Duhem before Breakfast[1]

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This essay will trace a certain argumentative thread within Pierre Duhem's philosophy of science that does not seem to have attracted much notice heretofore, yet helps greatly to motivate the anti-realist conclusions for which he is often cited. Most contemporary readings of Duhem trace his anti-realism largely to his celebrated articulation of what is now known as the "Quine-Duhem Thesis": the claim that any cherished hypothesis **H** can be protected against empirical disconfirmation through blaming some other hypothesis **H'** utilized in its problematic applications. Indeed, Duhem sometimes writes as if simple *modus tollens* was sufficient to establish this fact:

(H & H') e E
<u>-E</u>
-H v -H'

Yet, surely, this innocuous logical observation can't adequately support Duhem's quite sweeping claims that the choice of a physical ontology must represent, in the final analysis, a *matter of metaphysics rather than physics* proper. But it is precisely these strong philosophical contentions that inspire the modern anti-realists who frequently evoke Duhem for intellectual support.[2]

However, if we inspect, not <u>The Aim and Structure of Physical Theory</u>, but Duhem's prior and more technical work, <u>The Evolution of Mechanics</u>, we will discover that Duhem also believed he had established the far stronger thesis that is today entitled "the observational underdetermination of theory": the notion that two or more completely distinct theories can organize all possible observational evidence with equal adequacy. And Duhem believed that he could demonstrate this claim by a concrete example. He wrote:

> *Whatever may be the form of the mathematical laws to which experimental inference subjects physical phenomena, it is always permissible to pretend that these phenomena are the effects of motions,* perceptible or hidden*, subject to the dynamics of Lagrange.*[3]

Unfortunately, as in much of his writings, Duhem does not explain what he means by this remark as pellucidly as he might. The chief objective of this note is to supply a crisp reading for what he seems to have in mind.

It would be a matter of great interest for contemporary philosophy of science if this claim could be fully maintained. That distinct theories exist which are observationally equivalent in some strong sense is a doctrine to which many philosophers cling passionately but, arguably, represents a thesis for which no indisputably sound examples have yet been proposed. Most proposed cases of which I am aware either turn upon some misunderstanding of the relevant physics or involve factors that should seem innocuous to the average scientific realist. Many philosophers of the logical empiricist era--W.V. Quine representing a prime example--endorse the undetermination thesis in the absence of concrete illustrations largely because they believe that any collection of "observational consequences" can be supplemented in many ways by inequivalent extensions. But a “theory” is just such a logical extension, hence underdetermination follows. But many of us today feel that this logic-based defense of undetermination relies upon a totally unsuitable picture of how real life theories produce their "observational consequences." It would be far preferable if defenders of the claim could produce some genuinely convincing examples. Without attempting to evaluate the merits of other contenders here, I will simply state that Duhem's suggested pair of allegedly observationally theories represents as good an illustration of the expected behavior as I have encountered.[4]

We will find as we work through the details, however, that a key step in Duhem’s reasoning seems to rely upon a tacit assumption that a policy of *essential idealization* must be applied in setting up the equations for a continuous physical system property. In particular, Duhem presumed--and this assumption would have been widely shared in the physics community of his day (and by many within our own times as well)--that one must *intentionally misdescribe* matter’s qualities at a small size scale to permit the application of Newton’s laws and allied forms of mechanical principle. Smallish flexible or fluid elements within a continuous body must be falsely approached as if they were more rigid than they truly are. Such assumptions trace to some quite tricky problems concerning the specific kind of *physical infinitesimals* that are natural in mechanics. But let me stress that, in the phrase "physical infinitesimals", I have in mind some quite specific problems that arise when one sets up the equations for some continuously distributed system. These issues persist no matter how the *mathematical infinitesimals* native to the real line are treated, whether in the now standard Cauchy/Weierstrass manner, Robinson-style infinitesimals or some kindred point of view. The issues we must face are primarily ones of physics, not mathematics. Eventually, twentieth century research on continua decided that the problems posed by these “physical infinitesimals" must be approached in a considerably more sophisticated manner and that Victorian appeals to "essential idealization" represented a crude means for bypassing some rather complex methodological issues (these modern innovations were not driven by a desire to simply "clean up" the old work, but because no progress could be made with respect to more complex physical systems unless the old problems were more squarely addressed). In essence, by freely evoking "essential idealization" themes, physicists of Duhem's era had been employing *anti-realist philosophizing* to perform tasks that modern practitioners believe properly require *more sophisticated forms of applied mathematics*.

With the loss of the essential idealization thesis, Duhem's underdermination example collapses--his indistinguishable rivals become palpably distinguishable once again. Nonetheless, a thorough investigation of the entire enterprise reveals a lot about the history of how strong anti-realist themes crept into the philosophy of science (contrary to what one might expect, they were firmly formed long before the oddities of quantum mechanics came along). The purpose of this essay is to further our understanding of this historical genesis.

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To unpack both Duhem's underdetermination example and the rationale for essential idealization, we must review some of the competing views of matter that were actively debated during the classical era. Duhem himself hoped that what we now call "classical phenomena" could be organized in a fashion that regards matter as inherently *continuous media*--i.e., continuously distributed hunks of connected stuff. He also maintained that thermodynamic notions like *quantity of heat* and *internal energy* should enter physics as primitive notions coequal with mechanical concepts like *momentum* and *kinetic energy*. This *thermomechanical framework* should be sharply contrasted with the approach now offered in most modern texts on "classical mechanics" designed for physicists[5], which instead treat matter as composed, at its root, of swarms of isolated point masses bonded only through action at a distance forces. Here *heat* and *internal energy* are expected to derive statistically from more basic mechanical concepts. The point mass approach was originally suggested by S.J. Boscovich and was pursued, with varying degrees of loyalty, by the French "physical atomists" who worked near the beginnings of the nineteenth century. Strikingly enough, Duhem simply dismisses the Boscovichean approach as empirically unsound, for the standard late nineteenth century reasons (e.g., one obtains the wrong number of constants in Navier's equations for an elastic solid). When Duhem writes of "the effects of motions, *perceptible or hidden*, subject to the dynamics of Lagrange", he alludes to a third variety of foundational approach, quite distinct from Boscovich's, that Duhem believes can be rendered *observationally indistinguishable* from his own approach. The attempt to reorganize mechanics that Duhem has in mind is associated particularly with Heinrich Hertz but Duhem's discussion in fact tolerates a wider range of alternatives that might also qualify as "observationally indistinguishable" competitors. I'll explain the details of this somewhat forgotten approach in due course.

