The Logical Structure of Mathematical Physics

Joseph D. Sneed

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To my parents

PREFACE

This book is about scientific theories of a particular kind – theories of mathematical physics. Examples of such theories are classical and relativistic particle mechanics, classical electrodynamics, classical thermodynamics, statistical mechanics, hydrodynamics, and quantum mechanics. Roughly, these are theories in which a certain mathematical structure is employed to make statements about some fragment of the world. Most of the book is simply an elaboration of this rough characterization of theories of mathematical physics. It is argued that each theory of mathematical physics has associated with it a certain characteristic mathematical structure. This structure may be used in a variety of ways to make empirical claims about putative applications of the theory. Typically – though not necessarily – the way this structure is used in making such claims requires that certain elements in the structure play essentially different roles. Some play a "theoretical" role; others play a "non-theoretical" role. For example, in classical particle mechanics, mass and force play a theoretical role while position plays a non-theoretical role. Some attention is given to showing how this distinction can be drawn and describing precisely the way in which the theoretical and non-theoretical elements function in the claims of the theory. An attempt is made to say, rather precisely, what a theory of mathematical physics is and how you tell one such theory from another – what the identity conditions for these theories are. In connection with this, certain relations that might hold between theories of mathematical physics are discussed: the equivalence relation, exemplified by Newtonian and Lagrangian formulations of classical particle mechanics; and the reduction relation, exemplified by classical rigid body mechanics and classical particle mechanics. Finally, something is said about historical development of theories of mathematical physics – how people come to have them and come to give them up.

In Chapter I the notion of the logical structure of a scientific theory is discussed and something is said about what it means to exhibit the logical structure – provide a logical reconstruction – of an existing scientific theory. The role of various sorts of axiomatizations of theories in illuminating their logical structure is considered. Particular attention is given to axiomatized deductive theories in first order logic and definitions of set-theoretic predicates. It is argued that there actually exist axiomatizations of theories of mathematical physics that are essentially definitions of set-theoretic predicates. Yet, the relevance of these axiomatizations to illuminating the logical structure of these theories is not apparent. An answer to the question of how these set-theoretic axiomatizations illuminate logical structure is suggested. It is roughly this. The set-theoretic predicate defined by the axiomatization characterizes the formal, mathematical structure associated with the theory. This predicate is used to make the empirical statements of the theory. Several ways in which such predicates might be used to make empirical statements are considered in the subsequent four chapters.

In Chapter II the simplest proposal for using a set-theoretic predicate to make empirical claims is considered. The set-theoretic predicate is predicated of a singular term. Some questions are considered which arise in connection with any proposal for using set-theoretic predicates involving quantitative notions to make empirical claims. In this connection, the theory of measurement and the question of the "approximate" nature of claims in mathematical physics are discussed briefly. The proposal considered here is shown to be essentially equivalent to a common view of the way in which the statements of a theory are obtained from the axiomatized deductive theory in first order logic which axiomatizes the theory. Considerable effort is devoted to characterizing a theory-relative notion of theoretical term, independently of any sweeping epistemological assumptions. This notion is then employed to describe a difficulty that could be encountered in attempting to employ the sentence form proposed here in logical reconstruction. An attempt is made to relate this difficulty to more traditional discussions of the problem of theoretical terms.

A second proposal for using a set-theoretic predicate to make empirical claims is considered in Chapter III. Essentially the sentence-form is this. Given a model for the non-theoretical part of the predicate, there is some way of adding a theoretical part to this model to yield a model for the entire predicate. This proposal is quite similar to the method of dealing with theoretical terms proposed by Frank Ramsey [35].* The relation between these two proposals and some formal questions raised by taking this line, are examined. Among these questions is the possibility of getting on without theoretical terms. An attempt is made to define a concept of eliminability of theoretical terms that is applicable to situations in which a set-theoretic predicate is being used to illuminate the logical structure of a theory. Finally, the question of the adequacy of this proposal to the task of logical reconstruction is considered. Though the proposal appears to solve the problem of theoretical terms it is found to be inadequate because it fails to provide a means of accounting for the way measured values of theoretical functions appropriate to one application of the theory can be employed in other applications of the same theory.

