The Reasonable Effectiveness of Mathematics in the Natural Sciences

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Abstract

One of the most unsettling problems in the history of philosophy examines how mathematics can be used to adequately represent the world. An influential thesis, stated by Eugene Wigner in his paper entitled "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," claims that "the miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve." Contrary to this view, this thesis delineates and implements a strategy to show that the applicability of mathematics is very reasonable indeed.

I distinguish three forms of the problem of the applicability of mathematics, and focus on one I call the problem of uncanny accuracy: Given that the construction and manipulation of mathematical representations is pervaded by uncertainty, error, approximation, and idealization, how can their apparently uncanny accuracy be explained? I argue that this question has found no satisfactory answer because our rational reconstruction of scientific practice has not involved tools rich enough to capture the logic of mathematical modelling.

Thus, I characterize a general schema of mathematical analysis of real systems, focusing on the selection of modelling assumptions, on the construction of model equations, and on the extraction of information, in order to address contextually determinate questions on some behaviour of interest. A concept of selective accuracy is developed to explain the way in which qualitative and quantitative solutions should be utilized to understand systems. The qualitative methods rely on asymptotic methods and on sensitivity analysis, whereas the quantitative methods are best understood using backward error analysis. The basic underpinning of this perspective is readily understandable across scientific fields, and it thereby provides a view of mathematical tractability readily interpretable in the broader context of mathematical modelling. In addition, this perspective is used to discuss the nature of theories, the role of scaling, and the epistemological and semantic aspects of experimentation. In conclusion, we argue for a method of local and global conceptual analysis that goes beyond the reach of the tools standardly used to capture the logic of science; on their basis, the applicability of mathematics finds itself demystified.

Keywords: Applied Mathematics, Asymptotics, Backward Error Analysis, Error, Exact & Numerical Solutions, Logic of Mathematical Modelling, Mathematical Tractability, Philosophy of Mathematics, Philosophy of Science, Qualitative Behaviour, Rational Reconstruction, Scientific Theories, Selective Accuracy, Uncertainty, Unreasonable Effectiveness of Mathematics.

[T]here is no philosophy that is not founded upon knowledge of the phenomena, but to get any profit from this knowledge it is absolutely necessary to be a mathematician.

Daniel Bernoulli (1700-1782)

The goal we must always keep in sight is to strive to reach a secure judgment regarding the foundations of a science [...]. But to penetrate the sciences at all, study of special problems is certainly indispensable.

Karl Weierstrass (1815-1897)

There's no sense in being precise when you don't even know what you're talking about.

John von Neumann (1903-1957)

Although this may seem a paradox, all exact science is dominated by the idea of approximation.

Bertrand Russell (1872-1970)

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Chapter 1

Introduction: The Unreasonable Effectiveness of Mathematics as a Philosophical Problem

Inquiries into the nature of mathematics as a science of its own and into its role in empirical science have a venerable tradition. Many of the ideas underlying the philosophical systems of Plato, Aristotle, Descartes, Leibniz, Kant, Mill, Frege, Husserl, Russell, Carnap, *et al.*, have found their origin in disquisitions on this topic. The problem that is perhaps the most unsettling examines how mathematics can be used to adequately represent the world, given that mathematics displays a kind of exactness and necessity that appears to be in sharp contrast with the contingent character of worldly facts.

Some authors maintain that this problem—which I shall name the problem of the applicability of mathematics—is condemned to remain intrinsically mysterious. For instance, Wigner (1960) famously claimed that the "miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve." Be that as it may, Hamming (1980) outlined a number of strategies that could potentially demystify some aspects of the appropriateness of mathematics stressed by Wigner. Many other philosophers and mathematicians have weighed in on the debate, including Batterman (2007, 2010*a*), Bell (1986), Colyvan (2001), French (2000), Grattan-Guinness (2008), Pin-

cock (2009), Steiner (1989, 2002), and Wilson (2000). The philosophers and mathematical scientists who have written on the problem of the applicability of mathematics have emphasized many different aspects relevant to their own fields of expertise. Since it would lead us in too many orthogonal directions, it will be impossible to begin our investigation with a systematic review of the literature. However, it is possible to begin with a conceptual articulation of the problem. Accordingly, in this first chapter, I will articulate the problem of the applicability of mathematics (1) by relating it to other philosophical questions about mathematics (in section 1.1), (2) by describing the one instance of the problem I intend to address (in section 1.2), and (3) by explaining how the quest for a solution to that problem relates to fundamental questions in epistemology and philosophy of science (in section 1.3).

1.1 Three Central Philosophical Questions on Mathematics

One of the preeminent problems in the philosophical tradition concerns the nature of genuine knowledge, as opposed to mere opinion or belief: What constitutes a proper characterization of knowledge and what, if anything, ought to be considered genuine knowledge? Hence, epistemology endeavors to specify which propositions among the ones we believe can commendably be deemed knowledge. In the first known writing dedicated to this question, the *Theaetetus* (ca. 369 BC), Plato suggests that the problem be tackled by supplying conditions that beliefs and opinions have to satisfy in order to count as genuine claims to knowledge, such as being based on perception, being true, having a justification, *etc.* The suggestion that beliefs need justification has been found to be compelling since cases involving epistemic serendipity have to be excluded; yet, precisely characterizing what an adequate justification for a belief is has proved to be elusive. Nevertheless, our inability to exactly pinpoint a correct and general account of knowledge should not deter us; one can fruitfully adopt the fallback strategy of conceding that some disciplines provide

genuine knowledge, and attempt to progressively improve our understanding of what a correct justification is by examining those disciplines. Thus, following this strategy, the notion of justification that plays an essential role in our concept of knowledge is to be abstracted from particular instances of genuine knowledge.[1]

In this respect, mathematical disciplines have played a role quite distinct from the others. In the *Republic* (ca. 380 BC : book VI), Plato suggested that mathematics is the highest form of knowledge and—each for their own reasons—most natural philosophers in the tradition have shared this assessment. More than anything else, it has been claimed that if anything is to be considered knowledge, mathematics is. In this way, mathematical knowledge is typically regarded as an archetypal or paradigmatic case of genuine knowledge.

In order to understand the significance of mathematics as an archetypal object of study for epistemology, something has to be said about what mathematics is. This, however, has proved to be an exacting challenge. As von Neumann (1947) remarked, when addressing this very question, a "discussion of the nature of any intellectual effort is difficult *per se*—at any rate, more difficult than the mere exercise of that particular effort." As a result, the question has proved to be very controversial; indeed, it is not much of an exaggeration to say that it has received as many different answers as there are philosophers of mathematics. This being said, I will briefly sketch the typical approach to this question in order to better situate and differentiate the problem addressed by this dissertation.

For mathematics as for other kinds of knowledge, the first natural questions to ask are what this knowledge is about and how this knowledge is acquired. If we consider mathematical statements such as

S1. $3\frac{10}{71} < \pi < 3\frac{1}{7}$ (Archemedean approximation of π)

S2. $\int_a^b f(x)dx = F(b) - F(a)$ (Fundamental Theorem of Calculus)

[1]There is a delicate issue to address regarding this strategy. It does appear to either fall short of our original objective of providing a general normative concept of knowledge, or to commit the so-called is-ought fallacy. This concern will be addressed in subsection 1.3.1.

S3. $e^{i\pi} + 1 = 0$, (Euler's Identity)

it is important to determine what makes these statements genuine claims to knowledge and how we can ascertain the genuine character of these knowledge claims. Accordingly, two questions have traditionally occupied the centre of the stage:

Q1. What are mathematical objects?

Q2. How do we (or can we) obtain knowledge of those objects?

For instance, one might maintain along a classical empiricist line that mathematics is about empirical objects and that mathematical knowledge is acquired by experience only, just like any other kind of knowledge. This answer has been predominantly rejected, for it seems that the two questions above demand an answer which will differ from empirical knowledge claims such as

S4. All Cretans are liars.

S5. Hesperus is Phosphorus.

S6. Venus is the only planet in our solar system that rotates counter-clockwise.

To begin with, mathematical statements such as S1–S3 seem to be true independently of the physical world. Moreover, it seems that mathematical knowledge can be obtained *a priori*. As Strawson explains,

> [t]hat means that you can see that it is true just lying on your couch. You don't have to get up off your couch and go outside and examine the way things are in the physical world. (reported in Sommers, 2003)

Furthermore, mathematical objects transcend experience, since objects such as $10^{10^{10}}$ and ∞ are not, it seems, accessible by direct experience. The scope of experience is thus too limited. Finally, this proposal fails to capture the necessity and certainty that characterizes mathematical truths. As Einstein (1923) elegantly puts it,

> [t]he laws of mathematics, as far as they refer to reality, are not certain, and as far as they are certain, do not refer to reality.

Philosophers have often made claims in the same spirit:

> [t]he exactness of mathematics is an abstract logical exactness which is lost as soon as mathematical reasoning is applied to the actual world. (Russell, 1968 : 110)
>
> [. . .] the apparent contrast between the indefinite flux of sense-impressions and the precise and timeless truths of mathematics has been among the earliest perplexities and problems not of the philosophy of mathematics only, but of philosophy in general. (Körner, 1986 : 9)

As a result, mathematical knowledge seems to have a different logical status, to demand a different type of justification, and to have different semantic grounds. In response to these exigencies, many intricate solutions to the first two questions have been proposed; some influential proposals are listed in table 1.1, together with the main difficulties they face. More recent studies in philosophy of mathematics have developed at a more abstract level—by focussing on the notions of sets, proof systems, structures, and categories—in order to take account of the transformation of mathematics that began in the middle of the nineteenth century. Nonetheless, it has not fundamentally altered the questions on which philosophy of mathematics focuses.[2]

An important third question is often found entangled with these two. We *apply* mathematics to many real-world problems, both in science and in everyday life. In everyday life, our use of mathematics assists us in counting, accounting, planning, *etc.*, by providing us with efficient conceptual tools on the basis of which we can make sound decisions. Moreover, as we will amply discuss in this dissertation, we also use mathematics in the sciences with the purpose of knowing how the world works. This leads us to a third question concerning the epistemological import of mathematics:

Q3. What grounds are there for the applicability of mathematics to the physical world, whereby applications generate new empirical knowledge?

[2]This is not to say, however, that it has not opened the way for new approaches to deal with those questions.

Position	Answer to Q1	Answer to Q2	Main difficulty
Naive empiricism	empirical objects	experience	no necessary truths
Platonism	otherworldly universals	impossible (recollection)	assumes transmigration of souls
Ante rem realism	otherworldly universals	special intuition	problem of epistemic access
Nominalism (or *post rem* realism)	abstracted concepts	abstraction	knowledge-dependent & limited scope
In re realism (or "Aristotelian" realism)	*in re* universals	abstraction	limited scope & not parsimonious
Fictionalism	no object	stipulation	not all conventions are mathematically equal

Table 1.1: Some influential philosophical answers to questions Q1 and Q2.

This question introduces the *problem of the applicability of mathematics* on which this dissertation will focus. Insofar as it demands us to construct a bridge between mathematical statements and statements about the physical world, this question introduces a whole new set of difficulties that bring the philosophy of mathematics to "a treacherous frontier, where theorizing about the nature of thought, language, and the world must come together" (Avigad, 2007).

It is important to point out that, even if those three questions are essentially different, they cannot be treated in a completely independent manner. In some cases, certain types of answer to questions Q1 and Q2 make it relatively straightforward to satisfactorily account for aspects of the applicability of mathematics—*e.g.*, empiricism, nominalism, and *in re* realism—whereas other types of answers make it more difficult—*e.g.*, Platonism and fictionalism. The converse relation also obtains: some satisfactory accounts of the applicability of mathematics make it difficult to provide satisfactory answers to the first two questions—*e.g.*, naive empiricism. In fact, it is precisely this tension that, in my opinion, has proved to be the most vexing problem in traditional philosophy of mathematics.

In what order should these interrelated problems be treated? I think that there is no single right answer to that question, but that the choice of order

has nonetheless some importance. Philosophers have most often started with a certain conception of pure mathematics (answering Q1 and Q2) and then attempted to extend it to applied mathematics. Now, if one suspects that the conceptions of pure mathematics in question might be at least partly responsible for the conceptual difficulties inherent in the problem of applicability, then one must reverse the order. Many works in the recent literature have favored such a reversal. For instance, Wilson (2006) has argued that one must study the efficient descriptive strategies developed by applied mathematicians to understand how concepts in general work, and Maddy (2008) has argued that one should not think of the philosophy of pure mathematics independently of the philosophy of applied mathematics. The general idea is finely epitomized by Emch & Liu (2002 : 39):

> A superficial commentator might be tempted to advance that some—or even much—of pure mathematics is established in a "context-free" manner; we argue, in contrast to this caricatural view, that much of the appeal and integrity of the sciences [...] depends on the fact that their practitioners have to take into account the necessity of traveling back and forth between different "contexts."

For this reason, my dissertation will prioritize Q3, without assuming specific answers to Q1 and Q2.

1.2 Three Problems about the Applicability of Mathematics

Because of the resilience of the tension between considerations bearing on pure and applied mathematics, the applicability of mathematics does bear an aura of mystery. Part of the mystery, in my opinion, stems from gathering many problems that require different types of solutions under the same umbrella. Thus, in this section, I distinguish three aspects of the problem of the applicability of mathematics, which I call the problem of mixed sentences (in subsection 1.2.1), the problem of unexpected applicability (in subsection 1.2.2), and the problem of uncanny accuracy (in subsection 1.2.3).

1.2.1 The Problem of Mixed Sentences

In fact, we do not only formulate purely mathematical statements similar to S1–S3 and essentially empirical statements similar to S4–S6 above. Question Q3 is forced upon us by the existence of a third kind of knowledge claim, such as

S7. $P(\text{it will rain tomorrow}) = 70\%$

S8. $\mathbf{F} = \dfrac{d\mathbf{p}}{dt} = m\mathbf{a} = -k\mathbf{x}$ (Newton's 2nd law and Hooke's law)

S9. $PV = nRT$ (Perfect gas law)

S10. $\rho\left(\dfrac{\partial \mathbf{v}}{\partial t} + \mathbf{v}\cdot\nabla\mathbf{v}\right) = -\nabla p + \nabla\mathbf{T} + \mathbf{f}$ (Navier-Stokes equation)

Sentences of this kind involve both empirical concepts and mathematical concepts; they are neither exclusively about mathematical nor about worldly objects. I will call such statements *mixed sentences*.[3] Typically, statements of applied mathematics and of the mathematical sciences—such as mathematical physics, biology, economics, *etc.*—are of this type.

The problem of mixed sentences, as I call it here, focuses on providing semantic conditions meant to account for the meaningfulness of mixed sentences. One normally provides semantic rules of interpretation for sentences (often *via* truth-conditions) by determining conditions for the evaluation of the basic relations of predication, implication, identity, and other similar basic relations. However, providing a semantics for mixed sentences introduces some additional difficulties, as the basic relations range over different domains—of physical and of mathematical objects. For instance, our normal understanding of sentences such as "humans are mortal" is that if an arbitrary object is a human, then it also is mortal. There is a relation of inclusion. However, if we consider sentences such as "physical space is non-Euclidean," we are not

[3]The term 'mixed sentences' is often used in contemporary philosophy of science in a sense similar to 'bridge laws' or 'coordinative definitions.' However, I use the term 'mixed sentences' in deference to the long Aristotelian tradition of reflection on the status of what they called 'mixed sciences.' Balaguer (2009) also uses this phrase in this sense, and Pincock (2004, 2011) and Psillos (2011) use 'mixed statement.'

in fact claiming that physical space is one of the mathematical objects called non-Euclidean spaces. As opposed to what such a literal interpretation would suggest, the assertion should not be interpreted as saying that some physical objects are mathematical objects, but rather that they share certain features.

Accordingly, the problem of mixed sentences seeks a set of interpretation rules that will correctly articulate how features can be shared by mathematical objects and worldly objects, despite their very different nature. Many approaches have been developed to address this problem. A natural approach is to base those rules on a theory of abstraction. On the one hand, by explaining how our cognitive faculties abstract mathematical concepts from our experience, we can articulate how structures can be shared by physical and mathematical objects in cognitive terms. On the other hand, many proposals have been made to characterize the rules of interpretation on the basis of a logical (rather than cognitive) notion of abstraction. In fact, modern philosophy of mathematics is tightly connected with the efforts of Dedekind, Cantor, and Frege to formulate how mathematical objects can in some way be considered abstract correlates of empirical objects. Dummett (1991 : p. 293) explains the role of this problem in Frege's philosophy:

> Frege's objective was to destroy the illusion that any miracle occurs [in applications]. The possibility of the applications was built into the theory from the outset; its foundations must be so constructed as to display the most general form of those applications, and then particular applications will not appear a miracle.

The approaches based on a logical notion of abstraction have been developed in many ways. The idea behind them is to elaborate on the notion of shared structure in terms of morphisms relating empirical and mathematical systems. Typically, those are taken to be homomorphism or isomorphism (see, *e.g.*, Appendix A for a Suppesian instance of this approach).

The problem of mixed sentences focuses on the semantical aspect of the applicability to the extent that it seeks to explain how it makes sense to use mathematics to describe the world. However, there are other aspects of the problem of applicability which are not exhausted by issues related to the interpretability of mixed sentences, as the next two subsections show. In

particular, discussions of the interpretability of mixed sentences don't say much about situations in which the problems with applicability are not about the conditions of meaningfulness of mixed sentences, but about cases in which we have a multitude of meaningful mathematical representations that we would like to assess and mutually compare with respect to their accuracy.

1.2.2 The Problem of Unexpected Applicability

A second aspect of the problem of applicability has been emphasized in many publications, perhaps most famously in Wigner's paper on "the unreasonable effectiveness of mathematics in the natural sciences" (Wigner, 1960). He argues that the "miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve." The claims made in this paper are very evocative, but they are nevertheless hard to interpret, for Wigner addresses a collection of entangled problems. I will in what follows isolate two of these problems; in this section, I begin with the problem of unexpected applicability, as I call it.

Wigner adopts a view of mathematical concepts and objects that many mathematicians share. According to this view, the concepts of mathematics must be separated into two classes. On the one hand, we have the concepts of elementary arithmetic (*e.g.*, natural, rational, and irrational numbers) and those of elementary geometry (*e.g.*, the ratio of the circumference of a circle to its diameter); these "describe entities which are directly suggested by the actual world" (p. 2). On the other hand, we have the more advanced concepts of mathematics (*e.g.*, complex numbers, algebras, linear operators, Borel sets) for which this is not the case. According to Wigner, these "were so devised that they are apt subjects on which the mathematician can demonstrate his ingenuity and sense of formal beauty" (p. 3). In slightly different terms, he says that the more advanced concepts "are defined with a view of permitting ingenious logical operations which appeal to our aesthetic sense [...]" (p. 3).[4]

[4]This view is shared, *e.g.*, by Hardy (1940 : §28): "There are then two mathematics. There is the real mathematics of the real mathematicians, and there is what I have called the 'trivial' mathematics, for want of a better word. The trivial mathematics may be

He elaborates on the case of the complex numbers since they "provide a particularly striking example for the foregoing" (p. 3). He provides the following explication:

> Certainly, nothing in our experience suggests the introduction of these quantities. Indeed, if a mathematician is asked to justify his interest in complex numbers, he will point, with some indignation, to the many beautiful theorems in the theory of equations, of power series and of analytic functions in general, which owe their origin to the introduction of complex numbers. (Wigner, 1960 : p. 3)

Against the background of the assumption that the more advanced concepts of mathematics are introduced for the sake of exhibiting formal beauty, he asks: why are those more advanced concepts so effective for the formulation of the laws of physics?

This question introduces new aspects of the problem of applicability, especially for mathematical objects constructed and studied with no intended applications. How can it be that concepts of this sort find unexpected applicability in physics, sometimes in fundamental places? The main instance of this phenomenon that Wigner discusses is the unexpected applications of matrix algebra in quantum mechanics. According to him, it is miraculous that there are regularities at all "despite the baffling complexity of the world," it is a miracle that we can discover them, and it is also a miracle that mathematical concepts introduced for aesthetic reasons turn out to unexpectedly apply (sometimes at a very fundamental level).

It is certainly not easy to provide an answer to this problem. One strategy for dealing with it would consist in denying the very view of advanced mathematics that it presupposes. Thus, we may ask: is it true that nothing in our experience suggests introducing complex numbers? A widespread story, according to which $i = \sqrt{-1}$ is introduced by mathematicians to algebraically close the real numbers (in which the polynomial equation $x^2 + 1 = 0$ has no solution), lends credibility to the aesthetic account favored by Wigner. However, as Needham (1997) explains, it is the solution of intuitively meaningful

justified [... intuitively], but there is no such defence for the real mathematics, which must be justified as arts if it can be justified at all."

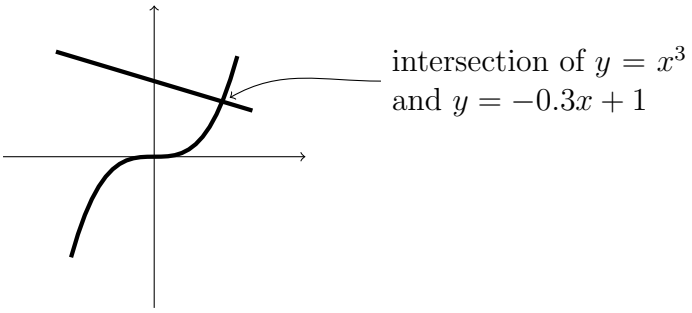


Figure 1.1: Intuitively meaningful cubic problem at the origin of imaginary numbers. The root is real, but its computation using Cardano's method requires imaginary numbers.

problems such as finding the roots of $x^3 = 3px + 2q$ that historically required introducing complex numbers (see figure 1.1).[5] This was required for the use of the computational method known at the time—the Cardano method. Thus, it is practically important *cubic* problems, and not the fancy of mathematicians craving to exert ingenuity, that historically justified introducing complex numbers.

It is possible, however, that there may remain parts of mathematics for which Wigner's assumption will still hold. However, the problem of unexpected applicability will be mostly solved if one could show that the main examples appealed to in the literature can be handled in a way similar to that of the complex numbers. However, I will not focus on this aspect of applicability either, but rather on another one that is complementary to both this one and to the problem of mixed sentences.

1.2.3 The Problem of Uncanny Accuracy

There is a third important aspect of the problem of applicability of mathematics that Wigner introduces in contradistinction to the problem of unexpected applicability. Here is how he introduced it:

> The observation which comes closest to an explanation for the mathematical concepts' cropping up in physics which I know is Einstein's statement that the

[5] See also Bell (1999).

> only physical theories which we are willing to accept are the beautiful ones. It stands to argue that the concepts of mathematics, which invite the exercise of so much wit, have the quality of beauty. However, Einstein's observation can at best explain properties of theories which we are willing to believe and has no reference to the intrinsic accuracy of the theory. (Wigner, 1960 : p. 7)

Thus, in addition to the considerations from the last section, there is a fundamental story to tell about the *intrinsic accuracy* of mathematical representations. As Wigner (1960 : p. 8) explains,

> [i]t is important to point out that the mathematical formulation of the physicist's often crude experience leads in an uncanny number of cases to an amazingly accurate description of a large class of phenomena. This shows that the mathematical language has more to commend it than being the only language which we can speak; it shows that it is, in a very real sense, the correct language.

The uncanny accuracy that Wigner describes extends to all aspects of mathematical modelling and theorizing. Mathematical representations are often based on crude experience, but they are also based on intrinsic limitations regarding what can be mathematically achieved. In this respect, computer simulations and numerical approximations introduce an additional dimension to the problem of uncanny accuracy. Hamming has a nice example of this:

> My first real experience in the use of mathematics to predict things in the real world was in connection with the design of atomic bombs during the Second World War. How was it that the numbers we so patiently computed on the primitive relay computers agreed so well with what happened on the first test shot at Almagordo? There were, and could be, no small-scale experiments to check the computations directly. [. . .] this was not an isolated phenomenon—constantly what we predict from the manipulation of mathematical symbols is realized in the real world. (Hamming, 1980 : p. 82)

Thus, we see that *mathematical representations of physical systems are constructed on the basis of uncertainty, measurement error, modelling error, analytical approximations, computational approximations, and other forms of guesses and ignorance that nonetheless often provide extremely accurate representations of systems.* On the basis of the commonsensical rule "garbage in, garbage out," this accuracy indeed appears to be uncanny. Thus, the problem of uncanny accuracy, as I will address it in this dissertation, can be formulated as follows:

Problems of Applicability	Examples of Solution Candidate
Mixed Sentences	Theory of cognitive abstraction, theory of logical abstraction, theory of shared structures in terms of morphisms
Unexpected Applicability	Einsteinian aesthetic discrimination, accounts of pre-established harmony, "tinted glasses" perspectivism, theory of embodied mind
Uncanny Accuracy	A concept of differential mathematical fitness

Table 1.2: Three problems of applicability and corresponding solution candidates.

> Given that the effective construction and manipulation of mathematical representation is pervaded by uncertainty, error, and approximation, how can their apparently uncanny accuracy be explained?

Chapter 2, chapter 3, and chapter 4 will describe the circumstances in which uncertainty, error, and approximation creep into our practice of mathematical modelling, and how they are handled.

To end this subsection, I would like to emphasize that this problem differs significantly from the other two problems of applicability (see table 1.2). The first two problems focus on the conditions of possibility for meaningfully using mathematics to gain knowledge about the real world.[6] This third problem is not asking how mathematical representations can meaningfully be built. Rather, it starts from the fact that applied mathematicians do, as a matter of fact, use mathematics to fruitfully gain knowledge of the world. However, it stresses the fact that—seemingly against all odds—the ingredients used to

[6] A perspective on mathematics that enhances its flexibility, thereby making it more resourceful, makes the expectation that we can satisfy those conditions of possibility more natural. In reference to his suggestion to replace absolute (or global) mathematics by local mathematics, Bell (1986 : 425) remarks that it "results, in my view, in a considerable gain in *flexibility of application* of mathematical ideas, and so offers the possibility of providing an explanation of their 'unreasonable effectiveness'. For now, instead of being obliged to force an intuitively given concept onto the Procrustean bed of absolute mathematics, with the attendant distortion of meaning, we are at liberty to *choose* the local mathematics naturally fitted to express and develop the concept. To the extent that the given concept embodies aspects of (our experience of) the objective world, so also will the associated local mathematics; the 'effectiveness' of the latter, i.e., its conformability with the objective world, thus loses its 'unreasonableness' and instead is shown to be a product of design."

cook up accurate representations are themselves lacking in accuracy in essential ways. Thus, the problem consists in explaining how to compare the virtues of different mathematical representations, and how to explain the success of those that have a comparative advantage. As I will explain later, I believe that a satisfactory solution to this problem consists in the specification of a concept of *differential mathematical fitness* that explains how the accuracy of our mathematical representations is not so uncanny after all. In the next section, I will explain in what respects this problem is not the exclusive turf of applied mathematicians, but also touches on deep philosophical problems.

1.3 Rational Reconstruction and the Epistemology of Science

Accordingly, this dissertation will furnish an account of why mathematics applies so successfully to the real world, despite apparently compelling *a priori* reasons that it should not. I now turn to a discussion of the philosophical methodology that appears to be proper for the resolution of this problem. Accordingly, the objective of this section is to delineate a philosophically sound methodology for this investigation, in a way that reveals how I intend my theses and arguments to be interpreted.

1.3.1 Philosophical Stakes in Rational Reconstruction

It is a fact that the practice of real scientists is pervaded with falsehoods, errors (intended and not intended), approximations, and uncertainty (including both known and unknown unknowns). Cases of epistemic serendipity, fortunate mistakes, aesthetic preferences, and personal idiosyncrasies of influential figures are also integral parts of real science. However, it does not follow that all those factors play an equally important role in epistemology. It is true that epistemology is descriptive insofar as it has "the task of giving a description of knowledge as it really is" (Reichenbach, 1938 : p. 1). However, the point of epistemology is to clarify what knowledge in general is. In a scientific context,

its point is to explain the reliability of scientific knowledge and to delimit its scope. Epistemology aims to provide grounds for *evaluating* what knowledge claims are in fact genuine knowledge; in particular, what claims, hypotheses, models, theories, methods should be considered scientifically warranted. Epistemology does not, as a result, take as its objects the actual thought processes of scientists, the actual words used by scientists, or even what scientists take their own activity to be. Rather it envisages a better scenario in which the claims, hypotheses, models, theories, and methods are accounted for not by fortunate mistakes, serendipity, *etc.*, but rather by a rationally compelling presentation they *ought* to have. Thus, to use the term introduced by Carnap (1928), the object of the epistemology of science is a *rational reconstruction* of science. The question, then, is:

> What aspects, if any, of scientific practice should bear on our normative, ideal image of science?

In particular, is the *de facto* omnipresence of idealization, approximation, and error a mere contingency that has little place in a rational reconstruction, or should it occupy a prominent place? Based on an examination of the practice of applied mathematicians, I will argue for the latter.

Nonetheless, my approach seeks to formulate a normative rather than a strictly descriptive discourse on science. That this is the objective of epistemology is, in my opinion, not controversial,[7] but there have been multiple debates regarding the relation between facts and norms in philosophy of science.[8] The essence of the controversy stems from the fear of committing the *is-ought fallacy* decried by Hume (1739 : Book III). As it happens to be the case, our paradigm for thinking about norms are the rules that prescribe action by determining what is to be considered legal or not, *e.g.*, 'one ought not to cross an intersection when the light is red.' In such cases, there is a very clear

[7]Some philosophers would disagree, especially if 'normative' is assimilated with 'prescriptive,' *i.e.*, with the enunciation of dictates to scientists. However, the normative aspect of epistemology also includes evaluative and comparative claims that are surely normative in character.

[8]This has been particularly important for philosophers of science who have tried to play up the role of history and sociology.

direction of fit: if someone crosses the intersection on a red light, it does not invalidate the law. Thus, particular actions are immaterial to whether the law stands or not. Since epistemology is also normative, it would seem to make it impossible to infer what should be considered science from what scientists actually do, as I suggest, for that would involve the same is-ought fallacy.

At the same time, most philosophers of science would grant that our rationally reconstructed ideal of science should stand in some relation of correspondence to actual science.[9] According to Brown (1980), this sort of situation leads many philosophers of science to a "schizophrenic attitude" toward the relation between facts and norms in philosophy of science. As he explains:

> On the one hand, they want their philosophical accounts of how science ought to be done to do justice to typical scientific practice; but on the other hand, they want to avoid any confusion of historical *facts* with philosophical *norms*. (Brown, 1980 : p. 236)

The objection leads us to a schizophrenic attitude to the extent that it assumes that there is no acceptable way to philosophically rationalize inferences from facts about typical scientific practice to aspects of our normative ideal of science, *i.e.*, that it always involves the is-ought fallacy. If this fear of committing the is-ought fallacy were sound, then it would represent a serious objection to the methodology I propose.[10]

As I have argued, the fact that there is omnipresence of approximation, error, and uncertainty in science motivates the study of the problem of the uncanny accuracy of mathematics without first answering the first two problems of applicability nor even the first two problems of philosophy of mathemat-

[9] This idea is clearly expressed by Reichenbach (1938 : p. 6): "In spite of its being performed on a fictive construction, we must retain the notion of the descriptive task of epistemology. The construction to be given is not arbitrary; it is bound to actual thinking by the postulate of correspondence."

[10] I take time to address this point because almost every time I have discussed the views I will articulate in the forthcoming chapters, I have met the argument that I was not clear on the descriptive and normative dimensions of my work, or that I was downright committing the is-ought fallacy by inferring standards of scientific knowledge from facts about what scientists do. Moreover, in discussions with philosophers of science, this argument had been presented against many of the authors who influenced my work. I thus judge it important to take some time to address the question.

ics. I consider it to soundly motivate an epistemological investigation, since the observations and analyses made would positively alter (at the very least complement) our ideal epistemological image of science. If, however, no such inference can be made, this motivation would be in jeopardy. To disperse such qualms, I will begin with a discussion of what I intend the normative aspect of my dissertation to be, in relation to the idea of rational reconstruction.

The dimension of the rational reconstruction process that generates an object of study suitable for a properly epistemological analysis of knowledge is often presented as an invective to distinguish the *context of discovery* from the *context of justification*. Here, discovery and justification should not be thought of as two temporally distinct processes—first, you discover something and then later you justify it—since the typical development of science involves alternating phases of discovery and justification that inform one another. As emphasized by Salmon (1970), there might be overlap between the two contexts. The distinction between the contexts is one between *processes* of discovery versus *methods* of justifications. The phrase "methods of justification" denotes what satisfactorily establishes knowledge claims, independently of the beliefs of the historical actors. Clearly, what is to be included in the context of justification is determined by what methods and tools are considered rational; different choices might result in different organizations of what belongs to what context.

The role of philosophy of science, from this point of view, is to determine by philosophical analysis what should count as satisfactorily establishing knowledge claims, that is, *what counts as a rational method of justification*. It is important to emphasize that which methods of justification are rationally admissible is not god-given; there is room for disagreement about justification, which may be rationally discussed by philosophers. It is in this respect that facts about actual science may have normative import:

> While the philosopher of science may be basically concerned with abstract logical relations, he can hardly afford to ignore the *actual methods* that scientists have found acceptable. If a philosopher expounds a theory of the logical structure of science according to which almost all of *modern physical science* is *methodologically unsound*, it would be far more reasonable to conclude that the

> philosophical reasoning had *gone astray* than to suppose that modern science is logically misconceived. (Salmon, 1970 : emphasis added)

This explains the normative relevance of actual scientific practice. Historical episodes confirm or disconfirm views on how rational reconstruction ought to be done because, if we use reconstructive tools that are misguided or insufficiently far-reaching, our assessment of historically important scientific events can turn out to be wrong:

> It would be a travesty to maintain, in any simple-minded way, that the historian of science is concerned only with matters of discovery, and not with matters of justification. In dealing with any significant case, say the replacement of an old theory by a new hypothesis, the historian will be deeply interested in such questions as whether, to what extent, and in what manner the old theory has been disconfirmed [...]. Such historical judgments—whether a particular historical development was or was not rationally justified on the basis of the evidence available at the time—depend crucially upon the historian's understanding of the logic of confirmation and disconfirmation. (Salmon, 1970 : 391)

To make his point, Salmon argues that the hypothetico-deductive model of confirmation cannot account for the rationality of important historical events. Under this view, confirmation is a process of deduction of observational consequences from hypotheses (together with initial conditions and auxiliary hypotheses) and of verification of whether the predictions (or retrodictions) in fact obtain. However, this approach does not provide the conceptual machinery necessary "for claiming that either of these laws is any better confirmed by the available evidence than any one of the infinitude of alternatives" (p. 393). Consequently, "it stands in dire need of supplementation" (p. 393). On that basis, we can make *normative* assessments of reconstructive toolboxes based on the discrepancy between the normative work that it is supposed to do and some facts about scientific practice.

The revision or supplementation of reconstructive tools is made on the basis of a diagnosis of facts about scientific practice to the effect that some tasks have to be performed, and yet cannot. In the case discussed by Salmon, he emphasizes the Bayesian observation that, in order to determine the degree to which a hypothesis is confirmed or disconfirmed, it is necessary to consider not only the observational consequences of this hypothesis, but also the conse-

quences of alternative hypotheses (*i.e.*, the likelihood of the evidence given the alternative hypotheses) and initial plausibility judgements of each of the alternatives (which the Bayesian approach captures with its priors). Moreover, he maintains that such factors are essentially absent from the hypothetico-deductive reconstruction schema.

The case I present here parallels Salmon's case.[11] Indeed, *to make correct judgements on the use of mathematics in science, it is necessary to have a correct understanding of the "logic" of model construction and model assessment.* However, as I will argue in the next subsection, the standard logical reconstructions fall short of this task. Thus, a revision or supplementation of the logical reconstructive tools is in order. If we were not carefully considering expanding the field of admissible reconstructive methods, almost all of mathematical sciences would be wrongly considered methodologically unsound. Thus, this dissertation is intended as a reflective study on the methods of mathematical reasoning actually used in applied mathematics, supplemented by normative conclusions regarding our ideal image of science.

1.3.2 Reconstruction Tools and the Problem of Uncanny Accuracy

The uncanny accuracy of mathematics has been claimed to be miraculous in the sense that it does not seem to admit of any rational explanation. However, as I have explained in the previous subsection, the line between what is rationally justifiable (or explicable) and what is not depends on the admissible resources that we grant for the reconstruction of scientific practice. One way to articulate the problem, then, is that the *a priori* reasons that make it hard to understand how mathematics applies so well (despite the fact that errors of all sorts are an intrinsic part of the way in which scientists represent the world and of the way in which they reason about concepts) result from an insufficiently

[11] There are other similar cases in the literature. For example, Batterman (2002*b*) argues that some physical phenomena involving universal behaviour necessitate a richer notion of explanation based on asymptotic reasoning. Similarly, Harper (1998) points out that a richer notion of empirical success underlies Newton's method.

rich way of rationally reconstructing scientific and mathematical knowledge. Accordingly, in order to demystify the allegedly miraculous character of the applicability of mathematics, insofar as the problem of uncanny accuracy is concerned, we need to suitably modify the catalogue of methods admissible for the rational reconstruction of the concepts of science and mathematics.