Before we do so, let us examine the problems of physical infinitesimals that motivate the thesis of essential idealization.[6] We simply need to examine some of the usual ways in which the equations for the simplest possible classical continuous system, a one-dimensional string or chain, get set up in the textbooks. And we don't need to return to the primers of Duhem's time to find the problematic expedients employed, for, with the notable exception of a rather sophisticated efforts directed towards sophisticated engineers, modern textbooks dealing with classical continua commonly replicate the old fashioned techniques (I consulted the well-known text of Morse and Feshbach[7], among others, when I framed the sample treatment below). For a more rigorous treatment, Stuart Antman's <u>Non-linear Problems of Elasticity</u> can be cited. With respect to the old-fashioned approaches, Antman comments:

> *most modern expositions...ask the reader to emulate the Red Queen by believing six impossible things before breakfast.*[8]

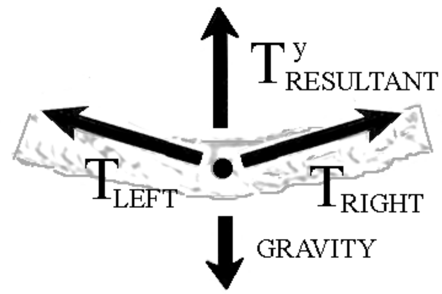
So let's inspect some of the "impossible things" we are commonly asked to accept in setting up the standard wave equation for a linear string.

Assume that we confront a hanging string that is pulled at its two boundaries by a tension T. For generality's sake, let's assume both that the string is moving and that a *variable* gravitational force pulls upon the string (we can either allow the string density or the applied field to be inhomogeneous). What equations should govern the string as a whole? Call this question *the global string problem*. Following the familiar motto that "physics is simpler in the small", we hope to resolve our global problem by first describing how the string looks "in the small." But how should "the small" of a continuously distributed string behave? Let's first assume in a naïve spirit that the "bodies" mentioned in Newton's laws of motion can be treated as *mathematical points*. In truth, it is rather hard to make sense of Newton's laws if "body" is not read as "point." For example, the Second law reads:

> *Change of motion in a body is proportional to the force applied, and*

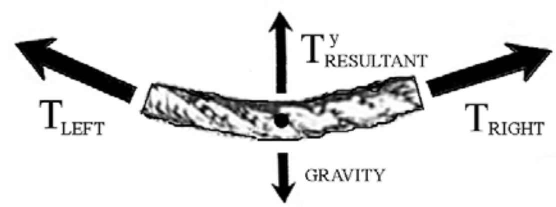
takes place in the direction of the straight line in which the force acts.

If Newton intended a "body" to constitute something spread out, then the description "the direction of the straight line in which the force acts" becomes ill-defined; a varying gravitational force might point in all sorts of directions across the breadth of the "body" and its sundry parts might respond by moving in sundry distinct directions (albeit rarely in straight lines since the "body" as a whole is welded together). So assume that we are looking at a localized "point" within our string. Let's first consider the situation when the "point" is at rest, so that its mass generates no inertial reaction against movement. The localized gravitational pull on our specimen "point" must be completely balanced by the tensions in the string if the string isn't to move. How does this equilibrium occur? Well, the tension vector that pulls the point on its left side, $\mathbf{T}_{left}$ must point along the string's tangent (otherwise the string would resist bending, which we postulate is not the case). Likewise, the tension to the right, $\mathbf{T}_{right}$, must behave similarly except that it pulls in an opposing direction. Let us now resolve these forces in the y-direction (= "up"), providing us with resolved components $\mathbf{T}^{y}_{left}$ and $\mathbf{T}^{y}_{right}$. Their vector sum $\mathbf{T}^{y}_{resultant}$ should represent the force balances the local downward gravitational pull $\rho(x)\mathbf{g}$ (where ρ is the mass density of our "point"). Plainly, the components $\mathbf{T}^{y}_{left}$ and $\mathbf{T}^{y}_{right}$ must differ in magnitude, for otherwise their resultant would vanish, If we now permit our "point" to move, then Newton's second law (or d'Alembert's principle) instructs us to add $\rho \partial^2 y/\partial t^2$ into our balance of "forces." Strictly speaking, this condition will only be applicable at the string's lowest point for elsewhere some of the gravitational pull will affect the endpoint tensions as well, leading to the celebrated difficulties of the catenary. To keep our discussion simply, I will assume that our string is light enough that we can ignore these effects (nothing material will hinge upon this simplification in what follows).



However, we are pretty clearly along to the road to incoherence: our "body" was supposed to be a *point*, but somehow we have assigned distinct y-components of a tension force $\mathbf{T}$ to its two "sides"? Since the left and right hand pulls on a point must be equal in magnitude and oppositely directed, doesn't the "resultant" of $\mathbf{T}^{y}_{left}$ and $\mathbf{T}^{y}_{right}$ need to be 0? How on earth does a *point* wind up possessing "sides" anyway?

