Introduction

This book can be read on three levels: first, as a description of the practices of Greek mathematics; second, as a theory of the emergence of the deductive method; third, as a case-study for a general view on the history of science. The book speaks clearly enough, I hope, on behalf of the first two levels: they are the explicit content of the book. In this introduction, I give a key for translating these first two levels into the third (which is implicit in the book). Such keys are perhaps best understood when both sides of the equation are known, but it is advisable to read this introduction before reading the book, so as to have some expectations concerning the general issues involved.

My purpose is to help the reader relate the specific argument concerning the shaping of deduction to a larger framework; to map the position of the book in the space of possible theoretical approaches. I have chosen two well-known landmarks, Kuhn's *The Structure of Scientific Revolutions* and Fodor's *The Modularity of Mind*. I beg the reader to excuse me for being dogmatic in this introduction, and for ignoring almost all the massive literature which exists on such subjects. My purpose here is not to argue, but just to explain.

THE STRUCTURE OF SCIENTIFIC REVOLUTIONS

The argument of Kuhn (1962, 1970) is well known. Still, a brief résumé may be useful.

The two main conceptual tools of Kuhn's theory are, on the one hand, the distinction between 'normal science' and 'scientific revolutions' and, on the other hand, the concept of 'paradigms'. Stated very crudely, the theory is that a scientific discipline reaches an important threshold – one can almost say it begins – by attaining a paradigm. It then becomes normal science, solving very specific questions within the framework of the paradigm. Finally, paradigms may change and, with them, the entire position of the discipline. Such changes constitute scientific revolutions.

What Kuhn meant by paradigms is notoriously unclear. One sense of a 'paradigm' is a set of metaphysical assumptions, such as Einstein's concept of time. This sense is what has been most often discussed in the literature following Kuhn. The focus of interest has been the nature of the break involved in a scientific revolution. Does it make theories from two sides of the break 'incommensurable', i.e. no longer capable of being judged one against the other?

I think this is a misguided debate: it starts from the least useful sense of 'paradigm' (as metaphysical assumptions) – least useful because much too propositional. To explain: Kuhn has much of interest to say about normal science, about the way in which a scientific community is united by a set of practices. But what Kuhn failed to articulate is that practices are just that – practices. They need not be, in general, statements in which scientists (implicitly or explicitly) believe, and this for two main reasons.

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

Second, beliefs, in themselves, cannot *explain* the scientific process. Statements lead on to statements only in the logical plane. Historically, people must intervene to get one statement from the other. No belief is possible without a practice leading to it and surrounding it. As a correlate to this, it is impossible to give an account of the scientific process without describing the practices, over and above the beliefs.

This book is an extended argument for this thesis in the particular case of Greek mathematics. It brings out the set of practices common to Greek practitioners, but argues that these practices were generally 'invisible' to the practitioners. And it shows how these practices functioned as a glue, uniting the scientific community, and making the production of 'normal science' possible. The study is therefore an empirical confirmation of my general view. But the claim that 'paradigms' need not be propositional in nature should require no empirical confirmation. The propositional bias of Kuhn is a mark of his times. *The Structure of Scientific Revolutions* may have signalled the end of positivism in the history and philosophy of science, but it is itself essentially a positivist study, belonging (albeit critically) to the tradition of the *International Encyclopedia of Unified Science*, its original place of publication. It is a theory about the production of propositions from other propositions. To us, however, it should be clear that the stuff from which propositions are made need not itself be propositional. The process leading to a propositional attitude – the process leading to a person's believing that a statement is true – consists of many events, and most of them, of course, are not propositional. Kuhn's mistake was assimilating the process to the result: 'if the result is propositional, then so should the process be'. But this is an invalid inference.

Much has happened since Kuhn, and some of the literature in the history of science goes beyond Kuhn in the direction of non-propositional practices. This is done mainly by the sociologists of science. I respect this tradition very highly, but I do not belong to it. This book should not be read as if it were 'The Shapin of Deduction', an attempt to do for mathematics what has been so impressively done for the natural sciences.[1] My debt to the sociology of science is obvious, but my approach is different. I do not ask just what made science the way it was. I ask what made science successful, and successful in a real intellectual sense. In particular, I do not see 'deduction' as a sociological construct. I see it as an objectively valid form, whose discovery was a positive achievement. This aspect of the question tends to be sidelined in the sociology of science. Just as Kuhn assimilated the process to the result, making them both propositional, so the sociologists of science (in line with contemporary pragmatist or post-modern philosophers) assimilate the result to the process. They stress the non-propositional (or, more important for them, the non-objective or arbitrary) aspects of the process leading to scientific results. They do so in order to relativise science, to make it seem less propositional, or less ideology-free, or less objective.

