Prologue

The quantitative data obtained in any physical experiment are recorded as finite, ordered sets of rational numbers. All such sets are *discrete*. However, when a physicist sits down to make sense of such data, the tools he or she employs are generally based upon the *continuum*: analytic (or at least smooth) functions, differential equations, Lie groups, and the like. It is the view of many eminent mathematicians that '*bridging the gap between the domains of discreteness and of continuity* . . . is a central, presumably even *the* central problem of the foundations of mathematics',[1] yet Fritz London did not seem to have had the slightest hesitation in writing, in the very first paragraph of his book on superfluidity,[2] 'that *new differential equations* were required to describe [the observed behaviour of]. . . "superfluid" helium. . . ' The physicist had stepped over the gap which has occupied philosophers for two millenia without even noticing that it existed![3]

This gap is but a fragment of one that separates theoretical from experimental physics. Some of the most important physicists of the first half of the twentieth century have expressed themselves on the subject, and it is instructive to compare their views. Dirac, for example, had the following to say:[4]

> The physicist, in his study of natural phenomena, has two methods of making progress: (1) the method of experiment and observation, and (2) the method of mathematical reasoning. The former is just the collection of selected data; the latter enables one to infer results about experiments that have not been performed. There is no logical reason why the second method should be possible at all, but one has found in practice that it does work and meets with reasonable success. This must be ascribed to some *mathematical quality in Nature*, a quality which the casual observer of Nature would not suspect, but which nevertheless plays an important role in Nature's scheme.

There can be no clearer acknowledgement of this gap than Dirac's remark: 'There is no logical reason why the second method should be possible at all.'

[1] See (Fraenkel, Bar-Hillel and Levy, 2001, p. 211).
[2] See (London, 1964, p. 1).
[3] The emphases in the quotations are in the originals.
[4] See (Dirac, 1938–39, first paragraph).

It is well known that Heisenberg stumbled upon matrix mechanics while attempting to express quantum theory entirely in terms of observable quantities. Many years later, he wrote an article in the literary journal *Encounter* in which he recounted the following:[5]

> It is generally believed that our science is empirical, and that we draw our concepts and our mathematical constructs from empirical data. If this was the whole truth, we should when entering a new field introduce only those quantities that can directly be observed, and formulate laws only by means of these quantities.
>
> When I was a young man I believed that this was just the philosophy which Albert Einstein had followed in his theory of Relativity. I tried, therefore, to take a corresponding and related step in Quantum theory by introducing the matrices. But when I later asked Einstein about it, he told me: 'This may have been my philosophy, but it is nonsense all the same. It is never possible to introduce only observable quantities in a theory. It is a theory which decides what can be observed...' What he meant was that...we cannot separate the empirical process of observation from the mathematical construct and concepts.

The 'mathematical quality in nature' of Dirac's description, acknowledged if not articulated by Einstein and Heisenberg, was a philosophical position that went back to the ancient Greeks – to geometry, measuring the earth, and arithmetic, the art of counting. But, in the last three decades of the nineteenth century, Georg Cantor had developed his theory of *transfinite numbers* which challenged this wisdom. Cantor introduced the notion of a *set*, and, using this notion, established several epoch-making results. One of these was a precise characterization of infinite sets.[6] Another was the proof that the set of all subsets of a given set is, in a precisely defined sense, larger than the original set. This construction, called the power-set construction, could be applied to infinite sets to yield an unending succession of infinite sets, each larger than its predecessor; a revolutionary idea in mathematics at the end of the nineteenth century. It was this freedom to pursue ideas, unfettered by constraints other than those of consistency, that – Cantor asserted – distinguished mathematics from the other sciences.[7]

It seems unlikely that Einstein, Dirac and Heisenberg were influenced in any way by Cantor's work. The same could not be said of Wigner, if only because of his friendship with von Neumann. Fifteen years before Heisenberg's *Encounter*

[5] See (Heisenberg, 1975, pp. 55–56).
[6] A brief but adequate introduction to Cantor's theory is given in Appendix A1.
[7] A summary of Cantor's position is given in the section entitled *The nature of mathematics* in (Dauben, 1990, pp. 132–133). References to original and secondary sources will also be found in this work.

article, Wigner, in his celebrated essay on *the unreasonable effectiveness of mathematics in the natural sciences*,[8] had asked the question: what is mathematics?, and answered it, paraphrasing the logician and philosopher of science Walter Dubislav, as follows: '... mathematics is the science of skillful operations with concepts and rules invented just for this purpose.' If that were the case – and many practising mathematicians today would affirm that it *is* indeed the case – the effectiveness of mathematics in the natural sciences would be difficult to understand.

