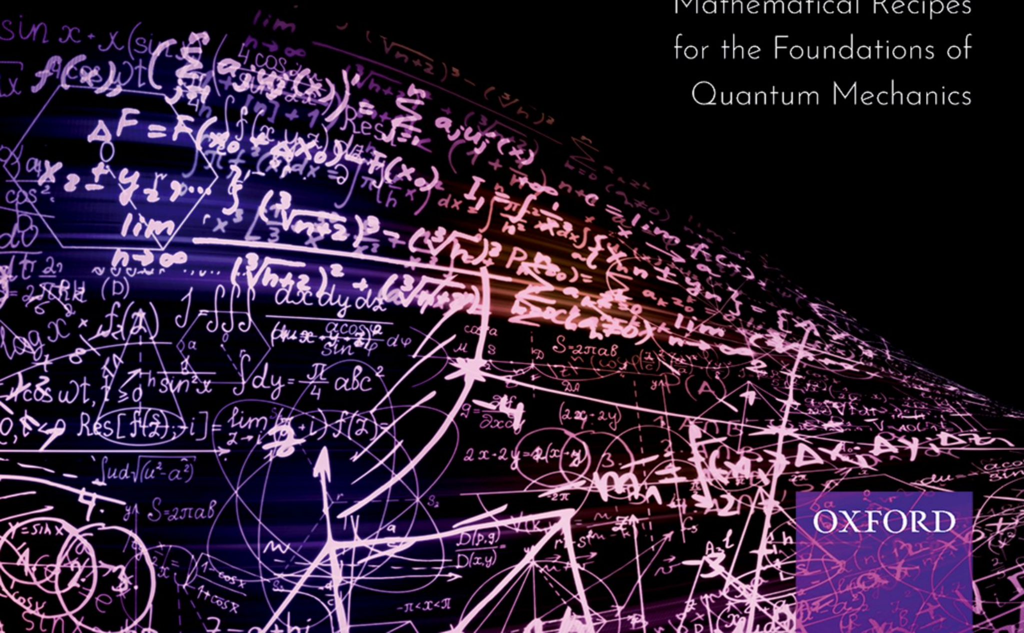
JIM BAGGOTT

THE QUANTUM COOKBOOK

Mathematical Recipes for the Foundations of Quantum Mechanics



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To myself, aged 18,

when I took my first course on quantum mechanics

Preface

So this was the situation which I found at Cornell. Hans [Bethe] was using the old cookbook quantum mechanics that Dick [Feynman] couldn't understand. Dick was using his own private quantum mechanics that nobody else could understand. They were getting the same answers whenever they calculated the same problems.

Freeman Dyson*

There are a number of reasons why quantum mechanics is a difficult subject, both to teach and to learn. For sure, the subject is mathematically very challenging. But it is also *philosophically* challenging, forcing as it does a complete rethink of our naïve classical preconceptions concerning the ways in which we seek to represent physical reality in a scientific theory, and what we might expect such a representation to be telling us about it. The first challenge is recognized, and respected. The second perhaps less so.

I firmly believe that presentations of quantum mechanics that focus on formalism at the expense of all experimental, historical, and philosophical context run great risks of losing all but the most able students. Of course, science does not—it cannot—respect history. We make progress in science by moving on, by building on what we've learned without worrying overmuch precisely how we learned it. But the simple truth is that quantum mechanics did not suddenly materialize overnight in the minds of its creators, fully formed, complete with all its axioms and principles. It was instead tortured from much more familiar classical physical descriptions, such as thermodynamics, statistical mechanics, electromagnetic theory, special relativity, and atomic theory, over a period of decades, as physicists struggled to interpret a series of ever more baffling experimental results.

Only later was a much higher level of abstraction introduced into quantum mechanics, in an attempt to establish a secure mathematical foundation that would eradicate all the confusing classical misconceptions inherited from its birth and early childhood. This was a process begun by Paul Dirac in *The Principles of Quantum Mechanics* (1930) and John von Neumann in *Mathematical Foundations of Quantum Mechanics* (first published in German in 1932). Such was their success that we tend to overlook just how alien their approach was at the time. For example, in his review of *Principles*, Wolfgang Pauli warned that Dirac's rather abstract formalism and focus on mathematics at the expense of physics held 'a certain danger that the theory will escape from reality'.* I fear he was right to be concerned.

Many students find the formalism completely baffling when they encounter it for the first time. Lectures and textbooks that dive straight into discussions of wavefunctions or vector spaces without any historical or philosophical context can leave students stranded, left to ponder: 'Just how did they get that?', and 'Where did that come from?'† If the formalism is delivered to students as though the philosophical problems of its interpretation do not exist or are irrelevant, this can give the misleading impression that we really *do* understand what quantum mechanics is all about. Those students who then fail to penetrate the fog of confusion are left to brood on their own inadequacy. This is unfortunate, as the charismatic American physicist Richard Feynman was closer to the truth with his famous quote: 'I think I can safely say that *nobody* understands quantum mechanics.'‡

To expose the real nature of the challenge, I believe it is helpful first to demonstrate that, despite appearances, mathematical complexity is not the principal problem. The second step is to provide some historical context, if only to explain that quantum mechanics was derived from real physics, not abstract mathematics. It also helps to explain how, *from the very beginning*, the physicists who helped to establish the theory were obliged to wrestle with its interpretation, arguing very energetically among themselves as they did so. Then we get the real insight. Nobody understands quantum mechanics because of its deep *philosophical* problems: we really don't understand what it *means*, possibly because we're not meant to.

The Quantum Cookbook is an attempt to provide a unique bridge between a popular exposition and a formal textbook presentation. The former tend to be necessarily extremely light on mathematical details, whereas the latter tend to be formalism-heavy, often paying little or no heed to problems of interpretation (though there are some notable exceptions). For curious readers with some background in physics and sufficient mathematical capability, neither popular exposition nor textbook provides them with what they need.

The book's mission is to expose the real nature of the problems with quantum mechanics by walking readers step-by-step through the derivation of its most important foundational equations, including one result from special relativity ($E = mc^2$) because of its importance at key points in the story. It aims to provide sufficient context to enable readers to come to their own conclusions about its interpretation and meaning. In the process of demystifying the mathematics as much as possible, I hope also to demonstrate how *flexibly* mathematics is often applied in science, through simplified models,

* Wolfgang Pauli, *Die Naturwissenschaften*, **19** (1931), 188–9, quoted in Helge Kragh, *Dirac: A Scientific Biography*, Cambridge University Press, Cambridge, UK, 1990, p. 79.

