EDITED BY
ROBERT
BATTERMAN

The Oxford Handbook *of* PHILOSOPHY OF PHYSICS

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Contents

Contributors

Guido Bacciagaluppi is Reader in Philosophy at the University of Aberdeen. His field of research is the philosophy of physics, in particular the philosophy of quantum theory. He also works on the history of quantum theory and has published a book on the 1927 Solvay conference (together with A. Valentini). He also has interests in the foundations of probability and in issues of time symmetry and asymmetry.

Jonathan Bain is Associate Professor of Philosophy of Science at the Polytechnic Institute of New York University. His research interests include philosophy of spacetime, scientific realism, and philosophy of quantum field theory.

Sorin Bangu is Associate Professor of Philosophy at the University of Bergen, Norway. He received his Ph.D. from the University of Toronto and has previously been a postdoctoral fellow at the University of Western Ontario and a fixed-term lecturer at the University of Cambridge, Department of History and Philosophy of Science. His main interests are in philosophy of science (especially philosophy of physics, mathematics, and probability) and later Wittgenstein. He has published extensively in these areas and has recently completed a book manuscript on the metaphysical and epistemological issues arising from the applicability of mathematics to science.

Robert Batterman is Professor of Philosophy at the University of Pittsburgh. He is a Fellow of the Royal Society of Canada. He is the author of *The devil in the details: Asymptotic reasoning in explanation, reduction, and emergence* (Oxford, 2002). His work in philosophy of physics focuses primarily upon the area of condensed matter broadly construed. His research interests include the foundations of statistical physics, dynamical systems and chaos, asymptotic reasoning, mathematical idealizations, the philosophy of applied mathematics, explanation, reduction, and emergence.

Gordon Belot is Professor of Philosophy at the University of Michigan. He has published a number of articles on philosophy of physics and related areas—and one small book, *Geometric possibility* (Oxford, 2011).

Craig Callender is Professor of Philosophy at the University of California, San Diego. He has written widely in philosophy of science, metaphysics, and philosophy of physics. He is the editor of *Physics meets philosophy at the Planck length* (with

Huggett) and the *Oxford handbook of the philosophy of time*. He is currently working on a book monograph on the relationship between physical time and time as we experience it.

Olivier Darrigol is a CNRS research director in the SPHERE/Rehseis research team in Paris. He investigates the history of physics, mostly nineteenth and twentieth century, with a strong interest in related philosophical questions. He is the author of several books including *From c-numbers to q-numbers: The classical analogy in the history of quantum theory* (Berkeley: University of California Press, 1992), *Electrodynamics from Ampère to Einstein* (Oxford: Oxford University Press, 2000), *Worlds of flow: A history of hydrodynamics from the Bernoullis to Prandtl* (Oxford: Oxford University Press, 2005), and *A history of optics from Greek antiquity to the nineteenth century* (Oxford: Oxford University Press, 2012).

Leo P. Kadanoff is a theoretical physicist and applied mathematician who has contributed widely to research in the properties of matter, the development of urban areas, statistical models of physical systems, and the development of chaos in simple mechanical and fluid systems. His best-known contribution was in the development of the concepts of "scale invariance" and "universality" as they are applied to phase transitions. More recently, he has been involved in the understanding of singularities in fluid flow.

John Byron Manchak is an Assistant Professor of Philosophy at the University of Washington. His primary research interests are in philosophy of physics and philosophy of science. His research has focused on foundational issues in general relativity.

Tarun Menon is a graduate student in Philosophy at the University of California, San Diego. His research interests are in the philosophy of physics and metaphysics, particularly time, probability, and the foundations of statistical mechanics. He is also interested in formal epistemology and the cognitive structure of science.

Margaret Morrison is Professor of Philosophy at the University of Toronto. She is the author of several articles on various aspects of philosophy of science including physics and biology. She is also the author *of Unifying scientific theories: Physical concepts and mathematical structures* (Cambridge, 2000) and the editor (with Mary Morgan) of *Models as mediators: Essays on the philosophy of natural and social science* (Cambridge, 1999).

Oliver Pooley is University Lecturer in the Faculty of Philosophy at the University of Oxford and a Fellow and Tutor at Oriel College, Oxford. He works in the philosophy of physics and in metaphysics. Much of his research focuses on the nature of space, time, and spacetime.

Laura Ruetsche is Professor of Philosophy at the University of Michigan. Her *Interpreting quantum theories: The art of the possible* (Oxford, 2011) aims to articulate questions about the foundations of quantum field theories whose answers might hold interest for philosophy more broadly construed.

Simon Saunders is Professor in the Philosophy of Physics and Fellow of Linacre College at the University of Oxford. He has worked in the foundations of quantum field theory, quantum mechanics, symmetries, thermodynamics, and statistical mechanics and in the philosophy of time and spacetime. He was an early proponent of the view of branching in the Everett interpretation as an "effective" process based on decoherence. He is co-editor (with Jonathan Barrett, Adrian Kent, and David Wallace) of *Many worlds? Everett, quantum theory, and reality* (OUP 2010).

Chris Smeenk is Associate Professor of Philosophy at the University of Western Ontario. His research interests are history and philosophy of physics, and seventeenth-century natural philosophy.

Sheldon R. Smith is Professor of Philosophy at UCLA. He has written articles on the philosophy of classical mechanics, the relationship between causation and laws, the philosophy of applied mathematics, and Kant's philosophy of science.

David Wallace studied physics at Oxford University before moving into philosophy of physics. He is now Tutorial Fellow in Philosophy of Science at Balliol College, Oxford, and university lecturer in Philosophy at Oxford University. His research interests include the interpretation of quantum mechanics and the philosophical and conceptual problems of quantum field theory, symmetry, and statistical physics.

Mark Wilson is Professor of Philosophy at the University of Pittsburgh, a Fellow of the Center for Philosophy of Science, and a Fellow at the American Academy of Arts and Sciences. His main research investigates the manner in which physical and mathematical concerns become entangled with issues characteristic of metaphysics and philosophy of language. He is the author of *Wandering significance: An essay on conceptual behavior* (Oxford, 2006). He is currently writing a book on explanatory structure. He is also interested in the historical dimensions of this interchange; in this vein, he has written on Descartes, Frege, Duhem, and Wittgenstein. He also supervises the North American Traditions Series for Rounder Records.

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INTRODUCTION

ROBERT BATTERMAN

When I was in graduate school in the 1980s, philosophy of physics was focused primarily on two dominant reasonably self-contained theories: Orthodox nonrelativisitic quantum mechanics and relativistic spacetime theories. Of course, there were a few papers published on certain questions in other fields of physics such as statistical mechanics and its relation to thermodynamics. These latter, however, primarily targeted the extent to which the reductive relations between the two theories could be considered a straightforward implementation of the orthodox strategy outlined by Ernest Nagel.

Philosophical questions about the measurement problem, the question of the possibility of hidden variables, and the nature of quantum locality dominated the philosophy of physics literature on the quantum side. Questions about relationalism vs. substantivalism, the causal and temporal structure of the world, as well as issues about underdetermination of theories dominated the literature on the spacetime side. Some worries about determinism vs. indeterminism crossed the divide between these theories and played a significant role in shaping the development of the field. (Here I am thinking of Earman's *A Primer on Determinism* (1986) as a particular driving force.)

These issues still receive considerable attention from philosophers of physics. But many philosophers have shifted their attention to other questions related to quantum mechanics and to spacetime theories. In particular, there has been considerable work on understanding quantum field theory, particularly from the point of view of algebraic or axiomatic formulations. New attention has also been given to philosophical issues surrounding quantum information theory and quantum computing. And there has, naturally, been considerable interest in understanding the relations between quantum theory and relativity theory. Questions about the possibility of unifying these two fundamental theories arise. Relatedly, there has been a focus on understanding gauge invariance and symmetries.

However, I believe philosophy of physics has evolved even further, and this belief prompts the publication of this volume. Recently, many philosophers have focused their attentions on theories that, for the most part, were largely ignored in the past. As noted above, the relationship between thermodynamics and statistical mechanics—once thought to be a paradigm instance of unproblematic theory reduction—is now a hotly debated topic. Philosophers and physicists have long implicitly or explicitly adopted a reductionist methodological bent. Yet, over the years this methodological slant has been questioned dramatically. Attention has been focused on the explanatory and descriptive roles of "non-fundamental," *phenomenological* theories. In large part because of this shift of focus, "old" theories such as classical mechanics, once deemed to be of little philosophical interest, have increasingly become the focus of deep methodological investigations.

Furthermore, some philosophers have become more interested in less "fundamental" contemporary physics. For instance, there are deep questions that arise in condensed matter theory. These questions have interesting and important implications for the nature of models, idealizations, and explanation in physics. For example, model systems, such as the Ising model, play important computational and conceptual roles in understanding how there can be phase transitions with specific characteristics. And, the use of the thermodynamic limit is an idealization that (some have argued) plays an essential, ineliminable role in understanding and explaining the observed universality of critical phenomena. These specific issues are discussed in several of the chapters in this volume.

In the United States during the 1970s and 1980s, there was a great debate between particle physicists who pushed for funding of high-energy particle accelerators and solid-state or condensed-matter theorists for whom the siphoning off of so much government funding to "fundamental" physics was unacceptable. A famous paper championing the latter position is Philip Anderson's "More Is Different" (1972). Not only was this a debate over funding, but it raised issues about exactly what should count as "fundamental" physics. While historians of physics have focused considerable attention on this public debate, philosophers of physics have really only recently begun to engage with the conceptual implications of the possibility that condensed matter theory is in some sense just as fundamental as high-energy particle physics.

This collection aims to do two things. First, it tries to provide an overview of many of the topics that currently engage philosophers of physics. And second, it focuses attention on some theories that by orthodox 1980s standards would not have been considered fundamental. It strives to survey some of these new issues and the problems that have become a focus of attention in recent years. Additionally, it aims to provide up-to-date discussions of the deep problems that dominated the field in the past.

In the first chapter, "For a Philosophy of Hydrodynamics," Olivier Darrigol focuses attention on lessons that can be learned from the historical development of fluid mechanics. He notes that hydrodynamics has probably received the least attention of any physical theory from philosophers of physics. Hydrodynamics is not a "fundamental" theory along the lines of quantum mechanics and relativity theory, and its basic formulation has not evolved much for two centuries. These facts, together with a lack of detailed historical studies of hydrodynamics, have kept the theory off the radar.[1] Darrigol provides an account of the development of hydrodynamics as a complex theory—one that is not fully captured by the basic Navier-Stokes equations. For the theory to be applicable, particularly for it to play an explanatory role, a host of techniques—idealizations, modeling strategies, and empirically determined data must come into play. This discussion shows clearly how intricate, sophisticated, and modern the theory of hydrodynamics actually is. Darrigol draws a number of lessons about the structures of phenomenological theories from his detailed discussion, focusing particularly on what he calls the "modular structure" of hydrodynamics.

Continuing the discussion of "old"—but by no means dead or eliminated—theories, Mark Wilson takes on the formidable task of trying to say exactly what is the nature of classical mechanics. A common initial reaction to this topic is to dismiss it: "Surely we all know what classical mechanics is! Just look at any textbook." But as Wilson shows in "What Is 'Classical Mechanics' Anyway?", this dismissive attitude is misleading on a number of important levels. Classical mechanics is like a five-legged stool on a very uneven floor. It shifts dramatically from one foundational perspective to another depending upon the problem at hand, which in turn is often a function of the scale length at which the phenomenon is investigated. In the context of planetary motions, billiards, and simplified ideal gases in boxes, the point-particle interpretation of classical mechanics will most likely provide an appropriate theoretical setting. However, as soon as one tries to provide a more realistic description of what goes on inside actual billiard ball collisions, one must consider the fact that the balls will deform and build up internal stresses upon collision. In such situations, the point-particle foundation will fail and one will need to shift to an alternative foundation, provided by classical continuum mechanics. Yet a third potential foundation for classical mechanics can be found within so-called analytic mechanics, in which the notion of a rigid body becomes central. Here constraint forces (such as the connections that allow a ball to roll, rather than skid, down an inclined plane) play a crucial role. Forces of this type are not wholly consistent with the suppositions central to either the point-particle or continuum points of view. A major lesson from Wilson's discussion is that classical mechanics should best be thought of as constituted by various foundational methodologies that do not fit

[1] Darrigol's recent *Worlds of Flow* fills this lacuna providing an exceptional discussion of the history (Darrigol 2005).

particularly well with one another. This goes against current orthodoxy that a theory must be seen as a formally axiomatizable consistent structure. On the contrary, to properly employ classical mechanics for descriptive and explanatory purposes, one pushes a foundational methodology appropriate at one scale of investigation to its limiting utility, after which one shifts to a different set of classical modeling tools in order to capture the physics active at a lower size scale. Wilson argues that a good deal of philosophical confusion has arisen from failing to recognize the complicated scale-dependent structures of classical physics.

Sheldon Smith's contribution adds to our understanding of a particular aspect of classical physics. In "Causation in Classical Mechanics," he addresses skeptical arguments initiated by Bertrand Russell to the effect that causation is not a fundamental feature of the world. In the context of classical physics, one way of making this claim more precise is to argue that there is no reason to privilege retarded over advanced Green's functions for a system. Green's functions, crudely, describe the effect of an instantaneous, localized disturbance that acts upon the system. It seems that the laws of motion for electromagnetism or for the behavior of a harmonic oscillator do not distinguish between retarded (presumably "causal") and advanced (presumably "acausal") solutions. If there is to be room for a principle of causality in classical physics, then it looks like we need to find extra-nomological reasons to privilege the retarded solutions. Smith surveys a wide range of attempts to answer the causal skeptic in the contexts of the use of Green's functions and the imposition of (Sommerfeld) radiation conditions, among other attempts. The upshot is that it is remarkably difficult to find justification within physical theory for the maxim that causes precede their effects.

The next chapter, by Leo Kadanoff, focuses on condensed matter physics. In particular, Kadanoff discusses progress in physically understanding the fact that matter can abruptly change its qualitative state as it undergoes a phase transition. An everyday example occurs with the boiling water in a teakettle. As the temperature increases, the water changes from its liquid phase to its vapor phase in the form of steam. Mathematically, such transitions are described by an important concept called an order parameter. In a first-order phase transition, such as the liquid vapor transition, the order parameter changes discontinuously. Certain phase transitions, however, are continuous in the sense that the discontinuity in the behavior of the order parameter approaches zero at some specific critical value of the relevant parameters such as temperature and pressure. For a long time there were theoretical attempts to understand the physics involved in such continuous transitions that failed to adequately represent the actual behavior of the order parameter as it approached its critical value. The development of the *renormalization group* in the 1970s remedied this situation. Kadanoff played a pivotal role in the conceptual development of renormalization group theory. In this chapter, he focuses on these developments (particularly, the improvement upon early mean field theories) and on a deeply interesting feature he calls the "extended singularity theorem." This is the idea that sharp, qualitatively distinct, changes in phase involve the presence of a mathematical singularity. This singularity typically emerges in the limit in which the system size becomes infinite. The understanding of the behavior of systems at and near phase transitions requires radically different conceptual apparatuses. It involves a synthesis between standard statistical mechanical uses of probabilities and concepts from dynamical systems theory—particularly, the topological conceptions of basins of attraction and fixed points of a dynamical transformation.

The discussion of the renormalization group and phase transitions continues as Tarun Menon and Craig Callender examine several philosophical questions raised by phase transitions. Their chapter, "Turn and Face the Strange ...Ch-ch-changes," focuses on the question of whether phase transitions are to be understood as genuinely emergent phenomena. The term "emergent" is much abused and confused in both the philosophical and physics literatures and so Menon and Callender provide a kind of road map to several concepts that have been invoked in the increasing number of papers on emergence and phase transitions. In particular, they discuss conceptions of reduction and corresponding notions of emergence: conceptual novelty, explanatory irreducibility, and ontological irreducibility. Their goal is to establish that for any reasonable senses of reducibility and emergence, phase transitions are not emergent phenomena, and they do not present problems for those of a reductionist explanatory bent. In a sense, their discussion can be seen as challenging the importance of the extended singularity theorem mentioned above. Menon and Callender also consider some recent work in physics that attempts to provide well-defined notions of phase transition for finite systems. Their contribution serves to highlight the controversial and evolving nature of our philosophical understanding of phase transitions, emergence, and reductionism.

Jonathan Bain's contribution on "Effective Field Theories" looks at several physical and methodological consequences of the fact that some theories at low-energy scales are effectively independent of, or decoupled from, theories describing systems at higher energies. Sometimes we know what the high-energy theory looks like and can follow a recipe for constructing low-energy effective theories by systematically eliminating high-energy interactions that are essentially "unobservable" at the lower energies. But, at other times, we simply do not know the correct high-energy theory, yet nonetheless, we still can have effective low-energy theories. Broadly construed, hydrodynamics is an example of the latter type of effective theory, if we consider it as a nineteenth century theory constructed before we knew about the atomic constitution of matter. Bain's focus is on effective theories in quantum field theory and condensed matter physics. His discussion concentrates on the intertheoretic relations between low-energy effective theories and their high-energy counterparts. Given the effective independence of the former from the latter, should one think of this relation as autonomous or emergent? Bain contends that an answer to this question is quite subtle and depends upon the type of renormalization scheme employed in constructing the effective theory.