The first thing to emphasize is that the methodology advocated here is called *rational* reconstruction, and not *logical* reconstruction, for a reason. Logical methods are undoubtedly rational when properly utilized, but logical analysis does not exhaust the field of rational justifications. However, perhaps because of its origins in the works of Carnap, the term 'rational reconstruction' is often assumed to refer to a strict endeavor of logical analysis.[12] As a matter of fact, many if not most of the influential reconstructions of aspects of scientific practice rely on the idea that scientific representations (whether they are theories or models) are sets of first-order sentences and that the relation between sentences is essentially determined by the standard proof-systems. It is key to influential works on the structure of theories (*e.g.*, Carnap, 1936, 1966), the problem of confirmation (*e.g.*, Hempel, 1945; Popper, 1959), models of explanation (*e.g.*, Hempel & Oppenheim, 1948; van Fraassen, 1980; Kitcher, 1989; Strevens, 2004), inter-theory relations (*e.g.*, Oppenheim & Putnam, 1958; Nagel, 1961), and the notion of law (*e.g.*, Hempel, 1945; Mackie, 1973). But these simple logical schemata are unlikely to offer the appropriate resources to deal with the pervasiveness of error in the construction and assessment of mathematical representations.[13]

The first respect in which those resources are not suitable is that the conditions of applications of those schemata require the premises to be true, or approximately true.[14] However, there are important aspects of the scientific methodology that are swept under the rug when one *assumes* that premises are true or approximately true, for a very large part of the applied mathemat-

[12]For instance, Hoyningen-Huene (2006 : p. 126) explains that for the logical empiricists, "justification ultimately uses as exclusive means formal logics and basic/protocol sentences."

[13]I do not mean to suggest, by this comment, that they are unsuitable for the reconstruction of *any* aspect of scientific practice.

[14]It is required since *ex falso sequitur quodlibet*.

ical work is concerned precisely with finding criteria to determine when such an assumption can be made. If one tries to understand normatively the practice of mathematical modeling, then the assumption of (approximate) truth amounts to begging the question, since it is exactly what one tries to show. As a result, those reconstruction schemata cannot assist us in determining which errors are approximations. Those errors might have been required either for the construction of the model or for the solution of the model; but in either cases, we need to determine which errors are approximations. The distinction between error and approximation is usually overlooked in the philosophical literature and yet, it is one of the most important in applied mathematics.

A second objectionable aspect of logical schemata, given the problem with which we concern ourselves, is that these formal methods are *too definite*, in the sense that they do not tolerate well—at least in their basic form—radical changes in well-definedness or in the conditions of applications of concepts depending on the type or the domain of application. In fact, if one wants to understand the application of mathematics in a quite general way, it is important to consider contexts of assessment so varied in their nature and circumstances that a purely schematic account is unlikely to do the work. As Wilson (2006 : 26-27) explains:

> This observation—that we must continually devise unexpected stratagems to further our slow linguistic advance upon the world—represents a vital *lesson from applied mathematics* from which we can all benefit. Many working philosophers, however, greatly underestimate the inferential difficulties that frequently prevent us from reasoning readily from premises to practical conclusions. Through one swift swipe of unjustified optimism, the practical obstacles that force conceptual evaluation to turn complex in real circumstance become removed from view. If, as is the wont of many professional philosophers, one deals exclusively in schemata ("theory T," "premises P," "conclusion C," etc.), one can pass an entire career without ever experiencing the retarding obstinacies of real practicality.

Following Wilson along this line, I will explore a more containing model of *terms of conceptual evaluation* such as 'attribute,' 'property,' 'predicate,' 'theory,' 'possibility,' 'predication,' 'validity,' 'truth,' and 'soundness,' that largely accommodates the canons of proof and model theory, while at the same time says something substantial about the intermediate regions of concept applica-

tion where these seem unsuited.

Finally, if the schematic logical methods were taken to be the *exclusive* tools for our study, we would lack the required flexibility. As I will argue, it is not any mathematics that applies equally well, but rather some specific type of mathematics. As Truesdell (1966 : 86) says, "[t]he first aim of modern philosophy is to describe and study natural phenomena by the most fit mathematical concepts." Accordingly, we should try to identify not what is common to how all concepts work, but rather try to articulate the idea of *differential mathematical fitness* of concepts.[15]

I will argue that the sort of considerations involved are not based on the notion of satisfaction by means of which the standard logical semantics are defined. An important feature of the discussion will be that the strictly logical analysis of conceptual and theoretical content camouflages important difficulties, including drastic differences between the behaviour of different types of mathematical concepts. Accordingly, it is important to take a step back, and not start our analysis from the schemata in question. Rather, one should look at the recipes of model construction: what steps and what ingredients are required to construct a model, and how the various operations in such recipes are justified. They involve forms of perturbation analysis that go far beyond the basic model theory and the basic statistical/probabilistic methods of more standard reconstructions. It is precisely in this sense that, just as did Salmon, this dissertation will revise and complement our reconstructive toolbox, and thereby affect our normative image of science.

This being said, it is still possible that, in the end, the two reconstructionist approaches will end up sharing the same global conceptions of science. To what extent this will be the case is, in my opinion, a conceptual question of a mathematical type that has not yet been addressed. Most of philosophy of science has shown little interest in this sort of question. This question *should* be addressed. But until it is, we should see the value in both reconstructionist approaches.

[15]The word "differential" must be understood as in "differential effect of treatments," not in reference to differential geometry.

1.3.3 Local and Global Analysis of of Concepts

In addition to having philosophical implications for our normative ideal of science, the study of the problem of uncanny accuracy has consequences for the way in which philosophers envisage conceptual analysis. I have already criticized some mainstream methods that underlie some approaches to the reconstruction of scientific practice. In this subsection, I sketch the alternative mode of conceptual analysis that the next chapters will support. This perspective on concepts was developed by Wilson (2006). Wilson's methodology is to draw lessons from applied mathematics for the way we do philosophy.

The view of reconstruction discussed above relies on the *classical picture* of concepts, as Wilson (2006) calls it. It is a view of conceptual content that has been developed at the turn of the twentieth century by philosophers (most notably Frege and Russell) and scientists pressed to solve important methodological problems in their fields of inquiry. It was designed to accomplish some *practical work* by guiding our use of *terms of conceptual evaluation*.[16] Here are the three defining elements that characterize the classical theory:[17]

1. we can determinatively compare different agents with respect to the degree to which they share "conceptual contents";
2. that initially unclear "concepts" can be successively refined by "clear thinking" until their "contents" emerge as impeccably clear and well-defined;
3. that the truth-values of claims involving such clarified notions can be regarded as fixed irrespective of our limited abilities to check them.

These appear irreprochable if we associate meaning and conceptual content with truth conditions. The most problematic point with the classical picture is the way in which it depicts what *grasping a concept* is. Wilson's

> [...] unhappiness with the classical point of view lies in the fact that it paints

[16] A recurrent theme in Wilson (2006) is that, with hindsight, our task as philosophers is to assess the classical picture based on how well it accomplishes the work it promised to accomplish, and to suggest alternatives where it fails. This results in a very application-focused approach to problems of philosophical logic that permeate philosophy of science and philosophy of mathematics.

[17] See Wilson (2006 : 4).

> an implausible portrait of human intellectual capacity and practicality, not that its somewhat hazy descriptive vocabulary can't be adapted to any situation that comes down the pike. (Wilson, 2006 : xvii)

What is it to grasp a concept? Classically, a concept has been grasped when it does not suffer any indetermination, in the sense of the third thesis above. Moreover, by the second thesis, one can always reach such a state of well-definedness if one thinks sufficiently clearly:

> These classical proposals for making corrections in our intellectual course were quite optimistic in character, maintaining that any diligent thinker can, if she only sets her mind to the task, permanently avoid the strange conceptual snares into which scientific topics otherwise fall. (Wilson, 2006 : 7)

As a result, when one grasps the content of a concept, one is supposed to have a complete mastery of its truth conditions as well as of its tractability conditions.[18] However, as we will see, this is exaggerated: analyzing a concept requires more than meditating on the definition of a concept and its logical consequences. As it turns out, tracing the truth conditions and the implications of concepts is a computationally challenging task.

So, what direction should we take? Many twentieth century philosophers have sensed something wrong with the classical picture and proposed various diagnoses. Most of their proposals have consisted in revisionary pictures of what it is to "grasp a concept" or to "understand a trait."[19] The idea is that what is objective in the meaning of a concept (*i.e.*, its content) is not determined by its alignment to the world. We should rather seek the locus of objectivity elsewhere. In this respect, standard proposals are that the objective character of conceptual content should be sought in the linguistic practices of a community of competent speakers, in the causal connections between speech acts and events in the world, in a web of beliefs that runs across scientific disciplines, *etc.* I share with Wilson the view that, in an important way, these

[18] This convergence reminds us of Laplace's demon (Laplace, 1819), whose epistemic state is so perfect that it transparently reflects its ontological underpinnings.

[19] Wilson calls this strategy "amphibolism." Specifically, "amphibolism" denotes "the wide spectrum of philosophical opinion that rejects as misguided any attempt to disentangle the "objective" contents of predicates from their more subjectively informed directivities [. . .]" (Wilson, 2006 : 78).

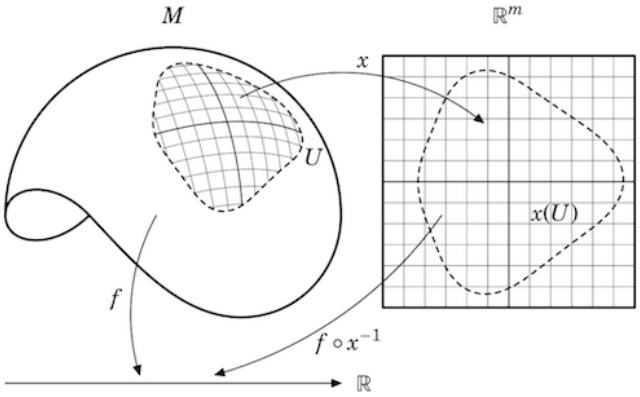


Figure 1.2: Manifold structure.

proposals throw the baby out with the bathwater. To find out whether we have committed this blunder, Wilson (2006 : 3) proposes this intuitive criterion:

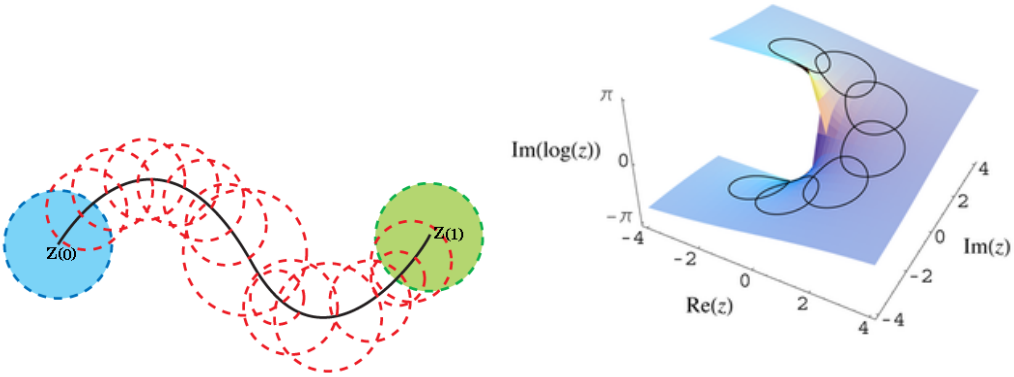
> We will have told the story of concepts wrongly if it doesn't turn out to be one where our usual forms of conceptual evaluation emerge as appropriate and well founded *most of the time*.

Concepts arise from practice and allow us to solve problems efficiently, in much the way that the classical picture describes. However, they sometimes fail to be as well-behaved as the classical picture would lead us to believe.

As a result, I will seek a different strategy. Employing two analogies from differential geometry, I will adopt the position that *concepts are locally classical*, but not necessarily globally.[20] The idea is that if we make conceptual analysis a primarily local endeavour, we do not have to change the locus of attributes; we rather need to understand how its various ranges of applications relate to the inner workings of the terms of conceptual evaluation. The classical picture only breaks down (and does so only some of the time) when we are trying to extend the use of these concepts to regions not planned for in the first place.

With this as a motivation, I will discuss terms of conceptual evaluation in analogy to the treatment of geometrical properties in a mathematical structure

[20]My first encounter with this strategy is a paper by Bell (1986), in a different context. Wilson also developed a version of this thesis, which is the one I am closer to in this section. More recently, this idea has played an important role in a work of Curiel (2011).



(a) Nice and smooth. (b) Not so nice and multivalued.

Figure 1.3: Analytic continuation.

called "manifold." See figure 1.2. If we start with a vast region of application M, we focus on the *local* behaviour of the concept over a part U. In a manifold, even if the global structure can take many forms, the local one is always Euclidean. In analogy, even if the global behaviour of a concept can take many refractory forms, the local behaviour, *i.e.*, the behaviour that has to do with well-understood applications, is classical. In a manifold structure, we try to connect these locally Euclidean neighbourhoods to do more heavy duty differential geometry by means of *atlases*. Similarly, as Wilson (2006) explains in his chapters 6 and 7, we connect our various local *patches* by constructing *theory facades*.

If we encounter no pitfalls, we will even be able to *extend* our understanding of particular concepts much beyond the patches corresponding to well-understood practical applications. Our reasoning about concepts in extended domains is developed in analogy to another important mathematical idea, namely, *analytic continuation*. The basic idea, when everything is nice and smooth, is that starting at a certain point $z(0)$ (see figure 1.3(a)), we are justified in assuming that a function (more specifically, its series expansion) works similarly in all neighbourhoods. As a result, we can reason that, if we go from a region of application to another, our understanding of the function will

remain just as good. In such cases, we can extend our understanding of the function to determine what we would find out at $z(1)$. However, the story is not always as even-textured (see figure 1.3(b)). To begin with, there might be *competing continuations* of our local understanding of a concept. The locality of our original domain of application can also hide a nonlinearity that makes extrapolation unreliable. For that matter, there might not be any coherent continuation to a given region. Such troubling situations relate to what Wilson (2006) describes under the colourful labels "multi-sheetedness," "cracked reasoning," "physics avoidance," *etc.*

From this point of view, there is a conceptual underdetermination with respect to "analytic" continuation. I will argue that, if this is problematic from the point of view of a convinced adherent to the classical picture, it is beneficial to our understanding of which mathematics applies well. These considerations emphasize that not all concepts are alike in their inner workings, *i.e.*, individual features of some particular kinds of concepts determine a non-universal way in which they are glued to the world. As a result, it is reasonable to expect that not all mathematical concepts will apply equally well. By characterizing and classifying such strange conceptual behaviours, we will then understand better in which way the world refuses to cooperate with our conceptual expectations. We will likewise have a notion of differential mathematical fitness providing reasons for which some mathematical concepts apply so well (while others do not). Importantly, this view respects the fact that successful concept application is something that requires caution and is not something that can be expected to be epistemologically transparent.

1.4 Summary of this Chapter and Outline of the Dissertation

In summary, this chapter has made the following points:

C1.1 There are three main interrelated philosophical questions about mathematics. Whereas the mainstream philosophy of mathematics literature

typically relegates questions on the applicability of mathematics to a secondary role, this dissertation explores the consequences of giving them priority.

C1.2 The problem of the applicability of mathematics comprises at least three different problems demanding essentially different answers. Whereas the mainstream philosophical literature has mostly ignored what I call the problem of uncanny accuracy, this dissertation focuses on it.

C1.3 The problem of uncanny accuracy demands as an answer a concept of differential mathematical fitness that explains the circumstances in which mathematics can be expected to apply well (or not). It is unlikely that this concept will be captured by the syntactic and semantic tools of logic standardly employed in philosophy of science.

C1.4 To demystify the applicability of mathematics, one needs to have the means to capture its rationality. Accordingly, it is imperative to supplement the philosophical toolbox used to rationally reconstruct scientific practice with methods and concepts apt to discuss accuracy (which are based on error theory, sensitivity analysis, and perturbation theory).

C1.5 Mainstream philosophical views on the reconstruction of scientific practice rely on the classical view of concepts, which relies on a naively optimistic view of what it is to grasp a concept. A view of concepts that distinguishes local and global conceptual analysis will better capture the challenges involved in the application of concepts.

In the next three chapters, this dissertation will provide a philosophical discussion of the ways in which mathematics applies to the world. The discussion will examine actual strategies and methods used by applied mathematicians and extract from them general patterns of reasoning. Chapter 2 examines the dominant features of 'the logic of mathematical modelling,' as I call it; it includes the recipes used to construct mathematical representations and the factors entering into their evaluation. Thus, chapter 2 delineates how error and uncertainty are handled in mathematical modelling. The first section of

chapter 3 pursues the same theme by focussing on the theories underlying physical modelling. The second section of chapter 3 discusses the experimental side of the concepts of error and uncertainty. Finally, chapter 4 clarifies the contribution of quantitative methods for the analysis and assessment of mathematical models. On the basis of these discussions, the dissertation concludes that to properly understand the logic of mathematical modelling we need the view of concepts essentially pioneered by Wilson (2006). By adapting this view, it thus become possible to philosophically systematize the patterns of successful applied mathematical reasoning examined in order to demystify the unreasonable effectiveness of mathematics.

Chapter 2

The Logic of Mathematical Modelling

This chapter examines the logic of model construction and model evaluation—or just logic of (mathematical) modelling for short.[1] In chapter 1, I have argued that understanding the operations involved in the construction of models and in the evaluation of their accuracy is the most important aspect of scientific methodology to properly reconstruct in order to demystify the unreasonable effectiveness of mathematics.

In section 2.1, I will make explicit what operations are involved in the construction of a mathematical model of a real system. In the remaining sections, I delineate patterns of analysis used to address the main difficulties related to the assessment of mathematical representations of real systems. Section 2.2 explains the way in which model equations are derived from modelling assumptions by means of model construction recipes. Section 2.3 explains the interdependence between the selection of modelling assumptions and the tractability of model equations. Those two sections make clear that the logic of modelling has an essential pragmatic component. Section 2.4 draws on this

[1] I do not use the word "logic" as it is used in modern works on mathematical logic, in which the proof and model theory of a logic is studied in formal languages. I use the word "logic" in its old-fashioned sense of "science or art of reasoning." This seems appropriate, since I will describe courses of thought encountered in the context of modelling, and I will discuss the conditions under which such courses of thought are valid, or sound, or reliable.

fact and identifies good modelling practices with effective strategies for information management. Finally, section 2.5 clarifies the senses and conditions under which a model can be considered to successfully represent a real system.

2.1 The Mathematical Study of Real Systems

The construction of a mathematical representation of a real system always involves some degree of idealization. Many definitions of 'idealization' exist in the literature, but here I simply use this term whenever a literally false statement (that is no tautologically so) is involved. Under such a definition, the omnipresence of idealization is incontrovertible, as Truesdell (1960 : 31) elegantly explains:

> Any mathematical theory of physics must idealize nature. That much of nature is left unrepresented in any one theory, is obvious; less so, that theory may err in adding extra features not dictated by experience. For example, the infinity of space is itself a *purely mathematical concept*, and all theories within this space must share in the geometrical idealization already implied.

This sort of idealization is characteristic of all types of representation, and is by no means limited to mathematical representation. According to the *Oxford Dictionary*, a representation (in the sense relevant here) is "the description or portrayal of someone or something in a particular way." A given thing could be represented in multiple ways depending on the choice of medium; such media could be, *e.g.*, a picture, a wooden scale model, a set of English sentences, or a set of mathematical equations. Any representation using a medium that differs in kind from the thing represented introduces a degree of idealization. This fact only reminds us that a good representation cannot merely duplicate the thing represented, but that it must rather encapsulate the relevant information.[2] Different types of representation complement each

[2]The idea that a representation that introduces no distortion whatsoever is better is sometimes part of philosophical arguments, at least implicitly. Borges (1975) illustrates convincingly in a short story that this view is untenable: "In that Empire, the Art of Cartography attained such Perfection that the map of a single Province occupied the entirety of a City, and the map of the Empire, the entirety of a Province. In time, those Unconscionable Maps no longer satisfied, and the Cartographers Guilds struck a Map of the Empire whose

other when different information is needed.

Given that our objective is to delineate the circumstances that make the use of mathematics so successful, in what follows we restrict our attention to mathematical representations and their effective use in the natural sciences. A natural starting point is to examine the way in which bodies are represented in mechanics to make them amenable to a mathematical study. Malvern (1976 : 1) depicts the situation as follows:

> The theory of mechanics, like all physical theories, deals, however, not with actual physical materials but with various idealized physical models of real materials, models capable of being represented by mathematical equations that can be solved to make predictions of the motions and deformations of the physical model material.

In classical mechanics, three different kinds of "idealized bodies" (or idealized "building blocks") are employed to represent physical systems (see, *e.g.*, Malvern, 1976; Wilson, 1998):

1. particles or mass-points;

2. rigid bodies;

3. deformable continuous bodies, or deformable bodies for short.[3]

Each of those types of idealized bodies gives rise to a specific approach to classical mechanics. In each case, there corresponds a branch of mathematical physics that studies the mathematical properties of systems of bodies of this type (respectively, mass-point mechanics, rigid body mechanics,[4] and continuum mechanics). However, as a matter of fact, there are no such things in reality as a mass-point particle or a perfectly rigid body; all real physical bodies occupy some space and are to some extent deformable. Moreover, based on

size was that of the Empire, and which coincided point for point with it. The following Generations, who were not so fond of the Study of Cartography as their Forebears had been, saw that that vast Map was Useless, and not without some Pitilessness was it, that they delivered it up to the Inclemencies of Sun and Winters. In the Deserts of the West, still today, there are Tattered Ruins of that Map, inhabited by Animals and Beggars; in all the Land there is no other Relic of the Disciplines of Geography."

[3]They are also called 'continuous media' or 'continua'.

[4]It is sometimes called rational or analytical mechanics.

knowledge acquired not from classical but rather from quantum mechanics, it would seem that bodies are not continuous, but rather composed of discrete entities. Nonetheless, those idealized bodies are the media used to construct mathematical representations of real physical systems in an astonishingly successful way.

The construction of mathematical representations involves many intertwined layers. Our first task will be to identify them and their mutual relations. To begin with, one may consider a hypothetical abstract system and attempt to understand what will happen to the bodies in this system. By 'abstract system,'[5] I mean a system specified in abstract terms such as idealized bodies, coordinates, initial or boundary instantaneous states, *etc.* A description of an abstract system is provided in a way that unambiguously determines a list of *modelling assumptions*; typically, this information is (or can be) summarized in a free-body diagram. Basically, the sort of mathematical analysis involved with such abstract problems is of the kind we find in introductory physics textbooks. For instance, we could consider an isolated system of two mass-point particles of mass m_1 and m_2 in states $s_1 = [\mathbf{x}_1, \dot{\mathbf{x}}_1]$ and $s_2 = [\mathbf{x}_2, \dot{\mathbf{x}}_2]$ at t_0 and acted upon only by the gravitational force. This being given, the mathematical task to perform would consist in *deriving* a differential equation (or a difference equation, or an equilibrium equation, *etc.*) expressing the states of m_1 and m_2 as a function of time (for any t or for some interval $t_a \leq t \leq t_b$) from the modelling assumptions. In other words, we *construct* a mathematical representation—which consists in a set of *model equations*[6]—from the modelling assumptions. From this mathematical representation, the task is then to *extract* information that characterizes the behaviour of the system. This step often involves significant computational difficulties that philosophers rarely acknowledge; I will return to this later.[7] In summary, in the case of abstract

[5]Perhaps a better word would be 'pretend' system, since we pretend that such a thing exists, without reference to what actually is.

[6]I will use the term 'model equation' to refer indiscriminately to differential equations of motion, difference equations, equilibrium equations, and the like.

[7]For now, I will just point out the fact that whether we need to extract *the* exact solution (if there is a unique solution), or just *a* solution (or a class of them) that are not necessarily exact is not pre-settled. All we need is information on the behaviour of the system.

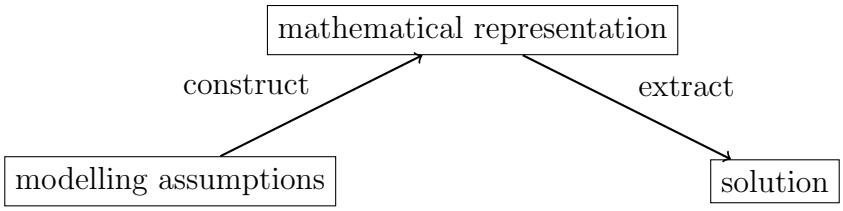


Figure 2.1: Schema of mathematical analysis of abstract systems.

systems, the schema of mathematical analysis is as in figure 2.1.

The construction of a mathematical representation of an *abstract system* is much less problematic than for real systems, since we are given everything that is mathematically relevant to the problem, *i.e.*, everything that should be taken into account in the mathematical representation. Such problems can be solved without ever asking if there are systems of this kind that actually exist. In the previous example, the fact that no system of two completely isolated point masses acted upon only by gravitation exist, is immaterial to the analysis. Even more importantly, it is also independent of whether an actual system that we wish to study mathematically is faithfully represented by this abstract model. In other words, it is an exercise in pure mathematics.

On the other hand, one may consider *real* systems, *i.e.*, systems as we actually encounter them in the universe we live in. In contrast with abstract models, a real model is not populated with mass-points, rigid bodies, or continuous media, but rather with things like tennis balls, blocks of concrete, $2'' \times 4''$ wood studs, steel I-beams, planets, galaxies, impure water, *etc.* Part of the mathematical analysis of such real systems involves the derivation of an equation of motion that dictates the temporal behaviour, exactly as for abstract systems. However, in this type of situation, as opposed to the first one discussed, we are *not* given everything that is mathematically relevant to the problem. In other words, we do not start from a given list of modelling assumptions, but from a raw, non-mathematically described real system. Prior to the derivation of model equations, one has to *select* modelling assumptions. The selection of modelling assumptions is a crucial step which is often plagued

with error and uncertainty.[8] As a result, the construction phase of the analysis is significantly more difficult for real systems.

It is also important to emphasize that the process of mathematical modelling of real systems typically begins with specific questions that determine what aspects of the system is the *behavior of interest*. Those questions are often very practical questions that impose a pragmatic (*i.e.*, context-dependent) dimension to the logic of modelling.[9] Here are examples of such questions:

- what is the efficiency of this machine?
- would this apparatus break under a typical variable load L?
- is this component better built with material A or B?
- would a certain solution containing likely impurities remain stable under a certain increase of temperature?

Because of that, in contexts of modelling, the solution to model equations is often only an intermediary step toward answering specific questions about the behaviour of interest. In this case, the schema of mathematical analysis is as in figure 2.2. Notice the differences with figure 2.1.

The task of mathematically modelling real systems is to derive a mathematical representation of the system that will allow us to correctly capture certain physical properties of the system. From this point of view, a good model does not have to correctly capture all aspects of the system, but only those relevant to the questions that we are interested with when we considered the system in the first place.

Accordingly, in order to properly characterize the logic of modelling, it will be convenient to adopt the mindset of an engineer trying to solve a problem, or to design a mechanism, or to build a machine or gadget of sort, without

[8] It would be premature at this point to outline the intricacies involved in this selection stage; this will be revisited throughout the chapter.

[9] This fact is often overlooked in philosophical discussions on the role of mathematics. As I will explain later, it is for the most part the case that solutions of model equations can only be assessed with respect to levels of tolerance dictated by the practical concerns that motivated the investigation.

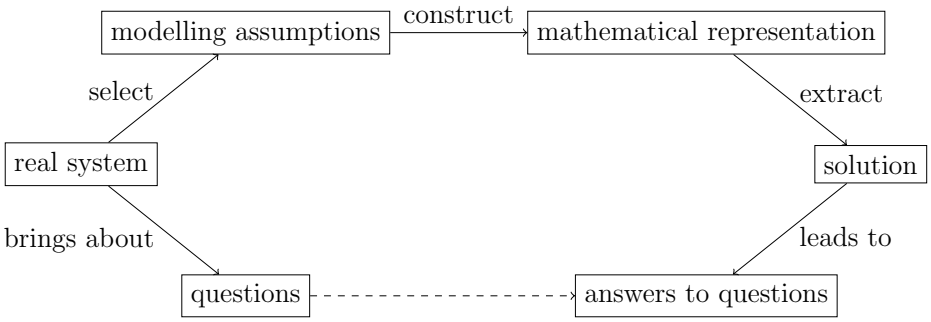


Figure 2.2: Schema of mathematical analysis of real systems. Compare with figure 2.1.

really having the privilege of hindsight that philosophers often presume in their rational reconstructions of scientific practice. For an engineer in that situation, the main objective is to use mathematics to answer contextually determinate questions in a situation that involves uncertainty and error at many different levels. To do so, the main challenge consists in using mathematical courses of thought that make it possible to properly manage uncertainty and error. In particular, if their origins are mis-diagnosed, mis-quantified, and if their consequences are mis-judged, dramatic consequences can ensue.

2.2 Deriving Model Equations from Modelling Assumptions

In order to make the procedure of derivation of model equations from modelling assumptions more definite, let us turn to a description of such procedures, and illustrate their use by examining two examples that are part of any basic physics curriculum. Both examples are mathematically simple; the first one is Kepler's two-body problem, and the second one is the analysis of the perfect lever. The case of the lever will be particularly interesting, since it allows us to discuss modelling of simple machines, a theme very much in the spirit of the engineering mindset mentioned above. The simple machines are elementary mechanical configurations of bodies in a system organized so that it changes

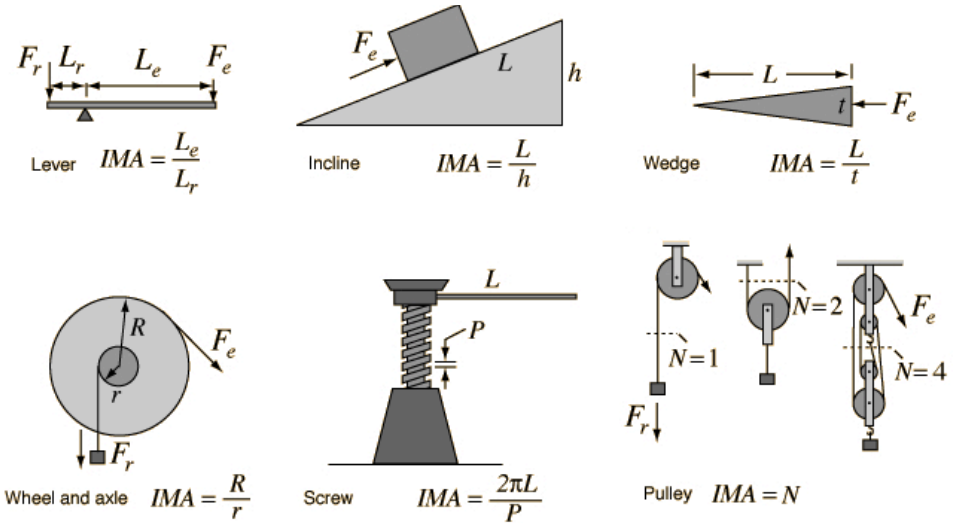


Figure 2.3: The six simple machines. *IMA* denotes the ideal mechanical advantage.

the direction or magnitude of a force to produce a mechanical advantage. The phrase "simple machine" most often refers to the following: lever, wheel and axle, pulley, inclined plane, wedge, and screw (see figure 2.3). Simple machines are very important, since they are often treated as the building blocks for more complicated machines. As a result, the process of mathematically modelling complex machines can be reduced to a series of modelling problems concerning simple machines.[10]

2.2.1 Model Construction Recipes

To begin, let us put the derivation procedures in context. The overarching question concerning machines is this:[11]

[10]Historically, many of the discussions in philosophy of science were devoted to understanding the methodological gambits involved in the analysis of mechanical systems. From this point of view, philosophy of science, as a study of scientific methodology, is not about how to interpret "fundamental" theories, and to this extent there is a significant difference with some dominant trends in contemporary philosophy of physics. Despite the fact that it is unreasonable to restrict philosophy of physics to this task, it is at its very core.

[11]In a similar way, Hall *et al.* (1961) describes the design process for machines as beginning with the identification of a *need* that defines the *problem*. On this basis, one can then ask whether some design will satisfy the need.

Q. Given a certain task, how should the machine be designed?

This question is quite general. As such, it incorporates many sub-questions. Consider these two examples:

1. Are the materials used for this machine design sufficiently strong to execute the task without breaking or being damaged?

2. Does the machine so designed have a mechanical advantage that figures better than other feasible designs?

The two questions differ in kind. The first question demands a yes/no answer concerning a single system corresponding to a given design. However, the second question involves comparing different systems corresponding to different designs. This involves a comparison of a collection of systems by means of a multiplicity of mathematical representations. This type of question is more complex and, for the time being, I will focus on questions that do not essentially involve the comparison of multiple systems. So, let us focus on the course of thought that brings us from a certain system to a mathematical representation of it. In figure 2.1 and 2.2, this corresponds to the arrow "construct."

I will refer jointly to the operations that are involved in this construction as "recipes of model construction," or just "recipes" for short. The term "recipe" is appropriate since the list of operations is procedural in nature, and yet it is not as strictly regimented as an algorithm is (texts on algorithmics often introduce algorithms using the analogy with cooking recipes). A good example of such a model construction recipe is *Euler's recipe*,which is characterized by the following five steps:[12]

(a) Delineate the class of bodies whose behaviour one wishes to study.

(b) Determine what specific forces act between these bodies, *i.e.*, what special force laws hold between them.

[12]What I present here as Euler's recipe is based on Wilson (1998) and Smith (2002). However, I changed the recipe of Wilson and Smith slightly in order for it not to be limited to systems of mass-points.

(c) Choose Cartesian coordinates and decompose each of the specific forces along the axes of this coordinate system.

(d) For each body under study, and for each axis, sum the component forces acting upon this body in the direction of the axis.

(e) Set this sum of forces equal to $m\frac{\mathrm{d}^2\mathbf{x}}{\mathrm{d}t^2}$ (Newton's Second Law).

In this case, the construction recipe encapsulates the logic of derivation of differential equations. Step (a) and (b) are the ones in which the *modelling assumptions* are selected. The modelling assumptions are statements having one of two forms:

1. There is a body B occupying a certain region at a certain time.[13]

2. There is a force acting on body B with magnitude r and direction $\hat{\mathbf{r}}$.[14]

In other words, the set of modelling assumptions is a list of existential statements that specifies the bodies, their types, and the forces acting on them.

The modelling assumptions are manipulated within a theory to obtain a set of model equations. In the recipe above, the theoretical background is used mainly in the last step by invoking Newton's second law.[15] Instead of using the word 'theory,' which is overloaded with different meaning in philosophical discussions, we could say that the construction of a model relies on kinematic assumptions.[16] The underlying kinematics comprises geometrical assumptions determining the structure of space and time, and some general principles of motion (such as Newton's second law). It is important to emphasize that kinematics does not address the existence of actual bodies, with the particular

[13]This could be made clearer by reference to the axiomatic theory of bodies developed by Noll and Truesdell (see, *e.g.*, Truesdell, 1974) and often used to describe the foundations of continuum mechanics, but I do not think it is necessary to go into the details here.

[14]Those quantities normally depend on the state of the system and on time (or some other variable parametrizing the states).

[15]I will assume that the lessons to be drawn from this analysis depend very little on what the fundamental nature of physical systems is, and as a result I will focus on the construction and evaluation of models in classical mechanics, mostly at the macroscopic level. I will defend this assumption in section 3.1.

[16]See Curiel (2011) for a similar characterization.

constraints associated with the nature of those bodies. However, we only begin to do mechanics *per se* once we add such constraints.[17] The branch that addresses the behaviour of bodies in this way is known as *dynamics*.

Thus, given a certain set of modelling assumptions and a kinematics, the construction recipes determine procedures to derive dynamical equations characterizing the temporal behaviour of the system, *i.e.*, equations describing the evolution of points or regions in a state space through time. Applied mathematicians often use the phrase *dynamical system* to discuss a system of equations describing states through time at a general level.[18] The evolution rule can be a differential equation (continuous time) or a difference equation (discrete time). Finding the trajectory in the state space prescribed by the rule from a given point is what we call *solving the system*. If the evolution rule is a differential equation, this amounts to integrating the system. However, many systems cannot be solved 'analytically,' which is why approximation methods are the main tool to extract information from dynamical systems (more on this point in section 4.1). This task corresponds to the stage labeled 'extract' in figures 2.1 and 2.2.

Concerning modelling assumptions, two important aspects of the construction recipe have to be emphasized, which are often overlooked in philosophical discussions. The first aspect is best presented by a simple example. Consider the situation in figure 2.4(a). In this case, we have a system with one body (the black ball) and one force acting on it (gravity). By itself, those two as-

[17] This is why many philosophers naively assume that a theory has observational consequences. But only mechanical, or equivalently, dynamical, models do so.

[18] Technically, a dynamical system is a triple

$$\langle M, T, \{\Phi^t\}\rangle\,.$$

The *state space* is an n-dimensional manifold M. The manifold comes with a family of smooth functions $\Phi^t : M \times T \to M$ that describes the evolution. If $T = \mathbb{R}$, Φ is a flow. If $T = \mathbb{N}$, Φ is a map. A flow is usually characterized by a differential equation of motion $x' = \phi(x)$. To get a trajectory, we need to specify some initial condition $x(t_0) = x_0$ or boundary conditions. ϕ is called the velocity vector field, and it is smooth on M. The vector field is not in M, but in the tangent space T_xM at x.