† Especially those who, like me, were plunged into quantum mechanics without first being introduced (even superficially) to classical Hamiltonian mechanics and special relativity. My first encounter with quantum mechanics was as a student studying for a degree in chemistry, and these topics did not belong in a chemistry curriculum.

‡ Richard Feynman, *The Character of Physical Law*, MIT Press, Cambridge, MA, 1967, p. 129. The italics are mine.

and limiting assumptions and approximations. Despite its 'unreasonable effectiveness', mathematics is still a *language*, one that leaves plenty of room for interpretation (and doubt).

The first nine chapters build these results more or less chronologically, unfolding pretty much as they were presented by those physicists who left their fingerprints all over quantum mechanics. These are the quantization of energy (Planck); the equivalence of mass and energy (Einstein); quantum numbers and quantum jumps (Bohr); wave–particle duality (de Broglie); operators, eigenfunctions, and eigenvalues (wave mechanics—Schrödinger); quantum probability (Born); the uncertainty principle (Heisenberg and Robertson); the exclusion principle and electron spin (Pauli and Heisenberg); and relativistic quantum mechanics (electron spin and antimatter—Dirac). Chapter 10 will be a little different in structure as it deals with the establishment of the standard quantum formalism based on the concepts of state vectors in Hilbert space (Dirac and von Neumann).

The noted contemporary theorist Lee Smolin told me recently that as an undergraduate student he had been extraordinarily fortunate. In the spring semester of his first year at Hampshire College in Amherst, Massachusetts, he learned about quantum mechanics from Herbert Bernstein, by Smolin's account a great physics teacher. The course concluded with detailed discussions of something called the EPR argument, named for Einstein, Boris Podolsky, and Nathan Rosen, and a famous theorem devised by John Bell. 'Bell's paper was not yet widely known and had by that time very few citations,' Smolin explained to me. 'That was probably the first and only quantum mechanics course for undergraduates that included EPR and Bell.'*

So, the final two chapters of *The Quantum Cookbook* cover topics that would not normally form part of an introductory course on quantum mechanics, though I would argue that they should (and Smolin would agree). These deal with the treatment of measurement in the quantum formalism (von Neumann) and the challenge posed by the interpretation of quantum entanglement and non-locality (Einstein, Bohm, and Bell).

Now, in setting out the book's ambitions I need to be absolutely clear. It is *not* my intention to provide a detailed historical analysis of these physicists' original publications, many of which are in any case intentionally obscure, as they sought to cover up underlying violence to the mathematics, unjustified assumptions, and occasional conceptual leaps of faith. After all, science doesn't much care *how* a theory is arrived at: what's important is how well the theory accommodates existing empirical facts and how well its predictions fare in the light of new examination.

The intention is rather to present the simplest possible derivations that are broadly consistent with the originals, which make use of current nomenclature, and which can be followed relatively easily. It's important that readers can appreciate the logic, the nature of the challenges, and the occasional bit of mathematical sleight-of-hand.

Demystifying the mathematics means taking nothing for granted. Each derivation is presented as a 'recipe' with listed ingredients, including standard results from the

* Lee Smolin, personal communication, 7 September 2017.

mathematician's toolkit, such as the odd trigonometric identity, Stirling's formula, a standard integral or two, or a Taylor series expansion. Each recipe is then set out in a series of hopefully easy-to-follow steps, such that readers with limited ability in algebra and differential calculus and a background in physics should be able to cope. I've tried to write these recipes sympathetically, for readers who—like me—will often struggle to follow the logic of a derivation which misses out steps that are 'obvious' to the author, or which use techniques that readers are assumed to know. More mathematically competent readers who do not need everything spelled out in this way may therefore prefer to skip the intermediate steps.

Either way, I'm hopeful that readers will agree with my conclusion. There are obvious exceptions, but for the most part these derivations are triumphs of physical intuition over mathematical rigour and consistency.

The purpose of *The Quantum Cookbook* is not to teach readers how to *do* quantum mechanics, and it is not intended as a course textbook (it doesn't include any worked examples or problems). My hope is that this might prove to be a useful supplementary text for an introductory course, one that helps readers understand how to *think* about quantum mechanics.

My personal relationship with quantum mechanics now spans more than 40 years. Aside from making me feel quite old, this means that my debt of thanks by now extends to innumerable teachers, researchers, and authors whose efforts have helped bring light and inspiration in equal measure. I'd like to acknowledge personal debts to Peter Atkins, whose lectures and textbook *Molecular Quantum Mechanics* provided much technical clarity and insight, and Ian Mills, my erstwhile colleague at the University of Reading, who provided essential guidance as my understanding of the philosophical dimensions of the theory began its long, slow awakening. I must also give thanks to Lee Smolin and Carlo Rovelli, with whom more recent discussions helped to remind me why this is such an endlessly fascinating subject, and which encouraged me to return to it.

And, of course, I owe eternal gratitude to Sonke Adlung, Ania Wronski, Lucia Perez, and the production team at Oxford University Press for (once more) giving me the opportunity to get this lot off my chest.

Jim Baggott
September 2019

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About the Author

Jim Baggott is an award-winning science writer. A former academic scientist, he now works as an independent business consultant but maintains a broad interest in science, philosophy, and history, and continues to write on these subjects in his spare time. His books have been widely acclaimed and include the following:

Quantum Reality*: The Quest for the Real Meaning of Quantum Mechanics - A Game of Theories* (2020)

Quantum Space*: Loop Quantum Gravity and the Search for the Structure of Space, Time, and the Universe* (2018)

Mass*: The Quest to Understand Matter from Greek Atoms to Quantum Fields* (2017)

Origins*: The Scientific Story of Creation* (2015)

Farewell to Reality*: How Fairy-tale Physics Betrays the Search for Scientific Truth* (2013)

Higgs*: The Invention and Discovery of the 'God Particle'* (2012)

The Quantum Story*: A History in 40 Moments* (2011, re-issued in 2015)

Atomic*: The First War of Physics and the Secret History of the Atom Bomb 1939–49* (2009, re-issued in 2015), shortlisted for the Duke of Westminster Medal for Military Literature, 2010

A Beginner's Guide to Reality (2005)

Beyond Measure*: Modern Physics, Philosophy and the Meaning of Quantum Theory* (2004)

Perfect Symmetry*: The Accidental Discovery of Buckminsterfullerene* (1994)

The Meaning of Quantum Theory*: A Guide for Students of Chemistry and Physics* (1992)

Prologue

What's Wrong with This Picture?

The Description of Nature at the End of the Nineteenth Century

Anyone already familiar with some of the more bizarre implications of quantum mechanics—its phantoms of probability; particles that are waves and waves that are particles; cats that are at once both alive and dead; its uncertainty, non-locality, and seemingly 'spooky' goings-on—might look back rather wistfully on the structure of classical mechanics. We might be tempted to think that classical mechanics offers a much more appealing or comforting description of nature, one that is unambiguous, definite, and certain.