My own contribution to the volume concerns a general problem in physical theorizing. This is the problem of relating theories or models of systems that appear at widely separated scales. Of course, the renormalization group theory (discussed by Kadanoff, Menon and Callender, and Bain in this volume) is one instance of bridging across scales. But more generally, we may try to address the relations between finite statistical theories at atomic and nanoscales and continuum theories that apply at scales 10+ orders of magnitude higher. One can ask, for example, why the Navier-Cauchy equations for isotropic elastic solids work so well to describe the bending behavior of steel beams at the macroscale. At the microscale the lattice structure of iron and carbon atoms looks nothing like the homogeneous macroscale theory. Nevertheless, the latter theory is remarkably robust and safe. The chapter discusses strategies for upscaling from theories or models at small scales to those at higher scales. It examines the philosophical consequences of having to consider, in one's modeling practice, structures that appear at scales intermediate between the micro and the macro.

There has been considerable debate about the nature of symmetries in physical theories. Recent focus on gauge symmetries has led philosophers to a deeper understanding of the role of local invariances in electromagnetism, particle physics, and the hunt for the Higgs' particle. Sorin Bangu provides a broad and comprehensive survey of concepts of symmetry and invariance in his contribution to this volume. One of the most seductive features of symmetry considerations comes out of Wigner's suggestion that one might be able to understand, explain, or ground laws of nature by appeal to a kind of superprinciple expressing symmetries and invariances that constrain laws to have the forms that they do. On this conception symmetries are, perhaps, ontologically and epistemically prior to laws of nature. This raises deep questions for further research on the relationship between formal mathematical structures and our physical understanding of the world.

Gordon Belot also considers issues of symmetry and invariance. His contribution explores the connections between being a symmetry of a theory—a map that leaves invariant certain structures that encode the laws of the theory—and what it is for solutions to a theory to be *physically* equivalent. It is fairly commonplace for philosophers to adopt the idea that, in effect, these two notions coincide. And if they do, then we have tight connection between a purely formal conception of the symmetries of a theory and a methodological/interpretive conception of what it is for two solutions to represent the same physical state of affairs. Belot notes that in the context of spacetime theories there seem to be well-established arguments supporting this tight connection between symmetries and physical equivalence. However, he explores the difficulties in attempting to generalize this connection in contexts that include classical dynamical theories. Belot examines different ways one might make precise the notion of the symmetries of a classical theory and shows that they do not comport well with reasonable conceptions of physical equivalence. The challenge to the reader is then to find appropriate, nontrivial notions of symmetries for classical theories that will respect reasonable notions of physical equivalence.

Yet another type of symmetry, permutation symmetry, is the subject of the chapter by Simon Saunders, entitled "Indistinguishability." He focuses on the proper understanding of particle indistinguishability in classical statistical mechanics and in quantum theory. In the classical case, Gibbs had already (prior to quantum mechanics) recognized a need to treat particles, at least sometimes, as indistinguishable. This is related to the infamous *Gibbs paradox* that Saunders discusses in detail. The concept of "indistinguishability" had meanwhile entered physics in a completely new way, involving a new kind of statistics. This came with the derivation of Planck's spectral distribution, in which Planck's quantum of action h first entered physics. Common wisdom has long held that particle indistinguishability is strictly a quantum concept, inapplicable to the classical realm; and that classical statistical mechanics is anyway only the classical limit of a quantum theory. This fits with the standard view of the explanation of quantum statistics (Bose-Einstein or Fermi-Dirac statistics): departures from classical (Maxwell-Boltzmann) statistics are explained by particle indistinguishability. With this Saunders takes issue. He shows how it is possible to treat the statistical mechanical statistics for classical particles as invariant under permutation symmetry in exactly the same way that it is treated in the quantum case. He argues that the conception of permutation symmetry deserves a place alongside all the other symmetries and invariances of physical theories. Specifically, he argues that the concept of indistinguishable, permutation invariant, *classical* particles is coherent and reasonable contrary to many claims found in the literature.

Margaret Morrison's topic is "Unification in Physics." She argues that there are a number of distinct senses of unification in physics, each of which has different implications for how we view unified theories and phenomena. On the one hand, there is a type of unification that is achieved via reductionist programs. Here a paradigm example is the unification provided by Maxwellian electrodynamics. Maxwell's emphasis on mechanical models in his early work involved the introduction of the displacement current, which was necessary for a field theoretic representation of the phenomena. These models also enabled him to identify the luminiferous aether with the medium of transmission of electromagnetic phenomena. Two aethers were essentially reduced to one. When these models were abandoned in his later derivation of the field equations, the displacement current provided the unifying parameter or theoretical quantity that allowed for the identification of electromagnetic and optical phenomena within the framework of a single field theoretic account. This type of unification was analogous to Newton's unification of the motions of the planets and terrestrial trajectories under the same (gravitational) theoretical framework. However, not all cases of unification are of this type. Morrison discusses the example of the electroweak theory in some detail, arguing that this unificatory success represents a kind of synthetic, rather than reductive, unity. The electroweak theory also involves a unifying parameter, namely, the "Weinberg angle." However, the unity achieved through gauge symmetry is a synthesis of structure, rather than of substance, as exemplified by the reductive cases. Finally, in calling attention to the difficulties with the Standard Model more generally, Morrison notes that yet a different kind of unification is achieved in the framework of effective field theory. This provides another vantage point from which to understand the importance of the renormalization group. Morrison argues for a third type of unification in terms of the universality classes, one that focuses on unification of phenomena but should be understood independently of the type of micro-reduction characteristic of unified field theory approaches.

As noted earlier, there continues to be significant research on foundational problems in quantum mechanics. Guido Bacciagaluppi's chapter provides an up-to-date discussion of work on two distinct problems in the foundations of quantum mechanics that are typically conflated in the literature. These are the problem of the classical regime and the measurement problem. Both problems arise from deep issues involving entanglement and the failure of an ignorance interpretation of reduced quantum states. Bacciagaluppi provides a contemporary and thorough introduction to these issues. The problem of the classical regime is that of providing a quantum mechanical explanation or account of the success of classical physics at the macroscale. It is, in essence, a problem of intertheoretic relations. Contemporary work has concentrated on the role of environmental decoherence in the emergence of classical kinetics and dynamics. Bacciagaluppi argues that the success of appeals to decoherence to solve this problem will depend upon one's interpretation of quantum mechanics. He surveys an ontologically minimalist instrumental interpretation and a standard, ontologically more robust or realistic interpretation.

The measurement problem is the distinct problem of deriving the collapse postulate and the Born rule from the first principles (Schrödinger evolution) of the quantum theory. In examining the measurement problem, Bacciagaluppi provides a detailed presentation of a modern, realistic theory of measurement that goes beyond the usual idealized discussions of spin measurements using Stern-Gerlach magnets. This discussion generalizes the usual collapse postulate and the Born rule to take into account the fact that real measurements are unsharp. It does so by employing the apparatus of positive operator value (POV) measures and observables. The upshot is that the measurement problem remains a real worry for someone who wants to maintain a standard, reasonably orthodox interpretation of quantum theory. Perhaps Everett theories, GRW-like spontaneous collapse theories, and so on are required for a solution.

The Everett, or Many Worlds, interpretation of quantum mechanics is the subject of David Wallace's chapter. It is well known that the linearity of quantum mechanics leads, via the principle of superposition, to the possibility that macroscopic objects such as cats can be found in bizarre states—superpositions of being alive and being dead. Wallace argues that a proper understanding of what quantum mechanics actually says will enable us to understand such bizarre situations in a way that does not involve changing the physics (e.g., as in Bohmian hidden variable mechanics or GRW spontaneous collapse theories). Neither, he claims, does it involve changing one's philosophy by, for example, providing an operationalist interpretation that imposes some special status to the observer or to what counts as measurement, along the lines of Bohr. Such interpretations are at odds with our understanding of, say, the role of the observer in the rest of science. Wallace argues for a straightforward, fully realist interpretation of the bare mathematical formalism of quantum mechanics and claims that this interpretation will make sense of superposed cats, and so on, without changing the theory and without changing our overall view of science. The straightforward realist interpretation that is to do all of this work is the Everett interpretation. Prima facie, this claim is itself bizarre: after all, the Everett interpretation has us multiplying worlds or universes upon measurements. Nevertheless, Wallace makes a strong case that an understanding of superposition as a description of multiplicity, rather than of the indefiniteness of states, is exactly what is needed. Furthermore, that is exactly what the Everett interpretation (and no other) provides. The bulk of Wallace's contribution examines various problems that have been raised for the Everett interpretation. In particular, he focuses on (1) the problem of providing a preferred basis—what actually justifies our understanding of superposition in terms of multiplicity of worlds, and (2) the probability problem—how to understand the probabilistic nature of quantum mechanics if one has only the fully deterministic dynamics provided by the Schrödinger equation. He argues that the contemporary understanding of the Everett interpretation has the resources to address these issues.

Laura Ruetsche's chapter "Unitary Equivalence and Physical Equivalence" investigates a question of deep physical and philosophical importance: The demand for criteria establishing the physical equivalence of two formulations of a physical theory. In "ordinary" quantum mechanics the received view is that two quantum theories are physically equivalent just in case they are unitarily equivalent. Any pair of theories purporting, say, to describe two entangled spin 1/2 systems are really just one and the same because of the Jordan and Wigner theorem showing that a theory that represents the canonical anticommutation relations for a system of n spins is unique up to unitary equivalence. A similar theorem due to Stone and von Neumann guarantees an analogous result for any Hilbert space representation of the canonical commutation relations for a Hamiltonian system. What are the consequences of the breakdown of unitary equivalence for those quantum systems for which these theorems fail to hold? Such systems include the infinite systems studied in quantum field theory, quantum statistical mechanics, and even simpler infinite systems like an infinite one-dimensional chain of quantum spins. She calls these theories collectively QM_∞. The plethora of unitarily inequivalent representations in these infinite cases demands that we revisit our assumptions about physical equivalence and the nature of quantum theories. Ruetsche examines various competing suggestions, or competing principles that may guide the investigation into this problem.

The next chapter, by Oliver Pooley, provides an up-to-date, comprehensive discussion of substantivalist and relationalist approaches to spacetime. Crudely, this is a debate about the ontology of our theories of space and spacetime. The substantivalists hold that among the fundamental objects of the world is spacetime itself. Relationists, to the contrary, deny that propositions about spacetime are ultimately to be understood in terms of claims about material objects and possible spatiotemporal relations that may obtain between them. Pooley presents a historical introduction, as well as a detailed discussion of the current landscape in the literature. Specifically, he considers recent relationist, neo-Machian proposals by Barbour, as well as dynamical approaches favored by Brown, and Pooley and Brown, that aim to provide a reductive account of the spacetime symmetries in terms of the dynamical symmetries of laws governing the behavior of matter. In addition, Pooley provides a current assessment of the impact of the so-called Hole Argument against substantivalism.

In "Global Spacetime Structure" John Manchak examines the qualitative, primarily topological and causal, aspects of general relativity. He provides an abstract classification of various local and global spacetime properties. In the global causal context he explicitly defines a set of causal conditions that form a strict hierarchy of possible casual properties of spacetime. The strongest is the condition of global hyperbolicity, which implies others including causality and chronology. Another set of global properties of spacetime concerns in what sense a spacetime can be said to possess singularities. Here he focuses on the notion of geodesic incompleteness. Manchak then takes up philosophical questions concerning the physical reasonableness of these various spacetime properties. In a local context, being a solution to Einstein's Field Equation is typically taken to be physically reasonable. But, global properties concerning the existence and nature of singularities and the possibility of time travel lead to open questions of philosophical interest that are currently being investigated.

Last, but not least, Chris Smeenk's contribution concerns philosophical issues raised in contemporary work on cosmology. A common view is that cosmology requires a distinctive methodology because the universe-as-a-whole is a unique object. Restrictions on observational access to the universe due to the finite speed of light pose severe challenges to establishing global properties of the universe. How can we know that the local generalizations we take to be lawful in our limited region can be extended in a global fashion? Here, of course, there is overlap with the discussions of the previous chapter. Successes of the so-called Standard Model for cosmology include big-bang nucleosynthesis and the understanding of the cosmic background radiation, among others. Challenges to the Standard Model result from growing evidence that if it is correct, then *most* of the matter and energy present in the universe is not what we would consider ordinary. Instead, there apparently needs to be dark matter and dark energy. Smeenk provides an overview of recent hypotheses about dark matter and energy, and relates these discussions to philosophical debates about underdetermination. A different kind of problem arises in assessing theories regarding the very early universe. These theories are often motivated by the idea that the initial state required by the Standard Model is highly improbable. This deficiency can be addressed by introducing a dynamical phase of evolution, such as inflationary cosmology, that alleviates this need for a special initial state. Smeenk notes that assessing this response to fine-tuning is connected with debates about explanation and foundational discussions regarding time's arrow. One very important aspect of recent work in cosmology is the appeal to anthropic reasoning to help explain features of the early universe. A second recent development, often related to anthropic considerations, is the multiverse hypothesis—the existence of causally isolated pocket universes. This chapter brings these fascinating issues to the fore and raises a number of philosophical questions about the nature of explanation and confirmation appropriate for cosmology.

It is my hope that readers of this volume will gain a sense of the wide variety of issues that constitute the general field of philosophy of physics. The focus of the field has expanded tremendously over the last thirty years. New problems have come up, and old problems have been refocused and refined. It is indeed my pleasure to thank all of the authors for their contributions. In addition, I would like to thank Peter Ohlin from Oxford University Press. A number of others contributed to this project in various ways. I am particularly indebted to Gordon Belot, Julia Bursten, Nicolas Fillion, Laura Ruetsche, Chris Smeenk, and Mark Wilson for invaluable advice and support.

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CHAPTER 1

FOR A PHILOSOPHY OF HYDRODYNAMICS

OLIVIER DARRIGOL

Among the major theories of physics, hydrodynamics is probably the one that has received the least attention from philosophers of science. Until recently, three circumstances easily explained this neglect. First, there was very little historical literature on which philosophers could rely. Second, philosophers tended to focus on fundamental theories such as relativity theory and quantum theory and to neglect more phenomenological theories. Third, they harbored a neo-Hempelian concept of explanation following which the foundations of a theory implicitly contain all its explanatory apparatus.[1] Even Thomas Kuhn, who brought the "normal" phases of science to the fore, restricted conceptual innovation to the revolutionary phases.[2] Since the fundamental equations of hydrodynamics have remained essentially the same for about two centuries, this view reduces the development of this theory to a matter of technical prowess in solving the equations.

In recent years these three circumstances have lost much of their weight. We now have fairly detailed histories of hydrodynamics.[3] The superiority of fundamental theories over lower scale or phenomenological theories has been multiply challenged, both within science and in the philosophy of science.[4] And there has been a growing awareness that explanation mostly resides in devices that are not contained in the bare foundations of a theory. For example, Mary Morgan and

I thank Robert Batterman for his useful comments on a draft of this essay.

1 For a criticism of the Hempelian view, cf. Heidelberger 2006, 49–50.
2 Kuhn 1961.
3 Darrigol 2005, hereinafter abbreviated as *WF*; Eckert 2005.
4 Cf., e.g., Cartwright 1983, 1999; Cat 1998.

Margaret Morrison have emphasized the role of models as mediators between theory and experiment; Jeffry Ramsey has argued the conceptual significance of approximations and "transformation reductions"; Robert Batterman has made explanation depend on strategies for the elimination of irrelevant details in the foundations; Paul Humphreys has placed computability at the center of his assessment of the nature and value of scientific knowledge. Eric Winsberg has shown the importance of extratheoretical considerations in judging the validity of numerical simulations based on the fundamental equations. Already in 1983, Ian Hacking and C. W. F. Everitt, who were more in touch with the actual practice of physicists than average philosophers, introduced "theory articulation" or "calculation" as an essential "semantic bridge between theory and observation."[5]

Granting that theory articulation is as philosophically important as the building of foundations, hydrodynamics becomes a topic of exceptional philosophical interest largely because of the huge time span between the establishment of its foundations and its successful application to some of the most pressing engineering problems. This delay is an indirect proof of the creativity needed to expand the explanatory power of theories. It enables us to observe a rich sample of the devices through which explanatory expansion may occur. Margaret Morrison, Michael Heidelberg, and Moritz Epple have recently given philosophical studies of two of these devices: Ludwig Prandtl's boundary-layer theory and his wing theory. The present essay is conducted in the same spirit.[6]

The first section gives a few historical examples of the means by which hydrodynamics became applicable to a growing number of concrete situations. The second provides a tentative classification of these means. The third contains a definition of physical theories that includes their evolving explanatory apparatus. Special emphasis is given to a "modular structure" of theories that makes them more amenable to tests, comparisons, communication, and construction.[7]

1. Some History

In the mid-eighteenth century, Jean le Rond d'Alembert and Leonhard Euler formulated the general laws of motion of a nonviscous fluid. In Euler's form, calling $\mathbf{v}$ the velocity of the fluid, P its pressure, ρ its density, and $\mathbf{f}$ an impressed force

[5] Morrison and Morgan 1999; Ramsey 1993, 1995; Batterman 2002; Humphreys 2004; Winsberg 1999; Hacking 1983, 215. Kuhn earlier applied the word "articulation" to the paradigms of normal science. In 1974, Hilary Putnam noted "in passing" a pervasive but neglected schema for scientific problems, "schema III," in which the fundamental laws of the theory and some auxiliary statements are known but the factual consequences are unknown (Putnam 1974, 261–62).

