ARISTOTELIAN REALISM

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1 INTRODUCTION

Aristotelian, or non-Platonist, realism holds that mathematics is a science of the real world, just as much as biology or sociology are. Where biology studies living things and sociology studies human social relations, mathematics studies the quantitative or structural aspects of things, such as ratios, or patterns, or complexity, or numerosity, or symmetry. Let us start with an example, as Aristotelians always prefer, an example that introduces the essential themes of the Aristotelian view of mathematics. A typical mathematical truth is that there are six different pairs in four objects:

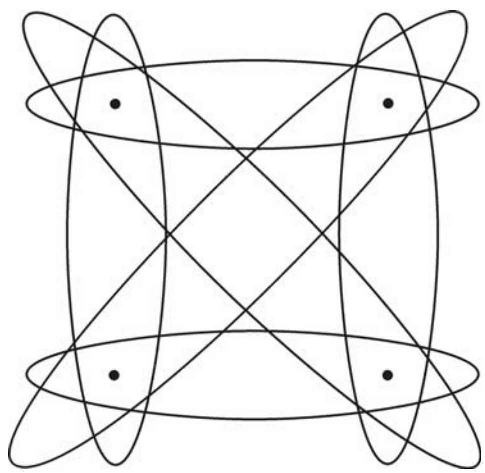

Figure 1. There are 6 different pairs in 4 objects

The objects may be of any kind, physical, mental or abstract. The mathematical statement does not refer to any properties of the objects, but only to patterning of the parts in the complex of the four objects. If that seems to us less a solid truth about the real world than the causation of flu by viruses, that may be simply due to our blindness about relations, or tendency to regard them as somehow less real than things and properties. But relations (for example, relations of equality between parts of a structure) are as real as colours or causes.

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The statement that there are 6 different pairs in 4 objects appears to be necessary, and to be about the things in the world. It does not appear to be about any idealization or model of the world, or necessary only relative to axioms. Furthermore, by reflecting on the diagram we can not only learn the truth but understand why it must be so.

The example is also, as Aristotelians again prefer, about a small finite structure which can easily be grasped by the mind, not about the higher reaches of infinite sets where Platonists prefer to find their examples.

This perspective raises a number of questions, which are pursued in this chapter.

First, what exactly does "structure" or "pattern" or "ratio" mean, and in what sense are they properties of real things? The next question concerns the necessity of mathematical truths, from which follows the possibility of having certain knowledge of them. Philosophies of mathematics have generally been either empiricist in the style of Mill and Lakatos, denying the necessity and certainty of mathematics, or admitting necessity but denying mathematics a direct application to the real world (for different reasons in the case of Platonism, formalism and logicism). An Aristotelian philosophy of mathematics, however, finds necessity in truths directly about the real world (such as the one in the diagram above). We then compare Aristotelian realism with the Platonist alternative, especially with regard to problems where Platonism might seem more natural, such as uninstantiated structures such as higher-order infinities. A later section deals with epistemology, which is very different from an Aristotelian perspective from traditional alternatives. Direct knowledge of structure and quantity is possible from perception, and Aristotelian epistemology connects well with what is known from research on baby development, but there are still difficulties explaining how proof leads to knowledge of mathematical necessity. We conclude with an examination of experimental mathematics, where the normal methods science explore a pre-existing mathematical realm.

The fortunes of Aristotelian philosophy of mathematics have fluctuated widely. From the time of Aristotle to the eighteenth century, it dominated the field. Mathematics, it was said, is the "science of quantity". Quantity is divided into the discrete, studied by arithmetic, and the continuous, studied by geometry [Apostle, 1952; Barrow, 1734, 10-15; *Encyclopaedia Britannica* 1771; Jesseph, 1993, ch. 1; Smith, 1954]. But it was overshadowed in the nineteenth century but Kantian perspectives, except possibly for the much maligned "empiricism" of Mill, and in the twentieth by Platonist and formalist philosophies stemming largely from Frege (and reactions to them such as extreme nominalism). The quantity theory, or something very like it, has also been revived in the 1990s, and a mainly Australian school of philosophers has tried to show that sets, numbers and ratios should also be interpreted as real properties of things (or real relations between universals: for example the ratio 'the double' may be something in common between the relation two lengths have and the relation two weights have.) [Armstrong, 1988; 1991; 2004, ch. 9; Bigelow, 1988; Bigelow & Pargetter, 1990, ch. 2; Forge, 1995; Forrest & Armstrong, 1987; Michell, 1994; Mortensen, 1998; Irvine, 1990, the "Sydney

School"]. The project has as yet made little impact on the mainsteam of northern hemisphere philosophy of mathematics.

The "structuralist" philosophy of Shapiro [1997], Resnik [1997] and others could naturally be interpreted as Aristotelian, if structure or pattern were thought of as properties that physical things could have. Those authors themselves, however, interpret their work more Platonistically, conceiving of structure and patterns as Platonist entities similar to sets.

2 THE ARISTOTELIAN REALIST POINT OF VIEW

Since many of the difficulties with traditional philosophy of mathematics come from its oscillation between Platonism and nominalism, as if those are the only alternatives, it is desirable to begin with a brief introduction to the Aristotelian alternative. The issues have nothing to do with mathematics in particular, so we deliberately avoid more than passing reference to mathematical examples

"Orange is closer to red than to blue." That is a statement about *colours*, not about the particular things that have the colours — or if it is about the things, it is only about them *in respect of their colour*: orange things are like red things but not blue things in respect of their colour. There is no way to avoid reference to the colours themselves.

Colours, shapes, sizes, masses are the repeatables or "universals" or "types" that particulars or "tokens" share. A certain shade of blue, for example, is something that can be found in many particulars — it is a "one over many" in the classic phrase of the ancient Greek philosophers. On the other hand, a particular electron is a non-repeatable. It is an individual; another electron can resemble it (perhaps resemble it exactly except for position), but cannot literally be it. (Introductions to realist views on universals in [Moreland, 2001, ch. 1; Swoyer, 2000]

Science is about universals. There is perception of universals — indeed, it is universals that have causal power. We see an individual stone, but only as a certain shape and colour, because it is those properties of it that have the power to affect our senses. Science gives us classification and understanding of the universals we perceive — physics deals with such properties as mass, length and electrical charge, biology deals with the properties special to living things, psychology with mental properties and their effects, mathematics with quantities, ratios, patterns and structure.

This view is close to Aristotle's account of how mathematicians are natural scientists of a sort. They are scientists who study patterns or forms that arise in nature. In what way, then, do mathematicians differ from other natural scientists? In a famous passage at *Physics* B, Aristotle says that mathematicians differ from physicists (in the broad sense of those who study nature) not in terms of subject-matter, but in terms of emphasis. Both study the properties of natural bodies, but concentrate on different aspects of these properties. The mathematician studies the properties of natural bodies, which include their surfaces and volumes, lines, and points. The mathematician is not interested in the properties of natural bodies considered as the properties of natural bodies, which is the concern of the physicist. [*Physics* II.2, 193b33-4] Instead, the mathematician is interested in the properties of natural bodies that are 'separable in thought from the world of change'. But, Aristotle says, the procedure of separating these properties in thought from the world of change does not make any difference or result in any falsehood. [Aristotle, *Physics* II.2, 193a36-b35].

Science is also the arbiter of what universals there are. To know what universals there are, as to know what particulars there are, one must investigate, and accept the verdict of the best science (including inference as well as observation). Thus universals are not created by the meanings of words. On the other hand, language is part of nature, and it is not surprising if our common nouns, adjectives and prepositions name some approximation of the properties there are or seem to be, just as our proper names label individuals, or if the subject-predicate form of many basic sentences often mirrors the particular-property structure of reality.

Not everyone agrees with the foregoing. *Nominalism* holds that universals are not real but only words or concepts. That is not very plausible in view of the ability of all things with the same shade of blue to affect us in the same way — "causality is the mark of being". It also leaves it mysterious why we do apply the word or concept "blue" to some things but not to others. *Platonism* (in its extreme version, at least) holds that there are universals, but they are pure Forms in an abstract world, the objects of this world being related to them by a mysterious relation of "participation". (Arguments against nominalism in [Armstrong, 1989, chs 1-3]; against Platonism in [Armstrong, 1978, vol. 1 ch. 7]) That too makes it hard to make sense of the direct perception we have of shades of blue. Blue things affect our retinas in a characteristic way because the blue is in the things themselves, not in some other realm to which we have no causal access. Aristotelian realism about universals takes the straightforward view that the world has both particulars and universals, and the basic structure of the world is "states of affairs" of a particular's having a universal, such as this table's being approximately square.

Because of the special relation of mathematics to complexity, there are three issues in the theory of universals that are of comparatively minor importance in general but crucial in understanding mathematics. They are the problem of uninstantiated universals, the reality of relations, and questions about structural and "unit-making" universals.

The Aristotelian slogan is that universals are *in re*: in the things themselves (as opposed to in a Platonic heaven). It would not do to be too fundamentalist about that dictum, especially when it comes to uninstantiated universals, such as numbers bigger than the numbers of things in the universe. How big the universe is, or what colours actually appear on real things, is surely a contingent matter, whereas at least some truths about universals appear to be independent of whether they are instantiated — for example, if some shade of blue were uninstantiated, it would still lie between whatever other shades it does lie between. One expects the science of colour to be able to deal with any uninstantiated shades of blue on a par with instantiated shades — of course direct experimental evidence can only be of instantiated shades, but science includes inference from experiment, not just heaps of experimental data, so extrapolation (or interpolation) arguments are possible to "fill in" gaps between experimental results. Other uninstantiated universals are "combinatorially constructible" from existing properties, the way "unicorn" is made out of horses, horns, etc. More problematic are truly "alien" universals, like nothing in the actual universe but perhaps nevertheless possible. However, these seem beyond the range of what needs to considered in mathematics — for all the vast size and esoteric nature of Hilbert spaces and inaccessible cardinals, they seem to be in some sense made out of a small range of simple concepts. What those concepts are and how they are make up the larger ones is something to be considered later.

The shade of blue example suggests two other conclusions. The first is that knowledge of a universal such as an uninstantiated shade of blue is possible only because it is a member of structured space of universals, the (more or less) continuous space of colours. The second conclusion is that the facts known in this way, such as the betweenness relations holding among the colours, are necessary. Surely there is no possible world in which a given shade of blue is between scarlet and vermilion?

At this point it may be wondered whether it is not a very Platonist form of Aristotelianism that is being defended. It has a structured space of universals, not all instantiated, into which the soul has necessary insights. That is so. There are three, not two, distinct positions covered by the names Platonism and Aristotelianism:

- (Extreme) Platonism — the Platonism found in the philosophy of mathematics — according to which universals are of their nature not the kind of entities that could exist (fully or exactly) in this world, and do not have causal power (also called "objects Platonism" [Hellman, 1989, 3], "standard Platonism [Cheyne & Pigden, 1996], "full-blooded Platonism" [Balaguer, 1998; Restall, 2003]; "ontological Platonism" [Steiner, 1973])

- Platonist or modal Aristotelianism, according to which universals can exist and be perceived to exist in this world and often do, but it is an contingent matter which do so exist, and we can have knowledge even of those that are uninstantiated and of their necessary interrelations

- Strict this-worldly Aristotelianism, according to which uninstantiated universals do not exist in any way: all universals really are *in rem*

It is true that the whether the gap between the second and third positions is large depends on what account one gives of possibilities. If the "this-worldly" Aristotelian has a robust view of merely possible universals (for example, by granting full existence to possible worlds), there could be little difference in the two kinds of Aristotelianism. But supposing a deflationary view of possibilities (as would be expected from an Aristotelian), a this-worldly Aristotelian will have a much narrower realm of real entities to consider. The discrepancy is not a matter of great urgency in considering the usual universals of science which are known to be instantiated because they cause perception of themselves. It is the gargantuan and esoteric specimens in the mathematical zoo that strike fear into the strict empirically-oriented Aristotelian realist. Our knowledge of mathematical entities that are not or may not be instantiated has always been a leading reason for believing in Platonism, and rightly so, since it is knowledge of what is beyond the here and now. It does create insuperable difficulties for a strict this-worldly Aristotelianism; but it needs to be considered whether one might move only partially in the Platonist direction. There is room to move only halfway towards strict Platonism for the same reason as there is space in the blue spectrum between two instantiated shades for an uninstantiated shade. The non-adjacency of shades of blue is a necessary fact about the blue spectrum (as Platonism holds), but whether an intermediate shade of blue is instantiated is contingent (contrary to extreme Platonism, which holds that universals cannot be literally instantiated in reality). It is the same with uninstantiated mathematical structures, according to the Aristotelian of Platonist bent: a ratio (say) whether small and instantiated or huge and uninstantiated, is part of a necessary spectrum of ratios (as Platonists think) but an instantiated ratio is literally a relation between two actual (say) lengths (as Aristotelians think). The fundamental reason why an intermediate position between extreme Platonism and extreme Aristotelianism is possible is that the Platonist insight that there is knowledge of uninstantiated universals is compatible with the Aristotelian insight that instantiated universals can be directly perceived in things.

The gap between "Platonist" Aristotelianism and extreme Platonism is unbridgeable. Aristotelian universals are ones that could be in real things (even if some of them happen not to be), and knowledge of them comes from the senses being directly affected by instantiated universals (even if indirectly and after inference, so that knowledge can be of universals beyond those directly experienced). Extreme Platonism — the Platonism that has dominated discussion in the philosophy of mathematics — calls universals "abstract", meaning that they do not have causal powers or location and hence cannot be perceived (but can only be postulated or inferred by arguments such as the indispensability argument).

Aristotelian realism is committed to the reality of relations as well as properties. The relation being-taller-than is a repeatable and a matter of observable fact in the same way as the property of being orange. [Armstrong, 1978, vol. 2, ch. 19] The visual system can make an immediate judgement of comparative tallness, even if its internal arrangements for doing so may be somewhat more complex than those for registering orange. Equally important is the reality of relations between universals themselves, such as betweenness among colours — if the colours are real, the relations between them are "locked in" and also real. Western philosophical thought has had an ingrained tendency to ignore or downplay the reality of relations, from ancient views that attempted to regard relations as properties of the individual related terms to early modern ones that they were purely mental.

[Weinberg, 1965, part 2; Odegard, 1969]

But a solid grasp of the reality of relations such as ratios and symmetry is essential for understanding how mathematics can directly apply to reality. Blindness to relations is surely behind Bertrand Russell's celebrated saying that "Mathematics may be defined as the subject where we never know what we are talking about, nor whether what we are saying is true" [Russell, 1901/1993, vol. 3, p.366].

