0039-3681(94)E0015-Z

The Formal Sciences Discover the Philosophers' Stone

*J. Franklin**

1. Introduction

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 fifty years of a number of new sciences, variously called the 'formal' or 'mathematical' sciences, or the 'sciences of complexity'[1] or 'sciences of the artificial'.[2]

The number of these sciences is large, very many people work in them, and even more use their results. It is a pity that philosophers have taken so little notice of them, since they provide exceptional opportunities for the exercise of the arts peculiar to philosophy. Firstly, their formal nature would seem to entitle them to the special consideration mathematics and logic have obtained. Being formal, they should appeal to the Platonist latent in most philosophers, especially those who suspect that most philosophical opinion about quantum mechanics, cosmology and evolution, for example, will probably be rendered obsolete by new scientific discoveries.

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. It will be argued that this appearance is

*School of Mathematics, University of New South Wales, Sydney 2052, New South Wales, Australia.
Received 8 *October* 1993; *in revised form* 28 *February* 1994.

[1]H. R. Pagels, *Dreams of Reason: The Computer and the Rise of the Sciences of Complexity* (New York: Simon & Schuster, 1988); D. L. Stein, *Lectures in the Sciences of Complexity* (Redwood City, CA: Addison-Wesley, 1989); M. M. Waldrop, *Complexity: The Emerging Science at the Edge of Order and Chaos* (New York: Simon & Schuster, 1992).
[2]H. A. Simon, *The Sciences of the Artificial* (Cambridge, MA: MIT Press, 1969; 2nd edn, 1981).

Pergamon

Stud. Hist. Phil. Sci., Vol. 25, No. 4, pp. 513–533, 1994.

0039–3681/94 $7.00+0.00

correct. Even if this is not so, and there is a gap between abstraction and reality, the gap is in some sense smaller here than it is elsewhere.

On the other hand, the word-oriented aspect of philosophy is also catered for. If one aim of studying philosophy is to be able to speak plausibly on all subjects, as Descartes says, then the formal sciences can be of assistance. They supply a number of concepts, like 'feedback', which permit 'in principle' explanatory talk about complex phenomena, without demanding too much attention to technical detail. It is just this feature of the theory of evolution that has provided a century of delight to philosophers, so the prospects for the formal sciences must be bright.

The formal sciences may appeal, too, to the many who feel that philosophers of science have chatted on to one another sufficiently about theory change, realism, induction, sociology, and so on, while real science has been producing a huge and diverse body of knowledge to which all that is totally irrelevant.

2. Examples

Since the article contends that the formal sciences are little known in the philosophical world, it is obviously impossible to assume any familiarity with them. There follows a minimal overview of these sciences, listing them and describing a typical problem or two in some of them. There is necessarily a considerable amount of pure description in this section. For convenience, the names of more or less identifiable sciences and sub-sciences are in bold type.

While antecedents can be found for almost anything, the oldest properly constituted formal science is **operations research** (OR). Its origin is normally dated to the years just before and during World War II, when multidisciplinary scientific teams investigated the most efficient patterns of search for U-boats, the optimal size of convoys, and the like.[3] Typical problems now considered are task scheduling and bin packing. Given a number of factory tasks, subject to constraints 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.[4] The methods used rely essentially on search through the possibilities, using mathematical ideas to rule out obviously wrong cases.

[3]F. N. Trefethen, 'History of Operations Research', in J. F. McCloskey and F. N. Trefethen (eds), *Operations Research for Management* (Baltimore: Johns Hopkins Press, 1954), vol. 1, pp. 3–35.

[4]C. D. Woolsey and H. S. Swanson, *Operations Research for Immediate Application: A Quick and Dirty Manual* (New York: Harper & Row, 1975).

To illustrate the diversity of activities in OR, the following are the subheadings in the American Mathematical Society's classification of 'Operations research and management science':[5]

Inventory, storage, reservoirs
Transportation, logistics
Flows in networks, 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.

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.

Another relatively old formal science is **control theory**, which aims to adapt a system, such as a chemical manufacturing plant, to some desired end, often by comparing actual and desired outputs and reducing the difference between these by changing the settings of the system.[6] To control theory belong two 'systems' concepts which have become part of public vocabulary. The first is feedback. (Of course, feedback *mechanisms* are much older,[7] but feedback as an object of abstract study came to prominence only with Wiener's work on '**cybernetics**' in the late 1940s.[8] The word 'feedback' is first recorded in English only in 1920, in an electrical engineering context; outside that area, it appears only from 1943.) The second concept is that of 'trade-off' ('a balance achieved between two desirable but incompatible features' — *Oxford English Dictionary*). It is first recorded in English in 1961.

[5]*Mathematical Reviews* (1990), Annual Index, Subject Index, p. S34.

[6]R. C. Dorf, *Modern Control Theory*, 5th edn (Reading, MA: Addison-Wesley, 1989); R. E. Kalman, P. L. Falb and M. A. Arbib, *Topics in Mathematical System Theory* (New York: McGraw-Hill, 1969).

[7]O. Mayr, *The Origins of Feedback Control* (Cambridge, MA: MIT Press, 1970).

[8]N. Wiener, *Cybernetics, or Control and Communication in the Animal and the Machine* (Cambridge, MA: Technology Press, 1948).