There is a persistent myth that, towards the end of the nineteenth century, such was the appeal of the classical structure that it seemed to physicists that all the most pressing problems had now been solved. In a lecture delivered to the British Association for the Advancement of Science in 1900, the great physicist Lord Kelvin (William Thomson) is supposed to have declared: 'There is nothing new to be discovered in physics now. All that remains is more and more precise measurement.'[1]

Except there is no evidence that Kelvin ever said this.[2] It's true that in *Light Waves and Their Uses*, a book based on a series of lectures delivered in 1899 to the Lowell Institute in Boston, Massachusetts, American physicist Albert Michelson wrote:[3]

> Many other instances might be cited, but these will suffice to justify the statement that 'our future discoveries must be looked for in the sixth place of decimals.' It follows that every means which facilitates accuracy in measurement is a possible factor in a future discovery.

It is perhaps not surprising that Michelson would want to extol the virtues of just the kind of precise measurement on which he'd built an international reputation. But in April 1900, Kelvin was warning that all was not well. A storm was gathering in the dynamical theory of heat and light.[4] We now know that the classical structure breaks down in the microscopic realm of atoms and subatomic particles, and Isaac Newton's laws of motion can't handle objects moving at or near light speed. However, within its domain of applicability, classical mechanics is surely free of mystery and much less prone to endless bickering about what it's all supposed to mean?

Except that it isn't, really.

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Make no mistake, despite its intuitive appeal, classical mechanics is just as fraught with conceptual difficulties and problems of interpretation as its quantum replacement. The problems just happen to be rather less obvious, and so more easily overlooked (or, quite frankly, ignored). Quantum mechanics was born not only from the failure wrought by trying to extend classical physical principles into the microscopic world of atoms and molecules, but also from the failure of some of its most familiar and cherished concepts. To set the scene and prepare us for what follows, I thought it might be worth highlighting some of the worst offenders.

The Interpretation of Space and Time

The classical system of physics that Newton had helped to construct, by 'standing on the shoulders of giants',[5] consists of three laws of motion and a law of universal gravitation. The *Mathematical Principles of Natural Philosophy*, first published in 1687, uses these laws to bring together aspects of the terrestrial physics of everyday objects and the 'celestial' mechanics of planetary motion, in what was nothing less than a monumental synthesis, fully deserving of its exalted status in science history. So closely did the resulting description agree with and explain observation and experiment that there could be little doubting its essential 'truth'. By the end of the nineteenth century it had stood, unrivalled, for more than two hundred years.

Unrivalled, but by no means unquestioned. Newton's mechanics might be intuitive but it demands a number of fairly substantial conceptual or philosophical trade-offs. Perhaps the most fundamental is that Newton's physics is assumed to take place in an *absolute* space and time. This is a problem because, if it existed, an absolute space would form a curious kind of container, presumably of infinite dimensions, within which some sort of mysterious cosmic metronome marks absolute time. Actions impress forces on matter and things happen *within* the container and all motion is then referred to a fixed frame, thereby making all motion absolute.

If we could take all the matter out of Newton's universe, then we would be obliged to presume that the empty container would remain, and the metronome would continue to tick. The existence of such a container implies a vantage point from which it would be possible to look down on the entire material universe, a 'God's-eye view' of all creation.

But a moment's reflection tells us that, despite superficial appearances, we only ever perceive objects to be moving towards or away from each other, changing their *relative* positions. This is relative motion, occurring in a space and time that are in principle defined only by the relationships between the objects themselves. If the motion is uniform, then there is in principle *no* observation we can make that will tell us if this object is moving relative to that object, or the other way around. In the *Mathematical Principles*, Newton acknowledged this in what he called our 'vulgar' experience.

If we can never perceive motion in an absolute space and time then we arguably have no good reason to accept that these exist. And if there is no absolute coordinate system of the universe; no absolute or ultimate inertial frame of reference against which all motion can be measured, then there can be no such thing as absolute motion. Newton's arch-rival, German philosopher Gottfried Wilhelm Leibniz, argued: 'the fiction of a finite material universe, the whole of which moves about in an infinite empty space, cannot be admitted. It is altogether unreasonable and impracticable.'[6] Now, any concept that is not accessible to observation or experiment in principle, a concept for which we can gather no empirical evidence, is typically considered to be *metaphysical* (meaning literally 'beyond physics').

Why, then, did Newton insist on a system of absolute space and time, one that we can never directly experience and which is therefore entirely metaphysical? Because by making this metaphysical *pre-commitment* he found that he could formulate some very highly successful laws of motion. Success breeds a certain degree of comfort, and a willingness to suspend disbelief in the grand but sometimes rather questionable foundations on which theoretical descriptions are constructed.

Classical Mechanics and the Concept of Force

Classical mechanics is the physics of the ordinary. Suppose we apply a force F for a short time interval, dt, to an object that is stationary or moving with constant velocity, v, in a straight line. In the *Mathematical Principles*, Newton explains that the force is simply an 'action', exerted or impressed upon the object, which effects a change in its linear momentum (p, given by the object's mass m multiplied by v), by an amount dp. If we assume that mass is an intrinsic property of the object and does not change with time or with the application of the force, then dp is then simply the mass multiplied by the change in velocity: $dp = mdv$.

Applying the force may change the magnitude of the velocity (up or down) and/or it may change the direction in which the object is moving. Newton's second law of motion is then expressed as $Fdt = dp\ (= mdv)$. This equation may not look very familiar, but we can take a further step. Dividing both sides by dt gives

$$F = \frac{dp}{dt}. \tag{P.1}$$

Logically, the greater the applied force, the greater the rate of change of linear momentum with time. But, as we've seen, $dp/dt = mdv/dt$. Obviously, dv/dt is the rate of change of velocity with time, or the object's *acceleration*, usually given the symbol a. Hence Newton's second law can be restated as the much more familiar

$$F = ma. \tag{P.2}$$

Force equals inertial mass times acceleration, and we think of inertial mass as the measure of an object's *resistance* to acceleration under an applied force. This is a statement of Newton's second law *equation of motion*.

Though famous, this result actually does not appear in the *Mathematical Principles*, despite the fact that Newton must have been aware of this particular formulation, which features in German mathematician Jakob Hermann's treatise *Phoronomia*, published in 1716.* It is sometimes referred to as the 'Euler formulation', after the eighteenth-century Swiss mathematician Leonhard Euler.