[6] Morrison 1999; Heidelberger 2006; Epple 2002.

[7] The foundations of hydrodynamics, though historically stable, are not devoid of philosophical interest. As Clifford Truesdell pointed out long ago, some of its basic concepts, such as the concept of internal stress, are indeed problematic (Truesdell 1968; Darrigol 2007). The relation of these foundations to general mechanics and to statistical mechanics (for instance, the kinetic theory of gases) is another philosophically interesting topic (Yamalidou 1998). For the sake of homogeneity, I confine this essay to post-foundational developments.

density, these laws are given by the equation of motion

$$\rho\left(\frac{\partial \mathbf{v}}{\partial t}+(\mathbf{v}\cdot\nabla)\mathbf{v}\right)=\mathbf{f}-\nabla P,$$

the equation of continuity,

$$\nabla\cdot\rho\mathbf{v}+\frac{\partial\rho}{\partial t}=0,$$

and the boundary condition that the fluid velocity next to the walls of a rigid container should be parallel to these walls. If the fluid has a free surface at which it touches another fluid, the boundary conditions (later provided by Lagrange) are the equality of the pressures of the two fluids, and the condition that a particle of the surface of one fluid should remain on its surface.[8]

Euler's derivation of the equation of fluid motion assumes the pressure between two contiguous fluid parts to be perpendicular to the separating surface, as is the case in hydrostatics. In 1822 Claude Louis Navier implicitly dropped this assumption by comparing the internal fluid forces with the molecular forces of his general theory of elasticity. The resulting equation of motion is the Navier-Stokes equation

$$\rho\left[\frac{\partial \mathbf{v}}{\partial t}+(\mathbf{v}\cdot\nabla)\mathbf{v}\right]=\mathbf{f}-\nabla P+\mu\Delta\mathbf{v},$$

which involves the viscosity μ. This equation was reinvented several times. There was much hesitation on the proper boundary conditions, although in 1845 George Gabriel Stokes correctly argued for a vanishing relative velocity of the fluid next to rigid bodies.[9]

From a mathematical point of view, the most evident goal of the theory is to integrate the equations of motion for any given initial conditions and boundary conditions. There are at least three reasons not to confine fluid mechanics to this goal:

1. In the case of a compressible fluid, the system of equations is not complete because one needs the relation between pressure and density. This relation implies thermodynamic considerations, and therefore forces us to leave the narrow context of fluid mechanics.
2. It is generally impossible to solve the equations by analytical means because of their nonlinear character. Moreover, the few restricted cases in which this is possible may have little or no resemblance with actual flow because of instabilities. Nowadays, numerical integration is often possible and is, indeed, sufficient for some engineering problems. This leads us to the third caveat.
3. The answer to most physical questions regarding fluid behavior is not to be found in the solution of specific boundary-value problems. Rather, the

[8] Euler 1755. Cf. Truesdell 1954; *WF*, chap. 1.
[9] Navier 1822; Stokes [1845] 1849. Cf. *WF*, chap. 3.

physicist is often interested in generic properties of classes of solutions. In mathematical terms, we need to have a handle on the structure of the space of solutions.

What do physicists do when the solution of boundary problems no longer serves their interests? In order to answer this question, we will consult some of the historical evolution of hydrodynamics.

1.1 Bernoulli's Law

From a practical point of view, the main result that Euler could derive from his new hydrodynamics was the law

$$P = \rho \mathbf{g} \cdot \mathbf{r} - \tfrac{1}{2}\rho v^2 + \text{constant}$$

relating the pressure P, the position $\mathbf{r}$, and the velocity $\mathbf{v}$ for the steady motions of an incompressible fluid that admit a velocity potential ($\mathbf{g}$ is the acceleration of gravity). This achievement may seem meager for the following reasons:[10] the law had already been derived by Daniel Bernoulli in the 1730s as an application of the conservation of live force (energy) to steady, parallel-slice, incompressible fluid motion; the law requires a narrow specialization of the theory; one aspect of this specialization, the existence of a velocity potential, is (or was) physically obscure (its original purpose was to simplify the equations of motion and to permit their integration); under this specialization, the law is a straightforward mathematical consequence of Euler's equations.

From these remarks, one might be tempted to judge that Bernoulli's law adds nothing significant to the fundamental equations of hydrodynamics. Yet the practice of physicists and engineers suggests the contrary: This law is used in many circumstances, surely more often than Euler's equations themselves. There are several good reasons for this:

(1) Bernoulli's law relates easily accessible parameters of fluid motion in a simple manner, without any reference to the subtleties of the underlying dynamics;
(2) it is related to the general principle of energy conservation, which bridges hydrodynamics with mechanics;
(3) it provides the basis for the hydraulicians' language of pressure head, velocity head, and gravity head; and
(4) this language is still used when the law is violated.

Although this last point may seem paradoxical, it illustrates a highly important mode of concept formation in the post-foundational life of a theory: the solutions of the general theory are characterized with reference to the solutions of a more workable specialization of this theory. The concepts engendered by the specialization

[10] Euler 1755; Bernoulli 1738.

thus enrich the language of the general theory. They are useful as long as the law is valid in parts of the investigated system and as long as the loci of its violations are sufficiently understood. In typical hydraulic systems, there are regular pipes and reservoirs in which the law applies with a known correction (viscous or boundary-layer retardation in pipes) and there are phenomenologically or theoretically known "losses of head" when some accidents, such as pipe-to-pipe connections or sudden enlargements of the section of a pipe, occur.

1.2 Surface Waves

Historically, the second successful application of Euler's equations was to the problem of water waves. In this case, specialization is also necessary: the fluid is taken to be incompressible and a velocity potential is assumed. Moreover, some approximations must be introduced to circumvent the nonlinearity of the equations. In a memoir of 1781, Joseph Louis Lagrange originally assumed waves of small amplitude and of length much larger than the depth of the water. In the mid-1810s, Siméon Denis Poisson and Augustin Cauchy did without the latter approximation. The resulting differential equation for the deformation of the water surface is linear, and it admits sine-wave solutions whose propagation velocity depends on the wavelength. At this (first-order) approximation, one may use an autonomous language of sine waves that is no longer reminiscent of the underlying fluid dynamics and that is equally applicable to other kinds of linear waves. All one needs to know is how to combine (superpose) various sine waves in order to accommodate given initial shapes or perturbations of the water surface. We here encounter a second case of bridging of hydrodynamics with other theories: the introduction of concepts that apply to similar modes of motion in different theories (optics, hydrodynamics, acoustics . . .).[11]

This is not to say that all linear wave problems are understood once we know the dispersion law (how the velocity of a sine wave depends on its wavelength). Historically, much effort was needed to understand the structure of a superposition of sine waves. Employing strictly mathematical methods, Poisson and Cauchy only succeeded in describing the wave created by a stone thrown into a pond. John Scott Russell (in 1844) and William Froude (in 1873) later observed that the front of a group of waves traveled at a smaller velocity than individual waves in the group. In 1876, Stokes gave the modern theoretical explanation in terms of phase and group velocity. Ten years later, William Thomson (Lord Kelvin) determined the form of ship waves by a clever application of these concepts. On the physical side of his deduction, he relied on the optical "principle of interference." On the mathematical side, he invented the method of the stationary phase, which is now commonly used in various domains of physics. Again, we have a case of concepts and tools generated in a region of a given theory but ultimately applied to regions

[11] Lagrange 1781; Poisson 1816; Cauchy [1815] 1827. Cf. *WF*, 35–47.

of many other theories (by region, I mean a restriction of the theory to a limited class of systems and boundary conditions). These concepts were partly derived by a mathematical process of specialization and approximation, partly by observation, partly by analogy with other domains of physics.[12]

Similar remarks apply to the case of nonlinear waves. George Biddell Airy and Stokes tamed nonlinear periodic waves by successive approximations to the fundamental equations, with applications to ocean waves and river tides. This was a mostly mathematical process of a cumbersome but fairly automatic nature. In contrast, Scott Russell observed solitary waves (isolated swells) of invariable shape long before theorists admitted their possibility. When Joseph Boussinesq and Lord Rayleigh at last deduced such waves from theory, it became clear that qualitative results (such as the deformation of traveling waves) derived by considering separately a small-depth (nondispersive) approximation and a small-amplitude (linear) approximation, no longer obtained when the depth and amplitude were both large. The compensation of the dispersive and nonlinear causes of deformation for waves of a properly selected shape is a mechanism which, again, applies to many other domains of physics.[13]

1.3 Vortex Motion

Early fluid mechanics usually assumed the existence of a velocity potential because it greatly simplified the fundamental equations and also because Lagrange had shown that it resulted from the equations of motion for a large class of boundary conditions (motion started from rest and caused by moving solids). Another reason, emphasized by British fluid theorists, was the fact that the velocity potential of an incompressible fluid obeys the same differential equation (Laplace's equation) as the gravitational, electric, and magnetic potentials. This formal analogy was a constant source of inspiration for Stokes, Thomson, and James Clerk Maxwell. It permitted an intuitive demonstration of some basic theorems of the abstract "potential theory," and it provided fluid-mechanical analogs of electrostatic, electrokinetic, and magnetostatic phenomena.

The general case in which no velocity potential exists was judged intractable until 1858 when Hermann Helmholtz discovered a few remarkable theorems that pushed this case to the forefront of the theory. As Cauchy and Stokes had earlier proved, the infinitesimal evolution of a fluid element can be regarded as the superposition of three kinds of motion: a translation of the center of gravity of the element, a dilation of the element along three mutually orthogonal axes, and a rotation. Formally, the rotation per unit time is half the vector $\boldsymbol{\omega} = \nabla \times \mathbf{v}$, which has the components $\partial v_z/\partial y - \partial v_y/\partial z$ etc. This vector, now called *vorticity*, vanishes if and only if there exists a velocity potential (in a connected domain). This

12 Stokes 1876; Thomson 1887b. Cf. *WF*, 85–100.

13 Airy 1845; Stokes 1847; Russell 1839; Boussinesq 1871; Rayleigh 1876a. Cf. *WF*, 69–84. Similar comments could be made about the compression waves studied by Euler and Lagrange.

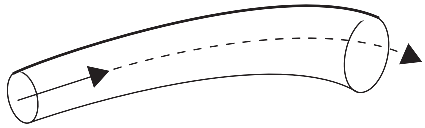

Figure 1.1 A portion of a vortex filament. The product of the vorticity (indicated by the arrows) by the normal section of the filament is a constant along the filament. It is also invariable during the motion of the fluid.

kinematic analysis of infinitesimal fluid motion is part of the conceptual furniture of modern fluid mechanics. Maxwell used it to develop the physico-mathematical concepts of curl and divergence that apply to any field theory. Helmholtz reinvented it to interpret the non-existence of the velocity potential and the vector $\boldsymbol{\omega} = \nabla \times \mathbf{v}$ geometrically.[14]

Helmholtz extended the geometrical interpretation to the "vorticity equation,"

$$\frac{\partial \boldsymbol{\omega}}{\partial t} + (\mathbf{v} \cdot \nabla)\boldsymbol{\omega} = (\boldsymbol{\omega} \cdot \nabla)\mathbf{v},$$

which derives from Euler's equations when the fluid is incompressible. For this purpose, he defined *vortex filaments* as thin bundles of lines everywhere tangent to the vorticity, and the *intensity of a filament* as the product of a normal section of this filament by the value of the vorticity in the section (see figure 1.1). He then showed that the intensity of a filament was a constant along a filament and that the vorticity equation was equivalent to the statement that the vortex filaments moved together with the fluid without altering their intensity. This theorem implies that the distribution of vorticity in a perfect liquid is in a sense invariant: it travels together with the fluid without any alteration.[15]

In this light, Helmholtz argued that the vorticity field (as today's physicists say) better represented arbitrary flows than the velocity field: its invariant properties completely determine the rotational component of the flow, while the irrotational component is ruled by the theorems of potential theory. With the help of an electromagnetic analogy, Helmholtz then determined the velocity fields associated with simple distributions of vorticity: straight vortex lines, vortex sheets, and vortex rings. He also calculated the interactions of vortices and verified his predictions experimentally.

The vortex sheets played an important role in Helmholtz's later writings. They are mathematically equivalent to a finite slide of fluid over fluid, and they should occur, according to Helmholtz, whenever a fluid is forced to pass the edge of an immersed body. As an illustration of this process, Helmholtz gave the formation of smoke jets when he blew the smoke of a cigar through his lips. Through ingenious reasoning, he proved the instability of the discontinuity surfaces or vortex sheets: any small bump on them must roll up spirally. This mechanism, now

14 Helmholtz 1858. Cf. *WF*, 149.
15 Cf. *WF*, 148–58.

called Helmholtz-Kelvin instability, plays an important role in many hydraulic and meteorological phenomena, as Helmholtz himself foresaw.[16]

Helmholtz not only meant to improve the applicability of hydrodynamics but also to equip this theory with a new mode of description for fluid motion in which vortices and discontinuity were the leading structural features. The enormous success of this project in the later history of hydrodynamics is somewhat paradoxical, because Helmholtz's theorems only hold in the unrealistic case of a perfect liquid. The physicists' use of the vorticity concept in much more general situations is comparable to the hydraulicians' use of the concept of hydraulic head in situations in which Bernoulli's theorem does not apply. In some cases of vortex motion, the effects of compressibility and viscosity can be shown to be negligible. In all cases, one can take Helmholtz's theorems as a reference and correct them through terms derived from the Navier-Stokes equation, as Vilhelm Bjerknes did in the late nineteenth century. As for the vortex sheets, we will see in a moment that in the early twentieth century Ludwig Prandtl used them to approximately describe important aspects of fluid resistance at high Reynolds number (low viscosity).[17]

In the historical examples discussed so far, it became increasingly difficult to produce the needed new conceptual apparatus. The degree of difficulty can be taken to be proportional to the time elapsed between the invention of Euler's equations and the introduction of this apparatus. For example, Bernoulli's law was easiest to derive, as it only requires a simple integration. But pure mathematics did not suffice to discover the laws of wave propagation on a water surface. Some intuition of interference processes (borrowed from optics), and also a few experimental observations (groups of waves, solitary waves), were instrumental. The discovery of the laws of vortex motion was even more difficult. A century elapsed from the time when d'Alembert and Euler gave the vorticity equation to the time when Helmholtz interpreted it through his theorem. Experiments or observations did not by themselves suggest this interpretation, though Helmholtz's efforts were, in fact, part of a project for improving the theoretical understanding of organ pipes. Helmholtz's success primarily depended on his ability to combine various heuristic devices including algebraic manipulation in the style of Lagrange, geometric visualization in the style of Thomson and Maxwell, and a focus on invariant quantities as exemplified in his own work on energy conservation.

