What is “Classical Mechanics” Anyway?

The fundamental law \[ F = ma \] is a blank form which acquires a concrete content only when the conception of force occurring in it is filled in by physics--- Hermann Weyl

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

Matters have been rendered more confusing by the fact that a conceptually simple surrogate for classical doctrine is readily available, even though its formally articulated doctrines skirt most of the tricky conceptual problems encountered within classical tradition. The tenets of this simple theory comprise the themes that we shall investigate under the heading of “point mass mechanics.” Within this approach the term “point mass” designates an isolated, 0-dimensional point that carries concentrated mass, mass point lattice connected rigid parts flexible beam
charge, etc. In contrast, there are two other sorts of “fundamental object” with which a “classical mechanics” can be potentially concerned: *rigid bodies*, understood as extended solids whose points never alter their relative distances to one another and *flexible bodies* such as fluids or solids that are completely malleable at every size scale. Commonly, the latter are also called *continua*, a practice we shall adopt here. Of course, any of these entities can be joined together in larger combinations, as when individual rods are assembled into a *mechanism* or one flexible body is embedded within another as a *composite* (e.g., a jelly doughnut).²

Mathematicians commonly label our continua as “fields” due to their distributed character (they require more complex forms of differential equations than their point mass and rigid body cousins). We will generally avoid the “field” terminology because constructions such as Maxwell’s electromagnetic field raise rather different conceptual issues than will be canvassed here (although indubitably part of “classical physics,” modern ideas about electromagnetism rarely operate in perfect harmony with the various views of matter we shall survey³).

In the sequel, I shall employ the phrase “*material point*” to designate a 0-dimension region within a continuously distributed body (either in its interior or along some bounding surface). In contrast to our point masses, material points are connected with one another quite densely and (usually) do not carry finite values of mass or impressed force (they, instead, only display mass and charge *densities* that sum to genuine masses and densities over regions of an adequate measure). The phrase “*analytic mechanics*” will often serve as a generic title for the sundry formalisms that deal with connected systems of rigid bodies.

As just noted, the “conceptually simple surrogate” for classical doctrine that most commonly dominates philosophical discussions of “Newtonian mechanics” comprises a set of prescriptions that make coherent sense only with respect to isolated *point masses* that never come into contact with one another. We shall discuss the specific features of these doctrines in section (iv). From a “point mass” perch, any appeal to rigid bodies or continua merely represents a convenient means of discussing large swarms of point masses held together through cohesive bonding at short scale lengths. Although modern undergraduate primers often encourage this viewpoint (partially due to its conceptual simplicity), we shall see that it cannot serve as an adequate bedrock for many of the established successes of classical physics technique and, worse yet, suppresses many of the difficult conceptual issues that have historically plagued the subject.

The deceptive simplicity available to the point mass approach traces largely to the fact that, within its frame, matter can exist only in the form of isolated
singularities, thereby sidestepping the substantial mathematical concerns that arise when extended objects come in contact with one another (on rare occasions, point masses can collide with one another, but these contacts only occur at fleeting moments that can usually be handled through appeal to conservation principles). As a result, point masses act upon one another only through action-at-a-distance forces\textsuperscript{4}, but higher dimensional objects require direct contact forces as well. As we’ll learn, getting action-at-a-distance forces and contact forces to work in tandem is a non-trivial affair, but it becomes a conceptual obligation that vanishes from view if we are allowed to restrict our “fundamental ontology” to point masses alone (modern texts commonly introduce their “fundamental tenets” in an exclusively “point mass” vein but start discussing rigid bodies and continua soon thereafter without much justificatory explanation). Historically, few of the great practitioners of classical mechanics would have genuinely accepted the point mass viewpoint as descriptively adequate and we won’t be able to appreciate the great philosophical struggles over the “nature of matter” within the classical era if we carelessly assume that the usual point mass formalism adequately captures the “essential core of classical physics.”

However, there are a wide range of subtle reasons it can easily look as if a specific classical author embraces the point mass viewpoint. As we’ll observe in (iv), Newton’s celebrated laws of motion are difficult to parse coherently unless terms like “body” are interpreted in a punctiform manner. A host of significant mathematical complexities attach to the notion of “material point” as it appears within continuum physics (i.e., as a point-sized region within a continuous body) and these are sometimes bypassed through the simple expedient of confusing embedded continuum points with the simple isolated singularities of the point mass treatment. We shall survey several of these shifts in the pages to follow. From a formal point of view, it is important to distinguish between the ordinary differential equations (ODEs) pertinent to point masses and analytic mechanics and the trickier partial differential equations (PDEs) required in continuum modeling.\textsuperscript{5}

The fact that the real world proves quantum mechanical within its small scale behaviors occasions confusion as well. Although particles like electrons appear to be “point-like” in their scattering behaviors, they also “fill” larger effective volumes courtesy of the uncertainty relations. In many cases, one obtains the requisite Schrödinger equation for a system of particles (which is a PDE describing a field spread out within a high dimensional space) by “quantizing” a parallel set of ODEs for a classical point mass system.\textsuperscript{6} But this mathematical linkage doesn’t entail that Nature behaves much like any classical point mass system at a small size scale.
Quite the contrary, constructing a classical system that can approximate the "effective volumes" of quantum clouds accurately at the size scale of so-called "molecular modeling" often requires classical blobs of extended size and flexibility. Most scientists working in the final epoch (= the late nineteenth century) when classical mechanics could plausibly claim to govern the world in its entirety rejected the point mass viewpoint as empirically inadequate to the blob-like characteristics of real life atoms and molecules (we’ll survey a few of the salient considerations later). Nonetheless, the convenient mathematical associations between the ODES for classical point mass models and the Schrödinger equation have encouraged a situation where most modern "classical physics" textbooks written for physics students emphasize point mass and analytical mechanics foundations strongly, because their chief pedagogical objective is to get to quantum mechanics quickly, not to develop classical mechanics as a useful discipline in its own right or in a manner that can readily accommodate most of the "classical physics" covered within a standard nineteenth century primer. One can usually find better "foundations" for continuum mechanics only in the literature directed towards materials science and allied forms of "engineering." Self-styled "physicists" of today are usually unfamiliar with this material, although it comprised the chief diet of their nineteenth century predecessors.

Scholars hoping to extract methodological "morals" from the struggles over "matter," "atoms" and "force" that occurred towards the end of the classical era will be misled if they only learn the "Newtonian physics" provided within a conventional "modern physics" curriculum, for the point mass formalism misses the conceptual complexities at the heart of the historical disputes. This essay will sketch some of the missing background and explain the curious factors that have impeded its modern day appreciation.

With respect to current textbook practice, one should never dismiss expedient policies of pedagogy lightly, for students can be lifted to higher levels of achievement more swiftly if an instructor does not dawdle excessively in niceties. But cutting intellectual corners early can prove worrisome later, as Horace Lamb remarks in a popular early twentieth century textbook:

*To a student still at the threshold of the subject the most important thing is that he should acquire as rapidly as possible a system of rules that he can apply without hesitation, and, so far as his mathematical powers will allow, with success, to any dynamical question in which he will be interested.* From
this point of view it is legitimate, in expounding the subject, to take advantage of whatever prepossessions he may have as are serviceable, whilst warning him against others which may be misleading. This is the course which [is] attempted ... in most elementary accounts of the subject. But if at a later stage the student, casting his glance backwards, proceeds to analyze more closely the fundamental principles as they have been delivered to him, he may become aware that there is something unsatisfactory about them from a formal, and even from a logical, standpoint... If the student’s intellectual history follows the normal course he may probably, after a few unsuccessful struggles, come to the conclusion that the principles which he is virtually, though not altogether expressly, employing must be essentially sound, since they invariably lead to correct results, but that they have somehow not found precise and consistent formulation in the text-books. If he chooses to rest content in this persuasion, deeming that form and presentation are after all secondary matters, he may perhaps find satisfaction in the reflection that he is in much the same case as the great masters of the subject: Newton, Euler, d’Alembert, Lagrange, Maxwell, Thompson and Tait (to name only a few), whose expositions, whenever they do not glide hastily over preliminaries, are all open, more or less, to the kind of criticism which has been referred to. The student, however, whose interest does not lie solely in the applications, may naturally ask whether some less assailable theoretical basis cannot be provided for a science which claims to be exact, and has a long record of verified deductions to its credit. 7

Whatever ones pedagogical objectives, the sins of simplification eventually catch up with the most practical of men, as they struggle to deal with real life situations more complicated than encountered in the lecture hall. Indeed, a considerable portion of our modern appreciation of the subtleties of mechanical doctrine was prompted in the 1920's by the modeling requirements of the paint and rubber industries, as their technicians sought reliable guidance in modeling such oddly behaved materials in a satisfactory manner. To achieve good results for these materials, these engineers discovered that they needed to “revisit old fundamentals” in a very deliberative fashion (Lamb wrote before such work commenced in earnest and did not realize that success in advanced “applications” requires a more subtle articulation of “basic principles” than he anticipated).

Viewed retrospectively, the degree to which the technical arcana of classical mechanics have impacted the development of scientifically attuned philosophy over the past several centuries is quite striking, even if this influence is not always recognized by modern readers. In this review, we shall sketch some of the chief
ways in which the subtleties of classical mechanics have impacted philosophy's parallel career. There are two major arenas in which these effects have arisen. First, many of our greatest historical thinkers (Newton, Leibniz, Kant, Duhem, etc.) directly struggled with the problems of classical matter and their developed philosophies often prove intimately entangled with the specific foundational pathways they chose to follow (the abstract ruminations of The Critique of Pure Reason, for example, appear to have derived in part from the nitty-gritty worries about flexible matter we shall review later⁸). Such portions of our philosophical heritage are often misunderstood nowadays simply because the true contours of the physical problems our forebears faced have been forgotten.

As a result of these struggles, the great philosopher-scientists formulated a wide range of philosophical attitudes including anti-realism and instrumentalism as a response to the technical oddities they confronted. The twentieth century logical empiricists who came later--after the chief focus of academic physics had shifted to quantum theory and relativity--were influenced by those older philosophical conclusions without adequate appreciation of the concrete issues which prompted them. For example, they fancied that, e.g., the old debates over "force" could be resolved briskly by the simple expedient of requiring the warring parties to articulate their fundamental doctrines in the format of an axiomatized theory. Once the subterranean issues had been flushed to the surface in this crisp manner, the logical empiricists presumed that they would prove quickly resolvable. In truth, they never applied their techniques to real life mechanics or any comparably complex physical subject, for they had become so convinced of the power of their "cures" that they never tried their remedies on an actual patient.⁹ On the basis of fond hope alone, they elaborated a large bank of methodological opinions concerning laws, explanatory structure, scientific vocabulary that would render general philosophy of science attractively simple if it had proved descriptively accurate. Unfortunately, many philosophers have continued to hew to these old presumptions as if they represented firm verities, illustrating Darwin's celebrated aperçu:

*False facts are highly injurious to the progress of science, for they often endure long; but false views, if supported by some evidence, do little harm, for everyone takes a salutary pleasure in proving their falseness.*¹⁰

Thus a large folklore of "false facts" concerning classical mechanics continues to shape contemporary philosophy along unprofitable contours even today. Such misapprehensions comprise a second legacy of the conceptual difficulties that naturally arise within the framework of "classical physics."
It is not the chief intent of this essay to pursue these satellite philosophical concerns with any vigilance, but to instead concentrate upon the key tensions that render classical doctrine hard to capture in the first place. Nonetheless, I hope that our prolegomena on larger themes suggests that significant points of general philosophical edification still lodge within the cracks of mechanics' hoary edifice. In my view, the considerations surveyed in this report supply vital lessons of wide applicability in the wandering ways of words: the subtle adjustments that cause innocuous-appearing vocabulary to vary significantly in their applicational effects. In that investigative spirit, then, let us delve into the weeds of "classical mechanics."

