then be defined as the study of:

- **i.** how mathematical thought is constructed (*description of processes*)
- **ii.** how those constructions are entangled in ideal realms of possibilia (*critique*)
- **iii.** how those entanglements are contrasted with the real (*induction*, in Peirce's sense).

Well beyond considerations of method and logic, mathematical creativity involves a complex web of "fulgurations, blanks, deviations", as noticed by Deleuze. In fact, [Poincaré 1908] and [Grothendieck 1983-86][(6)] explore with utmost detail the many zigzags and wanderings of their own inventions. In a sense, in those essays, the mathematician and the mathematical critic converge. But possibly the greatest example of a Mathematical Critic in the 20th century may be Albert Lautman[(7)] (see *Section 4* below), a towering figure not usually studied along the "normal" currents in the Philosophy of Mathematics. Lautman is the perfect example of the *Critic* — an outside observer: not a writer (*e.g.* Benjamin), not an artist (*e.g.* Warburg), not a composer (*e.g.* Schenker), not a mathematician — who describes, critiques, assesses, and synthesizes, all the complex variations and forces at work in a precise cultural context.

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§ 3. — Analytic Philosophy versus Critics: Around Galois and Riemann.

In a similar way to the now popular "Philosophy of Mathematical Practice" — which has been growing in the last decade, but was preceded (and already superseded) by the work of Javier de Lorenzo (see *Section 4* below) — Mathematical Criticism emphasizes a *return to mathematics*, a way to fully and faithfully look and assess *mathematical doing* (De Lorenzo's battle horse). The crucial thing becomes, first, to capture mathematics, in order to,

[(6)] For a broad and, at the same time, very precise, guide to Grothendieck, see [Zalamea 2019].

[(7)] *Cf.* [Lautman 1935-42]. See also [Lautman 2011], the extended Spanish translation of Lautman's complete works, with many texts not present in the French edition.

subsequently, reflect upon it. "Mathematical Critics" points to this *double process* of knowing, and thus turns out to be closely situated to any other critical action (Literary Criticism, Art Criticism, Musical Criticism, Film Criticism, etc.) In fact, the task of the *Critic* is double: (1) to explore, know, feel, *live* the works under study, and only then (2) describe, calibrate, evaluate, *explain* them. The first stage is *mandatory*: without a deep immersion in a work, there are no critics. It is *not valid* to perform a *substitution* of the work by a discourse upon it and by n^{th}-order crossing references between specialists which no longer say anything about the original work. Unfortunately, this is the case of the "Analytic Philosophy of Mathematics", which has substituted, with no major shame, mathematical works — "mathematical doing" — by self-adulatory *clique* references and by linguistic considerations, supposedly deductive, on logic (usually classic) and set theory (usually Cantorian), leaving aside all inventive power of mathematical thought (a Galois, a Riemann, a Grothendieck, for instance). The situation is as absurd as to pretend to analyze Modern art without ever mentioning Monet, Duchamp, or Picasso, Modern literature without studying Proust, Joyce, or Musil, or Contemporary Film without ever looking at Antonioni, Bergman, or Tarkovsky.

The "Analytic Philosophy of Mathematics" has restricted entirely its *substance* (reducing the ever growing complexity of mathematics to problems of foundations, around logics and sets) and has been essentially concerned with problems of *method*. The confusion is gigantic. As a good product [Shapiro 2005] of the school forcefully shows, some of the pillars of 19th century Modern mathematical thought — abstract algebra, complex variables, algebraic geometry, for example — are never mentioned[(8)]. How can one talk about Mathematics, without ever referring to Mathematics? It seems crazy but it has been done: it is just a *miracle* — really a *mirage* — of the linguistic turn used in the "Analytic Philosophy of Mathematics", which must be forcefully denounced and confronted. Many years of clever and inconsequential discourses in the "Philosophy of Mathematics" have been accepted in the Academia without pondering some healthy *critical distance* on them. A fact

[(8)] The indexes (of both subjects and proper names) at the end of the volume — badly named "The Oxford Handbook of Philosophy of Mathematics and Logic", while a more humble "The Oxford Handbook of Anglo-Saxon Analytic Philosophy of Logic" would be much more indicative — refer only two pages (of the 833 in the volume) to Galois and Riemann; Grothendieck does not even appear.

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about "Mathematical Criticism", if such a field turns out to be soundly developed[9], is that *mathematical substance* — a detailed attention to mathematical works, *forced* in step (1) above — will never disappear in its considerations, in sharp contrast with what has happened in the "Analytic Philosophy of Mathematics".

A good case study of the situation is the scant attention given in the "Analytic Philosophy of Mathematics" to Galois and/or Riemann. A quick search in the MSC database, shows, today, thousands of references to Galois and Riemann. Their role in *mathematical thought* is central to our understanding of the discrete and the continuous, of number and magnitude, of algebra and topology, of number theory and geometry. But the "Analytic Philosophy of Mathematics" *cannot see* them. On one hand, the structures they study, in the algebraic and differential realms, exceed logics and sets; on another hand, their very methods — Galois's *ambiguity* theory, Riemann's *uniformization* theorems — are inherently *vague*, in order to involve enough non-trivial automorphisms of the structures. An "analytical reduction" (around structures) and an "analytical clearness" (around methodology) are not only impossible, but would be *mathematically unsound*. In fact, the very *richness* of mathematics is directly correlated with what an analytical dissection throws to the garbage... In that sense, again, *if we want to understand mathematics*, Mathematical Criticism would be much more useful than a *blind* "Analytic Philosophy of Mathematics". Of course, if one wants *instead* to understand languages, logics and foundations, please turn to the "Analytic Philosophy of Logics and Sets", but *do not pretend to talk about Mathematics* in that restricted environment. We obtain thus a simple clarification of diverse trends in the "Philosophy of Mathematics" (see *Figures 2, 4* above).

A precise example of an important mathematical problem that an "Analytic Philosophy of Mathematics" approach would never capture is the eventual anticipation of Riemann surfaces in Galois's Last Manuscripts (*Sainte-Pélagie* Prison, July 1831 - March 1832; *Lettre Testamentaire*, May 29, 1832). Combining history and mathematical acumen, [Dieudonné 1962] anticipated the problem[10] as an instance of a (Lautmanian) mixture between the

[9] We hope to advance this program in our next monograph [Zalamea 2023].

[10] "One can imagine that Galois was very close to the idea of a «Riemann surface» of an algebraic function, and that such an idea must have been fundamental in his researches on what he called the «Theory of Ambiguity»" [Dieudonné 1962, p. *v*].

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elliptic-modular (Complex Variables) and the structural (Abstract Algebra). Galois had *seemed* to develop the algebraic architecture before the complex variable apparatus, as some traceable sources indicate: (*i*) an *additional* note (January 16, 1831) to the "Introduction" of the *Première Memoire*, where the young genius states that "the modular equations of elliptic functions cannot be solved by radicals"[(11)], underscoring an eventual application of a *prior* general algebraic theory to the specific case of the elliptic-modular case; (*ii*) the Sainte-Pélagie Manuscripts, with more than 70 pages on modular and elliptic *calculi* (by far the largest output of mathematical work done by Galois), advanced *after* his memoirs on non-solvability of equations. Nevertheless, a yet unnoticed indication prompts to the contrary, in support to Dieudonné's hypothesis. In fact, (*iii*) in the Prison Manuscript [176a] appears the cryptic addition of a date — "1er Mars 1827"[(12)] — which directs the Galois of 1831 to four years before. The annotation points to the beginnings of Galois's works (1827), but appears to be written at the end of his life (c. 1831). The situation is very surprising: could it be that there existed a *deep continuous* connection between 1827 and 1831, "invisible" to the eyes of the 1830 *superficial algebraic* fragments? The *Lettre Testamentaire*[(13)] offers a support to this perspective, unravelling *very similar methodologies* at work in Galois and Riemann: (*a*) visibility of the whole beyond the singular [8a], (*b*) canonical/iconical decompositions of equations [8b], (*c*) modular representations [9a, 9b], (*d*) uniformization of abelian integrals [10a, 10b], (*e*) final vision of an ambiguity/complexity theory [11a].

With this example, one sees that *without entering into the mathematics involved* (task (1) of the Critic above), one simply cannot expect to philosophize upon them (task (2)). *Without a deep immersion into mathematics*, many problems around content and method simply disappear of the panorama: an *inversion* between (*i*)-(*ii*) and (*iii*), that is, a wallowing that would change the *whole*

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(11) Manuscript [2a], *cf.* [Galois 1962, p. 43].

(12) The date appears in a digital copy of Galois's Manuscripts at the Institut de France, in the middle of calculations around complex transformations ($\frac{ak+b}{ck+d}$), infinity points (∞) and the order of a normal group ($\frac{p+1}{2}$). The fact that the date *does not* appear in Azra and Bourgne's critical transcription [Galois 1962, pp. 325-327] is extremely indicative. The editors simply could not imagine an *inversion* of Galois's creative imagination, where the elliptic world could emerge *first*, and *then* lead the way to inventiveness in the algebraic world.

(13) Manuscripts [8a-11a], *cf.* [Galois 1962, pp. 173-185].

understanding of our discipline, cannot be stated, or even detected. In this sense, a Mathematical Criticism must be developed, as (1) a first and mandatory condition to (2) allow to reflect upon mathematics. We would then be able to (1) *plunge* into the technical constructions that support the entire life of the discipline, in order to afterwards (2) *emerge*, breathe, and think about our immersive experience. The (1) deep, muddy, obscure waters of the Ocean support our (2) superficial, clear, bright balance, along waves in the ocean shore (*cf.* Deleuze's diagram, *Figure 3* above, contrasting (1) *Moby-Dick* and (2) our "whaling lines").

§ 4. — Some Pioneers of Mathematical Criticism. The Models RTSK.

Mathematical Criticism has to share some *common processes* with other criticisms (Literary Criticism, Art Criticism, Musical Criticism, Film Criticism, etc.): First, a *direct observation* of the creative works involved; second, a *strategy to analyze, decant, and synthesize back* the observed constructions; third, the elaboration of a *systematic stratification* of perspectives, calibrations, and assessments of the works discussed; fourth, the construction of a *general, global* architecture of knowledge based on the particular, local case studies offered by the *critique*. In this *continuous back-and-forth* between analysis and synthesis, observation and speculation, localization and globalization, immersion and emergence, Mathematical Criticism should never choose a perspective over others, and should never attempt to produce impoverished reductions of a complex state of affairs.

This *multidimensionality* of mathematical thought — combining words and images, proofs and hypothesis, definitions and intuitions, theorems and errors, concrete types and universal archetypes — has often been heralded (*e.g.* in [Poincaré 1908]) as one of the *crucial richness* of the discipline. Three paradigmatic critical enterprises, deeply attentive to that multidimensionality, can be here mentioned: the works of James Byrnie Shaw (1866-1948), Albert Lautman (1908-1944), and Javier de Lorenzo (born 1939), among other great explorers of mathematical thought, not reckoned by the usual trends in the "Philosophy of Mathematics". Shaw's *Lectures on the Philosophy of Mathematics* [Shaw 1918] constitute a miracle of perspicacity in the middle of the First World

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War[14], before the normative and stringent directions of the emerging "Analytic Philosophy of Mathematics" began to ravage the Anglo-Saxon world. Shaw explores *alternative mathematical spectra* with great insight: the geometrization of mathematics (chapter 3) *against* its classical arithmetization (chapter 2), the theories of operators, hypercomplexes and transformations (chapters 6-8) *against* logistics and deduction (chapters 5, 9), the analogical and dynamical forces in mathematics (chapters 4, 14-16), the centrality of form, invariants and functions (chapters 10-12) at the very *heart* of mathematical thought. Shaw offers direct comments on the *real Masters* of mathematics in the 19th century (Galois, Riemann, Kummer, Lie, Weierstrass, Klein, and, above all, Poincaré, his true intellectual mentor), on the French school in the Philosophy of Mathematics (Bergson, Brunschvicg, Winter, Milhaud, Boutroux), on Anglo-Saxon diagrammatical thinking (Hamilton, Sylvester, Peirce, Kempe), on the illustrious mathematicians of his time (Picard, Hadamard Russell, Hilbert). The panorama is entirely outstanding, going well beyond what many other texts will offer in the next hundred years, to really try to understand the "structure of mathematics" (transitions, spectra, dynamic forces) and its "central principles" (objects, form, invariance, functionality, ideality).

Albert Lautman's "Essay on Notions of Structure and Existence in Mathematics"[15], dedicated to the memory of his friend and mentor Jacques Herbrand, studies carefully the *structural* and *dynamical* conceptions of mathematics, interlacing the "life" of modern mathematics with a sweeping spectrum of dialectical actions: the local and the global (chapter 1); the intrinsic and the induced (chapter 2); the becoming and the finished — closely tied to the ascent and descent of understanding (chapter 3); essence and existence (chapter 4); mixtures (chapter 5); the singular and the regular (chapter 6). Lautman divides his thesis into two large parts ("Schemas of Structure" and "Schemas of Genesis") so as to emphasize one of his fundamental assertions regarding the mathematics of his epoch — that modern mathematics has a structural character (a prefiguration of the Bourbaki group, Lautman was close friends with Chevalley and Ehresmann) and consequently *mathematical creativity*

[14]The lectures were given at the University of Illinois in 1915, to a "club of graduate students". I thank here Charles Alunni for pointing me to this truly extraordinary essay, almost entirely forgotten in the 20th century.

[15]His thesis for a Doctorate in Letters (philosophy), Paris: Sorbonne, 1937, *cf.* [Lautman 1935-42, 2011].

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(the genesis of objects and concepts) is interlaced with the *structural decomposition* of many mathematical domains. Dialectical oppositions, with their partial saturations, and mixtures constructed to saturate structures, are linked to one another and to the underlying living processes of mathematical technique. For the first time in the history of modern mathematical philosophy, a philosopher conducts a *sustained*, *profound and sweeping survey of the groundbreaking mathematics of his time*. Confronting its technical aspects without ambiguity or circumlocution, and "dividing" it into basic concepts that he painstakingly explains to the reader, Lautman presents a strikingly rich landscape of the inventive currents of modern mathematics (his studies around Galois, Riemann, Poincaré, Hilbert may still be some of the best available introductions to their works). Thus, breaking with the usual *forms* of philosophical exposition, which used to (and, unfortunately, still do) keep the philosopher at a distance from *real* mathematics, Lautman opens an extraordinary breach in an attempt to seize upon the central problematics of mathematical creativity.