Considering the importance of structure in mathematics, important parts of the theory of universals are those concerning structural and "unit-making" properties. A structural property is one that makes essential reference to the parts of the particular that has the property. "Being a certain tartan pattern" means having stripes of certain colours and widths, arranged in a certain pattern. "Being a methane molecule" means having four hydrogen atoms and one carbon atom in a certain configuration. "Being checkmated" implies a complicated structure of chess pieces on the board. [Bigelow & Pargetter, 1990, 82-92] Properties that are structural without requiring any particular properties of their parts such as colour could be called "purely structural". They will be considered later as objects of mathematics.

"Being an apple" differs from "being water" in that it structures its instances discretely. "Being an apple" is said to be a "unit-making" property, in that a heap of apples is divided by the universal "being an apple" into a unique number of non-overlapping parts, apples, and parts of those parts are not themselves apples. A given heap may be differently structured by different unit-making properties. For example, a heap of shoes consists of one number of shoes and another number of pairs of shoes. Notions of (discrete) number should give some account of this phenomenon. By contrast, "being water" is homoiomerous, that is, any part of water is water (at least until we go below the molecular level). [Armstrong, 2004, 113-5]

One special issue concerns the relation between sets and universals. A set, whatever it is, is a particular, not a universal. The set {Sydney, Hong Kong} is as unrepeatable as the cities themselves. The idea of Frege's "comprehension axiom" was that any property ought to define the set of all things having that property is a good one, and survives in principle the tweakings of it necessary to avoid paradoxes. It emphasises the difference between properties and sets, by calling attention to the possibility that different properties should define the same set. In a classical (philosophers') example, the properties "cordate" (having a heart) and "renate" (having a kidney) are co-extensive, that is, define the same set of animals, although they are not the same property and in another possible world would not define the same set.

Normal discussion of sets, in the tradition of Frege, has tended to assume a Platonist view of them, as "abstract" entities in some other world, so it is not clear what an Aristotelian view of their nature might be. One suggestion is that a set is just the heap of is singleton sets, and the singleton set of an object x is just x's having some unit-making property: the fact that Joe has some unit-making property such as "being a human" is all that is needed for there to be the set

{Joe}. [Armstrong, 2004, 118-23]

A large part of the general theory of universals concerns causality, dispositions and laws of nature, but since these are of little concern to mathematics, we leave them aside here.

3 MATHEMATICS AS THE SCIENCE OF QUANTITY AND STRUCTURE

If Aristotelian realists are to establish that mathematics is the science of some properties of the world, they must explain which properties. There have been two main suggestions, the relation between which is far from clear. The first theory, the one that dominated the field from Aristotle to Kant and that has been revived by recent authors such as Bigelow, is that mathematics is the "science of quantity". The second is that its subject matter is structure.

The theory at mathematics is about quantity, and that quantity is divided into the discrete, studied by arithmetic, and the continuous, studied by geometry, plainly gives an initially reasonable picture of at least elementary mathematics, with its emphasis on counting and measuring and manipulating the resulting numbers. It promises direct answers to questions about what the object of mathematics is (certain properties of physical and possibly non-physical things such as their size), and how they are known (the same way other natural properties of physical things are known). It was the quantity theory, or something very like it, that was revived in the 1990s by the Australian school of realist philosophers.

Following dissatisfaction with the classical twentieth century philosophies of mathematics such as formalism and logicism, and in the absence of a general wish to return to an unreconstructed Platonism about numbers and sets, another realist philosophy of mathematics became popular in the 1990s. Structuralism holds that mathematics studies structure or patterns. As Shapiro [2000, 257-64] explains it, number theory deals not with individual numbers but with the "natural number structure", which is "a single abstract structure, the pattern common to any infinite collection of objects that has a successor relation, a unique initial object, and satisfies the induction principle." The structure is "exemplified by" an infinite sequence of distinct moments in time. Number theory studies just the properties of the structure, so that for number theory, there is nothing to the number 2 but its place or "office" near the beginning of the system. Other parts of mathematics study different structures, such as the real number system or abstract groups. (Classifications of various structuralist views of mathematics are given in [Reck & Price, 2000; Lehrer Dive, 2003, ch. 1; Parsons, 2004]). It is true that Shapiro [1997; 2004] favours an "*ante rem* structuralism" which he compares to Platonism about universals, and Resnik is also Platonist with certain qualifications [Resnik, 1997, 10, 82, 261]. But Shapiro and Resnik allow arrangements of physical objects, such as basketball defences, to "exemplify" abstract structures, thus allowing mathematics to apply to the real world in a somewhat more direct way that classical Platonism and so encouraging an Aristotelian reading of their work, while certain other structuralist authors place much greater emphasis on instantiated patterns.

[Devlin, 1994; Dennett, 1991, section II]

The structuralist theory of mathematics has, like the quantity theory, some initial plausibility, in view of the concentration of modern mathematics on structural properties like symmetry and the purely relational aspects of systems both physical and abstract. It is supported by the widespread concentration of modern pure mathematics on "abstract structures" such as groups and topological spaces (emphasised in [Mac Lane, 1986] and [Corfield, 2003]; background in [Corry, 1992]).

The relation between the concepts of quantity and structure are unclear and have been little examined. The position that will be argued for here is that quantity and structure are different sorts of universals, both real. The sciences of them are approximately those called by the (philosophically somewhat unsatisfying) names of elementary mathematics and advanced mathematics. That is a more exciting conclusion than might appear. It means that the quantity theory will have to be incorporated into any acceptable philosophy of mathematics, something very far from being done by any of the current leading contenders. It also means that modern (post eighteenth-century) mathematics has discovered a completely new subject matter, creating a science unimagined by the ancients.

Let us begin with some examples, chosen to point up the difference between structure and quantity. This is especially necessary in view of the inability of supporters of either the quantity theory or the structure theory to provide convincing definitions of what properties exactly should count as quantitative or structural. (An attempt will be made later to remedy that deficiency, but the attempted definitions can only be appreciated in terms of some clear examples.)

The earliest case of a mathematical problem that seemed clearly not well described as being about "quantity" was Euler's example of the bridges of Königsberg (see Figure 2). The citizens of that city in the eighteenth century noticed that it was impossible to walk over all the bridges once, without walking over at least one of them twice. Euler [1776] proved they were correct.

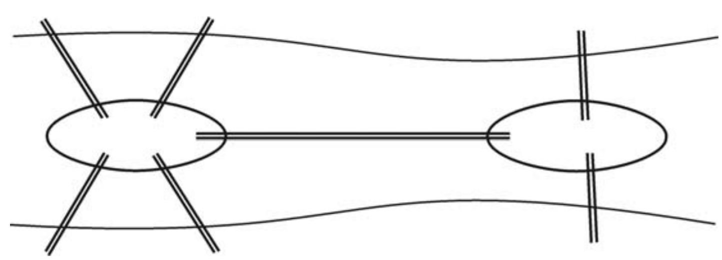

Figure 2. The Bridges of Königsberg

The result is intuitively about the "arrangement" or pattern of the bridges, rather than about anything quantitative like size or number. As Euler puts it, the result is "concerned only with the determination of position and its properties; it does not involve measurements." The length of the bridges and the size of the islands is irrelevant. That is why we can draw the diagram so schematically. All that matters is which land masses are connected by which bridges. Euler's result is now regarded as the pioneering effort in the topology of networks. There now exist large bodies of work on such topics as graph theory, networks, and operations research problems like timetabling, where the emphasis is on arrangements and connections rather than quantities.

The second kind of example where structure contrasts with quantity is symmetry, brought to the fore by nineteenth-century group theory and twentieth-century physics. Symmetry is a real property of things, things which may be but need not be physical (an argument, for example, can have symmetry if its second half repeats the steps of the first half in the opposite order; Platonist mathematical entities, if any exist, can be symmetrical.) The kinds of symmetry are classified by group theory, the central part of modern abstract algebra [Weyl, 1952].

The example of structure most discussed in the philosophical world is a different one. In a celebrated paper, Benacerraf [1965] observed that if the sequence of natural numbers were constructed in set theory, there is no principled way to choose which sets exactly the numbers should be; the sequence

$$\varnothing, \{\varnothing\}, \{\{\varnothing\}\}, \{\{\{\varnothing\}\}\}, \ldots$$

would do just as well as

$$\varnothing, \{\varnothing\}, \{\varnothing, \{\varnothing\}\}, \{\varnothing, \{\varnothing\}, \{\varnothing, \{\varnothing\}\}\}, \ldots$$

simply because both form a 'progression' or 'ω-sequence' — an infinite sequence with a start, which does not come back on itself. He concluded that "Arithmetic is ... the science that elaborates the abstract structure that all progressions have in common merely in virtue of being progressions." The assertion that that is all there is to arithmetic is more controversial than the assertion that ω-sequences are indeed one kind of order structure, and that the study of them is a part of mathematics.

Now by way of contrast let us consider some examples of quantities which seem to have nothing inherently to do with structure. The universal 'being 1.57 kilograms in mass' stands in a certain relation, a ratio, to the universal 'being 0.35 kilograms in mass'. Pairs of lengths can stand in that same ratio, as can pairs of time intervals. (It is not so clear whether pairs of temperature intervals can stand in a ratio to one another; that depends on physical facts about the kind of scale temperature is.) The ratio itself is just what those binary relations between pairs of masses, lengths and time intervals have in common ("A ratio is a sort of relation in respect of size between two magnitudes of the same kind": Euclid, book V definition 3). A (particular) ratio is thus not merely a "place in a structure" (of all ratios), for the same reason as a colour is not merely a position in the space of all possible colours — the individual ratio or colour has intrinsic properties that can be grasped without reference to other ratios or colours. Though there is indeed a system or space of all ratios or all colours, with its own structure, it makes sense to say that a certain one is instantiated and a neighbouring one not. It is perfectly determinate which ratios are instantiated by the pairs of energy levels of the hydrogen atom, just as it is perfectly determinate which, if any, shades of blue are missing.

Discrete quantities arise differently from ratios. It is characteristic of 'unit-making' or 'count' universals like 'being an apple' to structure their instances discretely. That is what distinguishes them from mass universals like 'being water'. A heap of apples stands in a certain relation to 'being an apple'; that relation is the number of apples in the heap. The same relation can hold between a heap of shoes and 'being a shoe'. The number is just what these binary relations have in common. The fact that the heap of shoes stands in one such numerical relation to 'being a shoe' and another numerical relation to 'being a pair of shoes' (made much of by Frege [1884, §22, p. 28 and §54, p. 66]) does not show that the number of a heap is subjective or not about something in the world, but only that number is relative to the count universal being considered. (Similarly, the fact that the probability of a hypothesis is relative to the evidence for it does not show that probability is subjective, but that it is a relation between hypothesis and evidence.) Like a ratio, a number is not merely a position in the system of numbers. There is a perfectly determinate number of apples in a heap, independently of anything systematic about numbers (and independent of any knowledge about it, such as through counting).

The differing origins of continuous and discrete quantity led to some classical problems in Aristotelian philosophy of quantity. The distinction between the two kinds of quantity was reinforced by the discovery of the incommensurability of the diagonal (a significance somewhat obscured by calling it the irrationality of $\sqrt{2}$): there can exist a continuous ratio that is not the ratio of any two whole numbers. That only increased the mystery as to why some of the more structural features of the two kinds of ratios should be identical, such as the principle of alternation of ratios (that if the ratio of a to b equals the ratio of c to d, then the ratio of a to c equals that of b to d). Is this principle part of a "universal mathematics", a science of quantity in general (Crowley 1980)? Is there anything to be gained, philosophically or mathematically, by Euclid's attempt to define equality of ratios without defining a way of measuring ratios (Book V definition 5)? Genuine and interesting as these questions are, they will not be attacked here. The purpose of mentioning them is simply to indicate the scope of a realist theory of quantity.

Two tasks remain. The first is to indicate where in the body of known truths the sciences of quantity and of structure, respectively, lie. The second is to inquire whether there are convincing definitions of 'quantity' and 'structure', which would support proofs of their distinctness, or other mutual relations.

The theory of the ancients that the science of quantity comprises arithmetic plus geometry may be approximately correct, but needs some qualification. Arithmetic as the science of discrete quantity is adequate, though as the Benacerraf example shows, the study of a certain kind of order structure is reasonably regarded as part of arithmetic too. The distinction between cardinal and ordinal numbers corresponds to the distinction between pure discrete quantity and linear order structures. But geometry as the science of continuous quantity has more serious problems. It was always hard to regard shape as straightforwardly ‘quantity’ — it contrasts with size, rather than resembling it — though geometry certainly studies it. From the other direction, there can be discrete geometries: the spaces in computer graphics are discrete or atomic, but obviously geometrical. Hume, though no mathematician, certainly trounced the mathematicians of his day in arguing that real space might be discrete [Franklin, 1994]. Further, there is an alternative body of knowledge with a better claim to being the science of continuous quantity in general, namely, the calculus. Study of continuity requires the notion of a limit, as defined and made use of in the differential calculus of Newton and Leibniz, and made more precise in the real analysis of Cauchy and Weierstrass. On yet another front, there is another body of knowledge which seems to concern itself with quantity as it exists in reality. It is measurement theory, the science of how to associate numbers with quantities. It includes, for example, the requirement that physical quantities to be equated or added should be dimensionally homogeneous [Massey, 1971, 2] and the classification of scales into ordinal, linear interval and ratio scales ([Ellis, 1968, ch. 4]; many references in [Diez, 1997], conclusions for philosophy of mathematics in [Pincock, 2004]).

In summary, the science of quantity is elementary mathematics, up to and including the calculus, plus measurement theory.

That leaves the ‘higher’ mathematics as the science of structure. It includes on the one hand the subject traditionally called mathematical ‘foundations’, which deals with what structures can be made from the purely topic-neutral material of sets and categories, using logical concepts, as well as matters concerning axiomatization. On the other hand, most of modern pure mathematics deals with the richer structures classified by Bourbaki into algebraic, topological and order structures [Bourbaki, 1950; Mac Lane, 1986].

There is then the final question of whether there are formal definitions of ‘quantity’ and ‘structure’, which will exhibit their mutual logical relations. For ‘quantity’, one may loosely call any order structure a kind of quantity (in that it permits comparisons on a kind of scale), but a true or paradigmatic quantity should be a relation in a system isomorphic to the continuum, or to a piece of it (for example, the interval from 0 to 1, in the case of probabilities) or a substructure of it (such as the rationals or integers) [Hale, 2000, 106]. One might go so far as to allow fuzzy quantities by a family resemblance, as they share the properties of the continuum except for absolute precision.