(ii)

Axiomatic Presentation. It will serve as a convenient benchmark for our investigations to recall that David Hilbert placed the rigorization of mechanics on his celebrated 1899 list of problems that mathematicians should address in the century to come (it is his sixth problem). He wrote:

*The investigations on the foundations of geometry suggest the problem: To treat in the same manner, by means of axioms, those physical sciences in which mathematics plays an important part; in the first rank are the theory of probabilities and mechanics.*

Indeed, Hilbert's own work in geometry and elsewhere comprised a chief inspiration for the logical empiricist program discussed in the previous section. Following this lead, we will serially examine the prospects for meeting Hilbert's challenge based upon the three foundational choices identified in (i): point masses, rigid bodies and continua. In his fuller statement, Hilbert mentions only the last two, probably on the grounds that few practitioners would have favored the point mass alternative at the time.

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

Incidently, when a textbook is labeled “foundations of mechanics,” the term probably doesn’t designate an enterprise of the sort contemplated here, but simply encompasses “the things one must first study in learning mechanics.” Insofar as a “best foundations for classical mechanics” goes, probably a continuum physics framework best qualifies for the job, but the mathematical requirements of this subject are far more demanding than arise within point mass or analytic mechanics (e.g., in utilizing PDEs rather than ODEs). But no sensible pedagogue would have her students tackle the former without a lot of prior acquaintance with ODEs. Indeed, PDEs weren’t invented until the 1750’s and the full array of mathematical devices requisite to the proper treatment of fluids and flexible solids weren’t adequately developed until well into the twentieth century. Even today, it remains standard textbook practice is to introduce continuous bodies casually as an “elaboration” upon the physics of point masses and rigid bodies, in the gradualist manner that Euler laid down in a primer of 1734:

The Grand Plan: Those laws of motion which a body observes when left to itself in continuing rest or motion pertain properly to infinitely small bodies, which can be considered as points. ... The diversity of bodies therefore will supply the primary division of our work. First indeed we shall consider infinitely small bodies ... Then we shall attack bodies of finite magnitude which are rigid. ... Thirdly, we shall treat of flexible bodies. Fourthly, of those which admit extension and contraction. Fifthly, we shall subject to examination the motions of several separated bodies, some of which hinder [each other] from executing their motions as they attempt them. Sixthly at last, the motion of fluids will have to be treated. 14

Such educational policies, although reasonable from a pedagogical perspective, commonly instill loose practices of “lifting” theses valid for, e.g., point masses over
to rigid bodies or continua without adequately checking that the claims continue to hold within these new arenas or even that core vocabulary retains its original meanings. These illicit transfers often prove so invisible in their operations that investigators struggling with complex problems have trouble tracing out their unhappy effects.

Hilbert placed the “foundations of classical mechanics” upon his list of important problems for precisely these reasons. He wrote:

*The physicist, as his theories develop, often finds himself forced by the results of his experiments to make new hypotheses, while he depends, with respect to the compatibility of the new hypotheses with the old axioms, solely upon these experiments or upon a certain physical intuition, a practice which in the rigorously logical building up of theory is not admissible.*

We will consider several examples of what he has in mind in some detail.

One also can’t appreciate the old puzzles of classical matter in their historical dimensions unless we keep the mathematical difficulties of continua firmly in mind. Scientists planning bridges or studying the musical qualities of violins in early eras didn’t have the luxury of waiting until the twentieth century to gather the tools they properly require. They simply had to cobble by with the mathematics they had on hand, even at the price of rather dodgy justifications. For example, due to the lack of clearly articulated PDE equations, Leibniz and his school couldn’t deal directly with the three-dimensional complexities of a shaking beam straight on; they were forced to dissect the problem as illustrated into a connected sequence of 1-dimensional tasks locally governed by ODE’s (e.g., “Assume that the beam is symmetrical in its cross section, then integrate along the line of its centroid. Consider the resulting moments and arrange them in a line along the ‘neutral axis’ to produce an equation for the beam’s static loading. Then investigate its shaking behaviors by turning on its acceleration as an additional ‘force’”) Newton followed a similar procedure in investigating how rotation affects the earth’s shape is affected by its rotation (he began his treatment with a one-dimensional “canal” through the planet’s interior). Even today, most textbook problems adopt similar reductive stratagems: witness the standard treatment of “the vibrating string.” But studying
physics within these reduced, lower dimensional settings can be very misleading from a "foundational" point of view (encouraging one to, e.g., think of stress as "simply a kind of force"). However, it is unlikely that classical physics could have staggered its way to an adequate treatment of continua without relying upon a broad array of results for systems that, from a foundational point of view, cannot represent their proper conceptual ingredients. It is also important to recognize that "philosophy" often played a critical historical role in easing guilt over these awkward "lifts" through claims that "the exact sciences must inherently idealize their subjects" and so forth. We shall survey how these injections of "philosophy" have operated in more detail later.

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

The contemporary philosopher's absolutist expectations largely trace to the
fact that scattering experiments and allied phenomena in subatomic physics seem to indicate the existence of a lowest layer of non-composite "fundamental particles" (although such issues are quite complex even here). But these phenomena are strongly quantum mechanical in their character and one shouldn’t automatically export "bottom layer" assumptions appropriate to modern quantum conceptions back to a wholly classical context. Indeed, we will fail to understand philosophical attitudes prevailing within the classical era if we do so. Most of the great scientists of that time tacitly recognized that descriptive success in reliable modeling invariably relies upon some tacit choice of scale length. Matter generally reveals a hierarchy of qualities depending upon how closely one inspects its structural details (it is traditional to designate this depth of focus by a "characteristic scale length" $\Delta L$). For example, on an observational scale $\Delta L^O$, well-made steel obeys simple isotropic rules for stretch and compression under normal loads. But closer inspection reveals that this macroscopic uniformity and toughness represents the resultant of a carefully engineered randomness at the level of the crystalline grain $\Delta L^G$ making up the material (such a scale length is sometimes dubbed the "mesoscopic level"). Considered at this lowered $\Delta L^G$ length, each component granule will stretch and compress in a more complicated manner than the bulk steel, but their randomized orientations supply the larger body with its simple behavior at the macroscopic level (so-called "homogenization theory" concerns itself with the details of how this $\Delta L^G$ scale to $\Delta L^O$ scale process operates). Lowering our focus to the molecular lattice $\Delta L^L$ composing the grain, we find that its capacity to transmit dislocations supplies the true underpinnings of the admirable toughness witnessed in the bulk steel at the much longer characteristic length $\Delta L^O$. If we attempt to capture these various scale-dependent behaviors individually utilizing classical modeling techniques alone (as we can, to a remarkable degree of success), we will generally find ourselves selecting different ontological "base units" according to the implicit scale length we have selected. In such a mode, civil engineers usually model a steel beam upon a $\Delta L^O$ scale as a single flexible body of considerable homogeneity, whereas technicians interested in steel manufacture typically concern themselves with the thermodynamics of structural formation at the $\Delta L^G$ level. As such, the latter often adopt an "ontology" of rigid crystalline forms
bound together into a complex material matrix (= rigid body mechanics supplemented by various stereotyped flexible elements\textsuperscript{16}). Initial efforts in modeling materials at the $\Delta L^1$ scale often employ point mass atoms bound together in an irregular grid. But a more refined approach to these same lattice "atoms" will instead assign them flexible shapes (at the cost of considerable computational complexity).

And so the modeling shifts proceed, each alteration in characteristic scale length commonly favoring a different "ontology" in its modeling material. Speculative philosophers such as Leibniz opined that this alteration of ontological units will continue forever as one descends to smaller scales (he dubbed this putative behavior "the labyrinth of the continuum," a phrase we'll later link to an important foundational regress). More cautious observers merely observed that experiment had not established any clear choice of "lowest scale unit" for classical mechanics. In this regard, it should be recalled that the evidence for "fundamental particles" only became overwhelming at the very end of the classical period, in the guise of Rutherford's experiments on radioactive scattering and the like. Once quantum mechanics enters our descriptive arena, its percepts increasingly dominate at smaller scale lengths and we eventually fall beyond the resources of classical modeling tools altogether. Unfortunately, the various "crossover points" at which classical treatments lose their accuracy do not favor any uniform choice of "fundamental classical entity." Sometimes point mass treatments supply the most convenient form of lowest scale classical modeling, but more often continua or rigid bodies provide better modeling accuracy. So while quantum mechanics may select certain entities as physically "bottom level," it does not follow that classical mechanics will do the same when considered upon its own merits. Accordingly, Hilbert's "sixth problem" formalization project shouldn't be saddled with the burden of satisfying a contemporary philosopher's expectations with respect to "bottom level ontology."

What we will want to investigate carefully, as part of our "foundationalist" enterprise, is the degree to which principles applicable on a higher scale level $\Delta L^*$ relate to those applicable at the lower length $\Delta L$ (such scale to scale relationships were dubbed "lifts" earlier). Indeed, Hilbert's own articulation stresses the importance of understanding these "lifts" more centrally than the simpler task of formalizing our three starting perspectives. He wrote:

\textit{Boltzmann's work on the principles of mechanics suggests the problem of developing mathematically the limiting processes, there merely indicated, that lead from the atomistic view to the laws of motion of continua. Conversely, one might try to derive the laws of motion of rigid bodies by a}
limiting from a system of axioms depending upon the idea of continuously varying conditions of matter filling all space continuously, these conditions being defined by parameters. For the question of equivalence of different systems of axioms is always of great theoretical interest.\textsuperscript{17}

Here Hilbert calls our attention to the various relationships between scale length that have been intensely studied in recent times under the general headings of "homogenization" and "degeneration."\textsuperscript{18} He observes that the vague invocation of "limits" rarely provides an adequately precise diagnosis of the relationships involved, an observation that modern investigations heartily underscore. Observe that Hilbert's final sentence suggests that he didn't anticipate that any of his suggested starting points will prove fundamental in the "bottom layer" sense just canvassed. According to the applicational task at hand, different modes of ontological dissection (e.g., flexible continua or Boltzmannian swarms of rigid bodies) may possess their descriptive utilities in the same manner in which alternative decompositions of geometry into "primitive elements" prove fruitful. Even so, Hilbert insists that we must guard against erroneously "lifting" physical doctrines from decompositional program to another without adequate precaution. In standard textbook practice, these "lifts" usually appear as dubious "derivations" of, e.g., rules of continua considered at a $\Delta L^*$ scale level on the basis of rigid body swarms at a $\Delta L$ scale. As we'll later see in detail, such improper doctrinal are common in practice and sometimes serve as the source of substantial conceptual confusion.\textsuperscript{19}

It is probably useful to consider a simple example of the problems that can arise in such $\Delta L$ to $\Delta L^*$ shifts. The term "force" has a notorious tendency to alter its exact significance as characteristic scale lengths are adjusted. At a macroscopic level, we regard the "rolling friction" that slows a ball upon a rigid track as a simple Newton-style force opposing the onward motion. But when we shift to a lower scale length, we learn that seemingly "rigid" tracks are not so firm after all, but elongate under the weight of the sphere to a non-trivial degree. So part of the work required to move our ball against friction consists in the fact that it must travel further than is apparent. But when we consider the "forces" on our ball at a macrolevel, we instinctively treat the track length as fixed and allocate the effects of its actual elongation to a portion
of the "force of rolling friction" budget. A similar phenomenon occurs with the "viscosity" of a fluid. When such adjustment in reference occur, one cannot legitimately lift a doctrine about "forces" applicable on scale level $\Delta L$ to scale level $\Delta L^*$, for "force" doesn't mean quite the same thing in the two applications.