There is a not very unified body of techniques that deal with finding and interpreting structure in large amounts of data, called, depending on the context, **(descriptive) statistics**, **pattern recognition**, **signal processing** or **numerical taxonomy**. The names of products are even more varied: if one purchases a 'neural net to predict parolee recidivism' or an 'adaptive fuzzy logic classifier', one actually receives an implementation of a pattern recognition algorithm. Statistics is a science rather more than fifty years old, but the word usually refers to probabilistic inference from sample to population, rather than the simple finding of patterns in data that is being considered here. When one finds the average or median of a set of figures, one is not doing anything probabilistic, but merely finding some central point in the data. Drawing a bar graph of the year's profits from various sources is likewise simply summarising the data, allowing its structures or patterns to become evident. A typical technique in these sciences is **cluster analysis**. One lists various features for items to be classified; for example, to classify the stringed instruments of various cultures, one could list the number of strings, the ratio of length to width, and so on. It will normally happen that these lists of features fall naturally into clusters: items within clusters share similar profiles of features, while there are few items in the large 'spaces' between clusters.[9] It is normally hoped that the clusters are meaningful, and will allow sensible classification of new items. **Scene analysis**, or **image processing**, performs similar tasks for data which is laid out in two or three dimensions,[10] while **signal processing** and **time series analysis** deal with data streams in time, such as stock market prices and meteorological records.[11]

Then there are several sciences that study flows — of traffic, customers, information, or just flows in the abstract. Where will there be bottlenecks in traffic flow, and what additions of new links would relieve them? Such questions are studied with mathematical analysis and computer modelling in **network analysis**. There are obvious applications to telecommunications networks.[12] (It is this science that most naturally uses the widely-known technique of the flow diagram. Such diagrams are perhaps more often used to design the flow of control in, say, a computer program, but that simply illustrates the commonality of structures in many of these sciences.) Suppose customers arrive at a counter at random times, but at an average rate of 1 per minute. If the serving staff can process them at only 1 per minute, a long queue will form for much of the time. It is found that to keep the queue to a reasonable length most of the

[9]B. S. Everitt, *Cluster Analysis* 2nd edn (New York: Gower Halsted, 1986); J. A. Hartigan, *Clustering Algorithms* (New York: Wiley, 1975); R. R. Sokal and P. Sneath, *Principles of Numerical Taxonomy* (San Francisco: Freeman, 1963).

[10]R. O. Duda and P. E. Hart, *Pattern Classification and Scene Analysis* (New York: Wiley, 1973).

[11]S. A. Tretter, *Introduction to Discrete-Time Signal Processing* (New York: Wiley, 1976); C. Chatfield, *Analysis of Time Series: An Introduction*, 4th edn (London: Chapman & Hall, 1989).

[12]L. R. Ford and D. E. Fulkerson, *Flows in Networks* (Princeton: Princeton University Press, 1962).

time, the capacity of service needs to be about $1\frac{1}{2}$ customers per minute. This is a result in **queueing theory**, a discipline widely applied in telecommunications, since telephone calls also arrive at random times, but with predictable average rates.[13] The famous work of Shannon[14] in **information theory** drew attention to the problem of measuring the amount of information in a flow of 0s and 1s. A sub-branch is the theory of **data compression**: most messages have many redundancies in them, in that commonly occurring parts (like the word 'the' in English text) can be replaced by a single symbol, plus the instruction to replace this symbol with 'the' upon decompression. This allows the message to be stored and transmitted more efficiently. There are applications (or at least, attempted applications) to the DNA 'code'.[15] The use of 'entropy' by Shannon in measuring information relates this subject to thermodynamics. The sense in which thermodynamics resembles the formal sciences is discussed below.

The concept of expected payoff of different possible strategies for various actors in a (competitive or co-operative) environment allows analysis of systems whose dynamics depend on the interactions of such decisions. This is **game theory**. Such systems include business negotiations and competition,[16] animals preparing to fight,[17] and stock market trading. The American Mathematical Society's classification has the following sub-headings (the details are not important, only the overall impression of the diversity of structure that games can have):[18]

2-person games
n-person games, $n>2$
Noncooperative games
Cooperative games
Games with infinitely many players
Stochastic games
Multistage and repeated games
Differential games
Pursuit and evasion games
Decision theory for games
Game-theoretic models

[13] B. D. Bunday, *Basic Queueing Theory* (London: Arnold, 1986).

[14] C. E. Shannon, *The Mathematical Theory of Communication* (Urbana, IL: University of Illinois Press, 1949).

[15] M. S. Waterman (ed.), *Mathematical Models for DNA Sequences* (Boca Raton, FL: CRC Press, 1989).

[16] J. von Neumann and O. Morgenstern, *Theory of Games and Economic Behavior* (Princeton: Princeton University Press, 1944); L. G. Telser, *Competition, Collusion and Game Theory* (London: Macmillan, 1972); M. Shubik, *Game Theory in the Social Sciences* (Cambridge, MA: MIT Press, 1982).

[17] M. Enquist, *Game Theory Studies on Aggressive Behaviour* (Stockholm: University of Stockholm Press, 1984).

[18] *Mathematical Reviews* (1990), Annual Index, Subject Index, p. S34.

Positional games
Games involving graphs
Games involving topology or set theory
Combinatorial games
Discrete-time games
Games of timing
Probabilistic games; gambling
Hierarchical games
Spaces of games
Applications of game theory.

Note again that 'Applications' is a separate section; game theory itself is an abstract study. Possibly to be seen as a part of game theory are some aspects of **mathematical economics**, dealing with such questions as how people's individual preferences issue in expressions of global preference, that is, prices.[19] (The better-known areas of mathematical economics, involving modelling of interest rates, unemployment, and so on, have not of course produced certain knowledge about real economies, for reasons much debated.)

More recently there have emerged some overlapping sciences variously known as the **theory of self-organising systems**, the **theory of cellular automata**, **artificial life**, **non-equilibrium thermodynamics** and **mathematical ecology**. They all deal with how small-scale interactions in large systems create global patterns of organisation. As an example, the paradigm of cellular automata is the Game of Life. On an indefinitely large grid of squares, some of the cells are initially chosen as 'live'. The board then evolves according to these rules for updating:

Death by overcrowding: if 4 or more of the 8 cells surrounding a live cell are live, the cell 'dies'.
Death by exposure: if only one, or none, of the 8 cells surrounding a live cell is live, it dies.
Survival: a live cell with exactly 2 or 3 live neighbours survives.
Birth: a dead cell becomes live if exactly 3 of its 8 neighbours are live.