Let us try a different approach. Suppose we stretch out our "body" so that it becomes "big enough" to possess distinct and clearly defined boundary points (we'll leave it undetermined for the moment whether this "bigger body" should be regarded as finite in length or not). We might observe that a wide range of terminology is used to denote the "stretched out" arena we are now considering: "element", "particle", "free body diagram", and so forth. Whatever we call it, we must credit our "element" with a well-defined exterior "boundary" because in continuum mechanics we must consider two kinds of "forces" that act in rather different manners. (1) There are "body" forces that pull upon the *inside* of a "body" such as gravitation and magnetism. Following d'Alembert's principle, we will also count the inertial acceleration $\rho \partial^2 y/\partial t^2$ as a "body force" because it acts inside the element, despite the fact that, strictly speaking, it shouldn't qualify as a true "force" at all. (2) However, there are also "surface" or "contact" forces that act along the element's *boundary*, such as our two endpoint tensions $\mathbf{T}_{\text{left}}$ and $\mathbf{T}_{\text{right}}$. Although commentators often overlook the point, the existence of these two distinct kinds of force create all sorts of foundational headaches for continuum mechanics that don't arise within Boscovichean "mass point" mechanics, simply because only "body forces" appear in the latter (for Boscovicheans, mass point surrogates for contact forces arise only as approximations to the actions of short range molecular attractions). In the case of our string, the notion of "boundary" degenerates into the consideration of its two endpoints, but in an "element" appropriate to a three-dimensional object, differing contact forces can push and pull everywhere across the complete surface that encloses the element's innards.[9]



Normally, if one stretches something that is long enough to possess distinct boundaries, its *insides* must enlarge as well. As long as we pretended that our string "body" is simply a dimensionless point, then the lack of room inside the "point" leaves no ambiguity as to where the applied gravitational force and the inertial reaction must act. But after our "element" gets enlarged enough to carry *distinct* contact tensions upon its two ends, it becomes quite unclear *where* inside the element gravitation and inertial reaction are supposed to apply. After all, we've set up our string so that the gravitational pull is variable--shouldn't we see that same variation across the breadth of our “element” as well? In other words, once we make our localized string "element" to be long enough to accept distinct end tensions, it should be long enough to accept a varying schedule of body forces within its innards, each acting in slightly different locales? Hso how will this rather complicated arrangement behave? A discouraging recognition now comes to mind: *hasn't our original "global string problem" simply replicated itself as our the "local string problem"*? Aren't we once again asking, "What happens inside a flexible stretch of string subject to variable forces when one pulls at its two ends?" How does "Nature is simpler in the small" assist us if we witness no substantive simplification in the problems we pose as we move inwards towards "the small"? We have turned a microscope upon a flea and discovered a hierarchy of smaller fleas biting each other *ad infinitum*.

Somehow we must find a way to render mechanical life inside our "element" simpler than it is globally. At this point, thoughts of the following sort were apt to strike a nineteenth century mechanist: "Gee, if we could only presume that our little string element represented a *rigid body*, then our difficulties could be resolved." The Edwardian hydrodynamicist Horace Lamb wrote with respect to a "chain"[10]:

> *The physical assumption which we make is that for equilibrium the forces acting upon an infinitesimal element must fulfil the same conditions as in the case of a rigid body.*[11]

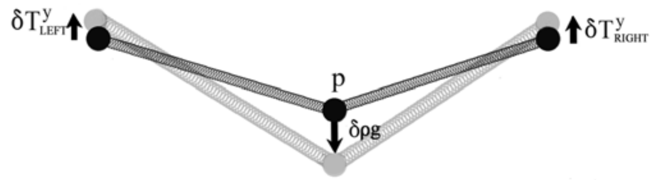
As we saw, in such an "element", a weight $\rho(x)\mathbf{g}$ pulls on the interior of the element while distinct tension forces pull on the left and right sides of the element (we'll assume that the right side sits higher in the gravitational field). A basic proposition of rigid body statics ("the three moment theorem") informs us that such an element can remain at rest only if the three applied forces continue to balance when *moved* to a common point. If we can permissibly evoke Lamb's "rigidification" principle, we can conclude that the two forces acting downward, viz. $\rho(x)\mathbf{g}$ and $\mathbf{T}^y{}_{left}$ must balance the upward pointing $\mathbf{T}^y{}_{right}$, which allows us to express the magnitude of the latter in terms of the two lower forces. Considering a moving string without any gravitational contribution, we can evoke d'Alembert's principle to argue in like fashion that the unknown "inertial reaction" $\rho\, \partial^2 y/\partial t^2$ must balance the y-components of the two endpoint tensions, i.e., $-T \sin \theta|_{left}$ and $T \sin \theta|_{right}$ respectively, where θ is the angle of the tangent to the x axis (we can assume that the *same* total tension must pull at both endpoints given that we have assumed that the gravitational contribution is small). And if $\theta|_{left}$ and $\theta|_{right}$ are relatively small, these magnitudes can be approximated as $-T\, \partial y/\partial x|_{left}$ and $T\, y/\partial x|_{right}$ respectively. Accordingly, as the size of our "element" shrinks towards zero, the sum of these two tension forces will approach $T\, \partial^2 y/\partial x^2$ *in the limit* and we can therefore regard this nonvanishing quantity as the proper "resultant" to balance the inertial reaction within an infinitely small portion of our string. In short, we obtain $T/\rho\, \partial^2y/\partial x^2 = \partial^2y/\partial t^2$, which is the standard "wave equation" governing the small vibrations of a light string. Arguing in this vein once again, we can add the gravitational contribution $\rho(x)\mathbf{g}$ for a “light” string.

What I've just sketched is a standard rationale for the one-dimensional string equation, more or less as it appears in many texts.[12] Clearly several "impossible things" have crept into our derivation. Why should we assume that our boundary points will move directly upwards, rather than sheering transversely? "We assume that the string is *inextensible*", goes the usual reply, but if that condition had literally applied, the string would have been unable to move from its rest configuration. In any case, how can we possibly decompose the common tension **T** into components on the left and right sides of our string element that differ in the y direction but *not* in the x direction? We'll consider how these "impossible things" can be disentangled from the fully legitimate considerations that are also registered within this rather muddled rationale for the usual string equation.