1.4 Instabilities

Exact solutions of Euler's or Navier's equations under given boundary conditions may differ widely from the flow observed in a concrete realization of these conditions. For instance, the flow of water in a pipe of rapidly increasing diameter never has the smooth, laminar character of exact steady solutions of the Navier-Stokes

16 Helmholtz 1868, 1888. Cf. *WF*, 159–71.
17 Bjerknes 1898; Prandtl 1905.

equation in this case. As Stokes already suspected in the 1840s, this discrepancy has to do with the instability of the exact steady solutions: any small perturbation of these solutions will induce wide departures from the original motion. Consequently, the knowledge of exact solutions of the fundamental equations or (more realistically) the knowledge of some features of these solutions under given boundary conditions is not sufficient for the prediction of observed flows. One must also determine whether these solutions or features are stable.[18]

In principle this question can be mathematically decided, by examining how a slightly perturbed solution of the equations evolves in time. As we saw, a first success in this direction was Helmholtz's prediction of the spiral rolling up of a bump on a discontinuity surface. Later in the century, Lord Rayleigh and Lord Kelvin treated the more difficult problem of the stability of plane parallel flow. Their results were only partial (Rayleigh's inflection theorem in the nonviscous case), or wrong (Kelvin's prediction of stability for the plane Poiseuille flow). Most of these questions exceeded the mathematical capacity of nineteenth-century theorists, and some of them have remained unresolved to this day. The efforts of Rayleigh and others nonetheless yielded a general method and language of perturbative stability analysis. Rayleigh linearized the equation of evolution of the perturbation, and sought plane-wave solutions. These solutions are "proper modes" whose oscillatory or growing character depends on the real or imaginary character of the frequency. This proper-mode analysis of stability goes beyond hydrodynamics: it originated in Lagrange's celestial mechanics and it can be found in many other parts of physics.[19]

As the mathematical discussion of stability was nearly as difficult as the finding of exact solutions of the fundamental equations, the most important results in this domain were reached by empirical means. Plausibly, the observed instability of jets motivated Helmholtz's derivation of the instability of discontinuity surfaces. Certainly, Tyndall's observations of this kind motivated Rayleigh's calculations for parallel flow. Most important, Gotthilf Hagen (1839) and Osborne Reynolds (1883) discovered that pipe flow, for a given diameter and a given viscosity, suddenly changed its character from laminar to turbulent when the velocity passed a certain critical value. The sharpness of this transition was a surprise to all theorists. From Reynolds to the present, attempts to mathematically determine the critical velocity (or Reynolds number) in cylindrical pipes have failed. This is a question of academic interest only, because unpredictable entrance effects (the way the fluid is introduced into the pipe), not the inherent instability in a pipe of infinite length, usually determine the transition.[20]

In the twentieth century, significant progress has been made in understanding the transition from laminar to turbulent flow. In the first half of the century, Ludwig Prandtl, Walter Tollmien, Werner Heisenberg, and Chia Chiao Lin proved the

[18] Stokes 1843. Cf. *WF*, 184–87.
[19] Rayleigh 1880; Thomson 1887a. Cf. *WF*, 208–18; Drazin and Reid 1981.
[20] Tyndall 1867; Hagen 1839; Reynolds 1883. Cf. *WF*, 243–63.

instability of the plane Poiseuille flow and unveiled the spatial periodicity of the mechanism of this instability.[21] In the second half of the century, developments in the theory of dynamical systems at the intersection between pure mathematics, meteorology, and hydrodynamics permitted a detailed qualitative understanding of the transition to turbulence, with intermediate oscillatory regimes, bifurcations, and strange attractors.[22] It remains true that most of the practical applications of hydrodynamics only require a rough knowledge of the conditions under which turbulence occurs. The source of this knowledge is partly theoretical and partly empirical. There is no easy way to gather it from the fundamental equations. In most cases, the best that can be done is to repeat Reynolds's rough argument that the full vorticity equation has two terms, a viscous term that tends to damp any eddying motion, and an inertial term which preserves the global amount of vorticity. The laminar or turbulent character of the motion depends on the ratio of these two terms, whose order of magnitude is given by the Reynolds number.

1.5 Turbulence

The state of motion that follows the turbulent transition is even more difficult to analyze than the transition itself. Casual observation of turbulent flow reveals its chaotic and multi-scale character. The detailed description of any motion of this kind seems to require a huge amount of information, much more than is humanly accessible (without computers at least). As Reynolds pondered, we are here facing a situation similar to that of the kinetic theory of gases: the effective degrees of freedom are too numerous to be handled by a human calculator. Unfortunately, turbulent motion is more often encountered in nature and in manmade hydraulic devices than laminar motion. Engineers and physicists have had to invent ways of coping with this difficulty.[23]

One strategy is to design the hydraulic or aeronautic artifacts so that turbulence does not occur. When turbulence cannot be avoided, one may adopt a purely empirical approach and seek relations between measured quantities of interest. For instance, nineteenth-century engineers gave empirical laws for the retardation (loss of head) in hydraulic pipes. A second approach is to find rules allowing the transfer of the results of measurements done at one scale to another scale. Stokes, Helmholtz, and Froude pioneered this approach in the contexts of pendulum damping, balloon steering, and ship resistance, respectively. They derived the needed scaling rules from the scaling symmetries of the Navier-Stokes equation or of the underlying dynamical principles. This is an example of a hybrid approach, founded partly on the fundamental equations, and partly on measurements of theoretically unpredictable properties.[24]

[21] Cf. Eckert 2008.
[22] Cf. Franceschelli 2007.
[23] Reynolds 1895. Cf. *WF*, 259–60.
[24] Stokes 1850; Helmholtz 1873; Froude [1868] 1957; 1874. Cf. *WF*, 256–58, 278–79.

In a third approach, one may completely ignore the foundations of fluid mechanics and cook up a model based on a grossly simplified picture of the flow. An important example is the laws for open channel flow discovered in the 1830s and 1840s by a few French Polytechnique-trained engineers: Jean Baptiste Bélanger, Jean Victor Poncelet, and Gaspard Coriolis. They assumed the flow to occur through parallel slices that rubbed against the bottom of the channel according to a phenomenological friction law, and they applied momentum or energy balance to each slice.[25]

In the 1840s Adhémar Barré de Saint-Venant emphasized the "tumultuous" character of the fluid motion in open channel flow and suggested a distinction between the large-scale average motion of the fluid and the smaller-scale tumultuous motion. The main effect of the latter motion on the former, Saint-Venant argued, was to enhance momentum exchange between successive (large-scale) fluid layers. Based on this intuition, he replaced the viscosity in the Navier-Stokes equation with an effective viscosity that depended on various macroscopic circumstances such as the distance from a wall. In the 1870s, Boussinesq solved the resulting equation for open channels of simple section and thus obtained laws that resembled Bélanger's and Coriolis's laws, with different interpretations of the relevant parameters.[26]

In 1895, Reynolds relied on analogy with the kinetic theory of gases to develop an explicitly statistical approach to turbulent flow. In the spirit of Maxwell's kinetic-molecular derivation of the Navier-Stokes equation, he derived a large-scale equation of fluid motion by averaging over the small-scale motions governed by the Navier-Stokes equation. Reynolds's equation depends on the "Reynolds stress," which describes the turbulent exchange between successive macro layers of the fluid. Like Saint-Venant's effective viscosity, the Reynolds stress cannot be determined without further assumptions concerning the turbulent fluctuation around the large-scale motion. There have been many attempts to fill this gap in the twentieth century. The most useful ones were Kármán's and Prandtl's derivations of the logarithmic velocity profile of a turbulent boundary layer. The assumptions made in (improved) versions of these derivations are simple and natural (uniform stress, matching between the turbulent layer and a laminar sublayer next to the wall), and the resulting profile fits experiments extremely well (much better than earlier phenomenological laws). The logarithmic profile is the basis of every modern engineering calculation of retardation in pipes or open channels.[27]

Despite powerful studies by Geoffrey Taylor, Andrey Nikolaevich Kolmogorov, and many others, the precise manner in which turbulence distributes energy between different scales of fluid motion remains a mystery.[28] There is no doubt, however, that the general idea of describing turbulent flow statistically has been fruitful since its first intimations by Saint-Venant, Boussinesq, and Reynolds. In the case

[25] Cf. *WF*, 221–28.
[26] Saint-Venant 1843; Boussinesq 1877. Cf. *WF*, 229–38.
[27] Reynolds 1895; Kármán 1830; Prandtl 1831 Cf. Eckert 2005, chap. 5; *WF*, 259–62, 297–301.
[28] Cf. Farge and Guyon 1999; Frisch 1995.

of turbulent fluid mechanics, as in statistical mechanics, a new conceptual structure emerges at the macroscale of description. Similar questions can be raised in both cases concerning the nature of the reduction or emergence. Does the microscale theory truly imply the macroscale structure? Is this structure uniquely defined? Can this structure be used without further reference to the microscale? Are there singular situations in which the reduction fails? The answers to these questions tend to be more positive in the case of statistical mechanics than in the case of the statistical theory of turbulence, because the relevant statistics are better known in the former than in the latter case.

1.6 Boundary Layers

From a practical point of view, two outstanding problems of fluid mechanics are fluid resistance and fluid retardation. Fluid resistance is the decelerating force experienced by a rigid body moving through a fluid. Fluid retardation is the fall of pressure or loss of head experienced by a fluid during its travel along pipes or channels. The two problems are related, since they both involve the mutual action of a fluid and an immersed solid. In 1768, d'Alembert challenged "the sagacious geometers" with the paradox that resistance vanished for a perfect liquid in his new hydrodynamics. There were various strategies to circumvent this theoretical failure. Some engineers determined by purely empirical means how the resistance depended on the velocity and shape of the immersed body. Others retreated to Isaac Newton's naïve theory by the impact of fluid particles on the front of the body, although some consequences of this theory (such as the irrelevance of the shape of the end of the body) had already been refuted. In the mid-nineteenth century, Saint-Venant, Poncelet, and Stokes traced resistance to viscosity and the production of eddies. With the damping of pendulums in mind, Stokes successfully determined the resistance of small spheres and cylinders by finding solutions to the linearized Navier-Stokes equation. For most practical problems, the larger size of the immersed body and the smallness of the viscosities of air and water imply that the nonlinear term of this equation cannot be neglected (the Reynolds number is too high). Stokes had nothing to say in such cases beyond the qualitative idea of dissipation by the production of eddies.[29]

In the ideal case of vanishing viscosity, the proof of d'Alembert's paradox implicitly assumes the continuity of the fluid motion. However, Helmholtz's study of vortex motion implies that finite slip of fluid over fluid is perfectly compatible with Euler's equations. Around 1870, Kirchhoff and Rayleigh realized that Helmholtz's discontinuity surfaces yielded a finite resistance for an immersed plate. According to Helmholtz, a tubular discontinuity surface is indeed produced at the sharp edges of the plate. The water behind the plate and within this surface is stagnant, so that its pressure vanishes (when measured in reference to its uniform value at

[29] D'Alembert 1768; Saint-Venant 1843; Poncelet 1839; Stokes 1850. Cf. *WF*, 135–39, 265–67, 270–73.

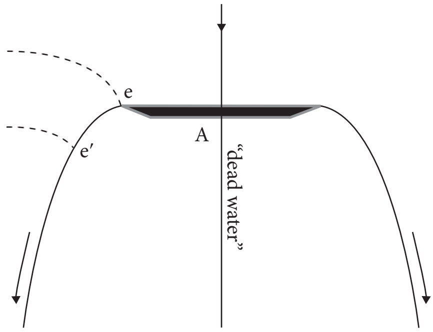

Figure 1.2 Discontinuity surface (ee′) formed when a downward flow encounters the disk A. From Thomson (1894, 220).

infinite distances from the plate) (see figure 1.2). Since the pressure at the front of the plate is positive, there is a finite resistance, which Kirchhoff and Rayleigh determined by analytical means. The result roughly agreed with the measured resistance.[30]

In the case of ships, the resistance problem is complicated by the fact that ships are not supposed to be completely immersed. Consequently, wave formation at the water surface is a significant contribution to the resistance. The leading nineteenth-century experts on this question, William John Macquorn Rankine and William Froude, distinguished three causes of resistance: wave resistance, skin resistance, and eddy resistance. Skin resistance corresponds to some sort of friction of the water when it travels along the hull. Eddy resistance corresponds to the formation of eddies at the stern of the ship; it is usually avoided by proper profiling of the hull. Rankine and Froude traced skin resistance to the formation of an eddying fluid layer next to the hull. They derived this notion from the observation that the flow of water around the ship, when seen from the deck, appears to be smooth everywhere expect for a narrow tumultuous layer next to the hull and for the wake. Rankine assumed the validity of Euler's equations in the smooth part of the flow and solved it to determine the hull shapes that minimized wave formation. Froude gave a fairly detailed description of the mechanism of retardation in the eddying layer, although he was not able to draw quantitative conclusions. In the end, Froude measured skin friction on plates, total resistance on small-scale ship models, and then used separate scaling laws for skin and wave resistance in order to determine the resistance of a prospective ship hull.[31]

In sum, Rankine and Froude distinguished two different regions of flow amenable to different theoretical or semi-empirical treatments and combined the resulting insights to determine the total resistance. Froude thus obtained the first quantitative successes in the problem of fluid resistance at a high Reynolds number.

30 Kirchhoff 1869; Rayleigh 1876b.
31 Rankine 1858, 1865, 1870; Froude 1874, 1877. Cf. Wright 1983; *WF*, 273–82.

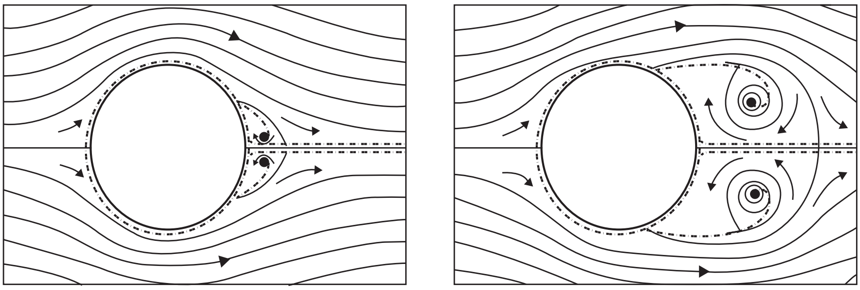

Figure 1.3 Formation of a discontinuity surface behind a cylinder. From Prandtl (1905, 579–80).

Although his and Rankine's considerations appealed to higher theory in several manners, they also required considerable empirical input.

The next and most famous progress in the high Reynolds-number resistance problem occurred in Göttingen, under the leadership of Ludwig Prandtl. Impressed by the qualitative success of Helmholtz's surfaces of discontinuity, Prandtl assumed that the solution of the Navier-Stokes equation for high-Reynolds flow around a body somewhat resembled a solution of Euler's equation (with strictly vanishing viscosity). In the latter solution, the fluid slides along the surface of the body, whereas for a viscous fluid the relative velocity of the fluid must vanish at the surface of the body. Consequently, for the real flow Prandtl assumed a thin (invisible) layer of intense shear that imitated the finite slide of the Eulerian solution. He also assumed that in some cases this layer could shoot off the surface of the body to mimic a Helmholtzian surface of discontinuity (with its characteristic instability resulting in an eddying trail). This is the so-called separation process. Outside the boundary layer, Prandtl naturally applied Euler's equations. Within the boundary layer, the intense shear allowed him to use an approximation of the Navier-Stokes equation that could be integrated to determine the evolution of the velocity profile along the body. For sufficiently curved bodies, Prandtl found that at some point the flow was inverted in the part of the boundary layer closest to the body. He interpreted this point as the separation point from which a (quasi) discontinuity surface was formed. In the case of a flat or little curved surface (for which separation does not occur), he determined the resistance by integration of the sheer stress along the surface of the body. He illustrated the separation process through experiments done with a tank and a paddle-wheel machine (figure 1.3).[32]

Comparison with Froude's earlier concept of eddying layer leads to the following remarks. Unlike Froude, Prandtl was able to determine theoretically and precisely the flow within the boundary layer. This determination requires a previous solution of the Eulerian flow problem around the body, because the evolution of the boundary layer depends on the pressure at its confines. Conversely, this evolution may

[32] Prandtl 1905. Cf. Eckert 2005, chap. 2; Heidelberger 2006; *WF*, 283–89.

induce separation, which necessarily affects the Eulerian part of the flow. Prandtl himself emphasized this interaction between the Eulerian flow and the boundary layer. Whereas Froude had no interest in separation (which ship builders systematically avoided), Prandtl had a precise criterion for its occurrence. Whereas Froude could only measure the sheer stress of the boundary layer, Prandtl could determine it theoretically.

So far the comparison seems to favor Prandtl. In reality, in many cases including ship resistance, the boundary layer has an internal turbulence that is not taken into account in Prandtl's original theory. In 1913, Prandtl's former student Heinrich Blasius found that beyond a certain critical Reynolds number, the edgewise resistance of a plate obeyed Froude's empirical law and not Prandtl's theoretical law. Prandtl explained that the profile of a laminar boundary layer could become unstable and thus lead to a turbulent boundary layer à la Froude. He used this notion to explain the bizarre reduction of the resistance of spheres that Gustave Eiffel had observed at a certain critical velocity: turbulence in a boundary layer, Prandtl explained, delays the separation process and thus sharply decreases the resistance. Paradoxically, it is when the boundary layer is turbulent that the global flow mostly resembles the smooth Eulerian flow.[33]

As the boundary layers around airplane wings are turbulent, Prandtl needed to know the sheer stress along such layers in order to determine the drag of the wings. He originally relied on plate resistance measurements, as Froude had done in the past. As was already mentioned, it became possible to calculate this stress in the 1830s when Kármán and Prandtl discovered the logarithmic velocity profile of turbulent layers.