Chapter IV considers an emendation of the Ramsey view. It employs existential quantification over theoretical-function places to avoid the problem of theoretical terms. In addition, it proposes that we add to our rendering of the theory's empirical content the claim that the theoretical functions that satisfy the existential claim for various applications, taken all together, satisfy certain *constraints* that operate across applications. It is argued that the notion of constraints on the array of theoretical functions appearing in different applications of the theory allows us to account for non-trivial measurements of theoretical-function values. In addition, regarding the empirical content of a theory to be a statement of this form provides some insight into certain "holistic" views of theories such as those held by Duhem [10] and Kuhn [21].

In Chapter V one final proposal for using a set-theoretic predicate to make empirical claims is considered. The motivation for considering this proposal is provided by examples of situations in which we apparently "postulate" or "hypothesize" that theoretical functions have some special form in certain applications of a theory. It appears that a logical reconstruction of theories with such claims will require something more than sentences of the form considered in Chapter IV. The proposal for dealing with such theories is roughly this. A number of predicates are employed, all of them defined by restrictions of the definition of the same basic predicate. These predicates are used to construct a sentence which says that there are theoretical functions which make all intended applications models for the basic predicate, make some designated sub-sets of intended applications models for the "restrictions" of this basic predicate, *and* satisfy certain constraints. The "restrictions" of the basic predicate are to characterize various special forms that the theoretical function is hypothesized to have. This claim is regarded as the central empirical claim of the theory.

In Chapters II–V some simple examples of set-theoretic predicates are used to illustrate the alternative ways such a predicate could be used to elucidate the logical structure of a theory of mathematical physics. In Chapter VI these alternatives are again illustrated in attempting to provide a logical reconstruction of a real theory of mathematical physics – the Newtonian formulation of classical particle mechanics. This serves to illustrate, in a more concrete way, the difficulties with some of these alternatives and ultimately provides a *sketch* of an adequate logical reconstruction of this theory. This sketch, together with the notion of eliminability of theoretical terms developed in Chapter III, provides a means of treating in a systematic and perspicious way some frequently raised questions about the epistemological status of the concepts of mass and force. It also illuminates questions about the measurability of masses and forces and the status of specific force laws in measuring forces.

The account of the logical structure of empirical claims in theories of mathematical physics developed in the first five chapters is exploited in Chapter VII to clarify some other questions about these theories. An attempt is made to say precisely what a theory of mathematical physics *is*. Roughly, such a theory is identified as an ordered pair consisting of its characteristic mathematical structure – including "essential" constraints on its theoretical functions – and its characteristic range of intended applications. This account is then employed to investigate the properties of two relations – equivalence and reduction – that hold between some theories of mathematical physics. The Lagrangian and Hamiltonian formulations of classical particle mechanics are considered as examples of theories that are, in some sense, equivalent to the Newtonian formulation of classical particle mechanics. Classical rigid body mechanics is considered as an example of a theory which reduces to classical particle mechanics.

In Chapter VIII the previously developed static account of the logical structure of theories of mathematical physics is brought to bear on the dynamic aspect of theorizing – the way theories grow and develop in time. Considerable attention is given to the question of what it means to say a person *has* a theory of mathematical physics. This leads to an account of the characteristic ways a person's beliefs may change while he still has the same theory – how he may expand the same theory to encompass a wider range of phenomena or say more about the same range of phenomena. Finally, something is said about how people come to have theories, how they cease to have them, and how the conceptual apparatus in theories once held is related to the conceptual apparatus in theories held subsequent to them.

Having mentioned some of the things that are attempted in this book, perhaps it is also appropriate to mention some that are not.

What is described in this book is a certain way of theorizing. It involves using mathematical structures in certain ways to make claims about parts of the world. Theories of mathematical physics are examples of this way of theorizing. They are perhaps the most familiar examples and, historically, the first examples of theorizing in this way. No claim is made in this book that physical theories must, or do in fact, provide the only successful examples of this way of theorizing. Whether or not there is, or could be, theorizing in the behavorial sciences conforming to this model is a question I do not attempt to answer.