Observe that the basic justification for our procedures rests upon the assumption that, as one moves towards the small, the behavior of elements below some indefinitely determined size scale begin to act like *rigid bodies* or, at least, elements whose behavior can be fixed by a smaller (viz. *finite*) number of parameters (this is the weaker assumption Duhem prefers). Such claims are often called *rigidification* assumptions--at some size scale, even thoroughly flexible bodies can be approximated by rigidly "frozen" parts. A.E.H. Love, one of the great elasticians at the turn of the century, expresses a mass point version of "freezing":

> *The necessity for a simplification arises from the fact that, in general, all parts of a body have not the same motion, and the simplification we make is to consider the motion of so small a portion of a body that the differences between the motions of its parts are unimportant. How small the portion must be in order that this may be the case we cannot say beforehand, but we avoid the difficulty thus arising by regarding it as a geometrical point. We think then in the first case of the motion of a point.*[13]

In the context of Love's fuller text, it turns out that, although rigidified mass points can serve as adequate “elements” for simple continua, more complicated assemblies of linked rigid bodies may be needed. Even in our string case, it is sometimes common to find its basic "element" treated as three mass points linked together by "weightless strings." Such texts commonly evoke the venerable *principle of virtual work* to determine the “element”’s behavior, where an "virtual displacement" δy_p is introduced at the central mass point p pulling it slightly upwards or downwards. This little pull on p will induce corresponding endpoint displacements δy_{left} and δy_{right}, which will cause the endpoint tensions $\mathbf{T}^y{}_{left}$ and $\mathbf{T}^y{}_{right}$ to perform a certain degree of “virtual work”, measured by the product $\mathbf{F}\ \delta y$. Our principle declares that the sum of these "virtual works" must vanish near equilibrium, from which we can once again calculate the magnitudes of $\mathbf{T}^y{}_{left}$ and $\mathbf{T}^y{}_{right}$ from $\rho(x)\mathbf{g}$. Once this basic static balance of forces has been established, the usual procedure is to once again to “turn on” the inertial reaction $\rho M^2 y/Mt^2$ through appeal to d'Alembert's principle (I have sketched such a "virtual work" approach to mechanical “elements” largely because Duhem himself favors a treatment of this type, asi t most readily generalizes to a thermomechanical setting).



We might observe that if we simply replace the phrase "pair of weightless strings" in the previous paragraph by "pair of strong attractive forces acting at a nearby distance", the previous derivation can *look as* if it describes a setup that conforms perfectly to a Boscovichean ontology involving mass points and action-at-a-distance forces only.[14] In fact, treatments of continuum “elements” that decompose them into alleged “point masses” can be encountered in a fair number of textbooks as well. The seeming equivalence of all of these diverse treatments encourages the sentiment, prevalent within the late classical era, that ontological scruples don't much matter once one has moved inside the special preserve of a material "element."

For our purposes, the most salient feature of these procedures is that they make the physics of continuous media follow a rather strange *logic*, for it seems that we cannot set up the proper equations for a continuous body unless we *intentionally misdescribe* its flexible substance, at some small size scale, as more rigid (or mass point-like) than it really is. The only way we can arrest an otherwise fruitless descent into the labyrinth of the continuum is to move our discussion artificially into a *simpler landscape* in the “small” consisting of mass points or assemblies of rigid bodies. The nearly universal employment of this strange methodology in the Victorian era has caused many modern commentators to misunderstand the basic physical ontology they largely accept. Very rarely do authors of the period straightforwardly agree with Boscovich that, at root, everything is genuinely composed of mass points or even rigid bodies. Whatever the exact nature of molecules, spectroscopy had empirically established that were capable of jiggling in the manners characteristic of strings, bells and other classical continua--they did not stay rigidly mute.[15] Nonetheless, as we've just seen, it seemed as if continuous media can't be coherently *described* from a methodological point of view unless we first assign them *unrealistic* substructures consisting of rigid parts as mediating elements. For this reason, most late nineteenth century mechanics primers begin by articulating its "fundamental laws" in a form that directly applies only to mass points and/or rigid bodies. Nonetheless, we should *not* understand the fundamental "ontology" of that textbook's universe to be constituted by the apparent subjects of those opening laws.

One can quite a bit of "anti-realist" sentiment expressed by a wide range of Victorian physicists. Much of this, it seems to me, trace to the foundational difficulties I have just sketched. In particular, such thinking encourages a belief in the general thesis that I called *the unavoidability of essential idealization* earlier: physics cannot approach the physical world without first introducing some interceding layer of false--or "idealized"--misdescription. The statistician Karl Pearson, who began his scientific career as an elastician, includes the ensuing brief for scientific antirealism in his influential <u>The Grammar of Science:</u>

> *I feel quite sure that to assert the real existence in the world of phenomena of all the concepts by aid of which we describe phenomena--molecule, atom, prime-atom--even if* [they be admitted] ad infinitum, *will not save us from having to consider the moving thing [we utilize in our mathematical treatments] to be a geometrical ideal, from having to postulate* [a fictitious entity which] *is contrary to our perceptual experience* [of a continuous world].[16]

As such, "essential idealization" represents a radical philosophical thesis that should be sharply distinguished from the humdrum observation that physicists often study unrealistic models of phenomena *for practical convenience*--say, when we run molecular modelings involving only a hundred molecules or when we model a natural shape by a perfect circle. Clearly, the assumption that the strange "replace by rigid elements" methodology we have canvassed must be followed in dealing with continuous media will encourage belief in the essential idealization thesis, whereas a straightforward Boscovitchian needn't see any "idealization" involved in her version of classical physics beyond the humdrum simplifications we introduce for practical purposes. One may personally dislike mass points, but they don’t qualify as “idealized” from a Boscovichian point of view. However, once we decide that a successful continuum physics must appeal to essential idealization to get its enterprises under way, we have good reason to doubt that science attempts to paint a *straightforwardly* realistic portrait of reality, for how can we explain the rigidified “fictions” that keep intervening in the stories we weave? Worse yet, the variety of “idealized element” stories we have canvassed seem rather indifferent to whether its innards string should be comprised of points, rigid bodies or other simplified arrangements. Accordingly, while a true Boscovichian needn't be tempted into anti-realism as a scientific philosophy, the Victorian advocate of continuous media were likely to find such a point of view methodologically unavoidable.