It must be admitted that the difficulty of defining ‘structure’ has been the Achilles heel of structuralism. As one observer says, “It’s probably not too gross a generalization to say that the main problems that have faced structuralism have been concerned with lack of clarity. After all, the slogans used to describe the view are nothing but highly evocative metaphors. In particular, philosophers have wondered: What is a structure?” [Colyvan, 1998]. The matter is far from resolved, but one suggestion involves mereology. ‘Structure’ it is proposed, can be defined as follows.

A property S is *structural* if and only if “proper parts of particulars having S have some properties T ... not identical to S, and this state of affairs is, at least in part, constitutive of S.” [Armstrong, 1978, vol. 2, 69] Under this definition, structural properties include such examples as “being a certain tartan pattern” [Armstrong, 1978, vol. 2, 70] or “being a baseball defence” [Shapiro, 1997, 74, 98] Plainly the reference in such properties to the parts having colours or being baseball players makes such structures not appropriate as objects of mathematics — not of pure mathematics, at least. Something more purely structural is needed. As Shapiro puts it in more Platonist language, a baseball defence is a kind of system, but the purer structure to be studied by mathematics is “the abstract form of a system, highlighting the interrelationships among the objects, and ignoring any features of them that do not affect how they relate to other objects in the system.” [Shapiro, 1997, 74]; or again, “a position [in a pattern] ... has no distinguishing features other than those it has in virtue of being the particular position it is in the pattern to which it belongs.” [Resnik, 1997, 203] These desiderata can be achieved by the following definition.

A property is *purely structural* if it can be defined wholly in terms of the concepts same and different, and part and whole (along with purely logical concepts).

To be symmetrical with the simplest sort of symmetry, for example, is to consist of two parts which are the same in some respect. To demonstrate that a concept is purely structural, it is sufficient to construct a model of it out of purely topic-neutral building blocks, such as sets — the capacities of set theory and pure mereology for construction being identical [Lewis, 1991, especially 112]

4 NECESSARY TRUTHS ABOUT REALITY

An essential theme of the Aristotelian viewpoint is that the truths of mathematics, being about universals and their relations, should be both necessary and about reality. Aristotelianism thus stands opposed to Einstein’s classic dictum, ‘As far as the propositions of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality.’ [Einstein, 1954, 233]. It is clear that by ‘certain’ Einstein meant ‘necessary’, and philosophers of recent times have mostly agreed with him that there cannot be mathematical truths that are at once necessary and about reality.

Mathematics provides, however, many *prima facie* cases of necessities that are directly about reality. One is the classic case of Euler’s bridges, mentioned in the previous section. Euler proved that it was impossible for the citizens of Königsberg to walk exactly once over (not an abstract model of the bridges but) the actual bridges of the city.

To take another example: It is impossible to tile my bathroom floor with (equally-sized) regular pentagonal lines. It is a proposition of geometry that ‘it is impossible to tile the Euclidean plane with regular pentagons’. That is, although it is possible to fit together (equally-sized) squares or regular hexagons so as to cover the whole space, thus:

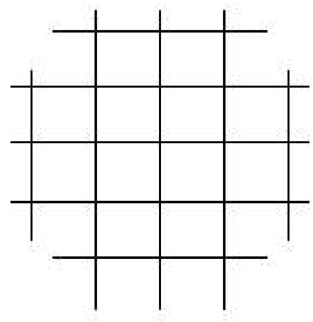

Figure 3. Tiling of the plane by squares

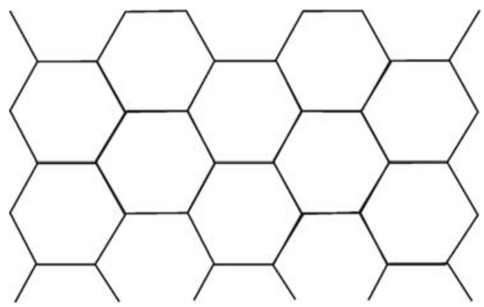

Figure 4. Tiling of the plane by regular hexagons

and it is impossible to do this with regular pentagons:

No matter how they are put on the plane, there is space left over between them.

Now the 'Euclidean plane' is no doubt an abstraction, or a Platonic form, or an idealisation, or a mental being — in any case it is not 'reality'. If the 'Euclidean plane' is something that could have real instances, my bathroom floor is not one of them, and it may be that there are no exact real instances of it at all. It is a further fact of mathematics, however, that the proposition has 'stability', in the sense that it remains true if the terms in it are varied slightly. That is, it is impossible to tile a (substantial part of) an almost Euclidean-plane with shapes that are nearly regular pentagons. (The qualification 'substantial part of' is simply to avoid the possibility of taking a part that is exactly the shape and size of one tile; such a part could of course be tiled). This proposition has the same status, as far as reality goes, as the original one, since 'being an almost-Euclidean-plane' and 'being a nearly-regular pentagon' are as purely abstract or mathematical as 'being an exact Euclidean plane' and 'being an exactly regular pentagon'. The proposition has the consequence that if anything, real or abstract, does have the shape of

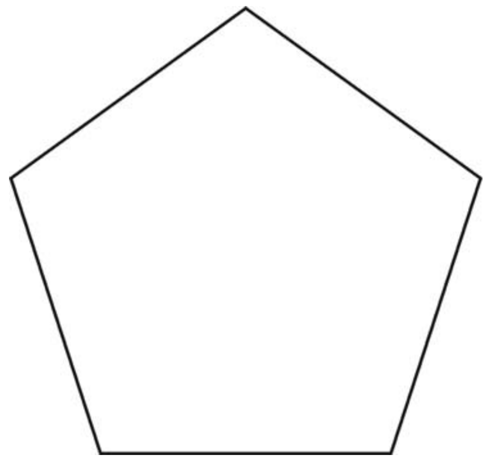

Figure 5. A regular pentagon, with which it is impossible to tile the plane

a nearly-Euclidean-plane, then it cannot be tiled with nearly-regular-pentagons. But my bathroom floor does have, exactly, the shape of a nearly-Euclidean-plane. Or put another way, being a nearly-Euclidean-plane is not an abstract model of my bathroom floor, it is its literal shape. Therefore, it cannot be tiled with tiles which are, nearly or exactly, regular pentagons.

The 'cannot' in the last sentence is a necessity at once mathematical and about reality. (A further example in [Franklin, 1989])

That example was of impossibility. The next is an example of necessity in the full sense.

For simplicity, let us restrict ourselves to two dimensions, though there are similar examples in three dimensions. A body is said to be symmetrical about an axis when a point is in the body if and only if the point opposite it across the axis is also in the body. Thus a square is symmetrical about a vertical axis, a horizontal axis and both its diagonals. A body is said to be symmetrical about a point P when a point is in the body if and only if the point directly opposite it across P is also in the body. Thus a square is symmetrical about its centre. The following is a necessarily true statement about real bodies: All bodies symmetrical about both a horizontal and a vertical axis are also symmetrical about the point of intersection of the axes:

Again, the space need not be Euclidean for this proposition to be true. All that is needed is a space in which the terms make sense.

These examples appear to be necessarily true mathematical propositions which are about reality. It remains to defend this appearance against some well-known objections.

Objection 1.
The proposition 7 + 5 = 12 appears at first both to be necessary and to say

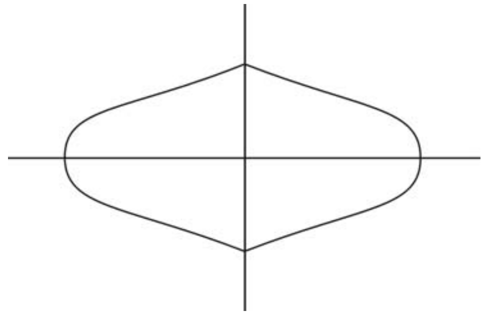

Figure 6. Symmetry about two orthogonal axes implies symmetry about centre

something about reality. For example, it appears to have the consequence that if I put seven apples in a bowl and then put in another five, there will be twelve apples in the bowl. A standard objection begins by noting that it would be different for raindrops, since they may coalesce. So in order to say something about reality, the mathematical proposition must need at least to be conjoined with some proposition such as, 'Apples don't coalesce', which is plainly contingent. This consideration is reinforced by the suspicion that the proposition $7 + 5 = 12$ is tautological, or almost so, in some sense.

Perhaps these objections can be answered, but there is plainly at least a *prima facie* case for a divorce between the necessity of the mathematical proposition and its application to reality. The application seems to be at the cost of introducing stipulations about bodies which may be empirically false.

The examples above are not susceptible to this objection. Being nearly-pentagonal, being symmetrical and so on are properties that real things can have, and the mathematical propositions say something about things with these properties, without the need for any empirical assumptions.

Objection 2.
This objection is perhaps in effect the same as the first one, but historically it has been posed separately. It does at least cast more light on how the examples given escape objections of this kind.

The objection goes as follows: Geometry does not study the shapes of real things. The theory of spheres, for example, cannot apply to bronze spheres, since bronze spheres are not perfectly spherical ([Aristotle, *Metaphysics* 997b33-998a6, 1036a4-12; Proclus, 1970, 10-11]). Those who thought along these lines postulated a relation of 'idealisation' variously understood, between the perfect spheres of geometry and the bronze spheres of mundane reality. Any such thinking, even if not leading to fully Platonist conclusions, will result in a contrast between the ideal (and hence necessary) realm of mathematics and the physical (and contingent) world.

It has been found that the problem was simply a result of the primitive state of

Greek mathematics. Ancient mathematics could only deal with simple shapes such as perfect spheres. Modern mathematics, by studying continuous variation, has been able to extend its activities to more complex shapes such as imperfect spheres. That is, there are results not about particular imperfect spheres, but about the ensemble of imperfect spheres of various kinds. For example, consider all imperfect spheres which differ little from a sphere of radius one metre — say which do not deviate by more than one centimetre from the sphere anywhere. Then the volume of any such imperfect sphere differs from the volume of the perfect sphere by less than one tenth of a cubic metre. So imperfect-sphere shapes can be studied mathematically just as well as — though with more difficulty than — perfect spheres. But real bronze things do have imperfect-sphere shapes, without any 'idealisation' or 'simplification'. So mathematical results about imperfect spheres can apply directly to the real shapes of real things.

The examples above involved no idealisations. They therefore escape any problems from objection 2.

Objection 3.
The third objection proceeds from the supposed hypothetical nature of mathematics. Bertrand Russell's dictum, 'Pure mathematics consists entirely of assertions to the effect that, if such and such a proposition is true of anything, then such and such another proposition is true of that thing' [Russell, 1917, 75] suggests a connection between hypotheticality and lack of content. Even those who have not gone so far as to think that mathematics is just logic have generally thought that mathematics is not about reality, but only, like logic, relates statements which may happen to be about reality. Physicists, Einstein included, have been especially prone to speak in this way, since for them mathematics is primarily a bag of tricks used to deduce consequences from theories.

The answer to this objection consists fundamentally in a denial that mathematics is more hypothetical than any other science. The examples given above do not look hypothetical, but they could easily be cast in hypothetical form. But the fact that mathematical statements are often written in if-then form is not in itself an argument that mathematics is especially hypothetical. Any science, even a purely classificatory one, contains universally quantified statements, and any 'All As are Bs' statement can equally well be expressed hypothetically, as 'If anything is an A, it is a B'. A hypothetical statement may be convenient, especially in a complex situation, but it is just as much about real As and Bs as 'All As are Bs'.

No-one argues that

> All applications of 550 mls/hectare Igran are effective against normal infestations of capeweed

is not about reality nerely because it can be expressed hypothetically as

> If 550 mls/hectare Igran is applied to a normal infestation of capeweed, the weed will die.

Neither should mathematical propositions such as those in the examples be thought to be not about reality because they can be expressed hypothetically. Real portions of liquid can be (approximately) 550 mls of Igran. Real tables can be (approximately) symmetrical about axes. Real bathroom floors can be (nearly) flat and real tiles (nearly) regular pentagons [Musgrave, 1977, §5].

The impact of this argument is not lessened even if the process of recasting mathematics into if-then form goes as far as axiomatisation. Einstein thought it was: his quotation with which the section began continues:

> As far as the propositions of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality. It seems to me that complete clarity as to this state of things became common property only through that trend in mathematics which is known by the name of 'axiomatics'. [Einstein, 1954, 233]

Einstein goes on to argue that deductive axiomatised geometry is mathematics, is certain and is 'purely formal', that is, uninterpreted; while applied geometry, which includes the proposition that solid bodies are related as bodies in three-dimensional Euclidean space, is a branch of physics. Granted that it is a contingent physical proposition that solid bodies are related in this way, and granted that an uninterpreted system of deductive 'geometry' is possible, there remain two main problems about Einstein's conclusion that 'mathematics as such cannot predicate anything about ... real objects' [Einstein, 1954, 234]

Firstly, non-mathematical topics, such as special relativity, can be axiomatised without thereby ceasing to be about real things. This remains so even if one sets up a parallel system of 'purely formal axiomatised special relativity' which one pretends not to interpret.

Secondly, even if some of the propositions of 'applied geometry' are contingent, not all are, as the examples above showed. Doubtless there is a 'proposition' of 'purely formal geometry' corresponding to 'It is impossible to tile my bathroom floor with regular pentagonal tiles'; the point is that the modality, 'impossible', is still there when it is interpreted.

In theory this completes the reply to the objection that mathematics is necessary only because it is hypothetical. Unfortunately it does nothing to explain the strong feeling among ordinary users of mathematics, such as physicists and engineers, that mathematics is a kind of tool kit for getting one scientific proposition out of another. If an electrical engineer is accustomed to working out currents by reaching for his table of Laplace transforms, he will inevitably see this mathematical method as a tool whose 'necessity', if any, is because mathematics is not about anything, but is only a kind of theoretical juice extractor.

It must be admitted that a certain amount of applicable mathematics really does consist of tricks or calculatory devices. Tricks, in mathematics or anywhere else, are not *about* anything, and any real mathematics that concerns them will be in explaining why and when they work; this is a problem the engineer has little interest in, except perhaps for the final answer. The difficulty is to explain how mathematics can have both necessity and application to reality, without appearing to do so to many of its users.

The short answer to this lies in the mind's tendency to think of relations as not really existing. Since mathematics is so tied up with relations of certain kinds, its subject matter is easy to overlook. A familiar example of how mathematics applies in physics will make this clearer.

Newton postulated the inverse square law of gravitation, and derived from it the proposition that the orbits of the planets are elliptical. Let us look a little more closely at the derivation, to see whether the mathematical reasoning is in some way about reality or is only a logical device for deriving one scientific law from another.