At this level of generality, dynamical system theory finds application in all mathematical sciences, and not only in physics.

(a) One body with one force, resulting in free fall.

(b) The same body subject to the same force; however, due to a second force, it results in rest.

Figure 2.4: Non-monotonicity of the logic of derivation of differential equations.

sumptions *do not* imply that the body's trajectory will be captured by the equation $\ddot{y} = -g$, that is, $y(t) = -\frac{1}{2}gt^2$. For suppose the two assumptions are satisfied, but we add another body and another force, as in figure 2.4(b). Then the ball is at rest, that is, $y(t) = 0 \neq -\frac{1}{2}gt^2$. Thus, in some sense, the logic of derivation of equations of motion is *non-monotonic*; if we add more modelling assumptions, the consequences are not necessarily preserved, *i.e.*, the inference schema[19]

$$\frac{\bigwedge_k M_k \to E}{\bigwedge_k M_k \wedge M_{k+1} \to E} \tag{2.1}$$

is not valid without restriction. An alternative way of looking at it is to assume that the construction contains an essential clause "and no other bodies nor forces are in the system" that is left implicit. Either way, to the extent that we are concerned with the dynamical consequences of a set of modelling assumptions, there is not much use in making a distinction between "omission" and "idealization" of aspects of systems.[20]

[19]The big wedge is a conjunction of all the existential statements in the set of modelling assumption and E is the statement expressing the model equation. M_{k+1} represents additional modelling assumptions.

[20]Nonetheless, some authors ascribe an important role to this distinction (*e.g.*, Jones, 2005).

The second aspect that should be emphasized is that there are two distinct senses of *facticity* and *empirical adequacy* in the logic of modelling. The first sense of facticity concerns the truth or accuracy of the modelling assumptions. The second sense of facticity concerns the truth or accuracy of the model equations. To discuss the virtues of mathematical representations, it is important to keep this distinction in mind. On the one hand, a set of literally true modelling assumptions can be far from factual with respect to the model equations.[21] This happens if the set does not contain the right modelling assumptions. On the other hand, a set of modelling assumptions containing modelling assumptions flagrantly lacking facticity can lead to factually accurate model equations. In other words, even if we lie about modelling assumptions, it does not imply that we lie about model equations.[22]

Even if there are two senses of facticity and empirical adequacy that differ and that are to some extent independent, there is a lack of systematic distinction in the literature. When philosophers discuss representational imperfections (see, *e.g.* Jones, 2005; Norton, 2011), it is hard to know if they are talking about the modelling assumptions or about the equation of state evolution $\mathbf{x}(t)$ (or the model equation of which it is a solution). But it is clear that, in order to understand how models represent systems, we need to be clear on whether the facticity is asserted with respect to the modelling assumptions or the model equations.

This being said, a last remark concerning the first sense of facticity must be made. For a modelling assumption, it is often possible to say whether they are factual or not. However, it is hard to directly assess such claims in a comparative way, *i.e.*, it is hard to determine whether an assumption is as far from the truth as another. That is even more difficult for *sets* of modelling assumptions. It is hard to determine whether a set of mostly accurate sentences will itself be accurate; in fact, it is hard to even see how such a notion of

[21] For instance, this would be the case if the set of modelling assumptions used in figure 2.4(a) were used to describe the behaviour of the body in figure 2.4(b).

[22] Pace Cartwright (1983), this of course does not mean that the laws of physics lie, since the modelling assumptions in question are not always laws. See Smith (2001). I will return on this point in section 3.1.

accuracy for sets could be directly defined. This is why we need the dynamical sense of facticity. As a matter of fact, since the modelling assumptions are only means (they are the ingredients of a recipe) to get to a representation, which is itself a means for answering questions about the behaviour of interest, the latter, dynamical sense of facticity is often by far the most important one.

2.2.2 Two Examples: Kepler's Two-body Problem and the Ideal Lever

To illustrate how Euler's recipe works, let us revisit the isolated system of two mass-point particles acted upon only by gravity described on p. 34. The analysis of this system is a special case of the problem known as Kepler's two-body problem. Step (a) of Euler's recipe is resolved by stipulation: there are two bodies, namely, two mass-point particles of mass m_1 and m_2 whose initial states at t_0 are $s_1 = [\mathbf{x}_1, \dot{\mathbf{x}}_1]$ and $s_2 = [\mathbf{x}_2, \dot{\mathbf{x}}_2]$. Similarly, step (b) of Euler's recipe is also resolved by stipulation: there is only one force acting on each of the two bodies, namely, gravitation. Since we work within classical mechanics, we assume that the force in question is specified by Newton's law of universal gravitation:

$$\mathbf{F}_G = Gm_1m_2\frac{\mathbf{r}}{\|\mathbf{r}\|^3} = Gm_1m_2\frac{1}{\|\mathbf{r}\|^2}\hat{\mathbf{r}}, \tag{2.2}$$

where $\mathbf{r} = \mathbf{x}_1 - \mathbf{x_2}$ is the vector between the two bodies, and $\hat{\mathbf{r}}$ is the directional vector $\mathbf{r}/\|\mathbf{r}\|$. We will write $\mathbf{F}_{ij}$ for the gravitational attraction of i on j; by Newton's third law, $\mathbf{F}_{ij} = -\mathbf{F}_{ji}$. Now, we turn to step (c) of Euler's recipe. Whether the initial states s_1 and s_2 are given in Cartesian coordinates or not, it is easy to obtain a representation in Cartesian coordinates by applying a transformation. This gives us a representation of $\mathbf{r}$ in Cartesian coordinates. Since $\mathbf{r}$ is the only vector quantity involved, we thus have a decomposition of

the specific force along the axes for each body:

$$\begin{aligned} F_{21,x} = Gm_1m_2\frac{r_x}{\|\mathbf{r}\|^3} \qquad F_{21,y} = Gm_1m_2\frac{r_y}{\|\mathbf{r}\|^3} \qquad F_{21,z} = Gm_1m_2\frac{r_z}{\|\mathbf{r}\|^3} \\ F_{12,x} = -Gm_1m_2\frac{r_x}{\|\mathbf{r}\|^3} \quad F_{12,y} = -Gm_1m_2\frac{r_y}{\|\mathbf{r}\|^3} \quad F_{12,z} = -Gm_1m_2\frac{r_z}{\|\mathbf{r}\|^3} \end{aligned} \tag{2.3}$$

Step (d) of Euler's method involves no work at all, since there is only one summand. Finally, we use Newton's Second Law $\mathbf{F} = m\mathbf{a}$ and set the sums of decomposed forces equal to $m\mathbf{a}$ (which equals $m\ddot{\mathbf{x}}$). We obtain two equations

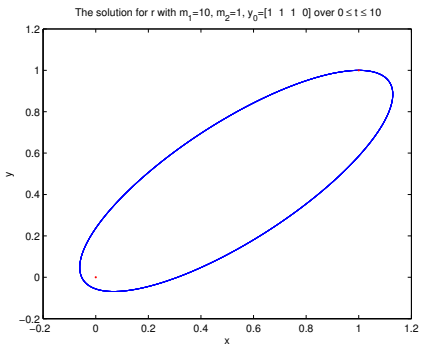
$$\ddot{\mathbf{x}}_{1,x} = Gm_2\frac{r_x}{\|\mathbf{r}\|^3} \qquad \ddot{\mathbf{x}}_{2,x} = Gm_1\frac{r_x}{\|\mathbf{r}\|^3} \tag{2.4}$$

for the x component and four other equations for the y and z components. But since $\mathbf{r}, \mathbf{x}_1$ and $\mathbf{x}_2$ are functionally dependent by definition, the system can be re-written

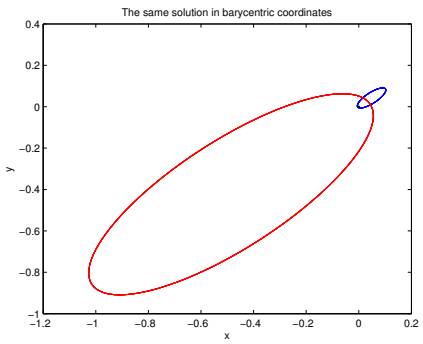
$$\ddot{\mathbf{r}} = \ddot{\mathbf{x}}_1 - \ddot{\mathbf{x}}_2 = \frac{F_{21}}{m_1} - \frac{F_{12}}{m_2} = G(m_1 + m_2)\frac{\mathbf{r}}{\|\mathbf{r}\|^3}, \tag{2.5}$$

which constitutes a system of three independent second-order differential equations, or, alternatively, six first-order differential equations (three of the position, three of the velocity). This set of differential equations is the mathematical representation resulting from Euler's recipe. Naturally, one could follow different recipes, such as the one developed by Lagrange, in which case an equivalent mathematical representation would obtain. In each case, we would have $6n$ equations (where n is the number of bodies) restricting the evolution of the states of each bodies.

Note, however, that the mathematical representation obtained here is *not* a function that directly describes the evolution of the states of the system. Formally, it has the form $\ddot{\mathbf{x}}(t) = \mathbf{f}(t, \mathbf{x}, \dot{\mathbf{x}})$ and not the form $\mathbf{x}(t) = \mathbf{f}(t, \mathbf{x}, \dot{\mathbf{x}})$. To obtain an equation of the latter form, it is necessary to *solve* the differential equation, *i.e.*, integrate the set of differential equations with respect to time. This is why, in figure 2.1 and 2.2, there is an arrow labeled "extract" between the mathematical representation and the solution. Chapter 4 will examine



(a) Solution for $\mathbf{r}$ around $\mathbf{0}$, which amounts to one body moving and the other being fixed.



(b) Solution for $\mathbf{x}_1$ and $\mathbf{x}_2$ rotating around the barycenter located at $\mathbf{0}$.

Figure 2.5: Time series $\mathbf{x}(t)$ for Kepler's two body problem.

this step in more details. Figure 2.5 displays the results of extracting the time evolution (or, in the dynamical system jargon, the time series) of the position states by means of a numerical scheme known as the Runge-Kutta-Fehlberg method.[23]

This recipe exemplifies how model construction recipes are part of the logic of modelling. However, this case only exemplifies the case of *abstract* models, as we find them in textbooks. In such cases, steps (a) and (b) of Euler's recipe are always trivial to execute, as the statement of the problem dictates which bodies are to be considered and what the forces to consider are, *i.e.*, what the proper modelling assumptions are. This is never the case for the construction of models of real systems. For the modelling of real systems, we have to *decide* how many bodies will be included in the model, what kind of idealized model will be used to represent those bodies, and what forces act on the bodies. As opposed to the construction of abstract models, the construction of real models thus presupposes a number of decisions. These require a justification; we will revisit this theme shortly.

For now, let us return to simple machines. We will construct a model to determine what the mechanical advantage of a lever is. The mechanical

[23] I used Matlab's command `ode45` with relative tolerance 10^{-6}.

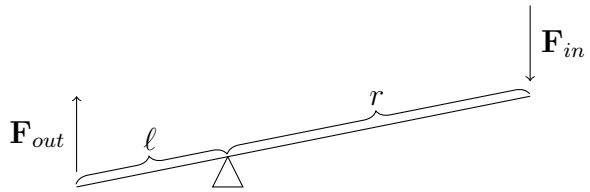


Figure 2.6: An ideal lever with the fulcrum situated between the points of application of the forces.

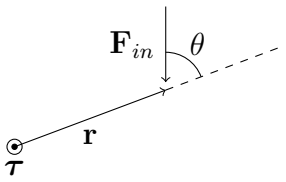
advantage *MA* is a dimensionless quantity defined as

$$MA = \frac{F_{out}}{F_{in}}, \tag{2.6}$$

where F_{in} is the magnitude of the force applied and F_{out} is the magnitude of the force resulting for the use of the machine. Typically, in a textbook, the problem stipulates what bodies and what forces are to be considered. Consider the configuration in figure 2.6. A force $\mathbf{F}_{in}$ is applied at one end of a beam of length $r + \ell$ supported by a fulcrum situated at length r from the extremity of the beam on which the force is applied. We assume that the only bodies in the system are the beam and the fulcrum; both of them are perfectly rigid bodies (there is no bending or breaking) and they are massless. Moreover, it is assumed that the fulcrum cannot be moved, and that there is no friction between the beam and the fulcrum. This ideal situation, in which there is no possible dissipation of energy and in which the lever system is completely isolated, is known as an *ideal lever*, or *perfect lever*.[24]

To analyze the mechanical advantage of a perfect lever, we need to make a few modifications to Euler's method. The class of bodies and the forces acting on the bodies are trivially established, since they are the problems' suppositions. However, step (c) of Euler's recipe has to be slightly modified to account for the fact that the fixity of the fulcrum and the rigidity of the beam force the motion to be rotational. Thus, instead of dealing with the forces

[24]If the machine does not *dissipate* or *absorb* energy, *MA* can be calculated by geometry alone. This is why it poses very limited computational difficulties; this is also why dissipative systems are studied by means of idealizations. Simple machines without friction or elasticity or wear (which all result in dissipation as heat) are called *ideal machines*.



$$\boldsymbol{\tau} = \mathbf{r} \times \mathbf{F} = \det \begin{bmatrix} \mathbf{i} & \mathbf{j} & \mathbf{k} \\ r_x & r_y & r_z \\ F_x & F_y & F_z \end{bmatrix}$$

$$\tau = \|\boldsymbol{\tau}\| = \|\mathbf{r}\| \cdot \|\mathbf{F}\| \sin\theta = rF \sin\theta$$

Figure 2.7: Relation of the torque about an axis to the applied force.

directly, we need to find the moments of force, or torques, as intermediaries. The moment of force is calculated with reference to an axis of rotation and an arm-vector that goes from the axis of rotation to the point of application of the force. In this case, we naturally choose the axis of rotation to be at the fulcrum, and the arm is the right-hand side of the beam $\mathbf{r}$. The moment of force is a vector perpendicular to both $\mathbf{r}$ and $\mathbf{F}$, in the direction of the axis of rotation, as in figure 2.7. The torque is orthogonal to both $\mathbf{r}$ and $\mathbf{F}$. How do we obtain F_{out} from this? Because the system is isolated, we can use the conservation laws characterizing classical mechanics; since the torque is related to the angular momentum $\mathbf{L}$, we will use the law of conservation of angular momentum. The angular momentum is $\mathbf{L} = \mathbf{r} \times \mathbf{p}$, where $\mathbf{p} = m\mathbf{v}$ is the linear momentum. So, we get

$$\frac{d\mathbf{L}}{dt} = \mathbf{r} \times \frac{d\mathbf{p}}{dt} + \frac{d\mathbf{r}}{dt} \times \mathbf{p} = \mathbf{r} \times m\frac{d\mathbf{v}}{dt} = \mathbf{r} \times \mathbf{F} = \boldsymbol{\tau}\,. \tag{2.7}$$

The law of conservation of angular momentum (which says that for a closed system, $\mathbf{L}$ is constant if and only if $\sum \boldsymbol{\tau}_{ext} = \mathbf{0}$) thus gives us a constraint on torque. Since $\boldsymbol{\tau}_{in} = \mathbf{r} \times \mathbf{F}_{in}$, $\boldsymbol{\tau}_{out} = \boldsymbol{\ell} \times \mathbf{F}_{out}$, and $\sum \boldsymbol{\tau}_i = \mathbf{0}$, we obtain

$$\|\mathbf{F}_{out}\| = \frac{\|\mathbf{r}\|\|\mathbf{F}_{in}\| \sin\theta}{\|\boldsymbol{\ell}\| \sin\phi} = \frac{\|\mathbf{r}\|}{\|\boldsymbol{\ell}\|}\|\mathbf{F}_{in}\|. \tag{2.8}$$

Since $\|\mathbf{r}\|/\|\boldsymbol{\ell}\|$ is the ratio that determines how far from the point of application of the force the fulcrum is, we see that F_{out} increases as ℓ decreases. Moreover,

the mechanical advantage is then simply

$$MA = \frac{F_{out}}{F_{in}} = \frac{\|\mathbf{r}\|}{\|\boldsymbol{\ell}\|}.$$

So, as we see, Euler's method is somewhat modified because we do not directly use the equation $\mathbf{F} = m\mathbf{a}$ to derive an equation of motion. But this is implicitly happening in relation to our use of conservation laws (namely, in equation 2.7). Thus, as we see, even for very simple problems, the model construction recipe has to be adjusted to accommodate the different kinds of physical concepts involved. This is why, despite the procedural nature, it is better understood as a recipe than as an algorithm.

2.3 Idealization and Tractability of Models

The examples from the previous section illustrate how model equations are derived from modelling assumptions. However, the cases that we examined do not really reflect the complexity involved in the mathematical modelling of *real* systems, since the set of modelling assumptions from which we derived model equations are extremely simplified compared to what would faithfully capture real physical systems. Needless to say, when we build a model for a system of real bodies, the inaccuracy and incompleteness of the modelling assumptions could very well lead us to answer questions about the behaviour of interest incorrectly. A traditional and widespread view to remedy this sort of situation has been discussed by Batterman (2002*a* : p. 21):

> A traditional view about modeling in the physical and applied mathematical sciences holds that one should try to find the most accurate and detailed mathematical representation of the problem at hand. [...] The aim here is to effect a kind of convergence between model and reality. One tries, that is, to arrive at a completely accurate (or 'true') description of the phenomenon of interest. On this view, a model is better the more details of the real phenomenon it is actually able to represent mathematically.

Using the terms employed here, the traditional view enjoins the modeller to use an *accurate and complete set of modelling assumptions* in order to guarantee

that the model equation derived from them correctly answers our questions about the system.

However, as Batterman (2002*a* : p. 22) argues, "the more details that are built into the model, the more intractable the mathematical equations representing the behaviour of interest are likely to be." That means that, even if we can somehow derive model equations from our accurate and complete set of modelling assumptions, it is likely that we will not be able to use them to make predictions and to obtain answers to our questions concerning the behaviour of interest. For that reason, one could say that those models are *too true to be good*.

In the case of Kepler's two-body problem, the discrepancy between the modelling assumptions and real systems lies in the fact that real bodies are not mass-points and that there are no gravitationally isolated systems of two bodies. Moreover, there normally are other forces that act on real bodies. If we add these elements to the list of modelling assumptions, we can derive another set of differential equation that will in principle better describe the evolution of the system through time. This will lead us to model equations for a many-body problem—potentially with additional non-gravitational forces—that will present serious problems for the extraction of solutions from the model equations.[25] Thus, the improvement of the accuracy of the modelling assumptions brings about a decline of the tractability of the model.

In the case of the machines, the modelling assumptions we used are misguiding to the extent that real machines are not ideal. If we consider what happens when a person uses a real lever, as opposed to an ideal lever, the situation is as in figure 2.8. The lever used has a mass, a volume, a variable density, it is deformable, there is internal shear and bending in the beam, there is friction with the fulcrum, the fulcrum might wiggle, there is dissipation of energy, and there are other forces acting upon the system. If we consider instead a system of masses and pulleys, we find that the pulleys are not perfectly circular, that they are not perfectly uniform (so that their moments of inertia

[25] More specifically, there will be no closed-form solution. We will discuss in section 4.1 what are the consequences of the lack of a closed-form solution.

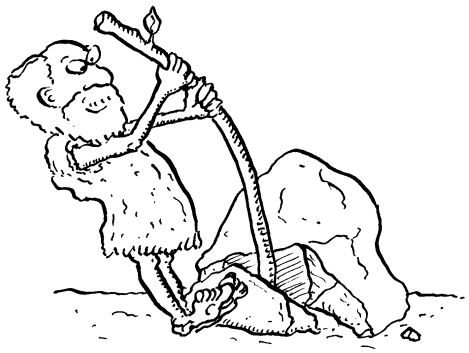


Figure 2.8: Real levers do not satisfy the idealizations used to study perfect levers (source unknown).

change), that there is friction and shear, that the cables stretch, that there might be wind moving the cables, that the temperature changes (so that the sizes of the pulleys and cables might change), *etc.* Thus, to obtain a reliable model of such systems, many factors that the analysis of ideal machines neglects would have to be considered. However, once again, the improvement of the accuracy of the modelling assumptions brings about a decline of the tractability of the model. In fact, we could no longer use simple geometrical methods applied to free-body diagrams to determine the consequences of our modelling assumptions, as we did in the previous section. As a result, the task of constructing the mathematical representation and of tracing its consequences would be significantly harder to execute.

Thus, there is a crucial dilemma between accuracy and completeness of modelling assumptions and tractability of model equations at the very core of the logic of mathematical modelling. What makes mathematical modelling difficult is that above all we must find a balance between accuracy, completeness, and tractability, as in figure 2.9. This being said, the fact that there are no gravitationally isolated systems of two mass-points or no real systems of perfect levers does not mean that there are no real systems that can be *treated as if* there were one. If it turns out to be the case that a real system can be understood by means of such a comparatively simple representation, then we have found a way to overcome the dilemma.

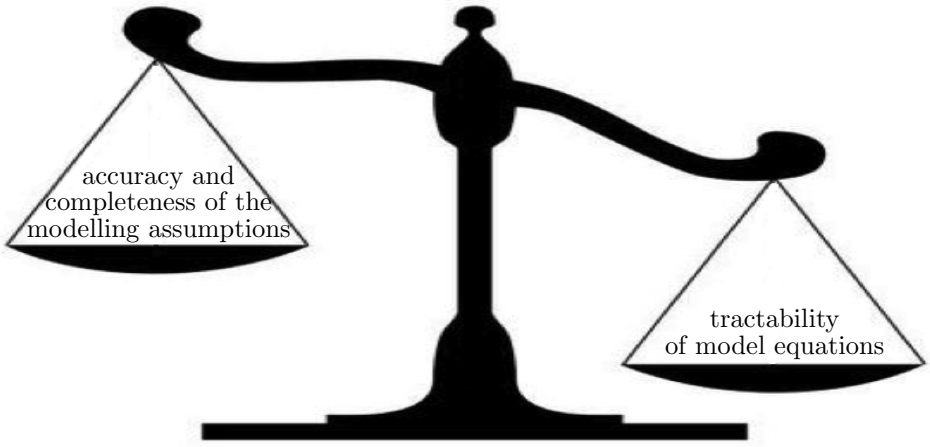


Figure 2.9: Balancing factors in mathematical modelling.

Such models, which have been successfully simplified and idealized in order to gain on the side of tractability, are called *minimal models*. What constitutes a successful simplification? How can we compare the consequences of minimal and complete models, given the intractability of the latter? The idea is to use perturbation methods, asymptotic analysis, and other forms of analysis of sensitivity to perturbation. From the exact solutions of the simplified model, one can use such methods to add correcting factors that correspond to rectification of the assumptions. We will return in more details to this later. For now, one should understand that the question of the accuracy of models *cannot* in general be simply addressed by saying "add more details to have a more accurate and complete set of modelling assumptions." There is always a cost-benefit analysis to perform, and the most important contribution of mathematics to modelling is that it provides the tools to do just that. Thus, counter-balancing the view of the role of applied mathematics as the language for formulating true representations of systems, there is the view that mathematics is "the art of finding problems we can solve," to quote the great mathematician Hopf.[26]

[26]Cited from Borel (1983).

2.4 Effective Management of Representations

As explained in the last section, one of the main challenges faced by mathematical modelling is to find a proper balance between the accuracy and completeness of the modelling assumptions used to generate a representation, and the tractability of equations constituting the representation. The former element is sought because we wish to correctly capture the properties and behaviours of bodies in a system. The latter is sought because we seek to *use* the representation in order to answer specific questions about some behaviour of interest. This introduces a pragmatic dimension, in that a representation that is not manageable, however true, accurate, or complete it is is entirely useless. To repeat the slogan used above, it is too true to be good.

One might wonder if the same holds of representations in general, or if it is limited to mathematical representations in the particular context of modelling real systems. In this section, I will suggest that it extends to other run-of-the-mill contexts as well, by discussing the representation of real systems by means of digital images. As we will see, they mirror mathematical modelling in important respects:

1. complete and accurate information is not needed to properly represent;

2. one can use procedures to determine how much information is needed, and which pieces of information are needed;

3. it is often imperative to discard superfluous information in order to have manageable representations.

Let us examine an example in detail.

A bitmap image is a picture which is formatted as an array of $m \times n$ pixels. As we have all experienced, if the number of pixels is insufficient, then the image is "pixelized" and has a bad quality. On such example is in figure 2.10. On the other hand, if the number of pixels is very large, then for many purposes it will occupy too much space and be very difficult to view or edit on certain computers. Bitmap images can be represented by an $m \times n$ matrix

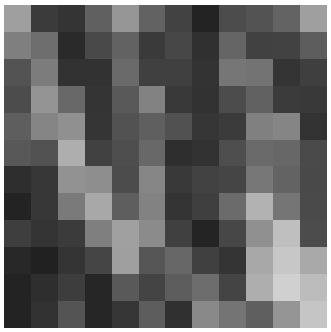


Figure 2.10: A low resolution bitmap image with 12×12 pixels.

$\mathbf{X}$ where each entry x_{ij} will be some integer. Moreover, bitmap images can be stored in many formats. The most basic format is 1-bit colour, which is also known as monochrome, in which case each pixel is either black or white (or any other two colours). In this case, each entry x_{ij} will take the value 0 or 1, and a colormap will associate those values with colours. For instance:

$$x_{ij} = 0 \leftrightarrow \text{pixel}_{ij} = \text{black} \qquad x_{ij} = 1 \leftrightarrow \text{pixel}_{ij} = \text{white}$$

Another common format is the 8-bit gray shades format. With 8 binary digits (also known as a byte), we can distinguish 256 values. We can then associate each entry x_{ij} with an integer $0 \leq n < 256$; the colormap will associate 0 with white, 255 with black, and the intermediary numbers are associated with 254 shades of gray.[27] The image in figure 2.10 is an 8-bit image with a gray shade

[27]Note that we could have a better image quality by adding to the bitrage of the matrix entries, thereby having a richer colormap. But this would have a cost for processing and storing the image, so it is not increased uselessly. Already, a certain constraint on the "resolution" of the information is set for the sake of manageability.

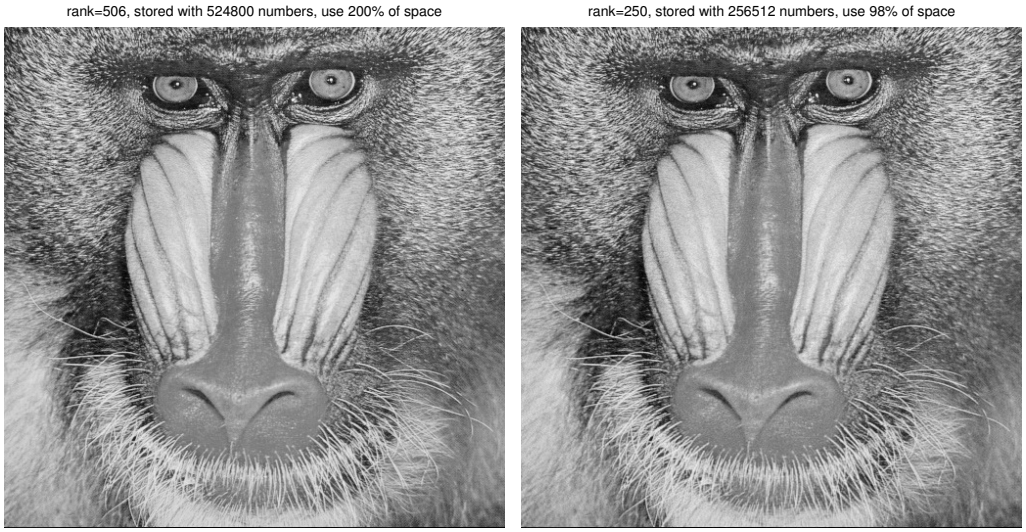
colormap associated with this matrix:

$$\mathbf{X} = \begin{bmatrix} 160 & 60 & 53 & 97 & 151 & 99 & 67 & 36 & 77 & 84 & 100 & 160 \\ 128 & 111 & 43 & 74 & 99 & 59 & 72 & 49 & 102 & 66 & 67 & 94 \\ 84 & 125 & 51 & 51 & 107 & 65 & 65 & 52 & 119 & 116 & 53 & 64 \\ 77 & 149 & 105 & 53 & 90 & 131 & 58 & 51 & 77 & 98 & 60 & 58 \\ 95 & 134 & 145 & 54 & 83 & 96 & 82 & 55 & 60 & 129 & 134 & 52 \\ 89 & 83 & 175 & 66 & 79 & 105 & 47 & 51 & 79 & 107 & 104 & 75 \\ 47 & 56 & 147 & 143 & 77 & 135 & 58 & 67 & 70 & 120 & 100 & 74 \\ 36 & 56 & 123 & 169 & 95 & 131 & 53 & 64 & 108 & 179 & 118 & 75 \\ 63 & 53 & 60 & 128 & 161 & 140 & 65 & 37 & 72 & 145 & 192 & 77 \\ 41 & 34 & 53 & 70 & 160 & 87 & 104 & 65 & 53 & 169 & 200 & 170 \\ 36 & 47 & 62 & 39 & 90 & 69 & 94 & 110 & 69 & 175 & 209 & 189 \\ 36 & 51 & 85 & 39 & 55 & 94 & 49 & 138 & 117 & 96 & 150 & 198 \end{bmatrix}$$

With the image, we can find the matrix, and with the matrix, we can generate the image. In other words, they have the same informational content. Given the association between bitmap images and matrices, it would seem that we need n^2 numbers to store an $n \times n$ pixel bitmap image, or some other close number of order $O(n^2)$. Now, the questions are:

- Are all those numbers equally important?
- Could it be that many of the details stored in those $O(n^2)$ numbers are inessential?
- And if some of details are superfluous, how much is superfluous and can we throw them away successfully?

Those questions are very important for computer scientists and graphic designers. In fact, the very possibility of compressing an image file depends on the possibility of throwing away information while capturing the essential features of the image. Thus, it provides us with a suggestive example of disposing of extraneous information to obtain a representation that we can manipulate as we wish or need. We will examine a method to throw away details in order to compress images. Note that I did not choose this method because it is efficient (there are better compression methods), but rather because it shows that *the mathematics involved is exactly the same as the mathematics used in many physical modelling contexts.* The method used, known as *principal*



(a) Original image, with 100% of the information displayed.

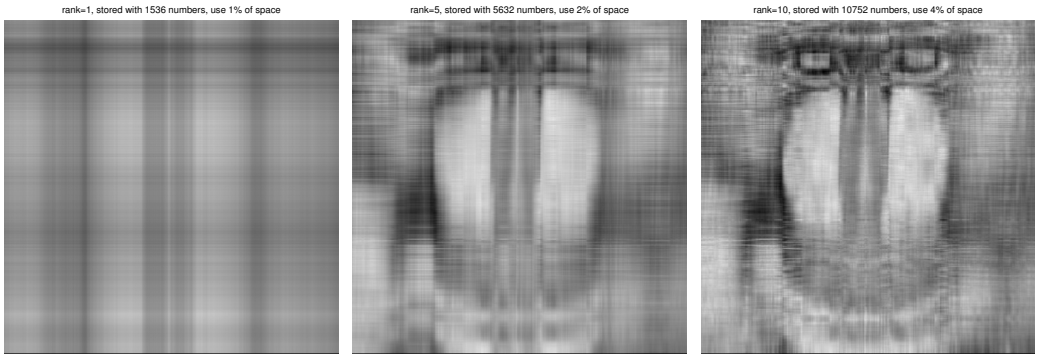
(b) Image with 50% of the information eliminated.

Figure 2.11: 512×512 pixel bitmap image of a mandrill face with an 8-bit gray shades colormap.

component analysis, is based on the *singular-value decomposition* of matrices (see, *e.g.*, Moler, 2004). This method strikingly resembles what Batterman (2002*a*) describes as intermediate asymptotics and dimensional analysis, and what many works on numerical methods describe as conditioning analysis (see, *e.g.*, Corless & Fillion, 201x; Higham, 2002), and it touches on the method of Lyapunov exponents for the study of chaotic methods (I will return to this below). The latter two also rely on the singular-value decomposition.

To illustrate the principal component analysis method, let us consider the image displayed in figure 2.11(a). In figure 2.11(b), we see the same image, except that half of the information has been thrown away using the method of principal component analysis.[28] It is hard to notice any big change between the two images. In fact, it takes a very good eyesight to notice any difference. This means that a lot of information contained in the matrix of the original image is not necessary to represent the mandrill face correctly.

[28]I will explain how I establish this percentage below.



(a) Image with 0.2% of the information.

(b) Image with 1% of the information.

(c) Image with 2% of the information.

Figure 2.12: Compressed versions of the image of the mandrill face.

Now, what would happen if we continued to discard information? It turns out that very little information is required to capture the most important traits of the image. The precise amount of information might vary from image to image. In the case of the mandrill face, we see that the minimal amount of information that still allows us to reconstruct the image would be clearly insufficient (that happens when 99.8% of the information is discarded), as shown in figure 2.12(a). Perhaps surprisingly, when 99% of the information is discarded, we can already discern a face (see figure 2.12(b)), and the mandrill face is already taking form when 98% of the information is discarded (see figure 2.12(c)). Yet, it is obvious that much is missing from those images. Given this, the interesting questions to ask are thus:

- how much information should be included? and
- is there a principled mathematical explanation for this amount?

Let us examine how the method works to answer them.

The original bitmap image of the mandrill face is mathematically represented by a 512×512 matrix and stored with 512^2 numbers, as explained before. The key to re-organizing the information contained in this matrix for the sake of deciding what can be thrown away lies in the central idea of linear algebra, namely, the idea that matrices can be factored. If, for a certain matrix

$\mathbf{A}$, we can find two other matrices $\mathbf{B}$ and $\mathbf{C}$ such that $\mathbf{A} = \mathbf{BC}$, then $\mathbf{B}$ and $\mathbf{C}$ are factors of $\mathbf{A}$. However, in linear algebra, we are usually not interested with arbitrary factors, but rather only with factors satisfying certain specific conditions, *e.g.*, being diagonal, triangular, unitary, *etc.* One such factoring is known as the singular-value decomposition,[29] or SVD for short.[30] The SVD gives

$$\mathbf{A} = \mathbf{U}\mathbf{\Sigma}\mathbf{V}^H$$

where $\mathbf{\Sigma}$ is a diagonal matrix with non-negative entries σ_k, arranged in order of magnitude, $\mathbf{U}$ and $\mathbf{V}$ are unitary matrices, and ^{H} denotes the complex conjugate transpose. The entries σ_k are known as singular values. To use this decomposition to analyze images, define a matrix that depends on the kth component by the outer product

$$\mathbf{E}_k = \sigma_k \mathbf{u}_k \mathbf{v}_k^H$$

where $\mathbf{u}_k$ is the kth column of $\mathbf{U}$, *etc.* Each column is a multiple of $\mathbf{u}_k$ and each row is a multiple of $\mathbf{v}_k$. Hence, all the matrices $\mathbf{E}_k$ so defined are rank-1 and orthogonal, *i.e.*, $\mathbf{E}_i \mathbf{E}_j^H = 0$ if $j \neq k$, and also $\|\mathbf{E}_k\| = \sigma_k$. Hence, using those matrices, we can write $\mathbf{A}$ as

$$\mathbf{A} = \mathbf{E}_1 + \mathbf{E}_2 + \ldots + \mathbf{E}_p$$

with $p = \min(m, n)$ (or just $p = n$ if the matrix is square). The contribution of each $\mathbf{E}_k$ to the quality of the image is determined by the size of σ_k. If σ_k is smaller, it has a smaller contribution, and *vice versa.* Finally, if we truncate at $r < p$, we have

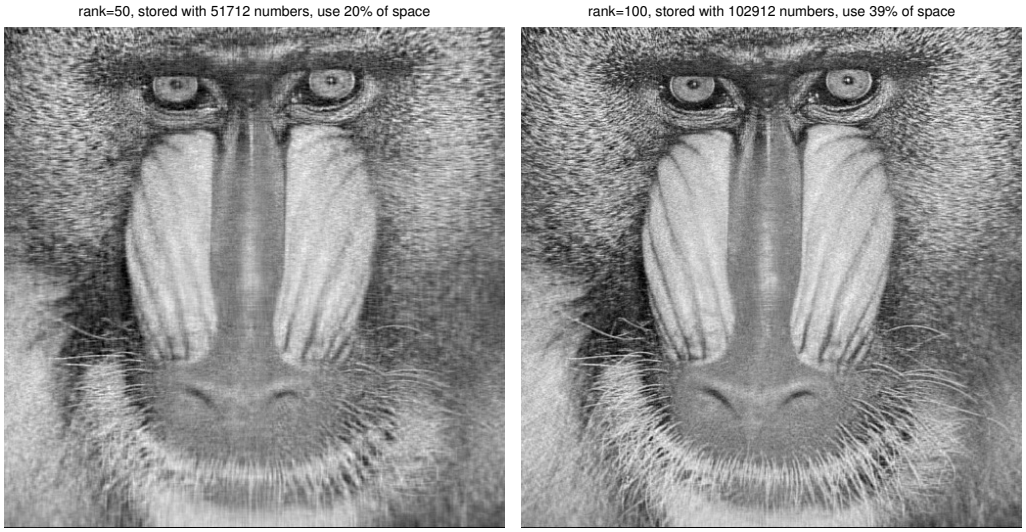
$$\mathbf{A}_r = \mathbf{E}_1 + \mathbf{E}_2 + \ldots + \mathbf{E}_r$$

which is a rank r approximation to $\mathbf{A}$. In fact, it can be shown to be the best rank r approximation to $\mathbf{A}$.[31] In this approximation, all the contributions of

[29] I will use the terms 'factoring,' 'factorization,' and 'decomposition' interchangeably.