It is now time to reflect on the relation that boundary-layer theory has to the foundational theory of Navier-Stokes. Prandtl's idea (if we believe his own plausible account) has its theoretical origin in the idea of using solutions to Euler's equations as a guide for solving the Navier-Stokes equation at a high Reynolds number. This is only a heuristic, because Prandtl had no mathematical proof that the low-viscosity limit of a solution of the Navier-Stokes equation is a solution of Euler's equation. Yet the motion imagined by Prandtl, with its Eulerian, high-sheer, and stagnant regions, clearly is an approximate solution of the Navier-Stokes equation. What is missing is a proof of the uniqueness of this solution (under given boundary conditions), as well as a general proof of its existence for any shape of the immersed body. With this concession, the boundary-layer theory can legitimately be regarded as an approximation of the Navier-Stokes theory.

An interesting feature of the boundary-layer theory is its use of different approximate equations in different regions of the flow. Our discussion of Bernoulli's law showed that this law is often used regionally (i.e., in laminar regions of the flow) with head losses localized in turbulent regions. Boundary-layer theory similarly introduces different regions of flow, although it does so in a more interactive manner. Each region is described through computable solutions of appropriate equations

[33] Prandtl 1914. Cf. Eckert 2005; *WF*, 293–94.

of motion, and the precise conditions for the matching of the regional solutions are known (continuity of pressure, stress, and velocity). These matching conditions imply causal relations between features of the two regions: for instance, the pressure distribution in the Eulerian region determines the evolution of the velocity profile in the boundary layer, and in the case of separated flow, the position of the separating surface affects the Eulerian region.[34]

In qualitative applications, Prandtl's theory may be restricted to the general ideas of a boundary layer, a free fluid, and their interaction sometimes leading to separation. In quantitative engineering applications, this picture must be supplemented with a law for the evolution of the sheer stress along a boundary layer (laminar or turbulent), and with quantitative criteria for separation and for the transition between laminar and turbulent layer. Granted that this supplementary information is available, the theory can be used without reference to the Navier-Stokes theory. The gain in predictive efficiency is enormous, as verified by the immense success of Prandtl's theory in engineering applications. Yet one should not forget that much of the supplementary information comes from the intimate connection between the boundary-layer theory and the Navier-Stokes theory. In fact the legitimacy of the whole picture depends upon this intimate connection. The boundary-layer theory, unlike the early French models of open channel flow, is not an ad hoc model that owes its simplicity to counterfactual assumptions. It is a legitimate articulation of the Navier-Stokes theory.

2. Explanatory Progress

The above examples make clear that in the course of its history, hydrodynamics has acquired a sophisticated explanatory apparatus without which it would remain merely a "paper" theory. The explanatory apparatus is presented in various chapters in modern textbooks. We will now reflect on the ways this apparatus was obtained, on its components, and on its functions.

2.1 The Sources of Explanatory Progress

In some cases, explanation was improved through blind mathematical methods. For instance, a simple integration yielded Bernoulli's law (after proper specialization), the symmetries of the Navier-Stokes equation yielded scaling laws, and standard approximation procedures yielded the theory of waves of small amplitude. Despite the relatively easy and automatic way in which these results were obtained, they considerably improved the explanatory power of the theory by directly relating quantities of physical interest.

34 Heidelberger 2006 rightly insists on this causal structure of the boundary-layer theory.

In other cases, more intra- or intertheoretical heuristics was needed. Kinematic analysis of the vorticity equation led to Helmholtz's vortex theorems; asymptotic reasoning led to Prandtl's notions of laminar boundary layer and separation; scaling and matching arguments led to the logarithmic velocity profile of turbulent boundary layers. These heuristics required an unusual amount of creativity; they involved intuitions bound to personal styles of thinking. Such intuitions are tentative and may lead to erroneous guesses. For instance, the great Kelvin erred in his stability analysis of parallel flow. A rigorous check of the compatibility of the conclusions with the fundamental equations is always needed.[35]

In still other cases, observations or experiments suggested new concepts such as group velocity, solitary waves, the stability or instability of laminar flow, and turbulent boundary layers. The very fact that pure theory was historically unable to lead to these concepts (and sometimes even resisted their introduction) shows the vanity of regarding them as implicit consequences of the fundamental equations. They nevertheless belong to fundamental hydrodynamics inasmuch as their compatibility with the fundamental equations can be verified a posteriori.

Lastly, the impossibility of solving the fundamental equation and the evident complexity of observed flows sometimes forced engineers and even physicists to arbitrarily and drastically simplify aspects of the flow. This happened for instance in early models of open channel flow. These models cannot be strictly regarded as parts of fundamental hydrodynamics, since some of their assumptions contradict both observed and theoretical properties of the flow. Yet their success suggests a looser sort of relation with the Navier-Stokes theory. In the case of open-channel flow, the models can be reinterpreted as re-parametrizations of the true equations for the approximate, large-scale motion derived from turbulent solutions of the Navier-Stokes equations.

In every case, the theoretical developments occurred with specific applications in mind: some kind of flow frequently observed in nature needed to be explained or the functioning of some instruments or devices needed to be understood. Purely mathematical methods broadly applied to general flow were of little avail. Insight was gained as a result of investigation directed at concrete and restricted goals. This is why the heroes of nineteenth-century and early twentieth-century fluid mechanics were either mathematically fluent engineers or physicists who had a foot in the engineering world.

2.2 The Components of Explanation

A first alley toward better explanation involves the restriction of the scope of a theory. The Navier-Stokes equations, regarded as the general foundation of hydrodynamics, can be specialized in various ways. There are homogeneous specializations or idealizations in which the restricted choice of parameters and kinds of systems

[35] On misleading intuitions in fluid mechanics, cf. Birkhoff 1950.

(boundary conditions) leads to more tractable integration problems or successful statistical approaches. Typical examples are irrotational Eulerian flow, low Reynolds-number flow, and fully turbulent flow. There are also heterogeneous specializations in which the restrictions on parameters and systems lead to flows that have different regions, each of which depends upon a specific simplification of the Navier-Stokes equations. This is the case for the high-Reynolds resistance problem and the airplane wing problem according to Prandtl. As was already mentioned, success here requires proper matching between the different regions.

Another explanatory resource is the identification of invariant structures of a flow belonging to a given class. The most impressive example of this sort is Helmholtz's demonstration of the conservation of vortex filaments. As the mind tends to focus on invariant aspects of our environment, the identification of new invariants often shape our descriptive language. As Helmholtz predicted, this has, in fact, happened in fluid mechanics: the vorticity field is now often preferred to the velocity field as a description of flow.

Third, instead of seeking structure in a given solution, we may attend to the structure of the space of solutions of the fundamental equation when the boundary conditions vary. For instance, we may ask whether laminar solutions are typical, whether small perturbations lead to different sorts of solutions: this is the issue of stability. We may also ask whether some classes of solution share common large-scale features, as we do in the statistical theories of turbulence. And, we may ask whether some properties or laws are generic in some regime of flow: this is the issue of universality, which we briefly touched with the logarithmic profile of turbulent boundary layers.

Lastly, explanation and understanding may come from linking hydrodynamics to other theories. We have encountered a few examples of this kind: potential theory, wave interference, group velocity, solitary waves, field kinematics, and proper-mode analysis of stability. In half of these cases, concepts of hydrodynamic origin were brought to bear on other theories and not vice versa. The cross-theoretical sharing of concepts nonetheless remains a token of their explanatory value.

2.3 A Pragmatic Definition of Explanation

As was stated above, the goal of fluid mechanics cannot be reduced to finding integrals of the fundamental equations that satisfy given boundary conditions. This is usually impossible by analytical means, and modern numerical means require a different simulation for each choice in the infinite variety of boundary conditions. As Batterman, Ramsey, and Heidelberger have argued, bare foundations do not answer the questions that truly interest physicists and engineers. Practitioners want to be able to characterize a physical situation by a humanly accessible number of physical parameters and to possess a picture of the situation that enables them to derive relations between these parameters in a reasonable amount of time. In other words, they need a concept of explanation that integrates our human capacity at representing and intervening. As Batterman emphasizes, this requires means for eliminating irrelevant details in our description of systems. This also implies the elaboration of a descriptive language, the concepts of which directly refer to controllable features of the system.[36]

With this pragmatic definition of explanation, it becomes clear that the earlier described developments of hydrodynamics served the purpose of increasing the explanatory power of the theory. Homogeneous specializations do so by offering adequate concepts and methods for certain kinds of flow. Heterogeneous specializations do so by combining the former specializations to describe flows that occur in problems of great practical import. The identification of invariant structures for certain classes of motion improves the economy of the representation. Attention to structure in the space of solutions enables us to decide to what extent smaller details of the motion affect the features of practical interest, and to what extent their effect can be smoothed out by some averaging process. Intertheoretical links produce familiar concepts that can indifferently be used in various domains of physics.

In this light, the practice of physics has more similarity with engineering than is usually assumed. The remark is not uncommon in recent writings in the philosophy of science. For instance, Ramsey revives J. J. Thomson's old characterization of theories as tools for solving physics or engineering problems; Epple compares the formation of Prandtl's wing theory to an engineering process combining multiple theoretical and experimental resources. In these scholars' view, the engineer only differs from the physicist by (usually) not participating in the invention of the theories and by his more systematic appeal to extra-theoretical components. Physicists and engineers not only share the goal of efficient intervention, they also share some of the means.[37]

Ramsey and Heidelberger insist that the articulation of theories implies the formation of new, adequate concepts. One could even argue that the bare Navier-Stokes theory has no physical concepts. It harbors only mathematical concepts such as the velocity field that correspond to an ideal description of the flow, ignoring molecular structure and presuming indefinite resolution. A concept, in the etymological sense of the word (*concipio* in Latin, or *begreifen* in German), is a mental means to grasp some concrete object or situation. Hydraulic head, vortices, wave groups, solitary waves, the laminar-turbulent transition, boundary layers, separation, etc. are concepts in this practical sense. The detailed velocity field or the various terms of the Navier-Stokes equation are not. What Thomas Kuhn once belittled as the "mopping up" of theories in the normal phases of science truly is concept formation.[38]

[36] Batterman 2002; Ramsey 1992, 1993, 1995; Heidelberger 2006.

[37] Ramsey 1995, 16; Epple 2002.

[38] Kuhn 1962, 24. Hilary Putnam similarly criticized another of Kuhn's characterizations of normal science: "The term 'puzzle solving' is unfortunately trivializing; searching for explanations of phenomena and for ways to harness nature is too important a part of human life to be demeaned" (Putnam 1974, 261).

3. Theories and Modules

3.1 Defining Physical Theories

Once we recognize the cognitive impotence of the bare foundations of a theory, we need a general definition of "theory" that is not limited to the fundamental equations and a few naïve rules of application. The definition must allow for evolving components, since the cognitive efficiency of any good theory always increases in time. It must include explanatory devices and it must allow the intertheoretical connectivity found in mature theories. The following is a sketch of such an enriched definition.[39]

A physical theory is a mathematical construct including:

(a) a *symbolic universe* in which systems, states, transformations, and evolutions are defined by means of various magnitudes based on Cartesian powers of **R** (or **C**) and on derived functional spaces.
(b) *theoretical laws* that restrict the behavior of systems in the symbolic universe.
(c) *interpretive schemes* that relate the symbolic universe to idealized experiments.
(d) methods of *approximation* and considerations of *stability* that enable us to derive and judge the consequences that the theoretical laws have on the interpretive schemes.

The symbolic universe and the theoretical laws are permanently given. They correspond to the "family of models" of the semantic view of physical theories. In the case of hydrodynamics, the symbolic universe consists in the velocity, pressure, and density fields for each fluid of the system, in the boundaries of rigid bodies that may or may not move, and in force densities such as gravity. The theoretical laws are the Navier-Stokes equations, boundary conditions, and (for compressible fluids) a relation between density and pressure that may involve modular coupling with thermodynamics (we will return to this point).

In the semantic view of theories, the empirical content of a theory is defined by an isomorphism between parts of the symbolic universe and empirical data; although the means by which this isomorphism is determined are usually left in the dark. The notion of an interpretive scheme is intended to fill part of this gap. By definition *an interpretive scheme consists in a given system of the symbolic universe together with a list of characteristic quantities that satisfy the three following properties.*(1) *They are selected among or derived from the (symbolic) quantities that define the state of this system.* (2) *At least for some of them, ideal measuring procedures*

[39] For a discussion of this definition and a comparison with the definition of Sneedian structuralists, cf. Darrigol 2008, 198–203.

are known. (3) *The laws of the symbolic universe imply relations of a functional or a statistical nature among them.* More specifically, interpretive schemes are blueprints of conceivable experiments whose outcomes depend only on relations between a finite set of mutually related quantities, a sufficient number of which are measurable. In some cases, the intended experiments may be designed to determine some theoretical parameters from the measured quantities. In other cases, the theoretical parameters are given, and theoretical relations between the measured quantities are verified. In all cases, the interpretive schemes do not contain rigid linguistic connections between theoretical terms and physical quantities; their concrete implementation is analogical, historical, and subject to revisions.[40]

The introduction of interpretive schemes implies a selection of systems and quantities from the infinite variety of elements in the symbolic universe of the theory. This selection can evolve dramatically with the number and nature of the imagined applications of the theory. The two main classes of interpretive schemes of early hydrodynamics were the pierced vessel, in which the efflux of water is related to the height of the water surface; and the resistance scheme in which a solid body immersed in a stream of water experiences a force related to the velocity of the stream. Another interesting scheme, Bernoulli's pipe of variable section, implied pressure measurement through vertical columns of water. A sample of later schemes includes the determination of the velocity of surface waves as a function of depth and wavelength, the visualized motion of vortices as a function of their relative configuration, the visualized lines of flow around an immersed body as a function of the asymptotic velocity, the drag and lift of a wing as a function of asymptotic velocity and angle of attack. Some schemes were reactions to well-identified practical problems and others to some new theoretical development. In the latter category, we may cite the determination of the separation point for the flow around an immersed sphere, the measurement of instability thresholds, Prandtl's aspiration of the boundary layer to prevent separation, and the post-theoretical visualization of laminar boundary layers.

For an interpretive scheme to serve its purpose as an experimental blueprint, a few conditions must be met: one must know how to realize concretely the system picked in the symbolic universe; one must know how to implement the ideal measuring procedures; one must be able to compute the relations between measured quantities and theoretical parameters; and one must know something about the stability of these relations. Point (d) of my general definition of theories is meant to meet these two last requirements. In this regard, the reader may consult the growing literature regarding the philosophy of approximation, numerical analysis, and stability. The following discussion is restricted to aspects of the working of interpretive schemes that have to do with the *modular structure* of theories.[41]

40 This is a considerable weakening of the logical-empiricist strictures on the meanings of theoretical terms.

41 A more detailed discussion is given in Darrigol 2008. Interpretive schemes supplemented with the requirement of computability are similar to Humphreys's "computational templates." According to Humphreys 2004, it is at the level of computational templates that questions about theoretical representation, empirical fitness,

3.2 Modules

By definition, a *module* is a component of a theory which is itself a theory, with a different domain of application. Our ability to apply a theory crucially depends on integrated modules. First, there are *defining modules* that serve to define some of the quantities in the symbolic universe. In the case of hydrodynamics, the list of these modules includes a Euclidian geometrical module that defines the spatial relations of the systems; a mechanical module that defines external force densities, external pressures, and the motion of immersed bodies; a thermodynamic module that defines relations between fluid density, pressure, and temperature (sometimes also heat transfer). These modular definitions enable us to transfer already known measuring procedures into the interpretive schemes of hydrodynamics. In the case of compressible fluids, they are essential to the completeness of the theory: no prediction can be made without knowing how the density varies according to the thermal properties of the system.

Second, there are *idealizing module*s obtained by simplifying the symbolic universe and retaining similar interpretive schemes (of course, the functional relations between schematic quantities are different). In the case of hydrodynamics, the most important modules of this kind are the theory of incompressible fluids, the theory of inviscid fluids, and the theory of incompressible inviscid fluids. Incompressibility enables us to ignore the coupling of hydrodynamics with thermodynamics. Inviscidity eliminates one term in the Navier-Stokes equations and yields Euler's simpler equations. The usefulness of these idealizations comes from the relative smallness of the compressibility of water and from the smallness of the viscosities of air and water.

Third, there are *specializing modules* that are exact substitutes of the theory for subclasses of schemes that meet certain conditions. For instance, Lagrange's theory of irrotational incompressible fluid motion can replace Euler's theory for schemes in which the fluid motion is started from rest by the motion of walls or immersed bodies; Helmholtz's theory of vortex motion can replace the incompressible specialization of Euler's theory for schemes based on the vortex structure.