(Updates occur simultaneously at each time step.) The remarkable thing is that certain initial configurations lead to complicated and unexpected developing patterns, such as shapes that, after a certain number of 'generations', have produced several copies of themselves.[20] Similar self-organising phenomena, in

[19]G. Debreu, *The Theory of Value: An Axiomatic Analysis of Economic Equilibrium* (New Haven: Yale University Press, 1959); J. S. Kelly, *The Arrow Impossibility Theorems* (New York: Academic, 1978); J. W. S. Cassels, *Economics for Mathematicians* (Cambridge: Cambridge University Press, 1981).

[20]M. Gardner, 'Mathematical Games: The Fantastic Combinations of John Conway's New Solitaire Game "Life" ', *Scientific American* **223** (1970), issue 4, pp. 120–123; E. R. Berlekamp, J. H. Conway and R. K. Guy, *Winning Ways for your Mathematical Plays* (London: Academic, 1982), vol. 2, ch. 25; W. Poundstone, *The Recursive Universe* (Oxford: Oxford University Press, 1987), pp. 24–47 and chs 4, 6, 8, 10.

which complex systems arise out of simple local interactions, have been discovered in thermodynamic systems far from equilibrium.[21] The study of systems of interacting species of predators and prey in **mathematical ecology** likewise involves the prediction and explanation of global phenomena from local ones. As prey increase, so do predators, though more slowly. Then if the prey decrease, hordes of hungry predators can nearly wipe out the prey, leading to the near-extinction of the predators too; then the prey can slowly revive. The discovery of chaotic patterns in the cycles of predators and prey was one of the early discoveries of chaos theory.[22] There has been, of course, much resulting speculation about evolution, the origin of the universe, learning in the brain, and so on,[23] some of which will doubtless amount to something someday.

Most of the formal sciences use computers and mathematical modelling in one way or another. Indeed, the advent of the computer has been one of the main factors in the success of these subjects, in allowing results to be obtained in large-scale cases where hand computation is not feasible. But over and above the applications of computing in each science, there exists a **theoretical computer science**. (Computer science is here opposed to computer engineering. Computing, in its early days, was dominated by electrical engineers, as it was something of an achievement to construct hardware that did anything at all. But with the maturing of hardware and software techniques, the subject has sought the respectability of theory.) One branch is **computational complexity theory**. Typically, one wants to measure the intrinsic complexity of a problem, in terms of the number of simple operations (like additions or comparisons of single digits) needed to solve it. Since computation time is proportional to the number of simple operations, this will show whether it is realistic to solve the problem by computer. For example, the addition of two n-digit numbers (with the usual school algorithm) requires between n and $2n$ single-digit additions. The exact number depends on how many carries there are, as illustrated in the following example, where $n=4$, and there are 3 carries:

$$\begin{array}{cccc} 1 & 0 & 3 & 6 \\ 3_1 & 9_1 & 8_1 & 7 \\ \hline 5 & 0 & 2 & 3 \end{array}.$$

This requires 7 single-digit additions. Thus, as n grows, the amount of computation needed grows linearly with n, being bounded by $2n$. By contrast,

[21]G. Nicolis and I. Prigogine, *Self-Organization in Non-Equilibrium Systems* (New York: Wiley, 1977); F. E. Yates (ed.), *Self-Organizing Systems: The Emergence of Order* (New York: Plenum, 1987).

[22]R. May, *Theoretical Ecology: Principles and Applications*, 2nd edn (Oxford: Blackwell, 1981); H. T. Odum, *Systems Ecology* (New York: Wiley, 1983).

[23]P. C. W. Davies, *The Cosmic Blueprint* (New York: Simon & Schuster, 1988); E. Jantsch, *Self-Organizing Universe* (Oxford: Pergamon, 1980); S. A. Kauffman, 'Antichaos and Adaptation', *Scientific American* **265** (1991), issue 2, pp. 64–70; T. Kohonen, *Self-Organization and Associative Memory*, 3rd edn (Berlin: Springer, 1989); Waldrop, *op. cit.*, note 1, chs 3, 6.

the travelling salesman problem (to find the shortest route that visits n cities once each, given the distances between the cities) demands an amount of computation that grows exponentially with n (at least, this is believed, though not proved). This problem of 'combinatorial explosion' makes the travelling salesman problem infeasible for large n (in practice, for n larger than about 30).[24]

Other issues studied in theoretical computer science include formal specification (to describe exactly what a program is intended to do), and the effects of a modular or 'structured programming' design of programs, which is intended to make understanding and modifying them easier and safer. There is also the discipline of '**program verification**', or proof of the correctness of computer programs, of which more later.

Usually included in computer science is **artificial intelligence** (AI). The core of AI has come to consist of a combination of computer science and operations research techniques. To play chess by computer, for example, one employs guided search through the space of all possible moves and counter-moves from a given position. Complexity theory reveals that the space of *all* possible moves is far too big to search, so one observes human players to extract 'heuristics', that is, programmable strategies for deciding which of the possible moves are most worth searching next.[25] AI might seem to contradict the assertion that there has been little philosophical interest in any of the formal sciences. It is true, of course, that the philosophy of mind has given much attention to AI, but only for its usefulness as a model of mental workings. True AI workers, on the contrary, tend to be embarrassed by the connection with the mind, and seek to re-badge their product as 'expert systems' or 'adaptive information processing'. The reason is that the computer science view of AI is of an independent discipline concerned with guided search through trees of possibilities, which can only be harmed in the marketplace by unfulfillable claims about imitating the human mind.