But I ask: what sort of a process is it, which makes possible a positive achievement such as deduction? And by asking such a question, I am

[1] E.g. (to continue with the distinguished name required by the pun) in Shapin (1994).

led to look at aspects of the practice which the sociologist of science may overlook.[2]

To return to Kuhn, then, what I study can be seen, in his terms, as a study of the paradigms governing normal science. However, this must be qualified. As regards my paradigms, they are sets of practices and are unverbalised (I will immediately define them in more precise terms). As regards normal science, there are several differences between my approach and that of Kuhn. First, unlike – perhaps – Kuhn, and certainly unlike most of his followers, the aim of my study is explicitly to explain what makes this normal science successful in its own terms. Further: since my view is that what binds together practitioners in normal science is a set of practices, and not a set of beliefs, I see revolutions as far less central. Development takes the form of evolution rather than revolution. Sets of practices are long-lived, in science as elsewhere. The historians of the *Annales* have stressed the conservatism of practice in the material domain – the way in which specific agricultural techniques, for instance, are perpetuated. We intellectuals may prefer to think of ourselves as perpetually original. But the truth is that the originality is usually at the level of contents, while the forms of presentation are transmitted from generation to generation unreflectively and with only minor modifications. We clear new fields, but we till them as we always did. It is a simple historical observation that intellectual practices are enduring. Perhaps the most enduring of them all has been the Greek mathematical practice. Arguably – while modified by many evolutions – this practice can be said to dominate even present-day science.[3]

THE MODULARITY OF MIND

It is still necessary to specify what sort of practices I look at. The simple answer is that I look at those practices which may help to explain the success of science. In other words, I look at practices which may have an influence on the cognitive possibilities of science. To

[2] While such an approach is relatively uncommon in the literature, I am not the first to take it; see, for instance, Gooding (1990), on Faraday's experimental practices.

[3] There is a question concerning the relation between mathematics and other types of science. I do not think they are fundamentally distinct. The question most often raised in the literature, concerning the applicability of Kuhn to mathematics, is whether or not there are 'mathematical scientific revolutions' (in the sense of deep metaphysical shifts. See e.g. Gillies (1992)). But what I apply to mathematics is not the concept of scientific revolution, but that of normal science, and in this context the distinction between mathematics and other types of science seems much less obvious.

clarify what these may be, a detour is necessary, and I start, again from a well-known study, Fodor's *The Modularity of Mind* (1983).

Fodor distinguishes two types of cognitive processes: 'input/output mechanisms' (especially language and vision), on the one hand, and 'central processes' (for which a key example is the fixation of belief – the process leading to a person's believing in the truth of a statement) on the other. He then argues that some functions in the mind are 'modules'. By 'modules' are meant task-specific capacities (according to this view syntax, for instance, is a module; that is, we have a faculty which does syntactic computations and nothing else). Modules are automatic: to continue the same example, we do syntactic computations without thinking, without even wishing to do so. Syntactic parsing of sentences is forced upon us. And modules are isolated (when we do such computations in this modular way, we do not bring to bear any other knowledge). Modules thus function very much as if they were computer programs designed for doing a specified job. The assumption is that modules are innate – they are part of our biological make-up. And, so Fodor argues, modules are coextensive with input/output mechanisms: whatever is an input/output mechanism is a module, while nothing else is a module. The only things which are modular are processes such as vision and language, and nothing else in our mind is modular. Most importantly, central processes such as the fixation of belief are not modular. They are not task-specific (there is nothing in our brain whose function is just to reach beliefs), they are not automatic (we do not reach beliefs without conscious thoughts and volitions), and, especially, they are not isolated (there are a great many diverse processes related to any fixation of belief). Since central processes appeal to a wide range of capacities, without any apparent rules, it is much more difficult to study central processes.

Most importantly for the cognitive scientist, this difference between modules and central processes entails that modules will be the natural subject matter of cognitive science. By being relatively simple (especially in the sense of being isolated from each other), modules can be described in detail, modelled, experimented on, meaningfully analysed in universal, cross-cultural terms. Central processes, on the other hand, interact with each other in complicated, unpredictable ways, and are thus unanalysable. Hence Fodor's famous 'First Law of the Nonexistence of Cognitive Science': 'The more global . . . a cognitive process is, the less anybody understands it.'[4]

[4] Fodor (1983) 107.