First of all, Newton did not derive the shape of the orbits from the law of gravitation alone. An orbit is a path along which a planet moves, so there needs to be a proposition connecting the law of force with movement; the link is, of course,

$$\text{force} = \text{mass} \times \text{acceleration}$$

Then there must be an assertion that net accelerations other than those caused by the gravitation of the sun are negligible. Ideally this should be accompanied by a stability analysis showing that small extra net forces will only produce small deviations from the calculated paths. Adding the necessary premises has not, however, introduced any ellipses. What the premises give is the local change of motion of a planet at any point; given any planet at any point with any speed, the laws give the force, and hence the acceleration — change of speed — that the planet undergoes. The job of the mathematics — the only job of the mathematics — is to add together these changes of motion at all the points of the path, and reveal that the resulting path must be an ellipse. The mathematics must track the path, that is, it must extract the global motion from the local motions.

There are two ways to do this mathematics. In this particular case, there are some neat tricks available with angular momentum. They are remarkable enough, but are still purely matters of technique that luckily allow an exact solution to the problem with little work. The other method is more widely applicable and is here more revealing because more direct; it is to use a computer to approximate the path by cutting it into small pieces. At the initial point the acceleration is calculated and the motion of the planet calculated for a short distance, then the new acceleration is calculated for the new position, and so on. The smaller the pieces the path is cut into, the more accurate the calculation. This is the method actually used for calculating planetary orbits, since it can easily take account of small extra forces, such as the gravitational interaction of the planets, which render special tricks useless. The absence of computational tricks exposes what the mathematics is actually doing — extracting global structure from local.

The example is typical of how mathematics is applied, as is clear from the large proportion of applied mathematics that is concerned one way or another with the solution of differential equations. Solving a differential equation is, normally, entirely a matter of getting global structure from local — the equation gives what is happening in the neighbourhood of each point; the solution is the global behaviour that results. [Smale, 1969] A good deal of mathematical modelling and operations research also deals with calculating the overall effects of local causes. The examples above all involved some kind of interaction of local with global structure.

Though it is notoriously difficult to say what 'structure' is, it is at least something to do with relations, especially internal part-whole relations. If an orbit is elliptical globally, its curvature at each point is necessarily that given by the inverse square law, and vice versa. In general the connections between local and global structure are necessary, though it seems to make the matter more obscure rather than less to call the necessity 'logical'. Seen this way, there is little temptation to regard the function of mathematics as merely the deducing of consequences, like a logical engine. It is easy to see, though, why mathematics has been seen as having no subject matter — the western mind has had enormous difficulty focussing on the reality of relations at all [Weinberg, 1965, section 2], let alone such abstract relations as structural ones. Nevertheless, symmetry, continuity and the rest are just as real as relations that can be measured, such as ratios of masses; bought and sold, such as interest rate futures; and litigated over, such as paternity.

Typically, then, a scientist will postulate or observe some simple local behaviour in a system, such as the inverse square law of attraction or a population growth rate proportional to the size of the population. The mathematical work, whether by hand or computer, will put the pieces together to find out the global effect of the continued operation of the proposed law – in these cases elliptical orbits and exponential growth. There are bad reasons for thinking the mathematics is just 'turning the handle' — for example it costs less than experiment, and many scientists' expertise runs to only simple mathematical techniques. But there are no good reasons. The mathematics investigates the necessary interconnections between the parts of the global structure, which are as real properties of the system studied as any other.

This completes the explanation of why mathematics seems to many to be just a deduction engine, or to be purely hypothetical, even though it is not.

Objection 4.
Certain schools of philosophy have thought there can be no necessary truths that are genuinely about reality, so that any necessary truth must be vacuous. 'There can be no necessary connections between distinct existences.'

Answer: The philosophy of mathematics has enough to do dealing with mathematics, without taking upon itself the refutation of outmoded metaphysical dogmas. Mathematics must be appreciated on its own terms, and wider metaphysical theories adjusted to take account of whatever is found.

Nevertheless something can be said about the exact point where this objection fails to make contact with the examples above. The clue is the word 'distinct'. The word suggests a kind of logical atomism, as if relations can be thought of as strings joining point particulars. One need not be F.H. Bradley to find that view too simple. It is especially inappropriate when treating things with internal structure, as typically in mathematics. In an infinitely divisible thing like the surface of a bathroom floor, where are the point particulars with purely external relations? (The points of space, perhaps? But the relations between tile-sized parts of space and the whole space either have nothing to do with points at all or are properties of the whole system of relations between points.)

All the objections are thus answered. The conclusion stands, therefore, that the three examples are, as they appear to be, mathematical, necessary and about reality.

The thesis defended has been that *some* necessary mathematical statements refer directly to reality. The stronger thesis that *all* mathematical truths refer to reality seems too strong. It would indeed follow, if there were no relevant differences between the examples above and other mathematical truths. But there are differences. In particular, there are more things dreamed of in mathematics than could possibly be in reality. Some mathematical entities are just too big; even if something in reality could have the structure of an infinite dimensional vector space, it would be too big for us to know it did. Other mathematical entities seem obviously fictions from the way they are introduced, such as negative numbers. Statements about negative numbers can refer to reality in some way, since one can make true conclusions about debts by using negative numbers. But the reference is indirect, in the way that statements about the average wage-earner refer to reality, but not in the direct sense of asserting something about an entity, 'the average wage-earner'. Indirect reference of this kind is not in principle mysterious, though it needs to be explained in each particular case. So it can be conceded that many of the entities mentioned in mathematics are fictional, without any admission that this makes mathematics unique; minus-1 can be seen as like fictional entities elsewhere, such as the typical Londoner, holes, the national debt, the Zeitgeist and so on.

What has been asserted is that there are properties, such as symmetry, continuity, divisibility, increase, order, part and whole which are possessed by real things and are studied directly by mathematics, resulting in necessary propositions about them.

5 THE FORMAL SCIENCES

Aristotelians deplore the narrow range of examples chosen for discussion in traditional philosophy of mathematics. The traditional diet — numbers, sets, infinite cardinals, axioms, theorems of formal logic — is far from typical of what mathematicians do. It has led to intellectual anorexia, by depriving the philosophy of mathematics of the nourishment it would and should receive from the expansive world of mathematics of the last hundred years. Philosophers have almost completely ignored not only the broad range of pure and applied mathematics and statistics, but a whole suite of 'formal' or 'mathematical' sciences that have appeared only in the last seventy years. We give here a few brief examples to indicate why these developments are of philosophical interest to those pursuing realist views of mathematics.

It used to be that the classification of sciences was clear. There were natural sciences, and there were social sciences. Then there were mathematics and logic, which might or might not be described as sciences, but seemed to be plainly distinguished from the other sciences by their use of proof instead of experiment, measurement and theorising. This neat picture has been disturbed by the appearance in the last several decades of a number of new sciences, variously called the 'formal' or 'mathematical' sciences, or the 'sciences of complexity' [Pagels, 1988; Waldrop, 1992; Wolfram, 2002]. or 'sciences of the artificial.' [Simon, 1969] The number of these sciences is large, very many people work in them, and even more use their results. Their formal nature would seem to entitle them to the special consideration mathematics and logic have obtained. Not only that, but the knowledge in the formal sciences, with its proofs about network flows, proofs of computer program correctness and the like, gives every appearance of having achieved the philosophers' stone; a method of transmuting opinion about the base and contingent beings of this world into the necessary knowledge of pure reason. They also supply a number of concepts, like 'feedback', which permit 'in principle' explanatory talk about complex phenomena.

The oldest properly constituted formal science is perhaps operations research (OR). Its origin is normally dated to the years just before and during World War II, when multi–disciplinary scientific teams investigated the most efficient patterns of search for U–boats, the optimal size of convoys, and the like. Typical problems now considered are task scheduling and bin packing. Given a number of factory tasks, subject to constants about which must follow which, which cannot be run simultaneously because they use the same machine, and so on, one seeks the way to fit them into the shortest time. Bin packing deals with how to fit a heap of articles of given sizes most efficiently into a number of bins of given capacities. [Woolsey & Swanson,1975]. The methods used rely essentially on search through the possibilities, using mathematical ideas to rule out obviously wrong cases. The diversity of activities in OR is illustrated by the the sub–headings in the American Mathematical Society's classification of 'Operations research and mathematical science': Inventory, storage, reservoirs; Transportation, logistics; Flows in network, deterministic; Communication networks; Flows in networks, probabilistic; Highway traffic; Queues and service; Reliability, availability, maintenance, inspection; Production models; Scheduling theory; Search theory; Management decision–making, including multiple objectives; Marketing, advertising; Theory of organisations, industrial and manpower planning; Discrete location and assignment; Continuous assignment; Case–oriented studies. [*Mathematical Reviews*, 1990]

The names indicate the origin of the subject in various applied questions, but, as the grouping of actual applications into the last topic indicates, OR is now an abstract science. Plainly, a philosophy of mathematics that started with OR as its typical example would have a different — more Aristotelian — flavour than one starting with the theory of infinite sets.

Other formal sciences include control theory (noted for introducing the now familiar concepts of 'feedback' and 'tradeoff'), pattern recognition, signal processing, numerical taxonomy, image processing, network analysis, data mining, game theory, artificial life, mathematical ecology, statistical mechanics and the various aspects of theoretical computer science including proof of program correctness, computational complexity theory, computer simulation and artificial intelligence. Despite their diversity, it is that clear they have in common the analysis of complex systems (both real systems and models of real systems). That is partly what accounts for their growing prominence since the computer revolution — computation can discover results about large systems by modelling them. But the role of proof in the formal sciences shows their commonality with mathematics. The general philosophical tendency of these sciences will therefore be to support a philosophy of mathematics that is structuralist (since the formal sciences deal with complexity, that is, a great deal of structure) and Aristotelian (since the structures are mostly realized fully in real world cases such as transportation networks or computer code).

The greatest philosophical interest in the formal sciences is surely the promise they hold of necessary, provable knowledge which is at the same time about the real world, not just some Platonic or abstract idealisation of it.

There is just one of the formal sciences in which a debate on precisely this question has taken place, and done so with a degree of philosophical sophistication. It is worth reviewing the arguments, as they address matters that are common to all the formal sciences. At issue is the status of proofs of correctness of computer programs. The late 1960s were the years of the 'software crisis', when it was realised that creating large programs free of bugs was much harder than had been thought. It was agreed that in most cases the fault lay in mistakes in the logical structure of the programs: there were unnoticed interactions between different parts, or possible cases not covered. One remedy suggested was that, since a computer program is a sequence of logical steps like a mathematical argument, it could be proved to be correct. The 'program verification' project has had a certain amount of success in making software error-free, mainly, it appears, by encouraging the writing of programs whose logical structure is clear enough to allow proofs of their correctness to be written. A lot of time and money is invested in this activity. But the question is, does the proof guarantee the correctness of the actual physical program that is fed into the computer, or only of an abstraction of the program? C. A. R. Hoare, a leader in the field, made strong claims:

> Computer programming is an exact science, in that all the properties of a program and all the consequences of executing it can, in principle, be found out from the text of the program itself by means of purely deductive reasoning. [Hoare, 1969]

The philosopher James Fetzer argued that the program verification project was impossible in principle. Published not in the obscurity of a philosophical journal, but in the prestigious *Communications of the Association for Computing Machinery,* his attack had effect, being suspected of threatening the livelihood of thousands. [Fetzer, 1988] Fetzer's argument relies wholly on the gap between abstraction and reality, and applies equally well to any case where a mathematical model is studied with a view to achieving certainty about the modeled reality:

> These limitations arise from the character of computers as complex causal systems whose behaviour, in principle, can only be known with the uncertainty that attends empirical knowledge as opposed to the certainty that attends specific kinds of mathematical demonstrations. For when the domain of entities that is thereby described consists of purely abstract entities, conclusive absolute verifications are possible; but when the domain of entities that is thereby described consists of non-abstract physical entities ... only inconclusive relative verifications are possible. [Fetzer, 1989]

It has been subsequently pointed out that to predict what an actual program does on an actual computer, one needs to model not only the program and the hardware, but also the environment, including, for example, the skills of the operator. And there can be changes in the hardware and environment between the time of the proof and the time of operation. In addition, the program runs on top of a complex operating system, which is known to contain bugs. Plainly, certainty is not attainable about any of these matters.

But there is some mismatch between these (undoubtedly true) considerations and what was being claimed. Aside from a little inadvised hype, the advocates of proofs of correctness had admitted that such proofs could not detect, for example, typos. And, on examination, the entities Hoare had claimed to have certainty about were, while real, not unsurveyable systems including machines and users, but written programs. [Hoare, 1985] That is, they are the same kind of things as published mathematical proofs.

If a mathematician says, in support of his assertion, 'my proof is published on page X of volume Y of *Inventiones Mathematicae*', one does not normally say — even a philosopher does not normally say — 'your assertion is attended with uncertainty because there may be typos in the proof', or 'perhaps the Deceitful Demon is causing me to misremember earlier steps as I read later ones.' The reason is that what the mathematician is offering is not, in the first instance, absolute certainty in principle, but necessity. This is how his assertion differs from one made by a physicist. A proof offers a necessary connection between premises and conclusion. One may extract practical certainty from this, given the practical certainty of normal sense perception, but that is a separate step. That is, the certainty offered by mathematics does depend on a normal anti-scepticism about the senses, but removes, through proof, the further source of uncertainty found in the physical and social sciences, arising from the uncertainty of inductive reasoning and of theorising. Assertions in physics, about a particular case, have two types of uncertainty: that arising from the measurement and observation needed to check that the theory applies to the case, and that of the theory itself. Mathematical proof has only the first.

It is the same with programs. While there is a considerable certainty gap between reasoning and the effect of an actually executed computer program, there is no such gap in the case Hoare was considering, the unexecuted program. A proof (in, say, the predicate calculus) is a sequence of steps exhibiting the logical connection between formulas, and checkable by humans (if it is short enough). Likewise a computer program is a logical sequence of instructions, the logical connections among which are checkable by humans (if there are not too many).

One feature of programs that is inessential to this reply is their being textual. So, one line taken by Fetzer's opponents was to say that not only could programs be proved correct, but so could machines. Again, it was admitted that there was a theoretical possibility of a perceptual mistake, but this was regarded as trivial, and it was suggested that the safety of, say, a (physically installed) railway signalling system could be assured by proofs that it would never allow two trains on the same track, no matter what failures occurred.