On several occasions in this book, I find it necessary to explicitly disclaim any intention of saying what a physical system is. For this reason, among others, the account of what a theory of mathematical physics is that appears in Chapter VII is admittedly deficient. I do not avoid this question because I believe it to be unimportant. I simply have nothing new to say about it. However, I do believe that a good many true and interesting things can be said about theories of mathematical physics without answering this question. I hope I have said some of these things.

In view of the pervasive role of probability concepts in contemporary mathematical physics, it must appear rather peculiar that a philosophical treatise on mathematical physics entirely avoids mentioning these concepts. This is no accident. There appear to be no insurmountable difficulties in describing precisely the mathematical structure associated with theories like statistical mechanics and quantum mechanics. Yet, a precise understanding of the empirical statements made with these structures is very elusive. Essentially, this understanding eludes us because we do not know how to interpret the probability measures appearing in these theories. Most physicists appear to believe that something like a relative frequency interpretation is appropriate. Yet, I know of no attempt to elucidate the empirical content of probabilistic theories of mathematical physics that avoids all the well known difficulties with this interpretation of probability. On the other hand, a thoroughgoing subjectivistic interpretation of the probabilities appears to require that we expand the "domain" of the theory's applications to include a human "observer" to whom the subjective probabilities belong. Even if this approach could be reconciled with our "unreconstructed" understanding of these theories, it remains to be shown that it can be carried out in detail. It is for these reasons that I avoid considering examples of probabilistic theories of mathematical physics. Whether ultimate clarification of the content of these theories will reveal that they too conform to my account of the logical structure of theories of mathematical physics, I can not say. However, I can see no compelling reason to think they will not.

The intellectual foundation for the ideas presented in this book clearly lies in the work of two philosophers: Frank Ramsey and Patrick Suppes. It has been my good fortune to be associated with the latter, both as student and colleague. His criticism and encouragement have been invaluable. Less obvious, but no less significant, is my debt to Donald Davidson. His conception of the philosophical enterprise has constantly influenced my approach to the problems treated in this book.

I have profited from the criticism and advice of many people in connection with this work. Extensive and detailed discussions with Professor John Wallace and Dr. Carole Ganz have contributed so much to shaping my thoughts on these matters that it is impossible to acknowledge their contributions in detail. Professor Carl Hempel graciously consented to read and comment upon an early draft of Chapters I–IV. I am much indebted to Professor Herbert Simon and Dr. Raimo Tuomela for their assistance in clarifying the notion of Ramsey eliminability (Chapter III). Professor Robert Causey and Mr. Gary Bower shared with me their insights into the theory of measurement. The early stages of my thinking about the historical development of scientific concepts (Chapter VIII) were influenced by discussion with Dr. Renate Bartsch and Dr. Lothar Schäfer. In ways too numerous to list, I have profited from the careful and insightful criticism of Professor Zoltan Domotor, Professor Stig Kanger, Mr. David Miller, Mr. Peter Oppenheimer and Dr. Krister Segerberg. To many others – students and colleagues – with whom I have discussed parts of this work, I am also indebted. I regret that space does not permit me to acknowledge explicitly these debts.

Though I have been the fortunate recipient of much good counsel, I have not always heeded it. For the errors and deficiencies remaining in the work, I alone am responsible.

The main features of the view of mathematical physics presented in Chapters I–VI were outlined in a course of lectures given at the University of Michigan in the winter of 1966. This view was developed further in

seminars at Stanford during 1967 and 1968. Some of the material in Chapter III was presented at the Third International Congress for Logic, Methodology and the Philosophy of Science at Amsterdam in 1968. I am indebted to Stanford University and the National Science Foundation for freeing my time during the fall and winter of 1969 to complete work on Chapters VII and VIII. Much of this work was done at the Philosophy Department, Uppsala University, Sweden to which I am indebted for providing office space and clerical assistance. Without the patient assistance of the secretarial staff in the Stanford Philosophy Department this work would never have appeared.