In his *Introducción al estilo matemático* [Lorenzo 1971], Javier de Lorenzo confronts some of the great figures of modern mathematics (Cauchy, Abel, Galois, Jacobi, Poincaré, Hilbert, the Bourbaki group, etc.) and argues that certain *fragments* of advanced mathematics — group theory, real analysis, and abstract geometries are his preferred examples — bring with them *distinct* ways of seeing, of intuition, of handling operations, and even distinct methods of deduction, in each of their conceptual, practical and formal contexts. De Lorenzo points out how mathematics "grows through contradistinction, dialectically and not organically", and thereby breaks with a traditional vision of mathematics, according to which it grows by accumulation and progress in a vertical ascent. He proposes instead a conceptual amplification of the discipline, in which new realms interlace with one another *horizontally*, without having to be situated one on top of the other. In *La matemática y el problema de su historia* [Lorenzo 1977], De Lorenzo postulates a radical historicity of doing mathematics. The references to advanced mathematics are classified in terms of three primary environments, within which, according to De Lorenzo, the major *ruptures and inversions* that gave rise to modern mathematics were forged: *the environment of 1827*, in which the program for the resolution of mathematical problems is inverted, setting out "from what seems most elusive in order to account for why [problems] can or cannot be resolved", and in

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which mathematics begins to feed on itself and its own *limitations*; *the environment of 1875*, in which the mathematical tasks of the previous half century are unified (groups, sets) or transfused from one register to another (geometrical methods converted into algebraic or axiomatic methods), generating the important constructions (Lie groups, point-set topology, algebraic geometry, etc.) that drove the development of mathematics at the outset of the twentieth century; *the environment of 1939*, in which the Bourbaki group fixed the orientation of contemporary mathematics around the notions of structure and morphism, inverted the focus of mathematical research, and moved toward a primordial search for relations between abstract structures (algebras, topologies, orders, etc.). In his studies, not only De Lorenzo prefigures the latter called "Philosophy of Mathematical Practice", but goes well beyond it in many aspects (contextual analysis, stylistic attention, horizontal translatability, historical acumen).

Ignoring the kind of efforts advanced by Shaw, Lautman, de Lorenzo, and by many other alternative "Philosophers of Mathematics" (*e.g.* Weyl, Mac Lane, Rota, Châtelet, etc.), the "Analytic Philosophy of Mathematics" has tried, unsuccessfully, to reduce Mathematics to Logic and Set Theory. Emphasizing, language, logic, and the fact that all mathematical concepts may be reconstructed inside the axiomatic architecture of sets, the reduction forgets that one thing is (1) to develop/create mathematics, and a very different one is (2) to present/represent those creations. Process (1) is done through images and structures which live in an intuitive *back-and-forth* between the discrete and the continuous, number and magnitude, algebra and topology, where a *mathematical* acumen is necessary. Process (2) is constructed, instead, through layers of language and axiomatic approximations, where a *logical* acumen is necessary. Often, the two processes have been disintegrated, showing no true understanding of "real mathematics" (meaning, for example, number theory, abstract algebra, geometry, topology, differential equations, functional analysis, etc.)

A true integration of processes (1) and (2) — the first one more synthetical/semantical, the second one more analytic/syntactical — becomes then a *minimum* condition to understand mathematics as a general form of thought. A simple way to ensure that minimum condition (a sort of *pendularity* between the analytical and the synthetical) is to understand *mathematics as a sheaf* (see *Figure 5*).

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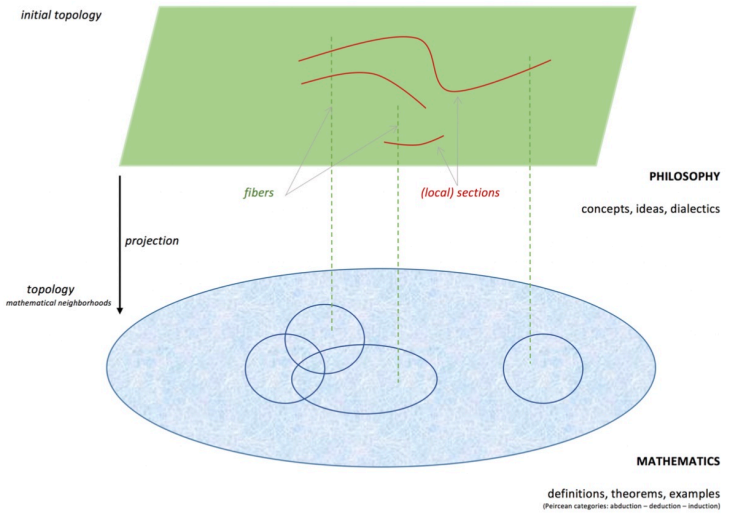


Figure 5. — Mathematical Thought as a Sheaf

In a bottom, *base* space, we place mathematics as it is usually presented — definitions, theorems, examples — emphasizing techniques, locality, necessity. Then, on an upper, *deployed* space, we place the fundamental ideas, images, intuitions that are projected and encrypted into the techniques of the bottom space. In this way, every technique (more akin to process (2) above) possesses over it a *fiber* of ideas (closer to process (1)). One of the main problems in order to understand mathematics as a true form of thought consists then to capture *sections* of ideas fostered by techniques, and to see if those *local sections* can be glued into *global* ones (determining thus some central forces of mathematical invention/creativity). This perspective forces at once a *natural shift* in the "ontology of mathematics": beyond objects *per se* (Absolute Ontology), or objects *per altri* (Transitory Ontology), objects are understood as photographs (fibers) of kinematic processes (sections) (Sheafification Ontology).

This simple model (S, for *sheaf* — topological locus of the study of obstructions and transfers between the local and the global) fails to take into account many other fundamental dimensions of mathematical thought: (*a*) the historical development of mathematics, (*b*) the stability of mathematics (layers of integral invariants — "archetypes" — beyond differential variations — "types"), (*c*) its

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irradiation to science and culture in general. Using some revolutionary ideas of Grothendieck (for a thorough introduction, see [Zalamea 2019]), one can expand the idea of a given, singular, sheaf, into a *multiplicity of sheaves* (T, for a *Grothendieck topos*) over a dynamic, evolving space (K, for a *Kripke model*) (see *Figure 6*). With these two expansions, we obtain a (TSK) model which handles well the requirements (*a*) and (*b*) above (historicity through K, stability through T).

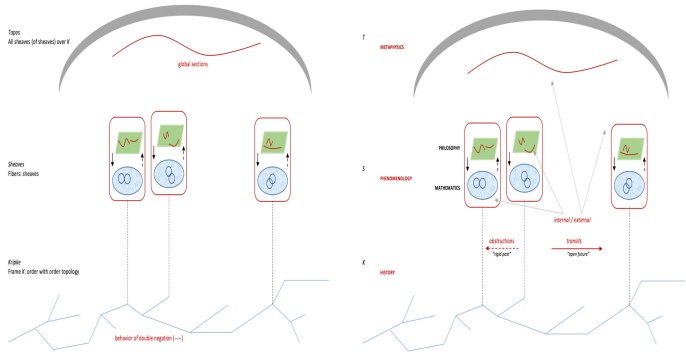


Figure 6. — Mathematics through a full (TSK) model

Finally, another expansion can integrate requirement (*c*), thanks to geometric ramifications (R, for a *Riemann surface*) (see *Figure 7*), where the many layers of culture (particularly around literature, art, and music) enter into a natural *dialogue* with the mathematical "archetypes" found at level (T). The result provides a rich, but simple, model RTSK (for a thorough discussion and many examples, see [Zalamea 2021]), where the complexity of mathematics is *not* reduced to one of its many dimensions.

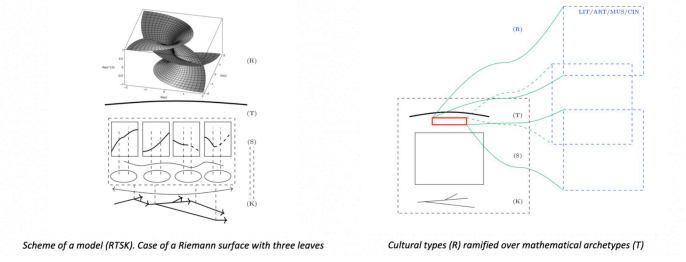


Figure 7. — A model RTSK: mathematics (TSK) ramified through cultural types (R)

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Working with the RTSK models, one can expect to develop some kind of sound *Mathematical Criticism* (expected result [Zalamea 2023]). In fact, the requirements exhibited in *Figures 2, 4*, and the desiderata listed at the end of *Section 1*, all are satisfied within the RTSK models. On one hand, the *full and faithful* functor *F* (*Figure 2*) is immediately realized by the many distinctions provided by the models: already at level of the "phenomenological" sheaf (S), *faithfulness* is obtained thanks to the good *topological* properties (local homeomorphism) of the projection between ideas (Mathematical Criticism) and techniques (Mathematics), and *fullness* comes handily from the very *set-theoretic* properties (surjectivity) of the projection. On another hand, the essential mediation, critique and inductive characteristics sought in a sound Mathematical Criticism (*Figure 4*), are obtained, in the mathematical realm, through the "metaphysical" level (T), and, in the cultural realm, through the "irradiation" level (R). Finally, characteristics (*i*)-(*iii*) for Mathematical Criticism listed at the end of *Section 1* are immediately incarnated in the RSTK models: *processes* (*i*) living at type (S) and archetype (T) levels, *modal/ideal critique of possibilia* (*ii*) living at invariance (T) and ramification (R) levels, *real entanglements* (*iii*) living at contextual, historical levels (K).

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Fernando Zalamea, Departamento de Matemáticas, Universidad Nacional de Colombia, Sede Bogotá.

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Regards and Insights on the Universe, an Interview with Sir Roger Penrose[1]

Jean-Jacques Szczeciniarz & Joseph Kouneiher

Abstract. This paper is the transcript of an interview with Sir Roger Penrose, which took place on November 15, 2021. The main aim of the paper is to explore with him the various aspects of his work.[2]

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§ 1. — Introduction.

Sir Roger Penrose is a British mathematician and mathematical physicist who brought an original and exceptional insight to the philosophy of science and particularly the philosophy of mathematics. He has received several prizes and awards, including the 2020 Nobel Prize for his work showing that black hole formation is a robust prediction of the general theory of relativity and the 1988 Wolf Prize which he shared with Stephen Hawking for "Penrose-Hawking singularity theorem". Penrose has made contributions to the mathematical physics of general relativity and cosmology.
He began his career as a mathematician in the 1950s with a thesis entitled, "Tensorial methods in algebraic geometry". He became

[1] Sir Roger Penrose is one of the most accomplished scientists of our time. We would like to thank him for sharing with us his conception of the world and his regard on the foundations of mathematics and physics and the contexts of emergence of his fundamental ideas and breakthrough. We express to him our gratitude, our admiration and our friendship.

[2] The text of the interview was enriched by complements and figures to illustrate the answers and the thought of Roger Penrose. They are taken from the excellent book *The road to reality : a complete guide to the laws of the universe* and others references (see bibliographies). With courtesy of Sir Roger Penrose.

interested in physics and general relativity and cosmology from the 1960s under the influence of the physicist Dennis W. Sciama who later became Stephen Hawking's supervisor.

Penrose introduced original mathematical methods from algebraic geometry and differential topology to solve the equations of the theory of general relativity and to better understand their predictions concerning the relativistic theory of stars and cosmology (Roger Penrose, 1987, 1988). In 1965, he demonstrated that what would later be called a black hole by John Wheeler is an inevitable consequence of general relativity applied to the gravitational collapse of a sufficiently massive star. This theorem discovered by Penrose also implies that the endpoint of the collapse of a black hole star is a singularity of space (Penrose, Roger, 1965). Penrose also introduced other geometric methods, in particular, what came to be known as Penrose-Carter diagrams, which led to important discoveries in the field of black holes and cosmology (Roger Penrose, 2004). The calculations made by Hawking to discover his famous quantum radiation of black holes use such a diagram.

Later, Penrose also developed original geometric ideas for a quantum theory of physics in curved space-time. His theories of 'twistors' and spin networks are used today to explore quantum theories of gravitation (Roger Penrose, 2004, 2018). In recent years, Penrose has also proposed and defended a new cosmology, known as Conformal Cyclic Cosmology, as an alternative to the theory of inflation, which he doubts. Penrose manifests the same doubt regarding the theory of superstrings (Roger Penrose, 2010, 2012).

We also owe Penrose, with the complicity of his father, and from the age of 16, some flat paradoxical representations of 3D objects that inspired the Dutch artist Maurits Cornelis Escher who evokes the now-famous 'Tribar' or Penrose triangle[(3)] in some of his works. Penrose's interest in geometry and puzzles also led to the discovery of a new type of mathematical tiling of the plane that was thought to be impossible and which would have very concrete applications many years later with the discovery of quasicrystals (Paul Steinhardt, 1996).

In this interview, we would like to look at the origins and the conceptual and philosophical foundations of certain aspects of Sir

[(3)] Which characterize Sir Penrose is his capability and uniquely visual way of experiencing mathematics. Penrose can obviously hack the equations, but he also has to see them, and he is astonishingly resourceful at coming up with visualisations.

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Roger Penrose's work and his insights into the foundations of mathematics and physics. Roger Penrose can legitimately be considered one of the true successors of Newton.

§ 2. — Foundations.

JJS & JK: Repeatedly you said that if you have a choice, your universe would have to be mathematical, and you expressed the hope that the complex numbers would underlie the actual universe. Do you think it is essential for understanding physics?

Roger Penrose: Well, that's an interesting question. The power and the beauty of complex numbers was something I learnt in mathematics when I did my degree in mathematics, both pure and applied Mathematics, so it was specifically mathematics. I was more attracted to pure mathematics than physics. In particular, there was a course on complex analysis and I did find complex analysis particularly amazing in the sense that it would be able to take advantage of what I had regarded, ever since my days as a mathematics undergraduate, as the 'magic' of complex analysis and holomorphic (i.e. complex analytic) geometry.
I had learnt that the complex number system had not only a profoundly deep power and elegance, but that it had also found a basic realization in its underlying role in the formalism of quantum theory. When I had begun to study quantum mechanics in a serious way, and particularly following the superb course of lectures given by Paul Dirac when I was a graduate student (in algebraic geometry) at Cambridge, I became fascinated by the quantum description of spin, and how the complex numbers of quantum mechanics were directly related to the 3-dimensionality of physical space, via the 2-sphere of spatial directions being appropriately identified as a Riemann (or Bloch) sphere of the ratios of pairs of complex numbers (quantum amplitudes) where, in the case of a massive particle of spin such as an electron (see Figure 1), we can think of these as being the complex components of a 2-spinor. Moreover, I had realized that in the relativistic context, there was another role for the Riemann sphere, this time as the celestial sphere that an astronaut in space would observe. The transformation of this celestial sphere to that of a second astronaut, moving at a relativistic speed while passing nearby the first would be one that preserves the complex

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structure of the Riemann sphere (i.e. conformal without reflection). The special (i.e. non-reflective) Lorentz group is thus seen to be identical with these holomorphic transformations of this Riemann sphere (Mobius transformations). Again, this was clear from the 2-spinor formalism, this time in the relativistic context.[4]

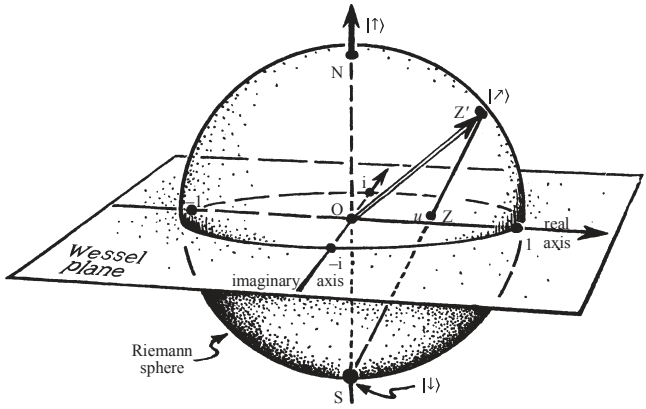


Figure 1. — The Riemann sphere (here in its role as a Bloch sphere) projects stereographically from its south pole S to the complex (Wessel) plane, whose unit circle coincides with the equator of the sphere. A general spin state $|\nearrow\rangle = w|\uparrow\rangle + z|\nearrow\rangle$, of a spin-1/2 massive particle is represented by the point Z on the Wessel plane denoting the complex number $u = z/w$, which is the stereographic image of Z' on the sphere (so S, Z, and Z' are collinear). The spin direction $\nearrow$ is then OZ, where O is the sphere's center.