Of course, if these innocent drifts were the only kinds of problematic "lift" to which mechanical practice was liable, serious conceptual debates wouldn't have arisen in the subject. But our humble illustration supplies a preliminary sense of the problems we must watch for. By the way, throughout our discussion I will employ "lift" in the "elevator" sense: one can go both up and down in a "lift."

The properties we ascribe to a system with respect to a upper scale length $\Delta L^*$ ("rolling on a rigid track") usually represent averages (or some allied form of "homogenization" or "degeneration") over the more elaborate behaviors we will witness at a finer scale of resolution $\Delta L$ ("stretching the molecular lattice"). Obtaining a workable scheme of physical description tailored to $\Delta L^*$ usually requires that a fair amount of fine detail gets frozen over in our modelings. In other words, we generally hope to capture only the dominant behaviors of our real life system within in our $\Delta L^*$ treatment and anticipate that we will sometimes need to "open up the suppressed degrees of freedom" whenever the complexities of the lower scale begin to intrude upon the patterns normally predominant at the coarser scale $\Delta L^*$. Generically, the utilization of a smaller set of quantities to capture system behaviors dominant upon a higher scale length $\Delta L^*$ is called a reduced variable treatment (the descriptive quantities utilized there can prove quite different from those that normally employed at a lower $\Delta L$ scale). There are a large number of ways in which these "reduced variable" models can arise. For example, a reasonable policy of homogenization might adjust its descriptive terms from those suited to a $\Delta L^0$ assembly of iron grains to a smoothed-over steel bar described as continuous at the $\Delta L^0$ level. But a quite different exemplar of reduced variable "freezing" can be witnessed in Newton's celebrated treatment of the planets. At the scale lengths appropriate to celestial mechanics, one can ignore the complexities attendant upon the earth's shape and size by modeling it as a simple point mass; rather than smearing out the properties of the planets over wider regions (as occurs in homogenization), we instead concentrate their extended traits upon much smaller supports. Such policies of compressing complex expanses into singularities (or other lower dimensional structures such as one-dimensional strings) are sometimes called degenerations (a term I regard as preferable to the misleading phrase "idealization"). Plainly, when very detailed astrophysical calculations are wanted, one must "open up" those internal complexities and treat the earth as a continuum subject apt to distort under rotational effects. But there are many forms of "reduced
variable” lift that involve a mixture of the two policies or other sorts of tactic altogether.

Some of the “anti-atomism” advocated by late nineteenth century scientists such as Duhem and Mach traces not to some obtuse dismissal of lower scale structure per se, but to the widely shared assumption that, in any application, modelers must invariably engage in such “freezing to a scale level” procedures. Their primary disagreement with other mechanists of their era primarily concerns the format that should be regarded as the optimal embodiment of “classical principle” within such a scale-sensitive setting. Specifically, Duhem and Mach maintained that “basic physics,” as an organizational enterprise, should develop tools that will prove maximally useful at any chosen scale length. This requirement almost automatically favors a “thermomechanical” approach of the sort we shall later survey in our discussion of flexible bodies. Their opponents, such as Ludwig Boltzmann, generally favored the simplest base ontology that could plausibly support the more complex forms of mechanics in a ΔL to ΔL* manner (they often employed point masses or connected rigid bodies as their base level ingredients). In these respects, we might observe that Duhem and Mach’s strictures better suit the methodological percepts of empiricists such as David Hume, who opined that any postulation of lower scale structure must be based upon “laws” directly verifiable at the laboratory level.

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

John and Hilary Ockendon provide a useful example:

This is a familiar story in applied mathematics: although we may have hoped for a comprehensive self-contained theory of compressible flow based on macroscopic principles of conservation of mass, momentum and energy, we have not been able to escape entirely from consideration of the intermolecular forces that determine not only the equation of state but also the correct macroscopic model for gas dynamics, especially in extreme
configurations such as shock waves.\textsuperscript{21}

The situation they have in mind is this. Success in treating a gas at everyday scale lengths $\Delta L^*$ as a continuous compressible fluid relies upon the assumption that the underlying molecules carry largish packets of momentum forward in macroscopically coherent way. If the gas becomes either too rarified or too dense in its travels, the requisite coherence dissipates and our $\Delta L^*$-linked gas equations will lose their descriptive utility, despite the fact that computers are wont to plot fictitious stories of how the gas evolves beyond the point of breakdown by relying upon a $\Delta L^*$ model beyond its scope of reliable operation. Indeed, a good deal of programming skill is often required to persuade our computers to halt their calculations before they wander into utter fantasy; computer scientists must frame cutoff rules that warn, “You’ve reached the limits of the modeling possible at this scale length; calculate no further.” As the Ockendons suggest, to obtain reliable results we must often\textsuperscript{22} shift to a scale resolution at a molecular length $\Delta L$ and struggle with the daunting complexities of molecular modeling (at a $\Delta L$ scale of resolution, the shock wave merely looks like a piled up collection of molecules). In fact, our $\Delta L^*$-scale gas equations actually become \textit{mathematically inconsistent} when the shock wave structures form (our $\Delta L^*$-level equations display a “blowup” in the mathematician’s sense). Computer programs often can’t detect this inconsistency (they merely think that densities have grown very large in the affected regions). Without careful programming to the contrary, such programs will merrily continue to plot ridiculous futures for our gas beyond the times at which their real world behaviors alter dramatically due to the emergence of the shock wave structures.

We shall return to the $\Delta L/\Delta L^*$ considerations that apply to shock waves several times in the sequel, as it represents an excellent prototype of a descriptive dilemma that often rises in physics.

Philosophers sometimes associate such higher scale descriptive gaps with traditional \textit{"ceteris paribus"} clauses, although that association strikes me as unnecessarily vague. We can often pinpoint quite accurately where the “holes” in a descriptive treatment will occur and precisely how they should be remedied.
The general explanation for such upper-scale gaps is quite straightforward: a useful selection of "reduced variables" at the ΔL* level will focus upon behaviors that dominate at that size scale. But, invariably, there will be special ΔL-level arrangements where the effects suppressed in our ΔL* treatment obtain equal or greater importance than the usual dominant behaviors. This, in fact, exactly the phenomenon to which the Ockendons draw our attention in the quotation above. In such situations, one must either retreat to modeling the gas at the original ΔL level (as the Ockendons recommend) or shift to some more complex recipe for "reduced variable" averaging at the ΔL* scale length (Ludwig Prandl’s celebrated "boundary layer theory" can be considered a classic exemplar of the second policy).

I shall sometimes call such shifts “escape hatches” for they provide ladders that allow us to evade the inferential instructions of a formalism that no longer serves its empirical purposes (we observed that computers must be carefully programmed to watch for the clues that suggest when such a remedy is advisable). But such practices create a formal difficulty for axiomatization projects in Hilbert’s vein because the domain of interest frequently becomes “reontologized” under the scale shift. In the Ockendon’s example, we switched from considering our gas as a continuous body to a ΔL treatment where it is comprised of point masses (or some allied variety of smaller blob). But axiomatic presentations rarely include provisos for such “ontology” shifts. Instead, we anticipate that their formal tenets will supply behavioral principles applicable to its “ontology” in all circumstances, even if, in real life practice, we would normally “escape” such descriptive straight jackets in favor of some revised treatment operating at a lower length scale ΔL. In short, conventional axiomatized theories are expected to supply principles that can govern even the “bad spots” within their ranges of empirical coverage. Such formal expectations lead many philosophers to further suppose that “classical mechanics” must completely specify the behaviors tolerated within its own parochial range of “possible worlds,” in spite of the fact that we would never apply such modelings to real world dominions of a strongly quantum mechanical or relativistic character.

But such dogmas presume that some fairly complete axiomatization of overall “classical mechanics” is available, a thesis we shall critically examine in this essay.

It is a striking psychological fact that we engage in allied ΔL* to ΔL "reontologizations" so commonly within science and everyday life that we scarcely pay attention to them: we merely observe that we’ve “opened up” the interior complexity of a system for more explicit treatment. We frequently overlook the fact that we have maintained neither a constant ontology nor a constant collection of mechanical principles throughout our deliberations. How, then, should we expect,
in a Hilbertian axiomatization with a fixed ontology and fixed doctrinal tenets, to deal with the "bad spots" that force us to shift to some other descriptive framework within real life applications? Here one is likely to presume (and Hilbert himself may have adopted such an attitude) that one should try to cover the "bad spots" somewhat artificially in order to obtain a self-sufficient brand of mechanics capable of standing on its own as an internally coherent and complete formalism. Indeed, there are many practical reasons why one might wish to delay a shift to a lower modeling scale $\Delta L$ as long as possible, even at the price of including odd and rather elaborate structural ingredients at the original $\Delta L^*_e$ scale length. For example, in the circumstances that the Ockendons describe, there is a famous policy (pioneered by Riemann, Hugoniot and others) that models the shock waves as singular surfaces in a manner that quite successfully postpones the need to escape to molecular considerations (we shall discuss these remedies in more detail later). In an allied spirit, recent work on phase changes employs novel interfaces and "configurational forces" to handle moving "phase change" structures within a solid without abandoning the general framework of continuum mechanics. 23

To be sure, rarely can one permanently escape the empirical need to reontologize a $\Delta L^*_e$ modeling through $\Delta L$ replacements by such higher scale expedients, but the partial successes of such non-exiting descriptive stratagems encourage the hope that we should be able to, e.g., articulate the general principles of continuum mechanics in a manner that prescribes consistent behaviors to gases under all classical feasible conditions, even if we would be loathe to trust its worst "bad spot" predictions within real life applications.

This represents a reasonable hope, but, in point of brute fact, adequate "filling in" principles have never been canonically adopted for any of our three basic classical "ontologies." Over the course of this essay, we shall document some of the "missing ingredients" that prevent us from filling out their respective axiomatizations in the manner that Hilbert anticipated. Such doctrinal holes have generally escaped conceptual notice due to our liberal propensity to reontologize problematic domains without thinking much about what we are doing. Leaf through any modern text book that claims to operate according to "foundational principles" suitable for point masses. With scarcely any effort, we soon encounter passages that shift our attention to some competitor frame of "classical mechanics" or to quantum theory without any apologies for the adjustment in point of view being offered. Such diverting discussions provide us with answers of great practical value--how our applicational tenets should be adjusted to suit such circumstances--, but they leave us, as would-be axiomatizers, with unresolved questions as to how the "bad spots" in the original doctrines should be patched over in the axiomatized
coverage we seek.

But let us now ask ourselves a commonsensical question. Considered from a practical point of view, is it really wise or meritorious to fill out a formalism in a manner that carries with it no discernible empirical merit? Mightn’t it be better to deliberately leave our stocks of physical principle somewhat incomplete, allowing its very “holes” to signal when we should look for suitable ΔL* to ΔL “escape hatches” (including those adjustments that abandon classical modeling techniques altogether in favor of intrinsically quantum tools)? In the Ockendons’ example, conventional continuum modelings generate “shocks” that, from a mathematical point of view, represent out-and-out descriptive contradictions (a so-called “blowup” occurs where points in the wave are credited with two incompatible velocities). Theoretically, we can impede the formation of such “shocks” by adding an empirically unmotivated viscosity term to the governing equations. But hiding the “shocks” in this manner is usually not a wise policy: one wants to employ the appearance of the “shock” contradiction precisely as a signal that we should abandon our current modeling practices in favor of something else (e.g., considerations at a molecular level or by employing the Riemann-Hugoniot trick). Indeed, explicit indications in the mathematics of when modeling problems begin to start should be greatly cultivated, for we surely want to avoid the fate of the computers who cheerfully compute worthless data simply because no one has told them to stop. As Horace Lamb implicitly notes in the quotation cited above, training in mechanics generally inculcates considerable skill in knowing when one should adventitiously shift from one modeling framework to another:

From this point of view it is legitimate, in expounding the subject, to take advantage of whatever prepossessions [the student] may have as are serviceable, whilst warning him against others which may be misleading.

