Rise, and (Impending) Fall of Physics Fundamentalism

Paul Teller

[prteller@ucdavis.edu](mailto:prteller@ucdavis.edu)

**1. Introduction and background material.** Many think of science as discovering laws and other important truths about the world. It is central to this way of thinking about science that the individual discoveries can be consistently “pasted together” to provide an increasingly detailed and unified representation of disparate parts of our complex world. The foregoing is a way of thinking about the monism introduced in Chapter 1 of this volume. But this is not the way modern science works, nor how it has worked from its inception in the scientific revolution of the Early Modern Period. In this chapter I will use a historical sketch to explain how this misconception arose and how it is now unraveling in favor of a much more accurate, and interesting, understanding of the objectives and methods of scientific accounts.

Truth plays a leading role in the story. Truth, as it is understood in philosophy and in much broader discourses, applies to a statement when the statement is correct without any qualification. A true statement is accurate, full stop. It cannot be made more accurate. The common conception of science takes it to provide a growing collection of truths. Since truths cannot be made more accurate, when a statement is known to be true, it becomes a permanent part of the scientific corpus. This way of conceiving of the scientific corpus is nicely expressed with the metaphor of a mirror of nature. Each true, known conclusion of science provides a little mirror, a “tile”, that perfectly “reflects” the corresponding part of nature. Science gradually amasses and fits together these tiles to provide a growing mirror of that part of nature that so far has been understood. This growing mirror of nature is one way – the way that I will have in mind – of understanding the monism discussed in chapters 1 and 2. In the exposition below the formulation of monism that I will discuss will take the more specific form of what I will call “physics fundamentalism”.

Readers who have dirtied their brains with the messiness of real science will be thinking: This mirror of nature metaphor is a bit of a caricature. What is the alternative? On the terminology of chapters 1 and 2, the denial of monism is called pluralism, and pluralism could take a number of different forms. On my understanding of this idea science provides what I will call “models”, representations that are no perfect mirror of nature, that are representations that can always be improved by making them more accurate or more precise or both. But a successful model gets things “right enough” so that it will not lead us astray when it comes to a limited range of intended applications. Taking science to provide not truths but models constitutes one way to understand pluralism, the doctrine that human knowledge is comprised by parts that may not fit together in the way that would be required by a growing exactly faithful mirror of nature. Indeed, if read in a literal minded way, sometimes successful models can outright contradict one another.  
   
 In preparation I will present some basic ideas, including a more detailed discussion of models, that we will need in following this story.

laws and Laws: Readers will be familiar with common simple laws of physics. Some examples:

Hooke’s law: F = kx: A spring will stretch a length x when pulled with a force F, k is a constant that characterizes the spring.

The ideal gas law: T = kVP, T the (absolute) temperature, P the pressure, V the temperature of a gas in a container. k is again a constant characterizing the gas and container in question.

These laws are not exactly correct. Hooke’s law fails completely if the spring is stretch too much, and even where it applies well it is not completely accurate. The ideal gas law holds exactly for an ideal gas, one with point particles that do not interact. For any real gas the law holds only approximately. Since these laws are far from completely accurate they do not fit into the exact mirror of nature metaphor. For guides such as Hooke’s and the ideal gas law I will use the term “law” written with a lower case “l”.

Let’s contrast these example with Newton’s second law, f = ma, where f is the total force that is applied to an object, m is the object’s mass, and a is the object’s acceleration. For more than two centuries it was taken, at least by most, to be exactly correct, strictly speaking true. Such a law would be a candidate for one of the tiles in that exact mirror of nature. When such a general claim is, or is taken or assumed to be, exactly true I will use the term “Law” written with upper case “L”. Such Laws are also often referred to as “Laws of nature” or Natural Laws”.

Mechanism: A mechanical clock is an exemplar. The idea is to understand the properties and behavior of a complex system in terms of the properties and orchestrated interaction of the parts. The root idea is that of a mechanical system, but the idea generalizes. One can think of electronic circuits as mechanisms, and likewise biological systems, from individual cells to whole organisms. Reference to mechanisms needs to include processes[[1]](#footnote-1), and where I use “mechanism”’ this should be heard as “mechanisms and processes”

Reduction: This is the idea of deriving the facts about a complex system from the facts about the parts. It has long been the dream of many that chemistry can be reduced to physics, that is that the laws of chemistry can be derived from the laws of physics, and with the help of these laws, also more specific facts as described by chemistry can be derived from facts as described by physics. Likewise it is hoped that biology can be reduced to chemistry, psychology to biology, etc. In this way everything is supposed to reduce to physical facts, in principle and hopefully someday in practice.