JJS & JK: So, the use of complex numbers starts with quantum mechanics and the use of spinors in quantum mechanics and 2-spinors in general relativity, then twistors.

RP: The twistors came later, that's right yes.
I knew nothing about quantum mechanics then. Still, I had a view at that time that I should explain: wouldn't it be wonderful if the physical world was dependent on complex numbers'
You first learn about real analysis and you learn about functions and how they can be smooth. You can differentiate them once, twice or three times and maybe not more. And you have different kinds of functions with different degrees of smoothness, it's very complicated. And when I learnt about complex analysis, it was all

[4] Twistor theory as an approach to fundamental physics, pp. 255–256.

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the same. There's only one degree of smoothness and then you can also expand things in power series. This was such an amazing thing to me.

I just knew about Newtonian mechanics and dynamics and things like that, and I had no feelings for quantum mechanics. And when I learned about quantum mechanics, I saw all the complex numbers are there! Particularly in relation to spin. I think what I found the most attractive was how the notion of spin makes use of complex analysis. It's a thing that sort of fell into place. It was certainly a big feeling.

I didn't start to learn physics in a serious way. Well, we did have a part of mathematics, I learnt about things like Lagrange's equations. I knew something about the general framework of physics at the time. I was in London when I did my mathematics degree and I went to Cambridge to study algebraic geometry.

So, this is again just pure mathematics. But I got to know Denis Sciama and the cosmologists there and I went to their lectures. There were three lectures courses that were way much more influential than the courses I was supposed to be doing. One was a course on mathematical logic and I learned about Gödel's theorem and computation and that was important for me. I went to a course by Hermann Bondi, a wonderful course on general relativity which I got a genuine feeling for the subject. I think that course was an amazing course for me and important in later life. And the third course was a course, as I said before, by Dirac on quantum mechanics.

And I certainly found the subject extremely beautiful and puzzling at the same time. And I remember his first lecture, in which he described that if you had a proton, it could be in two places: it could be here, it could be here and it could be here and here at the same time. Then he talked about a piece of chalk, and could it be here and here at the same time.

I think he broke it into parts to illustrate this, and my mind wandered. I remember I was looking out of the window thinking about something completely different and when my attention came back, he had moved on to the next topic and I was left puzzled by how pieces of chalk were never found in two places at once. It's probably fortunate that I didn't hear the explanation because it was probably an explanation to stop your worrying about the problem.

I don't know what he said, but it was probably something about energy and how much energy it will cost or something like that. I'm

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glad I didn't hear it because it might have stopped me from worrying about that problem. But I worried about it ever since which is also very important in my later work. So, I gave Dirac credit also for that.

Well, the other thing which was very important later on was when I was a graduate student and I had a year after I got my Ph.D. in algebraic geometry, I played around with general tensor systems. It wasn't particularly what I was supposed to be doing but never mind. And then I went for one year, I taught a course and again it was pure mathematics at Bedford College in London in Regents Park. It's a very beautiful area in the middle of the park. But then, I got a fellowship at Saint John's College Cambridge on the basis of my fellowship thesis which was actually in pure mathematics, as a matter of fact. But then, when I got to Cambridge, I decided that I would pay more attention to physics. Largely because of the influence of Dennis Sciama, who was a great friend of mine. He was a friend of my brother initially and I had an interest in cosmology which I picked up from hearing Fred Hoyle's radio talks about the steady-state model. And I found it very puzzling. Dennis himself was very enthusiastic about the merits of steady-state model, and that is probably what got me involved.

And I remember talking to Dennis Sciama on a visit I made to Cambridge and he decided to try to convert me to physics. He didn't succeed in converting me to physics in the sense of changing my subject which is what he wanted me to do. But he did in the sense of teaching me, an awful lot of physics. And we used to go on trips. He would drive in his car to Stratford where we would watch Shakespeare plays. Dennis was very keen on Shakespeare and that sort of thing and I got a lot out of that. Sometimes, he used to describe to me his idea about Mach's principle, and when he would drive very fast in his car around steep and curves and when you get thrown to the side of the car, he would say that's the action of the fixed stars you see. Very good at getting himself free of any blame!

But anyway, this was very useful to me to get to these general ideas about physics and it was also useful because he knew people. He was a very good mixer. He talked to anybody on the physics side. He was also Dirac's only graduate student which was interesting, and I think it was important. I went to the second part of Dirac lecture which was on quantum field theory. The first course was on

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quantum mechanics. That was basic. And at one point, he deviated from his normal course to talk about two components spinors, which was very mysterious, because I was told later by somebody who was interested in Dirac's, I think it was *Graham Farmelo*, he said to me it's very unusual for Dirac to deviate from his normal course. And he did, he deviated to talk about two components spinors and I think this was very strange because I had been talking to Dennis Sciama about those spinors. I found two components spinors very confusing. I tried to learn them from a book he'd recommended which was by a man called *Corson* which was almost unreadable. Dirac gave two or three courses on two spinors and it made it completely clear to me. He just put the key points and I just understood them. This was important for me.

Later on, I think I was in my second year as a research fellow, and Dennis again persuaded me to go to a lecture by David Finkelstein, about the coordinates, how you get through what we thought to be a singularity and you learn it's not a singularity it's just what we call a horizon now. And Finkelstein's lecture was very clear on this. I remember being very struck by the fact that what used to be thought to be a singularity at the Schwarzschild radius was not a singularity. Yet you still have the singularity in the middle and so you seem to have a problem. I wonder is it possible that there's a theorem that shows that you cannot get away from having a singularity somewhere. And I thought, well I wonder how you could prove such a theorem knowing nothing, almost nothing, about general relativity.

But then, I thought what do I know that perhaps most people working in general relativity don't know' Aaah, two components spinors! So, I decided to write general relativity in terms of two components spinors. And it worked beautifully particularly understanding the conformal curvature, which was the Weyl curvature, and it made it completely clear in a way that if I had not looked at two component spinors, I would not have got that understanding. It was very important for me that I had these insights into two component spinors from Dirac. And the puzzle I had about them initially, about what could it be, it was taking the square root of a vector: how can you take a square root of a vector?

It didn't make any sense to me, and then I understood how this worked and how when you use the two component spinors, it makes the Weyl curvature so simple. When you write it into tensors, it's very complicated and you can't see what's going on. But

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with two spinors it becomes very simple and very beautiful and this was my route. It took a long time before the singularity theorem. That was several years later because it was in 1958, I think, when the Finkelstein lecture happened, and it was in early 1964 that I had the insight into the theorem.

JJS & JK: So, in the beginning, the idea of 2-spinor attracted you the most?

RP : ⌈I liked to think of a 2-spinor (often referred to by physicists as a 'Weyl spinor') in a very geometrical way, and I realized that, up to an overall sign, a non-zero 2-spinor can be represented as a future-pointing null vector[(5)], referred to as the 'flagpole', together with a 'flag plane' direction through that flagpole. The flag plane would be a null half-plane bounded by the flagpole. This flag geometry can be thought of in the following way. Imagine the Riemann sphere $\mathscr{S}$ of null (i.e. light like) directions at some point O in space-time (see Figure 2.) We are thinking of the geometry in the tangent 4-space of the point O. The flagpole direction is represented by some point P on a sphere of cross-section of the future null cone of O, which we identify with $\mathscr{S}$, and we choose a point P' on $\mathscr{S}$ infinitesimally separated from P. The straight line extended out from P in the direction of P', when joined to O, defines the required flag half-plane. We note that as the point P' rotates about P, the flag plane rotates about the flagpole. The spinor itself is defined only up to sign by this geometry, but we must take note that if P' rotates continuously around P through 2π, the spinor becomes replaced by its *negative*. To reach the original 2-spinor by this procedure, the rotation of the flag plane would have to be through 4π.

(5) A vector pointing along the future null cone.

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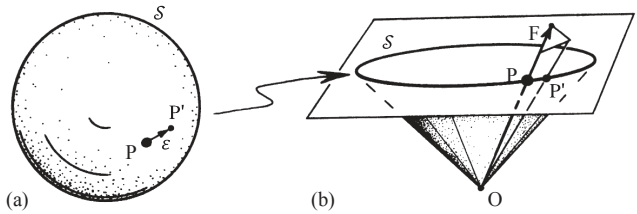


FIGURE 2. — (a) The space of null directions at some space-time point O is represented as a Riemann 2-sphere S. The flagpole direction of a 2-spinor is represented, on S, as the point P. Infinitesimally near to P is P', where the direction $\vec{PP'}$ provides the 2-spinor's flag plane. (b) In space-time terms, the 2-spinor's flagpole is shown as the null 4-vector $\vec{OF}$, where we realize S as a particular 3-plane intersection of the future null cone of O (all this taken in O's tangent 4-space), so that P lies on the line OF. The 2-spinor's flag plane is now seen as the null half-2-plane extending away from the line OF in the direction of P'.

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I had found that 2-spinor methods were surprisingly valuable in giving us insights into the formalism of *general relativity* that were different from those that the standard Lorentzian tensor framework readily provides. Most immediately striking was the very simple-looking 2-spinor expression for Weyl's conformal curvature. Whereas the usual Weyl-tensor quantity C_{abcd}, has a somewhat complicated collection of symmetry and trace-free conditions, the corresponding 2-spinor is simply a totally symmetric complex 2-spinor quantity Ψ_{ABCD}.
[...] I had become interested in the issue of finding solutions of the general equation

$$\nabla^{AA'}\phi_{ABC\ldots E} = 0$$

in (conformally) flat space-time, $\phi_{ABC\ldots E}$ being symmetric in its n spinor indices, the equation being the (conformally invariant) free-field equation for a massless field of spin $n/2$. This equation (together with the wave equation in suitably conformally invariant form) had a particular importance for me, and I believed it to have a rather basic status in relativistic physics. For I had come to the view that nature might have a 'massless' structure at its roots, mass itself being a secondary phenomenon. In around 1961, I had found a formula for obtaining the solution of this field equation from general data freely specified on a null initial hypersurface. I had formed the view that this formula had a certain kinship with the Cauchy

integral formula for obtaining the value of a holomorphic function at some point of the complex plane in terms of the function's values along a closed contour surrounding that point. I had felt that, in some sense, this massless field equation might be akin to the Cauchy-Riemann equations. There had to be an unusual 'complex' way of looking at Minkowski space, I had surmised, in which the massless field equations were simply a statement of *holomorphicity* — but in what sense could this be true?

There was one remaining feature that I felt sure must be represented, as part of this mysterious 'complex' way of looking at space-tine. This arose from a discussion that I had with Engelbert Schücking when I shared an office with him in the spring of 1961 at Syracuse University in New York State. Engelbert had persuaded me of the key importance of the quantum field theory of the splitting of field amplitudes into positive and negative frequency parts. I was not happy with the standard procedure of first resolving these amplitudes into Fourier components and then selecting the positive ones, as not only did this strike me as too 'top-heavy', but also the Fourier analysis is not conformally invariant — and I had come to believe that this conformal invariance, being a feature of massless fields, was important (again, something that had been stressed to me by Engelbert).

I had become aware that for complex functions defined on a line (thought of as the timeline) we may understand their splitting into positive — and negative — frequency parts in the following way. We view this timeline as being the equator of real numbers in a Riemann sphere which, as before, is the complex plane compactified by the single point labelled by '∞', but where the sphere is now being oriented somewhat differently from that of figure 2, with the real numbers now featuring as the equator (increasing as we proceed in an anti-clockwise sense un the horizontal plane), rather than the unit circle. Functions defined on this equatorial circle that extend holomorphically into the southern hemisphere (with usual conventions) are the functions of positive frequency, and those which extend holomorphically into the northern hemisphere are those of negative frequency [see Figure 3].

An arbitrary complex function defined on this circle can be split into a function extending globally into the southern hemisphere and one globally into the northern hemisphere'uniquely except for an ambiguity with regard to the constant part'and this provides us with the required positive/negative frequency split, without any

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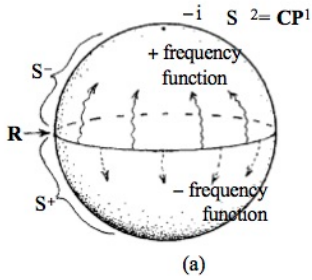


Figure 3. — Circle of positive/negative frequency split

resort to Fourier analysis. I wanted to extend this picture into something more global, with regard to space-time, and I had in mind that my sought-for 'complex' way of looking at Minkowski space should exhibit something strongly analogous to this division into two halves, where the boundary between the two could be interpreted in 'real' terms, in some direct way. This had then set the stage for the emergence of twistor theory!] [6]

JJS & JK: You mean that your reflection was motivated first by the idea of splitting.

RP: Yes, I remember the occasion very well because it was shortly after the assassination of Kennedy, and I was in Austin, Texas. It was a very traumatic occasion. I was there at the time with Engelbert Schücking, Rainer Sachs and Roy Kerr. So, we had a good number of people working on relativity theory there. Engelbert was in charge of the group, I think particularly. In Dallas was Ivor Robinson whom I knew very well and Wolfgang Rindler with whom I wrote my book on spinors later on. So, we were very close. And they were actually at the dinner that Kennedy was going to, and they kept wondering why he is not here. He has not come yet and this is because he had been assassinated. And I remember sitting in my office in Austin and I was there and the next-door office, I think it was Roy Kerr's office. And the phone rang and rang and rang and then had to get off. And in the next office, the phone rang and rang and rang right and then the next office it rang and rang and then somebody picked the phone up. And I saw Ray Sachs coming out and he was white as a sheet. And so, I phoned my wife and I knew the awful news.

[6] Twistor theory as an approach to fundamental physics, pp. 256–260.