The role that *essential* (as opposed to “practical”) idealization plays in this line of thought is crucial: mere “idealization” for convenience’s sake needn’t force one into any kind of radical philosophy at all. Unfortunately, these simple distinctions in categories of “idealization” are often entirely muddled within present day philosophical discussion. Too many contemporary writers are content simply to peek into a physics textbook and exclaim, "Ah, ha!; perfect circles and frictionless planes--*ergo* the Victorians were right to declare that the aim of physics is not to describe reality." But it is impossible to conduct a sensible discussion of "scientific realism" unless one better distinguishes the precise vein of “idealization” one has in mind.

Fortunately for scientific realists, twentieth century investigations into continuum physics have demonstrated that the physics of such media can--and, moreover, must!--be set up without passing through any strange intermediate stage of rigidifying idealization. I’ll return to the morals we might extract from this discovery later in the essay.

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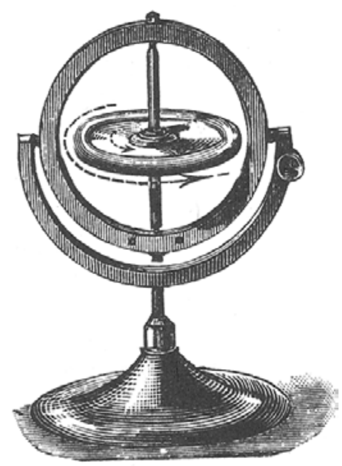
After these lengthy “physical infinitesimal” preliminaries, let's return to Duhem’s suggested example of distinct theories that seem equally compatible with exactly the same evidence. As we noted in passing, Duhem's own approach to continuous media required simplified “elements” but he allowed these to manifest other localized qualities than the traditional *mechanical* choices we utilized above. Specifically, in his own version of element “virtual work” he tolerated primitive "conjugate pairs" of quantities that represent inherently *thermodynamic* forms of "work", such as $T\ \delta S$ (where T is temperature and S is entropy) or $\mu\ \delta n$ (where μ is a chemical potential for some compound and n is its concentration).[17] A "mechanist," in contrast, will attempt to get by with just the variations familiar from mechanical tradition--**F** δy (as witnessed above in the string example) and **T** $\delta\theta$ (where **T** is a torque and θ a rotation). In other words, an element "free body diagram" for Duhem is allowed to be more "abstractly specified" (his term) in its behaviors than strict mechanists permit. Despite this enlarged, "thermomechanical" toleration of a wider range of "element" behaviors, Duhem must still appeal to essential idealization to get his continuum physics under way. That is, while he believes that any portion of a macroscopic continuous material will display infinitely many degrees of freedom, the adjustments possible within a Duhemian "element" must be restricted to a finite set (in engineering jargon, we must "lump" the distributed variables displayed at the macroscopic level to frame a finite set of parameters within the "element"). This continued assumption of essential idealization will prove critical in what follows.

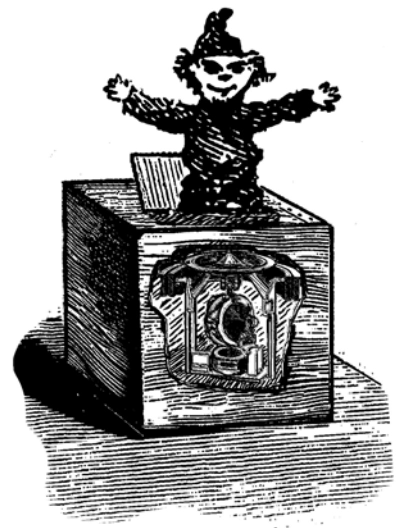
It is at this stage that Hertz' peculiar form of mechanics enters the picture as a potential "observationally equivalent" competitor. Following observations made earlier by Routh, Helmholtz and J. Thompson, Hertz studied the effects on a mechanical system if some subset of its quantities turned out to be *cyclic*--that is, their spatial positions do not effect the total energy of the complete system. An example: consider the spinning ring within a gyroscope. As soon as one "particle" within the ring rotates out of its present position, its place is immediately filled by some identical neighbor. For all intents and purposes, the overall behavior of the gyroscope as a whole will prove utterly indifferent to the *positions* of the ring's particles, although the gyroscope's overall movements will be drastically influenced by the angular *velocity* whereby the ring elements displace one another. Hence the formal definition of a "cyclic variable": a quantity whose *velocities* but not *positions* appear in a Lagrangian suitable for the composite system. If we are unable to observe the insides of our gyroscope directly--say, the gizmo comes encased within an opaque box--, the internal whirling will seem to supply a *hidden source of potential energy* that makes the box harder to move than it would otherwise prove. Without opening the box, we can't tell whether its resistence to movement is caused by some novel external force field or simply due to its hidden gyroscopic whirling. In other words, the



cyclic coordinate

unseen "hidden motions" of the gyroscope's cyclic quantities display a remarkable capacity to *imitate* the behavior of an externalized force potential.

And we can generalize this observation further. Let us set up some puppet on the exterior of our box that is mechanically linked to the gyroscope inside. We find it hard to move the puppet. Is this because a resisting *force* has been created inside the box (from a stretched spring, say) or merely because we have mechanically sped up the gyroscope's cyclic motion? Again, without opening the box, we can't tell--the two effects are perfect mimics of one another.



Behind these observations lie a number of general theorems on cyclic coordinates that Hertz exploited in setting up his mechanics. He first argued that all standard Newtonian "action at a distance" forces[18] can be adequately imitated by positing suitable hidden cyclic elements. The advantage of this reduction is that one might then maintain that any "active force" one observes is really the result of a change in how various hidden mechanical elements presently whirl (historically, Descartes also argued that all surface "potential" effects represent the products of hidden kinetic notions). As a result, the only "forces" that basic mechanics need consider are the entirely "reactive" contact forces associated with *geometrical constraints* in traditional mechanical parlance (that is, the "forces" that keep mechanical pieces linked together). Often Hertz' program is loosely described as "doing without forces"--the strategy is better described as one of "making do with only constraint forces."