So perhaps it is unwise to push a formalism’s axiomatized coverage beyond the limits of its real life modeling effectiveness?

This point of view suggests that we might look upon the inherited compendium of descriptive lore we call “classical mechanics” as a series of descriptive patches (corresponding to our three basic choices of “fundamental objects”) linked together at their descriptive “bad spots” by various ΔL* to ΔL escape hatches. However, whenever “manifolds” are constructed through sewing together local patches in this way, twisted topologies can potentially emerge in the final result (Klein bottles and Mobius strips provide classic illustrations of the
phenomena). Considering standard textbook lifts in this vein, we find that the
“funny topology” option appear to be realized, in the sense that our lowest scale
length forms of viable classical modeling (= the smallest ΔL at which one can
fruitfully employ some form of “classical mechanics” modeling before a descriptive
crossover into quantum physics transpires) are apt to belong to any of our three
basic categories, depending upon circumstance. In these respects, Mother Nature
shows little favoritism as to which of our three
basic “ontologies” of classical objects should be
viewed as “fundamental” from an applicational
point of view. If we attempt to understand
“classical physics” as a conceptual system closed
unto itself; we thereby obtain a structure like one of
those impossible Escher etchings: local plates
connected by staircases that never stabilize upon a
lowest landing. But such topographical oddities do
not indicate that “classical physics” has not served
its descriptive purposes perfectly well. As long as
the salient escape routes are clearly marked, our Escherish edifice serves a base
frame upon which a wide range of interconnected forms of “reduced variable”
modeling techniques can be conveniently located. By operating with a proper
regard for the requisite level shifts, we can thereby assemble the most fruitful
terminology yet devised for dealing with the complex physical world about us at
non-microscopic scale lengths: the shared language of “classical physics.” The
twisted topology within its connection manifold merely reflects the “exit from bad
patches” considerations that allow the scheme to cover extremely wide swatches of
application with great efficiency.

In my Wandering Significance,25 I’ve developed this organizational moral in
a more general format: we can work capably and effectively with an interlaced
patchwork of descriptive tools even if they lack internal conceptual closure in a
conventional sense. In that same book, I categorized such quilt-like assemblies as
“theory facades” on the grounds that “they look kinda like theories if you don’t
look at them too closely.” I shall employ this same terminology on occasion here.
Hilbert recognized that the materials offered in the textbooks of his time were
interconnected in a facade-like manner but he probably presumed --in a manner that
was entirely reasonable in the time in which he wrote26--that some specific
organizational pattern would hew most closely to Nature’s own operations. Due to
the rise of quantum mechanics, we no longer see “classical mechanics” as serving
that descriptive function and regard it instead as a useful scheme for categorizing
macroscopic objects effectively with manageable collections of effective “reduced variables.” From this second point of view, a scheme can prove syntactically more efficient if the denotational significances of its vocabulary are allowed to vary in manners that locally optimize their descriptive effectiveness. With respect to a ball rolling upon a table top, allowing “force” to adjust its precise focus as it is applied under different scales of resolution introduces an admirable brevity in our descriptive resources, which can serve us well as long as we do not become confused by such policies. Recent mathematical work upon effective “variable reduction” underscores these methodological morals.  

There is a popular movement in philosophy (which might be called “the disunity of science” school) that has considered derivational irregularities like the ones we shall investigate and have concluded, on that basis, that it was foolish for Hilbert to have presumed that the free-wheeling human practice we call “physics” could patiently sit for some solid portrait within axiomatics. I do not share such opinions (although some of my readers appear to believe that I do). My own formalist sentiments are more in accord with Hilbert’s and I particularly share his appreciation of the virtues that rigorous syntactic and semantic study offers to science. From my perspective the only difficulty with axiomatics (at least of a conventional flavor) is that it doesn’t provide the internal tools required to keep the various patches within a “reduced variable” facade from interfering with one another, while positively marking the “escape routes” one should follow when local descriptive resources fail. Such differences being noted, we can investigate the concrete structuring of a “theory facade” in the same mathematical spirit that prompted Hilbert to frame his sixth problem on mechanics.

In these respects, although we shall begin our survey with a rather critical scrutiny of the “derivations” found in every college textbook, we will eventually work our way to a brighter opinion of their functioning from a “facade” point of view. But sincere praise often requires a prolegomena of criticism, if the party to be honored has misidentified his own virtues at the outset. We must first learn that stock textbook manipulations do not truly operate as the “derivations” that their authors mistake them to be, before such \( \Delta L^* \) to \( \Delta L \) interconnections can be justly praised as the “escape hatches” that organize classical mechanics' facade into an effective descriptive instrument. Accordingly, the descriptive virtues and limitations that characterize the standard ontological divisions of “classical mechanics” must be individually surveyed before we can adequately appreciate the wider architectural role that standard “lifts” concretely facilitate.

To be sure, I agree with the “disunity of science” folks in the sense that the corpus of doctrine comprising “classical mechanics” continually enlarges in the
manner that Wittgenstein dubbed a “family resemblance”:

[W]e extend our concept[s]... as in spinning a thread we twist fiber upon fiber. And the strength of the thread lies not in the fact that some one fiber runs through its whole length, but in the overlapping of many fibers.\(^{29}\)

As such, the thick “thread” of classical mechanics gradually gains its descriptive strength through exploiting the lifts between “fibers” (= patches) that bind the whole into a unity. If such cross-linkages were not in place, the spectrum of natural behavior to which “classical physics” is applicable would appear far smaller than it really is (recall that, in effective “variable reduction,” descriptive effectiveness gets maximized through tuning descriptive terminology locally to the cases at hand). But nothing in this interwoven “family resemblance” character indicates that we can’t further strengthen our overall “thread” by extending each “fiber” to its maximal modeling capacities and through also tightening, through precification, the “lifts” that bind these longer strands together. Many of Wittgenstein’s followers see the passages on “family resemblance” as supplying excuses for approaching linguistic behavior only as a loose conglomerate of largely unanalyzable “practices,” in conformity to the recommendations stemming from the “disunity of science” school. But in modern cognitive science, our capacities to sort human facial features according to “family resemblance” are viewed as significant skills that require a deeper diagnosis with respect to the hidden mechanisms underlying such capacities. Just so: we likewise desire a richer understanding of how “reduced variable” theory facades serve our macroscopic descriptive interests as ably as they do. Examining the range of successful “classical” descriptive techniques in terms of facade-like strategies strikes me as a potential paradigm to follow in our efforts to understand, in sharper formal terms, why real life conceptual growth soften unfolds in a “family resemblance” manner. We shall briefly return to these wider musings at the essay’s end.

In the passages cited, Wittgenstein warns against the presumption that concepts like “force” possess some elusive “conceptual unity” that we can’t quite pin down. Lamb captures the same idea in the foregoing passage (quoted once again):

*If the student’s intellectual history follows the normal course he may probably, after a few unsuccessful struggles, come to the conclusion that the principles which he is virtually, though not altogether expressly, employing must be essentially sound, since they invariably lead to correct results, but that they have somehow not found precise and consistent formulation in the text-books.*

The analysis we shall supply in this essay will provide a number of firm and
relatively easy to identify factors that explain the strong “family resemblance” impression that every physics student extracts from her studies, one of which traces to the common reoccurrence of a “modeling recipe” built upon a “\( F = ma \)”-like frame. But that structural “commonality” is too weak to prevent the term “force” from experiencing localized “property dragging” that eventually blocks any effort to bind all of “classical physics” into a plausible axiomatized whole.

As we outline the ingredients that stitch the “facade” of classical mechanics into greater unity, we should recognize that, from a historical point of view, much of the “facade” needed to be in place before any proper understanding of its delicate “lifts” would have been possible. Hilbert often stressed the fact that topics are rarely ripe for axiomatization unless they have been developed to a reasonably mature condition. The same reservations hold for the prospects of an adequate “facade” analysis as well. As we’ll see, the supportive rationale for abandoning, e.g., a point mass modeling in favor of continuum physics (or vice versa) often traces to underlying considerations of the quantum mechanical behavior of matter on lower length scales. But none of the great Victorian practitioners of mechanics could have foreseen such rationales, although they were often aware that Nature hadn’t yet supplied them with dispositive reasons for preferring one form of classical “ontology” over another.

Indeed, the historical triumph of “classical mechanics” as a descriptive enterprise would have never occurred had the subject not lightly skipped over the many “lifts” that we shall criticize in some detail. Left undiagnosed, such linking policies can sometimes occasion substantive problems (in the next section, with Hilbert’s aid, we shall explore some of the attendant dangers). Historically, the price of a vigorous conceptual enlargement is often a lingering residue of confusion that can occasionally blossom into full paradox when suitably nurtured. And so the career of classical mechanics has transpired: full of predictive glories but also laden with mystifying elements that have led some of our greatest philosophical minds down the garden path to very strange assessments of our descriptive position within nature.

These tightly entangled successes and foibles explain, I think, why a careful study of wizened “classical mechanics” yet offers philosophy many valuable lessons as to how “successful conceptual developments” unfold within real life descriptive practice.

(iii)

**Wedge Predicate Lifts** As already observed, it was well into the twentieth
century before the mathematical ingredients needed to set up classical flexible bodies properly were assembled. In the meantime, physicists were forced to cobble along, stringing fragments of ill-fitting doctrine together with strands of “philosophy” in a gerry-rigged fashion. Hilbert was fully aware of these genetic difficulties; in fact, he regarded the task of clarifying the foundations of mechanics” as important for exactly this reason:

_The physicist, as his theories develop, often finds himself forced by the results of his experiments to make new hypotheses, while he depends, with respect to the compatibility of the new hypotheses with the old axioms, solely upon these experiments or upon a certain physical intuition, a practice which in the rigorously logical building up of theory is not admissible._

It is easy to see, in general logical terms, how substantive confusions can arise if Hilbert's warnings are not heeded. Suppose that we are operating within a fixed formal structure T and we begin employing some restrictive predicate Cx without verifying whether T tolerates any (or very many) subsystems of this type. If (∃x)Cx is actually incompatible with T, then standard logic assures us that all claims of the sort (∀x)(Cx ⊃ Gx) will follow from T, _no matter what content of Gx proves to be_ (simply because anything follows from contradictory premises). And this unwitting exploitation of contradictory data can occur in subtle and hard to notice manners, sometimes redirecting theoretical effort in ways that actually prove useful from an empirical standpoint. Employing C's that are adroitly but unwittingly cross-fertilized with ingredients _selectively extracted_ from T, practitioners can easily initiate new theoretical enterprises T^C without recognizing that they have strayed considerably from the doctrines with which they originally began. In such cases, the casual introduction of C has served _as a wedge predicate_ to shift our original T frame into a potentially different setting T^C. In the fullness of time, such superficial appearances of T-connectedness can prove harmful, for eventually the hidden divergencies between T and T^C must be diligently hunted down and diagnosed (vide the “force” example above). For such reasons, it has become standard practice within mathematics to explicitly verify that T and (∃x)Cx are actually compatible (and within what range of structures).