[30] I will explain the geometrical meaning of this factoring in more details later in this section.

[31] This is the Schmidt-Eckart-Young theorem.



(a) Image with 10% of the information. (b) Image with 20% of the information.

Figure 2.13: Three intermediary compressed versions of the mandrill face.

the terms with smaller singular values (from σ_{r+1} to σ_p) are discarded. Thus, if our matrix $\mathbf{\Sigma}$ contains 512 singular values and we use only 102 of them, we have discarded 80% of the information. This is the method I implemented in Matlab to generate the compressed images above.

How does this constitute the basis for a compression algorithm? Notice that to compute the matrix $\mathbf{A}_r$, we only need r columns of $\mathbf{U}$ and $\mathbf{V}$, and r diagonal entries of $\mathbf{\Sigma}$. We throw away the rest! Thus, instead of storing $O(n^2)$ numbers, we store $2rn+r = O(n)$ numbers only. The amount of storage space saved is mentioned in figure 2.11, 2.12, and 2.13.

As we see, this matrix decomposition gives us the basis to decompose the image in components and to determine which ones are the most important. But how does it help us to understand how many components provide an important contribution? From figure 2.11 and 2.12, we know that it is somewhere between 10 and 250. Let us examine two intermediate values in figure 2.13. There is a very important change between figure 2.12(c) and figure 2.13(a) (where the approximation goes from 5% information to 10%, but a smaller change between the latter and figure 2.13(b). How can this be explained using the

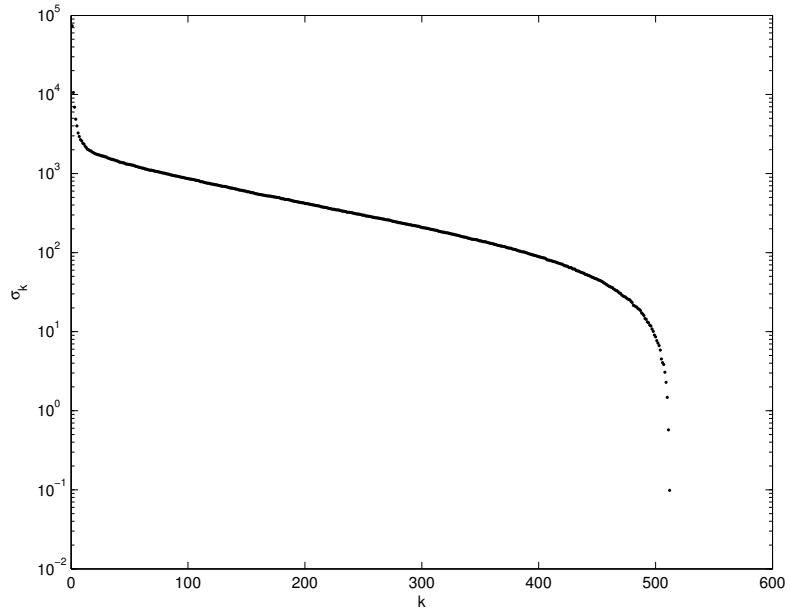


Figure 2.14: Semilog-y plot of the size of the singular values of the matrix associated with the mandrill face.

SVD decomposition? We have explained that the $\mathbf{E}_k$s summed up to obtain the approximation $\mathbf{A}_r$ of $\mathbf{A}$ depend on the corresponding σ_k. Thus, we can acquire the insight we need concerning how much information matters by inspecting the size of the singular values. See figure 2.14. As we see, the size of the singular values rapidly drop from almost 10^5 to about $1.5 \cdot 10^3$. This first section contains 35 ($\approx 7\%$) points. Then, there is a segment along which the singular values decrease slowly, until it reaches about 10^2. The last section of singular value below 10^2 contains 123 points ($\approx 25\%$).

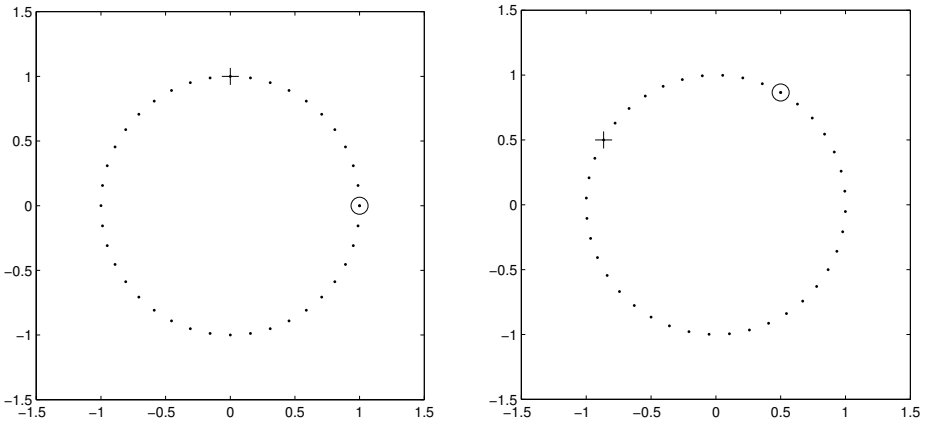
The examination of this curve explains well why we find the quality that we observe in the various figures presented here, given their respective amount of information. As we see in the progression of figure 2.12(a), 2.12(b), 2.12(c), and 2.13(a), there is a very steep gain in quality compared to the increase in information. As the other figures show, there is then a very slow increase of quality. The fact that the last half of the singular values are low compared to the values of the first 100 or so explains why there is only a very small difference between figure 2.11(b) and 2.11(a), despite doubling the amount of

information contained.

As in the case of intermediate asymptotics discussed by Batterman (2002*a*), we observe that some dimensions of the system contribute almost nothing to the essential information that we need to accurately represent the system. As a result we can accurately use a representation of lower dimension. Moreover, given the fact that we *want* and *need* to perform manipulations with our representation, it is *beneficial* to use the lower dimensional representation.

Finally, it is important to understand that the mathematical method used in principal component analysis, which is based on the singular-value decomposition, is not limited to image processing. It is a method of sensitivity analysis that has many applications all across applied mathematics. Moreover, like other methods of sensitivity analysis, it is grounded in the geometry of the problem. Fortunately, in the case of the singular-value decomposition, the geometry is simple enough to describe intuitively. A matrix $\mathbf{A}$ can be thought of as an operator that takes a vector as input and returns another vector; in other words, $\mathbf{A}$ acts on or transforms vectors or sets of vectors. Let us consider the collection of unit vectors that form a circle in the Cartesian plane, some of which are displayed in figure 2.15(a). The sensitivity to perturbation can thus be understood geometrically by examining the effects of the transformation matrix on the unit circle. Consider a first example, displayed in figure 2.15(b): an orthogonal or a unitary matrix transforms the unit circle into the unit circle, but does rotate it about the origin. In this case, the transformed vectors have the same length (in 2-norm) as the input vectors. Thus, if there is a small perturbation on the input vector, the exact same perturbation (in magnitude) will be found in the output. Because of their insensitivity to perturbations, those matrices are said to be perfectly well-conditioned. This is why, in error analysis and perturbation theory, unitary matrices are so cherished.

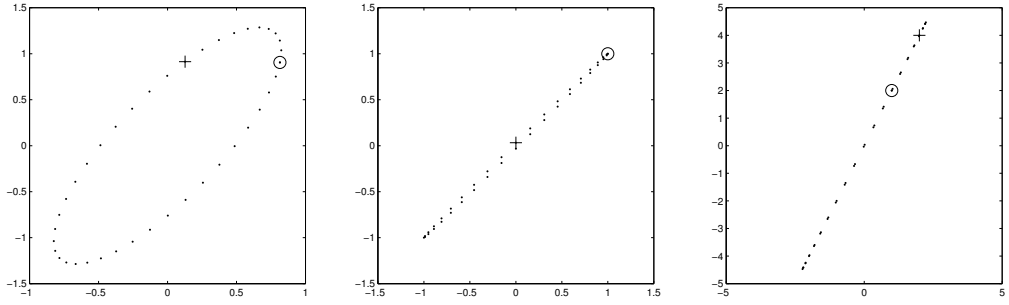
But things are not always so nice. Figure 2.16 displays the effect of matrices that are *not* perfectly well-conditioned. In figure 2.16(a), we see that a well-conditioned matrix transforms the unit circle by stretching in into an ellipse, and rotating it. As we have seen, the rotation has no impact on the sensitivity of the problem; the stretching, however, is the determining factor.



(a) Unit Circle (for vectors measured with the 2-norm)

(b) Orthogonal or Unitary Transformation of the Unit Circle

Figure 2.15: Matrices as transformations of the unit circle. Two points are marked with $\odot$ and + to make the action of the transformation clear.

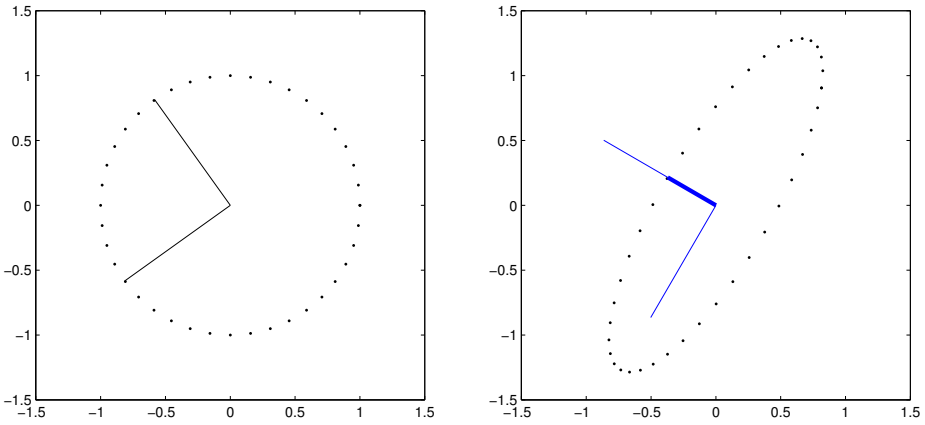


(a) Effects of a Well-Conditioned Matrix on the Unit Circle

(b) Effects of an Ill-Conditioned Matrix on the Unit Circle

(c) Effects of a Singular Matrix on the Unit Circle

Figure 2.16: Sensitivity analysis and aspect ratio of ellipses.



(a) The semi-axes of the unit circle are the columns of $\mathbf{V}$.

(b) The semi-axes of the ellipse are the columns of $\mathbf{U}$ scaled by the corresponding singular values.

Figure 2.17: Sensitivity analysis and the singular-value decomposition.

By visual inspection of the figure, we see that the aspect ratio of the ellipse, *i.e.*, the ratio of the lengths of the semi-major and the semi-minor axes of the ellipse, is about $3:1$. Thus, we can conclude that the transformation will be somewhat sensitive to perturbations, but not much. This is why the matrix is deemed well-conditioned. Figure 2.16(b) displays the effects of an ill-conditioned matrix. In this case, the aspect ratio is really bad, so we know that the transformation will be very sensitive to perturbations. Finally, in figure 2.16(c), we see the effect of a singular matrix. A singular matrix projects the circle onto a line, which can be regarded as an ellipse with an infinite aspect ratio. As a result, the matrix is infinitely ill-conditioned and the effects of perturbation can have devastating effects.

All this geometrical analysis of the effects of perturbation in linear systems is captured by the singular-value decomposition, as illustrated by figure 2.17. Again, we start with the unit circle, and decompose the transformation matrix $\mathbf{A}$ into its factors $\mathbf{U\Sigma V}^H$. The columns of the matrix $\mathbf{V}$ determine a set of unitary (or orthogonal) vectors forming a basis for the input vectors. The columns of $\mathbf{U}$ determine a set of unitary (or orthogonal) vectors forming a basis for the output vectors; the semi-axes of the resulting ellipse are scaled by the

corresponding singular values.[32] As a result, the smallest singular value represents the maximal *contraction* factor, and the largest singular value represents the maximal *stretching* factor. Numerical analysts call the ratio of the largest singular value to the smallest one the *condition number* (I will explain this concept in detail in section 4.4). For the geometrical reasons described above, this number entirely captures the sensitivity of the matrix to perturbations. However, it also allows us to infer that the largest singular values will have a larger impact on the increase in magnitude of the transformed vectors, and so we can use this fact to identify the principal components of a representation, as we have done with the mandrill face.

In fact, the sensitivity analysis based on the singular-value decomposition closely relates to how Lyapuonv exponents are used to understand dynamical systems, in particular to identify chaotic systems. Chaotic systems can be studied from many perspectives and, accordingly, chaotic motion can be defined in many ways.[33] In all cases, some intuitions drawn from physics motivate the various approaches: "the concept of 'chaos' recalls the erratic, unpredictable behavior of elements of a turbulent fluid or the 'randomness' of Brownian motion as observed through a microscope. For such chaotic motions, knowing the state of the system at a given time does not permit one to predict it for all later times." (Campbell & Rose, 1983 : vii) The idea is that a chaotic motion $\mathbf{x}(t)$ satisfying a deterministic nonlinear differential equation $\dot{\mathbf{x}}(t) = \mathbf{f}(t, \mathbf{x})$—or a difference equation—is *bounded* (*i.e.*, $\mathbf{x}(t)$ does not go to ∞ as $t \to \infty$), *aperiodic* (*i.e.*, for no T does $\mathbf{x}(t) = \mathbf{x}(t+T)$) and extremely *sensitive to initial conditions*. Now, if two trajectories $\mathbf{x}(t)$ and $\mathbf{z}(t)$ were uniformly diverging (*i.e.*, if the distance between the two trajectories were continuously increasing with t), at least one of them would be unbounded. But because of the non-linearity of the equation, the distance between the two curves varies in very erratic ways, and so does the rate of divergence. To establish sensitivity

[32] With n-vectors, instead of circles and ellipses, we would have n-spheres and n-dimension ellipsoids.

[33] Martelli *et al.* (1998 : p. 112) claim amusingly, "with a bit of exaggeration, that there are as many definitions of chaos as experts in this new area of knowledge." Concerning some of the conceptual issues involved with different definitional attempts, see Batterman (1993).

to initial condition, the important thing is that, *on average*, for finite time, the trajectories diverge from each other. This is exactly what positive Lyapunov exponents indicate. The Lyapunov exponents λ_i are defined as the *logarithms of the eigenvalues of* Λ, where

$$\Lambda = \limsup_{t\to\infty} \left(\mathbf{X}^T\mathbf{X}\right)^{\frac{1}{2t}}, \tag{2.9}$$

where $\mathbf{X}$ is the fundamental solution matrix of a system of ordinary differential equations. In other words, the Lyapunov exponents are closely related to the eigenvalues of $\mathbf{X}^T\mathbf{X}$. But the eigenvalues of $\mathbf{X}^T\mathbf{X}$ are just the squares of the singular values of $\mathbf{X}$ (since $\mathbf{X}^T\mathbf{X} = (\mathbf{U}\mathbf{\Sigma}\mathbf{V}^H)^H\mathbf{U}\mathbf{\Sigma}\mathbf{V}^H = \mathbf{V}\mathbf{\Sigma}^2\mathbf{V}^H$, we have $\mathbf{\Lambda} = \mathbf{\Sigma}^2$). If we take a small perturbation (or any other small displacement) in $\mathbf{x}$, that we label $\epsilon\mathbf{x}$, the singular-value decomposition gives us a nice geometrical interpretation of equation (2.9) using the ellipsoids mentioned above.[34] As a result, we see that the singular values show how much the fundamental solution matrix $\mathbf{X}$ stretches the perturbation vector $\epsilon\mathbf{x}$ (here, since the original equation is nonlinear, the singular values will be functions of t). Thus, if the Lyapunov exponents of $\mathbf{X}$ are positive, the average exponential growth will be *positive*, and so our initial-value problem will be sensitive to initial conditions. In this case, as opposed to the cases discussed above, the singular-value decomposition shows that it would *not* be justified to ignore details and perturbations of the system in order to correctly extract information about the states $\mathbf{x}$.

2.5 Comparing Mathematical Representations

Given the intricacies involved in the logic of modelling, it is important to ask:

[34]The explicit relation is this. If we take the logarithmic average of the singular values when $t \to \infty$ (because we are interested with average exponential growth), we get

$$\lim_{t\to\infty} \frac{1}{t} \ln \sigma_i(t) = \lim_{t\to\infty} \ln\left(\sigma_i^2(t)\right)^{\frac{1}{2t}} = \lim_{t\to\infty} \ln \mathrm{eig}_i(\mathbf{X}^T\mathbf{X})^{\frac{1}{2t}} = \ln \mathrm{eig}_i(\Lambda) = \lambda_i.$$

> What exactly do we mean when we say that a model (*i.e.*, a representation) is good, or that a model is just as good as another model?

This question can be answered in multiple ways, and implicit disagreement on this question is often a source of misunderstanding. In what follows, I examine some ramifications of this question and establish connections with the justification of the courses of thought and methodological gambits involved in mathematical modelling.

2.5.1 Six Senses in which Models Represent

In the context of our discussion, there are two main axes along which the question mentioned above can be developed. Firstly, this question can receive different answers depending on what is meant by 'good.' A natural and widely accepted view among philosophers is that a model is good if it is true. Another view, which is more in line with the actual practice of applied mathematicians and engineers, is that a model is good to the extent that it is close enough to truth (*i.e.*, sufficiently accurate). Many philosophers object to the concept of approximate truth; for example, Laudan (1981) claims that it is "just so much mumbo-jumbo." However, to the extent that approximate truth is to be understood in terms of accuracy, it should not be considered objectionable. Finally, an even weaker criterion—that could be called 'selective accuracy'—would further restrict the requirement of accuracy to only a few properties of the system that we find ourselves interested with; I consider this to be weaker because it allows for a model to be considered good given a set of questions, and not good given another set of questions.

Secondly, this question can receive two answers that map onto the distinction that we made above between two senses of facticity of models. The first sense of facticity concerns the accuracy of the set of modelling assumptions, and the second sense concerns the accuracy of the model equations. Thus, one can say that a model is good if it is derived from factual (or sufficiently accurate) modelling assumptions, or one can say that a model is good if its model equations are accurate. As a result, we obtain at least six different ways

Target / Criteria	Modelling assumptions	Model equations
Truth	T-MA	T-ME
Accuracy	A-MA	A-ME
Selective Accuracy	SA-MA	SA-ME

Table 2.1: Six senses of 'goodness' of a model (*i.e.*, a representation).

of answering whether a model is good (see table 2.1). To my knowledge, they do not have names that academics agree on, and so I will merely use acronyms to support the discussion below.

Which sense of 'good' will allow us to properly understand the logic of modelling? To begin with, it is important to realize that each of them has a role to play in our understanding of the role and success of mathematics in science. However, to the extent that we are trying to understand when *models* are good, the discussions that focus on modelling assumptions are not hitting the right target. The reason for which we construct a model is that our interest is not in the modelling assumptions *per se*, but rather in deriving model equations that represent the system well. In other words, our interest is not primarily set on what the system is, but rather on what happens in it. In turn, the model equations will be analyzed so that we can extract information to answer questions concerning the behaviour of interest. However, as we noticed above, the two sense of facticity are relatively independent, so *unless* one already has reasons to believe that the set of modelling assumptions contains the *right* assumptions, their accuracy will not guarantee the accuracy of the model equations. Consequently, given that we are attempting to understand when models are good, we should focus on the views in the right-hand column of table 2.1.

So, let our subsequent discussion of what a good model is target the model equations. Is truth an appropriate way to understand what a good model is? Firstly, we should emphasize that it is certainly not a bad one, in the following

sense: if a model equation exactly describes what happens in a system, *i.e.*, if it is literally true, then the model provides a good representation of the system. Thus, truth seems to be a *sufficient* condition for a model to be good; however, it is by no means *necessary*. One the one hand, if truth were required of a good model, then there would not be a widely usable criterion to determine when a model is good. On the other hand, a model is meant to help us answer questions about the behaviour of interest; if it is close enough to the truth to fulfill this function, then it must be considered good. Moreover, as argued, seeking exact truth might impede our ability to know what the model says. Thus, by elimination, the relevant senses in which a model is good is A-ME and SA-ME in table 2.1.

This provides support for the claim made in chapter 1, namely, that an elementary model-theoretic machinery is not sufficient to capture the semantical aspects of model construction and model evaluation. Accordingly, the relevant notion of adequacy of mathematical representations should not be defined only in terms of satisfaction. In the following subsections, I argue that it should rather be understood in terms of perturbation. More precisely, I argue that the semantic evaluation of our models should be characterized in reference to the effects of perturbations on the quantitative and qualitative behaviour of the system. Along this line, I will provide a perturbation-theoretic account of comparative accuracy of mathematical representations that will be seen to be the cornerstone of the justification of the methodological gambits employed in applied mathematics and, as a result, as explaining successes of mathematics in the natural sciences.

2.5.2 Qualitative Behaviour and Selective Accuracy

Now that we have narrowed down what a good model is, we should try to find out when a model is 'good enough.' Implicitly, this problem requires that we determine when a model is better than another, or just as good as another, *i.e.*, it involves the comparison of the accuracy of different model equations. As a part of the theory of dynamical systems, applied mathematicians have

developed many concepts that capture when two models are essentially similar. Some of them are rather simple, others are more complex and refined.

Models can be built with different objectives in mind. In some cases, it is important to obtain precise numerical information concerning the solution—*i.e.*, the states of the system on a given time interval—but in others we are only concerned with properties of the solution that only indirectly depend on the actual values of the states at times.[35] In other words, we are sometimes not interested in knowing exactly what happens in a system, but rather only in what kinds of thing happen in a system. In this case, we will consider a model to be selectively accurate (and therefore a good representation) if it implies the right kinds of things. This is why *qualitative analyses* based on the general theory of dynamical systems plays an important role in determining what a 'good' model is.

Let us begin our discussion of the qualitative aspect with a simple case. Suppose a model is characterized by the following model equation:

$$\dot{x} = x^2 - t, \qquad x_0 = x(0) = -\frac{1}{2}. \tag{2.10}$$

This is a differential equation of the state x with respect to time, and it also contains an initial condition. Together, they determine what will happen in the system. What would happen if the initial condition were instead 0, or -1, or -3? What if the same initial conditions were not given at $t = 0$ but rather at $t = 3$, $t = 5$ or $t = 7$? As it turns out, it would essentially change nothing, except for a very short initial time interval. The solution of equation (2.10) is displayed in figure 2.18 in bold, together with many other solutions using different initial values. We see that the trajectories all converge to the same one extremely rapidly. As a result, we can claim that the solution is insensitive to perturbations of the parameter x_0. When this is the case, we can say that two model equations with different values of x_0 are equally good, unless the behaviour of interest concerns the early time interval. Accordingly,

[35] Examples of such properties of solution include being periodic, approaching a limit cycle, having vanishing terms, being bounded, *etc.* In general, it includes all properties related to the structure of attractors.

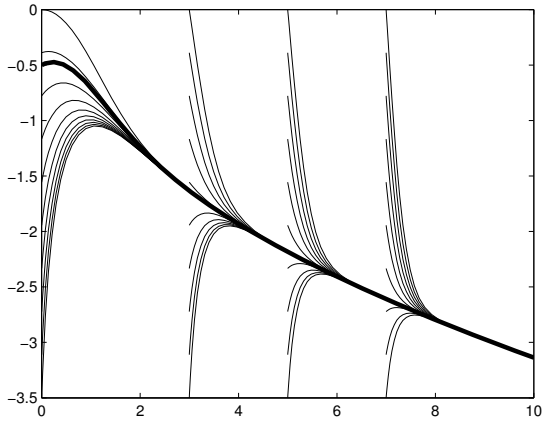


Figure 2.18: Equation (2.10) is an extremely robust differential equation.

we say that both the reference model and perturbed versions of the model are selectively accurate, both quantitatively and qualitatively.

In the above example, the sense in which the two model equations are equally good can be made more precise. Suppose we have a differential equation $\dot{x} = f(t, x)$ and two initial conditions $x_0 = \alpha$ and $x_0 = \beta$, and further suppose that $y(t)$ and $z(t)$ are their respective solutions. If we have

$$\lim_{t\to\infty} y(t) = x(t) \qquad \text{and} \qquad \lim_{t\to\infty} z(t) = x(t),$$

then we say that the two model equations have solutions that converge in the limit $t \to \infty$. Often, the solution of differential equations can be written as a sum of two solutions as follows:

$$x = x_{\text{transient}} + x_{\text{steady state}}$$

The transient solution is the one that makes the trajectories differ early on, but then it vanishes as time increases and the steady state solution becomes the dominant one. Differential equations with the same steady state solution will thus be considered to be equally good, provided that the transient behaviour is *not* crucial. *Ignoring transient behaviour is a typical case of selective accuracy.*

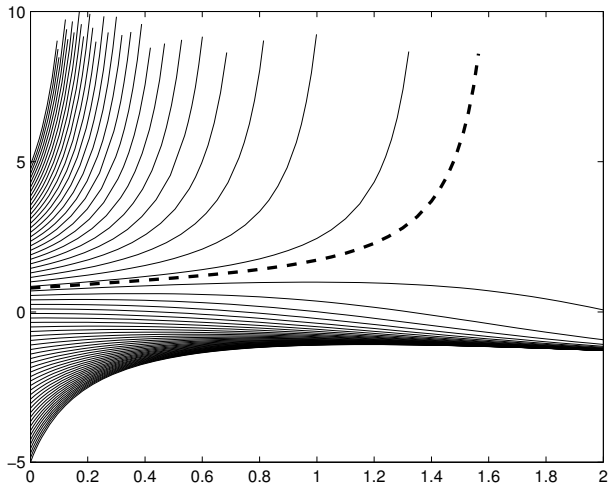


Figure 2.19: Qualitative change in the solution of $\dot{x} = x^2 - t$ at $x(0) = \sqrt[3]{1/2}$ (which is the dotted line).

In the example above, we would thus consider all systems for which $x(0) < \sqrt[3]{1/2}$ equivalent in this sense. However, for $x(0) \geq \sqrt[3]{1/2}$, the systems could not be considered equivalent, since they would diverge away from each other, as shown in figure 2.19. At $x(0) = \sqrt[3]{1/2}$, there is a qualitative change taking place in the behaviour of the system, *i.e.*, there is a *bifurcation*. Moreover, note that, if we set a value $x(0)$ less than, but close to the critical value, a perturbation of the system could easily push the system on the other side of the bifurcation line; thus great care would need to be taken in this region. As a result, we would not say that both the reference model $\dot{x} = x^2 - t$, $x(0) = \sqrt[3]{1/2} - \epsilon$, and slightly perturbed models are selectively accurate.

Above, we examined the sensitivity to changes in the initial conditions. What would happen if we made modifications in the function f in a differential equation $\dot{x} = f(t, x)$ instead? Consider the family of coupled two-dimensional linear systems of ordinary differential equations given by

$$\dot{x} = ax + by$$
$$\dot{y} = cx + dy$$

plus some initial conditions x_0 and y_0. It can be rewritten in matrix-vector notation as $\dot{\mathbf{x}} = \mathbf{A}\mathbf{x}$, where

$$\mathbf{x} = \begin{bmatrix} x \\ y \end{bmatrix} \qquad \text{and} \qquad \mathbf{A} = \begin{bmatrix} a & b \\ c & d \end{bmatrix}.$$

When we change the values of the entries of the matrix $\mathbf{A}$, what will happen to the behaviour of the system? One way to address this question is by looking at the type of attractor that obtains in the limit when $t \to \infty$. As it turns out, it entirely depends on the eigenvalues of the matrix $\mathbf{A}$, which are given by

$$\lambda = \frac{1}{2}\left(\text{Tr } \mathbf{A} - \sqrt{\text{Tr}^2\mathbf{A} - 4\det \mathbf{A}}\right),$$

where Tr $\mathbf{A}$ is the trace of the matrix $\mathbf{A}$ and $\det \mathbf{A}$ is its determinant.[36] Thus, the structure of the attractor to which the system tends as $t \to \infty$ is fully determined by the invariant quantities Tr $\mathbf{A}$ and $\det \mathbf{A}$. The whole situation can be summarized in a *bifurcation diagram*, as in figure 2.20. There, we see that there are six distinct possibilities for the structure of the attractor (sink, source, saddle, cycle, spiral sink, and spiral source). For instance, if Tr $\mathbf{A} > 0$, $\det \mathbf{A} > 0$, but $\text{Tr}^2\mathbf{A} > 4\det \mathbf{A}$, we have the rightmost case, namely, a source. Since the computation of those two invariant quantities is straightforward, it is not problematic to find out whether changes in a, b, c and d changes the qualitative behaviour of the system. To the extent that the limit behaviour of the system is of interest, it is thus justified to consider two systems in the same region of the bifurcation diagram to be equally good representations of a system. In fact, when this is the case, mathematicians call the system *topologically equivalent*, or topologically conjugate. Specifically, this notion of equivalence is based on continuously varying aspects of the models; two models are topologically conjugate if there is a homeomorphism

[36] The reason for which the eigenvalues of $\mathbf{A}$ are key is this. Using the eigenvalue decomposition $\mathbf{A} = \mathbf{Q}\mathbf{\Lambda}\mathbf{Q}^{-1}$, where $\mathbf{Q}$ is an orthogonal matrix of eigenvectors and $\mathbf{\Lambda}$ is a diagonal matrix of eigenvalues, we can write $\dot{\mathbf{x}} = \mathbf{A}\mathbf{x}$ as $\dot{\mathbf{x}} = \mathbf{Q}\mathbf{\Lambda}\mathbf{Q}^{-1}\mathbf{x}$. Making the substitution $\mathbf{y} = \mathbf{Q}^{-1}\mathbf{x}$ results in the *decoupled* system of equations $\dot{\mathbf{y}} = \mathbf{\Lambda}\mathbf{y}$, whose solutions are $y(t) = y_0 e^{\lambda t}$. Moreover, note that since those are straight-line solutions, either the largest λ will dominate the behaviour asymptotically, or there will be periodic solutions.

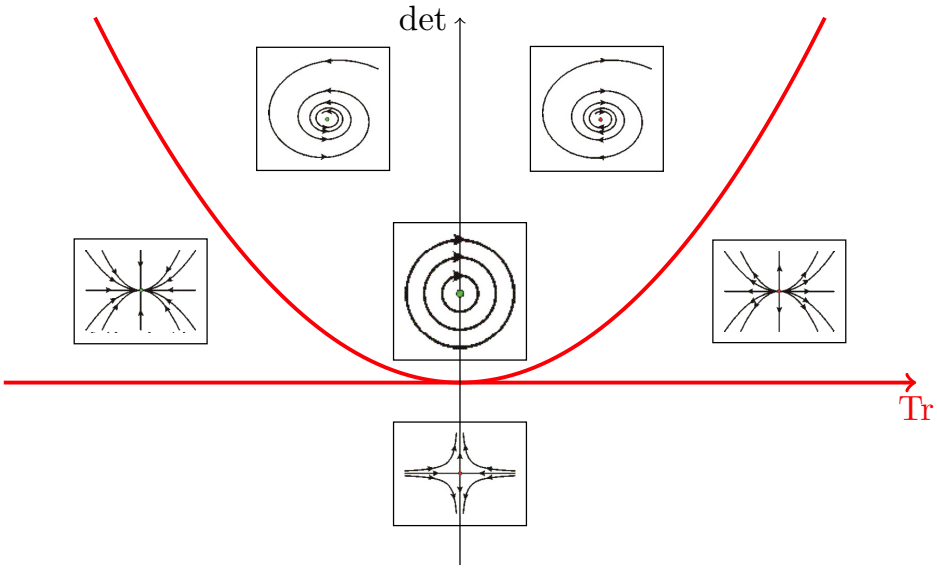


Figure 2.20: An example of bifurcation analysis that completely characterizes the qualitative behaviour of all possible two-dimensional linear systems of ordinary differential equations in terms of two invariant quantities (the trace and the determinant) only.

relating their solutions. There is a long tradition initiated by Poincaré (1892) and Birkhoff (1966) that uses the general theory of dynamical systems to qualitatively characterize the equivalence of systems in this way. It amounts to defining equivalence of systems in a parameter space, rather than by direct reference to the states of the system over a given time interval. Here again, the relevant notion of comparative 'goodness' of models should be understood as selective accuracy.

Many systems turn out to have some features that are insensitive to perturbations, while other are. Thus, even if the model is such that if there is any distortion, the information extracted about the states will be inaccurate, those distortion *can* be introduced to increase the manageability and tractability of the model concerning other properties. For instance, in the case of a chaotic system such as the Lorenz system (see figure 2.21), perturbation methods cannot accurately tell us what the states are, but they can very reliably tell us what is the dimension of the chaotic attractor, what its shape is, and the like.

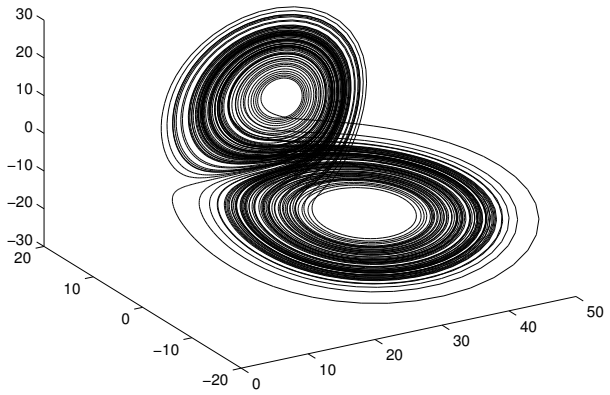


Figure 2.21: Phase portrait of the Lorenz system.

Those are properties insensitive to perturbations. As a result, it is crucial to bear in mind the questions we wish to answer and the behaviour of interest that must be understood in order to assess representations.

2.5.3 Universal Behaviour

The aspects of qualitative behaviour examined above fall under the theme that we examine in this subsection, namely, universal behaviour. At the same time, the study of universal behaviour is richer, since it is not purely qualitative. In a first sense, "universality" implies independence from initial conditions, *i.e.*, that the system will behave similarly under a broad range of initial conditions and perturbations, as above. This basic idea forms the basic theme of universal behaviour. In a second, deeper sense located at a higher level of abstraction, universality implies that many different systems of possibly quite different nature will reproduce identical quantitative behaviour in some region of a parameter space. The same basic theme is at work; however, we here consider a more abstract dynamical interaction between models of systems. Thus, in this second sense, universality is not only characteristic of a space of states, but of a space of systems.

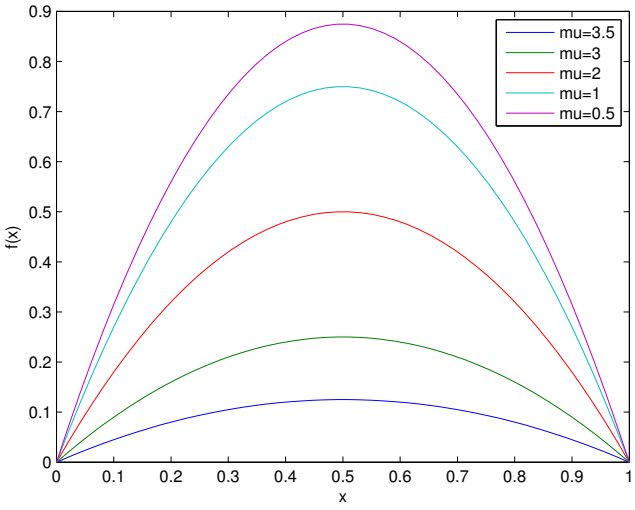


Figure 2.22: The logistic map for different values of the parameter μ.

To illustrate, let us use Feigenbaum's analysis of the "period-doubling route to chaos" as an example. We need to first discuss discrete dynamical systems to understand the setup of the argument. Consider the very typical logistic map:

$$x_{k+1} = f(x_k) = \mu x_k(1 - x_k)$$

where μ is some positive parameter (see figure 2.22). An equilibrium solution to a discrete time dynamical system is a point x^* such that $f(x^*) = x^*$. This solution, which is usually called "fixed point," is in general a function of the parameter μ. Here, the fixed points are $x_1^* = 0$ and $x_2^* = (\mu - 1)/\mu$. What happens away from equilibrium? In fact, if $|f'(x^*)| < 1$, the nearby points will *converge* to x^*, and they will otherwise *diverge* away from it.[37] So, if $\mu < 1$, $|f'(0)| < 1$ and all trajectories converge to $x^* = 0$. Moreover, we find that for

[37]This criterion works because if $|(x_{n+1} - x^*)/(x_n - x^*)| < 1$, points are getting closer to x^* as n increases. But, using a first-order approximation for f, we have

$$\left|\frac{x_{n+1} - x^*}{x_n - x^*}\right| = \left|\frac{f(x_n) - f(x^*)}{x_n - x^*}\right| = \left|\frac{f'(x^*)(x_n - x^*)}{x_n - a}\right| = |f'(x^*)|\,.$$

Thus, the fixed points are at the intersection of the curves $y = f(x)$ and $y = x$.