Idealization: I will use “idealize” very broadly to cover intentional misdescriptions that involve simplifications to make a problem more tractable without, hopefully, distorting the representation more than practical requirements allow. When a description is called an idealization, the implication is that, though not a completely accurate representation, the inaccuracies are hoped not to spoil intended applications

Models[[2]](#footnote-2): The root idea is of physical models, such as a model airplane or a “tinker toy” model of chemical compounds or the famous physical model of DNA:

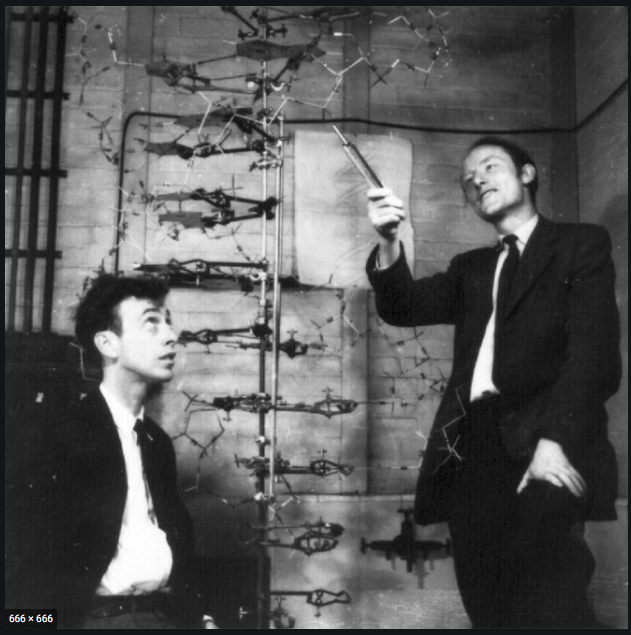
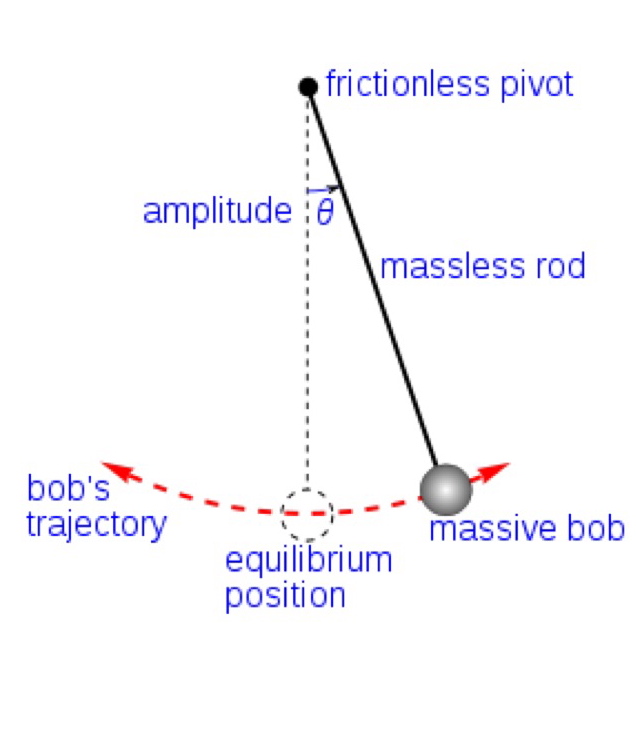


Image is from <https://www.reddit.com/r/HistoryPorn/comments/y6d0a/first_photo_of_james_watsons_and_francis_cricks/> Permission not yet requested

But models in science are most frequently abstract, presented with a formula or more complex description along with description or understanding of how the abstract structure is to be understood. A familiar example is the “simple gravity pendulum”:



Parts of the model

F = ma

Gravity the only force

Sin(θ) ≅ θ

Derive

θ(t) = θ0cos(t/p)

(Image is from <https://en.wikipedia.org/wiki/Pendulum> Text is mine. Permission not yet requested

This model is fashioned by idealized application of Newton’s second law, f = ma. It is specified that gravity is the only force and that the motion of a massive bob, idealized as a point mass, is constrained by a massless rigid rod turning on a frictionless pivot. The mathematics is simplified by replacing the sin of the amplitude angle, 𝜃, with 𝜃 itself, a good approximation when 𝜃 is small. The whole model is summarized with the formula 𝜃 = 𝜃0cos(t/p), 𝜃0 the amplitude (angle of the highest point of the swing), p the period (length of time to swing back and forth once), and t the time. This pendulum law is a prime example of the sorts of generalizations that summarize the content of an imprecise model that I refer to as “laws”, with lower case “l”.

Many, many other kinds of things can function as models, such as pictures, descriptions using words, and so on. The general idea is that models are representations that fall short of complete accuracy. In my usage, anything counts as a model as long it is being used as a representation and the representation is presented with the understanding that it is not completely accurate. Usually using the term “model”, like the term “idealization” carries the implication that inaccuracies won’t spoil use of the representation for current interests.