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It was several days later when as a way of kind of recovering ourselves. The Dallas people and the Austin people decided to have a holiday to try and recover. And so, they drove in several cars to San Antonio, and we went to the sea.
On the way back, the ladies all wanted to talk, and they had wanted to come in their cars and I was in the car with pista Oszvath[7] , who hardly spoke at all. He's Hungarian. He speaks English but not much, so I came back almost in silence in the car. And I was able to think about Ivor Robinson's construction of null solutions of the Maxwell equations.

⎪ Ivor Robinson, who had taken up a position at what later became the University of Texas at Dallas, had been working on finding global non-singular *null*[8] solutions of Maxwell's free-field equations in Minkowski space-time, where 'null' in this context means that the invariants of the field tensor F_{ab} vanish, i.e. $F_{ab}F^{ab} = 0 = *F_{ab}F^{ab}$ where $*F_{ab}$ is the Hodge dual of F_{ab}. Equivalently, in 2-spinor terms, $\phi_{AB}\phi^{AB} = 0$, which tells us that

$$\phi^{AB} = \kappa^A\kappa^B,$$

for some κ^A. It is not hard to show that the Maxwell equations then imply that the flagpole direction of κ^A points along a 3-parameter family — a *congruence* — of null straight lines, which turn out to be what is called 'shear-free', which means that although the lines may diverge, converge, or rotate, locally, there is no shear (or distortion) as we follow along with the lines.
[...] I became highly intrigued by the geometry of general Robinson congruences, and I soon realized that one could describe them in the following way. Consider an arbitrary spacelike 3-plane E in Minkowski 4-space $\mathbb{M}$. E will have the geometry of ordinary Euclidean 3-space, and each ray of the congruence will meet E in a single point, at which we can determine the location of that ray within $\mathbb{M}$ by specifying a unit 3-vector $\boldsymbol{n}$ at that point, pointing in the spatial direction that is the orthogonal projection into E of the null direction of L there. Thus, we have a vector field of $\boldsymbol{n}$'s within E to represent the Robinson congruence. After some thought I realized what the nature of this vector field must be. The $\boldsymbol{n}$-vectors are tangents to the oriented circles (together with one oriented straight

[7] His name is Istvan Ozsváth known to his friends as Pista.

[8] In the Maxwell field you have two null directions which are the directions along the light cone that the Maxwell field determines and when they coincide this is what's called null.

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line) obtained by stereographic projection of a family of Clifford parallels on a 3-sphere. See Figure 4 for a picture of this configuration, and reference (Roger Penrose, 1986) for a detailed derivation. The large arrow at the top right indicates the direction in which the configuration appears to move with the speed of light by continually reassembling itself in that direction, as E evolves into the future.

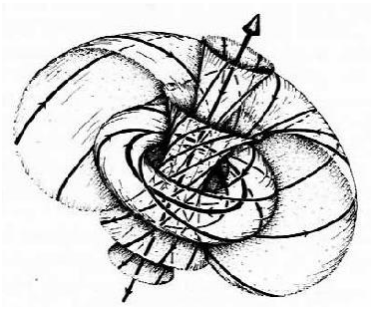


Figure 4. — A picture representing a non-null twistor: stereographic projection of Clifford parallels on a 3-sphere to Euclidean 3-space E. The tangent directions to the circles point in the direction (projected into E) of the rays of a Robinson congruence. By continuingly reassembling itself the entire configuration travels with the speed of light, as E evolves in time, in the direction of the large arrow at the top right.

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By examining this configuration and counting the number of degrees of freedom that such configurations have, I realized that the space of Robinson congruences must be 6-dimensional. Moreover, it was reasonably clear to me that by its very mode of construction, this space ought to have a *complex structure*, and so must be, in a natural way, a complex 3-manifold. Within this space would lie the space of special Robinson congruences, each of which would be determined by a single ray (namely L). The space of rays in $\mathbb{M}$ is 5-real-dimensional, and it divides the space of general Robinson congruences into two halves, namely those with a right-handed twist and those with a left-handed twist. The complex 3-space of Robinson congruences, which came to be known as 'projective twistor space' appeared to be just what I believed was needed, where the 'real' part of the space (representing light rays in $\mathbb{M}$, or their limits at infinity) would, like the 'real' equator of the Riemann sphere described before (Figure 3), divide the entire space into two

halves. This indeed appeared to be exactly the kind of thing that I was looking for!⌉[(9)]

Let me say it differently, Ivor wanted to find solutions that were non-singular everywhere and he had discovered these solutions which twisted around in a complicated way. And I was trying to understand this. And I think I realized that these were Clifford parallels. This is the three sphere and they were the circles on three spheres linking each other. But, you see when you take a light ray and you somehow push it into the complex instead of having all the light rays meeting a light ray, so you have these twisting congruences. So, I thought, well what is the dimensionality of these Clifford parallel configurations and I counted the dimensionality. And I realized it was six. One less than the dimensionality of light rays. So, if the light rays represent the real points and the Robinson twisting congruences represent the complex points in a sense, you add one dimension. And they can twist right-handed or left-handed, so you have a space which is divided into two halves. And the real parts, you see directly is the light rays and these twistors as I call them were these twisted in the complex part. So, I realized this was what I was looking for. But it took me a long time to realize. I first had to see how Maxwell's equations do, how the massless equations do. And I had, first of all, a way of doing it which is complicated. But, then I realized you could use quantum integrals to see how to split them into positive and negative frequencies. And it was not until I talked to Michael Atiyah and understood that it's cohomology. And the first cohomology splits into two halves positive and negative frequency and it did exactly what I was looking for. That took about three or four or five years, I can't remember. I was still stuck for a long time because this is only special relativity and all the people I was working with, were working on general relativity. What's the point?

Engelbert and Roy Kerr understood it, in a certain sense. It was very strange; the idea of the twistors became clear to me the next day after I returned. I heard Roy Kerr explaining something to Ray Sachs, and they were getting very excited. I came into the next office which was Roy Kerr's office and said what on earth is this, what are you talking about' And Roy was saying I will explain what it is. Roy said, 'I have this way of generating all the sheer free twisting congruences'. These were the things that Robinson had found a special

[(9)] Twistor theory as an approach to fundamental physics, pp. 260–262.

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one. Roy said he can find the general ones. And I asked well what are they ? He wrote down these formulae and said well you find an analytic function of these particular combinations of coordinates. I looked at them and I thought my god those are twistors.
So, his theorem was taken as an arbitrary holomorphic function in twisted space and this gives you a general twisting congruence, sheer free congruence. So that was the first application of twisted theory, the very next day. Just that is the coincidence.

JJS & JK: In the approach based on complex structures and more specifically holomorphy, we reason in terms of neighbourhood and not in terms of points. Thus, we can say that the holomorphic philosophy induces a new nonlocality in quantum mechanics, where the complex topology will be the fundamental structure. Do you agree with this?

RP: Yes, that's right. Yes, that certainly was a strong motivation. Holomorphic was a very strong motivation. I mean there is a nonlinearity and nonlocality.

⌈The twistor space that is referred to here, whose individual points represent light rays in space-time $\mathbb{M}$, is denoted by $\mathbb{PN}$. Thus, the point Z in $\mathbb{PN}$ corresponds to the locus Z (a light ray) inside $\mathbb{M}$ and the point R in $\mathbb{M}$ corresponds to the locus R (a Riemann sphere) inside $\mathbb{PN}$.
Now, an essential part of the philosophy of twistor theory is that ordinary physical notions, which normally are described in space-time terms, are to be translated into an equivalent (but non-locally related) description in twistor space. We see that the relationship between $\mathbb{M}$ and $\mathbb{PN}$ is indeed a non-local correspondence, rather than a point-to-point transformation. [...]
This locus R inside $\mathbb{PN}$, describes the 'celestial sphere' (total field of vision) of an observer at R, the celestial sphere of R being regarded as the family of light rays through R. As has been noted above, this sphere is naturally a Riemann sphere which is a complex 1-dimensional space. Thus, we think of space-time points as holomorphic objects in the twistor space $\mathbb{PN}$, in accordance with the complex-number philosophy underlying twistor theory. [...]
The space $\mathbb{PN}$, of light rays, does not itself immediately fit in with the 'holomorphic philosophy', however, because it is not a complex space. $\mathbb{PN}$ cannot be a complex manifold because it has five real dimensions and five is an odd number, whereas any complex n-manifold must have an even number, $2n$, of real dimensions. But if

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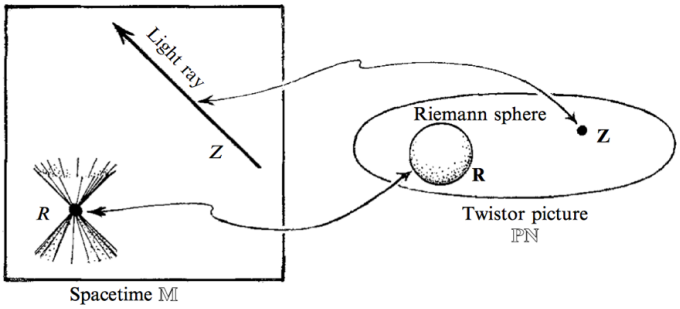


Figure 5. — A light ray Z in Minkowski spacetime $\mathbb{M}$ is represented as a single point Z in the twistor space $\mathbb{PN}$ (projective null twistor space); a single point R in M is represented by a Riemann sphere R in $\mathbb{PN}$ (this sphere representing the 'celestial sphere' of light rays at R).

we make our 'light rays' a little more like physical massless particles, by assigning them both spin (actually helicity) and energy, then we get a six-dimensional space $\mathbb{PT}$, which actually can be interpreted as a complex space of three complex dimensions. The space $\mathbb{PN}$ sits inside $\mathbb{PT}$, dividing it into two complex-manifold pieces $\mathbb{PT}^+$ and $\mathbb{PT}^-$, where $\mathbb{PT}^+$ may be thought of as representing massless particles of positive helicity and $\mathbb{PT}^-$, massless particles of negative helicity; see Figure 6. However, it would not be correct to think of twistors as massless particles. Instead, twistors provide the variables in terms of which massless particles are to be expressed. (This is comparable with the ordinary use of a position 3-vector x to label a point in space. Although a particle might occupy the point labelled by x, it would not be correct to identify the particle with the vector x).

It is noteworthy that this feature of space-time geometry is specific to the particular dimension and signature actually possessed by the physical space-time we are aware of. Indeed, the fact that the Riemann sphere plays an important role as the celestial sphere in relativity theory requires space-time to be 4-dimensional and Lorentzian, in stark contrast with the underlying ideas of string theory and other Kaluza-Klein-type schemes. The full complex magic of twistor theory proper is very specific to the 4-dimensional space-time geometry of ordinary (special) relativity theory and does not have the same close relationship to the 'space-time geometry' of higher dimensions.

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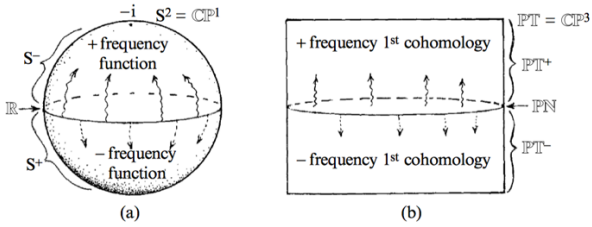


FIGURE 6. — An analogy between the Riemann sphere $S^2(= CP^1)$ and projective twistor space $\mathbb{PT}(= CP3)$. (a) A complex function (i.e. a '0th cohomology element'), defined on the real axis R of S^2, splits into its positive frequency part, extending holomorphically into what is here depicted as the northern hemisphere S^-, and its negative frequency part, extending into the southern hemisphere S^+. (b) A 1st cohomology element, defined on $\mathbb{PN}$ (and representing a massless field) splits into its positive frequency part, extending holomorphically into the top half $\mathbb{PT}^+$ of projective twistor space, and its negative frequency part, extending into the bottom half $\mathbb{PT}^-$.

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The twistor perspective leads us to a very different view of 'quantized space-time' from that which is often put forward. It is quite a common 'conventional' viewpoint that the procedures of quantum (field) theory are to be applied to the metric tensor g_{ab}, this being thought of as a tensor field on the space-time (manifold). The view is expressed that the *quantized metric* will exhibit aspects of 'fuzziness' owing to the Heisenberg uncertainty principle. One is presented with the image of some kind of four-dimensional space that possesses a 'fuzzy metric' so that, in particular, the null cones — and consequently the notion of *causality* — become subject to 'quantum uncertainties'. Accordingly, there is no classically well-defined notion of whether a space-time vector is spacelike, time like, or null. This issue had posed foundational difficulties for any too-conventional 'quantum theory of gravity', for it is a basic feature of QFT that causality requires field operators defined at space-wise separated events to commute. If the very notion of spacelike' is subject to quantum uncertainties (or has, itself, become a quantum notion), then the standard procedures of QFT, which involve the specification of commutation relations for field operators, cannot be directly applied.

Twistor theory suggests a very different picture. For now, the appropriate 'quantization' procedures, whatever they may be, must be applied within twistor space rather than within the space-time. By analogy with the way that, in the conventional approach, 'events' are left intact whereas 'null cones' become fuzzy, in a twistor-based approach it is now the light rays' that are left intact whereas 'events' become fuzzy.⌋[10]

JJS & JK: Your attachment to complex and sophisticated structures also implies a marked geometric aspect which shows that the geometric representations of mathematical structures are an essential part of your intellectual discipline. This highlights another key mathematical concept, that of conformal transformations.
The demonstration of the particularly sophisticated character of complex structures gives an a priori argument on the privileged mathematical character of these structures which you call holomorphic philosophy, can you elaborate on this philosophical conception?

RP: I use the geometric approach all the time and holomorphic geometry in particular. That's right. Well, you see my algebraic geometry upbringing was quite useful to me because I knew the Klein correspondence for instance. That was very basic to things like I do, and I had some feeling for algebraic geometry although I didn't have the feeling that the people who were the mainstream there at the time like Michael Atiyah who was a contemporary of mine. I knew Michael. We were both students under Hodge at the same time. But he was rather intimidating because his knowledge of algebraic geometry completely surpassed mine. He knew what was going on completely and I was very bewildered by what was going on.

⌈Twistor theory, as we have just seen, initially exploits a manifestation of complex number magic different from those to be found in quantum theory, namely the classical feature of space-time geometry that the celestial sphere can be regarded as a Riemann sphere, which is a 1-dimensional complex manifold. The idea is that this provides us with hints as to Nature's actual scheme of things, which must ultimately unify space-time structure with the procedures of quantum mechanics.⌋[11]

⌈For the space-time representation of the wave function of a free massless particle of general spin, the Schrödinger equation translates to a certain equation known as the massless free-field

[10] The road to reality : a complete guide to the laws of the universe, pp. 964–966.
[11] The road to reality : a complete guide to the laws of the universe, p. 967.