Newton's version of classical mechanics is expressed in terms of forces which result from the application of various mechanical 'actions'. Whilst it is certainly true to say that the notion of mechanical force still has much relevance today, the attentions of eighteenth- and nineteenth-century physicists switched from force to *energy* as the more fundamental concept. My foot connects with a stone, this action impressing a force on the stone. But a better way of thinking about this is to see the action as transferring energy to the stone.

Like force, the concept of energy also has its roots in seventeenth-century mechanical philosophy. Leibniz wrote about *vis viva*, a 'living force' expressed as mv^2, and he speculated that this might be a *conserved* quantity, meaning that it can only be transferred between objects or transformed from one form to another—it can't be created or destroyed. The term 'energy' was first introduced in the early nineteenth century and it gradually became clear that kinetic energy—the energy of motion—is not in itself conserved. It was important to recognize that a system might also possess *potential* energy by virtue of its physical characteristics and situation. It was then possible to formulate a law of conservation of the *total* energy—kinetic plus potential—largely through the efforts of physicists concerned with the principles of thermodynamics, which we will go on to examine later in this Prologue.

If we denote the kinetic energy as T and the potential energy as V, then the total energy is simply $T + V$. The kinetic energy T is given by

$$T = \frac{1}{2}mv^2 = \frac{p^2}{2m}. \tag{P.3}$$

It's helpful to understand how this relates to Newton's force, F. Differentiating (P.3) with respect to time gives

$$\frac{dT}{dt} = \frac{1}{2}m\frac{d\left(v^2\right)}{dt} = \frac{1}{2}m\left(v\frac{dv}{dt} + v\frac{dv}{dt}\right) = mv\frac{dv}{dt} = mva. \tag{P.4}$$

In (P.4) we have assumed the mass m to be independent of time and we have applied the product rule $d(uv)/dx = v\,(du/dx) + u\,(dv/dx)$ to the evaluation of $d\left(v^2\right)/dt$. We can now make use of the second law $F = ma$ and the chain rule

* Newton published a third edition of the *Mathematical Principles* in 1726 and, if he had been so minded, could have incorporated this version of the second law.

$$\frac{dT}{dt} = Fv \quad \text{and so} \quad dT = Fvdt = F\frac{dx}{dt}dt = Fdx. \tag{P.5}$$

Integrating then allows us to express the kinetic energy in terms of force as follows:

$$T = \int Fdx. \tag{P.6}$$

We can now put Newton's conception of force on a much firmer basis. We *define* the potential energy V as

$$V = -\int Fd\text{x}. \tag{P.7}$$

This shift in emphasis from force to energy in the eighteenth and nineteenth centuries meant that it made more sense to define the secondary property of force in terms of the primary property of potential energy:

$$F = -\frac{dV}{dx}. \tag{P.8}$$

Equations (P.7) and (P.8) make perfect sense. Lifting a heavy weight from its initial position on the floor to shoulder height involves the application of a force which changes the potential energy of the weight. The force applied is negative (as its acts *against* gravity), and transfers energy from the gravitational field into the potential energy of the weight. Letting go of the weight exposes it to the force of gravity, converting the gravitational potential energy it contains into kinetic energy, and it falls back to its initial position on the floor. The force is directed in such a way as to reduce the potential energy—hence the negative sign in (P.7)—driving the system 'downhill'. And the 'steeper' the shape of the potential energy curve (the faster the potential energy changes with position), the greater the resulting force, (P.8).

Setting up the relationship between force and potential energy in this way means that in a closed system which cannot exchange energy with the outside world the rate of change of total energy with time balances to zero—energy can be moved back and forth between potential and kinetic forms but the *total energy is conserved*:

$$\frac{dT}{dt} + \frac{dV}{dt} = mva + \frac{dV}{dx}\frac{dx}{dt} = mva + v\frac{dV}{dx} = v\left(ma + \frac{dV}{dx}\right) = v\,(ma - F). \tag{P.9}$$

We can see from this that the time derivatives of the expressions for kinetic and potential energy sum to zero—the total energy doesn't change with time.

This shift of emphasis led to a substantial and profound reformulation of classical mechanics, first by Italian mathematician and astronomer Joseph-Louis Lagrange (in 1764) and subsequently by Irish physicist William Rowan Hamilton (in 1835). This wasn't simply about recasting Newton's laws in terms of energy. Hamilton in particular greatly elaborated and expanded the classical structure and the result, called *Hamiltonian mechanics*, extended the number of mechanical situations to which the theory could be applied.

Newton's equation of motion $F = ma$ is formulated in terms of position coordinates (such as Cartesian coordinates x, y, z) and time. This is fine in principle for very simple systems involving at most one or two objects, but it quickly becomes problematic for systems involving large numbers of objects. To define the physical 'state' of a system consisting of, say, N objects, such that we can predict how the system will evolve in time, we would need to specify the position *and* the velocity of *each* of the N objects in three-dimensional space, at specific moments in time. It's not enough just to specify the positions—Newton's second law applies to objects that are already in a state of rest or uniform motion, so to predict what happens next we also need to know how fast and in which directions the objects are moving as the force is applied. In other words we need a total of $6N$ coordinates *for each object*.

We can think of the motion of the system as a 'trajectory' in an abstract $6N$-dimensional *configuration space*. Instead of positions and velocities, Hamilton's reformulation makes use of the positions of the objects and their *momenta*. If we keep things simple by restricting ourselves to a single object with inertial mass m moving along a single position coordinate x, then these *canonical coordinates* are (x, p), where p is again the object's linear momentum. Hamilton's choice defines what would subsequently become known as *phase space*.

The motion of the object is then represented by the *trajectory of a point in the phase space coordinates*. This gives us an advantage in more complex systems because instead of specifying the initial positions and velocities of all the objects in a $6N$-dimensional configuration space, in Hamiltonian mechanics we just need to specify the system's initial position in phase space. It then becomes possible to predict the future time evolution of the system from any starting point on its phase space diagram.

As we will draw on many of these concepts in what follows, it's worth taking the time here for a very brief and somewhat superficial look at Hamiltonian mechanics. The *Hamiltonian* of a classical system is simply the total energy, E, and is defined as

$$H\,(=E) = T + V. \tag{P.10}$$

In Hamiltonian mechanics we're obviously interested to know the behaviour of the Hamiltonian H with respect to the canonical coordinates, which in a single dimension are given by (x, p). This behaviour is summarized in Hamilton's equations of motion:

$$\frac{dp}{dt} = -\frac{\partial H}{\partial x} \quad \text{and} \quad \frac{dx}{dt} = \frac{\partial H}{\partial p}. \tag{P.11}$$

These equations may appear somewhat unfamiliar, but the second establishes a fairly straightforward connection between momentum and velocity. Remember, we assume that the potential energy V is independent of p, and from (P.3) we know that $T = p^2/2m$:

$$\frac{\partial H}{\partial p} = \frac{\partial T}{\partial p} = \frac{\partial}{\partial p}\left(\frac{p^2}{2m}\right) = \frac{p}{m} = v = \frac{dx}{dt}. \tag{P.12}$$

And the first is simply a restatement of Newton's second law:

$$-\frac{\partial H}{\partial x} = -\frac{\partial V}{\partial x} = F = \frac{dp}{dt}\ (= ma). \tag{P.13}$$

It's worth noting in passing that we've traded Newton's single equation of motion, which is a *second-order* differential equation (remember, $a = d^2x/dt^2$), for Hamilton's two *first-order* partial differential equations, (P.11).