Because models are “good enough” but imperfect representations of various phenomena or systems, they will often not be susceptible to being stitched together in a consistent and unified larger picture as required by monism. One model will distort in one way to make it easy enough to capture one phenomenon, a second model may distort in a different way to facilitate an account of another phenomenon; and the distortions may conflict. Fluid mechanics describes water as a continuous medium while statistical mechanics describes water as a collection of discrete particles. Later I will further discuss this and similar examples and examine the ways in which the imperfections of scientific models make science a pluralist enterprise rather than one of filling in a single mirror of nature.

**2. History.** [[3]](#footnote-3) We start with the so-called “scientific revolution” of the seventeenth century, thought of as the period in which modern science originated. This scientific revolution was in considerable part guided and summarized by two metaphors: That of a mechanism, exemplified by the mechanical clock (Shapin, 1996, pp. 33 and passim) and the so-called “God’s book of nature” (Shapin, 1996, pp. 68-9 and passim). The idea of the clock metaphor was to see the world as a giant mechanism which was to be explained and understood in the same manner that one understands the mechanism of a clock. There is, as we shall see, a tension between the clock metaphor and the metaphor of reading God’s Book of Nature. Galileo thought of God’s Book as written in the language of mathematics. (Shapin 1996 p. 69) This attitude leads naturally to the conception of natural Laws, brought to fruition in Newton’s mathematical physics. God, the perfect being, would not write anything that suffered any kind of inaccuracy or imprecision. So the generalizations found there would be Natural Laws, statements that are in no way defective.[[4]](#footnote-4)

These strands, of mechanisms and Laws, contrast but are intimately interdependent. Understanding mechanisms requires applying what were taken to be Laws. Laws have no explanatory punch until they are applied to physical systems, what we are metaphorically thinking of as mechanisms. Though so intertwined, we can think of the subsequent history as the two metaphors issuing in a bifurcation of focus. Laws were assumed to be perfect. But in practice, accounts of mechanisms were always highly imperfect.

Newton’s system was remarkably successful in covering certain cases of the motion of bodies, especially the motion of the planets. But the system had a glitch: Newton’s gravity was action at a distance. For the “mechanical philosophers”, as the authors of modern science were known, only contact action is “mechanical”. For many, action at a distance made no sense. (Hesse, 1955, 337-340) Newton’s response (Principia, General Scolium) was to declare that his laws enabled computation of the motion of bodies without any claims about the mechanisms that bring these motions about, which I will summarize as saying that the laws are “mathematical only”.

Newton’s “mathematical only” was followed by a tradition that focused on the Laws, issuing in the “rational mechanics” [[5]](#footnote-5) of Lagrange and Hamilton.[[6]](#footnote-6) At least as we think of it now, there developed a system of “fundamental physics” that was intended to cover the most basic Laws that govern the behavior of everything. This attitude is now epitomized in the oft quoted passage from Laplace:

Such perfection as the human mind has been able to give to astronomy affords but a feeble sketch of [an imagined intelligence capable of exact application of Newton's Laws to the whole world]....All our efforts in our search for truth tend, without respite, to approximate the intelligence we have imagined. (1814, 2-3; my translation)

Let’s return to the mechanism side of what I am thinking of as a bifurcated focus. Newton’s system was breathtakingly successful – where Newton and his contemporaries knew how to apply it. The system matched observational results for planetary motion, for projectiles and for the tides. The limitation was that forces must be put into Newton’s second law, f = ma, and for the most part Newton only had his law of gravitation as a description of a force – little was known about other forces. In the eighteenth century saw increasing understanding and mathematical treatment of other forces. Giving just one example D’Alembert, Bernoulli, and Euler developed a mathematical description of vibrating strings, based on elastic forces, and descriptions of the behavior of fluids based on ingenious methods of dealing with contact forces. Understanding elastic and contact forces gave rise to treatment of the more complicated force concepts, stress, strain, and shearing forces.

This and much similar work in physics fall squarely in the mechanism tradition, applying Newton’s and force laws to understand how the whole behaves in terms of how the parts interact. In practice this can never be done with 100% precision and so naturally leads to the practice of science as fashioning circumscribed and generally not completely accurate models of the phenomena to be understood. Materials were described as having the same properties as one considers smaller and smaller parts. Forces were usually treated as linear, as when a spring is stretched twice as much it exerts twice the restoring force. Results applied only insofar as materials sufficiently approximated the idealized descriptions.

The theory of heat provided another major strand in developing physics. The Nineteenth Century work on thermodynamics intellectually fell on the Laws side. A few very simple principles, in many respects independent of Newtonian mechanics, supported a good account of heat and temperature that made no assumptions about the composition of the materials involved. Statistical mechanics returned to the mechanisms style, once again using Newton’s laws to derive the thermal behavior of the whole in terms of the parts, characterizing heat and temperature in terms of the random motion of particles constituting a body. Statistical mechanics refined thermodynamics’ highly idealized account but was itself still highly idealized, working in terms of the random collisions among point particles, tiny spheres described as structureless, or with minimal description of internal structure.