A simple illustration of such a “wedge predicate” shift can be found within the opening pages of most modern texts in classical mechanics (our exemplar comes from Greenwood’s excellent _Classical Dynamics_). After an orthodox delineation of Newtonian point mass mechanics (in the manner of section (iv)), Greenwood quickly remarks:

_The equations of motion for the system of N particles can be written_ 

\[ m_i \frac{d^2 \mathbf{r}}{dt^2} = \mathbf{F}_i + \mathbf{R}_i \quad (i = 1, 2, \ldots, N) \]

_where m_i is the mass of the ith particle and where we have broken the total
force acting on this particle into two vector components, \( F_i \) and \( R_i \). \( F_i \) is called the applied force and \( R_i \) is the constraint force. Briefly, \( R_i \) is that force which insures that the geometrical constraints are followed in the motion of the \( i \)th particle.

Here "constraint" covers the seemingly innocuous assumptions that one commonly finds in textbooks: provisos such as “bead \( b \) slides frictionlessly along a wire \( W \)” or “particle \( i \) remains confined to the surface \( S \) of a geometrically perfect table.” But, from a rigorous point of view, has Greenwood properly established that such \( F_i \) and \( R_i \) decompositions are truly consistent with the postulates that he has set forth in the opening pages of his book?

The answer is clearly “no,” for point mass mechanics only tolerates forces that can approximately implement standard constraints. Why? For starters, no array of point particles exerting plausible forces that can hold our “bead” (modeled as a point mass \( b \)) to any fixed geometrical contour such as \( W \) (forces capable of performing this chore are commonly called “constraint forces” or “forces of reaction”—their hypothesized characteristics will prove very significant in the sequel). Now suppose that we were able to arrange a schedule of forces arising from the wire \( W \) that could bind bead \( b \) traveling at velocity \( v \) perfectly to \( W \)’s unyielding surface. Can these same forces perform the same chore if the velocity of the bead had instead been \( v^* \)? To the that, the forces will need to be velocity sensitive (because \( W \) must exert stronger forces upon a faster \( b \) to pull \( b \) to its requisite destination). But customary point mass interpretations of Newton’s third law forbid forces of this character (we’ll discuss this law more fully later). Forces of a permissible type can hold \( b \) near to \( W \), but only at the cost of some complex wobbling.

In fact, the point mass disharmonies with standard textbook constraints run deeper than this, for their posited restrictions are usually assumed to apply without significant friction (most textbook “bead on a wire” examples are tacitly frictionless). But suppose that bead and wire are composed of the same material. Shouldn’t the same cohesive forces that hold \( b \) and \( W \) together as individual objects also bind \( b \) and \( W \) to each other? (I recall puzzling about this question in high school physics). Indeed, if we adequately cleanse and polish the contacting surfaces
between \( b \) and \( W \), they will bound rather ferociously. To obtain plausible approximations to the frictionless sliding of real life experience, surfaces can only contact at widely dispersed asperities, should be lubricated by sundry fluid forms of surface gunk and display a capacity to melt at the high local temperatures (10,000° C) characteristic of ordinary rubbing! Some of the processes involved are deeply quantum mechanical in their chemistry and it is hardly evident that classical point mass mechanics contains the resources to produce even a mediocre simulacrum for frictionless sliding.\(^{32}\n
Accordingly, when a textbook offhandedly introduces a “constraint” like “\( b \) slides frictionlessly along wire \( W \),” it has engaged in a \textit{wedge predicate shift} that bypasses any discussion of the complicated internal arrangements needed to properly establish that any point mass system can behave in the posited manner. In the jargon of modern mechanics, such discussions have ignored the \textit{constitutive modeling} needed to underwrite the viability of the constraints. Although bypassing such thorny issues is often pedagogically wise, such offhanded policies can generate serious conceptual difficulties later on.

By the simple act of considering individual point masses like \( b \) together with much larger ensembles of such objects (the “wire” \( W \)), we have shifted from a tacit scale length \( \Delta L \) to a larger \( \Delta L^* \). A constraint, in fact, should be properly regarded as supplying \( \Delta L^* \) scale information with respect to how \textit{ensembles of \( \Delta L \) level objects approximately behave}. As Hilbert insists, in strict foundational work, we should not engage in such “wedge predicate lifts” without closer scrutiny.

Insofar as I am aware, Hilbert never observed that the “wedge predicate lifts” correlated with the familiar “constraints” of textbook practice illustrate foundational difficulties quite analogous to the \( \Delta L \) to \( \Delta L^* \) transitions that he explicitly highlighted. Like many of his contemporaries, he may have not taken the pure point mass point-of-view seriously. If we operate within a postulational framework based upon “analytical mechanics” (cf. section (v)), standard constraints merely represent reports on the manners in which rigid bodies can be attached to one another. As such, they represent \textit{primitive relationships} within the subject and no longer require elaborate “constitutive modelings” as their \( \Delta L \) scale supports. So perhaps Hilbert never wished to investigate any formalism based solely upon point masses.

Be that as it may, the practical effect of the offhanded introduction of “constraints” within most modern textbooks is to “lift” its readers surreptitiously from an initial point mass presentation into the realm of analytical mechanics, without providing an adequate treatment of their inter-relationships. To be sure, every student of the subject imbibes these shifts along with the first milk of physics
instruction. Such $\Delta L$ to $\Delta L^*$ adjustments are essential to setting tractable textbook problems swiftly before the pupil, but they often obscure the logical character of the "lifts" tacitly implemented within such introductions.

As we probe these issues in greater depth, we shall learn that the innocent-looking notion of a "constraint force" that arrives in tandem with "constraint" represents a quite problematic conceptual critter. Some of the deepest tensions in classical physics tradition trace to this quarter.

No doubt it appears pedantic to complain of such seemingly innocuous moves, yet our humble illustration directly displays the typical manner in which substantive shifts in characteristic scale length get implemented within standard physics instruction. Greenwood's text begins with point masses and action-at-a-distance forces acting between them, but the casual introduction of a "constraint" tacitly adjusts his discussion to structural arrangements applicable at a larger scale of physical size (e.g., the wire). Once that shift is tolerated, he can exploit our knowledge of that higher scale behavior (e.g., that beads frequently slide along wires) to simplify problems that, were they framed completely in a "bottom up" constitutive modeling mode, would prove intractable and unreliable. Bypassing such lower scale constitutive modeling is undoubtedly a wise policy, but, in doing so, we suppress a large number of relationships that scarcely differ, in their underlying logical character, from the "foundational questions" that Hilbert emphasized within his sixth problem (e.g., "under what kind of 'limit' will continuous behavior emerge from lower scale molecular movements?"). Modern experts recognize that, under the camouflage of such "lifts," the significance of the term "force" adjusts in a fashion where considerations of energy budget and capacity to perform macroscopic work become central. One of the prime reasons why the question "what does 'force' mean?" has proved hard to resolve historically traces to the fact that its physical significance is continuously hostage to property dragging "lifts" of the sort sketched here.

Observe, from an historical point of view, that the associative glue that notions like "constraint force" provide has played a significant role in binding together a number of previously disparate mechanical traditions. On one hand, we have the celestial mechanics developed by Newton which, to this day, remains the chief arena where the mathematics of point masses and action-at-a-distance forces
earns its greatest glories. But consider the parallel fund of rich physical knowledge that is directly entangled with "rigid bodies" and allied forms of geometrical *constraint*. Indeed, useful appeals of this flavor enjoy a considerably longer record of success within physical application than do any action-at-a-distance models of a Newtonian stripe. Thus ancient Greeks approached simple *mechanisms* (= assemblies of rigid parts hinged together in a manner that permits internal mobility) in terms of what we would now regard as their capacities to *transmit work*. For example, if a force \( F_1 \) can pull its rope through a (virtual) distance \( \delta_1 \), then, ignoring friction, a block and tackle should able to apply a transmitted force equal to \( F_2 \) where \( \delta_2 \) represents the (virtual) distance that the weight will lift (their "virtual work" \( F_1 \cdot \delta_1 + F_2 \cdot \delta_2 \) should sum to zero on the grounds that, if one could ever exceed the mechanism's natural mechanical advantage, one could construct, per impossibile, a perpetual motion device). An allied relationship applies within a standard lever arrangement. Likewise, one can presume that, if no frictional losses intervene in the "high striker" pictured, there will be an allied relationship between the mass \( x \) acceleration of the hammer blow across the lever to the mass \( x \) acceleration of the weight to be lifted (this posit is often dubbed "d'Alembert's principle"). In evaluating our sundry devices in terms of their capacities to *transmit work*, we are tacitly relying upon the fact that, in a well-designed machine, the energy losses due to friction will remain comparatively low. If so, we can bypass all of the tricky "Newtonian" calculations that determine how the internal parts within our little machines will press against one another, while yet auguring fairly accurately how effects will be transmitted across the device as a whole.

If the connecting links between these two forms of mechanical tradition had not been deftly forged with the assistance of bridging notions like "constraint force," it is hard to see how physics in a loosely Newtonian mold could have gathered its tremendous fund of descriptive credit. The recognition that a common "classical mechanics" could accommodate the heavens, the factory and the pool table proved vital to its historical triumph over its rivals. But this victory required that the vital strands of pre-existing mechanical tradition get patched together—even at the price of accepting a certain degree of "wedge predicate" dodginess. But the
fact that large battles can’t be won without alliances between local factions doesn’t entail that these tribes must lie in perfect harmony with one another. There is plainly “something right” in the historical manner in which branches of mechanics operating at different scale lengths became welded together through wedge predicate ploys of a “constraint force” ilk, but any precise understanding of the true linkages between these applications requires a far more nuanced discussion than one usually encounters. The trick becomes one of holding the whole edifice of “classical mechanics” together while gradually replacing the crude timbers that have long held up the roof.

Furthermore, critical reassessments of hallowed practices must be made if improvements in descriptive proficiency are to continue. Our central conclusion about “constraints” is that they operate in a fashion that bypasses the constitutive modelings required to connect one scale level properly with another. The fact that such policies can inhibit modeling practices was not widely noticed until workers in the rubber and paint industry began searching for a better understanding of their materials in the 1920's (such issues will be reviewed in section (vi)). The diagnostic terms we have employed—“constitutive modeling” and its cousins—only become canonically certified after Walter Noll’s important work in the 1950's.

As Hilbert observes, the problem that these ubiquitous wedge shifts create for foundational work is that one is apt to evade difficulties that emerge while dealing with one class of objects—point masses, say—by escaping across bridges leading to rigid bodies or continua (in the bead example, we escape the complexities of “constitutive modeling” through a lift to rigid bodies). Such ready alliances between size scales can prove very useful within practical modeling, especially in its early stages, but they may prevent us from attending to the conceptual tensions implicit in the “foundational” scheme we are studying. In the sequel, we shall systematically review the three traditional ontological frameworks for classical mechanics and examine their prospects for displaying enough internal coherence to serve as an adequate “foundation” for all the rest. Continuum mechanics comes out best by these lights, but only at the cost of a rather surprising array of provisos.

Before we do, let us pursue a closely allied scale shift with respect to the phrase “inertial behavior” a bit further. Newton’s first law maintains that an undisturbed “particle” will travel in a straight line (call this doctrine “simple inertia”). In an orthodox reading, the term “particle” is understand as an isolated point mass. But happens if our otherwise undisturbed particle is bound by some constraint in the manner of our bead? Our immediate
expectation, in the absence of frictional contact between bead and wire, is that the bead will maintain a constant speed relative to the wire. This assumption—that objects maintain their speed relative to a frictionless constraining surface—is sometimes called a “principle of generalized inertia” (there are several grades) and most textbooks will assume without much comment that students will observe this dictum when they consider such frictionless systems. As such, generalized inertia appears to be a simple elaboration upon simple inertia, but the general considerations of the previous paragraph indicate that this assumption cannot be correct, for elaborate microscopic arrangements must be set in place before any point mass approximation to “generalized inertia” behavior becomes remotely feasible. Indeed, it is uncertain whether classical point mass physics contains the internal resources to provide any models that genuinely behave in this way, even to rough approximation.