Taking stock, we may discern two world-views that are diametrically opposed to each other: the pre-Cantorian view that mathematics is, in everyday speech, discovered and not invented, and the post-Cantorian one that mathematics is invented, and not discovered.[9] It turns out, however, that between these metaphysical opposites, there is room for scientific analysis.

By a scientific analysis we mean (in the present context) one that is based upon physical principles and carried out by mathematical means. The precision required for such an analysis can only be attained by narrowing the field of enquiry. We shall confine ourselves to the following question: *is the differential calculus a discovery, or an invention*? Or, in scientific language: *is the differentiable structure of space-time a* consequence *of physical principles*?[10]

In Part I of this book, we shall establish some results that suggest that, subject to a certain caveat, the answer to the last question is in the affirmative. The physical principle that has these profound mathematical consequences is *causality* in the sense of Einstein and Weyl.[11] It turns out that the notion of Einstein–Weyl causality can be defined, as a partial order, on any infinite set of *points*, totally devoid of any predefined mathematical structure. Such *causally ordered spaces* can be *completed* – i.e., densely embedded in continua – in a unique manner, and the causal order can be extended, again uniquely, to the completed space. Furthermore, when these continua are finite-dimensional, they have the (unique) local structure of a differentiable manifold. If we agree to call a countably infinite set on which Einstein–Weyl causality is defined a *discrete space-time*, then the results can be stated as follows:

(i) Any discrete space-time can be *completed*, i.e., embedded in a continuum. The discrete space-time defines this continuum uniquely.
(ii) The causal order of the discrete space-time has a unique extension to its completion.

[8] See (Wigner, 1970, p. 224).
[9] What was simplistically described above as the pre-Cantorian view is actually a vast corpus in philosophy, with a history that goes back more than two millenia.
[10] The statement that the real line $\mathbb{R}$ has a differentiable structure is equivalent to the statement that there is such a subject as the differential calculus of a single real variable. A generalization will be found in Section A8.2.
[11] For details, see (Borchers and Sen, 2006).

(iii) The completion of a discrete, finite-dimensional space-time has the local structure of a differentiable manifold.

The results described above were obtained on the assumption that the notion of geometrical points (in the sense of Euclidean geometry) may be used in physics without further analysis. This assumption was strongly controverted by Wigner. Following earlier work by Wigner himself, Araki and Yanase established, within the framework of von Neumann's measurement theory, that an observable that does not commute with a conserved quantity cannot be measured precisely.[12] Since the position operator of a point-particle would seldom commute with the Hamiltonian, its position could not be measured precisely, which led Wigner to comment to Haag that 'there are those of us who believe that there are no points'.[13]

Part II of the book is an attempt to assuage Wigner's doubts. The strategy is extremely simple: try to show that the situation is no worse in quantum mechanics than it is in classical mechanics. But the validity of this procedure is based on the assumption that there are limits to the usefulness of Francis Bacon's motto '*dissecare naturam*'. In practical terms, a concept of measurement which is untenable in classical mechanics should be treated with suspicion in quantum mechanics.

John Bell, for example, has described quantum mechanics as 'our most fundamental physical theory'.[14] If quantum mechanics is fundamental and classical mechanics a mere $\hbar \to 0$ limit of it, then it is less than obvious how a comparison with the ills of classical mechanics can cure the ills of quantum mechanics. It is true that quantum mechanics 'explains' a set of natural phenomena that classical mechanics cannot; but it is equally true that the basic 'observables' of quantum mechanics are borrowings from the dynamical variables of classical mechanics: 'Who *is* the Potter, pray, and who the Pot?'[15]

Since the aim of theoretical physics is to understand physical phenomena that are observed, a theory – I maintain – should fit a particular *observational window*.[16] For example, the theory that is appropriate for describing the behaviour of ideal gases in thermodynamic equilibrium is inappropriate for describing the

[12] See (Araki and Yanase, 1960).

[13] This comment was made by Wigner after Haag's talk at the International Colloquium on Group Theoretical Methods in Physics in Philadelphia in 1986. Wigner's own account of his doubts will be found on page 207.

[14] The quotation, and the context, will be found on page 194.

[15] The *Rubayyat of Omar Khayyam*, translated by Edward FitzGerald.