We can get some sense for how this works by considering a simple example. In one-dimensional simple harmonic motion (such as a low-amplitude pendulum or an object suspended on a spring), an object of mass m oscillates back and forth with an angular frequency ω under the action of a 'restoring' force $F = -m\omega^2 x$. The Hamiltonian for this system is, therefore,

$$H = \frac{p^2}{2m} + \frac{1}{2}m\omega^2 x^2 \tag{P.14}$$

(remember, $V = -\int F dx$), and Hamilton's equations of motion are

$$\frac{dp}{dt} = -\frac{\partial H}{\partial x} = -m\omega^2 x \quad \text{and} \quad \frac{dx}{dt} = \frac{\partial H}{\partial p} = \frac{p}{m}. \tag{P.15}$$

If we define the initial position (x_0) to be the origin at time $t = 0$ (i.e. $x_0 = 0$), then the solutions of these equations have the particularly simple form

$$p = p_0 \cos \omega t \quad \text{and} \quad x = \frac{p_0}{m\omega} \sin \omega t, \tag{P.16}$$

where p_0 is the initial momentum. In a phase space with canonical coordinates (x, p), the motion describes an elliptical trajectory:

$$\frac{x^2}{(p_0/m\omega)^2} + \frac{p^2}{p_0^2} = 1. \tag{P.17}$$

Switching to a phase space description allows us to represent the mechanics in terms of the single trajectory of a point in a multidimensional space, summarizing the motion of the *entire system*, not the individual objects. This was a generalization discovered by French mathematician Henri Poincaré in 1888, from his study of the infamous three-body problem (and which also led him to appreciate the sensitivity of dynamical systems to initial conditions, later to become an obsession of chaos theory).

A year later, Poincaré noted a rather curious phenomenon. In an ideal mechanical system with a finite upper bound on the volume of available phase space (one in which no objects can escape the system and in which energy is conserved), within a sufficiently long, but finite, time the phase space trajectory will return to its starting point.* This is called *Poincaré recurrence*. No matter how many objects are involved, if the dynamics unfold from some starting configuration and we have sufficient patience, the system *will* return to this configuration.

The Troublesome Concept of Mass

The development of our understanding of potential energy in the nineteenth century allowed us to put Newton's concept of force on a much firmer basis, as we've seen. There would appear to be no reason to question our understanding of any of the other concepts which appear in Hamilton's equations. We haven't forgotten the problems of absolute space and time but we surely know what we mean when we talk about acceleration, momentum, and mass.

But what, precisely, *is* inertial mass? Newton provides a handy definition very early in the *Mathematical Principles*:[7]

> *The quantity of matter is the measure of the same, arising from its density and bulk conjunctly* . . . It is this that I mean hereafter everywhere under the name body or mass. And the same is known by the weight of each body; for it is proportional to the weight, as I have found by experiments on pendulums, very accurately made, which shall be shewn hereafter.

If we interpret Newton's use of the term 'bulk' to mean volume, then the mass of an object is simply its density multiplied by its volume. It doesn't take long to figure out that this definition is entirely circular, as Austrian physicist Ernst Mach pointed out many years later:[8]

> With regard to the concept of "mass", it is to be observed that the formulation of Newton, which defines mass to be the quantity of matter of a body as measured by the product of its volume and density, is unfortunate. As we can only define density as the mass of a unit of volume, the circle is manifest.

We have to face up to the rather unwelcome conclusion that in classical mechanics we don't really know what inertial mass is.

* Poincaré's theorem also requires that phase volume is conserved as the system evolves, which is true for all Hamiltonian systems by virtue of Joseph Liouville's 1838 theorem.

The Force of Gravity

In Newton's law of universal gravitation, two objects with masses m_1 and m_2 experience a force of gravity that is proportional to the product of their masses (are these the same as inertial mass?) and inversely proportional to the square of the distance between them, r, or $F = Gm_1m_2/r^2$, where G is Newton's gravitational constant.

This was another great success, but it also came with another hefty price tag. Although the symbol F might be the same, Newton's force of gravity is distinctly different from the kinds of forces involved in his laws of motion. The latter forces are *impressed*; they are caused by actions such as kicking, shoving, pulling, or whirling. They require physical contact between the object at rest or moving uniformly and whatever it is we are doing to change the object's motion. Newton's gravity works very differently. It is presumed to pass instantaneously between the objects that exert it, through some kind of curious action at a distance. It was not at all clear how this was supposed to work. Leibniz was again dismissive: 'This, in effect, is going back to qualities which are occult or, what is more, inexplicable.'[9]

Newton himself had nothing to offer. In a general discussion (called a 'general scholium'), which he added to the 1713 second edition of the *Mathematical Principles*, he wrote:[10]

> Hitherto we have explain'd the phænomena of the heavens and of our sea, by the power of Gravity, but have not yet assign'd the cause of this power... I have not been able to discover the cause of those properties of gravity from phænomena, and I frame no hypotheses.

Light Waves and the Ether

Newton sought to extend the scope of his mechanics to include light, and in his treatise *Opticks*, first published in 1704, he concluded that light is essentially 'atomic' in nature, consisting of tiny particles, or corpuscles. Two of his contemporaries, English natural philosopher and experimentalist Robert Hooke and Dutch physicist Christiaan Huygens, had argued compellingly in favour of a wave theory of light, and Newton's incendiary disputes with Hooke led him to postpone publication of *Opticks* until after Hooke's death in March 1703. Such was Newton's standing and authority that the corpuscular theory held sway for more than a hundred years.

But in a series of papers read to the Royal Society of London between 1801 and 1803, nearly eighty years after Newton's death, an English medical doctor (and part-time physicist) called Thomas Young revived the wave theory as the only logical explanation for the phenomena of light diffraction and interference. In one experiment, commonly attributed to Young (although historians are divided on whether he actually performed it), he showed that when passed through two narrow, closely spaced holes or slits, light produces a pattern of bright and dark fringes. These are readily explained in terms of a wave theory of light in which the peaks and troughs of the light waves from the two slits start out in phase, spread out beyond, and overlap. Where a peak of one wave is coincident with a peak of another, the two waves add and reinforce to produce constructive interference, giving rise to a bright fringe. Where a peak of one wave is coincident with a trough of another, the two waves cancel to produce destructive interference, giving a dark fringe.