From Duhem's richer point of view, such techniques pose a considerable challenge: isn't it possible that any of Duhem's *intrinsically thermodynamic* forms of potential (say, the chemical potential μ) might be *imitated* at a smaller size scale by *setting up hidden elements at a level smaller* than where Duhem has chosen to "freeze" his element level of description? Can't Hertz fairly boast to Duhem: "Anything you can do, I can do *smaller*"? Or, to quote Duhem's own words once again, isn't "it ... always permissible to pretend that these phenomena are the effects of motions, *perceptible or hidden*, subject to the dynamics of Lagrange?" But the stories Duhem and Hertz tell of how continuous bodies are composed appear completelt different ontologically: in the former's case, we find temperature and chemical activity arising at every size level, whereas in Hertz' world, everything devolves into atomic gyroscopes at some minute scale length. Unless we can open up the box, how can we tell which view is correct? On a macroscopic level, each setup imitates the other perfectly.

Well, to a hard bitten scientific realist such considerations alone won't seem completely upsetting: it is easy to supply historical cases of competing theories that can't be distinguished as long as suitable technology is lacking, e.g., an accelerator capable of shooting suitable probes into the innards of our materials. After a point, such probes will bounce back quite differently from a set of Hertzian molecules than from a Duhemian continuum.[19] Here, as I see it, is where a tacit assumption of "essential idealization" enters the picture. We can now rephrase the Hertz versus Duhem debate as a question of "How should fundamental mechanics select its basic 'elements'--should they be restricted to Hertz's limited palette of quantities or decked out in Duhem's richer array?" Once again, Hertz can crow to Duhem, "any 'element' of yours I can model smaller." But now there is no longer any question of resolving this new dispute by empirical probes; the need for essential idealization at the "element" level provides endless lower chambers in which Hertz and Duhem can permanently hide from any conceivable form of empirical disconfirmation.

But, as long as these divergent alternatives remain equally open, the contours of future research into continuous media will be greatly affected by the choice one selects. The Duhemian is content to frame her modelings in rich, thermomechanical colors whereas the Hertzian mechanist will spend much time constructing dour gyroscopic imitators of the Duhemian successes at a lower size scale. Duhemians regard the latter activities as a distracting waste of time, but "observational equivalence" of the two approaches shows that they cannot produce irrevocably *empirical grounds* for discouraging the Hertzians. This line of thought, in my opinion, captures some of the deeper reasons why Duhem believed that "taste" in the fundamentals of a physical theory ultimately represents a matter of "metaphysics" rather than empirics.

I should like to stress that Duhem's discussion in The Evolution of Mechanics of mechanist competitors such as Hertz ' program do not explicitly evoke the funny role that "essential idealizations" play within most nineteenth century approaches to continua. The conflict is presented simply as a dispute between those scientists who favor modeling a material simply with a *finite* number of thermodynamic parameters and those who prefer to seek a large (but finite) number of hidden parts, characterized entirely by mechanical qualities. The thesis of "essential idealization" was simply incorporated in their everyday mathematical manipulation of "infinitesimals" and did not warrant explicit mention, except in exceptional circumstances such as Pearson's The Grammar of

Science. Nonetheless, I think its tacit presence helps explain why a figure like Duhem would have found the Hertz/thermomechanics debate irresolvable by empirical means.

There are some additional considerations that help explain Duhem's point of view better. Anyone who works in the thermomechanics tradition recognizes that the "models" she builds are likely to only encode only *averaged material qualities* in her "element" choices. Given his researches, Duhem was very much aware that the macroscopic properties of a material like steel are deeply affected by the way in which its different component phases form into *grains*--from his point of view, a block of steel represents a complex conglomerate of smaller portions of purer forms of grain-sized continuous material. Nonetheless, metallurgists have set up "averaged" equations for steels that are quite satisfactory for most macroscopic practical purposes. In fact, the infinitesimal "elements" for these equations are nicely simplified because a steel acts nicely isotropic at a large scale (because the different orientations of the individual grain cancel out one another). To be sure, for other purposes, our metallurgists will need to consider the steel in its fully grained glory, where each individual grain will require a different choice of infinitesimal "element" to capture its local (and, generally, non-isotropic) behaviors. And Duhem would have also had good reason to suspect that even individual grains will eventually require their own granular treatments under a certain regimes of probing. Therefore the philosophy of the thermomechanical modeler is always to adopt the simplest infinitesimal "element" sufficient for the practical questions at hand.

If so, why should Duhem strongly recommend that, nonetheless, at every level of analysis, the generous thermomechanical configuration of qualities will prove the best frame to adopt? In fact, he was quite aware of the deep considerations about the behavior of materials that require thermodynamic notions like entropy to resolve the evolving behavior of shock waves as they form within a hammered steel block (Duhem was a specialist in this subject, inter alia).[20] In other words, most Hertzian mediums, considered at scale size A, must eventually appeal to entropy et al. to resolve their evolving behaviors *at that size scale*. The only way that a true-hearted mechanist can preserve her favored point of view is to claim that, at some much smaller scale B, "hidden motions" account for entropy's apparent handiwork at scale size A. In my opinion, few accounts of the "anti-atomist" debates of the late nineteenth century take account of this important "mechanical quantities alone cannot serve as an adequately closed basis for mechanics" theme. Some of the reason this theme has not been adequately noticed is that its contours have been obscured by the "essential idealization" difficulties canvassed here.