The following features of the program verification example carry over to reasoning in all the formal sciences:

- There are connections between the parts of the system being studied, which can be reasoned about in purely logical terms.
- The complexity is, in small cases, surveyable. That is, one can have practical certainty by direct observation of the local structure. Any uncertainty is limited to the mere theoretical uncertainty one has about even the best sense knowledge.
- Hence the necessity translates into practical certainty.
- Computer checking can extend the practical certainty to much larger cases.

Euler's example of the bridges of Königsberg, considered earlier, is an early example of network theory and an especially clear case for discussion. The number and importance of such examples has grown without bound, and it is time for more serious philosophical consideration of them.

6 COMPARISON WITH PLATONISM AND NOMINALISM

The main body of philosophy of mathematics since Frege has moved along a path unsympathetic to Aristotelian views. We collect here some comparisons of the present point of view with standard philosophy of mathematics and reply to some of the objections arising from it.

Frege set terms for the debate that were essentially Platonist. His language is Platonist about sets and numbers, and almost all subsequent philosophy of mathematics has either accepted Frege's views literally and hence embraced Platonism, or attempted to deploy broad-based nominalist strategies to undermine realism (Platonist or not) in general.

The crucial move towards Platonism in modern philosophy of numbers occurred in Frege's argument for the conclusion that numbers are not properties of physical things. From the Aristotelian point of view, there is a core of Frege's argument that is correct, but his Platonist conclusion does not follow. Frege argues, in a central passage of his *Foundations of Arithmetic*, that attributing a number to things is quite unlike attributing an ordinary property like 'green':

> It is quite true that, while I am not in a position, simply by thinking of it differently, to alter the colour or hardness of a thing in the slightest, I am able to think of the Iliad as one poem, or as 24 Books, or as some large Number of verses. Is it not in totally different senses that we speak of a tree as having 1000 leaves and again as having green leaves? The green colour we ascribe to each single leaf, but not the number 1000. If we call all the leaves of a tree taken together its foliage, then the foliage too is green, but it is not 1000. To what then does the property 1000 really belong? It almost looks as though it belongs neither to any single one of the leaves nor to the totality of them all; is it possible that it does not really belong to things in the external world at all? [Frege, 1884, §22, p. 28].

Frege's preamble in this passage is sound and his question "to what does the property 1000 really belong?" is a good one. The Platonist direction of his conclusion that numbers must be properties of something beyond the external world does not follow, because he has not included the Aristotelian option among those that make sense of the preamble. There are three possible directions to go at this point:

- An idealist or psychologist direction, according to which number is relative to how we choose to think about objects; Frege quotes Berkeley as taking that option but is firmly against it himself as unable to make sense of the objectivity of mathematics
- A Platonist direction, as Frege and his followers adopt, according to which number is either a self-subsistent entity itself or an objective property of something not in this world, such as a Concept (in Frege's non-psychological sense of that term) or an extension of a Concept (a set or function conceived Platonistically) [Frege, 1884, especially §72, p. 85]
- An Aristotelian direction, which Frege does not consider, according to which 1000 is not a property of the foliage simply but of the relation between the foliage and the universal 'being a leaf', while the foliage's being divided into leaves is a property of it "in the external world" as much as its green colour is

When Frege returns to the issue later in the *Foundations*, he expresses himself in language that is interpretable at least as naturally from an Aristotelian as from a Platonist perspective:

> ... the concept, to which the number is assigned, does in general isolate in a definite manner what falls under it. The concept "letters in the word three" isolates the *t* from the *h*, the *h* from the *r*, and so on. The concept "syllables in the word three" picks out the word as a whole, and as indivisible in the sense that no part of it falls any longer under the same concept. Not all concepts possess this quality. We can, for example, divide up something falling under the concept "red" into parts in a variety of ways ... Only a concept which falls under it in a definite manner, and which does not permit an arbitrary division of it into parts, can be a unit relative to a finite Number. [Frege, 1884, §54, p. 66]

On an Aristotelian view, Frege is here distinguishing correctly unit-making universals from others. The parallel he draws between them and a straightforward physical property like "red" is reason against his unargued Platonist understanding of "concepts". If red's being homoiomerous (true of parts) is compatible with red's being physical, it is unclear why being non-homoiomerous is in itself incompatible with being physical. Being large is not homoiomerous, in that the parts of a large thing are not all large, but that does not suggest that the property large is non-physical.

The degree of Frege's Platonism has been debated, as he does not emphasise the otherworldliness of the Forms and is content with the kind of Reason that performs mathematical proofs as a means of knowledge of them (rather than requiring a mysterious intuition). But the emphasis here is not so much on the interpretation of Frege as on the effect of his forceful statements of Platonism on later work.

Frege's Platonism, in logic as much as in mathematics, has dominated the agenda of later analytic philosophy of logic, language and mathematics. It has led to a characteristic view of what counts as an adequate answer to questions in those areas, a view that Aristotelians (and often other naturalists) find inadequate.

Characteristic features of the philosophy of mathematics of the last hundred years that seem to Aristotelians to be mistakes or at least unfortunate biases in emphasis inspired by Frege include:

- Regarding Platonism and nominalism as mutually exhaustive answers to the question "Do numbers exist?", and hence taking a fundamentalist attitude to mathematical entities, as if they exist as "abstract" Platonist substances or not at all
- Resting satisfied that a concept (e.g. structure, the continuum) has been explained if it has been constructed out of some simple Platonist entities such as sets
- Feeling no need to ask for an account of what sets are
- Emphasising infinities and downplaying the role of small finite structures, the counting of small numbers and the measurement of finite quantities

- Regarding the problem of the "applicability of mathematics" or "indispensability of mathematics" as a question about the relation of some Platonist entities (e.g. numbers) and the physical world
- Regarding measurement as a relation between numbers and measured parts of the world
- Taking the epistemology of mathematics to be mysterious because requiring access to a Platonist realm

We will examine how some of these issues have played out in the most prominent writings in the philosophy of mathematics in recent decades.

The assumption that the real alternatives in the philosophy of mathematics are Platonist realism or nominalism is pervasive in the philosophy of mathematics, as is clear from the survey of realism in Balaguer's chapter in this *Handbook*, as well as in standard works such as the *Routledge Encyclopedia of Philosophy*. In the introduction to this section, we found little non-Platonist realism to list, and that has not been taken with much seriousness by the mainstream of philosophy of mathematics.

The dichotomy also makes it too easy for nominalists to claim success if they analyse a concept without reference to numbers or sets. Hartry Field in *Science Without Numbers*, for example, proposed to "nominalize" basic mathematical physics. Typical of his strategy is his account of temperature, considered as a quantity that varies continuously over space. Temperature is often described in mathematical physics textbooks as a function (that is, a Platonist mathematical entity) from space-time points to the set of real numbers (the function that gives, for each point, the number that is the temperature at that point). Field rightly says that one can say what one needs to say about temperature without reference to functions or numbers. He begins with "a three-place relation [among space-time points] Temp-Bet, with y Temp-Bet xz meaning intuitively that y is a space-time point at which the temperature is (inclusively) between the temperatures of points x and z; and a 4-place relation Temp-Cong, with xy Temp-Cong zw meaning intuitively that the temperature difference between points x and y is equal in absolute value to the temperature difference between points z and w." He then provides axioms for Temp-Cong and Temp-Bet so as ensure they behave as congruence and betweenness should, and so that it is possible to prove a "representation theorem" stating that a structure $\langle A, \text{Temp-Bet}_A, \text{Temp-Cong}_A\rangle$ is a model of the axioms if and only if there is a function ψ from A to an interval of real numbers such that

a. for all $x, y, z,\ y\ \text{Temp-Bet}_A\ xz \leftrightarrow \psi(x) \leq \psi(y) \leq \psi(z)$ or $\psi(z) \leq \psi(y) \leq \psi(x)$

b. for all $x, y, z, w,\ xy\ \text{Temp-Cong}_A\ zw \leftrightarrow |\psi(x) - \psi(y)| = |\psi(z) - \psi(w)|$
[Field, 1980, 56]

Since the clauses to the right of the double-arrows refer to numbers and functions while the terms to the left do not, Field can rightly claim to have dispensed with numbers and functions understood Platonistically. But is the result nominalist? It is all very well to write Temp-Bet and Temp-Cong as if they are atomic predicates, but they can only perform the task of representing facts about temperature if they really do "intuitively mean" betweenness and interval-equality of temperature, and if the axioms describe those relations as they hold of the real property of temperature (to a close approximation at least). In virtue of what, the Aristotelian asks, is Temp-Cong taken to be, say, transitive? It must be required because congruence of temperature intervals really is transitive. Field has not gone any way towards eliminating reference to the real continuous property, temperature.

The case of the "construction of the continuum" well illustrates the second problem with Platonist strategy, arising from its analysis of concepts via construction of them out of sets. According to Platonists, an obscure concept such as the continuum or "structure", or the meaning of sentences in natural language, is adequately explained if the concept is constructed out of some simpler Platonist entities such as sets or propositions that are taken to be so basic they need no further explanation. Aristotelian scepticism about this strategy focuses on two points: firstly, the alleged self-explanatoriness of these basic entities, and secondly, on how we know that the proposed construction in sets or propositions is adequate to the original concept we were trying to explicate — or rather (since the question is not fundamentally epistemological) what it is that would make the construction an adequate explanation. We treat the second problem here, and the first in the next section.

What account is to be given of why that particular set of sets of sets of... is the (or a) correct construction of the explanandum, such as "the continuum"? We have an initial intuitive notion of the continuum as a continuous line, a universal that could be realised in real space (though whether real space is infinitely divisible is an empirical question, to which the answer is currently not known). [Franklin, 1994] There exists an elaborate classical construction of "the continuum" as a set of equivalence classes of Cauchy sequences of rational numbers, with Cauchy sequences and rational numbers themselves constructed in complex ways out of sets. What is it that makes that particular set an analysis of the original notion of the continuum? The Aristotelian has an answer to that question: namely that the notion of closeness definable between two equivalence classes of Cauchy sequences reflects the notion of closeness between points in the original continuum. "Reflects" means here an identity of universals: closeness is a universal literally identical in the two cases (and so satisfying the same properties such as the triangle inequality). The statement that closeness is the same in both cases is not subject to mathematical proof, because the original continuum is not a formalised entity. It can only be subject to the same kind of understanding as any statement that a portion of the real world is adequately modelled by some formalism, for example, that a rail transport system is correctly described as a network with nodes. The Platonist, however, does not have any answer to the question of why that construction models the continuum; the Platonist will avoid mention of real space as far as possible and simply rely on the tradition of mathematicians to call the set-theoretical construction "the continuum". The fact that Cantor constructed something with the exactly the properties assigned by Aristotle to the continuum [Newstead, 2001] is important but unacknowledged in the Platonist story.

Similar considerations apply to all of the many constructions of mathematical concepts out of sets. There is some mathematical point to the exercise, mainly to demonstrate the consistency of the concepts (or more exactly, the consistency of the concepts relative to the consistency of set theory). But there is no philosophical point to them. The Aristotelian is not impressed by the construction of a relation as a set of ordered pairs, for example. To see that as an analysis of relations would make the same mistake as identifying a property with its extension. [Armstrong, 1978, vol. 1 ch. 4] The set of blue things is not the property blue, nor is it in any sense an "analysis" of the concept blue. It is the property blue that pre-exists and unifies the set (and supports the counterfactual that if anything else were blue, it would be a member of the set). Similarly the ordered pair (3,4) is a member of the extension of the relation "less than" because 3 is less than 4, not vice versa. The same remarks apply to, for example, the definition of a group as a set with a binary operation satisfying the associative, identity and inverse laws. That definition only has point because of pre-existing mathematical experience with groups of symmetries that do satisfy those laws, and the abstraction from those cases is what makes the abstract definition of a group a correct one. The case of groups is an instance of the more general Bourbakist notion of (algebraic or topological) "structure" as a set-theoretical construction. [Corry, 1992] Certainly if one has sets one can construct any number of sets of sets of sets ... of them, but the Aristotelian demands an answer as to why one such construction is an adequate analysis of symmetry groups and another an adequate analysis of topology. That answer must be in terms of one construction sharing a property with symmetry groups and another sharing a different property with topology. It is the shared property, as the mathematician using the sets as an analysis knows, that is the reason for the whole exercise. The philosopher with less mathematical experience is likely the make the mistake (in Aristotle's language) of confusing formal and material cause, that is, of thinking something is explained when one knows what it is made of. Constructing some structure or concept out of sets does not mean that the structure or concept is therefore about sets, for the same reason as an ability to construct the concept out of wood would not make the concept one of carpentry.

There is thus nothing to recommend the idea that if the philosophy of mathematics can explain sets, it can explain anything in mathematics since "technically, any object of mathematical study can be taken to be a set." [Maddy, 1992, 4] That gives a partial explanation of why mathematicians find standard philosophy of mathematics so irrelevant to their concerns. If mathematicians are studying the structures that can be constructed in sets while philosophers are discussing the material in which they are constructed, there is the same mismatch of concerns as if experts in concrete pouring set themselves up as gurus on architecture.

In any case, if some concept is constructed out of sets, that is only an advance, philosophically, if the Platonist conception of sets is clear. That is not the case. David Lewis exposes the unclarity of the concept in Cantor ('many, which can be thought of as one, i.e., a totality of definite elements that can be combined into a whole by a law') and in mathematics textbooks. [Lewis, 1991, 29-31] There is no explanation provided of the relation of singletons to their elements, for example. Philosophers, Lewis implies, have done even worse with the problem of what a set is than the writers of mathematics textbooks. They have simply ignored it. And when Aristotelians have offered an answer, such as David Armstrong's suggestion that the singleton set of an object x is the state of affairs of x's having some unit-making property, [Armstrong, 1991] Platonists have ignored it on the grounds that they do not need it. Since any analysis of the basic Platonist entities in terms of something non-Platonist (such as states of affairs) would threaten the whole Platonist edifice, Platonists must pretend that their basic building blocks are perfectly clear and have no need of analysis.