Today this logic seems inescapable, but Young's conclusions were roundly criticized, with some condemning his explanation as 'destitute of every species of merit'.[11] Nevertheless, as the nineteenth century progressed, the wave theory gained a slow, if somewhat grudging, acceptance. Then, as is so often the case in science, perhaps the most compelling arguments in favour of the wave theory emerged from a seemingly unrelated discipline.

The intimate connection between the phenomena of electricity and magnetism was established over a long period of study in the nineteenth century, most notably through the extraordinary experimental work of Michael Faraday at London's Royal Institution. Drawing on analogies with fluid mechanics, over a ten-year period from 1855 Scottish physicist James Clerk Maxwell developed a theory of *electromagnetic* fields whose properties are described by a set of complex differential equations. These equations can be manipulated to give expressions for the space and time dependences of the electric field $\boldsymbol{E}$ and magnetic field $\boldsymbol{B}$ in a vacuum, as follows (again simplified to one dimension):

$$\frac{\partial^2 \boldsymbol{E}}{\partial x^2} = \varepsilon_0 \mu_0 \frac{\partial^2 \boldsymbol{E}}{\partial t^2} \quad \text{and} \quad \frac{\partial^2 \boldsymbol{B}}{\partial x^2} = \varepsilon_0 \mu_0 \frac{\partial^2 \boldsymbol{B}}{\partial t^2}. \tag{P.18}$$

In Eq. (P.18), ε_0 and μ_0 are the relative permittivity and permeability of free space, respectively. The former is a measure of the resistance of a medium (in this case, the 'vacuum') to the formation of an electric field—a certain fixed electric charge will generate a greater electric flux in a medium with low permittivity. The latter is a measure of the ability of a medium to support a magnetic field—applying a certain fixed magnetic field strength will result in greater magnetisation in a medium with high permeability.

Maxwell had made no assumptions about how these fields are supposed to move through space. But his equations not only demonstrate rather nicely the symmetry of the interdependent electric and magnetic fields, they also rather obviously describe wave motion. For a wave travelling in one dimension with velocity v, a generalized wave equation can be written as

$$\frac{\partial^2}{\partial x^2} \Psi(x, t) = \frac{1}{v^2} \frac{\partial^2}{\partial t^2} \Psi(x, t), \tag{P.19}$$

where $\Psi(x, t)$ is a generalized 'wavefunction'. From (P.18) and (P.19) we can deduce that $v = 1/\sqrt{\varepsilon_0 \mu_0}$. The velocity of Maxwell's 'electromagnetic waves' could now be determined from the experimental values of the relative permittivity and permeability of free space, which had been reported by German physicists Wilhem Weber and Rudolf Kohlrausch in 1856. Maxwell found that:[12]

> This velocity is so nearly that of light, that it seems we have strong reason to conclude that light itself (including radiant heat, and other radiations if any) is an electromagnetic disturbance in the form of waves propagated through the electromagnetic field according to electromagnetic laws.

But an electromagnetic disturbance in what? If we throw a stone into a lake, and watch as the disturbance ripples across the surface of the water, we conclude that the waves travel in a 'medium'—the water in this case. There could be no escaping the conclusion: electromagnetic waves had to be waves in some kind of medium. Maxwell himself didn't doubt that electromagnetic waves must move through the ether, a purely hypothetical, tenuous form of matter thought to fill all of space.

And here's another price to be paid. All the evidence from experimental and observational physics suggested that if the ether really exists, then it couldn't be participating in the motions of observable objects. The ether must be stationary. If the ether is stationary, then it is also by definition absolute: it fills precisely the kind of container demanded by an absolute space. A stationary ether would define the ultimate inertial frame of reference.

Newton required an absolute space that sits passively in the background and which, by definition, we can never experience. Now we have an absolute space that is supposed to be filled with ether. That's a very different prospect.

If the Earth spins in a stationary ether, then we might expect there to be an ether wind at the surface (actually, an ether drag, but the consequences are the same). The ether is supposed to be very tenuous, so we wouldn't expect to feel this wind like we feel the wind in the air. But, just as a sound wave carried in a high wind reaches us faster than a sound wave travelling in still air, we might expect that light travelling in the direction of the ether wind should reach us faster than light travelling against this direction. A stationary ether suggests that the speed of light should be different when we look in different directions.

Any differences were expected to be very small, but nevertheless still measureable with late-nineteenth-century optical technology. In 1887, American physicists Albert Michelson and Edward Morley performed experiments to look for such differences using a device called an interferometer, in which a beam of light is split and sent off along two different paths. The beams along both paths set off in phase, and they are then brought back together and recombined. Now, if the total path taken by one beam is slightly longer than the total path taken by the other, then when the beams are recombined, peak may no longer coincide with peak and the result is destructive interference. Alternatively, if the total paths are equal but the speed of light is different along different paths, then the result will again be interference.

But they could detect no differences. Within the accuracy of the measurements, the speed of light was found to be constant, irrespective of direction, suggesting that there is no such thing as a stationary ether. This is one of the most important 'negative' results in the entire history of experimental science.

Newton's laws of motion demand an absolute space and time that we can't experience or gain any empirical evidence for. Maxwell's electromagnetic waves demand a stationary ether to move in, but we can't gain any evidence for this either.

Atoms and the Second Law

The second law in question here is that of thermodynamics, the science born from the study of engines, and particularly the relationship between heat and work. French physicist and engineer Sadi Carnot is credited with establishing the basis for thermodynamics with his 1824 publication *Reflections on the Motive Power of Fire*, although some ten years passed before the merits of Carnot's work were realized by his fellow countryman Émile Clapeyron, who helped rid Carnot's theory of the concept of heat as a fluid, called caloric. Nine years later English physicist James Joule identified the mechanical equivalent of heat—motion and heat are equivalent and interchangeable—and helped to establish the law of conservation of energy. When Kelvin coined the term 'thermodynamics' in 1854, the conservation of energy was summarized as its first law.