(iv)

Fortunately for scientific realism's sake, modern studies have shown that the presumption that a level of idealization is required for classical continua simply represents a mistake that arises from nineteenth century attempts to perform distinct mathematical chores at the same time. Stuart Antman remarks:

> *In the early 1950's,* [Clifford] *Truesdell began a critical examination of the foundations of continuum thermomechanics in which the roles of geometry, fundamental law, and constitutive hypotheses were clarified and separated from the unsystematic approximation then and still prevalent in parts of the subject.*[21]

What this means in our string example is the following. When we assumed rigidification within our "element", we were simultaneously attempting to (1) define local quantities suitable for describing any continua; (2) articulate plausible constitutive relationships for those local quantities; (3) approximating the relationships given (2) through appeal to known macroscopic knowledge of the material's apparent symmetries (or near symmetries). With respect to task (1), we used the rigidification to both justify and locate a non-zero "resultant force" $\mathbf{T}^{y}_{resultant}$ stemming from the endpoint tensions that could be regarded as acting *inside the "element" at the same very same string point* as the body forces and the inertial reaction. In a modern treatment, one argues, through appeal to basic balance equations formulated as integral equations over extended bodies (not *points*), that objects like $\mathbf{T}^{y}_{resultant}$ generally (but not always) exist locally, although this "local object" turns out to not really be a *force*, but something *sui generis*, viz. a *stress tensor* (the conceptual differences get masked in the case through the drop in dimensionality from three to one[22]). The argument for this tensor stems from a fairly straightforward limit argument--indeed, one that was originally suggested by Cauchy himself--which does not require any appeal to infinitesimals (although, of course, Cauchy's reasoning can be replicated within a nonstandard analysis framework as well). Likewise, purely geometrical considerations dictate that local *geometrical* qualities like the string curvature M^2y/Mx^2 generally exist, although, once again, these properly represent forms of *strain tensor*. But, at this stage, we should not rush to connect $\mathbf{T}^{y}_{resultant}$ up with T M^2y/Mx^2 through the somewhat hokey geometrical arguments that employed in the stock setups we surveyed above. Instead, we must describe, *in the form of a new "constitutive equation" law*, the manner in which force-related *stress* relates to geometry-related *strain* in the metal comprising the string. "Hooke's law" is best known of these "constitutive equation" hypotheses and serves to ground our string derivation in a better way. quite adequately. In fact, the main reason that engineers needed to disentangle such constitutive assumptions from the geometrical reasonings listed under (1) is they couldn't work their ways to reasonable setups for complex materials like rubbers and paints otherwise. The very fact that most textbook derivations of the string equation such as we surveyed act as if everything follows from the geometry of the situation alone and that no constitutive assumptions are needed represents a sure symptom that matters have gone conceptually astray. In any case, utilizing the recommended modern docket of ingredients, we can write down a suitable equation to govern our string without requiring any belief in "impossible things" along the way. The hope that "physics will simplify in the small" becomes neatly realized in this alternative program through the employment of straightforward limiting arguments and no intermediate stage of essential idealization is required.

Although we have now managed to get to the breakfast table without needing to swallow impossibilities, we are likley to find some rather formidable nonlinear equations sitting on our plate-- equations that, even today, are very difficult to handle (if we ever become inclined to patronize the Victorians for their sloppy derivational practices, we should remember that, without their ameliorating assumptions, a rather nasty equation directly emerges as the "simplest" possible form of classical continuum description). To obtain a more tractable mathematics, we must typically find ways to exploit our prior knowledge of how vibrating strings behave to uncover *approximation recipes* that produce simpler equations (like the linearized wave equation we studied) that roughly imitate string behavior to a reasonable degree. In the modern approach, these stage (3) assumptions come after a more exact but intractable equation has been derived, so that the accuracy of our approximating substitute can be appropriately gauged. It is only at this stage that we are allowed to exploit the "unsystematic assumptions" of days gone by: that that the points on a string mainly wiggle in the y-direction; that they do not make a large slope with the x-axis and so forth. As we saw, in the old derivations, such approximations get made from the outset and can't be easily disentangled from moves appropriate to stages (1) and (2). Utilizing these approximating assumptions wisely, we can struggle along a more or less plausible path that leads back to our old friend, $T/\rho\ \partial^2 y/\partial x^2 = \partial^2 y/\partial t^2$, although, in truth, a rigorous mathematical justification for all of the corners we need to trim remains a somewhat elusive manner.[23] From this point of view, the Victorians (and their modern imitators) erred in regularly blending sound physical principle with rather uncontrolled approximation techniques. Of course, given the derivational ordeals that a more virtuous path requires, it is perfectly understandable why they would have believed that essential idealization represented a basic condition of scientific life.

But the true lesson of this history is that sometimes greater patience is wanted. In the fullness of time the coherent rationale that lies tangled up behind the surface veneer of a strange physical derivation often requires a good deal of careful investigation before its contours become evident ("Logic is eternal, Heaviside advises, "it can wait"). In retrospect, the thesis of essential idealization simply served as a convenient crutch that allowed the Victorians to stumble past some rather ferocious conceptual obstacles. That fact may justify the principle as an expedient, but not as a sound philosophical stance.

(v)

To summarize: (1) In the late classical period, anti-realism and its cousin, the thesis of essential idealization, emerged from the understandable methodological difficulties that arise when physicists attempted to apply the tools of prior mechanical tradition directly to continua. (2) As such, these two philosophical doctrines served the practical purpose of *rationalizing mathematical transitions* that, in hindsight, are better treated by segregating approximation techniques cleanly from fundamental physical principle. (3) Many allied doctrines commonally defended within philosophy of science today--we specifically considered the observational underdetermination of theories-- were also originally motivated by this same forgotten problematic. (4) The apparent need to handle physical infinitesimals in an essential idealization manner led thinkers like Duhem to conclude that many distinct approaches to mechanics could never be distinguished on empirical grounds. It was this observation that seems to have inspired his version of the famous Quine-Duhem thesis.

As noted earlier, many contemporary anti-realists still cite Duhem's <u>The Aim and Structure of Physical Theory</u> in support of their own favored themes. The largely forgotten history surveyed in this essay indicates that, in a very real sense, modern philosophy remains haunted by the ghosts of departed infinitesimals.

Notes:

[1] This essay was composed a 2001 Central A.P.A. symposium on physics' interactions with philosophy which also featured Michael Friedman and Mathias Frisch. I'd like to thank them both as well as Michael Liston for helpful comments. (Note added June, 2007).

[2] E.g., Nancy Cartwright and Bas van Fraassen.

[3] Pierre Duhem, The Evolution of Mechanics, J.M. Cole, trans. (Berlin: Springer, 2001), p. 78.