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equation⌋[12], we have seen at the beginning of our discussion:

$$\nabla^{AA'}\phi_{ABC\ldots E} = 0.$$

As I said before, $\phi_{ABC\ldots E}$ being symmetric in its n spinor indices, the equation being the (conformally invariant) free-field equation for a massless field of spin $n/2$. ⌈It turns out, remarkably, which there is an explicit contour-integral expression that automatically gives the general positive-frequency solution of the above massless free-field equations simply starting from the twistor function $f(Z^a)$. In fact, the expression also works perfectly well without this positive-frequency requirement, though the requirement is easily ensured in twistor formalism.⌋[13]

⌈The very existence of such an expression strikes me as being somewhat magical. The massless field equations seem to evaporate away in the twistor formalism, being converted, in effect, to *pure holomorphicity*, [and] when we examine this expression more carefully, we find that there is an important subtlety about how a twistor function is to be interpreted, and this relates in a striking way to the positive/negative frequency splitting of massless fields. This subtlety is also crucial to how twistor functions manifest themselves in active ways and provide us with curved twistor spaces. What is this subtlety? It is that twistor functions are not really to be viewed as 'functions' in the ordinary sense, but as what are called elements of *holomorphic sheaf cohomology*.⌋[14]

⌈What is sheaf cohomology? The ideas are fairly sophisticated mathematically, but actually very natural. [...] Now think of a manifold built upon which the transition functions f_{ij} differ from the identity by only an infinitesimal amount. [...] In fact, the class of function f_{ij} that one may be concerned within cohomology theory can be extremely general. In twistor theory, one normally deals with holomorphic functions. This gives us the notion of 'holomorphic sheaf cohomology'. [...] An important feature of cohomology is that it is essentially non-local. [...] This non-locality, for twistor functions, tells us that there is no significance to be attached to the value attained by f_{ij} at some particular point. We can, indeed, restrict down to a small enough open region surrounding that point and find that the cohomology element disappears completely. This non-locality, as exhibited by twistor functions (regarded as first

[12] The road to reality : a complete guide to the laws of the universe, p. 985.
[13] The road to reality : a complete guide to the laws of the universe, p. 986.
[14] The road to reality : a complete guide to the laws of the universe, pp. 986–987.

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cohomology elements) is tantalizingly reminiscent of the nonlocal features of EPR effects and quantum entanglement. In my opinion, there is something important going on behind the scenes here that may someday make sense of the mysterious non-local nature of EPR phenomena, but it has yet to be fully revealed, if so.
We are to think of this 'cohomology element' as being a 'thing' defined on space Q, which is a bit like a function defined on Q, but which is fundamentally non-local. One example of this kind of 'thing' is actually an entire (complex) vector bundle over Q. We recall that, in the definition of a bundle, that part of the bundle lying above a small enough region of the base space (here Q) is 'trivial', in the sense of being just a topological product. This is an example of the fact that if we restrict our first cohomology element down to a small enough region, it becomes 'trivial' also; *i.e.* it vanishes. Thus, the 'information' expressed in a cohomology element is something of a fundamentally non-local character. [... For example, consider the case of the] drawing of an 'impossible object' sometimes referred to as a 'Tribar' (Figure 7).

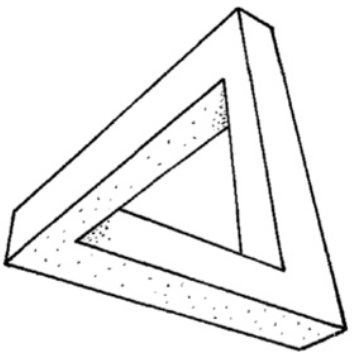


Figure 7. — Tribar

It is clear that the '3-dimensional object' that the drawing apparently depicts cannot exist in ordinary Euclidean space. Yet locally there is nothing impossible about the drawing. The impossibility is non-local and disappears if one considers a small enough region in the drawing. In fact, this notion of 'impossibility' in such a drawing can be expressed as a specific cohomology element. It is a relatively simple type of cohomology, however, where the functions f_{ij} are taken to be constants. [...]

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There are many applications of these ideas in mathematics, not all of which are concerned with holomorphicity. The 'sheaves' that one is mainly concerned within twistor theory are those expressed in terms of holomorphic functions, and there is a special magic in cohomology theory in this particular context. (Roughly speaking, the term 'sheaf' refers to the type of function that one is concerned with, but the sheaf notion actually applies considerably more generally than just to ordinary functions) There are many other types of use of cohomology, including some that have importance in the study of the Calabi-Yau spaces that occur in string theory, for example. Also, there are several other quite different ways of defining sheaf-cohomology elements, all of which can be shown to be mathematically equivalent, despite their very different appearances. In my opinion (sheaf) cohomology is an excellent example of a Platonic notion, where, like the system of complex numbers $\mathbb{C}$ itself, it seems to have a life of its own' going far beyond any particular way in which one may choose to represent it.[(15)]

JJS & JK: How do you describe the role of Cohomology and fiber bundle in physics?

RP: Well you can see how it works, partly. It's a puzzling story and certainly, twistor theory has not properly come to terms with general relativity.
You see, to me, the non-linear graviton was a very remarkable thing. In twistor theory, you can describe massless particles of any spin. And it works very well for electromagnetism because you've got spin plus one and spin minus one. Let's call it helicity so you've got the right-handed and the left-handed spin. But in twistor theory, one of these has a homogeneity which is zero and the other has a homogeneity which is minus four. The homogeneity is $-2S-2$, S is the helicity.
And so $2S$ is either plus $+2$ or -2, and it gives you either 0 or -4. So, the right-handed ones were -4 and the left-handed ones were zero. It looks very lopsided. Twistor theory was very lopsided. I began to think well maybe it is very lopsided, maybe physics is very lopsided. But then when you look at the graviton you've got the left-handed graviton which is a $+2$-homogeneity function and the right-handed graviton which was a -6.

[(15)] The road to reality : a complete guide to the laws of the universe, pp. 987–992.

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Particle	Helicity	Homogeneity
Graviton	$+2$	-6
Photon	$+1$	-4

Very lopsided, but for the $+2$ one, you could see how to take cohomology. We will think of the cohomology in a very simplistic way. Twistor space is basically two parts that are glued together. So, you have, one half of the top and half the twisted space and the other part and there's a glue (see Figure 6). You glue the two parts together and that gluing is done by means of a $+2$-degree homogeneous twistor function. And the gluing gives you a non-linear version of cohomology. If you just look at infinitesimal shift of one to the other, then that's given by a $+2$-homogeneity function. I can't explain this without going into detail.

It's cohomology as long as it's just an infinitesimal deformation. But if it's a real finite deformation then that gives you a general deformation of the twistor space. And you can look for the lines in this deformed twistor space. I should say that I give a lot of credit to Ted Newman, because Ted Newman was a very close colleague of mine. He worked in general relativity in Pittsburgh. I knew him well first, I guess, when I was in Syracuse because he was there for six months period or more. This was in 1961 when there were a lot of very good relativists, among them was Ted Newman. With Ted, we worked on spin coefficients. Ted had the idea of what he called H-space. It's a very ingenious idea.

If I had to explain it, I would have to talk about infinity first. I have found some insight initially when I went to a lecture by Andrzej Trautman when I was in Syracuse. Andrzej Trautman was one of the very distinguished polish physicists. And he worked with Ivor too. Andrzej Trautman was also important in understanding what was going on in particle physics, which is you're really looking for connections and bundles. And I think he was really the first to understand that. Although I'm not sure how much credit he gets from that now. He was the first to understand what you're really doing in strong interactions. I think the weak interactions are a little bit obscure. Electromagnetism was much more well-known. When you're looking at strong interactions, you're looking in fact at connections and bundles also. But, this is not what Andrzej Trautman was talking about in Syracuse.

He was talking about gravitational radiation and he was looking at the leading term in the radiation. He saw that the field was null

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when you looked at it with spinors. This is what I was doing after the Finkelstein talk, when I started to worry about how two spinors could be applied in general relativity.

I thought about the Weyl curvature and I realized that it was completely symmetrical with two spinors. It had to factorize into four directions, so you have these four directions along the light cone, and the special cases are when they converge in different ways. So, you can have all separate or two converging or three converging or two and two. So, these are the different ways, this was a nice way of characterizing. This was done completely differently by Petrov classification.

I didn't know about these directions. But I was realizing that this was important by thinking about general relativity. This was actually when I came back from Syracuse. Andrzej was talking about the null case. This is where they all coincide. So, when you look at the leading term, when you go right out to infinity, and what is the dominant form of gravitational radiations where they all coincide, you have these all null directions along the light cone all pointing together outwards with the wave. So, I learned that from Andrzej's talk. On the other hand, I'd heard about Ray Sachs theorem about how the leading term is like, that's $1/r$, $1/r^2$, $1/r^3$, $1/r^4$. We used to call this the peeling-theorem as you come back they peel off one after the other like this.

So, when I got back to London, I was thinking about these things and I realized that this peeling-theorem came out immediately if you realize that infinity was conformally smooth. You see the history here was that, after Andrzej's talk, I began to think: can you do this another way' I don't like all these complicated calculations. I'm very impressed by what he could do. But I'm not good at doing complicated calculations. I'd much rather think about it geometrically, what's going on? And so, I remember talking to Engelbert Schücking and I said well what we can understand about' Maybe we could do a conformal transformation, and I thought about conformally taking an inversion from a Schwarzschild solution. And I realized then that when you do invert it, infinity is singular. So, the point at infinity is singular, so it doesn't work! But Engelbert told me about Maxwell's equations being conformally invariant.

And I thought that's very beautiful. You have this conformal structure. You can change the scale factor in different places and this is another reason for being interested in the conformal structure. This was important for me. But, when I got back to England and began

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to think again about Ray Sachs peeling property, I realized that I was looking in the wrong place. If you look at space like infinity, it's singular yes. But how about a null-infinity where you go out along the light cones. And I thought at first, it must be singular. And then I looked at it and no, it's not! That's only $1/r$. It's not singular. That was an amazing revelation to me it's not singular and this gives you the peeling, all the leading term $1/r$, $1/r^2$, $1/r^3$. So that's the peeling property of Ray Sachs, simply a feature of the gravitational field, is finite. You have to think of it as a spin 2 field. And that spin 2 field is finite at infinity and that's facts is Sachs peeling. So, that was a big step for me and the right way to look at gravitational radiation. You do the conformal map and infinity is this cylinder.

That was a good way of thinking about radiation and you could understand Bondi's theorem about mass loss. It was a very nice way of looking at it. Looking at energy and momentum. And energy-momentum fits together as an energy momentum four-vector and you get momentum conservation as well as energy in terms of the Bondi formula and the generalization for momentum. But what about angular momentum?

Ted Newman started to think about what you needed basically was a surface of cross-section of this cylinderized infinity, which didn't shear. The shear at infinity looks like a saddle. The surface has intrinsic curvature to it so it's like a saddle circle. So, your curvatures in two directions are different. And it can be negative in one. And that's what gradation gravitational radiation does. You started off with something it looks spherical and it makes it saddle surface and you need it not to be saddle surface to do the angular momentum properly. So, what did Ted do, he says okay you move up your cylinder in complex direction, you don't just take a real time, you take a time that is complex, and you make get rid of the saddle surface. Crazy, completely crazy idea. He called this space H-space, the Heaven space. He had a nice Jewish joke. The Heaven space is where the good cones go. So, it's a good cone, you see, it's meant to be a cone. But when you take it with the sort of New York accent, it sounds like goy, where the good cones go. This was his Jewish joke, a New York tourist joke.

JJS & JK: Another subsidiary question concerns the development of spectral sequence and cohomological sequence in twistor theory.

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RP: Well, I think it does. But it's hard to understand and I'm sure it does it in ways that need to be understood as the next level. I think that's probably what it is. It's a pity Michael Atiyah isn't around because I think he would have revealed in this. You see, I think the reason we've never understood it properly is that we haven't gone to the next level. You see, in the Dirac equation, he went from two spinors to four spinors. But that's not quite what you do with twistors. You go from the two spinors to the four spinors of the conformal group and those are the twistors. Now what you have to do is to go from the four spinors of the conformal group to the eight spinors of the octonions, the spin octonions and this is a new realization. I think it's a new realization. This is the quantized version. I never noticed this before. I'd known about the quantized twistors for years, but I never looked at the group which they satisfy. And this is G_2^*. So, it's that simple. It's the split form of this simple group G_2. And it's just exceptionally simple; in the Coxeter-Dynking diagram you have a triple connecting line.

JJS & JK: The group G_2 appears even in string theory and many other frameworks.

RP: I wouldn't be surprised if it doesn't have a role. I've never looked at these things. It is a shame. Well, I was too slow. I've been too slow in thinking about things. I should have thought of this idea three years earlier.
I'm always slow. From when I was at school, I was much too slow. I was too slow with my non-periodic tiling, just too slow because my father and Escher died just before I thought of the non-periodic tiling. Both of them would have enjoyed them so much. I'm afraid that I missed the boat twice, three times.

JJS & JK: People recently tried to use twistors in string theory.

RP: String theory is a difficult one because it's certainly been very motivating by physical ideas. In a certain sense, it's still incomplete. The string theorists could say well yes that's still incomplete too.
The trouble is that they don't pay much attention to dimensionality. And the problem is if you have a space in which your functions exist, a function of many variables then you have many functions, or a certain amount of functional freedom which in physics is very restricted. This functional freedom is really function of three variables. And it's because it depends on the space, you find that the temporal behaviour is governed by the spatial behaviour. If you

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want to deviate from that you have to see where all the extra functional freedom disappears. In string theory, people don't address that question. They try to say, well the energy is enormously large and to excite these extra degrees of freedom you need to have too much energy. But I think that argument is just wrong because the energy is not all that large. The energy is certainly much less than the energy of the earth in its orbit around the sun.
The trouble is this energy is for the entire universe and the earth going around the sun has far more energy than you would need to activate these degrees of freedom. It's a point that has never been addressed by the string theorists. I mean, I tried to make this point, particularly in my book 'The road to reality'. And I had never seen anybody in the string theory world address those arguments.

JJS & JK: Did you discuss this issue with Edward Witten? He is interested to make the bridge between string and twistors theories.

RP: You see twistor theory is not like those theories, because you've got to stick with space-time, which has the dimensionality we see. Now, you could say that twistors space is six dimensional or eight dimensions. Well, the projective space is three complexes dimensional, so it's already six dimensional. But that's not the point. The points are these Riemann spheres if you like, and they have to be restricted to the space $\mathbb{P}N$ which is the five-dimensional subspace and then you have a four-dimensional family of these things. So, it's the right number of dimensions. So, with twistor theory, the dimensionality comes out right, but people don't pay much attention to that.
But there is a weakness in twistor theory right from the start. I mean it's been a combination of puzzling issues and things working in a way which I had much better than I had anticipated. In a way, I hadn't anticipated.
I always regretted having my conventions the wrong way around. Now it wouldn't have mattered except that when Edward Witten got interested in twistor theory. He went to my original paper where all the conventions are wrong, and he took the wrong conventions.