Physics’ theory of heat illustrates the interplay between the Laws and mechanisms side of the bifurcation. We see this also in the second half of the Nineteenth Century in Maxwell’s theory of electromagnetism. Physics had concluded, because of interference phenomena[[7]](#footnote-7), that light is a wave process. But a wave process, it was thought, must have some medium to “wave” in. So a supporting medium was postulated: the ether. (Hielbron, 203b; 273-4) Maxwell devised a theory of electricity and magnetism by supposing a complex mechanism that was supposed to operate in this ether. The way the parts were supposed to move revealed light as a propagating system of electric and magnetic fields. This was mechanistic theorizing in its greatest glory! The resulting equations describing light and the interacting behavior of electricity and magnetism worked with enormous accuracy – they are today as fundamental a part of physics as Newton’s laws. But the postulated underlying ethereal mechanism just didn’t mesh with the rest of physics. By the end of the 19th Century some returned to thinking of Maxwell’s equations as “mathematical only”: Hertz wrote that “Maxwell's theory is Maxwell's system of equations." (Hertz, 1893, 21).[[8]](#footnote-8) What was born in the tradition of mechanisms evolved into something in the tradition of laws.

So far I have focused on physics. A few words will indicate that most, but not all, of the rest of science falls on the mechanisms side of the bifurcation. Chemistry, as it grew in the seventeenth and blossomed in the nineteenth centuries always took Newton’s laws as constraints. It added new conceptions of energy and introduced laws of its own – ones that were always little ‘l’ laws. Very broadly, chemical accounts of molecular structure, properties, and reactions are paradigms of mechanistic thinking – understanding properties and behavior of the whole in terms of those of interacting parts. In particular, the conception of chemical bonds is a simplification – it is only in recent decades that quantum chemistry is producing more detailed account from the still idealized theory of quantum mechanics.

Biology developed with much less reliance on mathematically stated generalizations than did physics and chemistry. Evolutionary theory describes a process in terms of interaction of individual organisms with one another and with their environment. Cell and molecular biology also broadly fall into the mechanism side of things. A lot can be quantified. But where there are generalizations, whether qualitative of quantified, they are all, at best, little ‘l’ laws.

I can’t imagine that in the social sciences anyone would suppose that there are generalizations that meet the standards for capital “L” laws – both completely precise and completely accurate. But there is an interesting contrast in attitudes between a mechanisms attitude and one that we would need to characterize, not in terms of Laws, but analogously in terms of laws. We see this in the contrast between methodological individualism and methodological holism. Methodological individualism is a paradigmatic mechanist attitude in which the behavior of individual agents are the parts in terms of which the operation of the social whole is to be understood. Individualism contrasts with methodological holism according to which one can give generalizations about social phenomena in terms that stand on their own and are not to be derived from or understood in terms of facts about constituent individuals. This contrast stands out particularly clearly in the contrast between micro- and macro-economics. Microeconomics is straightforwardly in the mechanisms tradition, understanding largescale economic processes in terms of the economic activity and interaction of individual agents. Macroeconomics studies variables such as GDP, interest and unemployment rates, output, and consumption that describe an economy as a whole and theorizes about how these are functionally related without asking how these relations arise from the behavior of individual agents. Micro- and macro-economists are sometimes known to be caustically skeptical of one another’s disciplines, a circumstance that I am tempted to interpret as a manifestation of the two very different attitudes towards science that we have been following.

I will pause to connect what I have been writing about the Laws/mechanisms bifurcation in science to connect them with the idea of models. Recall that, in my usage, a model is any representation that is not completely accurate, with the implication that inaccuracies won’t spoil use of the representation for current interests. A great deal of science is devoted to finding mechanisms. Mechanisms are always, if we want to pay attention to fine details, extremely complicated. So the accounts that we fashion of mechanisms *always* count as models. We see this in all the foregoing examples, with hindsight going back to the scientific revolution. Insofar as science is a mechanism describing activity, it is a model building activity. Models are often summarized by laws, generalizations that fall short of the standard for Laws. What about Laws? Laws are understood to be completely precise and completely accurate. So they are not, or at least are not thought to be “mere” models. They are, or are alleged to be, Truths.

As we look back, we can describe two aspects of science at the end of the 19th century. We see science extensively engaged in fashioning models of mechanisms. Much more than is generally appreciated, building models of mechanisms covers an enormous fraction of scientific work. (I will mention some further important exceptions below.)

The second aspect was the work in physics that was taken to be the application of Laws. Newton’s Laws were often taken to be the final word, an accurate transcription from God’s book of nature. Some, possibly many, thought that at least in principle Newton’s Laws, in application to specific configurations of matter and forces, could be applied to give a scientific account of everything. Remember the quotation from Laplace! The fact that the thermal phenomena described by thermodynamics could be extracted from statistical mechanics may have impressed many – as we will see it certainly impressed mid-20th Century philosophers. I will consolidate these attitudes under the term “physics fundamentalism”: Physics discovers Laws from which, when applied to initial and boundary conditions, all empirical facts can be derived.