$1 < \mu < 3$, $x^* = 0$ becomes unstable and $x^* = (\mu - 1)/\mu$ is stable, and so attracts all trajectories. As μ becomes larger than the critical value $\mu = 3$, an interesting qualitative change takes place: the formerly stable fixed point becomes unstable, but is not replaced by another stable fixed point.

Following this transition, the logistic map will then exhibit periodicity. In this case, the motion will converge to a cycle that will oscillate between two points x_0^* and x_1^*). Since the period is two, applying f twice returns to the same point. This dependence can be expressed by

$$x_0^* = f(f(x_0^*)) \quad \text{and} \quad x_1^* = f(f(x_1^*)).$$

We say that f has a cycle of period 2, "2-cycle" for short. Consequently, we call the transition that takes place at the critical point $\mu = 3$ a period-doubling bifurcation.

When a period-doubling bifurcation takes place, the previously stable 2^n-point cycle becomes unstable, whereas a new 2^{n+1}-point cycle becomes a stable equilibrium. The first cycle is a fixed point of f^{2^n}, *i.e.*, of the function f composed with itself 2^n times. Similarly, the second cycle is the fixed point of $f^{2^{n+1}}$, *i.e.*, of the function f composed with itself 2^{n+1} times. It is easy to verify that $f^n(x)$ is a degree 2^n polynomial in x, and the key lies in the geometry of those polynomials. After $\mu = 3$, a period 2 orbit replaces the period 1 orbit. Since the points composing the 2-orbit are fixed points of f^2, and since the stability of fixed points is determined by the slope of a function at its fixed points, we must focus on the relation between the slope of f and the slope of f^2. The important aspect of this relation is as follows. Figure 2.23 depicts f and f^2 for different values of μ. Firstly, we observe that all the fixed points of f are also fixed points of f^2, since they intersect $y = x$ at exactly the same points. That this is the case is obvious, since $f^2(x^*) = f(f(x^*)) = f(x^*) = x^*$. We can see geometrically why a bifurcation occurs. As we can observe in figure 2.23, something crucial happens when $\mu = 3$. From $\mu = 2$ to $\mu = 3$, the slope of f as it crosses $y = x$ progressively increases from 0 to -1. At $\mu = 3$, the slope reaches one. Also, when $\mu > 3$, the slope is now smaller than -1, thus

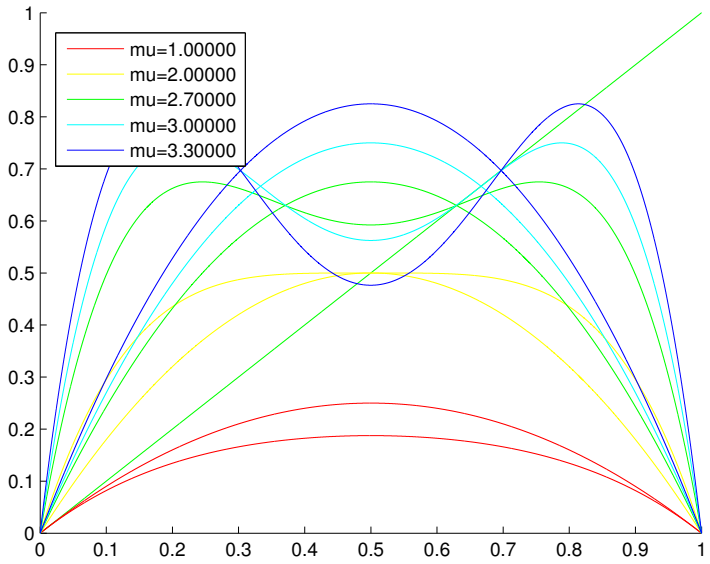


Figure 2.23: Geometrical features of the maps f and f^2.

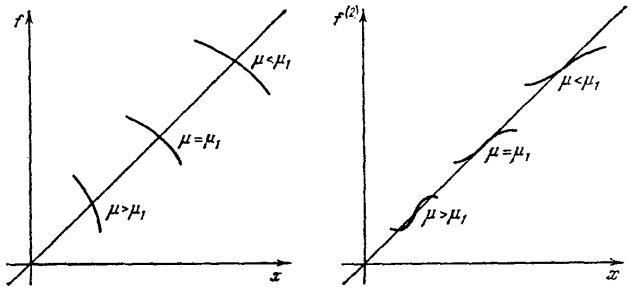


Figure 2.24: Relation between the slopes of f and f^2 (Vul *et al.*, 1984).

making the fixed point unstable. The period doubling is a consequence of the fact that as the slope of f^2 at the fixed point of f becomes larger than 1, two new intersection points of f^2 with $y = x$ are being created. Accordingly, f^2 now has *four* intersection points. The two intersection points it shares with f are unstable, but the remaining two are stable. The situation is summarized in figure 2.24. We thus have a clear geometrical explanation of why $\mu_2 = 3$ is a period-doubling bifurcation point.

Now, will the same relation hold for further period-doubling bifurcations? It turns out that the answer is 'yes.' In fact, it can be shown that this infinite

sequence converges to a finite value of μ, which is called μ_∞, that approximately equals $mu_\infty = 3.569934669\cdots$. Past this value, the system no longer exhibits periodicity; it rather exhibits chaos. Thus, there are values of μ for which no stable solution exists. Accordingly, the qualitative behavior of the dynamical system determined by the logistic map is described as a finite road to chaos through an infinity of period-doubling bifurcations. This qualitative behavior is captured by the structure of the bifurcation diagram of the logistic map (see figure 2.25(b)).

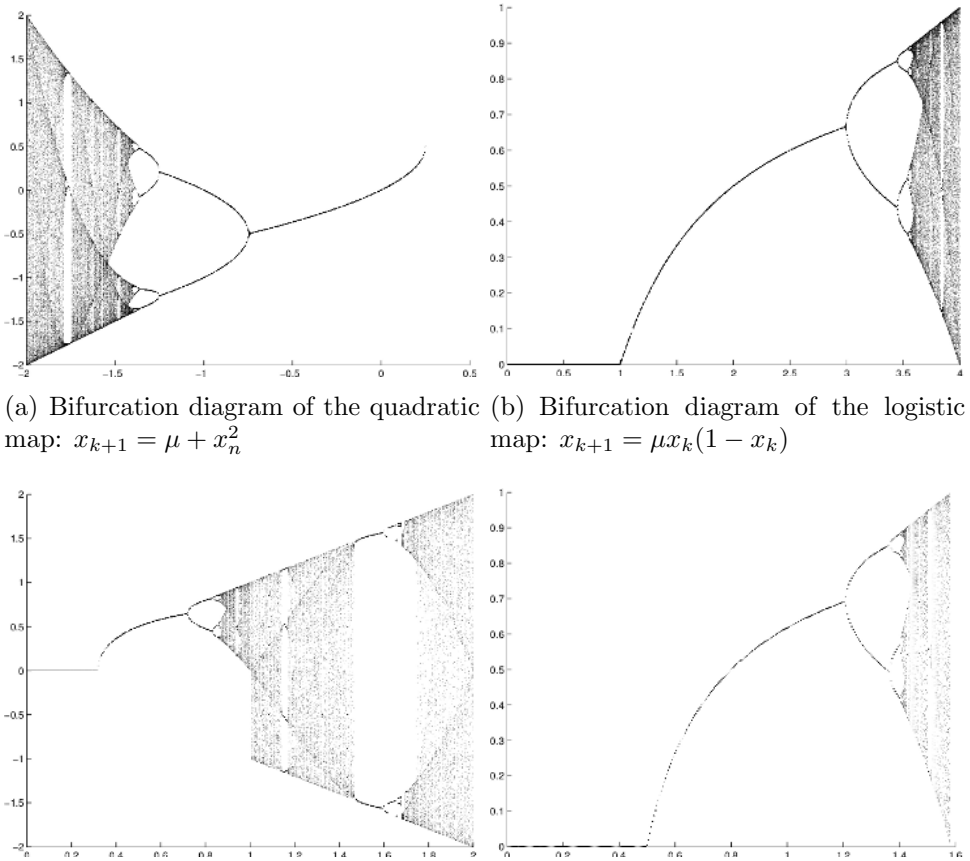
Universality comes to the forefront when we consider some other maps. In fact, consider the following maps:

- $x_{k+1} = \mu + x_n^2$;
- $x_{k+1} = -\mu x_k(x_k - 1)(x_k + 2)$;
- $x_{k+1} = \mu \sin \pi x_k$.

The expressions of those maps look quite different from the expression of the logistic map. However, they share an important feature: *their graph on* $[0, 1]$ *has a single "bump" upward*. Moreover, consider their bifurcation diagrams (see figure 2.25). Remarkably, we observe that they also undergo a finite road to chaos through an infinity of period-boudling bifurcations. It is an identical qualitative behavior shared by the four maps considered.

When we define a large *class* of functions which all share some behavior, we say that the behavior is *universal* for that class. For the behavior under consideration, *i.e.*, transition to chaos through an infinity of period-doubling bifurcations, it can be shown that any function with a "bump" will belong to the universality class. More precisely, the class of functions exhibiting this universal behavior is defined by the following conditions (Feigenbaum, 1978):

1. $f : [0, 1] \to \mathbb{R}$ is continuous, with a unique differentiable maximum $\bar{x}$;
2. $f(0) = f(1) = 0$, $f(x) > 0$ for $(0, 1)$ and f is strictly increasing on $(0, \bar{x})$ and strictly decreasing on $(\bar{x}, 1)$;



(a) Bifurcation diagram of the quadratic map: $x_{k+1} = \mu + x_n^2$

(b) Bifurcation diagram of the logistic map: $x_{k+1} = \mu x_k(1 - x_k)$

(c) Bifurcation diagram of a sinusoidal map: $x_{k+1} = \mu \sin \pi x_k$

(d) Bifurcation diagram of a 3^{rd} degree map: $x_{k+1} = -\mu x_k(x_k - 1)(x_k + 2)$

Figure 2.25: Bifurcation diagram of maps exhibiting period-doubling.

3. For some parameter value, f has two fixed points which are both unstable;

4. In the interval N about $\bar{x}$ such that $|f'(x)| < 1$, f is concave downward.

This example shows in a striking way that many details specific to the logistic map are really immaterial to its qualitative behavior. In fact, there are so many qualitative properties shared by all the functions of this class that one is brought to ask if there could not also be some *quantitative* features universally shared. Thanks to Feigenbaum's results, it is now known that the question can be answered in the affirmative.

The quantitative universality of some behavior for functions of very different forms is particularly striking. It is worth emphasizing that before Feigenbaum's discovery the possibility of quantitative universality was unsuspected.[38] For this discovery, Feigenbaum received the prestigious Wolf Prize in physics.[39] The quantitative universal property he identified is as follows. To begin with, let us denote the critical values at which the n^{th} period-doubling bifurcation occurs by μ_n. The first period-doubling bifurcation occurs at $\mu_1 = 3$, the second occurs at $\mu_2 = 1 + \sqrt{5}$, and so on. On this basis, we can define a quantity δ_n as follows:

$$\delta_n \equiv \frac{\mu_{n+1} - \mu_n}{\mu_{n+2} - \mu_{n+1}}.$$

The Feigenbaum constant δ is the limit as $n \to \infty$, and it takes the value

$$\delta = 4.669201609102990671853203821578\cdots .$$

[38] An anecdote recounted by Feigenbaum himself (Peitgen *et al.*, 1992 : Foreword) illustrates how unlikely such results were taken to be at the time: "[...] I had been directed to expound these results to one of the great mathematicians who is renowned for his results on dynamical systems. I spoke with him at the very end of 1976. I kept trying to tell him that there was a complete *quantitative* universality to these phenomena, and he equally often understood me to have duplicated some known *qualitative* results. Finally he said 'You mean to tell me these are metrical results?' And I said 'Yes.' 'Well, then you're wrong!' he asserted, and turned his back on me to terminate the conversation."

[39] On this occasion, the Wolf foundation declared that Feigenbaum's work "has opened up a whole new field of human endeavor [...] that quite transcends the traditional disciplinary boundaries. [...] It is hard to think of any other development in recent theoretical science that has had so broad an impact over so wide a range of fields, spanning both the very pure and the very applied." (Wolf Foundation, 1986)

The quantities δ_n characterize the rate of convergence of the many period-doubling paremeter values toward the accumulation value μ_∞. Yu (2009 : 316) emphasizes the importance of this constant, explaining that it is "a new mathematical constant, as basic to period-doubling as π is to circles." Moreover, there is a universal scale reduction parameter α such that

$$\alpha \equiv \lim_{n\to\infty} \frac{d_n}{d_{n+1}} = 2.5029078750958928\cdots,$$

where d_n is the distance between the critical point $x = 1/2$ and the nearest point in a the superstable cycle (that is, the cycle of points for which $f'(x) = 0$ at the intersection of $f(x)$ and $y = x$). The quantitatively universal behavior lies in the fact that all functions of the class defined above approach chaos at the same rate. The account of universality can also be given in deeper geometrical terms. In Feigenbaum's terms:

> Now, what is it that turns out to be universal? The answer, mostly, is a precise quantitative determination of the intrinsic geometry of the space upon which this marginal chaotic motion lives together with the full knowledge of how in the course of time this space is explored. Indeed, it was from the analysis of universality at the transition to chaos that we have come to recognize the precise mathematical object that fully furnishes the intrinsic geometry of these sort of spaces. (Peitgen *et al.*, 1992 : Foreword)

To really penetrate the significance of this result, we must understand its theoretical basis. This basis is a universal scaling theory that is connected to other important work by Kadanoff and Wilson on scaling in the context of renormalization, and which has been discussed primarily by Batterman (*e.g.*, 2002*b*, 2005, 2010*b*, 2011) in the philosophical literature. A sketch of its meaning will be drawn in what follows.

Renormalization is a technique that has been primarily developed in the context of statistical mechanics and quantum field theory. The main objective of this technique is to provide mathematically rigorous means to deal with theories that operate at very different scales. At different scales, the behavior of a system can be very different, thus leaving space for sometimes surprising disparities. In particular, renormalization has led to powerful re-

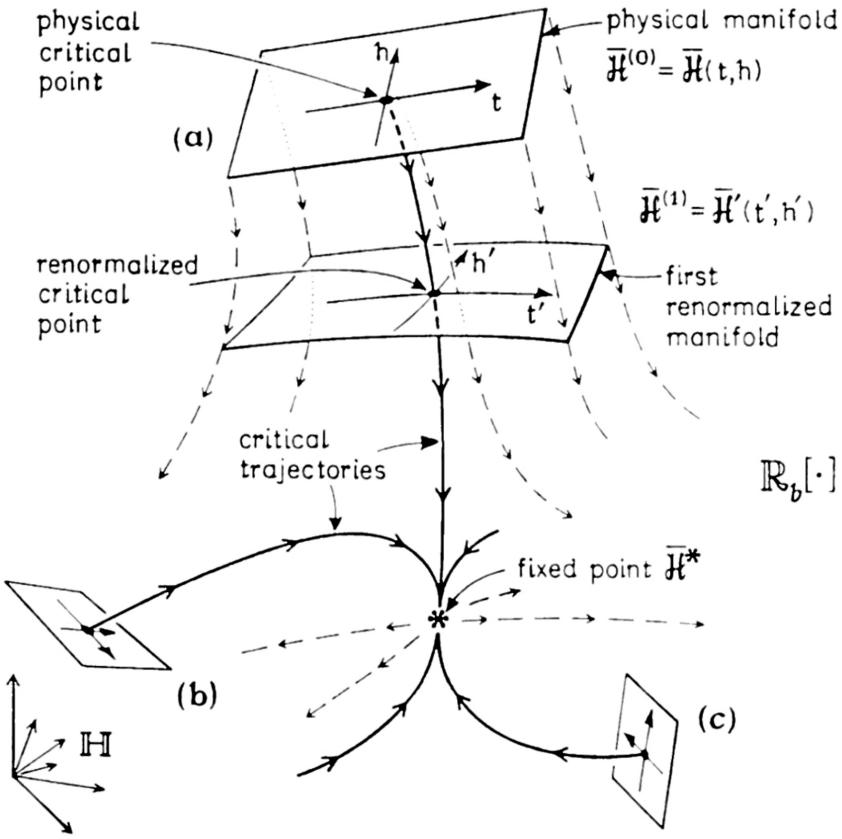


Figure 2.26: A depiction of the flow action of a renormalization operator in Hamiltonian mechanics (Fisher, 1998).

sults establishing classes of universality for physical systems near criticality. It is thus no surprise that renormalization is a central aspect of the mathematical story about universal behavior near transition to chaos in systems undergoing period-doubling bifurcations.

A renormalization is the action of an operator that universally characterizes the scale modification as a system undergoes a process.[40] A depiction of the flow action of a renormalization operator in Hamiltonian mechanics is in figure 2.26. In general, an analysis of a process *via* renormalization involves finding

[40]The concrete meaning of this general description will be unpacked below using the example under study in this section.

the given operator and examining its properties. In particular, finding the value μ_∞ will consist in solving the fixed point problem for this operator. Similarly, the problem of finding the limit of δ_n as $n \to \infty$ will amount to solving the fixed point problem for the operator acting on functions that double their periods. At the limit, we reach a fixed point since the transformation does not affect scale, *i.e.*, it is scale invariant.

How do we find the operator corresponding to period-doubling bifurcations? We have seen that the mechanism by which the function $f^{2^{n-1}}$ doubles its period at μ_n is the same as the mechanism by which the function f^{2^n} doubles its period at μ_{n+1}. As a result, there is a definite operator acting on functions that corresponds to period-doubling. Moreover, as we have seen, we only need to focus on the region around the maximum of f, *i.e.*, $1/2$ in the case of the logistic map. As n increases, the size of the relevant region decreases, *i.e.*, there is a scale reduction. The period-doubling operator results in a reduction of the distance between the maximum, $x = 1/2$, and the closest fixed point. This distance is the d_n used to defined α, the second Feigenbaum constant. So, when a function is acted upon by the period-doubling operator, it reproduces itself, but with a scale reduced by an α_n factor.

We can find a derivation of the renormalization operator for the period-doubling bifurcation sequence in Feigenbaum (1978), but I will rather present the simpler and shorter argument of Coppersmith (1999). As we have observed, the following scaling relation holds:

$$-\alpha \left(x_{2k} - \frac{1}{2} \right) = x_k - \frac{1}{2}$$

This relation simply captures the fact that the iterated values of x obtained with successive values of μ^s look the same under proper rescaling. Now, let $z_k = x_k - \frac{1}{2}$. This change of variable represents a translation of the system that centers it at the origin. Then, we can rewrite the above expression as $-\alpha z_{2k} = z_k$, which is equivalent to $-\alpha z_{2(k+1)} = z_{k+1}$. Note that it also implies $z_{2k} = -z_k/\alpha$. Now, since we are looking for an operator under the form of an iterated map, we let $z_{k+1} = g(z_k)$ and $z_{2(k+1)} = g^2(z_{2k})$. Hence, we obtain the

expression $-\alpha g(g(z_{2k})) = g(z_k)$, which can be rewritten as

$$-\alpha g(g(-\frac{z_k}{\alpha})) = g(z_k).$$

This expression is the condition that the renormalization operator must satisfy. Coppersmith (1999) calls this equation the *renormalization group equation*. How do we solve this equation to find g, α and δ? The standard method for finding g and α consists in expanding g in a Taylor series about $z = 0$ (which is the translated maximum), while the method for finding δ is based on functional eigenvalues. However, I will not go in those details here.

Rather, note that with a knowledge of α and δ, we can make reliable predictions concerning the location of bifurcation points as the scale changes. For instance, if we have the end value of the cycle for μ_n, we can project with elementary arithmetic only that the end value for μ_{n+1} will be given by

$$f^{2^n}_{\mu^s_{n+1}}\left(\frac{1}{2}\right) = \frac{1}{2} - \frac{f^{2^{n-1}}_{\mu^s_n}\left(\frac{1}{2}\right) - \frac{1}{2}}{\alpha}.$$

Similarly, if we have the first two superstable values of μ, or the first two bifurcation values of μ, we can find all the others by using the recurrence

$$\mu_{n+2} = \mu_{n+1} + \frac{\mu_{n+1} - \mu_n}{\delta}.$$

As I have argued, knowing where bifurcations occur is key to assessing how good a representation is, in the sense of selective accuracy. Thus, the quantitative universality revealed by renormalization group methods tells us much about whether our models are good, or rather, where in a space of parameter they can be confidently believed to be good.

2.6 Summary

In summary, this chapter has made the following points:

C2.1 Mathematical modelling involves the use of procedural model construc-

tion recipes to derive model equations from modelling assumptions within a theory. Importantly, the inferences from modelling assumptions to model equations are non-monotonic. It also involves the computationally challenging task of solving the model equations to obtain an expression of the temporal evolution of the states of a system.

C2.2 In addition, mathematical modelling of real systems involves the delicate task of selecting modelling assumptions. It cannot be simply argued that the more complete and accurate the set of modelling assumptions is, the better the model is; rather, one should seek a balance between the completeness and accuracy of the modelling assumptions and the tractability of the model equations. This introduces a pragmatic dimension governed by information management strategies.

C2.3 One can identify at least six senses in which a mathematical representation can be said to be good. I argued that the assessment of the representational virtues of mathematical models is made with respect to the model equations, not the modelling assumptions. Moreover, because models are sought to understand some aspects of a system, *i.e.*, the behaviour of interest, the notion of selective accuracy best captures the condition under which a model represents well.

C2.4 In many cases, precise quantitative information about the states of a system on a time interval is not required to assess a model. In this case, criteria to determine the comparative accuracy of models are based on perturbation methods that provide qualitative and quantitative information on what happens in systems. The notion of bifurcation provides a mathematical indicator of when information obtained by idealizing, simplifying, or perturbing the system can be utilized as reliably as if it was exact.

As we see, the perspective provided here on the logic of modelling is very different from the standard reconstruction of the role of mathematics in the natural sciences, *e.g.*, the hypothetico-deductive model of scientific methodol-

ogy. It emphasizes the importance of pragmatic considerations and explains that rigour nonetheless remains thanks to the use of semantical tools characterized in terms of perturbation, not satisfaction.

There remains important aspects of the logic of modelling to discuss. Chapter 3 and chapter 4 will discuss three of them. We have seen that models are built within theories; section 3.1 will discuss more systematically the contributions of those underlying theories to mathematical modelling. Section 3.2 will articulate the concepts of error in the context of scientific experimentation. As we will see, mathematical strategies based on perturbations devised to epistemologically track our grasp of systems is also at the heart of the theory of measurement. Finally, chapter 4 will examine the situation in which selective accuracy cannot be evaluated only on the basis on qualitative information. There, I will articulate the way in which the same perturbation methods are essential to extract numerical figures to characterize systems.

Chapter 3

Model, Theories, and Experimentation

In this chapter, we examine two themes that are related to the discussion of the logic of mathematical modelling from chapter 2. The first theme—examined in section 3.1—concerns the interplay between theory and models. We have seen that the construction of a model takes place within a theory. More precisely, on the basis of a set of modelling assumptions, we use some theoretical principles to derive model equations. The general existential form of modelling assumptions has been mentioned, and examples of theoretical principles have been given. Here, I will give more details and explain the different types of mathematical principles that enter in the model construction recipes. Moreover, I will discuss the advantages and disadvantages of some theoretical approaches in relation to modelling practices, by addressing issues related to the different scales at which theories operate. These issues of scale are meant to complement the discussion of minimal models from the previous chapter. Finally, I will compare this approach to the problem of the structure of theories with other influential philosophical approaches, and explain why it contributes to understanding the (hopefully, at this point less unreasonable) effectiveness of mathematics in the natural sciences.

The second theme—examined in section 3.2—concerns the empirical content of theories and models. I will use technical scientific literature to outline

the epistemological aspects of the theory of measurements, and explain that a sound understanding of the problems encountered in experimentation impose a certain type of mathematical analysis of models that will become important in the next chapter drawing lessons from numerical mathematics. In particular, I will explain the motivation for mathematical rules based on significant figures, as well as some of their limitations. On this basis, we will be ready for a more systematic discussion of the various types of errors and how they relate to each other.

3.1 Model and Theories

In this section, we further examine the relation between theories and models, with the objective of better characterizing the logic of mathematical modelling. As we have seen, a model is constructed within a theory. Thus, it is important to understand the way in which the structure of a theory and the kind of theory employed has an impact on the construction and evaluation of models.

3.1.1 Scale, Tractability, and Levels of Theories

There are multiple respects in which scale deeply matters in order to get a proper understanding of mathematical modelling in science. The first and most common set of circumstances with respect to which scale is discussed concerns the possibility of a certain type of system to exist at a given scale. In his classical essay *On Being the Right Size*, Haldane (1928) discusses many such cases. For example, could small animals like mice have a daily food consumption *dfc* comparable (in terms of ratio of their weight) to that of humans? No, because this ratio is proportional to mass, whereas the required daily caloric intake *dci* for small animal is mostly based on the amount of heat they dissipate, which is proportional to their surface. Assuming a certain density for such animals, we would say that $dfc \propto L^3$ whereas $dci \propto L^2$. Thus, by dimensional analysis, we see that we cannot make arbitrary length changes and preserve the same *dfc*. Perhaps the most famous example of this sort in

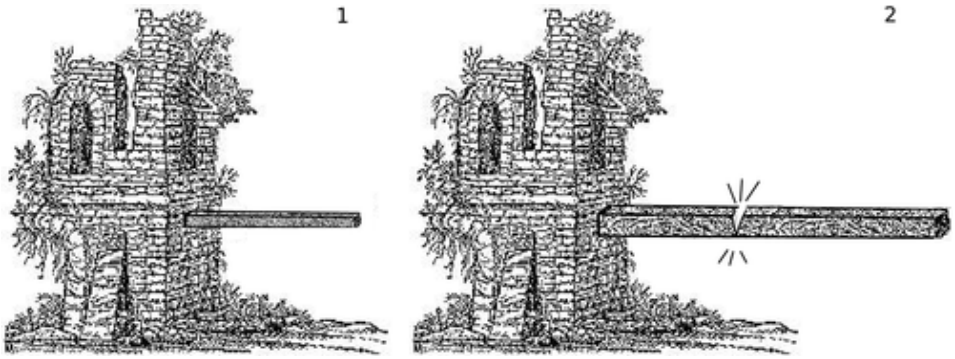


Figure 3.1: Scaling of a beam. One cannot just increase size of the beam while preserving the geometrical aspect ratio and expect things not to break. Image from `lightandmatter.com`.

physics is Galileo's square-cube law. If we consider a beam such as the one in figure 3.1, it is not always possible to double its size. This is because the mass of the beam is proportional to its volume (dimension L^3) whereas the breaking strength of the material is proportional to the the cross-sectional area (dimension L^2).[1] As a result, if we preserve the density of the beam and its geometrical aspect, but double its length, we are changing the ratio $L^2 : L^3$ of area to volume. Past a certain critical ratio, the beam will simply break under its own weight. *Mutatis mutandis*, we could argue that the stories about giants are impossible, since they could not walk without crumbling apart. Given that their constitution and aspect ratio is supposed to be as a normal human, the strength of their bones would increase in proportion to their cross-sectional areas, but the forces applied on the bones would increases in proportion to his volume (*i.e.*, mass by density). This sort of considerations reveals that scaling is not a symmetry of nature.

Scaling in this sense enters in our deliberation regarding the quality of a model. If our model predicts that the beam does not break, but it does, then it is an assessment failure. If we consider the analysis of simple machines, the fact that they do not depend on actual lengths and scales but only on the geometric configuration shows that scaling considerations have been idealized

[1] It also depends on the shape of the cross-sectional area, but that does not matter here.

away, which is problematic given the kind of questions considered in section 2.2.

However, there is another completely different set of circumstances that makes scaling extremely important for a proper understanding of the logic of mathematical modelling, namely, the relation between scale and mathematical tractability of model equations. This comes in two flavours. Firstly, the scale or size of parameters appearing in equations has consequences for the reliability of the solutions obtained by numerical methods. When parameters having values of widely different orders of magnitudes appear in some set of equations, there are important risks that the numerical answers will be wrong. We will return to this problem in chapter 4. Secondly, it is important to construct a model within a theory at the right scale in order to obtain tractable equations. We will further examine this aspect of the situation here.

To begin with, there are many types of theories in science. A common distinction between fundamental and phenomenological theories is often made. This terminology, however, seems to support the idea that fundamental theories are the "real thing," and that phenomenological theories are only used due to a temporary state of ignorance of the principles that would enable us to reduce it to a fundamental theory. For instance, Einstein (1919) claimed that when "we say that we have succeeded in understanding a group of natural processes, we invariably mean that a constructive [*i.e.*, fundamental] theory has been found which covers the processes in question." This attitude is widespread, and by no means restricted to Einstein. In what follows, I will argue that this claim relies on a caricatural view of science. Instead, phenomenological theories, too, are fundamental in physics, despite the fact that they do not explain processes by referring to elementary particles. The reasons for which there are phenomenological theories are of two kinds: practical and theoretical. Either way, their role is to allow engineers and applied mathematicians to actually solve problems and construct models. For these scientists, the only justification required to use phenomenological theories is that their concerns are completely legitimate, and that they can only satisfy them by means of phenomenological theories. Thus, to maintain neutrality, I

will use the phrase "structural theory" for what is usually called "fundamental theory."[2]

The main characteristic of the phenomenological perspective, as opposed to the structural one, is that its theories do not rely on our mathematical understanding of the physics of elementary particles. Accordingly, it does not require that we derive equations describing the behaviour of interest from laws governing particles. The paradigm of this approach in physics is the formulation of theories in terms of fields only:

> [...] we may construct a direct theory of the *continuous field*, infinitely divisible without losing any of its defining properties. The field may be the seat of motion, matter, force, energy, and electromagnetism. Statements in terms of the field concept are called *phenomenological*, because they represent the immediate phenomena of experience, not attempting to explain them in terms of corpuscles or other inferred [or hypothesized] quantities. (Truesdell, 1960 : 22)

The first step to appreciating the phenomenological perspective is to consider what a theory is supposed to do. A theory in physics is meant to give us the means to construct mathematical models of some aspects of nature that will allow us to understand the processes and/or phenomena under study. As such, they must isolate the essential physics and leave away what is merely incidental. Truesdell (1980 : 72) makes the same point forcefully:

> One good theory extracts and exaggerates some facets of the truth. Another good theory may idealize other facets. A theory cannot duplicate nature, for if it did so in all respects, it would be isomorphic to nature itself and hence useless, a mere repetition of all complexity which nature presents to us, that very complexity we frame theories to penetrate and set aside.

As I argued in chapter 2, incidental information only impedes tractability and manageability of mathematical representations.

For a given level of complexity, the amount of information that a theory contains about a given material is inversely proportional to the amount of information it contains about a class of material. Structural theories include more information about singular materials and, as a consequence, less information about a *class* of materials. For example, the dependence of a macroscopic

[2] It is Truesdell's phrase. See Truesdell (1984).

variable such as viscosity on temperature could be predicted by a kinetic theory. But in this case, for each specific instances of the laws of intermolecular force, the explanation offered by the structural theory would differ. Due to complexity, there are many cases where the solution to the force equations would be mathematically intractable. Under a phenomenological theory, on the other hand, such a dependence would be ignored, *i.e.*, the theory would be less definite regarding the relation between a macroscopic variable and its molecular support. This is why it would apply more broadly, to many different physical systems.

Phenomenological theories abstract certain aspects of physical systems, and structural theories abstract other aspects. As a result, there *prima facie* is a tension between the two theoretical approaches. But for mathematical modellers, the tension is rather benign. It only indicates the fact that the proper level of theory should be chosen depending on what the behaviour of interest is. It is a matter of information management. Again, if a theory were not simpler than the system that it is designed to model, it would serve no purpose whatsoever. It is meant to picture the system in a way such that the aspects of interest are emphasized while others are omitted.

But the tension becomes sharper when a philosophical analysis is performed in order to establish what the "ontology" of a theory is. The metaphysical concerns that a philosopher might have about theories is that it is allegedly ontologically committed to what kinds of things *really* exist, and to what the ultimate true principles governing nature are. However, we happen to know that it is false to claim that macroscopic bodies are continuous blobs of matter; they are composed of particles. Thus, it would seem that the truly fundamental theories are the structural ones.

Which of these two perspectives is right? A minimally committing response would be to emphasize that this question amounts to asking "which set of concerns is right?" and to emphasize that different academics are free to concern themselves with whatever they want. Along this line, we would conclude that different theoretical perspectives are to count as fundamental depending on which concerns one has. I think it is the right answer, but given the current

state of the literature in philosophy of science, one must add a proviso that has repercussions for what philosophy of science is supposed to be. It is one thing to be concerned with metaphysical questions, but it is another to claim that the very successful actual practice of scientists is driven or constrained by those questions. However, it is not uncommon in the literature to encounter the latter claim, especially by adherents of 'scientific realism.' Often, this way of thinking about science originates in the apparently harmless belief that science is primarily in the business of discovering ultimate truths. To make the claim tenable, a distinction between science proper and technology is typically invoked. Science, properly understood, is after truth, and as a result is driven or constrained by metaphysical questions; thus the scientifically fundamental theories really are, after all, the fundamental ones. This leads to a view of science which is entirely detached from the pragmatic concerns that I emphasized in chapter 2.

This is an oversimplification of the practice of scientific modelling and theorizing. A good succinct, yet suggestive reply has been given by Truesdell (1980 : 72-3):

> [. . .] if we would analyze the stagnation of traffic in the streets, to take into account the behaviour of the elementary particles that make up the engine, the body, the tires, and the driver of each automobile, however "fundamental" the physicists like to call those particles, would be useless even if it were not insuperably difficult. The quantum theory of individual particles is not wrong in studies of deformation of large samples of air; it is simply a model for something else, something irrelevant to matter in gross.
>
> With this sober and critical understanding of what a theory is, we need not see any philosophical conflict between two theories, one of which represents a gas as a plenum, the other as a numerous assembly of punctual masses. (Truesdell, 1980 : 72-3)

This is key to the inter-theoretic relationships between structural and phenomenological theories. To understand the behavior of bodies as they are met in real life, a corpuscular model would be of no use to a scientist. Thus, *contra* the naive view described above, we should agree with Truesdell & Noll (1965 : 2-3):

> Pedantry and sectarianism aside, the aim of theoretical physics is to construct

> mathematical models such as to enable us, from use of knowledge gathered in a few observations, to predict by logical processes the outcomes in many other circumstances. Any logically sound theory satisfying this condition is a good theory, whether or not it be derived from "ultimate" or "fundamental" truth. It is as ridiculous to deride continuum physics because it is not obtained from nuclear physics as it would be to reproach it with lack of foundation in the Bible. The conceptual success of the classical linear or infinitesimal field theories is perhaps the broadest we know in science: In terms of them we face, "explain", and in varying amount control, our daily environment: wind and tides, earthquakes and sounds, structures and mechanisms, sailing and flying, heat and light.

To the extent that our objective is to *rationally reconstruct* scientific methodology, this is the proper attitude. A proper understanding of *pure* science can only result from an understanding of its applications, and this implies integrating the pragmatic concerns and methods more readily associated with engineering. This, however, undermines the distinction between science proper and technology, and also mitigates the claim that science is after ultimate truths because it is driven by metaphysical questions.

Therefore, even if models of systems are constructed *within* a theory, many essential features of the logic of modelling are quite independent of the choice of theories, or even of our understanding of what a theory is. That is, many essential features of the logic of modelling are independent of our philosophical understanding of the structure of theories. Rather, our discussion of the selection of modelling assumptions as seeking a balance between accuracy, completeness, and tractability is reproduced in a discussion of the choice of a theory in which to construct models:

1. focussing on irrelevant details reduces tractability;
2. identifying dominant factors is key to understanding the essential physics;
3. determining the behaviour of interest is a prerequisite for assessing a mathematical representation.

Consequently, phenomenological approaches will often be favourable because they are the ones that will give us a tractable and sufficiently accurate representation of a real system.

3.1.2 The Structure of Theories: Characterizing Laws and Other Mathematical Principles

One of the most important and difficult problem in the philosophy of science is to characterize what a scientific theory is. The difficulty of this problem is that philosophical discussions require the characterization to be very fine-grained, because certain views of what scientific theories are may have significant repercussions for other philosophical problems, such as the problem of demarcation, the problem of confirmation, the problem of theory choice, *etc.* In the same vein, it might contribute to an explanation that the unreasonable effectiveness of mathematics is reasonable after all. Clearly, an inaccurate or over-simplistic perspective on scientific theories will result in misunderstanding key aspects of scientific methodology. It can even lead to claims that some aspect of our successful use of theories cannot be understood—which is, I argue, what happens with the problem of uncanny accuracy.