There is some theory of **computer simulation** applicable across all subject matters; it studies, for example, the losses in accuracy that arise in modelling a continuous situation on a digital computer.[26] It is possible to change what the variables in a computer simulation mean, rendering the same entity a simulation of something else. To take an excessively simple example, if money is invested at 1% per month compound interest, the accumulated amount after t months, P_t, is related to the amount of the month before, P_{t-1}, by

$$P_t = P_{t-1} + (1/100)\, P_{t-1}.$$

[24]H. S. Wilf, *Algorithms and Complexity* (Englewood Cliffs, NJ: Prentice-Hall, 1986).

[25]P. Frey (ed.), *Chess Skill in Man and Machine*, 2nd edn (New York: Springer, 1983); I. Bratko, *Prolog Programming for Artificial Intelligence*, 2nd edn (Wokingham: Addison-Wesley, 1990).

[26]D. Edwards and M. Hamson, *Guide to Mathematical Modelling* (Basingstoke: Macmillan, 1989); F. Neelamkavil, *Computer Simulation and Modelling* (Chichester: Wiley, 1987); B. P. Ziegler, *Theory of Modelling and Simulation* (New York: Wiley, 1976).

This equation expresses the local structure, the connection between the amounts in consecutive months. The bank's computer starts out with the original principal, and goes through step by step using this equation to calculate the accumulated amount after t months. The resulting global structure is represented by the familiar rising exponential growth curve. But P_t could just as well mean the temperature of a rod t notches from the left-hand end. If it happens that the temperature at any notch is 1% more than the temperature of the notch to its left, then the problem has the same local structure, and the same equation, and the same graph, showing the temperature increasing exponentially from its value at the left-hand end. What is being modelled on the computer is, therefore, independent of whether the quantity varying is money or temperature, and independent of whether these quantities are varying with respect to time or space.

The computer simulation of, say, the growth of a city, will exhibit phenomena explainable as the results of gradual accumulation of interactions among its parts, the details depending on the assumptions made about, for instance, the impact of siting a factory near a residential area on the medium-term development of the area.[27]

It is true that studying real phenomena by mathematical modelling involves measurement and observation, as well as purely formal work. This matter will be discussed in the last section.

In retrospect, certain aspects of theoretical physics have a character recognisably like the formal sciences. **Statistical mechanics**, going back to Maxwell and Boltzmann, looks at how macroscopic properties of gases, like pressure and temperature, arise as global averages of the movements of the individual particles.[28] The emphasis is not on details about the properties of the particles themselves, but on the transition from local to global properties. The same is true of **fluid dynamics**, especially in the very difficult study of turbulent fluids. The organisation of fluid flow into eddies and smoke rings is plainly not to be explained by examining the individual atoms more closely.[29] **Non-linear physics** treats more generally the ways in which complicated global structures can arise from simple local interactions.[30]

3. The Formal Sciences Search for a Place in the Sun

It is at first sight strange that so many new sciences have appeared, without attracting much interest from philosophers of science. It could be argued that

[27] P. M. Allen *et al.*, *Models of Urban Settlement and Structure as Dynamic Self-Organizing Systems* (Washington, D.C.: U.S. Dept of Transportation, 1981).

[28] J. von Plato, 'Probabilistic Physics the Classical Way', in L. Krüger *et al.* (eds) *The Probabilistic Revolution* (Cambridge, MA: MIT Press, 1987), vol. 2, pp. 379–407.

[29] J. O. Hinze, *Turbulence*, 2nd edn (New York: McGraw-Hill, 1975); J. Gleick, *Chaos* (London: Heinemann, 1988), pp. 119–153.

[30] R. Z. Sagdeev, D. A. Usikov and G. M. Zaslavsky, *Non-Linear Physics: From the Pendulum to Turbulence and Chaos*, trans. I. R. Sagdeev (Chur: Harwood Academic, 1988).

there is simply not much new in them, and, like accountancy perhaps, there is just nothing very philosophical about them, although many people work in them. It is more likely, however, that the philosophical profession has not created an internal representation of the formal sciences in general, because no one has clearly described their common core.

It is easy to say something imprecise about this, but harder to be definite. An attempt was made to group some of these topics together, and claim great things for them, under the name 'general systems theory'.[31] But the attempt was regarded on the whole as too vacuous to cast light on anything, and it made little impression on either the scientific or philosophical worlds. The problem was that just about anything is a 'system', so it is not clear what is the content of the assertion that something should be studied 'as a system'.

So, is it possible to say more precisely what it is that the formal sciences have in common, which distinguishes them from other sciences?

First of all, one might ask whether the formal sciences can be assimilated to something that is already adequately understood philosophically. The two candidates are engineering (or technology generally) and applied mathematics.

The question is not one of the status of engineering, or the philosophy of technology. While technology in general, and engineering in particular, have been rather neglected subjects in philosophy, it is generally agreed that what is interesting philosophically about them is precisely their *differences* from science. It is said, for example, that engineering is not well seen as simply applied science, for one reason or another.[32] But the formal sciences, though they arose in most cases out of engineering requirements, *are* sciences, and can be pursued without reference to applications. This is no more than can be said of most sciences; geometry was originally thought of in connection with land surveying but then studied in the abstract, just as more recently network flow analysis was invented for studying the flows of liquids, telephone calls and factory products, but can be studied without any reference to what material it is that flows.

So, are the formal sciences applied mathematics?

There are at least *some* reasons for regarding the formal sciences as something beyond applied mathematics. They are certainly not applied mathematics in the sense that they are applications of already existing bodies of pure mathematics: in almost all cases, the mathematics had to be created, to solve the problems thrown up by the demands of the subject. (But, then, the same is true of some parts of traditional applied mathematics.) More significantly, applied

[31]L. von Bertalanffy, *General System Theory* (New York: Braziller, 1969); G. J. Klir, *An Approach to General Systems Theory* (New York: Van Nostrand Reinhold, 1969); R. L. Flood and E. R. Carson, *Dealing with Complexity: An Introduction to the Theory and Application of Systems Science* (New York: Plenum, 1988); I. Hoos, *Systems Analysis in Public Policy: A Critique* (Berkeley: University of California Press, 1972).