Allied comments apply whenever issues concerned with the redirection of mechanical thrust emerge. As observed above, the material arrangements we usually label as “mechanisms” (watches, locomotive gearing, rope and tackle, the sewing machine mechanism illustrated, etc.) are designed precisely to convert one form of input work (turning the eccentric link at the bottom of our sewing machine apparatus) into a different form of output effort (wiggling the needle back-and-forth at the top). The relative magnitude of the resultant forces will be supplied by a purely geometrical calculation that reflects the mechanical advantage that the mechanism’s current configuration offers. The so-called “principle of least work” allows us to side-step the complicated constitutive processes we would need to examine if we were truly obliged to explain how our mechanism manages to transfer a capacity to perform work across its innards. In real life, this transfer takes time, but such processes are not reflected in the “reduced variable” treatment that standard analytical mechanics offers. The foundationally troublesome concept of “forces that perform work on the system” usually plays a crucial wedge predicate role in encouraging us to skip lightly over these tricky issues of internal transmission.

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

A little further consideration indicates that the best scheme for constructing smaller “reduced variable” sub-manifolds over a complex behavior may utilize a number of covering patches, constructed according to locally different recipes and glued together by some sort of “matching” procedure along their joins (techniques of this ilk appear as “intermediate asymptotics” and allied labels in the technical literature). Such covering recipes often require local adjustment from region to region, just as the optimal scheme for discussing a gas effectively requires a shift in modeling whenever the material becomes excessively rarified. If we now erase the underlying manifold and consider only the collection of “reduced variable” patches remaining, we obtain a funny “theory facade” strung together through sundry forms of border region “matching.” Sometimes the result will display funny “twists,” as in the Escherish manifold sketched above. And these, I think, are commonly the kinds of “facades” one obtains when one erases the quantum mechanical behavior that ultimately supports the successful descriptive techniques of “classical physics.”

There is a second aspect to the invocation of “constraints we should notice: a “generalized inertia” approach to a sliding bead tacitly exploits data drawn from a
mixture of scale levels. In the simplest forms of predicative circumstance, we start with a physical system--say, a swarm of point masses--and, commencing with their initial positions and velocities, plot their trajectories forward in time using a straightforward differential equation modeling. In such "pure" cases, we begin with data representable as points (in phase space) and calculate onward. But when we invoke constraints, we have supplied partial higher level data with respect to where those future trajectories will run (e.g., our bead will always stay on its wire). We are thereby obliged to blend point-based information (the initial conditions of our system) with data assignable to a surface (the locus of beads remaining on wires). If we can perform this mixing trick properly, we can save a lot of unnecessary agony: why should we painfully calculate ab initio where our bead will travel, if we already know part of the eventual answer? The celebrated Lagrangian and Hamiltonian formalisms are specifically designed to take deft advantage of "mixed data" of this ilk. However, subtle problems commonly arise whenever we try to persuade two classes of data to harmonize with one another, for the mathematical character of a "pure" solution to a differential equation may diverge subtly from the characteristics displayed in our incomplete "higher level" information (allied problems frequently occur in matching PDEs to their natural boundary conditions). In certain computational circumstances, such clashes can lead to inferential havoc and therefore merit careful monitoring. Indeed, a lot of subtle research in modern applied mathematics is devoted to such "harmonization" issues and, in certain cases, case, underlying physical doctrine becomes considerably altered through the slow process of figuring out how to make the various kinds of "data" pertinent to applications fit together coherently. Many of the subtle difficulties of continua trace to this kind of "harmonization" problem.

To summarize: rigorously justifying that our "generalized inertia" assumptions genuinely supply "reduced variable" sub-manifolds whose local trajectories closely resemble the underlying motions of legitimate point mass swarms represents a quite difficult task mathematically, as can be seen from the complexity of the constitutive assumptions required to elicit even a crude approximation to "frictionless sliding" in such terms. So it is little wonder, pedagogically, that undergraduate texts regularly evade these justificatory chores through swift chatter of a "wedge predicate" ilk rather than by following Hilbertian derivational practice (unfortunately, such texts commonly pretend that they are being "rigorous" when they bypass the obstacles). More careful books simply reset their "foundations" when they come to rigid and continuous bodies and remark that point masses are best viewed as convenient "degenerations" of more complex objects. And this is a wiser policy, for if any of our basic ontological
options does not function optimally in a foundational capacity, it is the point mass framework. The reasons for this are largely empirical, in a manner I shall explicate later.

We will now serially examine the detailed "foundational" process for our three basic choices for "fundamental object." Our main focus is to isolate the descriptive gaps (and accompanying "escape ladders") that prevent any of our options as happily capturing the expected "worlds of classical physics." We'll find that this lack of "completeness" does not inhibit the descriptive utility of classical modes of representation, although such arrangements run contrary to established philosophical expectations (and to those of Hilbert as well, in his axiomatization project as originally proposed).

(iv)

Point Mass Mechanics. Substantive foundational issues immediately arise if we scrutinize Newton's celebrated "laws of motions" with a critical eye. In their original form, these principles are hard to interpret with any exactitude due to the ambiguous manner in which Newton employs his terms. Here they are in Motte's translation:

Law I: Every body persists in its state of being at rest or of moving uniformly straight forward, except insofar as it is compelled to change its state by force impressed

Law II: The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.

Law III: To every action there is always opposed an equal reaction: or the mutual actions of two bodies upon each other are always equal, and directed to contrary parts.

Look carefully at law I. If a "body" represents an isolated point mass, then the phrase "moves uniformly straight forward" is not ambiguous. But what is the parallel intent if a rotating rigid object can be selected as a "body"? Or a packet within a compressible fluid? One cannot demand that every point within a freely moving boomerang must translate rectilinearly--: at best, they can rotate around some representative center within the full projectile (such as its center of mass). Allied interpretational problems affect Newton's remaining laws as does the
question of precisely where the “impressed forces” are supposed to act. Such unclarities have attracted much grumbling within commentaries:

*Newton had used the word 'body' vaguely and in at least three different meanings. [whereas] Euler realized that the statements of Newton are generally correct only when applied to masses concentrated at isolated points.*

Indeed, the three laws can be readily interpreted only if “body” is read as “isolated point mass” throughout, although this was neither Newton’s intent nor that of the many subsequent writers who have cited the three laws with approval, concurring in the sentiment expressed by Peter Tait and William Thomson (= Lord Kelvin) in their celebrated *Treatise on Natural Philosophy*:

*We cannot do better, at all events in commencing, than follow Newton somewhat closely. Indeed, the introduction to the *Principia* contains in a most lucid form the general foundations of Dynamics. The definitiones and Axiomata sive Leges Motūs, there laid down, require only a few amplifications and additional illustrations, suggested by subsequent developments, to suit them to the present state of science, and to make a much better introduction to dynamics than we find in even some of the best modern treatises.*

But such claims are very misleading. Why have “Newton’s laws” been allowed to stand so long in such a confusing form?

Generally some complicated “philosophy” of “characteristics scale lengths” lies in the background of such tolerance. To understand the salient issues, we must distinguish between genuinely isolated point masses and what should be called “representative points” contained within non-punctiform bodies. The hope is that once we know how a suitable “representative point” choice acts with respect to its wider surroundings, then we will know, to a tolerable degree of accuracy, how the complete swarm of points that it “represents” will also behave. In a well-known exemplar of such “representative point” modeling, Newton utilizes the relatively fixed centers of mass for the planets and sun to serve as “representative centers” for the large swarms of points that actually comprise those bodies. This stratagem addresses the major concerns of celestial mechanics quite effectively, but it remains approximative insofar as real life planets demonstrate appreciable degrees of asymmetry and flexible behavior. For a rigid and freely moving body, the center of mass can often serve as a suitable “representative point,” for the overall motion can then be decomposed into a primary center of mass movement coupled with subsidiary twisting of the remainder of the body around that locale (with a fluid, in contrast, the center of mass lacks this immediate descriptive utility). But if our body is not free but attached to an axis, then other traditional constructions such as
the “centers of percussion” and “oscillation” or “equivalent simple pendulums” may serve our reductive purposes better. In all cases, the selection of a suitable “representative center” can provide an excellent recipe for “variable reduction,” in the sense that the descriptive parameters needed can be reduced to a much smaller number than we might originally anticipate. On rare occasions, the full physics of a problem is preserved under such “representative center” reductions (i.e., if our planets happen to be perfectly rigid and symmetrical). More typically, one merely obtains a good approximation. In the early days of mechanics (before PDEs), many physical problems (i.e., the wiggling beam discussed earlier) could not be formulated in mathematical terms until various “representative center” reductions had been applied, due to the fact that the latter can often be formulated in ODE terms.

To this day, physics primers often rationalize their discussions of point masses through appeal to “representative point” considerations. Thus Thomson and Tait:

[The dimensions of a body are] of no consequence as long as it does not rotate, and as long as its parts preserve the same relative positions amongst one another. In this case we may suppose the whole of the matter in it to be condensed into one point or particle. We thus speak of a material particle, as distinguished from a geometrical point.38

Observe that a tacit appeal to characteristic scale lengths lies latent in such passages: “Suppose that parts P of a larger system S we wish to model at scale length ΔL* remain relatively rigid and do not rotate. If so, we can then model such small parts as ‘material particles’ at a scale length ΔL.” In truth, this claim is both vague (it doesn’t instruct us how the right “representative points” should be selected) and sometimes incorrect (in many instances, the authors later wind up selecting small mechanisms as their ΔL-level replacements for P). Nor does the policy really address the problem of what “Newton’s laws” might require under a “representative point” reading. In an orthodox celestial mechanics setting, a standard point mass reading of “Newton’s laws” can be applied to their centers of mass easily enough, but such requirements become murky when our “representative points” derive from connected systems such as mechanisms or continua.39

Beginning ones textbook with long discussions of point masses might be regarded as an unexceptionable policy if such passages merely served to introduce a convenient approximation technique; but, in fact, Thomson and Tait (and their many pedagogical heirs) also evoke the philosophy of “representative points” as a rationale for addressing the foundations of all classical mechanics from the mathematical perspective of point masses. In effect, such authors approach point
masses as both “approximations” and “foundational objects” at the very same time! In the background of such policies lie nebulous (but widespread) philosophical presumptions assumptions that physics has a right to impose certain mathematical structures upon the world in a voluntary mode: the scientist chooses the depth of scale ΔL to which she wishes to model a target system and selects, partially according to her own aesthetics, the “representative objects” she will employ within her ΔL-affiliated level of modeling. With nineteenth century writers, strong aromas of an associated Kantian philosophy permeate the air. Even today many authors associate the “foundational” qualities of point masses with a fundamental “descriptive liberty” allegedly available to the working scientist.

In my assessment (and, I think, the majority of rigorous modern work on “foundations”), “philosophy” of this favor largely serves as a convenient smokescreen for sidestepping the substantive mathematical obstacles that confront any straightforward attempt to provide coherent foundations for continuum physics. In section (vi), we shall scrutinize some of the popular “lifts” that facilitate such foundational evasions (shifts between “isolated points” and “representative points” are central to such discussions). But in assessing these policies critically, let us never lapse into ingratitude. We should never forget that nineteenth century “classical mechanics” could have never achieved its fulsome descriptive advances had it not hurried past “fundamentals” in these philosophically dodgy ways. The breadth of coverage reached in the Treatise on Natural Philosophy is truly astounding and so criticizing its authors for not employing refined mathematical tools not devised until the middle of the twentieth century is absurd.