The history of physics fundamentalism is complex, as far as I know really still to be written. But as we look back now at the turn of the 20th century, Laws loomed large. Some readers will be familiar with the following summary account: “At the end of the 19th Century….it was generally accepted that all the important laws of physics had been discovered and that, henceforth, research would be concerned with clearing up minor problems….” (Wikipedia, “History of Physics, <https://en.wikipedia.org/wiki/History_of_physics>)

Modern historical work tells a very different story. (Kragh, 2012). The problem was that the two major innovations in physics, thermal physics and electrodynamics, really didn’t fit with Newtonian physics. Newtonian physics is time reversable (a movie of a scenario satisfying Newton’s laws run in reverse also satisfies Newton’s laws). But thermodynamics and statistical mechanics have an asymmetric direction of time. Electrodynamics seemed to require the ether, and the ether just didn’t fit with ordinary matter as described by Newtonian mechanics. Numerous efforts within the Newtonian paradigm to treat the ether and ordinary matter in a uniform way failed miserably. For these and other reasons the late 19th Century saw a range of different interpretations and efforts to rework Newtonian mechanics. Mach eliminated matter or any other underpinnings altogether with an extreme phenomenalism: “Physics is experience, arranged in economical order”. (1898, 221) The energetics of Ostwald, Duhem, and others worked to eliminate matter in favor of a reworking of Newtonian mechanics based on energy as the basic concept. Another line of work attempted to rethink the ether, not as a kind of matter but in terms of Maxwell’s electrodynamics. A flood of speculative ideas was further fueled by a number of failed predictions and some utterly puzzling phenomena.[[9]](#footnote-9) Kragh, (2012, 28) concludes that “At the same time as the majority of physicists worked within the framework of Newtonian mechanics, there was a growing dissatisfaction”, a sense that something fundamentally different was needed.

Something – actually two fundamentally new theories, quantum mechanics and relativity – soon developed. But from where came the idea that the late 19th century physics community thought that all the important laws of physics had been discovered, leaving only details to be mopped up? Here is my own guess. On the one hand, in the first quarter of the 20th century relativity and quantum mechanics cleared up a great many of the prior problems.[[10]](#footnote-10) On the other hand, with hind sight, much of the work on the ether, energetics, and the like were simply failed physics and so physicists no longer discussed or even mentioned them. Looking back, say from mid 20th century, to the 19th century what was then in plain view? The laws of thermodynamics and statistical mechanics, the laws of electromagnetism, and the always overarching Newtonian laws. This fits with the quote from Laplace from the early 19th century, a quote often repeated in the 20th. Since my objective is how we do and how we should think about science now, it is this 20th century simplified view of 19th century physics that is here relevant. This 20th century summary of the 19th saw the Laws of Newton, thermal physic, and electrodynamics, a few unexplained experimental phenomena, and makes little or no mention of modeling in physics[[11]](#footnote-11) or the rest of science.

In 1905 Einstein published his Special Relativity that had Newtonian mechanics as a limiting case for velocities slow compared to light. In 1915 General Relativity replaced Newton’s action at a distance force of gravity with gravity as a phenomenon of curved space-time. In 1900 Planck found a way to avoid the ultraviolet catastrophe with what he took to be a completely ad hoc and problematic assumption. (Kragh, 2000) In 1905 Einstein applied Planck’s idea to explain the photoelectric effect, and in 1913 Bohr used Plank’s idea to formulate a preliminary form of quantum mechanics that explained the stability of electron orbits in atoms and various facts about the spectrum of light emitted from atoms. In 1925 Schrödinger and Heisenberg independently discovered modern quantum mechanics. The attitude appears to have been: *Now* we have the right Laws and we can proceed with physics as the fundamental science. (Galison, 1983, pp. 42-44) Dirac, a principal contributor to the new quantum mechanics, wrote in 1929 that “The underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known.” (Dirac, 1929, 174) Relativity and quantum mechanics were stated with powerful mathematics. This situation no doubt encouraged physics fundamentalism as thinking about science.

Mid 20th century thinking about physics was heavily influenced by Positivism. There is, again, a complex history while my interest is in the streamlined picture of science that, in mid 20th century, took hold in philosophy of science and seeped into a much wider intellectual consciousness. This Positivism was elegantly and approachably presented in Hempel’s little *Philosophy of Natural Science* (1966) from which I learned my first philosophy of science. On this Positivist form of empiricism, science is about what we observe, coded in theories that are collections of Laws. Observational conclusions are deduced from Laws and description of prior observations. These same deductions enabled prediction, confirmation, and explanation.