[16] The notion of an observational window is arrived at by attempting to understand Einstein's maxim, 'it is a theory which decides what can be observed'. First, the observer decides what he or she wants to observe, and devises a theory to account for the observed regularities. A 'description of physical phenomena' is a description of the temporal evolution of the *state* of a physical system. The observational window determines the variables of state. The latter are required to be *complete*, i.e., temporal evolution is required to map the space of states into itself. Finally, this requirement constrains what can, or cannot, be observed. The term 'observational window' was first used in (Roos and Sen, 1994).

scattering of alpha-particles by thin metallic foils, and vice versa. We assume that the theories we are working with are not 'theories of everything'. Their function is to permit logical deductions from well-defined premises. It is therefore reasonable to demand that each theory be internally consistent.[17] To sum up, I do not see the question: which is more fundamental – quantum mechanics or classical mechanics – as one that advances scientific enquiry.[18] Classical mechanics has provided us with a body of concepts in terms of which equations of motion can be precisely framed for several classes of state spaces. Quantum mechanics has not changed these concepts; it has added a single new concept, but the result has been a revolutionary change in the space of states, which is the same as its observational window. I therefore believe that the strategy mentioned earlier is well conceived.

After this explicit statement of the assumptions that underlie our endeavour, we may turn to the essential point. We want to show that, as far as limitations on the accuracy of a measurement are concerned, they are no worse in quantum mechanics than they are in classical mechanics. But what are the factors that limit the accuracy of measurements in classical mechanics?

In the theory called classical mechanics, there are no physical principles that limit the accuracy of measurements. Measurements are assumed to be instantaneous, and therefore even the position of a moving point-particle can be measured precisely at any instant of time. What, then, is the source of limitations on the accuracy of classical measurements on which we are trying to build our case? We begin with a few historical remarks, some of which are common knowledge while others have hardly entered into the consciousness of the scientific community.

Although Einstein's contributions were decisive in establishing the particle aspect of light, Einstein himself remained a lifelong sceptic of quantum mechanics. His exchanges with Bohr are well known;[19] Einstein remained unconvinced. However, Einstein also carried on a lifelong correspondence with Max Born on the subject. Born too failed to convince Einstein, but, in the process – sometime before 1954 – he came to a crucial realization: the reason why an exact determination of the state of a physical system – be it classical or quantum-mechanical – was impossible lay in the *mathematical structure of the real number system*. He

[17] Unfortunately, this demand cannot always be met. As far as agreement between theory and experiment is concerned, quantum electrodynamics (QED) is by far the most successful physical theory that we have, but it is known to be logically inconsistent. The logical inconsistency of QED was pointed out, from two different directions, by Dyson and Haag (Dyson, 1952; Haag, 1955). The explanation of this puzzle remains – or so the present author contends – the most important unsolved problem in theoretical physics. By *explanation* we mean a logical deduction from accepted premises, and not a *belief*, as articulated by Weinberg (Weinberg, 1995, p. 499, last paragraph).

[18] It may be that in making this assertion I am, as Keynes would have it, being driven by the ghosts of defunct philosophers.

[19] See, for example, (Wheeler and Zurek, 1983), which contains 47 pages on the Bohr–Einstein dialogue, and reprint of the EPR paper (Einstein, Podolsky and Rosen, 1935) and Bohr's reply to it.

pointed out a fact to which no physicist, before him, seems to have paid the slightest attention.[20] This fact is the following: the rational numbers are countable, and therefore form a set of Lebesgue measure zero on the real line. That is, almost every real number is irrational. Now the explicit decimal representation of an irrational number is nonrecurrent, and requires an infinite number of digits. It is therefore absurd to assert that the position of a point-particle on a real line can be measured precisely. At least ten years before the paper by Lorenz that set off the chaos revolution,[21] Born noticed what has since become known as the 'sensitive dependence on initial conditions' of nonlinear classical mechanics, and used these facts to make the following assertions about classical point-particle mechanics:

(i) From the viewpoint of the experimentalist, it makes little sense to talk about the position of a point-particle. What makes sense is the notion of a *probability distribution* about its position.
(ii) In view of the sensitive dependence on initial conditions, a second determination of the position of a point-particle – if successful – could be interpreted as *a reduction of the probability distribution*; it would effect a drastic change in the probability distribution.

We may now hone our strategy to the following. Since the measurement of a continuous variable in classical physics is possible only within an error ε, where ε is an arbitrarily small *but positive* number, what we have to show is that, given any $\varepsilon > 0$, the corresponding quantum-mechanical observable can be measured within this error.

To sum up: the gap between the domains of discreteness and of continuity in mathematics is equally *a gap between experimental and theoretical physics*. Here classical mechanics and nonrelativistic quantum mechanics are on a par with each other, as they both rely on the same local topological–geometrical structure of space-time.