[32]R. Laymon, 'Applying Idealized Scientific Theories to Engineering', *Synthese* **81** (1989), 353–371; T. H. Broome, 'Engineering and the Philosophy of Science', *Metaphilosophy* **16** (1985), 47–56.

mathematics does seem too narrow to include all, or even most, of the formal sciences. The early researchers in OR and most of the other formal sciences came as often from physics, engineering and biology as from mathematics, and found their skills just as relevant as those of the mathematicians. And it is interesting that librarians have extraordinary difficulty in classifying the formal sciences, though classification is their profession. Many of the books referred to in this paper are classified under Dewey numbers 001, 003 and 004 — that is, 'general'. This is wrong, since the formal sciences are obviously not 'general' in the sense of, say, encyclopedias. Clearly, they do have something in common, distinguishing them from other pursuits. The philosophical question is, what is it?

On the other hand, it is obvious that the formal sciences *are* either applied mathematics, or something very closely related. (Is it possible, then, to create a new formal science by placing the word 'mathematical' in front of the name of an old science? The title *Mathematical Ethology* seems to be still free; but there are already books on 'quantitative ethology'[33] — one will need to be quick. Perhaps *Mathematical Ethnology* would be a better bet.) It may in fact be a historical accident that the formal sciences are not actually called applied mathematics and housed in departments of applied mathematics. In the mid-century, mathematics was going through a particularly pure phase, obsessed with rigour and generality,[34] and was not receptive to new applied disciplines. Of the leading mathematicians, only von Neumann and Norbert Wiener took any serious notice of the new directions. By default, the formal sciences had to find academic homes in corners of departments of engineering, economics and business, psychology or whoever else would have them.

The important point philosophically is that nothing depends on there being any distinction between the formal sciences and applied mathematics. It is certainly not being maintained here that the formal sciences have discovered a new 'philosophers' stone' which mathematics has overlooked. It is not likely that the formal sciences have discovered ways of being certain about really instantiated structures which are essentially different to those in mathematics. The philosophical interest of the formal sciences is that they promise to circumvent the defences that philosophers have evolved against the claim that mathematics offers certainty about the real world. Those well-known defences are the ones summarised in Einstein's 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.[35]

[33]P. W. Colgan (ed.), *Quantitative Ethology* (New York: Wiley, 1978); D. McFarland and A. Houston, *Quantitative Ethology: The State–Space Approach* (Boston: Pitman, 1981).

[34]J. D. Gray, 'The Development of Mathematics: A response to Monna', *Nieuw Archief voor Wiskunde* **3** (1985), 1–6; Gleick, *op. cit.*, note 29, p. 52, p. 89.

[35]A. Einstein, *Ideas and Opinions* (New York: Crown, 1954), p. 233.

Variations of this thought include claims that mathematics is about 'idealisations' or 'abstractions', or that it is purely about what follows from (uninterpreted) axioms. Perhaps these defences can be overcome;[36] nevertheless, it may be easier to circumvent them by moving the battleground to sciences where the standard defences are not so easily deployed — or at least do not have the same initial plausibility. The next section works through some examples, to see how they do resist Platonist defences directly, without needing any tedious excursus through such questions as the reality or abstractness of numbers or sets.

It would be desirable to have a unified theory that covered mathematics, pure and applied, as well as the formal sciences, and explained both their affinity and their differences. This is so far no help, philosophically, as it is not known what the philosophical status of applied mathematics is. Apart from some efforts in Körner's book,[37] few philosophers of mathematics this century have directly attacked the problem — though most have been willing enough to accuse Platonism of failing to solve it. Interest in the philosophy of mathematics has centred on foundations (early in the century) and on whether certainty is attainable through proof, along with some allegedly sociological problems (more recently). This has left the relation of (pure) mathematics to the real world to be covered by the unexamined dual notions of 'application to' and 'idealisation from'.

There is one school in the philosophy of mathematics, however, whose opinions are capable of extension to cover the formal sciences. The founding event for the school was Benacerraf's observation, in his paper 'What Numbers Could Not Be',[38] that since the natural numbers could be regarded equally well as either the set

$$\phi,\ \{\phi\},\ \{\{\phi\}\},\ \ldots$$

or

$$\phi,\ \{\phi\},\ \{\phi,\{\phi\}\},\ \ldots$$

(or many others), the subject-matter of arithmetic could not be any of these particular sets, but must be something that those sets had in common. He concluded: 'Arithmetic is therefore the science that elaborates the abstract structure that all progressions have in common merely in virtue of being progressions.'

[36]J. Franklin, 'Mathematical Necessity and Reality', *Australasian Journal of Philosophy* **67** (1989), 286–294.

[37]S. Körner, *The Philosophy of Mathematics* (London: Hutchinson, 1960); cf. M. Steiner, 'The Application of Mathematics to Natural Science', *Journal of Philosophy* **86** (1989), 449–480.

[38]P. Benacerraf, 'What Numbers Could Not Be', *Philosophical Review* **74** (1965), 47–73, reprinted in P. Benacerraf and H. Putman (eds), *Philosophy of Mathematics*, 2nd edn (Cambridge: Cambridge University Press, 1983), pp. 272–294.