Most particularly what concern the twistor points along the light cone or the null twistor. Now you see, I thought the points were in terms of a vector but it's not a vector it's a co-vector. I realized much later when you understand the quantum mechanics and everything. It's a momentum, it's not a vector it's a co-vector. But I didn't know

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that at the time, so I made it a pointing vector. It's really a dual vector. I corrected it afterwards in my next paper, but E. Witten didn't refer to my next paper maybe. I've always regretted that the upstairs (indices) was should be downstairs and downstairs should be upstairs.

JJS & JK: Did you have the occasion to tell Edward about this?

RP: Oh, yes! but that didn't get anywhere. But you see, I am fussy about signatures. And working in Lorentzian geometry, I believe you have to have one time and three spaces.
Very clear. Well Riemannian geometry has four spaces. Now many people who work in differential geometry in Oxford work with four $(++++)$. In fact, I worked with self-dual and anti-self-dual. That was fine for them because you can have real ones which are $(+++)$ signature, which is self-dual. Whereas Lorentzian, they've got to be complex. Now in twistor theory, Atiyah and his group also picked up on twistor. They did important things with Richard Ward who was a student of mine and who developed these ideas. His ideas were picked up on by pure mathematicians. They did a lot of very interesting work on Yang-Mills theory. But this means in the twistor theory, they're not using a $3-1$ signature i.e. you're not using a space-time signature. But what Witten did, was a space-time which has two times and two spaces and then you make your twistor theory real. With the Lorentzian space-time signatures, the twistor space is $2-2$ signature. With the positive definite space-time structure, your twistor space is quaternion. It's quaternionic twisted theory. With the Witten type, he has two times in two spaces and the twistors theory is real. It's a real space-time.
And I find this is going in a completely different direction. I can see why he did it. He did it because he wanted to be able to talk about delta functions and step functions and polyhedral. And this kind of notion which if you're stuck with holomorphic, you can't do. And you've to look at hyper functions instead of those functions. And hyper functions are functions where you look at the analytic approach which I think is the right one. But it's more complicated and if you want to look at things where you can talk about general functions which can have steps and delta function, then you face not discontinuous and not smooth and so on. Okay, you can make twisted theory real but that means a space time that has two spaces and two times dimensions. To me, that's not physics theory. It's too removed from the physics.

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You see, I think the idea is that you do the physics in the wrong signature and then you do analytic continuations and you continue back to the right signature. I mean this goes back to these ideas for stationary space-time. It's quite useful to make the time imaginary, but it's very limiting if you want to think about physics generally. Because I mean your time is not such, time is just real time. It's all right if it's stationary but if it's a general space-time, this doesn't work.
So somehow, you're going into, in my view, the wrong direction. You may find that doing path integral or something like that is easier, because you don't have so many horrible divergences, if you're trying to minimize things and paths. But, in the Lorentzian space, it's a mess because they're not bounded, you can make them zero and the inequalities don't work somehow. This approach is limited because you're not doing what the physics is telling you to do.
You're doing something where the mathematics is attracting you in a certain direction which is limited. And if you're trying to do things like path integrals in a positive definite space where it may make some sense and then you try eventually to do a Wick rotation or something which means you change, your make things analytic and then you rotate back. This is not really, what physics is telling you to do. You're being motivated by things that you find are convenient mathematically but, in a way, not telling you where the physics is going. This is if I can express myself in a way which is perhaps slightly rude.

JJS & JK: You mentioned that all the time you were appealed by finding discrete numbers and complex analytic approaches. Do you think this new paradigm will play a role in the overall picture of physics in the future?

RP: It's a good question. You see, they were more or less equal and when I was trying to think about physics in a deep way. I was equally divided between combinatorial ideas and complex analytic ideas. And I produced this spin network scheme which is purely combinatorial. And I played along with that. I'm not sure how far it went. But I remember, I was very curious, because when I talked about this lecture I heard given by David Finkelstein, Dennis Sciama had driven me to London King's college and this converted me to work in general relativity. But David Finkelstein was working in relativity and I explained to him spin networks. And he then

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went on to work on combinatorial physics. So, we swapped subjects. I was working in combinatorial physics and he was working on general relativity.

⌈ My own particular goal had been to try to describe physics in terms of discrete combinatorial quantities, since I had, at that time, been rather strongly of the view that physics and space-time structure should be based, at root, on discreteness, rather than continuity. A companion motivation was a form of Mach's principle, whereby the notion of space itself would be a derived one, and not initially present in the scheme. Everything was to be expressed in terms of the relation between objects, and not between an object and some background space.

[I had concluded] that the best prospect for satisfying these requirements was to consider the quantum-mechanical quantity of total spin of a system. 'Total spin' is defined by the scalar quantity $j(= 1/2n)$ that measures the amount of spin as a whole, rather than a particular component of spin in some direction, measured by a quantity m. (The letters 'j' and 'm' are those commonly used in the discussion of quantum-mechanical angular momentum, taken in units of $\hbar$, where m ranges, in integer steps, between the integers or half integers $-j$ and j.) [...] Moreover, though direction-independent, n is nevertheless intimately related to directional aspects of space. It had seemed to me that total spin, as measured by the natural number n, was an ideal quantity to fix attention upon if one were interested in building up, from scratch, some discrete combinatorial structure that leads to a notion of actual physical space. As a further ingredient, if one sets things up in the right way, one could exhibit the quantum-mechanical probabilities as being pure probabilities, not dependent in detail on the way in which different parts of a physical apparatus might be oriented with respect to other parts. [...]

This was the sort of idea I had for getting pure probabilities, and I had formed the opinion that any such probability had to come out as a rational number (since it would amount to Nature making a random choice of some kind between a finite number of discrete possibilities). [...] All the units in a particular spin network are imagined as being initially produced from initial 0-units (although this would not be normally expressed explicitly in the diagram), so there is no bias with regard to any particular spatial direction. Subsequently, various pairs of units may then be brought together to form single units, and the spin values for the resulting units are

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noted. Individual units are also allowed to split into pairs of units. [...]
However, for the original spin-network theory, there was to be no actual background space-time presupposed. The idea was to build up all the required spatial notions simply from the network of spins and from the probabilities that arise (and these can be computed using quantum mechanical rules) when two units are brought together to make a third. A particular feature of these spin networks is that, at each such vertex, exactly three lines come together. This leads to uniqueness in the probability calculations. The topological (graph) structure of the spin network, together with the specification of all the spin numbers on the lines, is all that is required.
I developed an entirely combinatorial ('counting') procedure for calculating the required probabilities (which, in fact, are all rational). The rules originally come from the standard quantum mechanics of spin, but we can then 'forget' where they come from and simply consider the spin-network system as providing a kind of 'combinatorial universe'. It is then possible to extract the notions of geometry (ordinary Euclidean 3-geometry in this case) by considering spin networks that are 'large' in an appropriate sense. The picture is that a unit of large spin might be considered to define a 'direction in space' (to be thought of as like the axis of spin of a tennis ball, for example). We can envisage measuring the 'angle between the rotation axes' of two such large units by, say, detaching a 1-unit from one and attaching it to the other. The joint probability that one spin goes up while the other goes down, in this operation, gives a measure of the angle between the spin axes.
This almost works as it stands, but not quite, and a further ingredient is required. What is additionally needed is a means to distinguish the 'quantum probability', coming from the angle of spin axes between the large units, from the 'probability through ignorance' that can come about simply because of insufficient connections between the two large units. [...]
It will be seen that the underlying motivation behind the spin networks that I originally had is very different from that underlying the loop variable approach to space-time quantization, there being no actual place for gravity in the spin networks, as originally put forward. Of course, there is something very much in common between the two programmes, because, in each case, one is trying to break down the notion of space into something more discrete and

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quantum-mechanical. There is, however, the important difference that in the loop-variable context, the quantity n is really an *area* measure, rather than the spin measure of the original spin networks. [...] I regard as a necessary feature of the correct quantum gravity union that it must depart from standard quantum mechanics in some essential way, so that (state-vector reduction) $\boldsymbol{R}$ becomes a realistic physical process, [...] but my original use of spin networks did not address such metric issues nor, in fact, any aspect of gravity at all, the spin numbers n referring to *angular momentum*. However, my original ideas demanded that each of these numbers must be, in effect, the result of an individual measurement of total spin value (action of $\boldsymbol{R}$ at each edge), where probabilities arise in the bringing together of two units to make a third. If $\boldsymbol{R}$ is an objective gravitational phenomenon, then the involvement with gravitational processes would have to enter at this stage. [...] In that case, it is not possible to separate gravity from the probability issues of spin-network theory. It may be that the full combination of loop-variable and spin network ideas will need to incorporate state reduction into the formalism.⌋[(16)]

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§ 3. — Cosmology.

JJS & JK: You mentioned that after attending Finkelstein's lecture, you started to get interested in the problem of singularity.

RP: Yes, I was starting to think not in a way which was well. I learned enough about the Weyl curvature that was important to know about. Because it was so simple for spinors. I think people were put off because you write the conditions down in tensors and it's messy. And so, they say, oh look I won't bother with that unless they're very good at computations and then they're happy.
But with spinors, it's just simple. For, it's all the same symmetrical, the equation, the Bianchi identities. It's just two wave equations and the relationships with other spins are just beautifully clear. I'm trying to write something even now on *CCC* with Christopher Meisner which uses a theorem which I'm just realizing how easy it is. Looking at week field gravity in terms of two spinors is so much easier than if you're using ordinary methods. And you can talk

[(16)] The road to reality : a complete guide to the laws of the universe, pp. 947–955.

about the conservation laws of energy-momentum much more easily than you can if you try to write it all in conventional notations. There is a theorem. Let me just tell you this much, I don't think Christopher Meisner wants me to talk about it too much because we're still writing a paper. You see in Maxwell's theory, you have a nice Gauss law. Let's think you're in space-time. So, you've got four-dimensional space time and you've got a charge current going through in space time surrounding these by two spheres. Now you do integral over the 2-spheres and you can work out the electric charge and the magnetic charge if there was a magnetic charge. So, the electric charge is an integral over the 2-sphere and the magnetic charge is zero. Now that's just to spin one. In two spinors, that's two indices, how about four indices? Okay then look at spin two. And you have a quantity that changes the spin which moves you up and down into spin and that's a twistor. Twistor moves up by spin half so if you want to move up from spin one to spin two, you need a two-index twistor. That two index twistor converts the Gauss integral into a four integral and that four integral tells you the energy momentum inside the 2-sphere. Well, we can do that. I mean Ray Sachs wrote a paper where you do that sort of thing. But you can see immediately what this is because the twistor is the spin raising or spin lowering. Or I should say helicity lowering operator and the dual twistor is a basically raising operator.

JJS & JK: This gives us a connection with *CCC*. Anyway, we will ask you later on about the *CCC* and twistors theories.

RP: It's a beautiful connection, that's right.

JJS & JK: Another question concerning the singularity, because it seemed that you didn't share the same conception about the singularity with Stephen Hawking, right?

RP: That is true in a certain sense because the terminology was different. It is true that it appeared in the title of the paper. I think it was called 'space-time singular.' I can't remember the title of my paper, the one that got the Nobel Prize. Certainly, I didn't mention black holes because that terminology was not used then. "Gravitational collapse and space-time singularities", I think I did use the word 'singularities'.
But mainly the argument in the actual paper, I just said well look, there's a problem if the energy flux across that, light rays is never negative, maybe this that you have no Cauchy-surface, maybe you

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have non-completeness or something. I don't think I'd call it singularity. But of course, the idea is why does it stop' Why does your space-time stop' Well probably because curvature becomes infinite, but it doesn't say that. Now you see, when I gave my talk explaining my theorem at King's college in 1964, I remember J.L. Synge was there. He was a wonderful Irish relativist and he had written two books using a very geometrical point of view. So, I was very pleased he was there. He was not normally in London, but he went to my talk and I was very proud to have J.L. Synge present in my lecture not Stephen Hawking.

Stephen Hawking was not there, despite what the movie seems to imply. However, I gave a talk and Dennis Sciama heard about it. He was in Cambridge at the time. And so, he invited me to give a repeat talk in Cambridge. It was sometime in 1965. I think maybe January or February. And I gave a repeat talk, the same thing. And following the talk, I had a private session with Stephen Hawking and George Ellis possibly Brandon Carter but certainly George Ellis was there. And I remember describing the techniques I was using. And Stephen Hawking picked up very quickly on these ideas.

A few months later, he wrote a paper on using my actual theorem. Not changing the theorem but applying it in a cosmological context. So, you could derive a slightly weak theorem, but it was sure it was a theorem that showed that in cosmology that singularities were inevitable. But you see in cosmology, you don't have this problem. If you're looking at a local collapse, you're comparing an infinite initial surface with the finite region. You look at the boundary of the future of the trapped surface and it's, unless you have a singularity of some sort, a bounded finite region. And then you have a contradiction when you try to project that back into the infinite unbounded plane. You have a contradiction between the compact region in the black hole and the non-compact surface.

So, when Stephen Hawking tried to apply this thing in the cosmological context. He had to assume that the universe was non-compact. And so, he wanted not to have to assume that. So, he developed these ideas following this in many ways and introduced a lot of new ideas, new concepts. A lot of this was done with the help of Brandon Carter. I have to say Brandon Carter is an unsung hero in this story. Largely because Stephen had very good ideas. He was often a bit careless. So, there were sometimes mistakes in the arguments. But I would call these mistakes of the first kind, not mistakes of the second kind. When I say mistakes of the first kind,

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these are if you can get around them and correct them. And then they're all right.
Mistakes of the second kind are when you can't correct them. That is just a disaster. These were all mistakes of the first kind, and Brandon Carter noticed the mistakes, pointed them out to Stephen. And Stephen would then correct them. This happened almost right up to the Ph.D. thesis when there was some mistake still in the argument.
But the movie was wrong there, in that Kip Thorne was not present. Dennis Sciama and I were present. But Stephen had already found all mistakes and corrected them. So that was fine. But these were not serious mistakes, I would say. The ideas were certainly very good, and he had ideas for developing the techniques I use.
Mainly looking at things like what he called Cauchy horizons. You could take a region and look at what is the boundary or what points are determined by the data on that surface. So, you look at the past light cone and you could see that all the time-like pairs which go through that point and do they meet this initial surface. And ideas like that which were very important and enable you to move these theorems forward in a way which I had not done. And that we got together later at the end and wrote a paper together. But Stephen had developed a lot of these ideas originally in three papers in the Royal Society. So, he certainly carried these ideas further and not exactly single-handedly but a lot of the work one of the important it was done by him.

JJS & JK: Did he use your space-time diagram?

RP: Yes, he certainly knew about the diagrams. I think Brandon was a very important figure in the development of these ideas and to some extent, an unsung hero. I would say he formalized the diagrams. There was a big argument about should you call them Carter diagrams. My ideas were not formalized to the degree that he had achieved. I mean he had certainly looked at many solutions and showed how these diagrams could be used in a very much more formalized way than I had. I was thinking of more as helpful ideas and not necessarily formalizing them. I would call them either conformal diagrams or strict conformal diagrams. And these strict conformal diagrams are really Carter diagrams, you could say that. And the general ideas if you like those were the ones I was playing with initially. But I just prefer to call them 'disconformal' diagrams or strict conformal diagrams. But I'm not sure that even

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Brandon quite had this full strictness that I had eventually, with certain rules about when you have points which are filled in or unfilled in points and whether they represent circles. They're only spherically symmetrical solutions, strictly only apply in the case of spherical symmetry. And it's a question of whether the points at infinity are actually spheres or only points. So, the sphere went with the open points and the points with the closed points. So, I had a very strict definition of what these diagrams meant.