I am not a cognitive scientist (and this study is not an 'application' of some cognitive theory). I do not profess to pass any judgement on Fodor's thesis. But the facts of the development of cognitive science are clear. It has made most progress with Fodorean modules, especially with language. It has been able to say less on questions concerning Fodorean central processes. Clearly, it is very difficult to develop a cognitive science of central processes. But this of course does not mean that central processes are beyond study. It simply means that, instead of a cognitive science of such aspects of the mind, we should have a cognitive *history*. 'The Existence of Cognitive History' is the direct corollary to Fodor's first law. Fodor shows why we can never have a neat universal model of such functions as the fixation of belief. This is registered with a pessimistic note, as if the end of universality is the end of study. But for the historian, study starts where universality ends.

It is clear why cognitive history is possible. While there are no general, universal rules concerning, for example, reasoning, such rules do exist *historically*, in specific contexts. Reasoning, in general, can be done in an open way, appealing to whatever tools suggest themselves – linguistic, visual, for example – using those tools in any order, moving freely from one to the other. In Greek mathematics, however, reasoning is done in a very specific way. There is a method in its use of cognitive resources. And it must be so – had it not been selective, simplified, intentionally blind to some possibilities, it would have been unmanageable. Through the evolution of specific cognitive methods, science has been made possible. Specific cognitive methods are specific ways of 'doing the cognitive thing' – of using, for instance, visual information or language. To illustrate this: in this book, I will argue that the two main tools for the shaping of deduction were the diagram, on the one hand, and the mathematical language on the other hand. Diagrams – in the specific way they are used in Greek mathematics – are the Greek mathematical way of tapping human visual cognitive resources. Greek mathematical language is a way of tapping human linguistic cognitive resources. These tools are then combined in specific ways. The tools, and their modes of combination, are the cognitive method.

But note that there is nothing universal about the precise shape of such cognitive methods. They are not neural; they are a historical construct. They change slowly, and over relatively long periods they may seem to be constant. But they are still not a biological constant. On the one hand, therefore, central processes can be studied (and this is because they are, in practice, in given periods and places, performed methodically, i.e. not completely unlike modules). On the other hand, they cannot be studied by cognitive science, i.e. through experimental methods and universalist assumptions. They can only be studied as historical phenomena, valid for their period and place. One needs studies in *cognitive history*, and I offer here one such study.[5]

I have promised I would locate this book with the aid of two landmarks, one starting from Kuhn, the other starting from Fodor. These two landmarks can be visualised as occupying two positions in a table (see below), where cognitive history can be located as well.

		Sources of knowledge	
		Cultural	**Biological**
Status of knowledge	**Propositional knowledge**	Kuhnian history of science	
	Practices of knowledge	Cognitive history	Fodorean cognitive science

Cognitive history lies at the intersection of history of science and the cognitive sciences. Like the history of science, it studies a cultural artefact. Like the cognitive sciences, it approaches knowledge not through its specific propositional contents but through its forms and practices.

An intersection is an interesting but dangerous place to be in. I fear cognitive scientists may see this study as too 'impressionistic' while historians may see it as over-theoretical and too eager to generalise. Perhaps both are right; I beg both to remember I am trying to do what is neither cognitive science nor the history of ideas. Whether I have succeeded, or whether this is worth trying, I leave for the reader to judge.

5 It remains to argue that the subject of my study is a central process and not a module. Whether 'deduction' as such is a module or not is a contested question. Rips (1994), for instance, thinks it is a module; Johnson-Laird (1983) disagrees. I cannot discuss here the detail of the debate (though I will say that much of my study may be seen as contributing to Johnson-Laird's approach), but in fact I need not take any stance in this debate. What I study is not 'deduction' as such; what I study is a specific form, namely the way in which Greek mathematicians argued for their results. It will be seen that the mechanisms involved are very complex, and very different from anything offered by those who argue that deduction is a module. If indeed there is some module corresponding to deduction, then it is no more than a first-level stepping stone *used* in mathematical deduction (in much the same way as the modules of vision are necessary for the perception of mathematical diagrams, but yet we will not try to reduce mathematical cognition into the modules of vision).

PLAN OF THE BOOK

The first four chapters of the study describe the tools of the Greek mathematical method. The first two chapters deal with the use of the diagram, and chapters 3 and 4 deal with the mathematical language.