Idealizing and specializing modules are not by themselves sufficient to design effective interpretive schemes. We also need *approximating modules* that can be seen as limits of the theory for a given subclass of systems when a parameter of this class or a parameter of the symbolic universe (or a combination of both kinds of parameters) takes extreme but still finite values (the limit may involve statistical considerations). Hydrodynamic examples of modules of this kind concern the small-depth and small-amplitude limits of surface wave schemes, the high Reynolds-number limit of fluid resistance or fluid retardation schemes (boundary-layer theory), and the low Reynolds-number limit of these schemes (creeping flow). In most cases, it is only at

realism, and so on must be discussed; knowledge "in principle" must be subordinated to knowledge "in practice," which involves the available technologies of measurement and calculation.

the level of approximating modules that the functional relations between schematic quantities can be effectively computed.[42]

There is a last kind of modules, the *reducing modules*, that has more to do with the foundations of the theory than with its applications. These are theories diverted from their original domain of application in order to build the whole symbolic universe of another theory.[43] This is what happens, for instance, when the mechanics of a system of interacting mass points is used in Clerk Maxwell's manner as a molecular-kinetic-theoretical foundation for the Navier-Stokes equation.[44] There is a difference between saying that T is a reducing module of T′ and saying that T′ is an approximating module of T: in the latter case, the schemes of T′ are a subclass of those of T, whereas in the former case the schemes of T have nothing to do with the schemes of T′ (they lose their empirical realizability in the reducing process). In the case of reducing modules, the theory T is necessarily known before the reduction is done and the theory T′ may even be invented through the reduction, as was the case with Maxwell's theory of electrodynamics. With approximating modules, the theory T may or may not precede the theory T′. Whereas the Navier-Stokes theory preceded its boundary-layer module, Euler's hydrodynamics postdated its narrow-vase module à la Bernoulli. Maxwell's electrodynamics postdated its quasi-stationary module and wave optics postdated its rays-optics module.

Modules, qua theories, can have submodules. For instance, the incompressible idealizing module of the Navier-Stokes theory has an inviscid specializing module. More interestingly, the boundary-layer theory, as an approximating module, relies on defining modules that are idealizing, specializing, or approximating modules of the Navier-Stokes theory. These defining modules respectively correspond to inviscid fluid motion (in the "free fluid"), discontinuity surfaces (in the case of separation), and the boundary-layer equation. This means that a module of a theory can also be a submodule of another module of the same theory (see figure 1.4). It also means that the same theory can be a module of different theories. More evident examples of multiply inserted modules are Euclidian geometry and Newtonian mechanics, which are defining modules of all the main theories of classical physics.

The modular structure varies as the theory develops. The defining modules are there, by necessity, from beginning to end. Reducing modules may occur at any stage of the life of the theory: at its birth, in its middle age, or even at its death. An instance of the last case occurred when the electromagnetic theory of light replaced elastic-solid theories of light. Specializing and approximating modules are gradually introduced, for the sake of mathematical simplification and efficient application. The status of a module may vary. For instance, a defining module

42 Approximating modules correspond to what Jeffry Ramsey calls transformation reduction (Ramsey 1993, 1995).

43 In this case, being a module of another theory does not imply a sort of inclusion; but it remains true that a module of a theory serves this theory.

44 More exactly, the low-density gas specialization of the Navier-Stokes theory is an approximating module of the kinetic theory of gases, of which the mechanics of a set of interacting molecules is a reducing module.

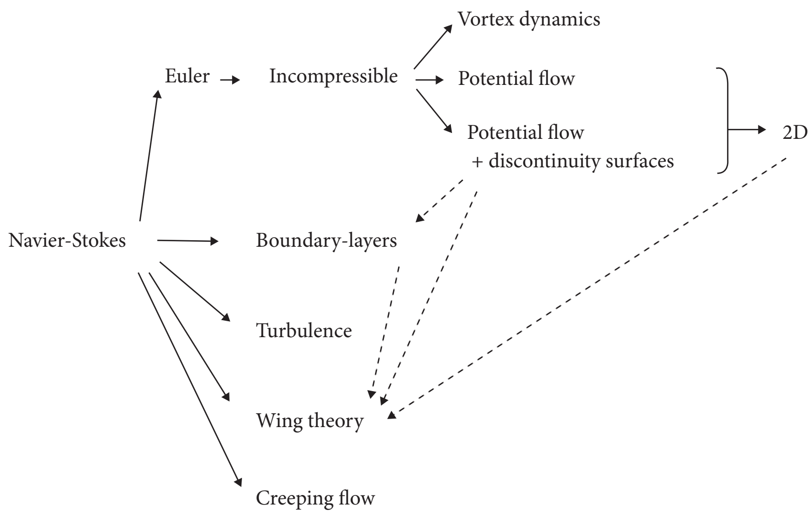

Figure 1.4 Some of the modular structure of modern hydrodynamics. The solid arrows correspond to specializing or approximating modules, the dotted arrows to defining or idealizing modules.

may become a reducing module or vice versa. In the course of the history of electrodynamics, mechanics was successively a defining module (Coulomb, Ampère, Neumann, Weber), a reducing module (Thomson, Maxwell), and again a defining module (Hertz). This variability of the status of modules is the reason why I have introduced a fairly wide spectrum of modular interrelations.

As I have argued elsewhere, modules play an essential role in the application, construction, comparison, and communication of theories.[45] In the case of hydrodynamics, the role of modules in permitting efficient applications of the theory is most evident. They yield conceptual structures that are better adapted to concrete problem situations than the bare Navier-Stokes equation. They instruct us about the choice of accessible, causally interrelated aspects of fluid motion and they tell us how to measure them. Through a nesting hierarchy of modules, we can capitalize on our concrete knowledge of the schemes of the most basic modules to imagine and control the complex experimental environment through which the predictions of higher-level theories are tested.

The constructive role of modules is evident in the case of defining and reducing modules. Idealizing, specializing, and approximating modules also help theory construction when they are known before the projective theory. They may play an instrumental role in theoretical unification or in the rejection of a tentative unification. And they may provide a "correspondence principle" for guiding the design of the symbolic universe of a new theory, as was the case when Bohr and

[45] Darrigol 2008.

Heisenberg appealed to classical electrodynamics in the construction of quantum theory.

The comparison of two theories requires shared interpretive schemes whose concrete realization is not tied to either of these theories. This is possible if all the schematic quantities can be defined by means of shared modules. For example, the predictions of various nineteenth-century theories of electrodynamics could be compared thanks to the sharing of electrostatic, electrokinetic, and magnetostatic modules. Shared modules are also essential for the communication between different subcultures of physics and other communities of scientists and engineers who use physics in their work. These shared modules enable someone to use results of a theory whose foundation he ignores or even rejects. They permit the sharing of apparatus whose functioning depends only on lower-level modules. Lastly, modular structure is essential to the teaching of theories. A typical textbook is organized by chapters that correspond to modules of the theory. Thus, the student can connect the new theory to other theories with which he is already familiar, he can get a grasp on how to apply the theory in concrete situations, and he can learn techniques that transcend the domain of this theory.

3.3 Models and Modules

In recent philosophy of science, there has been a strong emphasis on models as mental constructs that differ both from full-fledged theory and from narrow empirical induction. Mary Morgan and Margaret Morrison regard models as mediating instruments between theory and phenomena. In their view, models are partially autonomous from theory: some of their components have extratheoretical origins. The models help to shape theories as much as they rely on theory. They are more directly relevant to the empirical world than theories, at the price of a more limited scope. For all these reasons, Morgan and Morrison insist that models are not theories.

Yet (physics) models fit my definition of theories, since they necessarily have a symbolic universe, internal laws, and interpretive schemes. In my view, they differ from other theories only by having a smaller scope or less structural unity. This difference is largely a matter of degree and convention. The partial autonomy of models from more fundamental theories results from the modular character of their interconnection with these theories. Typically, fundamental theories are defining or reducing modules of models; or else models are approximating modules of a more fundamental theory.[46] The relation between models and theories is just a particular case of the modular relation between two theories. It therefore implies the same sort of mutual fitness without fusion. There is no need to sharply discriminate models

[46] In conformity with the physicists' usage, Morrison and Morgan also call "models" what I call a "reducing module." For instance, Maxwell' s mechanical model of 1862 for the electromagnetic field is a model in this sense. This kind of model widely differs from ad hoc models for limited classes of phenomena.

from theories once the modular structure of theories is taken into account. It is sufficient to recognize that some theories are more fundamental than others.

We may now revisit Prandtl's boundary-layer theory, which has received more attention from philosophers of science than any other aspect of hydrodynamics. The reason for this interest, no doubt, is the glaring cognitive superiority of Prandtl's theory compared to any earlier approach to the high Reynolds-number resistance problem. Margaret Morrison calls Prandtl's theory a model and insists on its extratheoretical origins in conformity with her general views on models. In her opinion, Prandtl's concept of boundary layer originated in an inductive inference from the flow patterns that Prandtl observed with his water mill and tank. Michael Heidelberger denies this reconstruction and favors an account in terms of theoretical heuristics. As he correctly remarks, laminar boundary layers could not be seen in Prandtl's tank, and Prandtl himself cited asymptotic reasoning as the true source of this concept. However, the scenario imagined by Morrison is frequently encountered in the history of hydrodynamics. For instance, Rankine and Froude's concept of eddying boundary layer did result from casual observation of the flow around a ship hull.[47]

Despite his disagreement with Morrison over the origins of Prandtl's theory, Heidelberger continues to call it a model. Presumably, he means to indicate that Prandtl's theoretical heuristics implied more creative guessing than would be needed in a mere deduction from the Navier-Stokes theory would engender, and that it created a new efficient, and fairly autonomous, conceptual structure. Prandtl himself did not call his theory a model. The reasons are not difficult to guess. The word was then used in Göttingen as a way to characterize semi-concrete theories that saved the phenomena without pretending to reach the true causes. In contrast, Prandtl's boundary-layer theory was meant to represent the true flow around bodies at a high Reynolds number; it did not imply any counterfactual hypothesis; and it was demonstrably compatible with the Navier-Stokes equation. In my terminology, Prandtl's theory was an approximating module of the Navier-Stokes theory. In conformity with the physicists' parlance, I would rather reserve the word "model" for theories that imply conscious simplifications of the system under consideration, for instance, the early nineteenth-century "models" of open channel flow.[48]

These terminological subtleties matter inasmuch as an overly generous use of the word "model" implies a neglect of the modular structure of theories, which I regard as pervasive and essential. Morrison's and Heidelberger's insights into the function of what they prefer to call models are nevertheless important. They both emphasize the impotence of bare fundamental theories and the need to supplement them with conceptual structures that somehow mediate between theory and experiment. And they both understand that unification, in the context of a fundamental theory, remains a desideratum. In a witty allusion to Nancy Cartwright's criticism

47 Morrison 1999, 53–60; Heidelberger 2006, 60–62.
48 For instance, Walther Ritz (1903, 3) called his vibrating-square theory of series spectra a "model" (his quotation marks).

of fundamental theories, Heidelberger claims that the Navier-Stokes theory "does not even lie about the world." At the same time, he understands that the boundary-layer theory, which so much improves the explanatory power of hydrodynamics, is an approximation of the Navier-Stokes theory. In my view, the moral is that the Navier-Stokes theory, or any other of the great theories of physics, should not be considered independently of its ever-increasing modular structure. Although the result of this evolution can never fulfill the dream of a transparent and automatic application of the fundamental equations to every conceivable situation, it has the organic unity and efficiency that we need in order to understand and control some of the physical world.[49]

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[49] Cartwright 1983; Heidelberger 2006, 64.

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CHAPTER 2

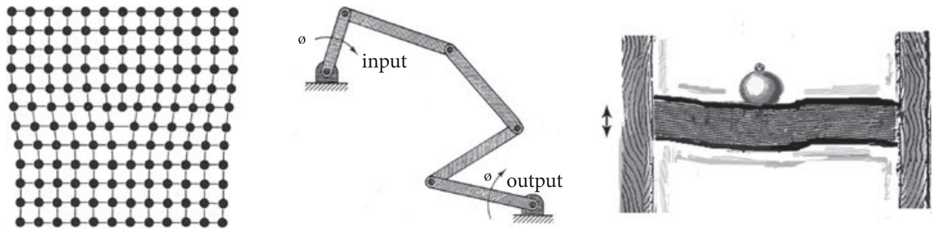
WHAT IS "CLASSICAL MECHANICS" ANYWAY?

MARK WILSON

1. Preliminary Considerations

One of the prominent sources of unhelpful folklore within philosophy is the historical controversy whose proper intricacies have been underappreciated. Misunderstood problems beget mistaken "morals" that can lead philosophical thinking astray for long epochs thereafter. This has occurred, to an extent that few philosophers recognize, with respect to the so-called "foundations of classical mechanics." As matters are commonly represented within modern college primers, "classical physics" appears to be a transparent subject matter firmly founded upon Newton's venerable laws of motion. But this placid appearance is deceptive. Any purchaser of an old home is familiar with parlor walls that seem sound except for a few imperfections that "only require a little spackle and paint." When those innocent dimples are opened up, the ancient gerry-rigged structure comes tumbling down and our hapless fix-it man finds himself confronted with months of dusty reconstruction. So it is with our subject, whose basic concepts can seem so "clear and distinct" on first acquaintance that unwary thinkers have mistaken them for a priori verities. But the true lesson of "classical mechanics" for philosophy should be exactly the opposite: the conceptual matters that initially strike us as simple and pellucid often unwind into hidden complexities when probed more adequately.[1]

[1] This is an extract (skillfully edited by Julia Bursten) of a longer survey to appear in a collection of essays entitled *Physics Avoidance*. I would like to thank Julia Bursten and Bob Batterman for their helpful advice.



mass point lattice connected rigid parts flexible beam

Figure 2.1

Matters have been rendered more confusing by the fact that a conceptually simple *surrogate* for classical doctrine is readily available, even though its formally articulated doctrines skirt most of the tricky conceptual problems encountered within classical tradition. The tenets of this simple theory comprise the themes that we shall investigate under the heading of "point-mass mechanics." Within this approach the term *point mass* designates an isolated, zero-dimensional point that carries concentrated mass, charge, and so on. In contrast, there are two other sorts of "fundamental objects" with which a "classical mechanics" can be potentially concerned: *rigid bodies*, understood as extended solids whose points never alter their relative distances to one another and *flexible bodies* such as fluids or solids that are completely malleable at every size scale (figure 2.1).

Commonly, the latter are also called *continua*, a practice we shall adopt here. Of course, any of these entities can be joined together in larger combinations, as when individual rods are assembled into a *mechanism* or one flexible body is embedded within another as a *composite* (e.g., a jelly doughnut).[2]

Mathematicians commonly label our continua as *fields* due to their distributed character. We will generally avoid this terminology and will not discuss classical electrodynamics at all. In the sequel, I shall employ the phrase *material point* to designate a zero-dimension region within a continuously distributed body (either in its interior or along some bounding surface). In contrast to our point masses, material points are connected with one another quite densely and (usually) do not carry finite values of mass or impressed force (they, instead, only display mass and charge *densities* that sum to genuine masses and densities over regions of an adequate measure). The phrase *analytic mechanics* will serve as a generic title for the sundry formalisms that deal with connected systems of rigid bodies.

As just noted, the "conceptually simple surrogate" for classical doctrine that most commonly dominates philosophical discussions of "Newtonian mechanics" comprises a set of prescriptions that make coherent sense only with respect to isolated *point masses* that never come into contact with one another.We shall discuss

[2] In textbooks, *ontologically mixed circumstances* (a point mass sliding upon a rigid plane) often appear. Usually these need to be viewed as degenerations of dimensionally consistent schemes (i.e., a ball sliding on a plane or a free mass floating above a lattice of strongly attracting masses).

the specific features of these doctrines in section 3. From a point-mass perch, any appeal to rigid bodies or continua merely represents a convenient means of discussing large swarms of point masses held together through cohesive bonding at short scale lengths.

The deceptive simplicity available to the point-mass approach traces largely to the fact that, within its frame, matter can exist only in the form of isolated singularities, thereby sidestepping the substantial mathematical concerns that arise when extended objects *come in contact* with one another (on rare occasions, point masses can collide with one another, but these contacts only occur at fleeting moments that can usually be handled through appeal to conservation principles). As a result, point masses act upon one another only through *action-at-a-distance forces*,[3] but higher dimensional objects require direct *contact forces* as well. As we will learn, getting action-at-a-distance forces and contact forces to work in tandem is a non-trivial affair, but it becomes a conceptual obligation that vanishes from view if we are allowed to restrict our fundamental ontology to point masses alone.