Subsequently, Resnik[39] and Steen[40] argued that mathematics is the science of 'patterns', Shapiro[41] and Parsons[42] that it was the science of 'structure'. Hellman[43] chose 'structural possibilities'. Forrest and Armstrong[44] took numbers to be certain real relations between properties, 4 being the relation that holds between being an aggregate of four parrots and being a parrot; Armstrong[45] argued further that sets are certain kinds of states of affairs. Bigelow and Pargetter[46] also took numbers to be relations, interpreted realistically, but emphasised ordinal relations and ratios of quantities. Field[47] argued that references in physics apparently to continuous functions should be construed as contentful claims about variations of quantities like temperature over space–time regions. As Maddy[48] remarks, though the differences between these views are real enough, they are small compared to the agreement. They agree that the objects of mathematics should not be interpreted in a Platonist sense, but should be reinterpreted as things available through ordinary sense perception.

From the present point of view, it is unfortunate that these writers also agree that the main point of the philosophy of mathematics is to explain what numbers are, as if once that were done, everything else would be clear. This is not obviously true. To have understood numbers is no guarantee that one understands symmetry, or continuity, or network topology. Those things are not made out of numbers. They are not made out of sets, either, though one can construct models of them in set theory — if one knows already exactly what structure one wants to imitate. Nevertheless, it is not hard to extend a view of numbers as structural to the view that there are many other structures, of which some are symmetry, continuity and network topology.

But thinking that one should start with numbers and 'extend' to symmetry and continuity already puts structuralism and similar theories at an unfair disadvantage, as it requires them to explain the most unfavourable example first. Numbers and sets are not structuralism's home turf. Cardinality is an

[39]M. Resnik, 'Mathematics as a Science of Patterns: Ontology and Reference', *Nous* **15** (1981), 529–550.

[40]L. A. Steen, 'The Science of Patterns', *Science* **240** (1988), 611–616.

[41]S. Shapiro, 'Mathematics and Reality', *Philosophy of Science* **50** (1983), 523–548, section 3, with further references; S. Shapiro, 'Structure and Ontology', *Philosophical Topics* **17** (1989), 145–171.

[42]C. Parsons, 'The Structuralist View of Mathematical Objects', *Synthese* **84** (1990), 303–346.

[43]G. Hellman, *Mathematics Without Numbers: Towards a Modal-Structural Interpretation* (Oxford: Clarendon, 1989); cf. P. Bricker, 'Plenitude of Possible Structures', *Journal of Philosophy* **88** (1991), 607–619.

[44]P. Forrest and D. M. Armstrong, 'The Nature of Number', *Philosophical Papers* **16** (1987), 165–186.

[45]D. M. Armstrong, 'Classes are States of Affairs', *Mind* **100** (1991), 189–200.

[46]J. Bigelow and R. Pargetter, *Science and Necessity* (Cambridge: Cambridge University Press, 1990), sections 2.5, 8.2, 8.3.

[47]H. Field, *Science Without Numbers* (Princeton: Princeton University Press, 1980), ch. 8.

[48]P. Maddy, 'Philosophy of Mathematics: Prospects for the 1990s', *Synthese* **88** (1991), 155–164; cf. A. D. Irvine, 'Nominalism, Realism and Physicalism in Mathematics', in A. Irvine (eds), *Physicalism in Mathematics* (Dordrecht: Kluwer, 1990), pp. ix–xxvi.

almost degenerate structure, as it arises merely from a heap's being divisible. In counting, the only relation between parts that is relevant is their mutual distinctness. It is only when richer inter-relations between parts are considered that symmetry, continuity and the others arise, and the structuralist view of mathematics comes into its own. It is also where the formal sciences begin.

A structuralist account of the formal sciences is, then, already available in structuralist philosophies of mathematics in general. The only thing to be added is an explanation of *what* structures exactly are studied by the particular disciplines. But that is a mathematical question, and the answer is found (if not always clearly expressed) in the axioms and definitions of each discipline. Topology studies one kind of structure, whose nature is captured by the definition of a topological space; information theory studies another. Conversely, a structuralist account of the formal sciences is an advantage for the philosophy of structuralism in mathematics. Since we recognise the similarity between the formal sciences, traditional applied mathematics, and pure mathematics, we should prefer a philosophy of mathematics that demonstrates their unity.

4. Real Certainty: Program Verification

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.[49]

Some other authors explain the idea entertainingly:

> By contrast [to hardware], a computer program is built from ideal mathematical objects whose behaviour is defined, not modelled approximately, by abstract rules. When an if-statement follows a while-statement, there is no need to study whether the if-statement will draw power from the while-statement and thereby distort its output, or whether it could overstress the while-statement and make it fail.[50]

Recently, the philosopher James Fetzer[51] 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's argument relies wholly on the gap between abstraction and 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.[52]

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.[53] And there can be changes in the hardware and environment between the time of the proof and the time of operation.[54] 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

[49] C. A. R. Hoare, 'An Axiomatic Basis for Computer Programming', *Communications of the Association for Computing Machinery* **12** (1969), 576–580.

[50] R. Stallman and S. Garfinkle, 'Against Software Patents', *Communications of the Association for Computing Machinery* **35** (1992), 17–22 and 121, at p. 19.

[51] J. H. Fetzer, 'Program Verification: The Very Idea', *Communications of the Association for Computing Machinery* **31** (1988), 1048–1063.

[52] J. H. Fetzer, 'Program Verification Reprise: The Author's Response', *Communications of the Association for Computing Machinery* **32** (1989), 377–381.

[53] J. Barwise, 'Mathematical Proofs of Computer System Correctness', *Notices of the American Mathematical Society* **36** (1989), 844–851.