Stanford, California, May 13, 1970

NOTE

* All bibliographic references in this preface and Chapters I to VIII refer to the bibliography, pp. 308–309.

INTRODUCTION

Since this book appeared in 1971 some of the material in it has been developed further in a variety of ways. Insightful criticism has revealed some unanticipated problems and, in some cases, led to significantly improved formulations of central ideas. Further applications of the book's approach to describing the logical structure of empirical theories have appeared. The possibilities of this approach for describing the chronological development of empirical theories have been further explored and attempts to produce specific detailed applications in this area are in progress. The appearance of an 'updated edition' provides a propitious occasion to review briefly these developments.

I have made no effort to be exhaustive in this review. Space limitations precluded this. That I have not explicitly mentioned some work does not necessarily mean that I do not believe the work to be valuable. In the accompanying 'updated bibliography' I have included all the relevant literature known to me. That something relevant has been omitted is purely a consequence of my ignorance or forgetfulness. No value judgment beyond that of 'relevance' was intended. Numbered references in this 'Introduction' refer to the 'Updated Bibliography':

The most extensive discussion of this book to appear in print is surely that of Stegmüller [47]. Stegmüller's main concern is elaborating and expanding the implications of the book's account of logical structure for theories of mathematical physics to describing the chronological development of empirical science in general. In particular, he sees in the 'reduction relation' (Chapter 7) between two theories with different conceptual structures a means of describing the phenomenon that Kuhn has called 'scientific revolution'. (For Kuhn's reaction see [24].) This possibility is discussed briefly in Chapter 8. Stegmüller develops these ideas quite a bit further. In doing this he places the so-called 'non-statement view' of empirical theories expounded in this book very carefully among its major competitors and evaluates them comparatively. Though exhaustive and detailed, Stegmüller's brief for the non-statement view has clearly not convinced everybody. For a critical account see Feyerabend's review [15].* Stegmüller also considerably simplifies the notation and corrects a number of technical mistakes. The reader who finds this book hard going should perhaps take a look at the Stegmüller discussion first.

Among the ideas in this book that have provoked a critical response is the criterion offered for distinguishing the theoretical from the non-theoretical concepts in a specific empirical theory (p. 28 ff). Roughly and intuitively a concept is theoretical in a theory if and only if *all* means of determining the truth-value of statements involving the concept presuppose the truth of the laws of the theory in question. Stegmüller emphasizes the essentially pragmatic character of the criterion. Others have found the criterion difficult to apply [29] and amenable to improvement in the direction of a purely semantical criterion [24]. My response to this criticism [43] is essentially this. The criterion follows immediately from my account of the logical structure of the empirical claims associated with theories. Evidence that this account is correct comes from acceptable reconstructions of real theories that exemplify it. My criterion of 'theoreticity' thus works roughly like a theoretical concept in my account of logical structure. You learn how to determine it from the law(s) in which it is used. I chose in this book to expound the criterion before expounding the theory in which it is embedded because I wanted to motivate the discussion of logical structure by tying it to the traditional 'problem of theoretical terms'. I now regret this choice. Better would have been to present my account of logical structure and then show that, when buying it (because it has worked for some examples and looks as if it may work for others), the purchaser gets as a premium a criterion for 'theoreticity-in-a-theory'.

Another focus of criticism has been the definition of 'reduction' (p. 222 ff). The major problem is that the reduction relation appears to be much too weak. That this may be true was first pointed out to me by W. Hoering. He conjectured that the relation might be so weak that, aside from theories that say something about the cardinality of their models, any two theories might have a weak reduction relation (in my sense) between them. The situation turns out not to be quite as bad as Hoering conjectured. W. Balzer has shown that there do exist theories not about cardinality that can not be related by a reduction relation. But Balzer's examples are highly contrived. As Mayer ([26] the only fragment of this discussion I know to have appeared in print) shows the reduction relation is still weak enough to permit the existence of reduction relations where they are intuitively not to be expected.