Nagel presented a similar picture in his widely used *The Structure of Science.* (1951) In this book Nagel presented a formal account of reduction that was accepted for many decades, using the alleged reduction of thermodynamics to statistical mechanics as the leading example. To reduce thermodynamics to statistical mechanics one provides “coordinating definitions” that define the terms of the reduced theory - thermodynamics - using terms from the reducing theory – statistical mechanics. Temperature is defined as, or identified with, average energy of motion of a collection of randomly moving particles. One then uses the Laws of the reducing theory and the coordinating definitions to deduce the Laws of the reduced theory. Oppenheim and Putnam’s much quoted “The Unity of Science as a Working Hypothesis” (1958) aired the hope that chemistry could be reduced to quantum mechanics, biology to chemistry, psychology to biology, and so on.

Note the focus on Laws and deduction: Definitions was all that was left of fashioning models to describe mechanisms! Deduction from laws were supposed to suffice for prediction, confirmation, and explanation. Physics fundamentalism had fashioned a grip on philosophy of science and the Western intellectual community very broadly.

1962 saw a backlash against Positivism in Kuhn’s *The Structure of Scientific Revolutions*. Kuhn built an account with two components. One was the idea of “scientific revolutions”, pictured as events in which a new theory displaces an older one, as when Relativity was said to displace Newtonian Mechanics. The old and new theories were claimed to be “incommensurable” – one could, literally, not understand the concepts of the new theory in the terms of the old theory, and adopting a new theory was likened to a religious conversion. It is the conception of scientific revolutions for which Kuhn’s account is best known, but it really isn’t part of the present story.

Important here is the other component, that of “normal science”. On Kuhn’s account normal science, between scientific revolutions, works with what he variously called “paradigms” or “disciplinary matrices”. These are composed of the kind of problem to be solved and the tools for solving them: Laws, methods for applying them, experimental methods…. Paradigms can’t be learned by rote – students have to go through a long apprenticeship to develop the necessary skills. With hind sight we can see Kuhn’s paradigms as model building systems (Teller, 2008): The Newtonian paradigm is a network of skills in fashioning models based on Newton’s Laws. There are likewise paradigms for chemistry, cell biology, and so on. Kuhn’s “normal science”, with its focus on skilled problem solving, differed radically from the Positivist picture of science as deductions from Laws. But at the time Kuhn’s normal science was not seen as upsetting the Laws view of science. It did not take steam out of the entrenched physics fundamentalism.

By the third quarter of the 20th Century Positivism was falling out of favor, for some because of Kuhn, but for most because of increasing appreciation of inadequacies in Positivism’s empiricism. By this time it was also well appreciated that general relativity and quantum theories, the latter now developed into quantum field theory, were not going to be the last word in fundamental physics. According to general relativity space-time is curved wherever there is matter, but no one knows how to formulate a quantum field theory on curved space-time. And no one knows how to formulate a quantum version of general relativity. Nonetheless, physics fundamentalism, if anything, tightened its grip on much of Western intellectual thought.[[12]](#footnote-12) The role of building inexact models to describe mechanisms was now invisible, hidden in reductive definitions, not noticed in Kuhn’s “normal science”. Very broadly, scientists who worked on non-fundamental physics (physics other than relativity and quantum theory) went about their model building work, giving lip service to physics fundamentalism.

All this began, slowly, to change in the fourth quarter or the 20th Century. Claimed successes of reduction were found not to work. (Silberstein, 2002; 93-98) In 1972 A prominent physicist, Philip Anderson, challenged physics fundamentalism in his “More is Different” published in *Science*, the flagship journal of the American Association for the Advancement of Science. The 1980’s saw the threat of the Superconducting Supercollider, a proposed “ultimate” particle accelerator that proponents argued would enable physics to finally get fundamental physics of the microcosm exactly right. The problem was the cost – headed for 10 billion dollars and up. Physicists doing non-fundamental research, who otherwise gave lip service to physics fundamentalism, now saw their funding in jeopardy. Physics fundamentalism, innocuous as a philosophical doctrine, became a danger to most physicists by threatening funding for non-fundamental physics (Martin, 2015; Galison 2016, 16)

At the same time philosophy of science began the “rediscovery” of the model building side of science, most prominently in Cartwright’s 1983 *How the Laws of Physics Lie* and Giere’s (1988) *Explaining Science.* What had changed? In the century’s third quarter, a large fraction of philosophers of science had fundamental physics, the perceived ideal of science, exclusively in view. This they analyzed “from the armchair”, arguing from an image of how it had been assumed science ought to work, perhaps encouraged by some fundamental physicists themselves. When philosophers of science worked with science at all, it was with fundamental physics, with its claimed Laws, now thought, finally, to be right. This situation started to change in the 1980s and 90s with a blossoming of work in philosophy of biology, psychology, and, later, economics. These scholars worked with the science and scientists in the disciplines they were interpreting. They saw close up the central role of scientific model building.