Carnot had imagined that useful work can be derived as heat 'falls' from a higher temperature to a lower temperature, just as falling water will turn a paddle wheel. But Carnot imagined that heat would be conserved, meaning that all the usable heat is transferred into work without loss, allowing the possibility of perpetual motion and obviously in conflict with the conservation of energy. In 1850, German physicist Rudolf Clausius resolved this problem by declaring as a principle that heat cannot spontaneously flow from a cold object to a hot object, with the rest of the universe remaining unchanged.* For a system undergoing a closed cyclic process in which heat is transformed into work which is then transformed back into heat, Clausius expressed this principle mathematically as an inequality:

$$\oint \frac{\delta Q}{T} \leq 0. \tag{P.20}$$

In this equation the increments δQ represent the net amount of heat added to a system from an external reservoir at temperature T. For processes that are cyclical *and reversible*, meaning that infinitesimal changes that maintain thermodynamic equilibrium can in theory restore the initial state, the equality holds. But for processes that are irreversible the inequality holds. The logic here is fairly simple. In an irreversible process the (positive) heat input divided by the higher temperature *will always be smaller* than the (negative) heat output divided by the lower temperature. Summing (or integrating) over the cycle means $\delta Q/T < 0$.

Clausius was able to show that the ratio $\delta Q/T$ is a quantity which depends only on the physical state of the system, and not on the details of the path taken to produce it. Hence it is a *property* of the system, also called a function of state (or state function). In 1865 he went a little further, and identified this property as the *entropy* (symbol S) of the system, which he now defined for *reversible* open paths connecting some initial state i with a final state f, as

* Kelvin formulated a similar principle at around the same time.

$$\Delta S_{rev} = S_f - S_i = \int_i^f dS_{rev} = \int_i^f \left(\frac{\delta Q}{T}\right)_{rev}. \tag{P.21}$$

The property of entropy accounts for the dissipative loss of heat (or energy) from the system, but to get a real sense for what this means we need to look at how Eqs. (P.20) and (P.21) can be combined. Equation (P.20) applies to a closed cycle which may involve paths that are reversible and/or irreversible, whereas (P.21) applies only to open paths that are reversible. So, imagine a closed cycle in which the path from initial to final state is irreversible, but the return path from final to initial state is reversible. From (P.20) we have

$$\oint \frac{\delta Q}{T} = \int_i^f \frac{\delta Q}{T} + \int_f^i \left(\frac{\delta Q}{T}\right)_{rev} \leq 0. \tag{P.22}$$

But the return path is reversible, and so from (P.21) we know that

$$\int_f^i \left(\frac{\delta Q}{T}\right)_{rev} = S_i - S_f. \tag{P.23}$$

Hence,

$$\int_i^f \frac{\delta Q}{T} + S_i - S_f \leq 0, \quad \text{or} \quad \Delta S_{irr} = S_f - S_i \geq \int_i^f \frac{\delta Q}{T}. \tag{P.24}$$

We see that the change in entropy from initial to final state in an irreversible process is *always greater* than the corresponding change for a completely reversible process, which is a direct consequence of applying Clausius' inequality. Heat transfer to a system increases its entropy, and heat transfer from a system will decrease its entropy, but factors that result in irreversibility (such as friction and other loss mechanisms) *will always increase the entropy*. We can see this more clearly by generalizing (P.24) for any irreversible process in an *isolated* system (one which doesn't exchange energy with the external environment). In such a situation $\delta Q = 0$ and

$$\Delta S_{irr} \geq 0, \tag{P.25}$$

which is a statement of the second law of thermodynamics.

This version of the second law was deduced by German physicist Max Planck in his 1879 doctoral thesis. He regarded it as a much more general statement, and so more fundamental and profound. For an isolated system energy will be conserved (first law) but entropy will inexorably increase to a maximum (second law) as the system achieves thermal equilibrium. Irreversibility and the increase in entropy are intimately linked, defining an 'arrow of time' such that any reverse process, spontaneously *decreasing* entropy, implies running *backwards* in time, 'so that a return of the world to a previously occupied state is impossible'.[13]

And therein lies another problem.

As the science of thermodynamics was being worked out in the nineteenth century, so too was an elaborate mechanical theory of atoms. Hard, impenetrable, indestructible atoms, no more sophisticated than those imagined by the atomist philosophers of ancient Greece, had been an accepted metaphysical pre-commitment of seventeenth-century mechanical philosophers such as Newton. This despite the fact that they were not really necessary and did not feature in the classical mechanics that these philosophers helped to establish. Newton's atomism was quite influential in the eighteenth century, but as atoms appeared to lie well beyond the scope of any available experimental or observational technology, they remained firmly speculative.[14]

In 1738, the Swiss physicist Daniel Bernoulli had argued that the properties of gases could be understood to derive from the rapid motions of the innumerable atoms or molecules that constitute the gas (hereafter referred to simply as 'atoms'). Gas pressure then results from the *impact* of these atoms on the surface of the vessel that contains them. Gas temperature is the result of the *motions* of the atoms. This *kinetic theory of gases* bounced around for a few decades before being refined by Clausius in 1857. Two years later Maxwell developed a mathematical formula for the distribution of the velocities of the atoms in a gas. As it is obviously impossible to keep track of the motions of large numbers of individual atoms, Maxwell was obliged to resort to probabilities and so derived a probability distribution. This was generalized in 1871 by Austrian physicist Ludwig Boltzmann, and is now known as the Maxwell–Boltzmann distribution.

Boltzmann built further on Maxwell's ideas, applying probabilities to the distribution of *energy* instead of velocity, as he worked to derive all the most important thermodynamic quantities based on the underlying motions of the system's constituent atoms. In 1877 he derived the expression for the entropy of an ideal gas which is carved on his gravestone,

$$S = k_B \ln(W), \tag{P.26}$$

where k_B is Boltzmann's constant and W is the number of microstates (the number of individual configurations of atomic positions and velocities or momenta that are possible). If it is assumed that all these microstates are equally probable, then the probability for each microstate is simply $1/W$. Bulk quantities such as pressure, temperature, and entropy summarize the macrostate of the system.

The second law can now be interpreted as the natural evolution of an isolated system towards the largest number of available microstates. If we pump a gas into one corner of an otherwise empty container, we anticipate that this system will evolve dynamically: the gas will expand and become diluted so that it fills all of the available space. The number of microstates (atomic positions and momenta) that are available in the final equilibrium situation is much greater than in the initial situation. Entropy increases.

We can now see how Hamiltonian mechanics is perfectly suited to the interpretation of thermodynamics in terms of complex systems involving the motions of large numbers of atoms. In his *Lectures on Gas Theory*, published in 1896, Boltzmann himself defined 'phase' to mean the collective state of a gas derived from the positions and momenta of all its constituent atoms, though he held back from calling it phase space.[15]

But towards the end of the nineteenth century the existence of atoms was still largely a matter for metaphysical speculation and many physicists were inclined to be rather stubborn about them. It's perhaps difficult for readers who have lived with the fallout from the 'atomic age' to understand why perfectly competent scientists should have been so reluctant to embrace atomic ideas, but we must remember that by 1900 there was very little evidence for their existence. Some physicists, such as the arch-empiricist Mach, rejected them completely. To make matters considerably worse, the statistical mechanical interpretation of thermodynamics produced conclusions which some physicists found extremely discomforting.