How is deduction shaped from these tools? I do not try to define 'deduction' in this study (and I doubt how useful such a definition would be). I concentrate instead on two relatively simpler questions: first, what makes the arguments seem *necessary*? (That is, I am looking for the origins of the compelling power of arguments.) Second, what makes the arguments seem *general*? (That is, I am looking for the origins of the conviction that a particular argument proves the general claim.) These questions are dealt with in chapters 5 and 6, respectively. In these chapters I show how the elements of the style combine in large-scale units, and how this mode of combination explains the necessity and generality of the results.

The final chapter discusses the possible origins of this cognitive mode: what made the Greek mathematicians proceed in the way they did? I try to explain the practices of Greek mathematics through the cultural context of mathematics in antiquity, and, in this way, to put deduction in a historical context.

A specimen of Greek mathematics

Readers with no acquaintance with Greek mathematics may wish to see a sample of it before reading a description of its style. Others may wish to refresh their memory. I therefore put here a literal translation of Euclid's *Elements* II.5, with a reconstruction of its diagram.[1]

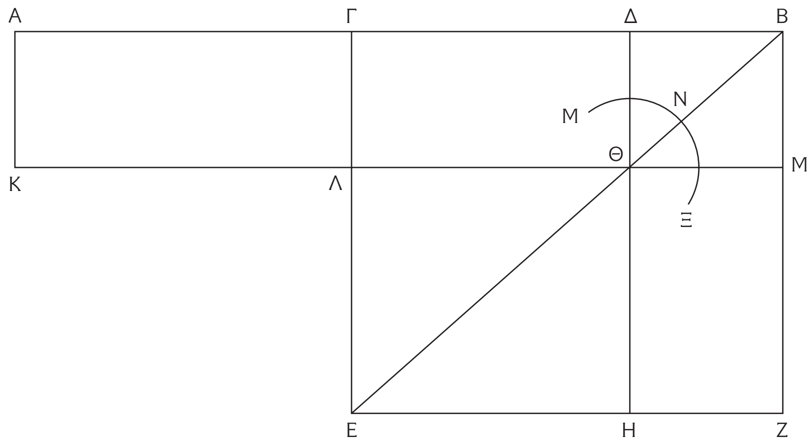
In this translation, I intervene in the text in several ways, including the following:

* I add the established titles of the six parts of the proposition. These six parts do not always occur in the same simple way as here, but they are very typical of Euclid's geometrical theorems. They will be especially important in chapter 6.
* I mark the sequence of assertions in both construction (with roman letters) and proof (with numerals). This is meant mainly as an aid for the reader. The sequence of assertions in the proof will interest us in chapter 5.
* Text in angle-brackets is my addition. The original Greek is extremely elliptic – a fact which will interest us especially in chapter 4.

Note also the following:

* Letters are used in diagram and text to represent the objects of the proposition in the middle four parts. These letters will interest us greatly in chapters 1–2.
* Relatively few words are used. There is a limited 'lexicon': this is the subject of chapter 3.
* These few words are usually used within the same phrases, which vary little. These are 'formulae', the subject of chapter 4.

[1] Note also that I offer a very brief description of the *dramatis personae* – the main Greek mathematicians referred to in this book – before the bibliography (pp. 316–22).



Euclid's Elements 11.5.

[*protasis* (enunciation)]

If a straight line is cut into equal and unequal <segments>, the rectangle contained by the unequal segments of the whole, with the square on the <line> between the cuts, is equal to the square on the half.

[*ekthesis* (setting out)]

For let some line, <namely> the <line> AB, be cut into equal <segments> at the <point> Γ, and into unequal <segments> at the <point> Δ;

[*diorismos* (definition of goal)]

I say that the rectangle contained by the <lines> AΔ, ΔB, with the square on the <line> ΓΔ, is equal to the square on the <line> ΓB.

[*kataskeuē* (construction)]

(a) For, on the <line> ΓB, let a square be set up, <namely> the <square> ΓEZB,
(b) and let the <line> BE be joined,
(c) and, through the <point> Δ, let the <line> ΔH be drawn parallel to either of the <lines> ΓE, BZ,
(d) and, through the <point> Θ, again let the <line> KM be drawn parallel to either of the <lines> AB, EZ,
(e) and again, through the <point> A, let the <line> AK be drawn parallel to either of the <lines> ΓΛ, BM.

[*apodeixis* (proof)]

(1) And since the complement ΓΘ is equal to the complement ΘZ;
(2) let the <square> ΔM be added <as> common;
(3) therefore the whole ΓM is equal to the whole ΔZ.
(4) But the <area> ΓM is equal to the <area> AΛ,
(5) since the <line> AΓ, too, is equal to the <line> ΓB;