One might naturally think that such a weak concept of reduction could not bear the burden of distinguishing 'progress' from 'mere change' in science that Stegmüller wants it to bear. So long as one focuses attention only on reduction between the 'cores' of theories this seems to be true. But what we demand of a reduction relation that depicts 'progress' is not only that it connect the cores, but all acceptable 'expansions' of these cores as well. Moreover we expect the same reduction relation to keep on working for yet-to-be-discovered expansions. The reduction relation is indeed weak enough that almost any two theories (more precisely 'theory cores') can be connected by at least one *ad hoc* reduction relation. But most of these will just not be 'lawlike' in the sense of being plausibly projectable to further expansions of the theory's applications. These features of reduction relations are discussed in [43].

There has also been some critical discussion of both the logical and set-theoretic foundations of the view of empirical theories presented here. From the standpoint of formal logic, the main questions are: (i) can propositional content of empirical theories, as conceived here, be captured in a formal language? (ii) if so, what kind of formal language would be required? (iii) what would be learned by formalization? Niiniluoto [35] provides an illuminating discussion of these questions. Swijtink [55] and van Bentham [6] offer a discussion of Ramsey eliminability (p. 49 ff) of theoretical terms for theories formalized in first order logic. Moulines and I [33] offer a discussion of the relative merits of formal-linguistic and set-theoretic axiomatization in the context of reviewing the pioneering work of Suppes in set-theoretic axiomatization of empirical theories.

From the point of view of set-theory the major problem is that models for set-theoretic predicates (like, for example CPM p. 115) do not, 'strictly speaking', comprise a set. 'Strictly speaking' here means 'set as conceived in any known consistent axiomatization of set-theory'. Rantala [38] provides a clear discussion of this problem, though it has also been noted by Scheibe [40] and others. That such classes of models are taken to be sets in this book and set-theoretic operations applied to them with wreckless abandon is solely a consequence of my own set-theoretic naivete. A modicum of sophistication acquired at the urging of friends has revealed to me the dangers of such unfastidiousness with set-theoretic ideas. Though the danger of set-theoretic inconsistency lurks in the pages of this book, no instance has thus far been pointed out to me. My expectation is that all results presented here would survive in a more careful treatment.

It is of course important to discover whether my expectation is correct. A number of approaches to remedying these foundational difficulties have been suggested. Scheibe [40] notes that the classes of models I consider here are 'species of structures' in the sense of Bourbaki [7]. Mayer [27] employs this idea. A reformulation of the material in Chapter 7 using the concept of structure species is being undertaken by Balzer and Moulines. Z. Domotor has suggested that these classes of models might profitably be viewed as 'categories' [25]. So conceived, the *r*-function in a frame for a theory of mathematical physics (D26, p. 166) is the familiar 'forgetful functor' of category theory. Category theoretic concepts also appear to provide a somewhat more elegant formalism for discussing invariance principles than that appearing in [46]. Rantala [38] suggests some other alternatives. It remains to be seen which of these approaches will provide the most economical foundational basis for this view of empirical theories and whether foundational investigations will afford any genuinely new insights (as distinct from more rigorous and/or elegant formulations) about the nature of empirical science. My present view is that we stand a good chance of learning something new and important from research in this direction. I encourage it.

The examples treated in the book have also been the subject of some criticism. The discussion of classical particle mechanics has been criticized for its superficial treatment of Galilean invariance ([37]; Skarzynski's comments). I still regard this discussion (pp. 149–50) as basically correct, though incomplete. The major lacuna is a means of building the commitment to Galilean invariance into the formal description of the theory. A means of doing this is described in [46] and illustrated by an application to a 'fragment' of Newtonian particle mechanics – collision mechanics. A means of seeing how invariance principles arise in empirical science (as distinct from simply describing them) is also suggested in [46]. I am currently attempting to apply it to particle mechanical theories.

Kuhn first called my attention to the implausibility of regarding generalized coordinates as theoretical concepts (pp. 206–207). I now regard the