JJS & JK: Question about at the black hole singularity, when Weyl curvature goes to infinity, does the quantum gravity still have a sense?

RP: Well you see this is a huge story and I think people are often very confused about that story. And the problem has to do with the theorems, particularly the theorems that Stephen and I developed and Bob Geroch. I should mention Bob Geroch was an important figure in this too. So, Brandon Carter, Bob Geroch and Stephen and George Ellis worked with Stephen on the book.

What I was concerned with initially was the gravitational collapse and the singularities in what we now call black holes and I sort of took the view that singularities are generic. That's the main lesson. Stephen said okay; let's look at the big bang. Is that generic?

Well you see, if it's a generic case you still get singularity, so you don't avoid it by having a bounce through a complicated non-generic type of singularity. So, that was the important argument he made. But the problem with all this is that the arguments are symmetrical in time. Everything we did future or past, we just change the sign. Go this way or that way, everything you do in one way it works the other way.

However, and I remember, this was an important very brief conversation I had with James Peebles. This must have been after the singularity theorems. I was probably still in London and visiting Princeton. And people were going from Princeton to Stevens Institute which was in Hoboken New Jersey on the river separating New York from New Jersey. Stevens Institute was a good spot for conferences on general relativity because the people in Syracuse could drive there from upstate New York and from Princeton. It was a good location. And they had frequent conferences there. I was about to join a car driving to this conference, and I noticed sitting in one of the other cars in the back was Jim Peebles. And I asked him a question, because I had been very worried about the

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fact that there were all these different cosmological solutions that had different kinds of singularities and were very complicated.
I asked him: 'well look there are all these complicated solutions in cosmology and all possible singularities you could have. Why do you cosmologists, only look at this simple case? Why don't you look at all these other possible singularities for the structure of the big bang" And he looked at me and he said, 'The code of the universe is not like that' I thought my God 'it isn't, is it?'.
I presume he was thinking about the microwave background and you see this very uniform structure. And it's not complicated like all these other solutions. So, this was to me, a huge point. It was not just, 'it's not like that', but 'why is it not like that?'
So, I began to worry about why the big bang is so special. And thus, it must be tied up with the second law of thermodynamics. And then I remember giving a talk somewhat later in Caltech. Here I have to reverse time slightly. Maybe I have to tell you about a conversation that somebody had with Richard Feynman.
Feynman had noticed that I was giving a talk about the second law of thermodynamics and cosmology. And he had told this colleague saying "oh look at this talk, this guy is talking about second law of thermodynamics and cosmology. I'm going to go and heckle".
I didn't know this. So, I gave my talk about cosmology and probably, I don't remember exactly, I was talking about the Weyl curvature and things like this. Somebody behind Feynman probably a Nobel Prize winner, because Caltech is full of them, started to heckle me. Suddenly Feynman turned around and pointed to the man and said, "You shut up and listen to what the man is saying". Feynman came up and said, "Look this is real advance it's really important" and I said "well look I don't know why is the Weyl curvature", and he said, "don't care about the second question you've made a big important step".

JJS & JK: You said the big bang is of a special kind.

RP: Yes, the big bang has to be of a special kind. What is special about it is probably the fact that Weyl curvature is zero. I think that the singularity in the big bang is a very peculiar kind of singularity. It's that case for which the Weyl curvature is zero or very small.
Take the example of a gas in a box, as the entropy goes up, it makes more uniform. But if there are stars going around each other, entropy goes up when they become less uniform, the Weyl curvature goes up and you get these singular states in black holes. And

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it's still singular state, but it's not anything like the big bang. So, the key point or the quick question was why the singularities in gravitational collapse are so utterly different from the big bang one. Everybody was saying, even me, "oh, its quantum gravity, we've got to learn about quantum gravity". Dennis Sciama, my great hero, who taught me so much physics, was a great proponent of the steady state model which was a very beautiful model. But when the big bang, and Penzias and Wilson saw this micro background, Dennis went around giving lectures to say: "I was promoting this steady-state model, I was wrong". You don't hear people or physicists say that very often. I had a lot of respect for Dennis. There is a double irony here because Dennis got a lot of graduate students studying the big bang by looking at quantum gravity. The irony is that he was still wrong. Exactly, it's still wrong because it can't be quantum gravity. In the beginning, I was on Dennis's side and think that we've got to learn quantum gravity. Quantum gravity is a very peculiar kind of quantum mechanics that is grossly time asymmetrical. It has to be a time asymmetrical theory and so I tried to say how you can have an asymmetrical gravitational theory for which the Weyl curvature has to be zero in initial singularities. So, I postulated the Weyl curvature hypothesis: The Weyl curvature has to be zero in the initial singularities.

It doesn't make any sense from quantum gravity. I did believe then and I still believe now, this is because combining gravity with quantum mechanics is not quantum gravity. It's partial; it's *gravitizing* quantum mechanics as much as it's quantum gravity. The collapse of the wave function has to be a gravitational effect. And the quantum mechanics, as we now understand it, is an incomplete theory. And the gravity was what changes it, but we're still stuck with that.

JJS & **JK**: So, you think that quantum mechanics should be changed?

RP: Yes absolutely. But I believe that from way back, but not quite in the same way as I do now. I think it's a stronger and quite slightly different view. I used to think that quantum gravity had to be an asymmetrical theory. But I think it's wrong to call it quantum gravity. So, when I say Dennis was wrong and I was wrong, I'm trying to say that it's not what we would call quantum gravity. It's something else.

So, this is where *CCC* comes in. That idea came much later. That was more like 15 years ago, I can't quite remember when I first

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thought about that, but it was the idea partly influenced by Paul Todd, who was a very good graduate student of mine originally. What I'm saying is that quantum gravity is not giving us an answer because it's a not very fruitful way of looking at it. The more fruitful way is to take Paul Todd's suggestion more seriously. So, his suggestion was: "look, let's study the type of singularity that we happen to find in the big bang, which is very special. Rather than just saying the Weyl curvature is zero, let's say it's extendable as a conformal manifold".

So, I'd vaguely thought about this myself, but I've never worked with him. Todd remarked that the Weyl curvature is not certainly zero. So, it has to be finite to be extendable. He worked on this and worked a lot of equations on it. And then I thought about CCC, and thus, it's got to be zero. That's because of some theorems, particularly Helmut Friedrich's theorem: if you have a positive cosmological constant and you have massless sources, then in the generic case, you're going to get zero Weyl curvature at infinity.

Well, you see it was awkward because I think Paul had some problems with it. I remember in his way of looking at it. And I said no you've got to do it this way and it means you have to look at it slightly differently. I can't remember what the problem was initially right now. But, if you're going to make the Weyl curvature equal zero, you run into problems that look like negative curvatures or negative energies or something like that. It's tied in with the production of initial dark matter. I think that's what it was. You need to have the creation of new material which is dark matter and that's in order to make *CCC* work. You have to have this initial material. And I think with Paul's way of looking at it you didn't have that. I'd have to go back and remember what he did there. But you had to generalize what he was looking at, and the generalization allows you to talk about an initial state which has dark matter in it.

JJS & JK: Quantum mechanics is working very well, with very beautiful results. The problem arises when you try to combine the two theories: general relativity and quantum mechanics.

RP: In a sense, when people think about quantum gravity, if that's the right term, they're looking in an unhelpful direction. They're trying to see how very high curvatures, Planck scale energies, all these kinds of things may change physics. Sure, it may. Why I say its unhelpful is because it's not guided by experiment.

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Whereas the other ends of the subject which is how gravity might affect the structure of quantum mechanics is very borderline. And when I say borderline, it means in a good sense that we're close to seeing these effects. I mean, my colleague Dirk Bouwmeester who is a Dutch physicist, who've been working on an experiment for decades trying to put a little tiny mirror into a superposition of two locations at once by beam-splitting a photon and hitting it many times with this photon and seeing whether you can see if that maintains itself or it comes one or the other.

It seems that he was getting very close to getting a result here. But he seems to have problems with getting anything cold enough and having very worries about temperature fluctuations and things like this, which is a difficult problem. I don't know exactly where it's gone this experiment at the moment. The last time, I heard him, and he changed it rather, I didn't quite understand the version that he was talking about. There are other types of experiment involving things like beads, little tiny spots which are put into superpositions. It's a bit like the mirror but in a different way. Or Bose-Einstein condensates and I think that's quite a fruitful way of looking at it. I have a colleague Yvette Fuentes, who is interested in putting Bose-Einstein condensates and making big enough ones, so you can actually see if you put your Bose-Einstein condensate into a double way and split it into two locations. Does that persist, or does it tend to leak one into the other spontaneously or does one of them disappear and become entirely in the other?

There is a new experiment that would see this effect. Because you can estimate at what level it should happen. So yes, these are things which are not out of the blue; they're not simply waving your hands and, who knows where. One can estimate from looking at conflicts between the principles of general relativity and the principles of quantum mechanics, and when I say that the principle of general relativity, which is fundamental, I mean the principle of equivalence. That is to say the equivalence between a gravitational field and an acceleration. For instance, Galileo's idea of dropping a big rock and a little rock, do they fall together as long as you can get rid of the air resistance? He appreciated that perfectly well air resistance would spoil it, then would they fall together? And he says yes. Is that correct' Well, as far as we know, yes. What effect does that have on quantum mechanics' A big effect because if you try and do your quantum mechanics in a way which is invariant under these replacing of the gravitational field by an acceleration, you run into

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problem with the gravitational vacuum or the vacuum. And this problem can be resolved by the reduction of the state. So, it gives you an idea of at what level should you expect to see that a superposition between a mass in two locations simultaneously one there and one here. How long can you preserve such a superposition and one can make an estimate of how long that should be? The experiments are not yet at the level to see this. But they're not too far. It's Planck scale energy in a certain sense, but it's not high energies like in particle physics. You're looking at big energies because they're spread you're looking at big things.

JJS & JK: What do you think about loop quantum gravity?

RP: It's better than strings. For a very important reason, they're using the right number of space and time dimensions. It's nearer to reality but it doesn't sort of come to grips with these questions as far as I can see. I don't know. I haven't seen a way of making it come to grip with it. I mean if it really did relate to the measurement issue. But you find amongst these loop people they're not very sympathetic towards *CCC*. You know I've talked to Ashtekar and of course lots of times with Carlo Rovelli and you know they take my view on board in the sense that they don't think I'm crazy. But they don't incorporate these ideas into it, at least I haven't seen it maybe I'm too limited in what I've seen them do.

JJS & JK: They use, to some degree, a version of the spin network basically.

RP: They do to some degree but that's not my spin network. It's more aiming at the right kind of thing. But on the other hand, I don't see them bringing in what I regard as essential. And what I've seen when they talk about the big bang. Ashtekar talks about these things and he certainly hasn't taken *CCC* on board at all. So, I mean, I'd be happy to look at it, if they can see a way of making the loops fit in with *CCC* in some sense that would be very exciting. I would be very keen to see some serious theory loop quantum gravity or whatever if it somehow could relate to *CCC*. You see the time asymmetry seems to be very important. They tend to look at more like bounces. You had something non-singular on both sides. There are other schemes, well like Steinhardt and so on. Then they have schemes where they try to get rid of inflation, but I can't see how to make that work. But I have a lot of trouble with it.

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Ashtekar has never taken it seriously. But this is so basic. It's the second law of thermodynamics; none of these theories take it seriously. See as far as I'm aware, *CCC* is the only theory, the only one I've seen which makes a serious stab at explaining and deriving the second law of thermodynamics. And you can say where does it go, where does the entropy go' Well it goes into the black holes. The entropy in the current universe is almost entirely in black holes.
By a huge factor, I'm just taking Beckenstein-Hawking as the formula for the black hole Entropy. And it dominates enormously absolutely everything else. Now this will go up and up and up and as the black holes swallow galactic clusters, the stars get swallowed and the other black holes get swallowed and eventually you just have one black hole. Finally, that evaporates away by Hawking evaporation. Maybe a lot of entropy goes out in the evaporation. Maybe it goes disappears in the singularity.
Doesn't make much difference to me, because in the conformal diagram, all that gets squashed into one teeny window point which on the other side is less than the Planck scale. So that means much much much less and it's not just less. It's horrendously less. So, what happens to all the information. So, it's lost.
It comes out that you get a lot of information energy coming out. That you get mass energy coming out of the next aeon but there's no room for that much information.

JJS & JK: What is the major problem in cosmology?

RP: This is my very biased personal opinion. The major problem in cosmology is they don't take CCC seriously.
And to take it seriously, you've got to think about various things. One is mass decay because the mass does have to disappear. That's connected with Higgs but there is more complicated story I think. So, the mass has to fade out in the very remote future. But it only fades away. It's never quite zero, I think. But that's one issue you have to have no inflation. Inflation spoils the picture. However, you don't need inflation. Well first of all it's supposed to smooth out the universe, but it doesn't. The universe is already smoothed out by the exponential expansion of the previous eon. The problem I guess is that inflation is often introduced in order to explain get getting rid of local divergences and things. I don't know, I'm no expert on inflation; I've deliberately not looked at it.
I've had long arguments with Alan Guth, he gets very upset about these things because of course he wants inflation. And I don't want

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inflation. Inflation would mess up any signal. If there was an inflationary phase of any significant degree, it would mess up signals coming from the previous eon. That would include the Hawking points. It would include the ring. Now you see, there are so many arguments and complaints. When Vahe looks at his rings, he did the analysis in an inappropriate way originally and so people complain about that. The paper was originally sent to the Royal Society and it was rejected and then they reconsidered it and accepted it on the condition that Vahe would produce his analysis. And this means of course doing the analysis. He didn't want to do this. So, we sent it to another journal and had been accepted before the change of mind had come from the Royal Society. They changed their mind already when it had been accepted by this other journal. I was rather keen on getting it back into the Royal Society but Vahe said in no way, he was fed up with Royal Society. He was going to send it to the European Physical Journal. So, we sent it there.
We did not put in the paper the analysis that he had done to prove the significance. Instead, we looked at the argument of what happens when you deform the sky and make them look like ellipses. And then the significance drops dramatically, that is to say, the number of rings you see, drops dramatically. However, it's not the kind of analysis that other people use, so they didn't pay any attention to us. We also had this second paper in which we looked at Planck analysis (not the WMAP). And you can see by eye the enormous anisotropy in the sources of these signals. And it's just completely obvious that it's not isotropic. People just say they pay no attention to it. Now you see a polish group, completely independently of us did an analysis completely different way much more conventional in their way of looking at it, and they kind came up with a confidence level of the rings. This is the rings not the Hawking points. Confidence level of 99.4 percent. Which people ignore because that's not enough signal for people in particles physics. It's fairly strong evidence. I think it was 99.6 in their original WMAP data. The Planck data was 99.4. But then later on, we did the Hawking point analysis which is completely different phenomenon. People were confused with this, because the way of doing the analysis which was very similar for the one of looking for rings and they thought 'oh, you're just looking at different rings'. Completely wrong, that was not what we were looking for. We are looking for spots. They should be called Hawking spots because that's what they are. And these spots have a 99.98 confidence level.