In the 1980s and 1990’s the few scholars studying model building in science attracted very little attention, but the community steadily grew. Starting with Machemer, Darden, and Craver’s 2000 “Thinking about Mechanisms”, philosophers of biology began discussing the central role of describing mechanisms in biology. As I have been at pains to emphasize, if “mechanism” is understood sufficiently broadly, building models of mechanism lies at the heart of most of the work in science in all disciplines. Mechanisms have become an active subject of philosophical analysis. At the time of writing, 2021, thinking in terms of models has become, in my perception, pervasive in philosophy of science. And it is seeping into the rest of philosophy. I remember inwardly slapping myself on the head the first time I heard an analytic metaphysician, in a talk, refer to the work he was describing as a “model”.

**3. Taking stock**. It is time to summarize current understanding of models and their role in science. I need first to emphasize a consideration that I have so far left to one side. I have focused on models as imperfect accounts of mechanisms, and I have done so because for mechanisms it is easy to see the role of models. I have also wanted to call attention to the central role in science of discovering mechanisms, something that, outside of science, has been all but invisible.[[13]](#footnote-13) But it is not just mechanisms that get described with inexact models. It is also the most basic laws of fundamental physics! Schrödinger’s equation, laws from quantum field theory, the field equations of General Relativity are all laws, not Laws.[[14]](#footnote-14) So, contrary to “reading God’s Book of Nature”, it is *all* of science that works by fashioning inexact models, that is representations that are not both completely precise and completely accurate. For certain questions quantum theories and relativity give better predictions and better explanations than does Newtonian mechanics, but in absolute terms the new theories are every bit as false as Newton’s.

Science is a pluralist model building enterprise calling for different model building schemes for different problems, models that can paint significantly different pictures, but for the problems to which they are addressed do well with both prediction and explanation. So-called “natural laws” (Newton’s laws, Schrödinger’s equation…) don’t describe the world directly. For example, to apply the law f = ma to anything one must fill in the relevant forces. Such generalizations do not provide truths, rather they function as “model building principles” ( Giere, 1999; 84-96), basic templates that are used rather like basic dress-patterns that are filled in and modified as one adds detail. In physics this is done in stages. For example, just specifying that the relevant force is a restoring force proportional to a displacement, always an approximation, yields a more specific but still quite general model template, the “harmonic oscillator” formula, 𝜃 = 𝜃0cos(t/p) from figure 1 that applies not only to pendula but to springs, oscillating electronic circuits, and much else. Further details can be filled in step by step, for example applying the harmonic oscillator equation to pendula, then to a specific pendulum with a given length, that finally can be used to model some concrete physical system. At every stage the complexity of things forces one to make mathematical and many other simplifications – for example a restoring force is never *exactly* linear.

Much else in the physical sciences, while using general principles along the way, goes to work more directly describing mechanisms, which, remember, is to be understood as including processes. Geology fashions models of processes which result in earth quakes and movement of tectonic plates. Astrophysics describes the mechanisms and processes of star and galaxy formation and how stars produce radiant energy. Perhaps stretching the mechanism metaphor a bit far, cosmology describes the structure and history of the whole universe. Chemistry uses many principles, such as those describing the formation of ionic and covalent bonds, to describe processes of chemical reactions. A great deal in biology fits the mechanism metaphor extremely well – understanding the operation of those incredible gizmos, living organisms.

There are also vital parts of science that do not fit under the mechanisms metaphor, for example Lagrangian and Hamiltonian methods in physics and the functional relations described in macroeconomics. What is important here is that all of these cases also provide characterizations that have to simplify and streamline because their subject matter is too complex to get things exactly right. I am using the term “model” for any representation that is presented with the understanding that it is not completely accurate. So these other scientific representations that clearly fall outside the metaphor of describing mechanisms also qualify as models. My historical sketch and just rehearsed micro-survey support the conclusion that science is anything but a true (capital “L”) Law finding enterprise. It is an enterprise of fashioning always limited and never exactly accurate models.

To come here at the end to how all this bears on pluralism in science, the topic of this volume: When not completely accurate, models that have overlapping subject matter often can’t be neatly stitched together. There is no problem with “conjoining” – listing together - unrelated models, say a macroeconomic model of the functional relation between rates of inflation and interest rates with a hydrodynamic model of water waves. But things don’t go so well if we try to juxtapose the hydrodynamical description of water with a model of water’s thermal behavior given by statistical mechanics. The former describes water as a continuous medium, the latter as a collection of discrete particles. Water can’t be both.[[15]](#footnote-15) But there is no logical conflict because the first is understood as saying that water is *very like* a continuous medium in this way, the second that water is *very like* a collection of discrete particles in that way. So this is no “different kinds of truth” pluralism. The same sort of situation arises for the law of the lever and an account of the rigidity that it assumes, for laws of friction and the complex electromagnetic accounts of how frictional forces arise, and for many, many other such cases.