Two influential views on this problem are the syntactic view and the semantic view. The former view is that a scientific theory is an axiomatized formal system together with some correspondence rules for their interpretation. Here, a theory is a deductively closed set of interpreted sentences. The latter view is that a scientific theory is not a linguistic entity, but rather a semantic one, thus eschewing the meta-theoretical complications related to the syntactic view. In other words, a scientific theory is a collection of models satisfying certain defining properties. As this brief summary shows, the two classical views of theories focus very much on what theories *are*; but given that we are interested with the logic of mathematical modelling and how it helps to explain the unreasonable effectiveness of mathematics, we must in addition focus on what theories *do* and *how they do it*. In particular, it will be essential to distinguish types of mathematical principles and determine their respective roles in mathematical modelling. That is, characterizing the structure of theories serve us in distinguishing the *types* of ingredients used in model construction recipes. To meet this objective, I look at the structure of a theory that is thoroughly grounded in applications, namely, classical con-

tinuum mechanics. As it turns out, examining continuum mechanics has the advantage that the physicists and applied mathematicians who have worked on its axiomatization have already made the important distinctions between the types of mathematical principles involved.

There are two main elements constituting the structure of continuum mechanics:[3]

(I) general principles, often referred to as field equations;

(II) constitutive equations, sometimes referred to as specializing relations.

Let us briefly describe them respectively. The most important property of the *general principles* is that they are *common to all media*. Thus, they are treated as genuinely universal claims. They determine the general mathematical structure that is used to describe motion, deformation, flow, *etc.* They are sometimes called "field equations of balance," but they are best known as *conservation laws*. The axioms of continuum mechanics usually state six conservation principles: conservation of *mass, linear momentum, moment of momentum, energy, electric charge*, and *magnetic flux*. Truesdell & Noll (1965 : 2) mention that there must be a seventh one, namely a principle of *irreversibility*, expressed in terms of the *entropy*, but that this law is not known in its general form.

As an example, take the law of conservation of linear momentum. This general principle tells us that if no external force acts on a system, then the rate of change of linear momentum over time is null, *i.e.*, linear momentum is constant. Such principles, formulated in terms of differential equations in the time variable t, are fundamental to the description of the behaviour of physical systems. Taken together, with a model of space-time, they form the mathematical structure that applies universally to all bodies in any circumstance,[4] and they are the proper subject of the branch of mathematics known

[3]The distinction between the two, despite its being ill-known among philosophers of science, is said to go as far in the history of mechanics as Bernoulli (James), Euler, and Cauchy (Truesdell & Noll, 1965; Wilson, 1998).

[4]A remark is in order. The mathematical structure is universal in the sense that it is

as *kinematics*. To make them operationally effective, however, we need other ingredients.

The second element is *constitutive equations*. The general principles, in themselves, are not sufficient to determine the motion, deformation, *etc.*, of bodies in a system. In order to formulate a determinate problem, it is required to specify body forces (*e.g.*, universal gravitation) and the kind of material on which the general principles and the body forces apply. The specification of a material (or of many different materials) is made by means of constitutive equations. An example of such a constitutive equation is Hooke's law, $\mathbf{F} = -k\mathbf{x}$. Hooke's law is an elasticity condition which characterizes classes of materials. Notice here that the law is not a differential equation, and that it does not contain a time variable. The constitutive equation would be different in problems involving inviscid fluids, viscous fluids, rigid bodies, elastic bodies, *etc.* Despite the fact that these equations are often labeled "laws," it must be mentioned that the name is somewhat inappropriate, especially given the difference between their status and the status of the general principles:

> The term "law" for specializing relations is mightily unfortunate, especially since most such "laws" are regarded now as being "approximate", honored by nature only in the breach by greater or lesser amounts; modern studies of the foundations of classical physics use *constitutive relation* to denote specializing hypotheses intended to model ideally the response of natural substances [...]. (Truesdell, 1981 : 562)

There is another characteristic of constitutive equations that make one doubt whether they are proper laws: these statements cannot be universal laws of nature, since they contradict one another. Because they define ideal materials, they must not be expected to all give us the same results. Moreover, it is important to understand that they are *not* a consequence of the general principles:

> There is no reason *a priori* why either should ever be physically valid, but it is an empirical fact, established by more than a century of test and comparison, that each does indeed represent much of the mechanical behaviour

treated as if it were. No particular constraints on its application is suggested by the theory. However, this is strictly true only insofar as we are dealing with classical (non-quantum) systems, in non-general relativistic spacetime.

> of many natural substances of the most various origin, distribution, touch, colour, sound, taste, smell, and molecular constitution. Neither represents all the attributes, or suffices even to predict all the mathematical behaviour, of any one natural material. (Truesdell & Noll, 1965 : 2)

Given the infinite number of constitutive equations available in continuum mechanics, virtually any aspect of natural bodies can be represented by a constitutive equation. It can thus be doubted that they are nomic statements.

Moreover, their application is more similar to definitions than to laws:

> But, what is interesting is that, surprisingly, the very engineers who apply these laws take them to be *definitions* of materials. [...] Thus, satisfaction of Hooke's linear relation between stress and strain counts as the lone criterion for being a Hookean elastic solid. Hooke's law should be read, then, as "if the material is a Hookean elastic solid, then it will have a linear relationship between its stress and its strain." How do we know if something is a Hookean elastic solid? We check to see if it has a linear relation between its stress and its strain. What does one say, then, about materials that do not obey the relation implied by Hooke's law? One merely say that they are not Hookean elastic solids and tries to figure out what the heck they are for the sake of modelling them. Thus, constitutive equations are generally taken by the very people who apply them to be empty tautologies. (Smith, 2002 : 255)

There *seems* to be an *ad hocness* to this way of relating the equations to data. But it is how problems are fixed in continuum mechanics: no problem is completely determined analytically, *a priori*. The general principles form an underdetermined system, insufficient to yield specific answers unless further equations are supplied. We must add specializing relations, and this role is fulfilled by the constitutive equations.

In terms of the model construction recipes described in chapter 2, we can characterize the role of conservation laws and constitutive equations. Remember that a model construction recipe is a procedure to derive model equations from modelling assumptions. Modelling assumptions in continuum mechanics are of the form "there exists a body B occupying a certain region R" and "there is a force F acting on body B." The former type of modelling assumptions specify *initial conditions* and *boundary conditions*. The latter type of modelling assumptions specify the constitutive equations, *i.e.*, what type of body a certain body B is. If we take a statement such as "there is a Hookean

elastic solid attaching masses A and B, which are 1m apart," we see that it is a combination of the two forms of modelling assumptions.

The model equations that result from the construction of a model are of the form $\dot{x}(t) = f(t, x)$, $x(t_{k+1}) = f(x_k)$, *etc.*, *i.e.*, they are equations of the states of the system with respect to time which, provided a solution exist, dictate the temporal evolution of a system. In this context, the role of the universal laws is to allow us to connect the constitutive equations together to obtain a model equation. For instance, we could connect together an initial condition $x(0) = x_0$ and a constitutive equation $\mathbf{F} = -k\mathbf{x}$ with the universal law $\mathbf{F} = m\mathbf{a}$ to obtain the dynamical model equation $m\ddot{\mathbf{x}} = -k\mathbf{x}$. Together, they lead to a definite problem, with a definite and unique solution. This characterization is thus seen to be in harmony with the model construction recipes discussed.

However, some common characterizations of aspects of the methodology of science do not harmoniously fit this picture. An example is the old but still influential model of explanation proposed by Hempel & Oppenheim (1948):

$$\frac{C_1, C_2, \ldots, C_m \qquad L_1, L_2, \ldots, L_n}{E}$$

where $C_1, C_2, \ldots, C_m$ are initial conditions and $L_1, L_2, \ldots, L_n$ are laws. If we take the schema as is, it leaves many important things unspecified. Typically, the laws $L_1, L_2, \ldots, L_n$ are taken to be empirical regularities (some authors in addition require that the laws in question have some modal properties, *e.g.*, counterfactual robustness). An empirical regularity is the temporal succession of some occurrent property. However, in our discussion above, we have seen that the only type of mathematical principles susceptible of stating a temporal succession of events are the solutions of model equations, not assumptions or laws fed into model construction recipes. To the extent that the laws are ingredients used as hypotheses to derive phenomena, they must be either constitutive equations or general principles. However, neither of them has the required mathematical form to be called an empirical regularity. On the other

hand, if we disregard for a moment the generally accepted view that the laws $L_1, L_2, \ldots, L_n$ are regularities, we see that the hypothetico-deductive schema would have to be enriched in order to correctly capture how laws are used in science. In fact, it would be necessary to mention that the list of laws would need to contain at least one general principle, and at least one constitutive equation. Doing this, however, would bring us back to the discussion of chapter 2, which is in essence very different from where the discussions based on this model typically go; since we have shown that unless there is already a reason to believe that we have selected the right modelling assumptions, we are not in a position to assess the quality of the model.

Moreover, this model assumes that we have effective means to logically decide whether E follows from the premises, which is generally not the case in modelling contexts. In contradistinction, our reconstruction emphasized the importance of seeking a balance between the accuracy and completeness of the modelling assumptions and the tractability of model equations, without which there is no guarantee that we can determine exactly what the consequences of the model equations are. However, if a philosophical view on mathematical modelling and theorizing does not have the resources necessary to capture understand the effective extraction of information from model equations, then it does not account for the success of science. By focussing on the crisp formal schema that could be used to reconstruct aspects of scientific methodology and by focussing on the truth of the modelling assumptions, much of contemporary philosophy of science thus fails to properly capture what makes science successful.

3.2 Experimental error and the mathematics of uncertainty

In this section, we turn to a different theme. We will examine the fundamental concepts of the theory of measurements, paying specific attention to its semantic and epistemological dimensions. As we will see, the articulation of

the semantic and epistemological dimensions is essentially the same as when we are dealing with errors that do not stem from experimentation. In both cases, it is essential to think about the validity of mathematical models of real systems in relation to the analysis of perturbations. Moreover, preparing the grounds for the next chapter, we will see that the theory of measurement already suggests that we use a kind of mathematics that operates on quantities containing uncertainty. This will be articulated in terms of relations between exact mathematical operations and modified operations. These themes will be carried to the analysis of numerical solutions in chapter 4.

3.2.1 Accuracy, Error, and Uncertainty of Measurement

> Errors using inadequate data are much less than those using no data at all.
>
> Charles Babbage

As we have seen in section 2.2, the construction of a model susceptible of generating prediction (or retrodiction) and explanation of the behaviour of a system requires the specification of the value for some parameters. Those parameters may be the primitive quantities in which the states of the system are expressed,[5] or some other derived quantity.[6] In either case, the value of this parameter will be supplied on the basis of *measurements*. Accordingly, a topic which is central to any serious discussion of the epistemological and semantical dimensions of scientific modelling is the theory of measurement.

Interestingly, what philosophers call 'theory of measurement' fails to properly characterize the central epistemological and semantic problems of experimentation in the same way that the more standard accounts of scientific modelling and theorizing fail to properly characterize them with respect to the more theoretical part of science.[7] In contrast, I will introduce some elemen-

[5] Examples include position, momentum, temperature, pressure, spin, *etc*, as the context may be.

[6] Very often, it will be a parameter describing the "bulk behaviour" of some material.

[7] Appendix A briefly discusses the state of the philosophical literature concerned with the theory of measurement.

tary aspects of the theory of measurement as scientists reconstructed it for themselves, and follow with a discussion of the mathematical aspects of modelling that have roots in measurement.

No scientist would argue with the fact that all experimental data has some degree of imperfection, in that experimental results always contain errors.[8] As a result, numerical values gathered in experiments are always likely to be wrong (*i.e.*, inexact). This, however, does not imply that the values reported are bad, for they may convey entirely satisfactory information. To ensure that inexact values reported are informative, scientists have to diagnose the possible sources of measurement error and must try to design an experimental setup that will ensure that the error is minimized (or satisfactorily small, given what is already known).

Thus, the role of measurements of parameters is to determine (1) *a* value of the parameter and (2) an estimate of the uncertainty associated with the measurement. The central concepts involved in a theory of measurement are thus the concepts of error, uncertainty, and, as we will see later, propagation of error and uncertainty. From this point of view, the philosophical dimension of this task revolves around two problemns. The central *semantic problem* of a theory of measurement is

> How close to the "true" value of the parameter is the measured value?

and the central epistemological problem is

> Given that there is always uncertainty about the error, when should (most likely wrong) measured values be considered good estimates of the value of the measurand?

The epistemological and semantic problems related to errors in measurements are, as we see, essentially the same as those discussed in chapter 2 in relation to the logic of modelling. The difference is not in the problems and concepts

[8] Sometimes, in combination with theoretical consideration, the exact value of some parameter might be inferred from some measurements; for instance the value of the exponent in the inverse-square law (Harper, 1998, 2012). However, that this is the case is shown within the context to be described below, and does not contradict it.

per se, but rather in the stage of model construction at which they come forth. Moreover, as we will see in chapter 4, the same thing can be said about numerical errors (perhaps surprisingly, for some readers).

Let us begin our discussion of the concepts related to measurement. Many aspects of the discussion will sound familiar to whomever has taken high school science (physics, chemistry, and biology). Perhaps because of its simplicity, this material is rarely included in works on philosophy of science. However, because the topic contains subtle points, I will not eschew its discussion. The points emphasized will make clear that the approach to characterizing scientific methodology that I have articulated concerning the logic of modelling also applies to the study of the empirical basis of mathematical expressions.

The discussion to follow is based on the so-called "GUM approach" to the theory of measurement.[9] Pressed by a necessity to have rigorous standards for the discussion of measurement, various organizations dedicated to providing practical guidance to working scientists have collaborated to establish a cohesive approach. This work is surprisingly recent; it has only started in the early 1980s (Taylor & Kuyatt, 1994 : p. 11). The relevant technical documentation includes the guidelines of the National Institute for Standards and Technology (Taylor & Kuyatt, 1994) and the technical reports from the Bureau International des Poids et Mesures (Joint Committee for Guides in Metrology, 2008, 2009). In their reports and in what follows, the terminology is used according to the International Organization for Standardization (2004).

A measurement is a process involving a *system* and an *apparatus*. The quantity that is subject to measurement is called the *measurand*. The value resulting from the measurement using a certain apparatus is known as the *indication value*. The difference between the value of the measurand (what is often called the "true value") and the indication value is the *error*. As we see, error is a *semantic* notion, relating to a *matter of fact* relating two numerical values. It is not about what we know, ignore, wish to know, or even can know. It must be distinguished from the *epistemological* notion of uncertainty:

[9] 'GUM' stands for 'Guide to the expression of Uncertainty in Measurement.'

> For example, the result of a measurement after correction can unknowably be very close to the unknown value of the measurand, and thus have negligible error, even though it might have a large uncertainty. (Taylor & Kuyatt, 1994 : p. 2)

It is important to stress the distinction since, in many textbooks and academic papers, error and uncertainty are used interchangeably.[10] The methodological rules then become inextricably unclear. Even if error and uncertainty are confused everywhere in the literature, they *appear* clear to readers since both are merely understood as interval within which the "true value" lies. Nonetheless, error intervals and uncertainty intervals express very different things. Moreover, it is to be noted that the semantic notion of error is the notion that does for scientists the work that *approximate truth* does in many philosophical discussion. When the error is small, the measurement is said to be *accurate*.[11]

Philosophers often wrongly use the terms 'error' an 'approximation' interchangeably. However, scientists have practical problems that prevent them from ever forgetting that error and approximation are very different concepts. As a matter of fact, *some errors are approximations, but others are not.* The term 'error' applies whenever something that is not exactly true is asserted. Sometimes, the error will be small and we will claim that the assertion is nonetheless accurate, *i.e.*, that it is an approximation. From this point of view, *the task of both the experimenter and of the applied mathematician is to a large extent to determine which errors are approximations.* By not making the distinction, philosophers fail to acknowledge what is the bread and

[10] In fact, the distinction is so often diregarded that the official documents repeat multiple times that "[t]he difference between error and uncertainty should always be borne in mind." (Taylor & Kuyatt, 1994 : p. 2)

[11] Taylor & Kuyatt (1994 : p. 14) say that accuracy is a qualitative concept, by which they mean that "one should not use it quantitatively, that is, associate numbers with it." This is right that no number should be associated with it, perhaps, but then it does not make it a qualitative concept as philosophers and logicians more generally use the term, *i.e.*, as something that does not admit of degrees, that either applies or not. We use Carnap (1966) to distinguish between qualitative, comparative, and quantitative concepts, and deem accuracy a comparative concept.

Note also that the term 'precision' should not be used for 'accuracy.' VIM does not define precision because of the many definitions that exist for this word (Taylor & Kuyatt, 1994 : p. 14). However, precision is often measured by the number of correct digits.

butter of entire scientific disciplines among the most important to understand scientific methodology.

How do we determine if an error is small enough to be considered an approximation? This is a question that is epistemological, because it demands that we determine whether the error is small enough, but also a specification of the *criteria* by which that judgment is made. To begin with, we are making a measurement *because* we do not know the value of the measurand; as a result, we cannot have a direct criterion to determine the error. All the scientist can do is to provide an *estimate* of the error based on what is known about the system and the measurement apparatus.[12]

This is why, in modern expositions of the theory of measurement, the role of uncertainty is given priority over that of error for the formulation of methodological rules. See, *e.g.*, what the report of the National Institute of Standards and Technology says:

> In general, the result of a measurement is only an approximation or estimate of the value of the specific quantity subject to measurement, that is, the *measurand*, and thus the result is complete only when accompanied by a quantitative statement of its uncertainty. (Taylor & Kuyatt, 1994 : p. 1)

With the notions of error and uncertainty disentangled, we must now rectify things that are part of many classroom presentations.[13] Understandably, the first step toward a correct estimation of the uncertainty of the results of a measurement is a diagnosis of the possible sources of measurement error. With this purpose, textbooks typically introduce the distinction between two kinds of measurement error:

1. random error, and

2. systematic error.

They are also often referred to as Type A and Type B error, respectively. On the one hand, *random errors* are unpredictable. They are variations in the

[12] "In general, the error of measurement is unknown because the value of the measurand is unknown. However, the uncertainty of the result of a measurement may be evaluated." (Taylor & Kuyatt, 1994 : p. 15).

[13] The importance of those rectification is stressed by BIPM and NIST.

Random Error	Systematic Error
vibration in the floor → fluctuation in balance	during the time required to measure the mass of a fluid, some evaporates
air currents → fluctuation in balance	during the time required to measure length, the temperature is not controlled and changes
electrical noise in a multimeter	mis-calibrated balance will cause all the measured masses to be wrong

Table 3.1: Examples of random and systematic error.

measurements that the experimenter cannot control (or can control only very limitedly). In terms of probability, it is an error that is just as likely to be above or below the real value; so, for random error, averaging a large number of measured values should, in principle, largely reduce the magnitude of the error. On the other hand, *systematic error* cannot be controlled as random error, *i.e.*, averaging will be of no help. Systematic error is caused by the design of the experiment. Its impact can only be alleviated by modifying the design; however, it is often very hard to find a setup that has no systematic error. See table 3.1 for examples. Systematic error is particularly problematic, from an epistemological point of view, because there is no way to determine whether there is a systematic error.[14]

In addition, there is a third thing that is often called human error. Given that it can be eliminated and that both random and systematic error cannot, we call it "mistake" instead of "error" to prevent equivocation. Typical examples of mistakes are: spilling substances, reading a measuring device incorrectly,[15] bad calculations in the design phase, wrong formulas used in the

[14] "Like the value of the measurand, systematic error and its causes cannot be completely known." (Taylor & Kuyatt, 1994 : p. 15) As a consequence of this fact, it is often claimed by metrologists that confirmation or disconfirmation is not what characterized experimental success, but that finding a design that isolates a parameter so that it can be measured without systematic error is.

[15] However, the kind of error that is caused by the experimenter's eyes' inability to read the exact level of liquid in a graduated cylinder, for example, is a random error, not a human error.

design phase, inadequately cleaning the equipment, using the wrong substance, not following the protocol, *etc.* These reflect no uncertainty in the data; rather, it completely invalidates the data.

Now, there are problems with this way of presenting things. Consider the equation

$$\mu - \iota = \delta \,,$$

where μ is the measurand, ι is the indication value, and δ is the error. What should we say when presented with the question: is δ a random error or a systematic error? The first thing to emphasize is that δ is just a number, so it is neither random nor systematic. Thus, in agreement with the Joint Committee for Guides in Metrology (2008, 2009) and Taylor & Kuyatt (1994), we should talk of *random effect* and *systematic effect* on the measurement (instead of random and systematic error), and try to estimate how much of δ *arises from* random effect, and how much from systematic effect. Moreover, when results from a measurement are reported, it is with an estimate of the error, *i.e.*, the uncertainty, and not with the error itself. Thus, scientists following the standards will report the measured value together with a quantitative statement of the uncertainty; the uncertainty will have components arising from random effects, and others from systematic effects. One can only determine if an uncertainty component arises from systematic or random error in reference to the particular measurement process being executed; in a different process, the same component could be of a different nature.[16]

Each of the uncertainty components that contribute to the uncertainty of the measurement are represented by an estimated standard deviation, termed *standard uncertainty*. This standard deviation may be or may not be evaluated statistically, as case A or B may be. We talk of *Type A* and *Type B* evaluation of uncertainty. Type A uses any appropriate statistical method. However, the procedure is not as straightforward for the assessment of type B uncertainty

[16]'In the words of Taylor & Kuyatt (1994 : p. 16), 'the adjectives "random" and "systematic", while appropriate modifiers for the word "error", are not appropriate modifiers for the word "uncertainty" (one can hardly imagine an uncertainty component that varies randomly or that is systematic)."

components:

> A type B evaluation of standard uncertainty is usually based on scientific judgement using all the relevant information available, which may include
> – previous measurement data,
> – experience with, or general knowledge of, the behavior and property of relevant materials and instruments,
> – manufacturer's specifications,
> – data provided in calibration and other reports, and
> – uncertainties assigned to reference data taken from handbooks.
>
> (Taylor & Kuyatt, 1994 : p. 2)

We have seen how *repeating* the experiment can be used to successfully control error arising from random effects. By carefully considering the factors mentioned, *replicating* the experiment can be used to successfully control error arising from systematic effects.[17]

The list of contributing factors mentioned appears to be quite complete and yet, the method of evaluation of type B uncertainty does not give us grounds to precisely state standard deviations (as in the type A case). As a result, the method might seem unsatisfactory; however, it would be unreasonable to expect more, given the epistemological nature of the problem:

> The word "uncertainty," by its very nature, implies that the uncertainty of the result of a measurement is an estimate and generally does not have well-defined limits. (Taylor & Kuyatt, 1994 : p. 17)

The situation is similar to what we had in chapter 2, where we could not provide exact bounds on the accuracy of model equations.

At this point, we have methods to estimate standard uncertainty components of type A and type B. The task that remains is to find the *combined standard uncertainty* of a measurement result. The combined standard uncertainty, as its name suggests, combines together all the uncertainty components of type A and B to generate a total estimate of uncertainty. The usual method used to combine uncertainty is the common statistical method used to combine standard deviations known as *law of propagation of uncertainty* (also

[17] It is important to distinguish repeatability and replicability. Repeatability consists in doing the measurement multiple times with the same apparatus; replicability involves changing the apparatus, each having their own systematic biases.

Bazooka Joe is showing a friend a fossilized bone. The friend asks how old it is and Bazooka Joe responds that it is one hundred million and three years old. "How do you know that?" asks the friend. Bazooka Joe responds "The museum expert told me it was a hundred million years old and that was three years ago."

Figure 3.2: An incorrect use of mathematics to an uncertain value.

often called *root-sum-of-squared* or even just RSS).

3.2.2 The Mathematics of Uncertainty

We have seen that the concepts of error and uncertainty are intrinsic aspects of the values of the parameters fed into the model-construction recipe. What does the presence of error and uncertainty imply for the mathematics used to analyze models? The key challenge here is to develop mathematical techniques that permit us to deal adequately with operations on quantities not known exactly. Accordingly, it is central to an understanding of the role of mathematics in science to articulate the *strategies used to manage this sort of uncertainty*.

The most basic approach to do this is that based on *significant figures* (or, alternatively, *significant digits*). The idea is easily understood from the Bazooka Joe comic displayed in figure 3.2 (reported by Ruekberg (1994) in the *Journal of Chemical Education*). The author then explains the pedagogical relevance of the joke as follows:

> The author readily admits that the joke is not *very* funny. That it is funny at all is because even children, just old enough to chew gum without swallowing it, realize that something is wrong about Bazooka Joe's computation: the accurate three years cannot be added to the ball park figure of a hundred million years. (Ruekberg, 1994)

The author claims that the computation is wrong even if, arithmetically speaking, it is irreproachable. Thus, when there is uncertainty, there is a methodologically important sense of 'correctly using mathematics' that differs from the standard one. To delineate this second sense, we examine the two roles played by significant figures.

Understood as a tool occupying a central place in a strategy for the management of uncertainty, the first role of significant figures is to faithfully report the uncertainty in experimental measurements, in a simple way that does not involve the statistics that are central to the GUM approach.[18] More precisely, significant figures are a tool to faithfully report the *accuracy* and the *precision* of the results of a measurement, given the *resolution* of the measuring device. It is important to keep in mind the distinction between accuracy and precision. Accuracy is about having the answer right, *i.e.*, about having a small error. Precision is about having many digits. For instance, 3.166666666666667 is a very precise (16 digits) but (for many purposes) very inaccurate (2 digits) approximation to π. On the other hand, $4.54 \cdot 10^{-5}$ is a not very precise (3 digits) but quite accurate (precise to the order 10^{-6}) approximation to e^{-10}. Similarly, a measuring instrument can be very precise, and yet inaccurate. As Kahan & Darcy (1998) explain, "[p]recision is to accuracy as intent is to accomplishment." A basic objective of the use of significant figures is to not be fooled by measurements that are more precise than accurate; thus, a basic rule is to not report results of measurements with more digits than are accurate, for those extra digits would not be significant. Precision is a property of a linguistic object (namely, of the numeral representing a number in a given number system) whereas accuracy is a semantic notion.[19] Limiting the number of figures reported to the significant figures is a way to make the semantics transparent by showing it in the form of the linguistic expressions used to report results.

A simple example that does not require considering the details of how a measurement device works is this. Consider the population of my home town; I would guess it is about 5000. It would probably be wrong to think that it is the exact number of people residing there, though it might be. Now, just how

[18] There is a commonly held view among scientists that approaches in terms of intervals and deviations are not meeting all mathematical expectations: "Besides, nobody wants error bounds; we desire final results known to be reliable because their errors have been proved inconsequential." (Kahan & Darcy, 1998)

[19] An analogy might help: 'being precise' is to 'being a well-formed formula' what 'being accurate' is to 'being true.'

many significant digits are there in that number? We need to know something about the real answer to determine how many significant digits there are! Or at least, the person who makes the measurement must be able to report the number of significant figures appropriately (in my case, only the 5 is, and there is an uncertainty about it).

The resolution of an instrument is the maximum error that the instrument produces under pre-specified circumstances (*e.g.*, value range, ambient temperature, humidity, pressure, *etc.*).[20] However, the resolution cannot be smaller than the precision of the instrument. To illustrate this point with a simple example, if a ruler's smallest division is 1mm, then we cannot specify what a length is by measuring with this ruler is to less than half of a millimeter. What we obtain from an instrument with this precision is a number having the format $x.yyz$cm, were x is the integer part, yy are the certain digits, and z is the uncertain digit (there is only one of those, the last one). The last digit is only an estimation. Moreover, under the pre-specified conditions mentioned above, if the instrument is properly calibrated, then each digit within the precision of the instrument is taken to be significant. Accordingly, *the significant figures of a number are those digits that carry meaning contributing to its precision*, and indirectly contributing to its accuracy, provided that some assumptions about calibration are satisfied.[21] Thus, the signifiant figures of a number are the digits necessary to specify our knowledge of that number's precision; and in nice contexts, this also reveals the accuracy.

Whereas this first role of significant figures is to *represent* uncertainty numerically by imposing conditions on precision, the second role of significant figures is to permit the formulation of computation rules to determine how uncertainty *propagates*. In introductory texts, those rules are formulated in terms of limits on the numbers of significant digits (or significant decimal places) that may be retained in order to faithfully track uncertainty. We use the scientific notation to prevent ambiguities in the count of significant

[20]The manufacturers of equipment often indicate how accurately and precisely it can measure.

[21]Of course, if the pre-specified conditions of calibration are not met, then the reasoning does not hold.

figures.[22]

If we return to the Bazooka Joe joke above, such rules are meant to prevent one from saying that $1 \cdot 10^8 + 3 = 1.00000003 \cdot 10^8$ years. Rather, since the $100,000,000$ years number is only a ballpark estimate known to 1 significant figure, the addition of 3 years should not be considered significant, so that $1 \cdot 10^8 + 3 = 1 \cdot 10^8$. The number 3 added here is within the level of uncertainty, so it cannot affect the significant figure. Similarly, if you are told that the population of a city is 50000 with only two significant digits (*i.e.*, $5.0 \cdot 10^4$), and that 78 persons immigrate, 289 are born, and 198 have died, you get

$$5.0 \cdot 10^4 + 78 + 289 - 198 = 5.0 \cdot 10^4.$$

It does not make the population $50,169$. We know that the population increased by 169 for sure, but we do not know what it was to begin with. So, the correctly reported sum, from the point of view of managing uncertainty, is $5.0 \cdot 10^4$.

Now, strictly speaking, the sum $1 \cdot 10^8 + 3 = 1.00000003 \cdot 10^8$ is arithmetically false. To avoid difficulties of this kind, one could introduce a new mathematical operation $\oplus$ that validates uncertainty-preseving manipulations such as $1 \cdot 10^8 \oplus 3 = 1.00000003 \cdot 10^8$. Notice that, with such uncertainty-preserving operations, we are not dealing with *approximate* equalities (denoted by '$\approx$') holding between exact quantities, but rather we are dealing with *exact* equalities (denoted '=') holding between quantities known uncertainly.[23] Here are examples of rules governing such modified operations:

1. Exact numbers do not affect the number of significant figures. For example, $2.02 \otimes \pi = 6.35$ (and not $6.34601716\ldots$).
2. For addition and subtraction, the answer contains the *same number of decimal places* as the least precise operand used in the calculation. For

[22] If we do not, then it is hard to say how many significant figures there are. Clearly, in 0.000314 there are 3. But there is ambiguity in the trailing zeros. 1200 can have 2, 3, or 4 sig figs. But if we write $1.2 \cdot 10^3$ or $1.200 \cdot 10^3$, the ambiguity disappears.

[23] Importantly, this is a standard way to discuss another kind of mathematical operations dealing with error, namely, floating-point arithmetical operations. See also Corless & Fillion (201x) or Fillion (2011).

example $456.367963 \ominus 452.1 = 4.3$ (and not 4.267963). The idea is that you cannot add to or subtract from something not known.

3. For multiplication and addition, the answer contains the *same number of significant figures* as the least precise operand used in the calculation. For example, $72.5674 \otimes 3.34 = 2.42 \cdot 10^2$ (and not 2.42375116).
4. For logarithms, only those numbers to the right of the decimal place of the operand count as significant. For example, $log^*_{10}(1.25 \cdot 10^{-6}) = 5.903$ (and not 5.9031).

Such a set of rules constitute what is called a *significance arithmetic*.

Now, these rules should not be thought of as being perfectly reliable. Rather, they work as rules of thumb. One might be surprised that there are, well into the twentieth century, many publications debating how significant digits should be analyzed and understood.[24] For example, suppose we need to average measurement results. Consider five measurements of, say, mass, known to 2 decimal places, and compute the significant average as follows:

$$\frac{5.73 \oplus 5.68 \oplus 5.66 \oplus 5.71 \oplus 5.68}{5} = \frac{28.46}{5} = 5.962$$

This result follows from the rules mentioned above, and yet the average has more precision than the individual measurements! Intuitively, we would expect 5.69, seeing to it that the average does not contain more than 2 decimal places. To ensure that there is only uncertainty on the last digit, we would have to round to the first decimal place, resulting in 5.7, since the accumulation of error can affect the first decimal place of the average too. And this would be required in cases where the first digit changes from measurement to measurement.

Now, perhaps this could be fixed by refining the rules of our significance arithmetic along those lines; it would pose no technical difficulty to articulate a refined rule parsing the numerals fed into the calculations. However, for any set of such rules based on significant digits, it would be relatively straightforward to generate problematic cases. The lesson to draw is this:

> Ideally, arithmetic precision should be determined not bottom-up (solely from

[24] The example below is discussed by Schwartz (1985).

> the operand's precisions) but rather top-down from the provenance of the operands and the purposes to which the operation's result, an operand for subsequent operations, will be put. (Kahan & Darcy, 1998)

Thus, we see that as an attempt to provide context-independent syntactic rules meant to support management of uncertainty and its propagation, significance arithmetic has limitations.

Context-free regimentation of the courses of thought by means of syntactic rules, it seems, must give way to rules of thumb based on semantic considerations essentially based on the analysis of the effects of uncertainty. To the extent that the analysis of the propagation of uncertainty parallels the analysis of the effect of perturbations, we have a case identical to that discussed in chapter 2. To see that there are parallel to each other, consider that the basic methods for examining the propagation of small *errors* are taught in calculus, and that more advanced methods are the trade in perturbation theory and in numerical analysis. But these methods can be used in a straightforward way to see how *uncertainty* propagates through calculations. Suppose that one knows the values of three parameters x, y and z, with an uncertainty of plus or minus $\Delta x, \Delta y$, and Δz. From these, one wants to find the value of a quantity $f(x, y, z)$ with its uncertainty. Then, to a first-order approximation, the resulting uncertainty is

$$\delta f = \left|\frac{\partial f}{\partial x}\right| \Delta x + \left|\frac{\partial f}{\partial y}\right| \Delta y + \left|\frac{\partial f}{\partial z}\right| \Delta z \, .$$

Suppose that we want to find the density of a sphere, given that we know that the mass is $m = (83.1 \pm 0.1)$g and the diameter is $d = (2.55 \pm 0.02)$cm. Then

$$\rho = \frac{m}{V} = \frac{m}{\frac{4}{3}\pi\left(\frac{d}{3}\right)^3} = \frac{6m}{\pi d^3} = \frac{6 \cdot 83.1}{\pi (2.55)^2} g cm^3 = 9.57 \frac{g}{cm^3}$$

Then the maximum possible error consistent with our uncertainty of the operands is

$$\Delta\rho = \frac{6}{\pi d^3}\Delta m + \frac{18m}{\pi d^4}\Delta d = 0.2\frac{g}{cm^3}$$

Thus, we have an expression specifying how our uncertainty on m and d prop-

agates to ρ. Note that, to find the maximum possible error consistent with the uncertainty, it is important to used absolute values. However, in many cases, the error will be smaller than the maximum possible error, since the errors will cancel one another. So, the real error is typically smaller than the maximum possible error.

3.3 Summary

In summary, this chapter has made the following points:

C3.1 Issues concerning the scale at which theories operate matter for the logic of modelling in many respects. In particular, using a theory at the wrong scale will often impede the tractability of the resulting model equations. Thus, the claim that low-scale theories are fundamental and that phenomenological theories are introduced for pragmatic reasons only is based on a naive view of science.

C3.2 Characterizing the structure of theories grounded in applications contributes to explaining the effectiveness of mathematics, to the extent that it characterizes different types of mathematical principles and their roles in the construction and evaluation of models. We have identified the role of conservation laws and of constitutive equations; moreover, we have distinguished them from so-called 'empirical regularities.' This shows that standard reconstructions of scientific practice fall short of explaining the success of science.

C3.3 The semantic notion of accuracy and the epistemological notion of uncertainty are intrinsic to the theory of measurement. Once their proper use is understood, it is seen to match precisely their use in the logic of mathematical modelling. In particular, the management of uncertainty in the former case, and the management of error in the latter case, use the same mathematical tools drawn from perturbation theory.

C3.4 To understand how one should evaluate the proper use of mathematics in contexts involving uncertainty, it is important to understand that different operations depending on the significance level of the operands must be used. Moreover, rules governing such operations cannot be reliably articulated in definite syntactic terms, but rather should be adapted depending on the semantic and pragmatic contexts.

At this point, we have a general characterization of the logic of mathematical modelling, as well as a characterization of the underlying role that theories and experimentation play. We have examined most types of errors and uncertainties encountered in the context of modelling and we have seen how mathematics can be used to manage them. As we see, the effectiveness of mathematics is not only that it provides a language to express relations found in nature and that it provides a rigorous context in which to carry inferences, but that in addition it contains self-checking methods to ensure that the semantic and epistemological deficiencies accumulated during the modelling process will not undermine the resulting representations. However, there is one more type of error to discuss, in relation to the solution of model equations. In the next chapter, we examine this case, and we present a general schema of analysis that captures the self-checking virtue of mathematics mentioned above.

Chapter 4

The Solution of Model Equations and the Interpretability of the Results

> [...] the assumption that as soon as a fact is presented to a mind all consequences of that fact spring into the mind simultaneously with it [...] is a very useful assumption under many circumstances, but one too easily forgets that it is false.
>
> Turing (1950)

In our depiction of the logic of modelling in chapter 2, we have seen that the process of modelling involves the essential step of *extracting* information from model equations (themselves derived from modelling assumptions) in order to answer questions that we have concerning the behaviour of interest of some system. Philosophers and scientists alike often assume that the extraction of information in question requires *solving* the model equations. For instance, if the model equations are differential equations $\dot{\mathbf{x}} = \mathbf{f}(t, \mathbf{x})$, then extracting information to answer questions regarding what will happen in the system would amount to finding a function $\mathbf{x}(t)$ (possibly unique, if it exists at all) that can be used to calculate the states of the system at a given time t. But as

we will see, exact solutions are often neither required nor desired, since a careful examination shows that they do not always do the work that is expected from them. Epistemologically speaking, a proper analysis of scientific methodology cannot ignore this very important computational step.