However, there is a wide range of subtle reasons why it can easily *look as if* a specific classical author embraces the point-mass viewpoint. As we will observe in section 3, Newton's celebrated laws of motion are difficult to parse coherently unless terms like "body" are interpreted in a punctiform manner. A host of significant *mathematical complexities* attach to the notion of "material point" as it appears within continuum physics (i.e., as a point-sized region within a continuous body), and these are sometimes bypassed by confusing embedded continuum points with the simple isolated singularities of the point-mass treatment. We shall survey several of these shifts in the pages to follow. From a formal point of view, it is important to distinguish between the *ordinary differential equations* (ODEs) pertinent to point masses and analytic mechanics and the trickier *partial differential equations* (PDEs) required in continuum modeling.[4]

The fact that the real world proves *quantum mechanical* within its small-scale behaviors occasions confusion as well. Although particles like electrons appear to be "point-like" in their scattering behaviors, they also "fill" larger effective volumes courtesy of the uncertainty relations. In many cases, one obtains the requisite Schrödinger equation for a system of particles (which is a PDE describing a field spread out within a high dimensional space) by "quantizing" a parallel set of ODEs for a classical point-mass system.[5] But this mathematical linkage does not entail

[3] If a mathematical treatment happens to make two point masses coincide, that occurrence is generally viewed as a *blowup* (= breakdown of the formalism) rather than a true contact. It is often possible to push one's treatment through such blowups through appeal to sundry conservation laws and the rationale for these popular procedures will be scrutinized in section 3.

[4] Modern investigations have shown that true ODEs and PDEs are usually the resultants of foundational principles that require more sophisticated mathematical constructions for their proper expression (integro-differential equations; variational principles, weak solutions, etc.). We shall briefly survey some of the reasons for these complications when we discuss continua in section 4 (although such concerns can even affect point-mass mechanics as well). For the most part, the simple rule "ODEs = point masses or rigid bodies; PDEs = continua" remains a valuable guide to basic mathematical character.

[5] Often internal variables such as spin are tolerated in these ODEs, even though they lack clear counterparts within true classical tradition.

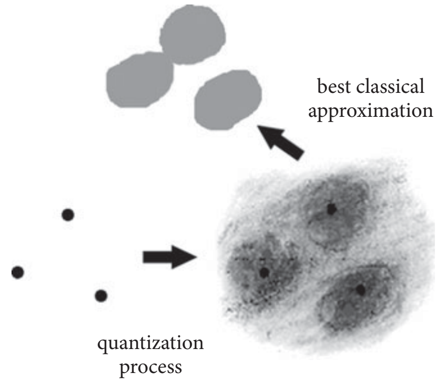


Figure 2.2

that nature behaves much like any classical point-mass system at a small size scale (figure 2.2).

Quite the contrary, constructing a classical system that can approximate the "effective volumes" of quantum clouds accurately at the size scale of so-called "molecular modeling" often requires classical blobs of extended size and flexibility. Most scientists working in the final epoch when classical mechanics could plausibly claim to govern the world in its entirety, namely the late nineteenth century, rejected the point-mass viewpoint as empirically inadequate for the bloblike characteristics of real-life atoms and molecules.

Nonetheless, there are convenient mathematical associations between the ODEs for classical point-mass models and the Schrödinger equation, so many contemporary physicists and philosophers of physics are familiar with the point-mass formalism alone. However, scholars hoping to extract methodological morals from the struggles over "matter," "atoms," and "force" that occurred toward the end of the nineteenth century will be misled if they study point-masses only, for it misses the conceptual complexities at the heart of the historical disputes. Viewed retrospectively, the degree to which the technical arcana of classical mechanics have impacted the development of scientifically attuned philosophy over the past several centuries is quite striking, even if this influence is not always recognized by modern readers. In this review, we shall sketch some of the chief ways in which the subtleties of classical mechanics have impacted philosophy.

There are two major arenas in which these effects have arisen. First, many of our greatest historical thinkers (Newton, Leibniz, Kant, Duhem, and others) directly struggled with the problems of classical matter, and their developed philosophies often prove intimately entangled with the specific foundational pathways they chose to follow.[6] Such portions of our philosophical heritage are often

[6] The abstract ruminations of *The Critique of Pure Reason*, for example, appear to have derived in part from the nitty-gritty worries about flexible matter that we shall review later. We look forward to Michael Friedman's big book on these issues.

misunderstood nowadays simply because the true contours of the physical problems our forebears faced have been forgotten. Second, as a result of these struggles, the great philosopher-scientists formulated a wide range of philosophical attitudes including anti-realism and instrumentalism as a response to the technical oddities they confronted. The twentieth-century logical empiricists who came later—after the chief focus of academic physics had shifted to quantum theory and relativity—were influenced by those older philosophical conclusions without adequate appreciation of the concrete issues that prompted them. Unfortunately, many philosophers have continued to hew to these old presumptions as if they represented firm verities, illustrating Darwin's celebrated aperccu: "False facts are highly injurious to the progress of science, for they often endure long; but false views, if supported by some evidence, do little harm, for everyone takes a salutary pleasure in proving their falseness."[7] A large folklore of "false facts" concerning classical mechanics continues to bend contemporary philosophy along unprofitable contours even today.

It is not the chief intent of this essay to pursue these satellite philosophical concerns with any vigilance, but to instead concentrate upon the key tensions that render classical doctrine hard to capture in the first place. Nonetheless, I hope that our prolegomena on larger themes suggests that significant points of general philosophical edification still lodge within the cracks of mechanics' hoary edifice.

2. Axiomatic Presentation

It will serve as a convenient benchmark for our investigations to recall that David Hilbert placed the rigorization of mechanics on his celebrated 1899 list of problems that mathematicians should address in the century to come (it is his sixth problem). He wrote, "The investigations on the foundations of geometry suggest the problem: To treat in the same manner, by means of axioms, those physical sciences in which mathematics plays an important part; in the first rank are the theory of probabilities and mechanics."[8] Indeed, Hilbert's own work in geometry and elsewhere comprised a chief inspiration for the logical empiricist program. Following this lead, we will serially examine the prospects for meeting Hilbert's challenge based upon the three foundational choices identified in section 1: point masses, rigid bodies, and continua.

Since this essay will conclude that Hilbert's objectives cannot be completely satisfied with respect to classical mechanics in the manner anticipated, let me first distance this evaluation from a popular viewpoint with which it might be otherwise

[7] Charles Darwin, *The Descent of Man and Selection in Relation to Sex*, Part II (New York: American Dome, 1902), 780.

[8] David Hilbert, "Mathematical Problems," in *Mathematical Developments Arising from Hilbert Problems*, ed. Felix Browder (Providence, RI: American Mathematical Society, 1976), 14.

confused. Many recent philosophers have responded to the axiomatic expectations of the logical empiricist school by concluding that science cannot be usefully studied in a formal manner at all. "Real life physics represents an ongoing practice," they claim, "and any attempt to capture its free-spirited antics within the rigid net of mathematical formalization represents an intrinsic distortion." But this is not what I shall claim, for I reject such a point of view entirely. Writing idly of "practices" in the loose manner of such authors offers little prospect for either appreciating or correctly identifying the concrete conceptual difficulties to be documented in this essay. Indeed, it was precisely through careful formal studies in Hilbert's manner that twentieth-century practitioners eventually reached a much sharper understanding of the fundamental requirements of continuum mechanics than was available in 1899. Indeed, Hilbert's own lectures in 1905 and the pioneering efforts of his student, Georg Hamel, comprised early landmarks along this long and tortuous development.[9] The only anti-Hilbertian moral we will extract from our examination is that a descriptive regime can often address large-scale objects more successfully if its underpinnings are structured in an overall "theory facade" manner somewhat at odds with standard axiomatic expectations. In every other way, I completely endorse the motivating intent of Hilbert's sixth problem.

We cannot appreciate the old puzzles of classical matter in their historical dimensions unless we keep the mathematical difficulties of continua firmly in mind. Scientists planning bridges or studying the musical qualities of violins in early eras did not have the luxury of waiting until the twentieth century to gather the tools they properly require. They simply had to cobble by with the mathematics they had on hand, even at the price of rather dodgy justifications. For example, due to the lack of clearly articulated PDE equations, Leibniz and his school could not deal directly with the three-dimensional complexities of a shaking beam straight on; they were forced to dissect the problem as illustrated into a connected sequence of one-dimensional tasks locally governed by ODEs (figure 2.3).

Newton followed a similar procedure in investigating how rotation affects the earth's shape: he began his treatment with a one-dimensional "canal" through the planet's interior.[10] Even today, most textbook problems adopt similar reductive stratagems: witness the standard treatment of the vibrating string.

Studying physics within these reduced, lower-dimensional settings can be very misleading from a "foundational" point of view (encouraging one to, e.g., think of stress as simply a kind of force). However, it is unlikely that classical physics could have staggered its way to an adequate treatment of continua without relying upon a broad array of results for systems that, from a foundational point of view, cannot represent their proper conceptual ingredients.

Finally, to appreciate the historical debates over classical physics in a proper context, we must disentangle the term "foundations" from certain absolutist demands

[9] Georg Hamel, *Theoretische Mechanik: Eine einheitliche Einführung in die gesamte Mechanik* (Berlin: Springer Verlag, 1949).

[10] Isaac Newton, *Principia*, vol. 1 (Berkeley: University of California Press, 1966), 349.

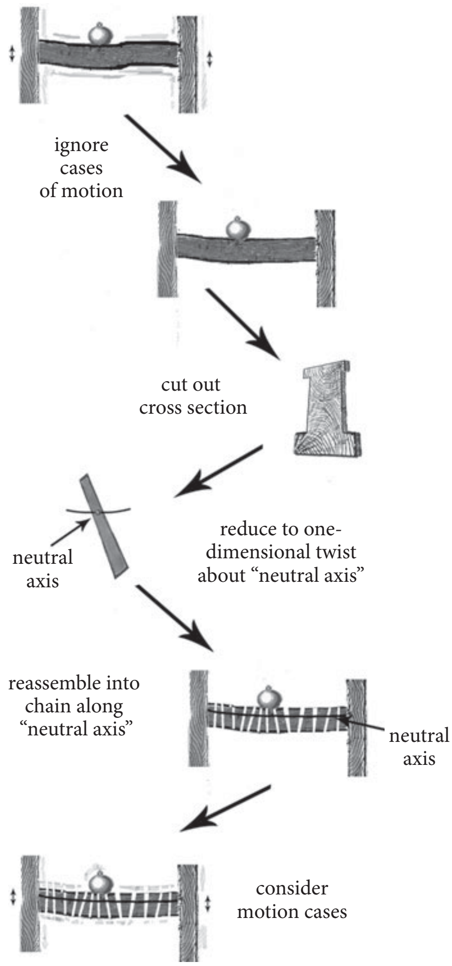


Figure 2.3

that contemporary philosophers are inclined to make. If we mark out clear axiomatic "foundations" for point masses, say, have we thereby selected an absolute *bottom layer of entities* from which any other object or system considered within a classical frame should be constructed? Many contemporary philosophers almost instinctively answer "yes," but the more prevalent historical assumption would have rejected "ultimate foundations" for classical mechanics in that vein. Indeed, calls for axiomatization per se need not inherently favor any unique choice of "ideology and ontology" in an absolutist manner, for one may instead believe that different selections of base entities and primitive terms may prove better suited for different agendas. Indeed, nineteenth-century mathematicians influenced by Julius Plücker maintained that traditional Euclidean geometry lacks any privileged basic ontology—there is no special reason to regard *points* as the subject's primitive objects rather than lines or circles. Indeed, a chief objective of traditional "foundational" work within geometry was interested in learning how the subject appears when it is dissected into alternative choices of elementary forms (points, lines, circles, etc.), under the assumption that each dissection into "primitives" offers fresh insights into the structural relationships that interlace the subject. Hilbert may have approached his sixth-problem axiomatization project with similarly tolerant expectations.

Most of the great scientists of Hilbert's time tacitly recognized that descriptive success in reliable modeling invariably relies upon some tacit *choice of scale length.* Matter generally reveals a hierarchy of qualities depending on how closely one inspects its structural details (it is traditional to designate this depth of focus by a "characteristic scale length" ΔL). For example, on an observational scale ΔL^O, well-made steel obeys simple isotropic rules for stretch and compression under normal loads (figure 2.4).

But closer inspection reveals that this macroscopic uniformity and toughness represents the resultant of a carefully engineered randomness at the level of the crystalline grain ΔL^G making up the material (such a scale length is sometimes dubbed the "mesoscopic level"). Considered at this lowered ΔL^G length, each component granule will stretch and compress in a more complicated manner than the bulk steel, but their randomized orientations supply the larger body with its simple behavior

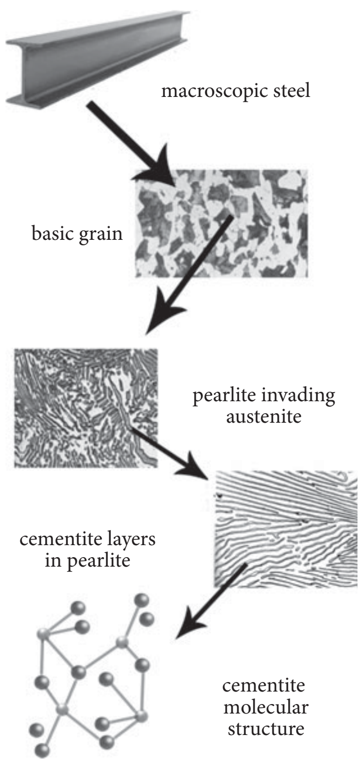


Figure 2.4

at the macroscopic level (so-called "homogenization theory" concerns itself with the details of how this ΔL^G scale to ΔL^O scale process operates). Lowering our focus to the molecular lattice ΔL^L composing the grain, we find that its capacity to *transmit dislocations* supplies the true underpinnings of the admirable toughness witnessed in the bulk steel at the much longer characteristic length ΔL^O. If we attempt to capture these various scale-dependent behaviors individually utilizing classical modeling techniques alone (as we can, to a remarkable degree of success), we will generally find ourselves selecting different ontological base units according to the implicit scale length we have selected. In such a mode, civil engineers usually model a steel beam upon a ΔL^O scale as a single *flexible body* of considerable homogeneity, whereas technicians interested in steel manufacture typically concern themselves with the thermodynamics of structural formation at the ΔL^G level. As such, the latter often adopt an ontology of *rigid crystalline forms* bound together into a complex material matrix. Initial efforts in modeling materials at the ΔL^L scale often employ *point-mass atoms* bound together in an irregular grid. But a more refined approach to these same lattice "atoms" will instead assign them flexible shapes—at the cost of considerable computational complexity. And so the modeling shifts proceed, each alteration in characteristic scale length commonly favoring a different "ontology" in its modeling material.

Here is a useful way to think about the relationships between scale sizes. In presuming that the point masses within a rigid part retain their comparative distances, we are actually pursuing a rough-hewn stratagem for profitable variable reduction, in the sense that we are attempting to evade consideration of the huge class of descriptive parameters needed to fully fix the position and velocity of every point mass within its surrounding rigid-body cloud. By treating the cloud as a united whole, we can track its *dominant behaviors* with a simple choice of six descriptive parameters (three to locate its center of mass; three to mark its angles of rotation around that center). But in tracking these values, we are only attending to the dominant behavior of the cloud because any normal collection of point masses will need to jiggle in very complex ways as they move forward. So our six rigid-body coordinates count as an effective set of *reduced variables* for our complicated point-mass swarm. Modern mathematicians like to picture such reductions as consisting of the trajectories etched upon a smallish "reduced manifold" sitting inside some much larger dynamic space. Our point-mass swarm (which is symbolized within a standard high dimensional "phase space" as the movements of a single dot) will wander throughout the larger space in an exceedingly complicated way, but it may fly fairly close (for certain portions of its journey at least) to a smaller "reduced variable" manifold, as illustrated (figure 2.5).

If so, we can gauge its complex movements with reasonable accuracy by simply tracking its shadow upon the surface of the reduced manifold. Such reduced-variable techniques have been long employed within celestial mechanics and it remains the hope of modern modelers in, for example, hydrodynamics that some allied set of reduced quantities might be found to simplify the refractive complexities within those topics.

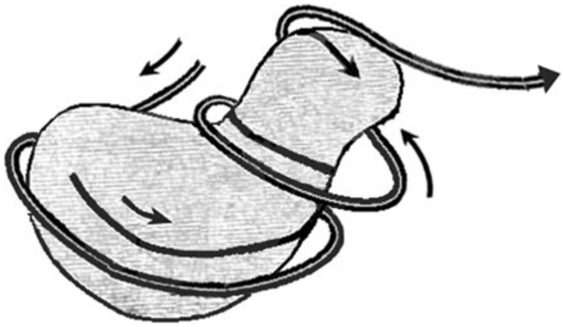


Figure 2.5

Speculative philosophers such as Leibniz opined that this alteration of ontological units would continue forever as one descends to smaller scales. More cautious observers have merely observed that experiment had not established any clear choice of lowest scale unit for classical mechanics. In this regard, it should be recalled that the evidence for fundamental particles only became overwhelming at the very end of the classical period, in the guise of Rutherford's experiments on radioactive scattering and the like. Once quantum mechanics enters our descriptive arena, its percepts increasingly dominate at smaller scale lengths and we eventually fall beyond the resources of classical modeling tools altogether.