The Platonist mindset prefers to rush into the higher infinities and the technicalities associated with them, at the expense of achieving a correct philosophical view of the simpler finite cases first — cases such as counting small numbers, measuring small quantities, timetabling and the like. Philosophers of mathematics have been quick to accept that physics requires the full ontology of traditional real analysis, including the continuum conceived of an infinite set of points, and hence have conceived their task as essentially including an explanation of the role of infinities. But that does things in the wrong order. Firstly, the simple should in general be explained first and extended to the complex, so it is natural to ask first that we understand small numbers and counting before we ask about infinities. Secondly, the computer age has shown how to do most mathematics with finite means. A symbolic manipulation package such as Mathematica or Maple can do almost all mathematics needed for applications (and more pure mathematics than most mathematics graduates can do) but it is a finite object and manipulates only finite objects (such as formulas). It is possible to put forward with at least some degree of credibility an "ultrafinitist" philosophy that admits only finite numbers, [Zeilberger, 1991] which if not philosophically convincing is a sufficient reminder of how much of the mathematics one needs to do can be done in a strictly finite setting. Proposals that the universe (including space and time) is finite and can be adequately described by a discrete (though computationally intensive) mathematics in place of traditional real analysis [Wolfram, 2002, esp. 465-545] also cast doubt on whether infinities are really needed in applied mathematics.

Nowhere is the divergence between the Aristotelian and Platonist standpoints more obvious than in how they begin the problem of the applicability of mathematics. Even that description of the problem has a Platonist bias, as if the problem is about the relations between mathematical entities and something distinct from them in the "world" to which they are "applied". On an Aristotelian view, there is no such initial separation between mathematics and its "applications".

That undesirable assumed split between mathematical entities and their "applications" is first evident in accounts of measurement. Considering the fundamental importance of measurement as the first point of contact between mathematics and what it is about, it is surprising how little attention has been paid to it in the standard literature of the philosophy of mathematics. What attention there has been has tended to concentrate on "representation theorems" that describe the conditions under which quantities can be represented by numbers. "Measurement theory officially takes homomorphisms of empirical domains into (intended) models of mathematical systems as its subject matter", as one recent writer expresses it. [Azzouni, 2004, 161] That again poses the problem as essentially one about the association of numbers to parts of the world, which leads to a Platonist perspective on the problem. The Aristotelian insists that the system of ratios of lengths, for example, pre-exists in the physical things being measured, and measurement consists in identifying the ratios that are of interest in a particular case; the arbitrary choice of unit that allows ratios to be converted to digital numerals for ease of calculation is something that happens at the last step. (similar in Bigelow & Pargetter, 1990, 60-61]

Fregean Platonism about logic and linguistic items has also contributed to a distorted view of the indispensability argument, widely agreed to be the best argument for Platonism in mathematics. It is obvious that *mathematics* (mathematical practice, mathematical statement of theories, mathematical deduction from theories) is indispensable to science, but the argument arises from more specific claims about the indispensability of reference to mathematical entities (such as numbers and sets), concluding that such entities exist (in some Platonist sense). As Quine put the argument:

> Ordinary interpreted scientific discourse is as irredeemably committed to abstract objects — to nations, species, numbers, functions, sets — as it is to apples and other bodies. All these things figure as values of the variables in our overall system of the world. The numbers and functions contribute just as genuinely to physical theory as do hypothetical particles. [Quine, 1981, 149-50]

As stated (and as further explained by Quine and Putnam) that argument implies an attitude to language both exceedingly reverent and exceedingly fundamentalist, an attitude that was only credible — in the mid-twentieth-century heyday of linguistic philosophy when it was credible at all — in the wake of Frege's Platonism about such entities as propositions and the objects of reference. Later more naturalist perspectives have not found it plausible that the language tail can wag the ontological dog in that way.

It is true that the careful defence of the indispensability argument by Colyvan is not so easily dismissed. Nevertheless it preserves the main features that Aristotelians find undesirable, the fundamentalism of the interpretation of reference to entities (if it cannot be paraphrased away) and the assumed Platonism of the conclusion. Colyvan does begin by redefining "Platonism" so widely as to include Aristotelian realism. [Colyvan, 2001, 4] That is not a good idea, because Plato and Aristotle do not bear the same relation as Cicero and Tully, and the name "Platonism" has traditionally been reserved for a realist philosophy that contrasts with the Aristotelian. But in any case Colyvan's discussion proceeds without further notice of that option. The strategies for the realist, he says, are either a mysterious perception-like "intuition" of the Forms, or an inference to mathematical objects as "posits" similar to black holes and electrons, which are not perceived but are posited to exist by the best physical theory. And he takes it for granted that the Platonism to which he believes the indispensability argument leads denies the "Eleatic principle" that "causality is the mark of being". The numbers, sets or other objects whose existence is supported by the indispensability argument are, he believes, causally inactive, in contrast to scientific properties like colours, and hence he argues that the Eleatic principle is false. [Colyvan, 2001, ch. 3] Cheyne and Pigden [1996], however, argue that any indispensability argument ought to conclude to entities that have causal powers, as atoms do: it is their causal power that makes them indispensable to the theory. 'If we are genuinely unable to leave those objects out of our best theory of what the world is like ... then they must be responsible in some way for that world's being the way it is. In other words, their indispensability is explained by the fact that they are causally affecting the world, however indirectly. The indispensability argument may yet be compelling, but it would seem to be a compelling argument for the existence of entities *with* causal powers.' At the very least, the existence of atoms *causally* explains the observations that led to their postulation. It is not clear what corresponds in the causal of Platonic mathematical entities.

But surely there is something far-fetched in thinking of numbers as inferred hidden entities like atoms or genes? The existence of atoms is not obvious. It is only inferred from complex considerations about the ratios in which pure chemicals combine and from subtle observations of suspensions in fluids. On the other hand, a five-year-old understands all there is to know about why $2 + 2 = 4$. Kant's view that we understand counting thoroughly because we impose the counting structure on experience [Franklin, 2006] may be going too far, but he was right in believing that we do understand counting completely, and do not need inference to hidden entities or information on the web of total science to do so. It is the same with symmetry and any other mathematical structure realised in the world. It can be perceived in a single instance and understood to be repeated in another instance, without any extra-worldly form of symmetry needing to be inferred.

If the Platonist insists that the question was not about "applications" of numbers like counting by children but about the Numbers themselves, he faces the dilemma that was dramatised by Plato and Aristotle as the Third Man Argument. What good, Aristotle asks, is a Form of Man, conceived of as a separate entity from the individual men it is supposed to unify? What does it have in common with the men that enables it to perform the act of unifying them? Would not that require a "Third Man" to unite both the Form of Man and the individual men? An infinite regress threatens. [Plato, *Parmenides* 132a1-b2l; Fine, 1993, ch. 15]. The regress exposes the uselessness of a Platonic form outside space and time and without causal power, even if it existed, in performing the role assigned to it. Either the individual men already have something in common that makes them resemble the Form of Man, in which case the Form is not needed, or they don't, in which case the Form has no power to gather them together and distinguish them from non-men. The same reasoning applies to the relation of numbers and sets (conceived of as Platonic entities) to counting and measurement. If a five-year-old can see by counting that a parrot aggregrate is four-parrot-parted, and knows equally well how to count four apples if asked, no postulation of hidden other-worldly entities can add anything to the child's understanding, as it is already complete. The division of an apple heap into apple parts by the universal 'being an apple', and its parallel with the division of a parrot heap into parrot parts, is accomplished in the physical world; there is no point of entry for the supposed other-worldly entities to act, even if they had any causal power. Epistemologically, too, counting and measurement are as open to us as it is possible to be (self-knowledge possibly excepted), and again there is neither the need nor the possibility of intervention by other-worldly entities in our perception that a heap is four-apple-parted or that one tree is about twice as tall as another.

7 EPISTEMOLOGY

From an Aristotelian point of view, the epistemology of mathematics ought to be easy, in principle. If mathematics is about such properties of real things as symmetry and continuity, or ratios, or being divided into parts, it should be possible to observe those properties in things, and so the epistemology of mathematics should be no more problematic than the epistemology of colour. An Aristotelian point of view should solve the epistemology problem at the same time as it solves the problem of the applicability of mathematics, by showing that mathematics deals directly with properties of real things. [Lehrer Dive, 2003, ch. 3]

Plainly there are some difficulties with that plan, for example in explaining knowledge of some of the larger and more esoteric structures such as infinite-dimensional Hilbert spaces, which are not instantiated in anything observable. Nevertheless, it would be impressive if the plan worked for some simple mathematical structures, even if it did not work for all.

It would be desirable if an epistemology of mathematics could fulfill these requirements:

- Avoid both Platonist implausibilities involving contact with a world of Forms and logicist trivializations of mathematical knowledge

- At the lower level, be continuous with what is known in perceptual psychology on pattern recognition and explain the substantial mathematical knowledge of animals and babies

- At the higher level, explain how knowledge of uninstantiated structures is possible

- Explain the role of proof in delivering certainty in mathematics
- Explain the mental operation of "abstraction", which delivers individual mathematical concepts "by themselves"

If those requirements were met, there would be less motivation either to postulate Platonist intuition of forms, or to try to represent mathematics as tautologous or trivial so as not to have to postulate a Platonist intuition of forms.

Animal and infant cognition is not as well understood as one would wish, as experiments are difficult and inference from the observed behaviour problematic. Nevertheless it is clear in general terms that animals and babies, though they lack language, have high levels of generalization, memory, inference and inner experience. In particular, babies and animals share a numerical sense, as has become clear through careful experiments in the 1980s and 90s. To have any numerical ability (as opposed to just estimating sizes of heaps), a baby or animal must achieve three things:

- Recognition of objects against background — that is, cutting out discrete objects from the visual background (or discrete sounds from the sound stream) [Huntley-Fenner, Carey & Solimando, 2002]
- Identifying objects as of the same kind (e.g. food pellets, dots, beeps)
- Estimating the numerosity of the objects identified (the phraseology is intended to avoid the connotations of "counting" as possibly including reference to numbers or a pointing procedure, and exactitude of the answer)

Human babies can do that at birth. A newborn that sucks to get nonsense 3-syllable "words" will get bored, but perks up when the sounds suddenly change to 2-syllable words. [Bijeljac-Babic, Bertoncini & Mehler, 1993] Monkeys, rats, birds and many other higher animals can choose larger sets of food items, flee another group that substantially outnumbers their own, and with training press approximately the right number of times on a bar to obtain food. Babies and animals have an accurate immediate perception (called "subitization") of one, two and three items, and an inherently fuzzy estimate of larger sets — it is easy to tell the difference between 10 and 20 items, but not between 10 and 12. Various experiments, especially on the time taken to reach judgements, show that the reasons lie in an internal analog representation of numerosity; the persistence of this representation in adults is shown by such facts as that subjects presented with pairs of digits are slower at judging that 7 is greater than 5 than that 7 is greater than 2. None of these judgements involve anything like counting, in the sense of pairing off items with digits or numerals. [Review in Dehaene, 1997, chs 1-2; update in Xu, Spelke & Goddard, 2005]

There has been less research on the perception on continuous quantities. But infants of no more than six months can distinguish between the same and different heights of similar things side by side, and can be surprised if liquid poured into a container results in a grossly wrong final height of the liquid (though they are poor at judging quantities against a remembered standard). [Huttenlocher, Duffy & Levine, 2002] Four-year-olds can make some sense of the scaling of ratios needed to read a map. [Stea, Kirkman, Pinon, Middlebrook & Rice, 2004)] Mature rats also have some kind of internal map of their surroundings. [Nadel, 1990]

But if animals are inept at counting beyond the smallest numbers, they are excellent at perceiving some other mathematical properties that require keeping an approximate running average of relative frequencies. The rat, for example, can behave in ways acutely sensitive to small changes in the frequencies of the results of that behaviour. [Review in Holland, Holyoak, Nisbett & Thagard, 1986, section 5.2] Naturally so, since the life of animals is a constant balance between coping adequately with risk or dying. Foraging, fighting and fleeing are activities in which animal evaluations of frequencies are especially evident. Those abilities require some form of counting, in working out the approximate relative frequency of a characteristic in a moderately large dataset (after identifying, of course, the population and characteristic).

Very recently, it has become clear that covariation plays a crucial role in the powerful learning algorithms that allow a baby to make sense of its world at the most basic level, for example in identify continuing objects. Infants pay attention especially to "intermodal" information — structural similarities between the inputs to different senses, such as the covariation between a ball seen bouncing and a "boing boing boing" sound. That covariation encourages the infant to attribute a reality to the ball and event (whereas infants tend to ignore changes of colour and shape in objects). [Bahrick, Lickliter & Flom, 2004]

There is also much to learn on how the lower levels of the perceptual systems of animals and humans extract information on structural features of the world afforded by perception, for example, what algorithms are implemented in the visual system to allow inference of the curvature of surfaces, depth, clustering, occlusion and object recognition. Decades of work on visual illusions, vision in cats, models of the retina and so on has shown that the visual system is very active in extracting structure from — sometimes imposing structure on — the raw material of vision, but the overall picture of how it is done (and how it might be imitated) has yet to emerge. (A classic attempt in [Marr, 1982].)

We have reached the furthest limits of what is possible in the way of mathematical knowledge with the cognitive skills of animals. According to traditional Aristotelianism, the human intellect possesses an ability completely different in kind from animals, an ability to abstract universals and understand their relations. That ability, it was thought, was most evident in mathematical insight and proof. The geometry of eclipses, Aristotle says, not only describes the regularities in eclipses, but demonstrates why and how they must take place when they do. [Aristotle, *Posterior Analytics,* bk II ch. 2] A true science differs from a heap of observational facts (even a heap of empirical generalizations) by being organised into a system of deductions from self-evidently true axioms which express the nature of the universals involved. Ideally, each deduction from the premises allows the human understanding to grasp why the conclusion must be true. Euclid's geometry conforms closely to Aristotle's model. [McKirahan, 1992] The Aristotelianism of the medieval scholastics argued that such an ability to grasp pure relations of universals was so far removed from sensory knowledge as to prove that the "active intellect" must be immaterial and immortal. [Kuksewicz, 1994]

Perhaps those claims were overwrought, but they were right in highlighting how remarkable human understanding of universals is and how different it is from sensory knowledge. Let us take a simple example.

Euclid defines a circle as a plane figure "such that all straight lines drawn from a certain point within the figure to the circumference are equal". That is not an arbitrary definition, or an abbreviation. A circle at first glance is not given with reference to its centre — perceptually (to an animal, for example) it is more like something "equally round all the way around". Understanding that Euclid's definition applies to the same object requires an act of imaginative insight. The genius of the definition lies in its suitability for use in proofs of the kind Euclid gives immediately afterwards, proofs which would be very difficult with the more obvious phenomenological definition of a circle. [Lonergan, 1970, 7-11]

We are ready to move toward the notion of proof. If we gain knowledge of $2 \times 3 = 3 \times 2$ not by rote but by understanding the diagram

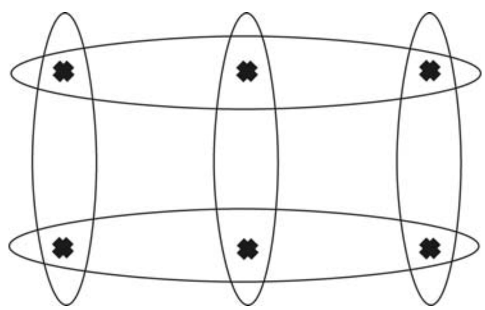

Figure 7. Why $2 \times 3 = 3 \times 2$

then we have fulfilled the Aristotelian ideal of complete and certain knowledge through understanding the reason why things must be so. We can also understand why the size of the numbers is irrelevant, and we can perform the same proof with more rows and columns, leading to the conclusion that $m \times n = n \times m$ for any whole numbers m and n. The insight permits knowledge of a truth beyond the range of actual or possible sensory experience, evidence again of the sharp difference in kind between sensory knowledge like subitization and intellectual understanding.