However, in their reflections on science, philosophers and scientists alike often ignore the computational aspects that impose limits on what information can be extracted from sets of equations. An example of this inadvertence arises in Carnap's discussion of Laplace's demon:

> In my opinion, determinism is a special thesis about the causal structure of the world. It is a thesis that maintains that this causal structure is so strong that, given a complete description of the entire state of the world at one instant in time, then with the help of the laws, any event in the past or future can be calculated. This was the mechanistic view held by Newton and analyzed in detail by Laplace. (Carnap, 1966 : p. 217)

To be sure, Carnap does not believe that a person with the cognitive capabilities of Laplace's demon exists. Rather, the claim that he is making is that determinism is equivalent to the following conditional sentence: if we were given the state-vector $x_k(0)$ for all bodies k in the world, and the laws governing this world, then we would know the unique function $x_k(t)$ specifying the states of all bodies k at any time t (whether it is past, present, or future). This is a fairly standard characterization of determinism.[1] However, it is *not* what Carnap said; he did not say that if the antecedent clauses were satisfied, there would be unique functions $x_k(t)$, but rather that for any t, we could *calculate* the states $x_k(t)$. The two claims are extremely different: just because a function exists does not mean it can be calculated! In fact, much of computer science and applied mathematics seeks to develop symbolic and numerical recipes to bridge the gap between the two claims. For them, it is crucially important to be not only *effective*, but also *efficient*. It is important to emphasize this point in order to properly understand the role of mathematics in the sciences. In this respect, this chapter will draw lessons from numerical analysis for the epistemology of science.

[1] There is a problem with this characterization given the discussion on the structure of theories in the previous chapter. But I will not pursue this objection here.

The chapter will examine a number of aspects of the process of extraction of information from models. Ultimately, those aspects contribute to constructing the concept of mathematical tractability promised in chapter 1, which is a hybrid of the notions of verifiability and effective calculability. The concept of tractability presented here explains how to bridge the gap mentioned above in the context of mathematical modelling. To begin this chapter, we will examine the types of exact solutions of mathematical problems. Following that, a discussion of inexact solutions—often called *numerical* solutions—will explain the motivation for their use in mathematical sciences.

4.1 Types of Exact Solutions and Their Virtues

As I emphasized in the last sections of chapter 2 in relation to the qualitative analysis of dynamical systems, it is often sufficient to only know the bifurcation points and the asymptotic behaviour. In such cases, only qualitative features of the solution are required; explicit expressions solving the model equations or numerical figures specifying the states of the system would not add anything relevant for the questions on which we fixed our attention.

However, in many cases, precise numerical figures are important to answer the questions we have concerning a system. In such cases, a qualitative analysis of the behaviour of the system will not suffice. This situation is so common that Feynman even claimed this:

> The whole purpose of physics is to find a number, with decimal points, *etc.*! Otherwise you haven't done anything.[2]

This is a slightly exaggerated claim, but it is still important to emphasize that, often, the precise numbers matter for our understanding of a system. This raises the question: how, exactly, do we get to a number? If the number sought is one that describes the state of a system at a given time, then the number will typically be obtained by finding a solution to a model equation,

[2]Cited from Yu I. Manin, *Mathematics and Physics*, Boston: Birkhauser, 1981, p. 35 in Wilson (2006).

Figure 4.1: A way of getting to a number.

and then the solution will be evaluated at the required time. This process is an instance of the schema of figure 4.1. As we see, this process crucially depends on evaluating a mathematical expression of the solution of the given problem. This, in turns, crucially depends on finding an explicit expression for a solution to the model equation (and, in addition, one that is computable in some sense). Again, this crucially depends on whether a solution exists at all.

Philosophers of science who have not examined the computational aspects of the use of mathematics in science are often surprised to find out that many processes that go from a problem to a number do not match that of figure 4.1. Later in the chapter, we will discuss the case in which we use numerical recipes to get to a number which is close enough to the number that would be obtained by following this schema. This case differs in that we do not need an expression for the solution; we only need to do numerical manipulations to obtain an approximate number. However, before moving to this case, it is important to understand some nuances related to exact solutions. There are many types of exact solutions to a mathematical problem; moreover, some types of exact solutions do not give us means to get to a number in the way sketched in figure 4.1. This fact is often overlooked, even among philosophers of science concerned with computation. Let us outline the main points.

To begin with, it is not uncommon to see the phrases 'exact solution,' 'algebraic solution,' 'analytic solution,' and 'closed-form solution' used interchangeably in the philosophical (and even sometimes in the mathematical) literature. Typically, those phrases are used to characterize an epistemological context in which exactness prevails, *i.e.*, in which approximations are of no concern. However, there are important differences between them. Among the phrases above, 'exact' is the most general term that refers to any mathematical objects that satisfies the conditions constitutive of the problem, and the

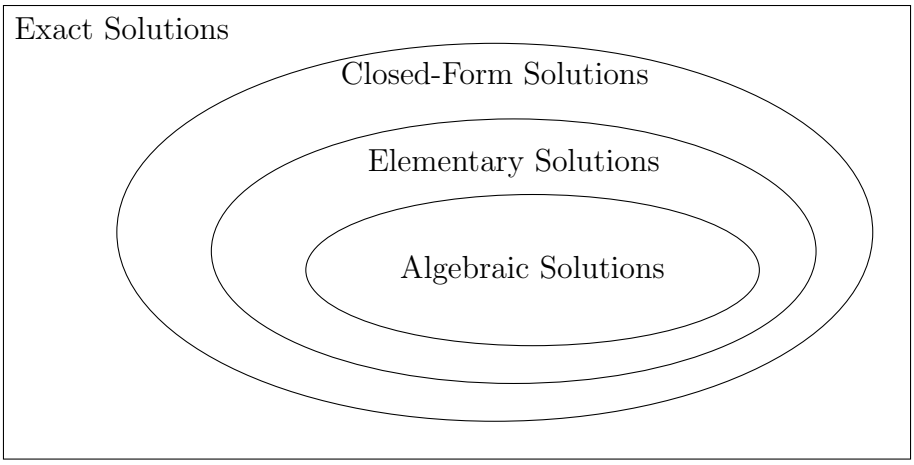


Figure 4.2: Venn diagram of the types of exact solutions

others are particular cases of exact solutions arranged as in figure 4.2.

Consider an arbitrary problem that happens to have a unique function as exact solution. To begin with, we say that the problem has an algebraic solution if the solution can be written as a finite combination of algebraic operations. So, the question of whether there is an algebraic solution depends on what is in the class of algebraic functions. Note that the property 'having an algebraic solution' depends on the existence of an expression of a given type that captures the function that solves the problem (and not only on the existence of a solution). The same can be said for elementary and closed-form solutions. The classes of operations that are admissible for expressions of the solution are as follows:

- Algebraic expressions admit the following operations: addition, subtraction, multiplication, division, and exponentiation with integral and fractional exponents;
- Elementary expressions admit all elementary algebraic operations, plus exponents and logarithms in general (and so they include trigonometric and inverse trigonometric functions as well);
- Closed-form expressions include all closed-form expressions, plus many other "well-understood functions," in particular the so-called *special*

functions (but not any limit or integral).[3]

From their mutual relations, we see that it might be the case that some problems have an closed-form solution without having an elementary solution, and that some problems have an elementary solution without having an algebraic expression. For example, as is well known, the Ruffini-Abel impossibility theorem shows that degree five polynomials

$$p(x) = c_5x^5 + c_4x_4 + c_3x^3 + c_2x^2 + c_1x + c_0 = 0, \qquad c_5 \neq 0$$

generally have no algebraic solutions (that is, they have no solutions expressible with algebraic expressions, which include radicals). That is of course not the same as saying that there is no solution, because the existence of a solution is guaranteed by the fundamental theorem of algebra. Rather, the theorem says that the solution cannot be expressed using a form that is particularly convenient for the sake of calculations.[4] Moreover, it does not only say that no such expression has been found so far; rather, it says that no such expression will ever be found.

The same situation applies for closed-form solutions. For example, the common Gaussian integral

$$I = \int_0^x e^{-x^2} dx$$

has no elementary solution, *i.e.*, there is no elementary expression to capture the solution of this integral. The model equations of very simple physical systems do not have closed-form solutions. Consider the simple pendulum displayed in figure 4.3. The only forces acting on the ball are the tensile force toward the hinge exerted by the massless rigid rod and downward gravity.

[3]Borwein & Crandall (2010) review seven different approaches to defining what the class of closed-form solutions contains; importantly, those disagree on the extension of the class. Moreover, as they report (from Weisstein), "an infinite sum would generally not be considered closed-form. However, the choice of what to call closed-form and what not to is rather arbitrary since a new 'closed-form' function could simply be defined in terms of the infinite sum." The idea is that, at a given stage of development of mathematics, any function that is well-understood is to be considered closed-form.

[4]However, they *do* have a solution in terms of elliptic functions, whose computation is nowadays understood well. This is a closed-form solution that is not elementary.

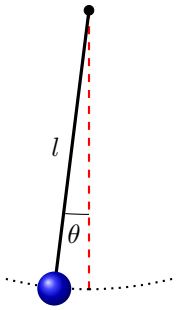


Figure 4.3: Simple pendulum with $m = 1\text{kg}$.

Thus, we can derive the model equation

$$\frac{d^2\theta}{dt^2} = -\frac{g}{l}\sin\theta\,.$$

This simple equation turns out not to have an elementary solution.[5] However, it does have an exact solution, in terms of Jacobi elliptic functions, which is closed-form. Be that as it may, instead of being satisfied with this exact non-elementary solution, physicists often approximate the problem by taking the limit $\theta \to 0$, *i.e.*, by examining what happens for small angles. Then, the model equation reduces to the simple harmonic oscillator

$$\frac{d^2\theta}{dt^2} = -\frac{g}{l}\,,$$

which has the simple elementary solution $\theta(t) = c_1 \sin\omega t + c_2 \cos\omega t$, where $\omega = \dot{\theta}$. Interestingly, if we take a simple harmonic oscillator, and then add a linear factor to the model equation, we can again have a situation that has no elementary solution. This is also easy to imagine in a physical setup. If you consider a mass attached to a Hookean spring, the model equation would be $\ddot{x} = -x$ (suppose the stiffness k is 1), a simple harmonic oscillator. However, in real systems, stiffness is not constant. We can try to understand what would

[5] Another famous example of this situation is the global solution of the n-body problem provided by Wang (1990). It is analytic, but does not have a closed-form representation.

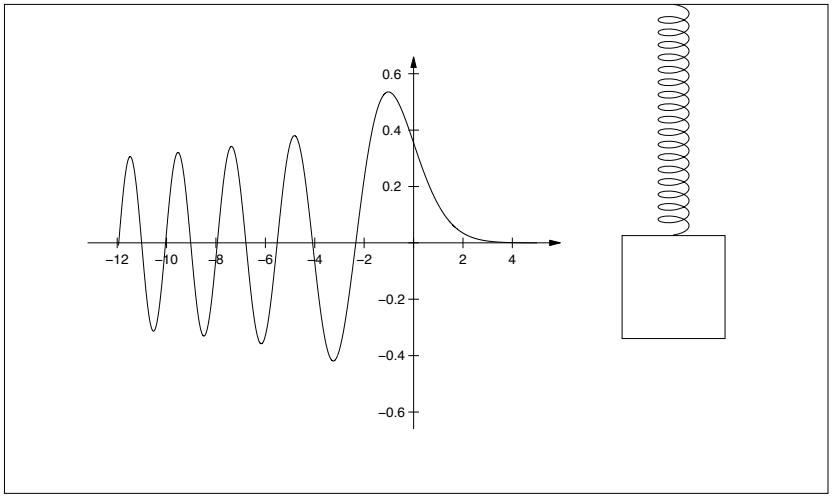


Figure 4.4: Oscillator with stiffness linearly increasing with t.

happen if the stiffness increased linearly with time, so that

$$x'' + tx = 0.$$

See figure 4.4. It turns out that the solution of this model equation is the Airy function, which cannot be expressed as an elementary function. Thus, small changes in the physical circumstances can drastically alter the kind of solution afforded by the model equations.

When there is an exact solution, but no elementary solution, it is necessary to rely on series representation of the solution to evaluate it at some time. With respect to calculations, the difficulty with infinite series representations is that we cannot sum an infinite number of terms. It then seems that we can evaluate the solution to arbitrary accuracy by using increasingly long (but finite) truncated series. An interesting situation arises when we have a perfectly good analytic solution in the form a uniformly convergent Taylor series, which converges so slowly, that it ends up being of no practical use for computation. The Airy function mentioned above is a good example of this. The Airy function can be represented by the following uniformly convergent

series:

$$\begin{aligned}\mathrm{Ai}(x) &= \frac{1}{\pi}\int_0^\infty \cos\left(\frac{1}{3}t^3 + xt\right) dt \\ &= 3^{-\frac{2}{3}}\sum_{n=0}^{\infty}\frac{x^{3n}}{9^n n!\Gamma(n+\frac{2}{3})} - 3^{-\frac{4}{3}}\sum_{n=0}^{\infty}\frac{x^{3n+1}}{9^n n!\Gamma(n+\frac{4}{3})}\,,\end{aligned}$$

where Γ is the Gamma function (see Bender & Orszag, 1978). Note that, even if, theoretically, the series converges for all x, it is of no practical use. If we use a standard Taylor series computation in standard floating-point arithmetic to compute $f(-12.82)$, near the tenth zero, the absolute error grows very fast as x increases negatively. Even if the series converges uniformly, the floating-point computation does not.[6] The same loss of convergence would arise for other finite precision arithmetics, or for computations involving data containing some inaccuracies. This limitation mirrors that of systems of significance arithmetic, which have been discussed in section 3.2.2.

Thus, to the extent that we need to get numbers, calculability is very important. And as we have seen, calculability is most straightforward when we have an expression that we can evaluate. This obtains when we have finite expressions capturing solutions, *i.e.*, when we have algebraic or elementary solutions. The requirement of exactness is insufficient to the extent that it allows for solutions that cannot be expressed finitarily. However, as we will discuss below, even finitarily representable solutions do not guarantee that no problems will arise in calculation. Thus, the lesson of this discussion is this: when only qualitative behaviour is of interest, exact solutions are not very important. On the other hand, when quantitative information is required, exact solutions will in general not give us a straightforward recipe as in figure 4.2. This recipe would be within reach, however, if the exact solution were capturable by an algebraic or elementary expression. This is why, even when a problem is susceptible of receiving an exact solution, applied mathemati-

[6]Notice that increasing the floating-point precision will not stop that from happening. Is this really a catastrophe? From the modeling point of view, no. The difficulty stems from radical scale changes, and in this context, it makes sense to consider scale as a fundamental factor in our search for solutions.

cians often approximate the description of the system in order to derive model equations that have a closed-form solution or even elementary or algebraic solutions. However, this implies that, for small changes in our description of the system, the character of the solutions can change significantly. But given that mathematical modelling is an activity practiced in a context where uncertainty is always present, this means that our emphasis on exact solution will not, in general, guarantee that from the solutions we will be able to compute accurate numerical results. These considerations lead us to a more inclusive way of dealing with the extraction of quantitative information from mathematical representations inspired by the works of numerical analysts.

4.2 Numerical Solutions of Problems and Mathematical Tractability

The construction of a mathematical model is a process that seeks to capture the essential synchronic or diachronic features of a system by deriving equations from modelling assumptions. Moreover, in order to make predictions or to explain phenomena by means of the model equations, it is crucial to find the solutions when the quantitative aspects dominate. The process of solving the model equations typically involves mathematical operations such as evaluating functions, finding zeros of functions, solving systems of equations, solving difference or differential or integral equations, *etc.* However, as we have seen, exact solutions often fall short of the work we would like them to do, and we need the stratagems devised by applied mathematicians.

Different branches of mathematics develop different methods to find solutions of such problems; here, I will focus on numerical analysis. Numerical analysis is often succinctly described as "the theory of constructive methods in mathematical analysis" (Henrici, 1964). This definition, however elegant, is nonetheless incomplete, for it does not specifically address the main purpose of numerical analysis. Elsewhere, I defined it as follows:

> A slightly more long-winded definition would also specify that this discipline

> develops, studies, and compares *efficient numerical methods* designed to find numerical *approximations* to the solution of mathematical problems arising in applications, while *quantifying* the magnitude of the computational error and *qualifying* the possible resulting misrepresentation of the system. (Fillion, 2011)

The first question to address to understand the role of numerical analysis in science is: why would a discipline devote so much effort to *approximate* solutions, instead of developing new methods to find *exact* solutions? I already explained that exact solutions do not always provide us with the best mathematical answers to our problems. Let us summarize the reasons, since knowing *why* we have to talk about approximations will suggest *how* we should talk about them.

I suggest that there are four reasons. The first reason is a pragmatic one, namely, the exigencies of scientific practice:

> The applications of mathematics are everywhere, not just in the traditional sciences of physics and chemistry, but in biology, medicine, agriculture and many more areas. Traditionally, mathematicians tried to give an exact solution to scientific problems or, where this was impossible, to give exact solutions to modified or simplified problems. With the birth of the computer age, the emphasis started to shift towards trying to build exact models but resorting to numerical approximations. (Butcher, 2008)

Thus, there are pressing demands from scientists to reliably simulate complex systems with many parameters, which are typically remarkably hard to solve analytically. The second reason is also pragmatic: even if the equations we obtain from our models are exactly formulated, there is always an appeal to experimental data; in this respect, there is a practical necessity to resort to modification, uniformization, compression, and simplification of the data. In addition, since there is always a certain degree of uncertainty in measurements, an understanding of the effects of perturbations on the solutions of our models is already required.

The third reason is brought about by theoretical necessity, as I explained in the previous section. More specifically, mathematicians have produced many impossibility theorems, *i.e.*, they have shown that some types of problems are not solvable in some ways, so that there is no computational route that

leads to numerical figures. For instance, Abel and Galois showed that it is not possible to solve general polynomial equations of degree five or more in radicals (although there is a less-well-known algorithm using elliptic functions for the quintic itself). Liouville showed that many important integrals could not be expressed in terms of elementary functions (and provided a basic theory to decide just when this could in fact be done). Turing has shown that some number-theoretic problems cannot be finitarily decided. Finally, the fourth reason is that it is important to look for approximate solutions because exact solutions might be of little value, as we have seen.

In such cases, we *have* to resort to approximation in order to use our mathematical models to predict and explain phenomena. Accordingly, the central problem of numerical analysis is an *epistemological* one:

> When one cannot know the true solution of a mathematical problem, how should one determine how close to the true solution the approximate solution is?

The similarity with other traditional questions about the adequacy of our knowledge with reality is striking.

Now, given that both the nature of mathematics in itself and the role of mathematics in science require a perspective and a theory of numerical approximation to answers, *how* should we talk about computational error? The guiding principle is that numerical methods should be discussed as part of a more general practice of mathematical modelling as found in applied mathematics and engineering. Once mostly absent from texts on numerical methods, this *desideratum* has become an integral part of much of the active research in various fields of numerical analysis. This might seem obvious, but it is somewhat in disagreement with a perspective on numerical analysis that has been developed by mathematicians having primarily in mind the construction of general-purpose software suites.

The computation required by each type of problem is normally determined by an *algorithm*, *i.e.*, by a procedure performing a sequence of primitive operations leading to a solution in a finite number of steps. Numerical analysis is a mathematical reflection on complexity and numerical properties of algorithms

in contexts that involve *data error* and *computational error*. In the study of numerical methods as in many other branches of mathematical sciences, the reflection involves a subtle conception of *computation*. With a precise understanding of computation at hand, we can refine our views on what is computationally achievable, and if it turns out to be achievable, how much resources are required.

The classical model of computation used in most textbooks on logic, computability, and algorithmic analysis stems from metamathematical problems addressed in the 1930s; specifically, while trying to solve Hilbert's *Entscheidungsproblem*, Turing developed a model of primitive mathematical operations that could be performed by some type of machine affording finite but unlimited time and memory. This model, that turned out to be equivalent to other models developed independently by Gödel, Church, and others, resulted in a notion of computation based on *effective computability*. From there, we can form an idea of what is "truly feasible" by further adding constraints on time and memory.

Nonetheless, scientific computation requires an alternative, *complementary* notion of computation, because the methods and the objectives are quite different from those of metamathematics. A first important difference is the following:

> The point of view of this book is that [...] the Turing model (we call it "classical") with its dependence on 0s and 1s is fundamentally inadequate for giving such a foundation to the modern scientific computation, where most of the algorithms—which origins in Newton, Euler, Gauss, et al.—are *real number algorithms*. (Blum *et al.*, 1998 : 3)

Blum *et al.* (1998) generalize the ideas found in the classical model to include operations on elements of arbitrary rings and fields. But the difference goes even deeper:

> Rounding errors and instability are important, and numerical analysts will always be experts in the subjects and at pains to ensure that the unwary are not tripped up by them. But our central mission is to compute quantities that are typically uncomputable, from an analytic point of view, and to do it with lightning speed. (Trefethen, 1992)

Even with an improved picture of effective computability, it remains that the concept of computability that matters for a large part of applied mathematics (including engineering) is the different idea of *mathematical tractability*, understood in a context where there is error in the data, error in computation, and where approximate answers can be entirely satisfactory. Trefethen's seemingly contradictory phrase "compute quantities that are typically uncomputable" underlines the complementarity of the two notions of computation.

In order to articulate more precisely what is meant by the claim that 'we should evaluate numerical methods in their modelling context,' we need to explain the way in which measures of computational error can be directly interpreted in terms of modelling error. To do so, I discuss the concept of modelling error in more detail in the next section. On this basis, I will then present a formal framework to characterize the relation between computational and modelling error, and the accuracy of mathematical representations.

4.3 Types of Error in Mathematical Modelling

Now that we have explained why we should care about approximate computation, we ask: what kinds of error do we encounter in modelling contexts? We have seen many kinds already, but we want a general classification. This section builds on a paper by Von Neumann & Goldstine (1947) that is often considered to be the first instance of a modern error analysis (see, *e.g.*, Wilkinson, 1971; Grcar, 2011). It describes and classifies the types of errors that are encountered in the construction and the solution of a mathematical model. The types of error arising in model construction are as follow:

$$\left.\begin{array}{r}\text{systemic error}\\ \text{experimental error}\end{array}\right\} \text{ modeling error}$$

$$\left.\begin{array}{r}\text{truncation \& discretization error}\\ \text{roundoff error}\end{array}\right\} \text{ computational error}$$

On the one hand, modelling error includes what philosophers of science have called omission, simplification, distortion, idealization, and abstraction (I jointly call them 'systemic' error). They thus include things such as neglecting air resistance on a projectile, neglecting the gravitational influence of distant stars and not-so-distant celestial bodies, assuming the constancy of parameters that are not constant (*e.g.*, the stiffness of a spring), and treating elastic bodies as being rigid (*e.g.*, a billiard ball collision). But it also includes experimental errors of various kinds (see section 3.2). On the other hand, computational errors are essentially of three types. Truncation error amounts to replacing functions $f(x)$ (often characterizing vector fields) and integrals $\int f(x)dx$ (often characterizing the motion of a body in phase space) by truncated asymptotic series in a perturbation parameter ε, *i.e.*,

$$f(x,\varepsilon) = \sum_{k=0}^{N} f_k(x)\phi_k(\varepsilon),$$

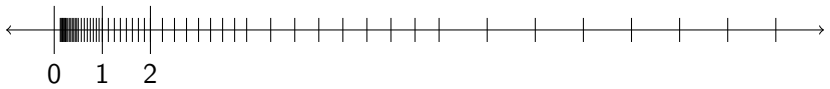
for some collection of gauge functions $\{\phi_i\}_{0\leq i\leq N}$. Expressions of this sort have to be truncated, since we often have no closed form solutions, and it is impossible to add an infinite number of terms in series. Secondly, discretization error is the error incurred by replacing a continuous parametrized flow $\dot{\mathbf{x}} = \mathbf{f}(t, \mathbf{x}(t); \boldsymbol{\mu})$ in phase space by a discrete map of the form

$$x_{k+1} = \Phi(t_k, x_k, \ldots, x_0, h, \mathbf{f}, \boldsymbol{\mu}).$$

This substitution is the basis for most methods of numerical differentiation and integration.[7] Finally, we typically do not compute the value of functions using field arithmetic (*e.g.*, the familiar arithmetic of real numbers), since computers cannot handle such entities. Thus, it is replaced with a finite computer arithmetic known as floating-point arithmetic (see figure 4.5). In essence, it involves replacing the real line by a "floating-point number line." As we see in figure 4.5(b), it is not really a line; this is why it is important to consider the role of roundoff error in mathematical representation. Those are mathe-

[7]They are thus key for dynamical simulations.

(a) Structure of single-precision floating-point numbers. There is a bit for the sign, 8 bits for the exponent, and the remaining 23 bits are for the fractional part.



(b) Discreteness of the floating-point "number line", with variable density.

Figure 4.5: Floating-point numbers.

matically simple examples of the sort of dialectic between the continuous and the discrete that puzzles many authors (*e.g.* Bell, 2005). All of these computational approximations are made because we can only execute finite, discrete operations. Computational error typically arises in steps (c), (d), and (f) of Euler's recipe (see section 2.2).

Now, let me return to Euler's recipe in order to identify the potential sources of modelling error and their nature. The first step includes the specification of the number and types of bodies (*i.e.*, mass-point particles, rigid bodies, continuously deformable bodies) that are part of the system. Two kinds of modelling error can be introduced here: we can neglect the presence of some bodies altogether, and we can assume that some bodies are simpler than they in fact are (*e.g.*, assuming that a body is rigid, that it is a point particle, or that a fluid is inviscid). It also includes the specification of a number of parameters, such as the kinematical constants (*e.g.*, mass, charge, shear stress, *etc.*) and the initial values of state variables. The former introduce systemic error and the latter introduce experimental error.

The second step involves a decision about which body-force laws will apply between bodies. For example, one can often suppose that gravitational effects or electromagnetic effects can be neglected. Moreover, this step involves the choice of constitutive equations, as well as the values of the phenomenological parameters they contain. A simple example would be the choice of Hooke's law $\mathbf{F} = -k\mathbf{x}$ for a spring; there is a source of error in the choice of the parameter

k, but also in the fact that springs are not exactly Hookean, since their stiffness is non-constant. At this stage again, we find both systemic and experimental error. Accordingly, it is steps (a) and (b) of the model construction procedure that we should focus on to understand modelling error.

Note that, to decide whether a model so constructed accounts for some set of phenomena, the solution has to be *effectively computed*, whether exactly or not. In other words, without effective computation, one cannot decide whether the model accounts for the phenomena, *i.e.*, one cannot determine what the observational consequences are. Moreover, it should be emphasized that, as a result of this requirement of effective computability, most situations involve a choice between further idealizing the assumptions contributing to the construction of the model and being able to solve the equations exactly, or having less idealized modelling assumptions and being forced to use computational methods that contain an error component.[8] This is why the computational aspects of science cannot be altogether ignored, if one wishes to adequately reconstruct the confirmational and explanatory aspects of science.

These considerations should provide a sufficient clarification of our guiding principle: the role of mathematics in science prescribes that *computational errors should be analyzable in the same terms as modelling and experimental errors*. By that we mean that if truncation, discretization, and roundoff errors are small compared to the modelling and experimental error, then for all we know, our approximate numerical answer might be the right one. In the case of Euler's recipe, that means that the error that occurs when solving the modelling equations should be reflected back into errors occurring in steps (a) and (b).

4.4 Backward Error Analysis

In this section, I describe a formal model that will allow us to identify the key problems and methods of error analysis. On this basis, I will explain how computational error can be physically interpreted. I will present things in

[8]This point is articulated more thoroughly by Batterman (2002*a*).

an abstract way, so that it has a generality sufficient for our purpose. It is important to recognize the generality of the method. The analysis extends to many problems in science and engineering, *e.g.*, function evaluation, polynomial equations, series algebra, root finding, numerical linear algebra, numerical quadrature, numerical differentiation, numerical solutions of ordinary differential equations, partial differential equations, partial differential equations and many others.[9]

To begin with, we represent a mathematical problem by an operator φ, that has an *input* (data) space $\mathscr{I}$ as its domain and an *output* (result, solution) space $\mathscr{O}$ as its codomain:

$$\varphi : \mathscr{I} \to \mathscr{O},$$

and we write $y = \varphi(x)$. If φ is a function, we use the symbols $f, g, \ldots$ as usual. But φ need not be a function; for instance, we can study problems involving differential and integral operators. That is, in other cases, both x and y will themselves be functions. Thus, for a given problem φ, the image y can have many forms. For example, if the reference problem φ consists in finding the roots of the equation $\xi^2 + x\xi + 2 = 0$, then for each value of x the object y will be a set containing two numbers satisfying $\xi^2 + x\xi + 2 = 0$, *i.e.*,

$$y = \left\{ \xi \mid \xi^2 + x\xi + 2 = 0 \right\}. \tag{4.1}$$

In general, we can then define a problem to be a map

$$x \xrightarrow{\varphi} \left\{ \xi \mid \phi(x, \xi) = 0 \right\}, \tag{4.2}$$

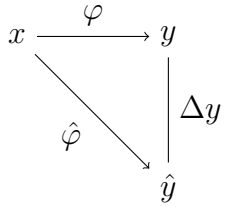
where $\phi(x, \xi)$ is some function of the input x and the output ξ (in the example above, $\phi(x, \xi) = \xi^2 + x\xi + 2$). The function $\phi(x, \xi)$ is called the *defining function* and the equation $\phi(x, \xi) = 0$ is called the *defining equation* of the problem. As another simple example, suppose one has two vectors $\mathbf{u}$ and

[9] I cannot go into the details of the analysis for such problems here. The interested reader can consult a survey in Corless & Fillion (201x).

$\mathbf{v}$, and that the problem is to find the Euclidean distance between them. The problem is represented by a map that sends the pair $(\mathbf{u}, \mathbf{v})$ to the value $\sqrt{(v_1 - u_1)^2 + (v_2 - u_2)^2}$. Another type of problem is the differentiation and integration of functions, where the problem is actually a linear operator $\mathscr{L}$ sending a function $u(t)$ to another function, *e.g.*, $\mathscr{L}(u) = u'(t)$. Yet another case would be the solution of differential equations, where the problem φ is a map sending an initial-value problem to its solutions (if it has one, of course); *e.g.*,

$$f : \left\{ \frac{du}{dt} = u, \quad u(0) = u_0 \right\} \mapsto \left\{ u = u_0 e^t \right\}.$$

Since φ is the problem we are interested with in the first place, we call it the *reference problem*. In many cases, however, we do not have a way to determine the exact solution y to the problem φ at our disposal; this happens in the cases described earlier. In this very typical case, one can construct a modified problem (using discretization, truncation, and roundoff) for which we can find an exact solution in a very efficient way. Accordingly, we introduce the notion of an *engineered problem* $\hat{\varphi}$ (which is by design computable). For some Δy, we obtain this commutative diagram:



(4.3)

The Δy is called the *forward error*, and is defined by $\Delta y = \hat{y} - y = \hat{\varphi}(x) - \varphi(x)$. Dividing by y gives the *relative* forward error, denoted δy. It represents the difference between the exact and the approximate solution. Accordingly, we can write both $\hat{y} \approx \varphi(x)$ or $\hat{y} = \hat{\varphi}(x)$. In this way, instead of saying that $\hat{y}$ is the *approximate solution to* φ, we can say that it is the *exact solution to* $\hat{\varphi}$. This allows us to emphasize that, instead of focusing on approximate truth, we focus on modified problems; then the investigation is turned into one of characterizing *nearness* of problems. Moreover, modified problems can be

thought of as resulting from model equations derived from slightly modified modeling assumptions.

Replacing the reference problem by an engineered problem can lead to surprisingly large forward error. To take a simple example, if we compute these sums on a pocket calculator, chances are that it will return different values:

$$
\begin{aligned}
s_1 &= 10^{20} + 17 - 10 + 130 - 10^{20} \\
s_2 &= 10^{20} - 10 + 130 - 10^{20} + 17 \\
s_3 &= 10^{20} + 17 - 10^{20} - 10 + 130 \\
s_4 &= 10^{20} - 10 - 10^{20} + 130 + 17 \\
s_5 &= 10^{20} - 10^{20} + 17 - 10 + 130 \\
s_6 &= 10^{20} + 17 + 130 - 10^{20} - 10
\end{aligned}
$$

The (wrong) answers will probably be 0, 17, 120, 147, 137 and -10. This example, however, is not conceptually of much interest, since we know the exact answer; it is 137. Let us examine more interesting cases.

In fact, it is surprising to many that this happens in very simple physical setups. A simple example arise from setups described by a simple homogeneous second-order linear differential equation, say $\ddot{x} + 20000\dot{x} + x = 0$, which could represent an oscillating mass attached to a Hookean spring immersed in a thick fluid occasioning large damping (here, 20000 would be the damping coefficient). Then a solution to this equation will have the form $x(t) = ce^{\lambda t}$, where λ is a root of the quadratic $x^2 - 20000x + 1 = 0$ and c is some constant. If we use the quadratic formula to find the roots on a calculator with standard precision, we find that one of the root returned is 0. However, it is not hard to figure out that true value is approximately $5 \cdot 10^{-5}$. The difference is small, and yet if we consider the difference between ce^{0t} and $ce^{5 \cdot 10^{-5} t}$ for large values of t, it can have major repercussions, as we see in figure 4.6. From this we can infer that the problem in question is sensitive to perturbations, since a small variation in the value of the eigenvalue λ can provoke a bifurcation.

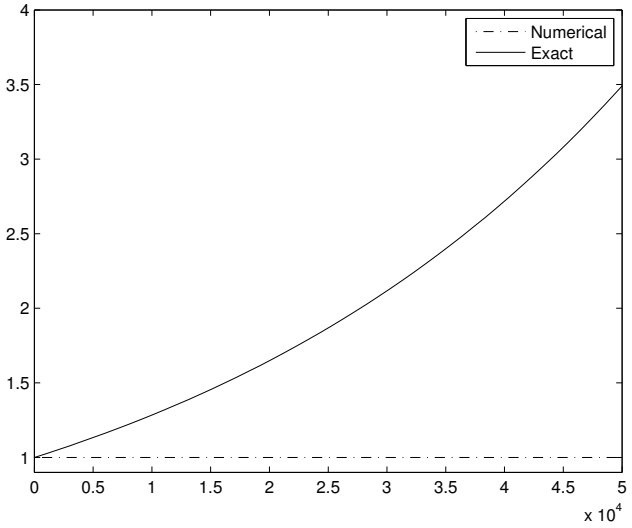


Figure 4.6: Important qualitative difference resulting from small change in an eigenvalue.

This example, however, is again of limited conceptual interest, since it is relatively easy to find the exact answer and use it as benchmark. But it is not so for many common problems arising in practice. Again, consider the example of the Airy function mentioned above. If we use a standard Taylor series computation in standard floating-point arithmetic to compute $f(-12.82)$, near the tenth zero, the absolute error is

$$\Delta y = |\mathrm{Ai}(x) - \mathrm{AiTaylor}(x)| = 0.002593213070374,$$

and the relative error is $\delta y \approx 0.277$, only accurate to two digits! Moreover, as we see in figure 4.4, the error grows very fast as $-|x|$ increases. Even if the series converges uniformly, the floating-point computation diverges.

Knowing that the forward error has a certain size, however, is not informative enough. Having a forward error as small as possible is a desideratum, but there remains the question of determining acceptance criteria: when is the forward error small enough to satisfy our modelling needs? This is why, in applications, it is also important to consider errors in x, the input data of the reference problem φ. This error can have many different sources, *e.g.*, error in

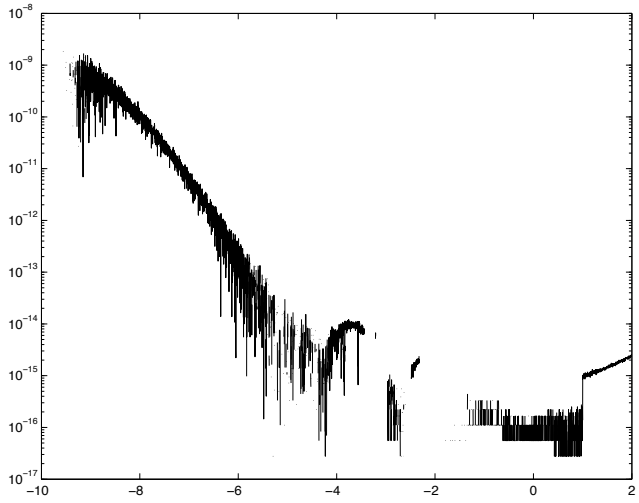
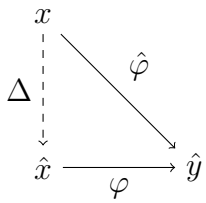


Figure 4.7: Error in the evaluation of the Airy function with respect to x.

preparation of the system, measurement of the data, and perturbations of the system. We thus define a quantity $\Delta x = \hat{x} - x$ that corresponds to the size of a modification of x. The smallest such Δx that makes this diagram commute,

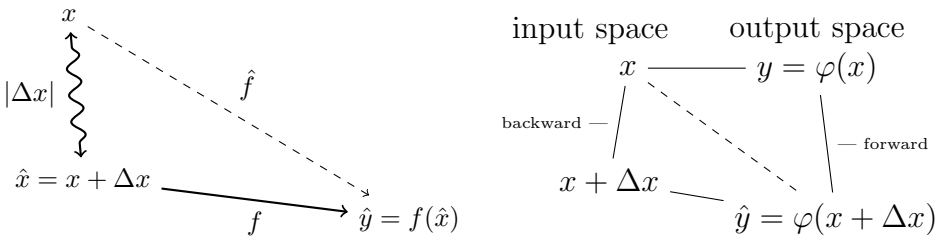


,

is called the *backward error*. As we can see in figure 4.8(a), we factor the map $\hat{\varphi}$ through $\hat{x}$ instead of through y (as was done equation 4.3). This is advantageous since in general we can exactly find or closely estimate Δx, even though we may have no direct information concerning the value of Δy.