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Now there is an irony here, which I won't even talk about. There is an irony, because I've had a long conversation with Alan Guth. And there is something which wasn't quite right in our paper. I'm not going to tell you what it was. We're now having a new paper that was not aimed at this thing not being right it was aimed at something quite different, but we have to correct this thing. And it's extremely interesting because what is not quite right in that paper, is more interesting than if it was right.
But I don't think I'll go into that. If you want to explain that, you can listen to him. I have to give him credit because he pointed out something which we hadn't noticed. It doesn't help him because these points if they're there at this level, inflation has to be wrong. These points don't come from inflation. Inflation would wipe them out. I can't see however in inflationary theory, you would get these spots. You would say 'look elsewhere effect'. In fact, that's what I was trying to say. What they call a 'look elsewhere effect'. I don't see how it can possibly be a look elsewhere effect. He has more of a case than I thought originally. Because there is a certain mistake which is very curious how this mistake happened.

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§ 4. — Philosophy.

JJS & JK: In your opinion, which philosophy best describes your view of the world or your way of thinking?

RP: I am very platonic in the sense that the platonic world exists independently of humanity if you like. It's a world on its own, which does not depend on. It's being perceived by anybody. So, if there were no beings in this aeon that mathematics would still exist, just as well as if they were beings in this aeon. Suppose we were wiped out tomorrow by a nuclear explosion. People are being distracted by global warming instead you see. Then what happens to the mathematical world, it doesn't get destroyed just because people don't think about it, it's still there. So, I'm a Platonist in that sense.
But is the question that the world itself operates according to mathematical laws? With that, I think too it does raise a very tricky question. If the world was a deterministically operating world, then there wouldn't be much scope, except in the initial conditions perhaps. You could say well you put all your uncertainties or whatever

you liked in the initial conditions or you say reduction of the state according to standard quantum mechanics comes about through random effects. I don't believe in standard quantum mechanics because I believe that the reduction of the state is where the randomness comes in the Schrödinger equation. There is nothing random in the Schrödinger equation. The Schrödinger equation describes how the state evolves. It's a deterministic equation, where's the randomness' The randomness comes because you change your mind about what describes the world. You say "whoops, I thought it was the Schrödinger equation! No, no, it wasn't". It's suddenly become a density matrix. Where did that come from' Well, because I'm ignoring certain degrees of freedom in the world or something. It doesn't make any sense.

People have their way of fumbling away and getting probabilities out of it. In my view, they're completely wrong because despite the fact that we have this really great very distinguished mathematician Von Neumann, I don't know what he really thought. It's very strange; I wonder what they were really thought. I did talk to him about this question. I think he didn't have quite such a clear view of what he thought. He certainly thought one view was that somehow consciousness was what collapsed the wave function. But then, it doesn't tell you what consciousness is. Where does it come from? So, you see, my view is turning that around and saying that consciousness comes from collapsing of the wave function. But where does that come from' Does the free will give us non-deterministic view? Does it make any sense? Don't ask me, I can worry about it and I probably will. It's not clear that it's deterministic. It's not even clear what that means. It's not clear whether there is a consciousness. Certainly, most of the collapse of the wave functions are not what we would call conscious in the ordinary sense. I have persuaded my colleague Stuart Hameroff who wanted to call all collapses of the wave function, conscious. Our argument was to say "no, no I call it proto-conscious". That's not really conscious. It's the building blocks out of which consciousness is constructed. Okay well, that's the view we take. But what does it mean to have this building block. Is that building block something which is deterministic in some deep way that we don't know' I don't know certainly. We don't know. It could be anything. It could be deterministic. I'm saying it's non-computable. That's not deterministic in the normal sense of this word. It's something very subtle in physics.

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JJS & JK: You believe in a deep unity between mathematics and physics, do you think they are, in a sense, the same?

RP: Yes, well it's not the same; I think it would be a mistake to think of them as the same and even the motivations are different. Still, there is some overlap between the motivations. In mathematics, you're not constrained by having anything to do with the physical world. And most of mathematics doesn't have any direct physical significance. I mean you know I have this picture which I often drew with the three worlds: the world of mind, the mathematics and the physical world and how they relate to each other. And the picture was only a very small part of the world of mathematics you actually find encompasses the action of the physical world and only a small part of the physical has to do with our conscious understanding. So, only a small part of conscious understanding has to do with mathematics. So, it was a sort of irony or a paradox almost but somehow how does this little small part encompass everything in the next world and then the small part encompasses everything in the next world and the small part encompasses everything in the next one and I rather like that picture because it seems to indicate a paradox.

JJS & JK: Do you think that the mathematical world is more fundamental and primary than that of physics?

RP: In a certain sense, but I'm not sure. I think I did have that view. Perhaps yes, it's sort of. I put it at the top which meant it is in a certain sense more primary. But I was trying to make them more on an equal footing in a way. So, I wasn't trying to emphasize that point and certainly most of mathematics sure it has its own existence. But the existence of that mathematics is of a different kind from the existence of physical things, and only a small part of the action of the mathematical world is found to have this direct relevance to physics. And complex numbers are a very big important part of that small part. So, the complex numbers are a small part of the small parts if you like.
But nevertheless, they have an enormous implication with regards to quantum mechanics. And then I was trying to extend that. And I certainly was thinking about spin and I spent a lot of time trying to encompass spin as a basic notion, but it really wasn't enough.
And you had to have to go from the spinors to the twistors. So, you need to extend them. I mean twistors are again a form of spinors, but they are exactly not the spinors for the Lorentz group. They're

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spinors for the conformal group. And then there's another step, I think in the last few months, and I found satisfaction in that, which is only, that you actually have another step which has the twistors, you pair up a twistor with a dual twistor and then you find, they naturally have a split octonion nature or a G_2. This particularly strange simple group is this G_2 but the Lorentzian version which is G_2. I only learned this a few months ago by thinking about it. I sort of thought about this puzzle for years but not really getting seriously into it. And I realized that this is the quantized version of twistor theory, but what I didn't realize was that the algebra of the quantized version of twistor theory has this G_2 symmetry. It's there; it's hiding there all the time!

JJS & JK: You don't like the big and huge group. You prefer the simple one.

RP: Yes, that's absolutely right yes.

JJS & JK: In the Road to Reality, you use the idea of sophistication when you consider complex numbers as the primary fabric of the Universe, the founding element of twistor theory, and the argument is based on two fundamental pillars:

- **i)** the extraordinary precision of scientific theories formulated in the language of mathematics and
- **ii)** the dependence of the accuracy of physical theories on the sophistication of the mathematical formalism used.

RP: You're right exactly. It's not just that mathematics is beautiful or clever or even sophisticated. Let me mention again one big area which is not an area which I have pursued. Which is the area of string theory. The initial idea I always found it actually rather beautiful. I liked the idea when I first heard about string theory. But what I did not like is how you had to go up into these high dimensions in order to make the theory work. Now you see in certain sense, people are very attracted by the mathematics of string theory. But to me, this is going off in the wrong direction. And it's a bit hard to explain what I mean here. It's in some sense as if you're forcing physics into mathematics.
It's partly to do with this world and only a small part of it seeming to have to do with the physical world and there's a lot of very beautiful, very sophisticated, very elegant powerful mathematics which as far as we know has almost nothing to do with the physical world.

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And I guess, this is true in string theory. You have ideas which can influence many different areas of mathematics in ways which had not been thought of before. And this is a very powerful idea. But nevertheless, I don't see its connection with the physical world.

It's kind of they got lost. There may have been a connection originally with certain some of the ideas, may be Riemann surfaces and so on, when you're really at the basic beginnings of the idea of strings. But it's kind of gets departed from what at least can be seen to be directly connected with physics. And so, I think that there are too many people persuaded by the beauty in mathematics, which is there I'm sure, and to believe that physics has to fit in with it because it's so beautiful. And this is a dangerous position to take. So that's where I kind of diverge from where many people.

JJS & JK: Alain Connes has the idea that mathematics is independent of Human Being. And even, he illustrated this with the example of aliens: if we have to meet alien people, what we could have for communication would be the numbers, because they have the same atomic spectrum.

RP: Yes, I believe that. We have this paper which Vahe and I wrote, which also appears in the European Journal of Theoretical Physics, where we play with the idea that there might be communication by very advanced civilizations in the previous aeon. And they could send their signals. Which is completely crazy, but not so crazy, it might even be true. In some way, we have no concept of at the moment. There could be signals.

JJS & JK: What is your advice for the new generations?

RP: Very tricky one. You see, when people ask that sort of question I tend to say: 'do what interests you, do what excites you.' That's fine, I guess, for people who are going to do really good work in science. But it needs an awful lot of people doing pretty dreary work. I mean a lot of calculations, that are done or under putting things on computers okay, if you find that exciting, that's good sure, that's very good.

You see, I wouldn't find it exciting myself but that's just my fault. We need to have people who find that exciting and that's very good because that's a lot of things where things go these days. You have to see how to put a problem on a computer and make the computer do the hard work. You understand what you're doing, all the understanding is done by the human, but the calculations are done by the

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computer that's fine. And if that's what excites you, great, that's fantastic, that's really good because that's very important and what will happen in science. It probably wouldn't help me much because I would find that a bit dreary but that's just my trouble. But people who find that exciting are very important in the world perhaps more important to science development.
I'm not sure that's a tricky question because I think there is a danger of getting wrapped up in the schemes and getting blinded to where they have to be changed. I think this happens in quantum mechanics. You see the theory works so beautifully well and you can do so many things that you could never do before. That it makes you think, it has to be right. And the people who try to argue there's got to be something wrong with it, somehow have to be dismissed as cranks. Most of them are cranks, that's the trouble.
It doesn't mean that there isn't something deep in what they're trying to do. But it's just as well that most people don't do that. But you have to realize perhaps the limitations of the subject so to see that there are limitations in quantum mechanics, it's probably rather important too.

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Jean-Jacques Szczeciniarz, Université
Paris Diderot, Paris 7.
Joseph Kouneiher, Université de Nice
Sophia Antipolis.

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Mathematics and Cognition

Yuri Manin

Mathematics is the language for description of possibilities

A research in biology is the study of living matter. A research in astronomy is the study of celestial matter. A research in chemistry is the study of varieties of matter, their interrelations and interactions. We are observing and measuring something existing in real world, we devise and develop experiments (usually, not in astronomy), and as a result of these actions we construct an explanatory paradigm that for a period of historical time becomes the landmark in the development of scientific knowledge.

But what do we study when we do mathematics?

One possible answer is this: we study ideas that can be treated as if they were realities.

Each such idea ("a mathematical object") should be rigid enough in order to keep its form in every context that it can be used. At the same time, each such idea must be charged with a rich potential of forming connections with other ideas. When an initial complex of ideas is born, connections between ideas might also acquire the status of mathematical objects, thus building a new level of the giant hierarchy of abstractions.

A domain of the lowest level of this hierarchy constitute images of stable material things devoid of all their individual qualities except of their distinguishability from other such things. This leads to the first steps of counting: one, two, three, four... Arguably, another domain of this lowest level is filled by the basic geometric images: a point, a line/curve, a segment,... Contemporary brain studies throw some light at this distinction that correlates with their localisation in neuron nets. But for this essay it is more important, that already at the next floor of our Tower of Babel these domains become bridged: we can start counting some characteristic traits of our mental geometric images, and say that a point has dimension zero, a curve has dimension one, a domain has dimension

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two. Very soon the resources of our linguistic, "left brain" cognitive images supersede resources of the "right brain", spatio–temporal ones, and what earlier was only a poetical "bridge", now becomes the most serious, mathematical cognitive tool.

Marvellously, it turns out that even abstractions of very high level can somehow mirror reality: knowledge about our world acquired by physicists can be adequately expressed *only* in the language of mathematics.

Nevertheless, a subconscious image of this Tower of Babel often repudiates thinkers with different mind dispositions: believers, philosophers, applied mathematicians, experimental scientists...

In order to better understand how mathematics is applied to the understanding of real world, it is convenient to consider three modalities of such applications: as model, theory or metaphor.

A mathematical model describes (qualitatively or quantitatively) a certain class of possible observations, but prefers not lay claim to anything more than that.

Quantitative models produce very precise predictions, such as predictions of observable movements of planets of solar system.

Qualitative models help understanding such phenomena as stability *vs* instability, attractors (limit states, independent of initial conditions in a certain range), phase transitions that happen when a complex system in its evolution crosses a boundary between two phases (water/ice) or between two basins with different attractors.

Theories differ from models by higher pretensions. The psychological drive of continuous creation of theories is a conception of reality existing independently of material world and raising over it, reality that is knowable only with tools of mathematics.

A mathematical metaphor as a cognitive tool suggests that a certain complex net of observations can be compared with a definite mathematical construction.

A mathematical theory is an invitation to construct working models. A mathematical metaphor is an invitation to think about what we already know. Of course, such a subdivision is neither rigid, nor absolute.

Historically, development of mathematics in a sense is parallel to the development of human languages. Both mathematics and languages serve as bridges between objective reality and its reflection in human collective consciousness.

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I am convinced that science, including mathematics ("pure" or "applied") is not a moving force of our civilisation. Thanks to science, we have engines and maps, but science does not decide for us where we should go and where to stop. To believe otherwise means to retreat into times of archaic perception of knowledge as a form of magic, when a person predicting eclipse (or any other development with unclear outcome) was treated as a sorcerer provoking this or that outcome. In fact, the biological function of thinking consists rather in rather *preventing* undesirable outcomes.

Ancient Magi and shamans were also describing the space of potentialities, rather than a direct neighbourhood of a tribe settlement, with its dwellers, movements and needs. Magi did not solve practical everyday problems, it was the function of tribal chief. But the chief lent the ear to his shaman, advisor, who was supposed to find the right thing to do.

The celebrated story that reached us from antiquity is the story of the Croesus, king of Lydia. Preparing a campaign against Cyrus the Great of Persia he decided to ask advice of several most famous Oracles. He accepted the prophecy of Pythia, the priestess over the Oracle of Apollo in Delphi: "If Croesus goes to war, he will destroy a Great Empire". The ambiguity between two possible interpretations of the words "Great Empire" was resolved by his psychology of the war leader. We all know the outcome.

To sum up: mathematics describes the phase space of the real world, the space of its possible developments. It studies the laws, defining various options for development and "initial" or "boundary" conditions: the data necessary of the choice of actual trajectory.

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Yuri Manin,
Moscow State University.

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