[54] M. M. Lehman, 'Uncertainty in Computer Application', *Communications of the Association for Computing Machinery* **33** (1990), 584–586.

of proofs of correctness had admitted that such proofs could not detect, for example, typos.[55] 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.[56] 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[57] — '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

[55]D. Gries, *The Science of Programming* (New York: Springer, 1981), p. 5.
[56]C. A. R. Hoare, 'Programs are Predicates', in C. A. R. Hoare and J. C. Stephenson (eds), *Mathematical Logic and Programming Languages* (Englewood Cliffs, NJ: Prentice-Hall, 1985), pp. 141–155.
[57]Unless he is Hume: *Treatise* I.IV.i.

two trains on the same track, no matter what failures occurred.[58] The advertisement that said:

> VIPER is the first commercially available microprocessor with both a formal specification and a proof that the chip conforms to it

was felt by the experts to be a danger to the gullible public, but not an impossibility in principle.[59] An aggrieved purchaser began legal action on the grounds that the proof was not complete, but the bankruptcy of the plaintiff unfortunately prevented this interesting philosophical debate from being pursued in the courts.[60]

5. Real Certainty: The Other Formal Sciences

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.

Let us follow these assertions through in an example from another of the formal sciences. The example itself is an old one, due to Euler in the mid-eighteenth century. Then an isolated curiosity, it is now regarded as the first study in the topology of networks. The citizens of Königsberg noticed that it seemed to be impossible to walk across all seven bridges over the River Pregel, without walking across at least one of them twice (see Fig. 1). Euler proved their conjecture correct, using the simple idea that if one enters and leaves a land area, one uses up two of the bridges. Thus, all the land areas (except the two chosen for the start and finish) must have an even number of bridges leaving them, or there will necessarily be bridges left over, no matter what route is chosen.[61] But in the example, all four land areas have an odd number of bridges leaving them, so a path going across all the bridges exactly once is impossible.

[58] H. M. Müller, letter, in *Communications of the Association for Computing Machinery* **32** (1989), 506–508; cf. Hoare, *op. cit.*, note 56.

[59] J. Dobson and B. Randell, 'Program Verification: Public Image and Private Reality', *Communications of the Association for Computing Machinery* **32** (1989), 420–422.

[60] D. MacKenzie, 'Computers, Formal Proofs and the Law Courts', *Notices of the American Mathematical Society* **39** (1992), 1066–1069.

[61] L. Euler, 'The Koenigsberg Bridges', trans. in *Scientific American* **189** (1953), issue 1, pp. 66–70; cf. J. Franklin, 'Mathematics, the Computer Revolution and the Real World', *Philosophica* **42** (1988), 79–92.

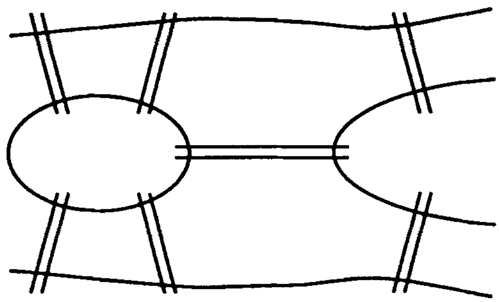

Fig. 1. The Königsberg bridges.

Notice also that it is possible to solve the problem without any ingenuity at all, by simply checking by computer whether all the possible paths which do not go over any bridge twice (there are certainly less than a thousand of these) go over all bridges once. It is a less exciting method, but the result is exactly the same: it demonstrates an impossibility about an actual physical thing, resulting from its structure. No idealisation is needed to get a mathematical result.

There are two ways to see what has happened, one epistemological and one metaphysical. On the first view, there is no metaphysical necessity involved, since how bridges are arranged is a contingent fact. The only necessities, as with any mathematical model, lie within the model. The interest in these examples, and in the formal sciences more generally, would be that the model–reality gap is narrow, in that it is very easy to establish by direct perception whether the model does apply to reality. Then, the small amount of data would result in many facts being deducible, some of them surprising. The formal sciences would be of interest primarily for the narrowness of the gap, but it would be wrong to claim that they had discovered any 'philosophers' stone' for discovering necessities.

Here, the stronger metaphysical reading will be defended. The difficulty with dismissing necessity claims on the grounds that how the bridges are arranged is contingent is that it proves too much. *Any* necessity claim, about any aspect of reality, could be so dismissed. The necessity of 'scarlet things must be red' (whatever kind of necessity that is) is not subverted by the observation that it is a contingent matter whether something is scarlet. The same even applies to 'red things must be red'. The necessity is not in things' being scarlet or red, but in the necessary connection between being scarlet and being red (or being red and being red). (Nor is it a 'hypothetical' necessity, though one can express it if one desires as 'if something is scarlet, then it is red'; but any connection between universals can be so expressed.) It is the same with the bridges. Of course it is a contingent matter where the bridges were built. The necessity lies in the connection between the bridges having the arrangement they do and the properties of paths through them. And like scarlet, arrangements of bridges and properties of paths are possessed not by abstract models, but by real systems.

The motivation for trying to replace the bridges by a model in which to perform deductions may be further reduced by the following considerations. Checking the paths is the same kind of activity as checking the steps in a (fully expanded, purely syntactic) proof in symbolic logic or set theory. It gives the same kind of certainty, for the same reason. If one succeeded in expressing Euler's proof, or the brute force proof, as a sequence of steps in predicate calculus, one would not have achieved either more certainty, or certainty of a different kind. There would be exactly the same kind of necessary connection between the individual steps, resulting in certainty, modulo the usual understanding that one has not misperceived any of the symbols. Again, one *can* move to a model made out of sets, but that model has literally the same topological structure as the real system of bridges. A truth about the network structure applies to the bridges as directly as to the sets, not to the bridges via the sets.

In fact, one can just as well regard a proof in symbolic logic as an exercise in network theory, as vice versa. In the lattice of propositions, some are linked directly to one another by logical relations like modus ponens and contraposition. The relations are purely syntactic, that is, checkable by direct inspection of the symbol strings. One seeks a proof, that is, a path through the lattice from premise to conclusion. This is why it is irrelevant that mathematical proofs and computer programs are logical, or textual, while bridges are stone or steel, and structured entities in the other formal sciences may be electronic, biochemical, mental, astral, legal, flesh, fish or fowl.