Switching our focus from forward error to backward error gives rise to a very general and powerful method called *backward error analysis*.[10] The objective here is to explain the error in the computed solution $\hat{y}$ in terms of errors in the input x. In other words, we ask: *how much error in the input*

[10]The name "backward" comes from the fact that a crucial way of analyzing modelling in modern error theory consists in, so to speak, reflecting back the error Δy in Δx (Wilkinson, 1971).



(a) Backward error (b) The general picture

Figure 4.8: Backward error analysis: The general picture.

would be required to explain all *output error?* Formally, this happens when the diagram in figure 4.8(b) commutes. Thanks to this change of perspective, the central question is now:

> When we modified the reference problem φ to get the engineered problem $\hat{\varphi}$, for what set of data have we actually solved the problem φ?

If solving the problem $\hat{\varphi}(x)$ amounts to having solved the problem $\varphi(x + \Delta x)$ for a Δx smaller than the modeling error, then our solution $\hat{y}$ can be considered completely satisfactory.

On the basis of the presentation of section 2, the situation can be put in an even more suggestive way: if the computational error committed in the steps (c), (d), and (f) of Euler's recipe corresponds to a backward error smaller than the modelling error committed in the steps (a) and (b) of Euler's recipe, then our computed solution is as satisfactory as the modelling context can demand (no matter how large the forward error is). In such a case, we have successfully extracted the observational consequences from our model and we can use those numerical values to compare with observable phenomena.

The success of this formal model to analyze computational error in terms of modelling error is best illustrated with the case of initial-value problems. The *standard form of an initial value problem* is

$$\dot{\mathbf{x}}(t) = \mathbf{f}(t, \mathbf{x}(t)), \qquad \mathbf{x}(t_0) = \mathbf{x}_0, \tag{4.4}$$

where $\mathbf{x}(t) : \mathbb{R} \to \mathbb{C}^n$ is the vector-solution as a function of time, $\mathbf{x}_0 \in \mathbb{C}^n$ is

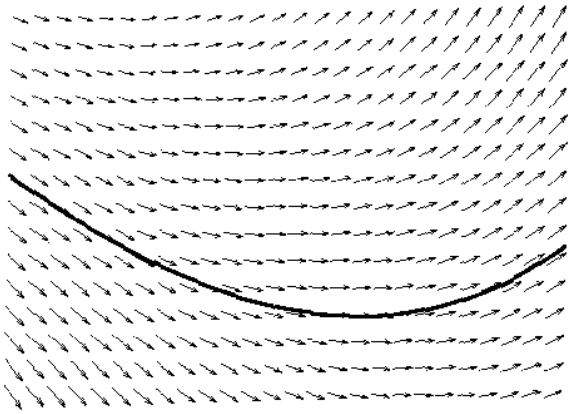


Figure 4.9: A vector field with a nearly tangent computed solution.

the initial condition, and $\mathbf{f}: \mathbb{R} \times \mathbb{C}^n \to \mathbb{C}^n$ is the function equal to $\dot{\mathbf{x}}$. For dynamical systems, $\mathbf{f}$ is a velocity vector field (or slope field) and $\mathbf{x}$ is a curve in phase space that is tangent to the vector field at every point (see figure 4.9). Typically, the solution of this problem will not be directly computable. In this situation, we then resort to some numerical procedure to solve the differential equation. In accord with the formal model proposed, let $\hat{\mathbf{x}}(t)$ be the solution of an engineered problem (say, the map computed by RK45—a fourth-order Runge-Kutta method known as the Dormand-Prince method—that we would denote $\hat{\varphi}$ here). The backward error turns out to be given by the expression $\Delta(t) = \dot{\hat{\mathbf{x}}} - \mathbf{f}(t, \hat{\mathbf{x}}(t))$. As a result, we can express the original problem in terms of a modified, or perturbed problem, so that our computed solution is an *exact* solution to this modified problem[11]:

$$\mathbf{z} = \mathbf{f}(t, \mathbf{z}) + \Delta(t).$$

From the point of view of dynamical systems, the backward error measures how far from satisfying the differential equation our computed trajectory $\hat{\mathbf{x}}(t)$ is, *i.e.*, how close it is to being tangent to the vector field. In figure 4.9, we see a trajectory that is nearly tangent to the vector field. In an even more suggestive way, we can say that the backward error allows us to find to which

[11] In Corless & Fillion (201x), we call such an equation in $\mathbf{z}$ a *reverse-engineered* problem.

perturbed vector field our computed solution is tangent. Thus, as $\Delta(t)$ is a small homogenous quantity, we can think of it as a modelling error, say a wind blowing on the system, or a small gravitational attraction from a distant body, or a measurement error on some parameters. This is the key point that underlies the claim that the formal model provides measures of computational errors that are directly interpretable in terms of modelling error.

Now, the next question is: *what is the relationship between the forward and the backward error*? The relationship we seek lies in a *problem-specific*[12] coefficient of magnification, *i.e.*, the sensitivity of the solution to perturbations in the data, that we call the *condition of the problem*. The *normwise relative condition number* κ is the supremum of the ratio of the relative change in the solution to the relative change in input, which is expressed by

$$\kappa_{rel} = \sup_x \frac{\|\delta y\|}{\|\delta x\|} = \sup_x \frac{\left\| \dfrac{\Delta y}{y} \right\|}{\left\| \dfrac{\Delta x}{x} \right\|} = \sup_x \frac{\left\| \dfrac{\varphi(\hat{x}) - \varphi(x)}{\varphi(x)} \right\|}{\left\| \dfrac{(\hat{x} - x)}{x} \right\|}$$

for some norm $\|\cdot\|$. As a consequence, we can show that the relation

$$\|\delta y\| \leq \kappa_{rel} \|\delta x\| \tag{4.5}$$

holds between the forward and the backward error. We clearly see from this inequality that the condition number acts as a magnifying factor of the error in the data. Knowing the backward error and the condition number thus gives us an upper bound on the forward error. If κ has a moderate size, we say that the problem is *well-conditioned*. Otherwise, we say that the problem is *ill-conditioned*.[13] Thus, if the problem is well-conditioned, *i.e.*, $\kappa \approx 1$, then the error in the solution cannot possibly be much larger than the error in the data. In such a case, we can conclude that our strategy provides a solution

[12] Well-conditioning must be distinguished from the concepts of stability and sensitivity of a problem-solving method. The intuitive idea of *numerical stability* is similar to that of conditioning, but it is a property of methods rather than problems.

[13] Infinitely ill-conditioned problems are known as ill-posed problems in analysis, following Hadamard. See Earman (1986) for a discussion in the philosophical literature.

that is just as good as the exact solution to the reference problem, even if this solution is unknown. This gives us a tremendous epistemological insight of the semantical value of our solution.

The condition number, depending on the context, will be given by mathematical quantities such as vector and matrix norms, Lipschitz constants, Gröbner functions, Lyapunov exponents, and other coefficients of sensitivity/stability commonly used in perturbation theory. As a result, not only is the measure of computational error directly interpretable in terms of modelling error, but the analysis of the quality of solutions mirrors the standard methods of perturbation theory for dynamical systems, including systems studied in physics, chemistry, biology, economics, *etc.*

Finally, a last technical point. To automate error analysis, we use the concept of residual. In general, the forward error and the backward error cannot be assessed directly by computational means. The important point is that the residual is *always computable*. Consider two canonical examples of 'residuals':

1. $\mathbf{r} = \mathbf{A}\hat{\mathbf{x}} - \mathbf{b}$ in numerical linear algebra;

2. $\Delta(t) = \hat{x}'(t) - f(t, \hat{x}(t))$ in numerical solution of ordinary differential equations.

Based on those two paradigmatic cases, and on the definitions given in equation 4.1 and 4.2, I formulated a general definition of residual in Fillion (2011). Given the reference problem φ—whose value at x is a y such that the defining equation $\phi(x, y) = 0$ is satisfied—and an engineered problem $\hat{\varphi}$, the residual r is defined by

$$r = \phi(x, \hat{y}). \tag{4.6}$$

As we see, we obtain the residual by substituting the computed value $\hat{y}$ (*i.e.*, the exact solution of the engineered problem) for y as the second argument of the defining function. Since $\hat{y}$ is computable by design, it follows that the residual is always computable. Note that strategies of error analysis based

on the residual differs from the strategies based on the backward error and the forward error. On the one hand, an error analysis attempting to validate results by providing bounds on the backward or forward error does so *a priori*. On the other hand, one can just use some numerical solution to a problem, and then use this computed answer to determine if it is good. This sort of analysis is called *a posteriori*, and it is in general easier to use and to implement in computer software.

From these considerations, we thus obtain a complete procedure to assess the quality of numerical solutions:

1. For the problem φ, use an engineered version of the problem to compute the value $\hat{y} = \hat{\varphi}(x)$.

2. Compute the residual $r = \phi(x, \hat{y})$.

3. Use the computed value of the residual to obtain an estimate of the backward error (*i.e.*, reflect the residual back as a perturbation of the input data).

4. Determine how satisfactory the computed solution is. Answer in one of two ways:

   (a) If you do not require an assessment of the forward error, but only need to know that your have solved the problem for small enough perturbation Δx, conclude that your solution is satisfactory if the backward error (reflected back from the residual) is small enough.

   (b) If you require an assessment of the forward error, examine the condition of the problem. If the problem is well-conditioned and backward error is small in comparison to the modelling error, then conclude that the computed solution is satisfactory.

In Corless & Fillion (201x), we have called this procedure residual-based *a posteriori* error analysis. It explains how one can implement measures of errors and interpret them as perturbations of the vector field supposed by the formulation of the problem. As a result, we obtain a precise notion of nearness

of problems for the physical cases posed in terms of dynamical systems. The key point is that the resulting analysis makes explicit the relation between the qualitative aspect of mathematical representation perturbation-theoretically characterized in terms of bifurcations and its quantitative aspect. This establishes in a rigorous way that evaluation of mathematical representation is not limited to the search for an exact model. Rather, it is about characterizing classes of models by perturbation methods.

4.5 Summary

In summary, this chapter has made the following points:

C4.1 The epistemological obstacles between solutions and computed values cannot be overlooked in order to understand the role of mathematics in the natural sciences. Moreover, the existence of exact solutions to problems does not imply that approximations can be ignored, since perturbations and approximations can affect the type of an exact solution, and thereby affect its possible use.

C4.2 To understand the role of computation in science requires a notion of mathematical tractability that complements the notion of effective computability. By allowing us to "compute typically uncomputable quantities," the stratagems devised by applied mathematicians allow us to grasp the consequences of selecting given sets of modelling assumptions.

C4.3 There are four types of error in the construction and evaluation of a model. A computed solution can be considered as good as an exact (but typically unknown) solution if the computational error is small in comparison to the modelling error.

C4.4 Backward error analysis is a general perspective that explains how to relate computational and modelling error. Instead of focussing on approximate solutions to a reference problem, it focussed on exact solutions of modified problems. Their quality can be assessed by estimating an

equivalent perturbation of the reference problem and by analyzing the sensitivity to perturbations.

As we see, thanks to this way of interpreting computational error, there is a perfect parallel between the way in which error is managed in modelling and in computational contexts. This completes our discussion of the strategies implemented to manage error in the logic of mathematical modelling.

Chapter 5

Conclusion: Demystifying the Applicability of Mathematics

This dissertation aimed to demystify the so-called 'unreasonable effectiveness of mathematics in the natural sciences' by showing that it is very reasonable after all. That is, it is reasonable provided that we give ourselves the means to properly rationally reconstruct the successful practices in applied mathematics and the natural sciences. In particular, to discuss whether mathematics is effective in a way that is reasonable or unreasonable, it is essential to properly capture what is the effectiveness that we are meaning to characterize. In chapter 1, I have distinguished three different senses in which one could claim mathematics to be effective, and I have argued that each of them demand a different type of answer; in fact, the three of them are intermingled in Wigner's paper. As I have argued, the problem of uncanny accuracy is particularly important, since it is one thing for mathematics to be a language sufficiently expressive to describe nature, but it is another one to explain that it gives us means to discriminate among many possible uses of the mathematical language to describe nature.

In fact, understanding the effectiveness of mathematics demands an account of the way in which mathematics is used in practice to actually and successfully describe the world (otherwise, we only have only an account of the use of mathematics, not of its *effective* use).

A characterization of the way in which mathematics is used to describe the world must contain two parts, which I jointly refer to as the 'logic of mathematical modelling': (1) a characterization of the patterns of reasoning used to generate mathematical expressions meant to represent systems and (2) a characterization of the methods used to justify the validity of the claim that a set of mathematical expressions represent systems. To the extent that this succeeds, we have an argument that addresses Wigner's challenge to explain that "mathematical language has more to commend it than being the only language which we can speak" and that "it is, in a very real sense, the correct language" (Wigner, 1960 : p. 8). As I explained in chapter 1, the problem of uncanny accuracy is addressed by providing a concept of mathematical fitness that explains the circumstances in which and the process by which mathematical models effectively represent systems.

As I argued, the use of mathematical representations, including their generation and the examination of their consequences, involves the following steps: identifying a behaviour of interest in a real system, selecting modelling assumptions (including the selection of a scale), deriving model equations from modelling assumptions within a theory, extracting the relevant qualitative and/or quantitative information from model equations and, finally, using this information to answer our questions about the behaviour of interest. However, as I have stressed, effective modelling strategies necessarily rely on various gambits that further idealize systems in many ways in order to make model equations tractable. If the model equations are not tractable, the strategies will not be effective, since they will not give us grounds to answer our questions about the behaviour of interest. But then, there would be no effective use of mathematics to discuss at all! Thus, it is essential to describe how good modelling practices seek a balance between the completeness and accuracy of modelling assumptions and the tractability of model equations. Accordingly, mathematical modelling is essentially about strategies for the management of information based on the premise that only essential information should be included in the model, and that superfluous information should be discarded.

The key challenge to understanding the modelling gambits that crucially

contribute to the success of applied mathematics is to characterize the sense in which mathematical representations should be considered good. There is an essential pragmatic dimension to it, since one cannot say whether a representation is good or not without knowing what it is supposed to do; thus, the evaluation of mathematical representations depends on what questions on the behaviour of interest are addressed, and not only on their fit with the system. Moreover, the fit in question should not be understood in terms of truth, but rather in terms of accuracy. As a result, a mathematical model is a good representation of a system if it is selectively accurate.

To determine effectively whether a mathematical model is selectively accurate, *i.e.*, to have effective means for assessing a model, it is essential to effectively extract the relevant information from the model equations. This can be done is many ways, depending on the sort of information sought. As I have explained, it is sometimes done using qualitative methods and sometimes it is done using quantitative methods. Our examination of the virtues of exact and numerical solutions has given us ground to conclude that, in both the qualitative and the quantitative case, asymptotic reasoning and perturbation analysis is playing the essential role for the justification of the methods of extraction of information. As a result, it is only possible to understand the rationality of the semantics of mathematical modelling by appealing to the notion of perturbation, *i.e.*, by addressing the reasons for which different systems—sometimes extremely different—can be represented by the very same model equations.

Now, what does this imply for the version of the problem of the applicability studied in this dissertation, the problem of uncanny accuracy? The problem was formulated in chapter 1 as follows: Given that the construction and manipulation of mathematical representation is pervaded by uncertainty, error, and approximation, how can their apparently uncanny accuracy be explained? The answer we have reached, then, is that even when constructed representations are based on assumptions that happen to be incorrect (whether we know it or not), they can be perfectly satisfactory, *i.e.*, selectively accurate. From this point of view, the success of mathematics in the natural sciences is

not so much to be attributed to the richness of its expressive means and to the breadth of possible structures that it can study, but rather to its capacity to indicate when error and uncertainty might lead us astray, and when it does not. Qualitatively, this means that mathematics can tell us when a representation is robust under perturbations. In particular, bifurcation analysis tells us in what regions of application perturbations will not change the qualitative behaviour of the system, and near what values of parameters small error and uncertainty will possibly have dramatic effects. Quantitatively, this means that mathematics gives us the means to perform sensitivity analysis that will reveal just how much numerical figures can be trusted.

In one sentence, the explanation is thus: the success of mathematics in the natural sciences is due not only to the fact that it has virtually unlimited expressive resources, but also to the fact that it has the effective resources to determine when we can reach sound conclusions from mistaken assumptions. As we see, there is nothing unreasonable about it. Thus, we have successfully demystified the effectiveness of mathematics in the natural sciences, insofar as it is understood as a problem about the uncanny accuracy of mathematics.

Appendix A

The "theory of measurement"

The *theory of measurement* is the theory that is meant to provide a systematic treatment of the subject of measurement. However, it appears that this theory deals with a notion of measurement that differs significantly from that provide by official documents establishing standards for the scientific method (International Organization for Standardization, 2004; Joint Committee for Guides in Metrology, 2008, 2009; Taylor & Kuyatt, 1994). In fact, what is predominantly referred to as 'the theory of measurement' in the philosophical literature only incidentally consider the notions of error and uncertainty; rather it provides a general framework to discuss the way in which measurement can be justified, in an abstract way that is based on mathematical *representation theorems*. This theory's usefulness is claimed to come primarily from the criteria of meaningfulness[1] it provides. For instance, it provides criteria to determine when arithmetic quantities can be added, multiplied, *etc.*, in a meaningful way. It can also provide logical grounds for deciding when sentences of the type "A temperature of 40°C is twice as hot as a temperature of 20°C" are meaningful and, if so, true. Suppes & Zinnes (1962) identify the two fundamental problems for the analysis of arbitrary procedures of measurement. The first is the *representation problem*; it seeks justification of the assignment of numbers to objects or phenomena. The second is the *uniqueness problem*; it seeks a

[1] In the terms introduced in chapter 1, it answers the problem of mixed sentences by providing criteria for their interpretability.

specification of the degree to which this assignment is unique.

The name for these problems comes from the technical apparatus used to discuss arbitrary procedures of measurement. Concerning the first problem, the justification of the assignment of numbers to objects or phenomena is taken to require showing that some mathematical structure has the same structure as an empirical structure. For instance, for natural-number-valued measurements, one would show that the arithmetic of natural numbers and an empirical structure have the same structure. "Having the same structure" is capture using the notions of isomorphism or homomorphism. From this point of view, the significance of an isomorphism result is that "we may then use our familiar computation methods, applied to arithmetical structure, to infer facts about the isomorphic empirical structure" (Suppes & Zinnes, 1962 : p. 4). The formal apparatus of the theory of measurement thus understood is based on the Tarskian notion of relational system. A relational system $\mathfrak{M}$ is an order finite $(n+1)$-tuple $\langle A, R_1, \ldots R_n \rangle$, where A is the *domain* of the relational system and $R_1, \ldots, R_n$ are relations on A. If R_i is an m_i-ary relation, we can then define the *type* of the relational system by the n-tuple $\langle m_1, \ldots, m_n \rangle$. The key notion of isomorphism can then be defined as follows: Two relational structure $\mathfrak{M}_1 = \langle A, R \rangle$ and $\mathfrak{M} = \langle A', R' \rangle$ of type $\langle 2 \rangle$ are *isomorphic* if and only if there is a one-one function $f : A \to B$ such that, for all $a, b \in A$, $R(a, b)$ iff $f(a)R'f(b)$. If we wish to assign the same number to two different objects, the one-one requirement is dropped and the systems will then be said to be homomorphic.

A measurement does not relate any two relational system, but rather it relates one *empirical* structure and one *numerical* structure. A numerical structure is a relational system whose domain A is a subset of $\mathbb{R}$. An empirical structure is such that its domain is a set of identifiable entities, such as weights, persons, statements, sounds, *etc* (Suppes & Zinnes, 1962 : p. 9). A similar distinction holds for relations. However, not any numerical system should be chosen in order to satisfyingly solve the representation problem. An "appropriately chosen numerical relational system" is one such that it is both simple and familiar. This desideratum cannot be formulated precisely; it is

scale type	admissible transformation
absolute	identity
ratio	similarity (multiplication by $c \in \mathbb{R}^+$)
difference	translation
interval	positive linear
order	monotone
classificatory	arbitrary one-one
nominal	arbitrary one-one

Table A.1: Example of scales and admissible transformations.

said to be "elusive" (Suppes & Zinnes, 1962 : p. 11).

The problem of uniqueness studies the degree to which measurements are unique. More specifically, the problem consists in determining what scale type results from some procedure. The scale type will then determine up to what kind of transformation the measurement is unique. Scales are defined as follows. Let $\mathfrak{M}$ be an empirical relational system and let $\mathfrak{N}$ be a full (*i.e.*, $A = \mathbb{R}$) numerical relational system. Let $f : \mathfrak{M} \to \mathfrak{N}'$ be a homomorphism, where $\mathfrak{N}'$ is a subsystem of $\mathfrak{N}$. Then, the triple $\langle \mathfrak{M}, \mathfrak{N}, f \rangle$ is called a *scale*. The problem is then to characterize what transformations of the scales can be applied in a way that preserves representation; examples of scales and their corresponding admissible transformations are in table A.1. There is a nondenumerably infinite number of types of scales, but these are the most familiar. Note that the analysis of scales does not depends on the existence of some actual empirical relations or processes.

Different types of measurement are defined in this theoretical framework: fundamental, derived, pointer, *etc.* In order to grasp the conception of measurement at the center of this theory, let us consider the definition of fundamental measurement provided by Suppes & Zinnes (1962 : p. 30): A *fundamental measurement* of a set A with respect to the empirical system $\mathfrak{M}$ involves the establishment of a fundamental numerical assignment for $\mathfrak{M}$, or in other words, involves the establishment of a representation theorem for $\mathfrak{M}$. Note that it is not a *parameter* which is measured, but a *set*. There also is a theory of derived measurement, which I won't review.

The notion of measurement that comes closest to the common idea of measurement involving a measuring device in a laboratory is that of *pointer measurement*. A pointer measurement is a numerical assignment which is based on the direct readings of some validated instrument. For such a measurement, it is necessary to be able to constructed a validated instrument; in order to do so, one must utilize some established empirical laws and theories. For instance, Hooke's law and the theory of gravitation for the measurement of mass or weight; then a spring that satisfies Hooke's law within the desired accuracy—under "standard" conditions of temperature, humidity, *etc.*—is used to build the instrument. Finally, the instrument is calibrated by determining which amount of mass would correspond to which elongation of the spring (you do it for two weights and then make equal separations in between). This task, as well as the calibration of the pointer device, is deemed unimportant for the theory of measurement, as there is no need to go in the details. It is deemed a "practical problem" (Suppes & Zinnes, 1962 : p. 37).

In terms used by Tal (2011), these so-called theories of measurement do not treat the concepts of error and uncertainty as intrinsic. As such, they are different from the methodological analyses that belong to the theory of measurement as it is envisaged by physicists, chemists, biologists, *etc*, and other experimental sciences, which I review in section 3.2. It just cannot be seen in any plausible way as reconstructing the task that scientists actually *do* have to do if they want to get any decent models at all. However, very little has been said in the philosophical literature on the theory of measurement thus understood.

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1 Areas of Specialization

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3 Education

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Book

R.M. Corless & N. Fillion (forthcoming, under contract). *A Graduate Survey of Numerical Methods*, Springer: New York. (≈850pp.)

We are currently making the final revisions to the manuscript. The book is likely to be split in two volumes, at our editor's request.

Articles

1. M. Kao, N. Fillion & J.L. Bell (2010). "Critical Study of Jean-Pierre Marquis: *From a Geometrical Point of View: A Study of the History and Philosophy of Category Theory*," *Philosophia Mathematica*, 18(2): pp. 227–234.
2. N. Fillion (2008). The Kolmogorov-Gödel Translation of Classical Arithmetic into Intuitionistic Arithmetic. In: A. Cupillari (Ed.), *Proceedings of the Canadian Society for History and Philosophy of Mathematics, Vancouver, June 2008*: pp. 77-88.
3. Fillion, N. (2007). La méthode axiomatique et la philosophie. *Phares*, 7: 36-53.
4. Fillion, N. (2006). Épistémologie et sociologie de la connaissance. *Phares*, 6: 193-213.

Some of my work in progress is described in the Research Statement.

8 Talks

1. "Backward-Error Analysis Revisited," 30th Southern Ontario Numerical Analysis Day: SONAD 2012, Department of Computer and Mathematical Sciences, University of

Toronto, 11 May 2012.

2. "Modelling Error in Mathematical Representation," PGSA Colloquium Series, Department of Philosophy, the University of Western Ontario, 30 February 2012 (non-referreed contribution).
3. with Robert Corless, "Computation and Explanation," The Plurality of Numerical Methods and their Philosophical Analysis, Institut d'histoire et de philosophie des sciences et techniques, Université Paris-I Panthéon-Sorbonnes, 3-4 November 2011.
4. with Robert Moir, "Explanation and Abstraction: The Case of Backward Error Analysis," Philosophy of Science Association (PSA), Montréal, November 2010.
5. "Clinical Equipoise and the Ethics of Adaptive Trials," Canadian Society for the Study of Practical Ethics (CSSPE), Concordia, Montréal, 30 May-1 June 2010.
6. with Robert Moir, "Modeling and Explanation: Some Lessons from Modern Error Theory," Canadian Society for History and Philosophy of Science (CSHPS), Concordia University, Montréal, 28-30 May 2010.
7. R.W. Batterman, N. Fillion, R. Moir, J. Overton (2010). "Idealization in Scientific Explanation," Western Research Day, March 24, 2010 (poster).
8. with Robert Moir, "A Step Forward with Backward Error," PGSA Colloquium Series, Department of Philosophy, the University of Western Ontario, 10 March 2009 (non-referreed contribution).
9. "Aristotle's Logic: A Comparison of Łukasiewicz's and Corcoran-Smiley's Reconstructions," Buffalo Logic Colloquim, Department of Philosophy, the University at Buffalo, State University of New York, 16 October 2009 (invited).
10. "Two Traditions in Logic," PGSA Colloquium Series, Department of Philosophy, the University of Western Ontario, 14 October 2009 (non-referreed contribution).
11. "Conséquences observationnelles en mécanique des continua," $77^{\text{ième}}$ Congrès de l'ACFAS, University of Ottawa, 11-15 May 2009.
12. "Logique aristotélicienne: Ontologie Formelle ou Épistémologie Formelle?," $77^{\text{ième}}$ Congrès de l'ACFAS, University of Ottawa, 11-15 May 2009.
13. "Explanation in Phenomenological Theories of Physics," University of Waterloo Philosophy Graduate Colloquium, 24 October 2008 (invited).
14. "The Kolmogorov-Gödel Translation of Classical Arithmetic into Intuitionistic Arithmetic," Canadian Society for History and Philosophy of Mathematics (CSHPM), University of British Columbia (UBC), Vancouver, 1-3 June 2008.
15. "Aristotle's Logic and its Modern Reconstructions," Canadian Society for History and Philosophy of Science (CSHPS), University of British Columbia (UBC), Vancouver, 3-5 June 2008.
16. "The Semantics of Conditionals," 15^{th} University of Waterloo PGSA Conference, Department of Philosophy, University of Waterloo, 9-10 April 2008.
17. "Intuitionism and Logicism on the Foundations of Arithmetic," PGSA Colloquium Series, Department of Philosophy, the University of Western Ontario, 30 February 2008 (non-referreed contribution).
18. "La distinction fregéenne sens/référence et les conditions de possibilité de la métathéorie," $75^{\text{ième}}$ Congrès de l'ACFAS, Université du Québec à Trois-Rivières, 7-11 May 2007.
19. "Aristotelian and Modern Logic," 4^{th} Annual GPSA Colloquium, Concordia University, 4-5 May 2007.
20. "L'axiomatique: Théorie Générale des Structures Conceptuelles," Colloque étudiant, Université Laval, 28 April 2006.

9 Awards and Distinctions

***Competitive Research Awards* (from national or provincial competitions)**
Schmeelk Canada Foundation
Richard J. Schmeelk Canada Fellowship ($40,000), 2009-2011

Social Sciences and Humanities Research Council of Canada
Doctoral Fellowship ($20,000), Declined, 2009-2010

Ontario Graduate Scholarship
Doctoral Fellowship ($15,000), Declined, 2009-2010

Fonds Québécois de la Recherche sur la Société et la Culture
Doctoral Fellowship ($60,000), 2006-2009

Ladislaw-Goncarow Foundation
Ladislaw-Goncarow Scholarship to study in Russia (app. $6,500), 2003

Departmental Research Awards
The University of Western Ontario
Applied Mathematics ($7,131), 2010-2011
Philosophy ($28,524), Partly declined, 2006-2010

Université Laval
Admission Scholarship for the M.A. Degree in Philosophy ($500), 2004

Academic Award
Collège Mérici
Bourse d'Excellence du Collège Mérici. Two-year scholarship met for achieving 4$^{\text{th}}$ place at the provincial competition of chemistry/biology (app. 3,200$), 1998

Teaching Award
The Society of Graduate Students, the Graduate Teaching Assistants' Union, and the School of Graduate and Postdoctoral Studies at UWO
Graduate Student Teaching Award Nominee, 2008-2009

10 Teaching Experience

Courses taught: 5
Courses TAed: 13

Lecturer & Instructor

The University of Western Ontario **2008-2012**

- Philosophy of Science, 2012
- Basic Logic (6-week intensive equivalent to a full-year course), 2010
- Introduction to Philosophy (Full-year Course), 2008–2009

Teaching Assistant

The University of Western Ontario **2006-2010**

- Numerical Methods (graduate course in applied mathematics), 2010
- Introduction to Logic (First half of the full-year Course), 2009
- Critical Thinking (Full-year Course), 2007–2008
- Biomedical Ethics (Full-year Course), 2006–2007

Russian State University for Humanities **2004-2005**

- French for Philosophers, 2004, 2005
- French Culture in North America, 2005

Université Laval **2003-2006**

- Introduction to Philosophy of Science, 2004, 2006
- Analytic Philosophy of Language, 2006
- Philosophy of Knowledge, 2005

Guest Lectures

1. "Computation in Scientific Explanation," in the course *Contemporary Philosophy of Science* (Andrew Wayne), Department of Philosophy, University of Guelph, 17 November 2010.
2. "Basic Concepts of Game Theory," in the course *Decision Theory* (Brian Woodcock), Department of Philosophy, University of Western Ontario, 28 March 2006.
3. "Le réalisme épistémologique de Karl Popper," in the course *Introduction à l'Épistémologie des Sciences* (Daniel Descroches), Faculty of Philosophy, Université Laval, 19 March 2003.
4. "Induction, Vérification et Falsification," in the course *Histoire des Sciences* (Luc Tremblay), Département d'Histoire et Civilisations, Collège Mérici, 2 November 2002.

11 Research Experience

Research Fellow

University of Western Ontario **2013**

- Postdoctoral researcher jointly appointed in applied mathematics (work for David J. Jeffrey and Robert M. Corless) and in statistics and actual sciences (work for David Bellhouse)

University of Pittsburgh **2011**

- Visiting Scholar (hosted by Robert W. Batterman)

Research Assistant

The University of Western Ontario **2007-2010**

- Department of Applied Mathematics, 2010
- Rotman Canada Research Chair in Philosophy of Science, 2009-2012
- The Joseph L. Rotman Institute of Philosophy, 2009
- Science, Epistemology and Ethics Research Lab, 2007–2008

Université Laval **2002**

- Center for Optics, Photonics, and Laser (COPL)

12 Professional Activities

Referee

- Society for Exact Philosophy, 2011
- Canadian Philosophical Association Annual Meeting, 2010, 2011
- Logic, Mathematics, and Physics Graduate Philosophy Conference, 2008, 2009, 2010, 2011, 2012

Conference Organization

- Logic, Mathematics, and Physics Graduate Philosophy Conference
  Department of Philosophy, University of Western Ontario
  2010: Co-organizer with E. Doyle. Keynote Speaker: Kevin Kelly (Carnegie Mellon)
  2009: Organizer. Keynote Speaker: David Malament (UC Irvine)
  2008: Organizer. Keynote Speaker: Mark Wilson (Pittsburgh)
- Philosophy Graduate Students Association Colloquium Series
  2008-2009: Co-organizer with K. Biniek.
  2007-2008: Organizer.

Academic Committees

- Steering Committee, Graduate Representative, 2009-2010
  Joseph L. Rotman of Science and Values, University of Western Ontario
- Graduate Program Committee, Graduate Representative, 2005-2006
  Faculty of Philosophy, Université Laval
- Association des Chercheurs en Philosophie, Vice-president, 2005-2006
  Faculty of Philosophy, Université Laval
- Faculty Administration Board, Graduate Representative, 2003-2004
  Faculty of Philosophy, Université Laval
- Association des Étudiants(es) Prégradués(ées) en Philosophie, President, 2001-2002
  Faculty of Philosophy, Université Laval
- Faculty Administration Board, Undergraduate Representative, 2001-2002
  Faculty of Philosophy, Université Laval

13 Graduate Coursework

(c): for credit; (a): Audit; (s): Sitting in (unofficially)
Courses for credit: 20
Courses audited and sat in: 15

University of Western Ontario, Department of Philosophy

(s) Symmetry in Philosophy and Physics, C. Smeenk	F2012
(s) Advanced Topics in Logic, J. L. Bell	W2012
(a) Philosophy of Applied Mathematics, R. Batterman	W2010
(c) Prospectus Course (Explanation in Modern Error Theory), R. Batterman	S2009

(c) Category Theory, J. L. Bell W2009
(c) Topics in Bioethics, C. Weijer W2009
(a) Aristotle's Philosophy of Science, D. Henry W2009
(a) Explanation & Evidence, C. Smeenk F2008
(c) Mathematical Idealizations in Physics, R. Batterman W2008
(c) The Continuous, the Discrete, and the Infinitesimal, J. L. Bell W2008
(a) Foundations of Relativity Theory, C. Smeenk W2008
(a) Topics in Analytic Philosophy (Math), W. Demopoulos W2008
(c) Conditionals, W. L. Harper F2007
(a) Philosophical Foundations of Modern Physics, R. DiSalle F2007
(c) Constructivity in Mathematics and its Underlying Philosophy, J. L. Bell W2007
(c) Game Theory, W. L. Harper W2007
(c) Aristotelian Logic, J. Thorp W2007
(a) Contemporary Analytic Philosophy II, W. Demopoulos W2007
(a) Contemporary Analytic Philosophy I, W. Demopoulos F2006
(c) Philosophy of Mathematics, J. L. Bell F2006
(c) Philosophy of Probability, I. Pitowsky F2006
(c) Survey of Philosophy of Science, W. Myrvold F2006

University of Western Ontario, Department of Applied Mathematics
(c) Applied and Computational Complex Analysis, R. Corless F2010
(c) Asymptotics and Special Functions, R. Corless W2010
(c) Mathematical Methods for Engineers, P. Yu W2010
(c) Advanced Numerical Analysis, R. Corless F2009

University of Western Ontario, Department of Mathematics
(c) Foundations of Mathematics, M. A. Dawes F2007

University of Pittsburgh, Deparment of Philosophy
(s) Topics in Philosophy of Physics, G. Valente F2011
(s) Indeterminism, Branching Time and Branching Spacetime, N. Belnap W2011

Carnegie Mellon University, Department of Philosophy
(s) Philosophy of Mathematics, W. Sieg F2011
(s) Seminar on Formal Epistemology, K. Kelly W2011

Université Laval, Department of Philosophy
(c) Épistémologie de l'économie classique (Reading Course), O. Clain S2004
(c) Philosophie analytique, R. Bilodeau W2004
(c) Grammaire générative et transformationnelle du rêve freudien, F. Tournier W2004
(c) Épistémologie des sciences humaines, F. Tournier F2003

14 Technical Skills

Mathematical Programming in MATLAB and MAPLE. I am comfortable with writing programs to run discrete-time and continuous-time mathematical simulations of situations arising in many disciplines. My background in numerical analysis also gives me the means to guarantee the validity of simulation results.

Web Programming in HTML & PHP (with some MySQL)

I designed many fully integrated interative websites, which include the Rotman Science, Epistemology and Ethics Research Institute's website, the University of Western Ontario Philosophy of Science Research Group's website, the BIPED's (Biology, Philosophy and Evolution Discussion group) website, many conference websites, many course websites for different professors, as well as my personal website.

Technical Workshops & Lectures

- (With A. Botterell) "Websites," Workshops for Graduate Students in Philosophy 2008-2009, Department of Philosophy, The University of Western Ontario, January 2009.
- "Typesetting in LaTeX" Informal two-session workshop for faculty members and graduate students, Department of Philosophy, The University of Western Ontario. September 2007, February 2010.

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