Issues about truth intertwine with issues about pluralism. It is often assumed, if tacitly, that scientific conclusions are TRUE. If they were, they could be juxtaposed to develop that growing mirror of nature. This issue is complicated by the consideration that inaccuracies can be smoothed over by vagueness. Is the conclusion that water is H2O true? I certainly won’t say that it is false. But it is highly imprecise. (Halsted, 2018 ,726-7: VandeWall (2007) When one works to refine this crude statement, simplifying idealizations start to creep in. Working in the other direction, inaccuracies can always be smoothed over by making statements imprecise (Teller, 2017; 149-153). To give an analogy, think of starting with a sharp but inaccurate picture. Now smudge the inaccuracies. The picture is no longer inaccurate but now is out of focus. Imprecision is inaccuracy by other means.

The tradeoff between inaccuracy and imprecision constitutes a qualification of the pluralism for which I am arguing: The tradeoff allows that one may be able to fit together different conclusions of science as long as inaccuracies are defanged with some vagueness or imprecision of statement. What we can’t have is an assembly of conclusions that are both completely accurate and completely precise. For every practical interest there is a level of precision that will suffice – excess precision is a practical vice. But intellectually we value as high a combination of precision and accuracy as we can muster. Sometimes, as in the case of hydrodynamics and statistical mechanics, this results in accounts of the highest intellectual value that when properly understood are not in logical conflict but can be consistently conjoined only by making their shortfall in precision explicit. This isn’t any deep metaphysical claim. Rather, it is the contingent claim that, as a matter of fact, the world is far too complicated in comparison with our modest human epistemic powers to achieve a rendering of nature that is both completely accurate and completely precise (Teller, 2018). At least not this side of any current intellectual horizon.

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1. A point almost universally neglected in the philosophy mechanisms literature. But see scattered comments in Glennan (2019). [↑](#footnote-ref-1)
2. The term “model” gets used somewhat differently by different authors. In my (2001) readers will find a more detailed presentation of the idea and its role in science. [↑](#footnote-ref-2)
3. I am here, and broadly in my exposition, giving a streamlined sketch of a vastly more complex history. This sketch serves to further introduce the relevant conceptions and to give a very rough idea of how some of these ideas evolved. [↑](#footnote-ref-3)
4. I would be happily surprised to find that any participants in the scientific revolution expressed this sort of thing explicitly. But it is arguably an implicit, deep seated attitude that guided many of them. [↑](#footnote-ref-4)
5. The term was used by Newton and is to be understood as “theoretical” or “mathematically described”. Throughout all of modern physics there is enormous variation in the extent to which theorists understood the principles as “mathematical only” or as representing material causes. [↑](#footnote-ref-5)
6. Lagrangian and Hamiltonian mechanics start with the concept of energy rather than force, sidestepping worries about force acting at a distance. [↑](#footnote-ref-6)
7. As when the “crests” and “troughs” of a wave process add up or cancel. [↑](#footnote-ref-7)
8. See (Heiman, 1971) for an account of how Hertz argued this in great detail. [↑](#footnote-ref-8)
9. The anomalous perehilion of Mercury, no ether wind, no “ultraviolet catastrophe”, the photoelectric effect, discreteness of atomic spectra, stability of electron orbits in the Rutherford atom, Compton scattering, problems with accounting for specific heat of substances at low temperatures, radioactivity…  
    [↑](#footnote-ref-9)
10. Quantum mechanics and both special and general relativity are time reversable, so the puzzle about the reversability of thermal physics remains to this day. [↑](#footnote-ref-10)
11. I’ve mentioned that Maxwell developed his theory of electromagnetism on the basis of extensive modeling very much in the mechanistic spirit. Heilbron (1982) documents that such mechanistic modeling was widely used and discussed by late 19th Century physicists. [↑](#footnote-ref-11)
12. For this grip on the quantum side of the physics community, see Galison, (1986, 44-48 and 2016, 15) [↑](#footnote-ref-12)
13. At a conference in 1996 a prominent physicist, working in non-fundamental physics, took me aside to inquire what philosophers of science were up to. I excitedly explained the burgening recent work on scientific modeling. I can summarize the physicist’s response, made with words much gentler than the ones I am about to use: You philosohers of science are just finding that out NOW? [↑](#footnote-ref-13)
14. Schrödinger’s equation is not relativistically correct. Laws from quantum field theory are based on a flat-space-time, but space-time is curved. The field equations of General Relativity do not mesh with quantum principles. The closest to a counterexample I have been offered are conserevation laws. But physical conservation laws arise from physical symmetries, and none of those symmetries are exact. [↑](#footnote-ref-14)
15. Morrison (2015, 178-187) sketches the same sort of situation for the many different models of nuclei. [↑](#footnote-ref-15)