Consider six points, with each pair joined by a line. The lines are all coloured, in one of two colours (represented by dotted and undotted lines in the figure). Then there must exist a triangle of one colour (that is, three points such that all three of the lines joining them have the same colour).

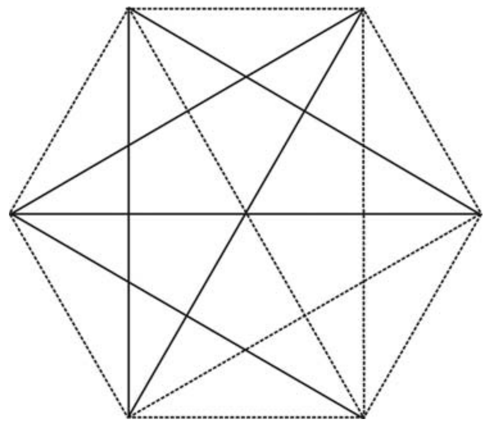

Figure 8. Six-point graph colouring

Proof. Take one of the points, and call it O. Then of the five lines from that point to the others, at least three must have the same colour, say colour A. Consider the three points at the end of those lines. If any two of them are joined by a line of colour A, then they and O form an A-colour triangle. But if not, then the three points must all be joined by B-colour lines, so there is a B-colour triangle. So there is always a single-coloured triangle. ■

There is nothing in this proof except what Aristotelian mathematical philosophy says there should be — no arbitrary axioms, no forms imposed by the mind, no constructions in Platonist set theory, no impredicative definitions, only the necessary relations of simple structural universals and our certain, proof-induced insight into them.

Unfortunately there is a gap in the story. What exactly is the relation between the mind and universals, the relation expressed in the crude metaphor of the mind "grasping" universals and their connection? "Insight" (or "eureka moment") expresses the psychology of that "grasp", but what is the philosophy behind it? Without an answer to that question, the story is far from complete. It is, of course, in principle a difficult question in epistemology in general, but since mathematics has always been regarded as the home territory of certain insight, it is natural to tackle the problem first in the epistemology of mathematics.

It is not easy to think of even one possible answer to that question. That should make us more willing to give a sympathetic hearing to the answer of traditional Aristotelianism, despite its strangeness. Based on Aristotle's dictum that "the soul is in a way all things", the scholastics maintained that the relation between the knowing mind and the universal it knows is the simplest possible: identity. The soul, they said, knows heat by actually *being hot* ("formally", of course, not "materially", which would overheat the brain).

That theory, possibly the most astounding of the many remarkable theses of the scholastics, can hardly be called plausible or even comprehensible. What could "being hot formally" mean? Nevertheless, it has much more force for the structural universals of mathematics than for physical universals like heat and mass. The reason is that structure is "topic-neutral" and so, whatever the mind is, it could in principle be shared between mental entities (however they are conceived) and physical ones. While there seem insuperable obstacles to the thought-of-heat being hot, there is no such problem with the thought-of-4 being four-parted (though one will still ask what makes it the single thought-of-4 instead of four thoughts).

In fact, on one simple model of (some) mathematical knowledge, the identity-of-structure theory is straightforwardly true. If a computer runs a weather simulation, what makes it a simulation is an identity of structure between its internal model and the physical weather. The model has parts corresponding to the spatiotemporal parts of the real weather, and relations between the parts corresponding to the causal flow of the atmosphere. (The correspondence is very visible in an analog computer, but in a digital computer it is equally present, once one sees through the rather complicated correspondence between electronically implemented bit strings and spatiotemporal points.) That certainly does not imply that the structural similarity between mental/computer model and world is all there is to knowledge — that would be to accept thermostat tracking as a complete account of knowledge. In the weather model case, there must at least be code to generate and run the model and more code to interpret the model results, for example in announcing a cold front two days ahead. Nevertheless, it is clear that it is perfectly reasonable for structural type identities between knower and known to be an essential part of knowledge, and that that thesis does not require any esoteric view of the nature of the mind.

The possibility of mental entities having literally the same structural properties as the physical systems they represent has implications for the certainty of mathematical knowledge. If mental representations literally have the structural properties one wishes to study, one avoids the uncertainty that attends sense perception and its possible errors. The errors of the senses cannot intrude on the relation of the mind to its own contents, so one major source of error is removed, and it is not surprising if simple mathematical knowledge is accompanied by a feeling of certainty, predicated on the intimate relation between knower and known in this case. That is not to maintain that such knowledge is infallible just because of this close relation. In dealing with a complex mental model, especially, such as a visualized cube, the mind may easily become confused because the single act of knowledge has to deal with many parts and their complicated relations. A mental model of some complexity may even be harder to build and to compute with than one of similar complexity in wood — although experts at the mental abacus are very fast, most people find a physical abacus much easier to use. Nevertheless, the errors of perception are a large part of the reasons for our uncertainty about matters of fact, and the removal of that source of error for a major branch of knowledge is a matter of great epistemological significance.

8 EXPERIMENTAL MATHEMATICS AND EVIDENCE FOR CONJECTURES

If mathematical realism — whether Platonist or Aristotelian — is true, then mathematics is a scientific study of a world "out there". In that case, in addition to methods special to mathematics such as proof, ordinary scientific methods such as experiment, conjecture and the confirmation of theories by observations ought to work in mathematics just as well as in science. An examination of the theory and practice of experimental mathematics will do three things. It will confirm realism in the philosophy of mathematics, since an objectivist philosophy of science is premised on realism about the entities and truths that science studies. It will suggest a logical reading of scientific methodology, since the methods of science will be seen to work in necessary as well as contingent matter (so, for example, the need to assume any contingent principles like the 'uniformity of nature' will be called into question). And it will support the objective Bayesian philosophy of probability, according to which (some at least) probabilities are strictly logical — relations of partial implication between bodies of evidence and hypothesis.

Mathematicians often speak of conjectures as being confirmed by evidence that falls short of proof. For their own conjectures, evidence justifies further work in looking for a proof. Those conjectures of mathematics that have long resisted proof, as Fermat's Last Theorem did and the Riemann Hypothesis still does, have had to be considered in terms of the evidence for and against them. It is not adequate to describe the relation of evidence to hypothesis as 'subjective', 'heuristic' or 'pragmatic'; there must be an element of what it is rational to believe on the evidence, that is, of non-deductive logic. Mathematics is therefore (among other things) an experimental science.

The occurrence of non-deductive logic, or logical probability, in mathematics is an embarrassment. It is embarrassing to mathematicians, used to regarding deductive logic as the only real logic. It is embarrassing for those statisticians who wish to see probability as solely about random processes or relative frequencies: surely there is nothing probabilistic about the truths of mathematics? It is a problem for philosophers who believe that induction is justified not by logic but by natural laws or the 'uniformity of nature': mathematics is the same no matter how lawless nature may be. It does not fit well with most philosophies of mathematics. It is awkward even for proponents of non-deductive logic. If non-deductive logic deals with logical relations weaker than entailment, how can such relations hold between the necessary truths of mathematics?

Work on this topic has therefore been rare. There is one notable exception, the pair of books by the mathematician George Polya on *Mathematics and Plausible Reasoning.* [Polya, 1954; revivals in Franklin, 1987; Fallis, 1997; Corfield, 2003, ch. 5; Lehrer Dive, 2003, ch. 6] Despite their excellence, Polya's books have been little noticed by mathematicians, and even less by philosophers. Undoubtedly this is largely because of Polya's unfortunate choice of the word 'plausible' in his title — 'plausible' has a subjective, psychological ring to it, so that the word is almost equivalent to ‘convincing’ or ‘rhetorically persuasive’. Arguments that happen to persuade, for psychological reasons, are rightly regarded as of little interest in mathematics and philosophy. Polya made it clear, however, that he was not concerned with subjective impressions, but with what degree of belief was *justified* by the evidence. [Polya, 1954, vol. I, 68] This will be the point of view argued for here.

Non-deductive logic deals with the support, short of entailment, that some propositions give to others. If a proposition has already been proved true, there is of course no longer any need to consider non-conclusive evidence for it. Consequently, non-deductive logic will be found in mathematics in those areas where mathematicians consider propositions which are not yet proved. These are of two kinds. First there are those that any working mathematician deals with in his preliminary work before finding the proofs he hopes to publish, or indeed before finding the theorems he hopes to prove. The second kind are the long-standing conjectures which have been written about by many mathematicians but which have resisted proof.

It is obvious on reflection that a mathematician must use non-deductive logic in the first stages of his work on a problem. Mathematics cannot consist just of conjectures, refutations and proofs. Anyone can generate conjectures, but which ones are worth investigating? Which ones are relevant to the problem at hand? Which can be confirmed or refuted in some easy cases, so that there will be some indication of their truth in a reasonable time? Which might be capable of proof by a method in the mathematician’s repertoire? Which might follow from someone else’s theorem? Which are unlikely to yield an answer until after the next review of tenure? The mathematician must answer these questions to allocate his time and effort. But not all answers to these questions are equally good. To stay employed as a mathematician, he must answer a proportion of them *well*. But to say that some answers are better than others is to admit that some are, on the evidence he has, more reasonable than others, that is, are rationally better supported by the evidence. This is to accept a role for non-deductive logic.

The area where a mathematician must make the finest discriminations of this kind — and where he might, in theory, be guilty of professional negligence if he makes the wrong decisions — is as a supervisor advising a prospective Ph.D. student. It is usual for a student beginning a Ph.D. to choose some general field of mathematics and then to approach an expert in the field as a supervisor. The supervisor then chooses a problem in that field for the student to investigate. In mathematics, more than in any other discipline, the initial choice of problem is the crucial event in the Ph.D.-gathering process. The problem must be

1. unsolved at present

2. not being worked on by someone who is likely to solve it soon

but most importantly

3. tractable, that is, probably solvable, or at least partially solvable, by three years' work at the Ph.D. level.

It is recognised that of the enormous number of unsolved problems that have been or could be thought of, the tractable ones form a small proportion, and that it is difficult to discern which they are. The skill in non-deductive logic required of a supervisor is high. Hence the advice to Ph.D. students not to worry too much about what field or problem to choose, but to concentrate on finding a good supervisor. (So it is also clear why it is hard to find Ph.D. problems that are also (4) interesting.)

It is not possible to dismiss these non-deductive techniques as simply 'heuristic' or 'pragmatic' or 'subjective'. Although these are correct descriptions as far as they go, they give no insight into the crucial differences among techniques, namely, that some are more reasonable and consistently more successful than others. 'Successful' can mean 'lucky', but 'consistently successful' cannot. 'If you have a lot of lucky breaks, it isn't just an accident', as Groucho Marx said. Many techniques can be heuristic, in the sense of leading to the discovery of a true result, but we are especially interested in those which give reason to believe the truth has been arrived at, and justify further research. Allocation of effort on attempted proofs may be guided by many factors, which can hence be called 'pragmatic', but those which are likely to lead to a completed proof need to be distinguished from those, such as sheer stubbornness, which are not. Opinions on which approaches are likely to be fruitful in solving some problem may differ, and hence be called 'subjective', but the beginning graduate student is not advised to pit his subjective opinion against the experts' without good reason. Damon Runyon's observation on horse-racing applies equally to courses of study: 'The race is not always to the swift, nor the battle to the strong, but that's the way to bet'.

It is true that similar remarks could also be made about *any* attempt to see rational principles at work in the evaluation of hypotheses, not just those in mathematical research. In scientific investigations, various inductive principles obviously produce results, and are not simply dismissed as pragmatic, heuristic or subjective. Yet it is common to suppose that they are not principles of *logic*, but work because of natural laws (or the principle of causality, or the regularity of nature). This option is not available in the mathematical case. Mathematics is true in all worlds, chaotic or regular; any principles governing the relationship between hypothesis and evidence in mathematics can only be logical.

In modern mathematics, it is usual to cover up the processes leading to the construction of a proof, when publishing it — naturally enough, since once a result is proved, any non-conclusive evidence that existed before the proof is no longer of interest. That was not always the case. Euler, in the eighteenth century, regularly published conjectures which he could not prove, with his evidence for them. He used, for example, some daring and obviously far from rigorous methods to conclude that the infinite sum

$$1 + \tfrac{1}{4} + \tfrac{1}{9} + \tfrac{1}{16} + \tfrac{1}{25} + \dots$$

(where the numbers on the bottom of the fractions are the successive squares of whole numbers) is equal to the prima facie unlikely value $\pi^2/6$. Finding that the two expressions agreed to seven decimal places, and that a similar method of argument led to the already proved result

$$1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} + \ldots = \frac{\pi}{4}$$

Euler concluded, 'For our method, which may appear to some as not reliable enough, a great confirmation comes here to light. Therefore, we shall not doubt at all of the other things which are derived by the same method'. He later proved the result. [Polya, 1954, vol. I, 18-21]

Even today, mathematicians occasionally mention in print the evidence that led to a theorem. Since the introduction of computers, and even more since the recent use of symbolic manipulation software packages like Mathematica and Maple, it has become possible to collect large amounts of evidence for certain kinds of conjectures. (Comments in [Borwein & Bailey, 2004; Epstein, Levy & de la Llave, 1992]) A few mathematicians argue that in some cases, it is not worth the excessive cost of achieving certainty by proof when "semirigorous" checking will do. [Zeilberger, 1993]

At present, it is usual to delay publication until proofs have been found. This rule is broken only in work on those long-standing conjectures of mathematics which are believed to be true but have so far resisted proof. The most notable of these, which stands since the proof of Fermat's Last Theorem as the Everest of mathematics, is the Riemann Hypothesis.

Riemann stated in a celebrated paper of 1859 that he thought it 'very likely' that

> All the roots of the Riemann zeta function (with certain trivial exceptions) have real part equal to $1/2$.