For allied reasons, the mere fact that Victorian writers frequently discuss “the laws of classical physics” in a manner that can be interpreted coherently only under an “isolated point mass” reading does not indicate that they actually favored such an ontological vantage point. Lord Kelvin is quite explicit to the contrary:

*We have long passed away from the stage in which Father Boscovich is accepted as being the originator of a correct representation of the ultimate nature of matter and force. Still, there is a never ending interest in the definite mathematical problem of the equilibrium of motion of a set of points endowed with inertia and acting upon one another with any given force. We cannot but be conscious of the one grand application of that problem to what used to be called physical astronomy but which is more properly called dynamical astronomy, or the motions of the heavenly bodies. We have cases in which we have these motions instead of the approximate equilibriums or infinitesimal motions which form the subject of the special molecular dynamics that I am now alluding to.*

He here refers to the Bosnian priest, Roger Joseph Boscovich, who articulated a
clear point mass approach to mechanics in 1758, which was subsequently adopted, to varying degrees of allegiance, by the French "atomist" school later in the century. Later we'll examine some of the empirical reasons why Kelvin believed that Boscovich's day had passed.

In point of fact, even the author who first formulated governing principles for point masses in a coherent manner--Leonhard Euler--rejected that point of view as a viable "foundational" stance:

*Finally, let those philosophers turn themselves which way so ever they will in support of their monads, or those ultimate and minute particles divested of all magnitude, of which, according to them, all bodies are composed, they still plunge into difficulties, out of which they cannot extricate themselves. They are right in saying that it is proof of dullness to be incapable of relishing their sublime doctrines; it may however be remarked that here the greatest stupidity is the most successful.*

The targets of his satire are the followers of Christian Wolfe, who anticipated many aspects of Boscovichian doctrine.

Accordingly, we should often anticipate a tacit reliance upon a "representative point" methodology when we read traditionalist writers on "material points," for it is rare that they truly subscribe to either Boscovich's point of view or to the straightforward point mass "foundationalism" currently endorsed within "classical physics" primers oriented towards the requirements of quantum physics. Plainly, allowing terms like "material point" to bend with such "representative point" vagaries will prove ruinous for any clarification program in Hilbert's vein.

Accordingly, for the remainder of this section, we shall approach the mathematical formalism of point masses from the point of view of someone who truly embraces that ontology and will eschew any dodgy philosophy of "descriptive liberty" that contends that this same framework can be coherently applied in some hazy "representative point" manner to "foundational objects" that are actually rigid bodies or continua.

It is worth observing that Newton himself proved somewhat wobbly with respect to precise content of his own first law, in that he offers as an illustration the fact that a rotating hoop will continue in its angular movements if not acted upon by "outside forces."

*A spinning hoop, which has parts that by their cohesion continually draw one another back from rectilinear motions, does not cease to rotate, except insofar as it is retarded by the air.*

Plainly a tacit appeal to some *generalized* inertia principle is implicated: the activities of the wholly "internal" forces within a rigid body should not affect its overall rotation. Newton, of course, knew that this same claim will not hold for a
flexible object such as the earth or a falling cat. The "rigidity" of the hoop somehow underpins a "lift" that converts an inertial principle relevant to isolated point masses into a requirement upon composite objects operating at the scale size of a hoop. But shouldn't Newton have properly attended to the "constitutive modeling assumptions" that render the internal constitution of a rigid ring different from a cat or a flexible earth? Yes--as we have already observed, such forms of ΔL to ΔL* scale lift pose the same kinds of justificatory problems as arise when we shift from a ΔL-level swarm of interacting molecules to their "averaged" statistical mechanics at level ΔL*.

Modern scholarship generally credits the standard modern reading of Newton's second law (alternatively dubbed the principle of momentum balance) to Euler, who introduces the expectation that "F = ma" supplies the central framework upon which suitable sets of ODE modeling equations for point mass modeling can be assembled (in the sequel this scheme will be called "Euler's recipe for point mass mechanics"). It unfolds as follows. Choose a target system S to model in a point mass mode. Count the number of masses one needs in S. For each i ∈ S, write down the following framework for constructing a well-posed set of (vectorial) ordinary differential equations:

\[ m_i \frac{d^2 \mathbf{q}_i}{dt^2} = \sum \mathbf{f}_i(j, t). \]

In this formula, \( m_i \) is the mass of the particle numbered as "i," \( \mathbf{q}_i(t) \) is its vector location at time \( t \) and the various \( \mathbf{f}_i(j, t) \) will supply the strengths of specific forces applicable to particle i that have their origins in a particle j (for \( i \neq j \)). But this merely lays down the basic scaffolding we will need for a properly completed equational set. In particular, we must yet specify the sundry \( \mathbf{f}_i(j, t) \) in concrete ways that can lead to a set of equations that are uniquely solvable (at least most of the time) with respect to an arbitrary set of initial conditions (supplementary "data" that provides \( \mathbf{q}_i(t_0) \) and \( \frac{d\mathbf{q}_i(t)}{dt} )t_0 \)) information for each particle i ∈ S and some particular time \( t_0 \). To achieve such formal closure within a completed "recipe," each \( \mathbf{f}_i(j, t) \) must represent a special force law that links the strength of the \( \mathbf{f}_i(j, t) \) forces somehow to the \( \mathbf{q} \)-locations of particles i and j (as we'll see, Newton's third law puts sharp restrictions on the nature of this dependency). The basic prototype for a "special force law" of this ilk is Newton's law of gravitation:

\[ \mathbf{f}^G_i(j, t) = (G, m(i), m(j))/|\mathbf{q}_i(t) - \mathbf{q}_j(t)|^2, \]

but allied principles are needed to govern all other applicable forces (such as those responsible for the cohesion and repulsion of matter). Each additional law is expected to carry in its wake its own range of material constants, such as the charges c(i) that show up in the static form of Coulomb's law:

\[ \mathbf{f}^C_i(j, t) = (K, c(i), c(j))/|\mathbf{q}_i(t) - \mathbf{q}_j(t)|^2 \]
(where such c(i) can sometimes carry negative values). We are said to have supplied a constitutive modeling\textsuperscript{45} for the system $S$ in point mass terms once we have specified $S$ 's full complement of particles i and the values of the applicable “force law” constants $m(i), c(i)$, etc. (the list of constants then tells us how many $f_{ij}(j,t)$ terms are “turned on” within $S$).

One of the most frustrating aspects of the classical point mass tradition is that it never fully resolved what these special “force laws” (besides gravitation) should be. Modern molecular modelers frequently utilize sundry mixtures of 6\textsuperscript{th} and 12\textsuperscript{th} power principles between point masses to simulate the molecular interactions within a gas, but no one maintains that such rough rules enjoy any canonical status within classical mechanics. This incompleteness traces to the fact that Nature has indicated no special preference for classical principles governing, say, small scale cohesion and repulsion because she has decided to let matter behave in a strongly quantum mechanical manner in such close quarters. So molecular modelers are left with a rather diffuse collection of “principles that might possibly model close range interaction ably,” with the final choice being decided by “what appears to work.” Indeed, textbooks frequently sidestep the need to fill in the “special force law” holes in an Euler’s recipe modeling through various forms of evasion, such as the appeals to rigid body “constraints” that we have already examined (claiming that a point mass “slides freely along a wire” represents a departure from Euler’s recipe\textsuperscript{46}). Typically, such diversionary appeals tacitly shift us into ontological realms natural to continua and rigid bodies, which approach the problems of “cohesion” in an inherently different manner than currently contemplated.

The absence of enough “special force laws” in the point mass setting engenders another familiar difficulty for point mass mechanics that is usually “solved” by shifting the underlying ontological framework. Suppose we construct a “constitutive modeling” for the solar system, where we treat the sun and planets as point masses and the only special force we turn on is gravitation. The resulting Euler’s recipe equational set will be “formally well-posed” in the sense that we are supplied the right number of equations to potentially possess unique solutions given initial conditions. But that is merely a “formal” guarantee in the sense that it tells us that we are somewhere in the ballpark of getting the solutions we wanted, but doesn’t completely assure us that they really exist. And we have good grounds for worrying about this. Rocket designers appreciate the fact that one can supply a
projectile with a significant increase in kinetic energy by slingshotting it through the
strong gravitational field of a planet (the technique was used several times to
generate enough boost to propel the Cassini space probe to Saturn). Since point
masses have no size and because we've not included any sort of repulsive force in
our equational set, can a particle possibly extract enough energy from its planetary
near approaches to produce an infinite velocity boost within a finite span of time?
This was a famous mathematical question that was settled in the affirmative by
Zhihong Xia\textsuperscript{47} in the 1980's. As such, the velocity "blowup" indicates that the
viability of our point mass modeling has self-destructed of its own accord. Rather
than searching for repulsive "special force laws" that might inhibit the effect,
physicists typically brush the problem aside, "Oh, you've just neglected the finite
size of real planets." Yes--that's true, but they have thereby escaped to the
dominions of rigid body or continuum mechanics in making such appeals.

A somewhat more subtle form of apologetics invokes "hard shell" potentials
around each planetary point mass at a finite distance, ready to supply an impulsive
kick to any invading particle that touches that boundary surface (in the mode of the
invisible "force fields" beloved of scientific fiction writers). Writers who make
such appeals no longer abide by the strict requirements of Euler's recipe and instead
turn off the application of Newton's laws during the instants of impact \(\left( \frac{d^2q}{dt^2} \right) \)
becomes undefined in those intervals). In their stead they substitute various more or
less plausible forms of energetic consideration to settle how point masses will
move away from a collision. Such appeals to "impactive phenomena" are
extremely common in practice and we shall discuss the tactic more fully at the end
of this section, in connection with the "coefficient of restitution" approach to
billiard ball collisions. Probably the best way of viewing such tactics (from a pure
"Euler's recipe" perspective) as smooth but sharply increasing \(f(j, t)\) repulsive
forces that have been crushed into approximating singularities in such a way that
their internal specifics can be overlooked.

The full battery of "special force laws" that point mass mechanics requires is
further skirted by the common practice of presuming that, however the true missing
laws precisely operate, their net effects can be linearized or otherwise simply
approximated as long as their activities are not strong (the telltale symptom of this
ploy are terms in equations such as a linear "\(W_0(x,t)\)" whose "special force law"
origins are left hazy). Many of the descriptive successes of nineteenth century
physics were prosecuted under the guidance of such approximatizing assumptions.
It is to be expected that such modelings will frequent self-generate "holes" in their
descriptive coverage due to the fact that their solution sets can evolve into situation
where the presumptive ansatz that "the activities of the unspecified force law can be
approximated by \( W_q(x,t) \) must plainly fail (thus the effects of gravity can be adequately linearized as long as the relevant cannon balls stay near the earth, but such linearized equations also accept solutions where the projectiles rise to great heights, thereby undermining the presumed conditions of their validity). It is striking that if one inspects the stock point mass modelings provided in popular textbooks, very few of them completely satisfy the provisos of Euler’s modeling recipe and instead invoke some tactic for "special force law" avoidance. Such methodologies of "avoidance" can be prudent in practice, through preventing the merits of a modeling from being held hostage to the delicate specifics of an unproven "special force law."