[4] For my own thinking on these topics, see "The Observational Uniqueness of Some Theories" Journal of Philosophy 77 (1980) and "The Double Standard in Ontology" Phil. Studies 39 (1981).

[5] Continued interest in continuum mechanics has become (except for a few special cases) the dominion of engineers and applied mathematicians, rather than physics departments per se.

[6] The general problematic sketched in this section is discussed in greater detail in my "Sympathy for Mechanists," to be posted in Arch Phil Sci.

[7] P.M. Morse and Herman Feshback, Methods of Theoretical Physics I (New York: McGraw-Hill, 1953).

[8] Stuart Antman, Nonlinear Problems of Elasticity (New York: Springer-Verlag, 1995), pp. 11-2.

[9] Incidently, standard derivations rarely explain why we don't need to assign "top" and "bottom" boundaries to our string body beyond their endpoints. Such issues prove deeply entangled with tricky questions of when we have any right to adjust the spatial dimensionality presumed in Newton's laws from three to lower values.

[10] In textbook jargon, a "chain" is comprised of one-dimensional elements where the gravitational body forces are non-negligible, whereas these can be neglected within a "string." In neither case does any resistence to bending arise.

[11] Statics (London: Cambridge University Press, 1933), p. 182. Lamb immediately extends the "rigidification" principle to hold for finite lengths as well: "As a necessary consequence, the ordinary conditions of equilibrium will be satisfied for any finite portion of the string." Here Lamb is clearly following the manner in which William Thompson (Lord Kelvin) and P.G. Tait introduce "rigidification" in §564 of their Treatise on Natural Philosophy, Vol. II, (New York: Dover, 1962) (retitled as Principles of Mechanics and Dynamics). For another exemplar, see Louis Brand, Vectorial Mechanics (New York: John Wiley And Sons, 1930), pp. 187-90. See the excellent survey of "The Principle of Rigidification" by James Casey, "The Principle of Rigidification," Arch Hist Sci 43 (1992). Casey restricts his attention to the manner in which Thompson and Tait's appeal to "rigidification" manages to regain the effect of standard requirements of balance of momentum and moment of momentum; in my view, some "justification" of "rigid body" disguised approximation techniques is also intended.

[12] In fact, there are a large number of variations possible, some of which evoke Taylor series expansions in the place of rigidification, although the exact rationale for these procedures are commonly left dubious.

[13] Theoretical Mechanics (Cambridge: Cambridge University Press, 1906), p. 2. Love is a good example of an author who seems to espouse a Boscovichean point of view, but also allows his "particles" to lie in contact with "reactive stresses" between them, doctrines that are not consistent with a strict mass point of view (cf. pp. 347-352). Love's later A Treatise on the Mathematical Theory of Elasticity (New York: Dover, 1944) is much clearer on fundamentals and he admits, "The hypothesis of material points and central points does not now hold the field" (p. 14). His "Note B" (pp. 616-27), however, suggests a lingering personal nostalgia for mass points. His "Historical Introduction" provides an admirable précis of nineteenth century investigations.

[14] Such physicists have been fooled by what I elsewhere describe as a *semantic mimicry*. See my Wandering Significance (Oxford: Oxford University Press, 2006), pp. 656-7 for a discussion of how Ludwig Boltzmann was misled by considerations of this sort.

[15] See J.C. Maxwell’s beautiful argument to this effect: “Atom” in his Collected Scientific Papers (New York: Dover, 1952).

[16] The Grammar of Science (London: Thoemmes Continuum, 1992), p. 298. Pearson's thesis, although it is hazily expressed, is that one must always expect to begin with mass points in a mathematical treatment of nature, although, as more precise experimental information is gathered, the size scale at which these "ideal elements" will be introduced will require readjustment to ever lower levels (from *atom* to *"prime atom"* and beyond). As we'll see, Duhem agrees with Pearson's basic thesis, with the caveat that one will never get the number of elastic constants right if only mass points are utilized in the modeling (Pearson, judging by his comments in History of Elasticity, never accepted the empirical disconfirmation of Navier's "rari-constant" approach to the subject.

[17] Pierre Duhem, Mixture and Chemical Combination (Dordrecht: Kluwer, 2002) and Thermodynamics and Chemistry (New York: John Wiley, 1913).

[18] He assumes that these will be always derivable from potentials, as will be provable under a suitable reading of the Third Law in a point mass framework. It should be added that Hertz is frequently nebulous about critical details in the program. Since Duhem doesn't seem to take exception to these, we shall not discuss those factors here.

[19] Those contemporary authors who defend the underdetermination thesis often cite theory choices that depend critically on what happens at "far away distances." Such theories often recast the *local* descriptions provided in some starting theory T by claiming that some hypothetical range of far away matter, electromagnetic absorber or whatever acts as a competing cause for the local behaviors. Such examples generally seem unpersuasive to the stout-hearted realist who asks, "Why don't we simply probe the far away regions to see if your funny stuff is out there?" The fact that we can't now (and, in some cases, ever) run such experiments is not viewed as dispositive. Duhem's example, as I have reconstructed it here, is based instead upon a descent towards the indefinitely small in a fashion that permits a cross-fertilization with the essential idealization theme. This combination then blocks the standard realist response to "far away distance" constructions in a interesting manner (which is not to say that I find Duhem's considerations persuasive, for the reasons I will canvass in section (iv)). In a recent note, I have suggested that Kant may have toyed with allied themes in The Metaphysical Foundations of Natural Science. See my "Back to Back to Kant" forthcoming in Michael Dickson and Mary Domski, ed., Synthesis and the Growth or Knowledge.

[20] For a discussion, see Wandering Significance, p. 364.

[21] Antman, Elasticity, p. viii.

[22] Strictly speaking, rationalizing the dimensionality drop should be added to list of tasks to disentangle. See Diarmuid Ó Mathúna, Mechanics, Boundary Layers and Function Spaces (Boston: Birkhaüser, 1989).

[23] For a beautiful exposition of these matters, see Stuart Antman, "The Equations for the Large Vibrations of Strings" Amer. Math. Monthly 87 (1980). .