This is the still unproved Riemann Hypothesis. The precise meaning of the terms involved is not very difficult to grasp, but for the present purpose it is only necessary to observe that this is a simple universal proposition like 'all ravens are black'. It is also true that the roots of the Riemann zeta function, of which there are infinitely many, have a natural order, so that one can speak of 'the first million roots'. Once it became clear that the Riemann Hypothesis would be very hard to prove, it was natural to look for evidence of its truth (or falsity). The simplest kind of evidence would be ordinary induction: Calculate as many of the roots as possible and see if they all have real part $1/2$. This is in principle straightforward, though computationally difficult. Such numerical work was begun by Riemann and was carried on later with the results below:

Worker	Number of roots found to have real part $1/2$
Gram (1903)	15
Backlund (1914)	79
Hutchinson (1925)	138
Titchmarch (1935/6)	1,041

'Broadly speaking, the computations of Gram, Backlund and Hutchinson contributed substantially to the plausibility of the Riemann Hypothesis, but gave no insight into the question of why it might be true.' [Edwards, 1974, 97] The next investigations were able to use electronic computers, and the results were:

Lehmer (1956)	25,000
Lehman (1966)	250,000
Rosser, Yohe & Schoenfeld (1968)	3,500,000
Te Riele, van de Lune *et al* (1986)	1,500,000,001
Gourdon (2004)	10^{13}

It is one of the largest inductions in the world.

Besides this simple inductive evidence, there are some other reasons for believing that Riemann's Hypothesis is true (and some reasons for doubting it). In favour, there are:

1. Hardy proved in 1914 that infinitely many roots of the Riemann zeta function have real part $1/2$. [Edwards, 1974, 226-9] This is quite a strong consequence of Riemann's Hypothesis, but is not sufficient to make the Hypothesis highly probable, since if the Riemann Hypothesis is false it would not be surprising if the exceptions to it were rare.

2. Riemann himself showed that the Hypothesis implied the 'prime number theorem', then unproved. This theorem was later proved independently. This is an example of the general non-deductive principle that non-trivial consequences of a proposition support it.

3. Also in 1914, Bohr and Landau proved a theorem roughly expressible as 'Almost all the roots have real part very close to $1/2$'. This result 'is to this day the strongest theorem on the location of the roots which substantiates the Riemann hypothesis.' [Edwards, 1974, 193]

4. Studies in number theory revealed areas in which it was natural to consider zeta functions analogous to Riemann's zeta function. In some famous and difficult work, André Weil proved that the analogue of Riemann's Hypothesis is true for these zeta functions, and his related conjectures for an even more general class of zeta functions were proved to widespread applause in the 1970s. 'It seems that they provide some of the best reasons for believing that the Riemann hypothesis is true — for believing, in other words, that there is a profound and as yet uncomprehended number-theoretic phenomenon, one facet of which is that the roots ρ all lie on Re $s = 1/2$'. [Edwards, 1974, 298]

5. Finally, there is the remarkable 'Denjoy's probabilistic interpretation of the Riemann hypothesis'. If a coin is tossed n times, then of course we expect about $1/2n$ heads and $1/2n$ tails. But we do not expect *exactly* half of each. We can ask, then, what the average deviation from equality is. The answer, as was known by the time of Bernoulli, is $\sqrt{n}$. One exact expression of this fact is:

   > For any $\varepsilon > 0$, with probability one the number of heads minus the number of tails in n tosses grows less rapidly than $n^{1/2+\varepsilon}$. (Recall that $n^{1/2}$ is another notation for $\sqrt{n}$.)

Now we form a sequence of 'heads' and 'tails' by the following rule: Go along he sequence of numbers and look at their prime factors. If a number has two or more prime factors equal (i.e., is divisible by a square), do nothing. If not, its prime factors must be all different; if it has an even number of prime factors, write 'heads'. If it has an odd number of prime factors, write 'tails'. The sequence begins:

2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	...
		2^2		2×3		2^3	3^2	2×5		$2^2 \times 3$		2×7	3×5	2^4		
T	T		T	H	T			H	T		T	H	H		T	...

The resulting sequence is of course not 'random' in the sense of 'probabilistic', since it is totally determined. But it does look 'random' in the sense of 'patternless' or 'erratic' (such sequences are common in number theory, and are studied by the branch of the subject called misleadingly 'probabilistic number theory'. From the analogy with coin tossing, it is likely that

> For any $\varepsilon > 0$, the number of heads minus the number of tails in the first n 'tosses' in this sequence grows less rapidly than $n^{1/2+\varepsilon}$.

This statement is equivalent to Riemann's Hypothesis. Edwards comments, in his book on the Riemann zeta function,

> One of the things which makes the Riemann hypothesis so difficult is the fact that there is no plausibility argument, no hint of a reason, however unrigorous, why it should be true. This fact gives some importance to Denjoy's probabilistic interpretation of the Riemann hypothesis which, though it is quite absurd when considered carefully, gives a fleeting glimmer of plausibility to the Riemann hypothesis. [Edwards, 1974, 268]

Not all the probabilistic arguments bearing on the Riemann Hypothesis are in its favour. In the balance against, there are the following arguments:

1. Riemann's paper is only a summary of his researches, and he gives no reasons for his belief that the Hypothesis is 'very likely'. No reasons have been found in his unpublished papers. Edwards does give an account, however, of facts

which Riemann knew which would naturally have seemed to him evidence of the Hypothesis. But the facts in question are true only of the early roots; there are some exceptions among the later ones. This is an example of the non-deductive rule given by Polya, 'Our confidence in a conjecture can only diminish when a possible ground for the conjecture is exploded.'

2. Although the calculations by computer did not reveal any counterexamples to the Riemann Hypothesis, Lehmer's and later work did unexpectedly find values which it is natural to see as 'near counterexamples'. An extremely close one appeared near the 13,400,000th root. [Edwards, 1974), 175-9] It is partly this that prompted the calculators to persevere in their labours, since it gave reason to believe that if there were a counterexample it would probably appear soon. So far it has not, despite the distance to which computation has proceeded, so the Riemann Hypothesis is not so undermined by this consideration as appeared at first.

3. Perhaps the most serious reason for doubting the Riemann Hypothesis comes from its close connections with the prime number theorem. This theorem states that the number of primes less than x is (for large x) approximately equal to the integral

$$\int_2^x \frac{dt}{\log t}$$

If tables are drawn up for the number of primes less than x and the values of this integral, for x as far as calculations can reach, then it is always found that the number of primes less than x is actually *less than* the integral. On this evidence, it was thought for many years that this was true for all x. Nevertheless Littlewood proved that this is false. While he did not produce an actual number for which it is false, it appears that the first such number is extremely large — well beyond the range of computer calculations. It gives some reason to suspect that there may be a very large counterexample to the Hypothesis even though there are no small ones.

It is plain, then, that there is much more to be said about the Riemann Hypothesis than, 'It is neither proved nor disproved'. Without non-deductive logic, though, nothing more can be said.

Another example is Goldbach's conjecture that every number except 2 is the sum of two primes, unproved since 1742, which has considerable evidence for it but is believed to be far from being solved. Examples where the judgement of experts that the evidence for a conjecture was overwhelming was vindicated by subsequent proof include Fermat's Last Theorem and the classification of finite simple groups. [Franklin, 1987]

The correctness of the above arguments is not affected by the success or failure of any attempts to formalise, or give axioms for, the notion of non-deductive support between propositions. Many fields of study, such as geometry in the time of Pythagoras or pattern-recognition today, have yielded bodies of truths while still resisting reduction to formal rules. Even so, it is natural to ask whether the concept *is* easily formalisable. This is not the place for detailed discussion, since the problem has nothing to do with mathematics, and has been dealt with mainly in the context of the philosophy of science. The axiomatisation that has proved serviceable is the familiar axiom system of conditional probability: if h (for 'hypothesis') and e (for 'evidence') are two propositions, $\mathrm{P}(h|e)$ is a number between 0 and 1 inclusive expressing the degree to which h is supported by e, which satisfies

$$P(\text{not}-h|e) = 1 - P(h|e)$$
$$P(h'|h\&e) \times P(h|e) = P(h|h'\&e) \times P(h'|e)$$

While some authors, such as Carnap [1950] and Jaynes [2003] have been satisfied with this system, others (e.g. Keynes [1921] and Koopman [1940]) have thought it too strong to attribute an exact number to $\mathrm{P}(h|e)$ in all cases, and have weakened the axioms accordingly. Their modifications are essentially minor.

Needless to say, command of these principles alone will not make anyone a shrewd judge of hypotheses, any more than perfection in deductive logic will make him a great mathematician. To achieve fame in mathematics, it is only necessary to string together enough deductive steps to prove an interesting proposition, and submit the results to *Inventiones Mathematicae.* The trick is finding the steps. Similarly in non-deductive logic, the problem is not in knowing the principles, but in bringing to bear the relevant evidence.

The principles nevertheless provide *some* help in deciding what evidence will be helpful in confirming the truth of a hypothesis. It is easy to derive from the above axioms the principle

If $h\&b$ implies e, but $P(e|b) < 1$, then $P(h|e\&b) > P(h|b)$.

If h is thought of as hypothesis, b as background information, and e as new evidence, this principle can be expressed as 'The verification of a consequence renders a conjecture more probable', in Polya's words. [Polya, 1954, vol. II, 5] He calls this the 'fundamental inductive pattern'; its use was amply illustrated in the examples above. Further patterns of inductive inference, with mathematical examples, are given in Polya.

There is one point that needs to be made precise especially in applying these rules in mathematics. If e entails h, then $P(h|e)$ is 1. But in mathematics, the typical case is that e does entail h, though this is perhaps as yet unknown. If, however, $P(h|e)$ is really 1, how is it possible in the meantime to discuss the (non-deductive) support that e may give to h, that is, to treat $P(h|e)$ as not equal to 1? In other words, if h and e are necessarily true or false, how can $P(h|e)$ be other than 0 or 1?

The answer is that, in both deductive and non-deductive logic, there can be *many* logical relations between two propositions. Some may be known and some not. To take an artificially simple example in deductive logic, consider the argument

If all men are mortal, then this man is mortal
All men are mortal
Therefore, this man is mortal

The premises entail the conclusion, certainly, but there is more to it than that. They entail the conclusion in two ways: firstly, by *modus ponens*, and secondly by instantiation from the second premise alone. More complicated and realistic cases are common in the mathematical literature, where, for example, a later author simplifies an earlier proof, that is, finds a shorter path from established facts to the theorem.

Now just as there can be two deductive paths between premises and conclusion, so there can be a deductive and non-deductive path, with only the latter known. Before the Greeks' development of deductive geometry, it was possible to argue

All equilateral (plane) triangles so far measured
have been found to be equiangular
This triangle is equilateral
Therefore, this triangle is equiangular

There is a non-deductive logical relation between the premises and the conclusion; the premises support the conclusion. But when deductive geometry appeared, it was found that there was also a deductive relation, since the second premise alone entails the conclusion. This discovery in no way vitiates the correctness of the previous non-deductive reasoning or casts doubt on the existence of the non-deductive relation.

That non-deductive logic is used in mathematics is important first of all to mathematics. But there is also some wider significance for philosophy, in relation to the problem of induction, or inference from the observed to the unobserved.

It is common to discuss induction using only examples from the natural world, such as, 'All observed flames have been hot, so the next flame observed will be hot' and 'All observed ravens have been black, so the next observed raven will be black'. This has encouraged the view that the problem of induction should be solved in terms of natural laws (or causes, or dispositions, or the regularity of nature) that provide a kind of cement to bind the observed to the unobserved. The difficulty for such a view is that it does not apply to mathematics, where induction works just as well as in natural science.

Examples were given above in connection with the Riemann Hypothesis, but let us take a particularly straightforward case:

The first million digits of π are random
Therefore, the second million digits of π are random.

('Random' here means 'without pattern', 'passes statistical tests for randomness', not 'probabilistically generated'.)

The number π has the decimal expansion

$$3.14159265358979323846264338327950288419716939937...$$

There is no apparent pattern in these numbers. The first million digits have long been calculated (calcultions now extend beyond one trillion). Inspection of these digits reveals no pattern, and computer calculations can confirm this impression. It can then be argued inductively that the second million digits will likewise exhibit no pattern. This induction is a good one (indeed, everyone believes that the digits of π continue to be random indefinitely, though there is no proof), and there seems to be no reason to distinguish the reasoning involved here from that used in inductions about flames or ravens. But the digits of π are the same in all possible worlds, whatever natural laws may hold in them or fail to. Any reasoning about π is also rational or otherwise, regardless of any empirical facts about natural laws. Therefore, induction can be rational independently of whether there are natural laws.

This argument does not show that natural laws have no place in discussing induction. It may be that mathematical examples of induction are rational because there are *mathematical* laws, and that the aim in natural science is to find some substitute, such as natural laws, which will take the place of mathematical laws in accounting for the continuance of regularity. But if this line of reasoning is pursued, it is clear that simply making the supposition, 'There are laws', is of little help in making inductive inferences. No doubt mathematics is completely lawlike, but that does not help at all in deciding whether the digits of π continue to be random. In the absence of any proofs, induction is needed to support the law (if it is a law), 'The digits of π are random', rather than the law giving support to the induction. Either 'The digits of π are random' or 'The digits of π are not random' is a law, but in the absence of knowledge as to which, we are left only with the confirmation the evidence gives to the first of these hypotheses. Thus consideration of a mathematical example reveals what can be lost sight of in the search for laws: laws or no laws, non-deductive logic is needed to make inductive inferences.

These examples illustrate Polya's remark that non-deductive logic is better appreciated in mathematics than in the natural sciences. [Polya, 1954, vol. II, 24] In mathematics there can be no confusion over natural laws, the regularity of nature, approximations, propensities, the theory-ladenness of observation, pragmatics, scientific revolutions, the social relations of science or any other red herrings. There are only the hypothesis, the evidence and the logical relations between them.

9 CONCLUSION

Aristotelian realism unifies mathematics and the other natural sciences. It explains in a straightforward way how babies come to mathematical knowledge through perceiving regularities, how mathematical universals like ratios, symmetries and continuities can be real and perceivable properties of physical and other objects, how new applied mathematical sciences like operations research and chaos theory have expanded the range of what mathematics studies, and how experimental evidence in mathematics leads to new knowledge. Its account of some of the more traditional topics of the philosophy of mathematics, such as infinite sets, is less natural, but there are initial ideas on how to rival the Platonist and nominalist approaches to those questions. Aristotelianism will be an enduring option in twenty-first century philosophy of mathematics.

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