Statistics have a dark side. They deal with *probabilities*, not certainties. What thermodynamics argues to be unquestionably irreversible and a matter of irresistible natural law, statistics argues that this is only the most probable of many different possible alternatives. The conflict was most stark in the interpretation of the second law and in 1895, with Planck's approval, his research assistant Ernst Zermelo took the argument directly to the atomists in the pages of the German scientific journal *Annalen der Physik*.

If we were to release two gases of different temperature in a closed container, the second law predicts that the gases will mix and the temperature will become uniform, with the entropy of the mixture increasing to a maximum. However, according to the atomists, the behaviour of the gases is a consequence of the underlying mechanical motions of the atoms of each gas, and the equilibrium state of the mixture is simply the most probable of many possible states. Furthermore, such dynamical systems could be expected to exhibit Poincaré recurrence, implying that, if we wait long enough, the system will eventually return to its initial far-from-equilibrium state, with the gases once more separated at different temperatures. Such a possibility runs directly counter to the second law, which insists that in an isolated system undergoing spontaneous change, entropy can never decrease, Eq. (P.25).

Boltzmann had no real alternative but to accept what statistical mechanics implied. Entropy does not always increase, he argued, in contradiction to the most common interpretation of the second law. It just almost always increases. Statistically speaking, there are many, many more states of higher entropy than there are of lower entropy, with the result that the system spends much more time in higher entropy states. In effect, Boltzmann was saying that if we do indeed wait long enough, we might eventually catch a system undergoing a spontaneous reduction in entropy. This is as miraculous an event as a smashed cocktail glass spontaneously reassembling itself, to the astonishment of party guests.

To Planck, this stretched the interpretation of his cherished second law to breaking point. It may have been that Planck was not averse to the atomic theory per se—he was certainly well aware of the theory's successes. But he judged that it was unlikely to offer a productive approach to a deeper understanding of thermodynamics. In a letter to Wilhelm Ostwald in 1893 he declared that the atomic theory was nothing less than a 'dangerous enemy of progress'.[16] Matter is continuous, not atomic, he insisted. He had no doubt that atomic ideas would eventually have to be abandoned, despite their success, 'in favour of the assumption of continuous matter'.[17] In his historical analysis, American philosopher Thomas Kuhn argues that Planck's 'continuous medium' would subsequently become the ether.[18]

In seeking to find a way to refute Boltzmann's statistical arguments, Planck chose as a battleground the physics of 'black body' radiation. And this is where our story really begins.

NOTES

1. See, for example, Walter Isaacson, *Einstein: His Life and Universe*, Simon & Schuster, London, 2007, p. 90.
2. Although this statement is widely attributed to Kelvin, Isaacson could find no evidence to suggest that Kelvin had actually said it. See Isaacson, *Einstein*, p. 575.
3. A. A. Michelson, *Light Waves and Their Uses*, University of Chicago Press, Chicago, 1903, p. 24.
4. In April 1900 Kelvin delivered a lecture to the Royal Institution in London in which he declared that there were several 'clouds' over the dynamical theory of heat and light. See Helge Kragh, *Quantum Generations: A History of Physics in the Twentieth Century*, Princeton University Press, Princeton NJ, 1999, p. 6.
5. Isaac Newton, letter to Robert Hooke, 15 February 1676. See: https://digitallibrary.hsp.org/index.php/Detail/objects/9792
6. Gottfried Wilhelm Leibniz, from his correspondence with Samuel Clarke (1715–16), *Collected Writings*, edited by G. H. R. Parkinson, J. M. Dent & Sons, London, 1973, p. 226.
7. Isaac Newton, *Mathematical Principles of Natural Philosophy*, first American edition translated by Andrew Motte, published by Daniel Adee, New York, 1845, p. 73.
8. Ernst Mach, *The Science of Mechanics: A Critical and Historical Account of Its Development*, 4th edition, translated by Thomas J. McCormack, Open Court Publishing, Chicago, 1919, p. 194 (first published 1893). See also Max Jammer, *Concepts of Mass in Contemporary Physics and Philosophy*, Princeton University Press, Princeton, NJ, 2000, p. 11; and O. Bellkind, *Physical Systems*, Boston Studies in the Philosophy of Science, **264** (2012), 119–44.
9. Gottfried Wilhelm Leibniz, *New Essays on the Human Understanding*, reproduced in *Collected Writings*, ed. Parkinson, p. 167.
10. Newton, *Mathematical Principles*, p. 506.
11. This quote is taken from Henry Brougham, *The Edinburgh Review, or Critical Journal*, **1**, January 1803, pp. 450–6. This was an anonymous review, but Young correctly identified the author as Brougham, then a barrister and later Lord Chancellor of England. For more details on the controversy, see Christine Simon, 'Thomas Young's Bakerian Lecture', *The Fortnightly Review*, 2014: http://fortnightlyreview.co.uk/2014/09/thomas-young/#fnref-13923-13
12. J. Clerk Maxwell, 'A Dynamical Theory of the Electromagnetic Field', *Philosophical Transactions of the Royal Society of London*, **155** (1865), 466.
13. This statement appears in the very first introductory paragraph of Planck's thesis, *Über den zweiten Hauptsatz der mechanischen Wärmetheorie* (On the Second Law of Thermodynamics), 1879. Quoted in Thomas S. Kuhn, *Black-body Theory and the Quantum Discontinuity 1894–1912*, University of Chicago Press, Chicago, 1978, p. 16.

14. See Alan Chalmers, *The Scientist's Atom and the Philosopher's Stone: How Science Succeeded and Philosophy Failed to Gain Knowledge of Atoms*, Springer, London, 2011, especially Chapters 6 and 7.
15. David D. Nolte, 'The Tangled Tale of Phase Space', *Physics Today*, April 2010, 33–8.
16. Max Planck, letter to Wilhelm Ostwald, 1 July 1893. Quoted in J. L. Heilbron, *The Dilemmas of an Upright Man: Max Planck and the Fortunes of German Science*, Harvard University Press, Cambridge, MA, 1996, p. 15.
17. Max Planck, *Physikalische Abhandlungen und Vorträge*, Vol. 1, Vieweg, Braunschweig, 1958, p. 163. Quoted in Heilbron, *Dilemmas*, p. 14.
18. Kuhn, *Black-body Theory*, p. 23.

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