If one is still inclined to think that any instantiation in physical materials must create a gap between abstract system and (possibly faulty) mechanism, then one must remember that there is the same gap in logic. The distinction the Poles used to make between 'socialism' and 'really existing socialism' has a counterpart in that between logic and actually implemented logical inference. Since formal systems are systems of symbol-types, not symbol-tokens,[62] the act of classifying tokens into types must be part of any implementation. Therefore, in a brain, syntactic symbol processing of discrete symbols has only the reliability permitted by the optical character recognition capabilities of the underlying architecture. These are limited in principle, and unreliable in fact, as anyone checking the addition of a column of figures, or proof-reading, knows. And this is always assuming that the brain does implement deduction by syntactic symbol processing; if deduction in humans is actually done with models or simulations, as many experiments suggest,[63] then real logic is even more obviously on a par with the other formal sciences.

[62]M. Tiles, *Mathematics and the Image of Reason* (London: Routledge, 1991), ch. 5.
[63]P. N. Johnson-Laird and R. Byrne, *Deduction* (Hove: Erlbaum, 1991).

It is with Euler's diagram in mind that we should attempt to fit the formal sciences into the long war of the Empiricists and the Rationalists. In the Empiricist's heaven, science is mostly observation, and the organising of observations into universal statements. There are no 'necessary connections between distinct existences' (not logically or mathematically necessary connections, at least, even if there may be 'nomic' necessities). D'Alembert[64] describes the Rationalist's heaven: most of science consists of mathematical deduction from certain extremely simple facts. These facts are, in the best case, symmetry principles clear *a priori*, and in the worst case easily measurable numerical relationships like Galileo's law of free fall. Developments in the formal sciences suggest we are closer to the Rationalist's heaven than, perhaps, we believed. The computer has much shortened our stay in the Rationalist's purgatory, the frustrating state of being unable to perform the complicated deductions we know must be possible.

As Aristotle says, discussing the relation of propositions in optics and astronomy to those in mathematics: 'For here it is for the empirical scientists to know the fact and for the mathematical to know the reason why; for the latter have the demonstrations of the explanations.'[65]

6. Experiment in the Formal Sciences

Real certainty for armchair work — surely this is too rosy a picture of the formal sciences? If it were right, it ought to be possible to issue real-world predictions by computer, without needing to do any experiments. Anyone who has worked in applied mathematics knows it is rarely like that. It is well known that fitting a realistic mathematical model to actual data is in general difficult. Sometimes, as in meteorology and macroeconomics, it is virtually impossible.

To explain when experiment and fitting to data are necessary, one must return to the gap Fetzer insisted on between the abstract model and the real world. Everyone agrees that formal work can proceed with the usual necessity of mathematics, provided one keeps within the model. The important point is that there is wide variability in the certainty in deciding whether the real world has the structure described by the model. The model–reality gap may be wide or narrow. The word 'model' directs attention to cases where fitting is difficult, by the implied suggestion that there may be many models, among which it is difficult to choose. The extreme case is stock-market prediction, where there are plenty of models, but nearly total uncertainty as to which if any fit the data. Any case where an underlying structure has to be inferred from insufficient data

[64] D'Alembert, article 'Expérimentale', in *Encyclopédie*, vol. 6 (Paris, 1756), reprinted in J. Lough, *The Encyclopédie of Diderot and D'Alembert* (Cambridge: Cambridge University Press, 1969), pp. 68–81.

[65] Aristotle, *Posterior Analytics* 79a4–6; cf. *Metaphysics* 1078a14–17; H. G. Apostle, *Aristotle's Philosophy of Mathematics* (Chicago: University of Chicago Press, 1952), ch. 4.

will be like that to a greater or lesser extent. The examples above were chosen near the opposite extreme, even, so it was argued, to the extent that there was no gap at all. What structure a system of bridges or a computer program has is open to perceptual inspection, with the practical certainty that attends unimpeded sense perception. So all the hard work is in the mathematics, and the results are directly applicable, again with practical certainty. Examples like the statistical mechanics of gases fall somewhere in between, but still closer to the formal end. Whether the kinetic theory of gases is true is a contingent fact, not easily established. But it is in fact true, and the way temperature arises from the random motion of gas particles is a matter of necessity. Though it is harder than in the case of the bridges to determine if things have the properties, there is real necessity in the *connections* of the properties. Being provable, it is a stronger necessity than nomic or Kripkean necessities.

There is another kind of experiment in the formal sciences: 'numerical experiment'. It also contributes to uncertainty in the formal sciences, but it should be distinguished from model-fitting work. It is part of the purely mathematical investigations, and is used when the mathematical model is hard to solve ('solving' generally means deriving global from local structure). Usually, the problem is that the model is too complex for the mathematical methods available, but it may also happen, as in chaos theory, that a quite simple model does not admit of a solution by normal methods. In such cases one runs the model on a computer, perhaps with various choices of values of parameters, and graphs the results in an effort to understand the structures that result. Any conjectures based on these experiments will be uncertain (unless a proof can be found later). That sort of uncertainty, though, is found even in pure mathematics. There, the Riemann Hypothesis and other conjectures that have resisted proof are studied by collecting numerical evidence by computer;[66] there is enough work of this kind to justify a journal *Experimental Mathematics*. The existence of numerical experiments is therefore not an objection to the claim that the formal sciences can often achieve mathematical certainty about the world. Instead it confirms their affinity with pure mathematics.

Acknowledgements — I am grateful to Peter Hall, Peter Forrest and a referee for *Studies in History and Philosophy of Science* for valuable comments.

[66]J. Franklin, 'Non-Deductive Logic in Mathematics', *British Journal for the Philosophy of Science* **38** (1987), 1–18.