Unfortunately, the various crossover points at which classical treatments lose their accuracy do not favor any uniform choice of fundamental classical entity. Sometimes point-mass treatments supply the most convenient form of lowest-scale classical modeling, but more often continua or rigid bodies provide better modeling accuracy. So while quantum mechanics may select certain entities as physically "bottom level," it does not follow that classical mechanics will do the same when considered upon its own merits. Accordingly, Hilbert's sixth-problem formalization project should not be saddled with the burden of satisfying a contemporary philosopher's expectations with respect to bottom-level ontology. What we will want to investigate carefully, as part of our "foundationalist" enterprise, is the degree to which principles applicable on a higher scale level ΔL^* relate to those applicable at the lower length ΔL. I call such transfers of doctrine across size scales *lifts*, and I employ "lift" in the elevator sense: one can go both up and down in a hoist.

Hilbert's own articulation stresses the importance of understanding these lifts more centrally than the simpler task of formalizing our three starting perspectives. He wrote:

> Boltzmann's work on the principles of mechanics suggests the problem of developing mathematically the limiting processes, there merely indicated, that lead from the atomistic view to the laws of motion of continua. Conversely, one might try to derive the laws of motion of rigid bodies by a limiting from a system of axioms depending upon the idea of continuously

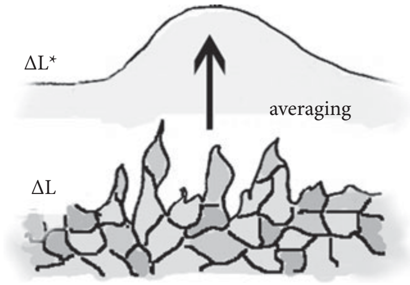


Figure 2.6

> varying conditions of matter filling all space continuously, these conditions being defined by parameters. For the question of equivalence of different systems of axioms is always of great theoretical interest.[11]

Here Hilbert calls our attention to the various relationships between scale length that have been intensely studied in recent times under the general headings of "homogenization" and "degeneration."[12] He observes that the vague invocation of "limits" rarely provides an adequately precise diagnosis of the relationships involved, an observation that modern investigations heartily underscore. Observe that Hilbert's final sentence suggests that he did not anticipate that any of his suggested starting points would prove *fundamental* in the bottom-layer sense just canvassed. According to the applicational task at hand, different modes of ontological dissection (e.g., flexible continua or Boltzmannian swarms of rigid bodies) may possess their descriptive utilities in the same manner in which alternative decompositions of geometry into "primitive elements" prove fruitful. Even so, Hilbert insists that we must guard against erroneously lifting physical doctrines from one decompositional program to another without adequate precaution (figure 2.6).

In standard textbook practice, these lifts usually appear as dubious "derivations" of, for example, rules of continua considered at a ΔL^* scale level on the basis of rigid body swarms at a ΔL scale. As we will later see in detail, such improper doctrinal transfers are common in practice and sometimes serve as the source of substantial conceptual confusion.[13]

[11] Hilbert, "Mathematical Problems," 15.

[12] I do not have the space to survey such modern studies here, which attempt to, for example, recover the tenets of rigid body mechanics from continuum principles by allowing certain material parameters to become infinitely stiff (thus "degeneration"). Generally the results are quite complex, with corrective modeling factors emerging in the manner of Prantdl's boundary layer equations. Sometimes efforts are made to weld our different foundational approaches into unity through employing tools like Stieltjes-Lesbeque integration. More generally, a "homogenization" recipe *smears out* the detailed processes occurring across a wide region ΔW in an "averaging" kind of way, whereas "degeneration" instead concentrates the processes within ΔW onto a spatially singular support like a surface (the Riemann-Hugoniot approach to shock waves provides a classic exemplar).

[13] After a sufficient range of mechanical considerations has been surveyed in later sections, we shall be able to sketch a more favorable view of the useful offices that standard textbook lifts provide. I should also add that we shall generally consider our "ΔL to ΔL^* lifts" in two simultaneous modes: (1) as a modeling shift

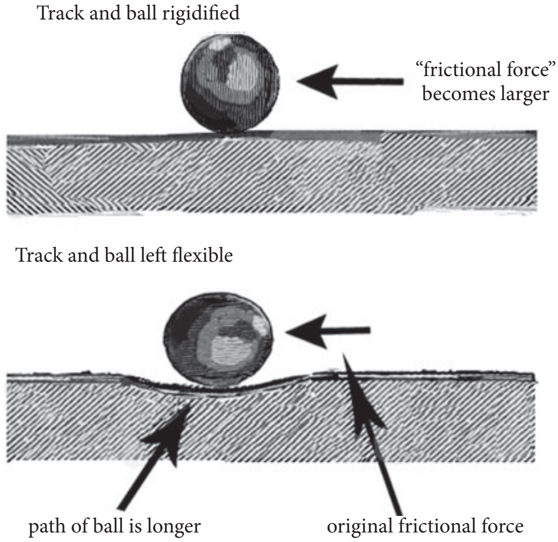


Figure 2.7

Consider a simple example of the problems that can arise in such shifts from ΔL to ΔL^*. The term *force* has a notorious tendency to alter its exact significance as characteristic scale lengths are adjusted. At a macroscopic level, the "rolling friction" that slows a ball upon a rigid track is a simple Newton-style force opposing the onward motion. But at a lower scale length, the seemingly "rigid" tracks are not so firm after all: they elongate under the weight of the sphere to a nontrivial degree. So part of the work required to move our ball against friction consists in the fact that it must *travel further* than is apparent. But when we consider the "forces" on our ball at a macrolevel, we instinctively treat the track length as fixed and allocate the effects of its actual elongation to a portion of the "force of rolling friction" budget (figure 2.7). A similar phenomenon occurs with the "viscosity" of a fluid.

When such adjustments in reference occur, one cannot legitimately lift a doctrine about "forces" applicable on scale level ΔL to scale level ΔL^*, for "force" does not mean quite the same thing in the two applications. Of course, if these innocent drifts were the only kinds of problematic lift to which mechanical practice was liable, serious conceptual debates would not have arisen in the subject. But these humble illustrations supply a preliminary sense of the problems we must watch for.

The properties we ascribe to a system with respect to an upper-scale length ΔL^* ("rolling on a rigid track") usually represent *averages* (or some allied form of homogenization or degeneration) over the more elaborate behaviors we will witness

from one *finite scale length* to another (e.g., from ΔL^G to ΔL^O in our steel bar example) and (2) as a mathematical shift from a *lower dimensional object* (a point mass or line) to a higher dimensional gizmo such as a three-dimensional blob. Properly speaking, these represent distinct projects, although, in historical and applicational practice, they blur together.

at a finer scale of resolution ΔL ("stretching the molecular lattice"). Obtaining a workable scheme of physical description tailored to ΔL^* usually requires that a fair amount of fine detail gets *frozen over* in our modelings. In other words, we generally hope to capture only the *dominant behaviors* of our real-life system within in our ΔL^* treatment and anticipate that we will sometimes need to open up the suppressed degrees of freedom whenever the complexities of the lower scale begin to intrude upon the patterns normally predominant at the coarser scale ΔL^*.

Generically, the use of a smaller set of quantities to capture system behaviors dominant upon a higher scale length ΔL^* is called a *reduced variable treatment*. There are a large number of ways in which these reduced-variable models can arise. For example, a reasonable policy of *homogenization* might adjust its descriptive terms from those suited to a ΔL^G assembly of iron grains to a smoothed-over steel bar described as continuous at the ΔL^O level.[14] But a quite different exemplar of reduced-variable "freezing" can be witnessed in Newton's celebrated treatment of the planets. At the scale lengths appropriate to celestial mechanics, one can ignore the complexities attendant upon the earth's shape and size by modeling it as a simple point mass. Rather than smearing out the properties of the planets over wider regions (as occurs in homogenization), we instead concentrate their extended traits upon much smaller supports.

Such policies of compressing complex expanses into singularities (or other lower-dimensional structures such as one-dimensional strings) are sometimes called *degenerations* (a term I regard as preferable to the misleading phrase *idealization*). Plainly, when very detailed astrophysical calculations are wanted, one must open up those internal complexities and treat the earth as a continuum subject apt to distort under rotational effects. However, there are many forms of reduced-variable lift that involve a mixture of the two policies or other sorts of tactic altogether.

Some of the anti-atomism advocated by late nineteenth-century scientists such as Duhem and Mach traces not to some obtuse dismissal of lower scale structure per se, but to the widely shared assumption that, in any application, modelers must invariably engage in such "freezing to a scale level" procedures. Their primary disagreement with other mechanists of their era concerns the format that should be regarded as the optimal embodiment of "classical principle" within such a scale-sensitive setting. Specifically, Duhem and Mach maintained that "basic physics," as an organizational enterprise, should develop tools that will prove maximally useful *at any chosen scale length*. This requirement almost automatically favors a "thermomechanical" approach of the sort described in the discussion of flexible bodies in section 5. Their opponents, such as Ludwig Boltzmann, generally favored the simplest base ontology that could plausibly support the more complex forms of

[14] Strictly speaking, a lift to continuous variables from an ODE-style treatment involving a large number of discrete variables at the ΔL level should not be called a "reduced variable" treatment, as we actually *increased* the number of degrees of freedom under the lift (normally, a true "reduced variable" treatment will supply a ΔL^* level manifold lying near to some submanifold contained within the ΔL phase space). However, the descriptive advantages of a lift to continuous variables often resembles those supplied within a true "reduced variable" treatment, so in the sequel I will often consider both forms of lift under a common heading.

mechanics in a ΔL to ΔL^* manner (they often employed point masses or connected rigid bodies as their base level ingredients). In these respects, we might observe that Duhem and Mach's strictures better suit the methodological percepts of empiricists such as David Hume, who opined that any postulation of lower-scale structure must be based upon "laws" directly verifiable at the laboratory level.

Prima facie, we might reasonably expect that it should prove possible to formalize any of our three basic ontologies independently of one another, placing them on their own bottoms, as it were. Thus Hilbert probably anticipated that we should be able to frame distinct axiomatic encapsulations for point masses, rigid bodies and flexible bodies and then proceed to investigate how ably such formalisms relate to one another under ΔL to ΔL^* lifts. However, a somewhat surprising obstacle impedes such projects, whose various ramifications will comprise the bulk of this essay. They collectively trace to the simple consideration that if we attempt to frame general principles applicable to a higher ΔL^* scale length based upon behaviors operative on a lower scale length ΔL, we will find that our ΔL^* level principles generally *display gaps, holes, or gross inaccuracies* in special circumstances.

The general explanation for such upper-scale gaps is quite straightforward: a useful selection of "reduced variables" at the ΔL^* level will focus upon behaviors that *dominate* at that size scale. But, invariably, there will be special ΔL-level arrangements where the effects suppressed in our ΔL^* treatment obtain equal or greater importance than the usual dominant behaviors. I shall sometimes call such shifts "escape hatches," for they provide ladders that allow us to evade the inferential instructions of a formalism that no longer serves its empirical purposes. But such practices create a formal difficulty for axiomatization projects in Hilbert's vein because the domain of interest frequently becomes re-ontologized under the scale shift. But axiomatic presentations rarely include provisos for ontology shifts. Instead, we anticipate that their formal tenets will supply behavioral principles applicable to its ontology in all circumstances, even if, in real-life practice, we would normally escape such descriptive straitjackets in favor of some revised treatment operating at a lower length scale ΔL.

In short, conventional axiomatized theories are expected to supply principles that can govern even the bad spots within their ranges of empirical coverage. Such formal expectations lead many philosophers to further suppose that "classical mechanics" must completely specify the behaviors tolerated *within its own parochial range of possible worlds*, in spite of the fact that we would never apply such modelings to real-world dominions of a strongly quantum mechanical or relativistic character. But such dogmas presume that some fairly complete axiomatization of overall "classical mechanics" is available, a thesis we shall critically examine in this essay.

Let us now ask ourselves a commonsensical question. Considered from a practical point of view, is it really wise or meritorious to fill out a formalism in a manner that carries with it no discernible empirical merit? Mightn't it be better to deliberately leave our stocks of physical principle somewhat incomplete, allowing its very holes to signal when we should look for suitable ΔL^* to ΔL escape hatches? Indeed, explicit indications in the mathematics of when modeling problems begin should

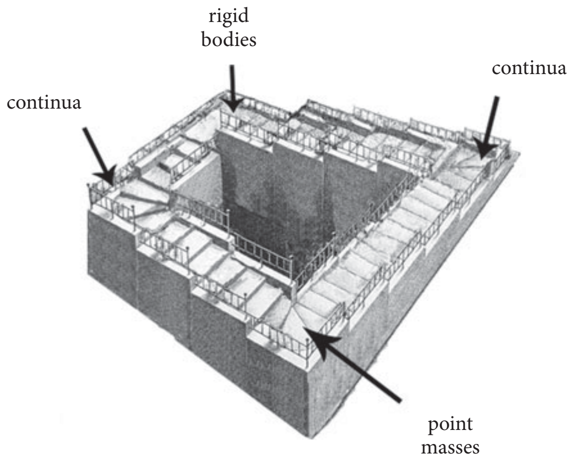


Figure 2.8

be greatly cultivated, for we surely want to avoid the fate of the computers who cheerfully compute worthless data simply because no one has told them to stop.[15] Training in mechanics generally inculcates considerable skill in knowing when one should adventitiously shift from one modeling framework to another. So it is sometimes unwise to push a formalism's axiomatized coverage beyond the limits of its real-life modeling effectiveness.

This point of view suggests that we might look upon the inherited compendium of descriptive lore we call "classical mechanics" as a series of descriptive patches (corresponding to our three basic choices of fundamental objects) linked together at their descriptive bad spots by various ΔL^* to ΔL escape hatches. However, whenever manifolds are constructed through sewing together local patches in this way, twisted topologies can potentially emerge in the final result (Klein bottles and Möbius strips provide classic illustrations of the phenomenon). In these respects, nature shows little favoritism as to which of our three basic ontologies of classical objects should be viewed as "fundamental" from an applicational point of view.

If we attempt to understand "classical physics" as a *conceptual system closed unto itself*, we thereby obtain a structure like one of those impossible Escher etchings: local plates connected by staircases that never stabilize upon a lowest landing (figure 2.8).

But such topographical oddities do not indicate that "classical physics" has not served its descriptive purposes perfectly well. As long as the salient escape routes are clearly marked, our Escherish edifice serves a base frame upon which a wide range of interconnected forms of reduced-variable modeling techniques can be conveniently located (I sometimes call structures of this sort *theory facades*). By operating with

15 In many statistical problems, the population under review is artificially increased to an infinite size, simply so that the applicable mathematics will supply crisp answers to the questions we commonly ask. Left to its own devices, *mathematics is rather stupid* in a literal-minded kind of way and finds it very difficult to answer questions in a "well, almost all of the time" vein, which is often the best that can be achieved with respect to a finite population. But if the same community is modeled as infinite, we can often fool the mathematics into supplying us with the brisk replies we desire.

a proper regard for the requisite level shifts, we can thereby assemble the most fruitful terminology yet devised for dealing with the complex physical world about us at nonmicroscopic scale lengths: the shared language of "classical physics." The twisted topology within its connection manifold merely reflects the "exit from bad patches" considerations that allow the scheme to cover extremely wide swatches of application with great efficiency.

The historical triumph of "classical mechanics" as a descriptive enterprise would have never occurred had the subject not lightly skipped over the many problematic transitions of the sort we shall survey. Historically, the price of a vigorous conceptual enlargement is often a lingering residue of confusion that can occasionally blossom into full paradox when suitably nurtured. And such has been the career of classical mechanics: full of predictive glories but comingled with mystifying transitions that have led some of our greatest philosophical minds down the garden path to strange assessments of our descriptive position within nature. In the sequel, we consider three basic descriptive patches handed down to us in our classical legacy and examine the typical confusions that arise when one shifts from one framework to another without noticing.

3. Point-Mass Mechanics

Let us first consider the point-mass formalism of classical mechanics, suggested by Newton's familiar formulation of the fundamental laws of motion. To begin, it is worth noting that substantive foundational issues immediately arise if we scrutinize these laws with a critical eye. In their original form, these principles are hard to interpret with any exactitude due to the ambiguous manner in which Newton employs his terms. Here they are in Motte's translation:

> Law I: Every body persists in its state of being at rest or of moving uniformly straight forward, except insofar as it is compelled to change its state by force impressed.
> Law II: The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.
> Law III: To every action there is always opposed an equal reaction: or the mutual actions of two bodies upon each other are always equal, and directed to contrary parts.[16]

Look carefully at Law I. If a "body" represents an isolated point mass, then the phrase "moves uniformly straight forward" is not ambiguous. But what is the parallel intent if a *rotating rigid object* can be selected as a body? Or a packet within a compressible

[16] Isaac Newton, *op. cit.* 13–14.