As is well known, von Neumann's measurement theory, the source of Wigner's doubts, requires the intervention of the observer's 'conscious ego'. The mathematical part of the theory cannot account for the reduction of the wave packet; it is, as we shall find, a theory of entanglement (the term was coined by Schrödinger three years after the appearance of von Neumann's book) rather than a theory of measurement. A resolution of Wigner's doubts requires, first and foremost, a resolution of the quantum measurement problem in a *mathematical* manner: namely, a theory that accounts for the reduction of the wave packet in which appeal to the observer's conscious ego is replaced by a significantly weaker mathematical hypothesis.

[20] In (Sen, 2008) I referred to this fact as 'known to all but honoured by none'. I was wrong; Max Born had seen its implications more than 50 years earlier.

[21] The reference is to (Lorenz, 1963).

Such a theory has been proposed by Sewell, and extended by the present author to continuous spectra.[22] This theory may aptly be described as a bridge over the Heisenberg cut, with movement across it being controlled by the Schrödinger–von Neumann equations. In this theory observables with discrete, rational spectra can be measured precisely, and therefore observables with continuous spectra can be measured with an error $\varepsilon > 0$, where ε can be made arbitrarily small. This is not very different from the measurement of a continuous variable in classical mechanics, *if the result of measurement is constrained to be a rational number*. Therefore one is tempted to claim that quantum mechanics does not make the situation any worse than it already is in classical physics.

However, Sewell's theory assumes that the state spaces of object and apparatus are finite-dimensional. As is well known, the canonical commutation relation $qp - pq = \mathrm{i}\hbar$ cannot be realized on finite-dimensional vector spaces; it runs afoul of the identity $\mathrm{Tr}\,(qp - pq) = 0$. This causes no trouble in measurement theory, as quantum-mechanical uncertainties are negligibly small by comparison with errors of observation.[23] Nevertheless, the assumption may be at variance with the general principles of quantum mechanics as they are commonly understood, and that is why the above claim should be tempered with caution.

Is it possible to lift the assumption of finite-dimensionalities from Sewell's measurement theory? The answer is not known, but to do so it will almost certainly be necessary to devise a framework (within nonrelativistic physics) for describing interactions of microscopic quantum systems with macroscopic systems *considered as a whole*. In the opinion of the present author, this is a key unsolved problem in nonrelativistic quantum mechanics.

The development of Parts I and II is mathematically rigorous. The results that are quoted without proof – and there are many – *have been proven*. The phrase 'it may be shown that…' (or something similar) invariably means 'it has been shown that…'. I use the former phrase because it sounds better to my ears. Again, mindful of the intended readership, many concepts defined in the appendices are recapitulated in footnotes to the text, or else a reference is given to the page on which it is defined. The word 'page' (or 'pages') is spelled out when it refers to a page in this book, and abbreviated to p. (or pp.) when it refers to some other source.

In a book such as the present one, it is neither possible nor desirable – or so the present author contends – to avoid expression of the opinions and beliefs that guide the endeavours of physicists. My personal opinions may diverge from the consensus (or, when there is no consensus, from commonly held views), and I have tried to keep the two separate. My personal beliefs and opinions are

[22] See (Sewell, 2005; Sen, 2008).

[23] The observable consequences of quantum mechanics derive mainly from the superposition principle, which holds on any linear space, irrespective of its dimensionality.

expressed either in the first person singular, or marked by a qualifier as in the first sentence of this paragraph.

Many of the references cited have been reprinted in various collections. Many articles originally published in German have been translated into English. I have referred to reprint volumes and English translations wherever I have had access to them, but I have omitted the names of the translators (which were not always available). Some of the references to books have been dictated by the desire to provide a historical perspective, but without pretensions to historical scholarship; others, by what I own, or have access to. Few of them are of recent vintage; I have not referred to later editions or reprints that I have not been able to consult.

Part I of this book is based entirely on the special theory of relativity; Part II, entirely on *nonrelativistic* quantum mechanics. Since I do not consider the unity of physics to be a good working hypothesis for a mathematical treatment,[24] I think that nonrelativistic quantum mechanics should stand as an autonomous, logically consistent edifice, despite its inadequacy as a physical theory at high energies. But I should add that nonrelativistic mechanics, both classical and quantum, assumes mathematical structures on space and time that appear to have their origins on Einstein–Weyl causality.

As stated earlier in different words, we have fallen short of our goal of deciphering whether the differential calculus is a discovery or an invention. But the search has revealed some new questions of interest in mathematics, theoretical physics and possibly in experimental physics, and these are discussed, or speculated upon, in the Epilogue.

[24] This is only one possible viewpoint among many. Theorists have long been attempting to integrate a larger class of problems as one unit, and some of the more recent attempts, such as string and superstring theories, have led to phenomenal advances in mathematics. If these endeavours succeed in reaching even a few of their goals, I will have to change my opinion.

Part I

Causality and differentiable structure