I’ve already appealed to a basic notion from applied mathematics that is critical here: that of a "formally closed set of equations." This means that one has on hand the requisite number of equations to solve for all of the dependent variables that appear therein (the rough rule is one equation for each additional quantity). As just noted, this count supplies only a "formal" guarantee, for internal failures such as blowups or failures of uniqueness can spoil the expected closure nonetheless. Nonetheless, "formal closure" remains an important requirement and many philosophers (those who glibly talk of "the possible worlds of classical physics," for example) apparently presume that "Newtonian mechanics" in its point mass form possesses adequate resources to achieve the requisite equational closure within its modelings. In real life, such expectations are deeply ill-founded simply because an inadequate number of "special force laws" have been developed, leaving the exact contours of "point mass mechanics" quite nebulous. Students of point mass "classical physics" need to recognize the wide variety of evasive stratagems that have allowed the subject to achieve great descriptive successes, despite the fact that practitioners have never squarely confronted the intrinsic "foundational" holes at the heart of the point mass perspective (as noted before, its rivals possess resources for dealing with "close range contact forces" in a markedly different manner).

From a formal point of view, "point mass mechanics" appears like the emperor and his new clothes: it pretends to wear garments that, in fact, it does not yet possess.

Absent any definitive resolution of what its full complement of "special force laws" \( f_j(j, t) \) should comprise, our Eulerian recipe employs \( F = ma \) as a skeletal frame upon which a formally closed differential equation modeling might be eventually assembled as soon as we locate adequate supplementary skin and clothes for the task (the standard point mass models one encounters in textbooks are formally closed, but various flavors of "special force law" avoidance have probably entered into their framing). Certainly, one cannot coherently discuss whether, e.g., "classical mechanics is deterministic" until these issues of "tolerated force law"
have gotten fleshed out in some fuller manner.  

While on this topic, a few additional words on the term "constitutive modeling" may prove helpful. The term, in its original usage, stems from modern work in the foundations of continuum mechanics. In that setting (where we again have a "recipe" (Cauchy's, analogous to Euler's) that serves as the primary frame upon which PDEs of the requisite sort can be built (in manner we will survey further in section (vi)). In this setting, the analog of our point mass "special force laws" are continuum mechanics' sundry relationships between stress and strain (or strain rate) that capture the behavioral differences between a substance such as iron and water or putty. For example, the usual modeling rules for iron tell us that it resist stretching and compressive stresses in a linear (= "Hooke's law") fashion across a narrow but important range and obeys a more complicated regime outside those limits. Water, in contrast, resists distortion only feebly, and is largely affected by the rate at which applied stresses are applied. Putty, finally, acts according to a complicated mixture of these two basic flavors of response. Since the 1950's, following the suggestions of Walter Noll and others, such relationships have been called "constitutive laws or principles." In historical practice, the need for clear relationships of this class in the foundations of the subject were often obscured by the frequent invocation of various impure strategies for "constitutive principle avoidance" (e.g., appeals to component rigid parts, dimension dropping, linearization and so forth). But difficulties in deciding how very complex materials (like the rubber and paint mentioned before) should be modeled suggested to authorities like Noll that the "impure" stratagems should be cleanly isolated from the "pure" constitutive relationships. We will discuss all of this more fully under the heading of "tasks A and B" in section (vi).

Returning to point mass physics, the local analogs to Noll's purist "constitutive principles" are the special force laws that get "turned on" within a specific Euler's recipe modeling $S$ (which we can alternatively specify by simply crediting the members of $S$ with an appropriate set of "special force law" material constants (e.g., mass and charge)). Once the "material constant" ingredients in a modeling have been specified, then the behaviors that distinguish a point mass treatment of iron from a point mass treatment of putty will have been fully set in place (insofar as punctiform mechanics can provide plausible modeling for either material). Hence "special force laws" represent the natural point mass analog to the "constitutive laws" of modern continuum mechanics. In both cases, we must learn to watch out for "physics avoidance" stratagems that bypass some of the expected ingredients in the relevant "recipe." As we have noted, such "constitutive modeling" evasions frequently take the form of "mixed scale level" lifts in which
the descriptive vocabulary natural to a higher scale $\Delta L^*$ ("slides along a wire") becomes invoked in a manner that allows the modeler to evade the non-trivial constitutive modeling concerns she would otherwise need to confront had she remained resolutely at the original modeling scale $\Delta L$.

Many kind words can uttered in praise of "impure" mixed scale techniques within practical forms of real world modeling, but such interventions typically occasion considerable confusion whenever we attempt to delineate the proper "foundations" of a specific approach to classical physics (from that perspective, we should immediately become suspicious whenever complex $\Delta L^*$ behaviors are invoked without any constitutive modeling underpinnings being provided). Through such innocent-looking "wedge predicate lifts," classical physics, over its long and distinguished career, has managed to cobble by without ever setting any of its potential "foundations" firmly in place (instead preferring to borrow cups of sugar from its ontological neighbors whenever its own resources prove bare).

Let me inject a pertinent remark on the term "law" insofar as it connects with these considerations, in light of the fascination that the topic holds for contemporary philosophy. If point mass mechanics had managed to flesh out its docket of "special force laws" and they had all emerged as "universal" in the manner of Coulomb's law (that is, they always hold but can be "turned off" if the associated charges are zero), then most philosophers would no doubt regard the collection as an unimpeachable set of "laws." But in its parallel "constitutive"endeavors, modern continuum mechanics makes no attempt to whittle its cognate collection of stress/strain principles down to a small set of comparable "universality." Instead, workers in the field merely endeavor to inventory the vast range of constitutive possibilities into useful but hugely inclusive groups: e.g., materials that are completely isotropic in their responses, materials "without memory" that display no hysteresis effects, those that do, and so forth. The basis for attributing a specific set of "constitutive equations" to a material generally trace to direct experimentation within a testing laboratory and do not rely upon any form of internal modeling comparable to those provided under the point mass approach. 49 Much of the animus towards unreliable "molecular modeling" expressed by nineteenth century practitioners such as Mach and Duhem trace to a preference for "derive ones constitutive assumptions from direct experiment, not from dubious 'special force' laws" in exactly the manner that modern writings on continua have adopted. Under this approach, there will be many "constitutive principles" for which exactly one exemplar exists anywhere in the universe and many more that possess no instances at all. Yet none of these behavioral principles can be "derived" from anything more "fundamental" within the framework of continuum mechanics. Insofar as I
can determine, many philosophers now presume, as a matter of unshakable conviction, that “the fundamental laws of nature” must subscribe to sundry “few in number but universal in application” expectations. Well, the Euler’s recipe expectations of point mass mechanics support such a point of view but the “constitutive principles” of continuum mechanics do not (despite the fact that they are commonly dubbed “laws” by its practitioners). So I’ve never understood how “laws should be few in number but universal in application” can be considered an a priori certitude, given that major branches of history “classical mechanics” fail to meet such expectations. In any event, there are several widely discussed aspects of Newton’s second law that merit quick remark. Implicit within our Eulerian recipe is the assumption that sound modeling equations can be set up for every system $S$ based upon their Cartesian locations $q_i(t)$ within “Absolute Space” or, more minimally, with respect to some choice of inertial frame. Newton himself, insofar as I can tell, never quite made such a claim, for he generally set up his equations utilizing what are often called “natural coordinates”—quantities that possess a palpable physical significance within the target system itself. Thus, in the case of a bead sliding along a curved wire, arc length along the wire qualifies as a “natural coordinate,” whereas the bead’s location within an externally defined frame does not. Within celestial mechanics, this distinction enjoys little purchase, but the issue becomes pertinent when material constraints (such as “moving along a rigid wire”) come into play. We shall discuss some of the ramifications of this distinction under “analytic mechanics” later in the essay.

Our recipe also makes the tacit assumption that our locations $q_i(t)$ will always possess smooth derivatives as well, an assumption that Newton himself might have rejected if the issues were clearly set before him (he conceptualized forces in terms of “impulses,” which might sum to derivative-less functions). We have already noted that many writers allow collisions and the like to violate our “smoothness” requirement.

Newton’s third law in its original formulation (as the so-called principle of “action = reaction”) seems patently hazy and has been historically subject to substantially divergent interpretations. In a modern point mass reading, it is usually regarded as placing various strong restrictions upon the “special force laws” we are allowed to employ in Euler’s recipe:

(3a) All forces arise between pairs of particles and have their source in one of the pair.
(3b) These forces are directed along the line between the masses (the forces are “central”) and opposite in magnitude (“balanced”).
(3c) The strength of these forces depends only upon the spatial separation between the bodies and not, say, upon their relative velocities.

In other words, if a special force law claims that mass \(j\) exerts a specific force \(f_j(j, t)\) upon mass \(i\), then \(j\) must exert a reciprocating force \(f_i(i, t)\) upon \(i\) equal in magnitude to \(f_j(j, t)\) but reversed in direction (observe that only action-at-a-distance forces are relevant within a point mass setting, so \(f_j(j, t)\) acts at \(i\)'s position, whereas \(f_i(i, t)\) acts at \(j\)'s position). Although Newton's own law of gravitation suits these requirements, it is unclear that he would have accepted the (3a-c) supplements in the strength stated. Requirement (3c), for example, stands in apparent conflict with most varieties of frictional force because their strength generally depends upon the rate \((dq(x)/dt)\) whereby bodies slip past one another. (3b) seems to rule out sheering forces, such as arise when one layer of water slips over another, or the sideways force that a charged particle feels near a magnetic pole (note, however, that some of these situations only pertain to larger objects in contact along an interface and may not directly concern us now). One of the chief reasons for making such strong restrictions on forces is that they are required to establish vital tenets like Galilean relativity, balance of angular momentum and the conservation of energy within a point mass frame (Newton didn't maintain energy conservation himself). This is because the underlying notion of "potential energy" requires some restriction akin to (3c).

We should observe that "constraint forces," as were introduced in section (iii), cannot strictly meet these strictures either (because they must act in a velocity-sensitive fashion). In truth, such gizmos are conceptually rather different from the action-at-a-distance forces under review here and properly belong to the realm of rigid bodies. We shall discuss them further under that heading.

Partially due to its vaguely expressed contours, Newton's third law often serves as a significant site of substantial "lifts" within mechanics. Let's look at a typical example in the context of a familiar scientific toy: a line of steel ball pendulums lying adjacent to one another. If the ensemble is struck by a falling ball \(b_R\) to the right, it will come to rest and the ball \(b_L\) at the left end will fly off. But as soon as gravity pulls \(b_L\) into collision with the group, \(b_L\) will halt and \(b_R\) will fly off again, to return, more or less, to its original state. And back and forth the knocking oscillations will go, until friction eventually brings the ensemble to rest. And it is natural to conceptualize this situation in this manner. The originally falling \(b_R\)
externally exerts an impactive force upon its first member of the adjacent ensemble \( b_{R,1} \), which then imparts a congruent internal force upon its nearest neighbor \( b_{L,2} \) and so on across the array until we reach \( b_{L,5} \). Since \( b_{L} \) lacks any leftward neighbor upon which to exert a leftward force, it is forced to convert that potential into its own kinetic motion, which will be of the same magnitude as \( b_{R} \) originally possessed, under the presumption that the masses \( m_L \) and \( m_R \) are identical. Expressed in ersatz “third law”-style jargon, we can say: ball \( b_{R} \) originally supplies an impressed external force on the boundary, which excites a spectrum of internal forces in direct contact with another. Because of the third law, these internal forces will exactly cancel each other out in terms of any work they can perform on the ensemble, hence the central packet of balls will display no visible movement. But ball \( b_{L} \) lacks any balancing neighbor, so it is forced to convert the impressed force upon it into its own kinetic movement.

Often related reasoning is presented in a somewhat more elaborate guise. Rather than allowing \( b_{R} \) to fall against the group, let us simply push against the entire group at ball \( b_{R} \) with an applied force of magnitude \( F \). What countervailing force should we apply to \( b_{L} \) to maintain the whole group in equilibrium? \(-F\), obviously. Let us now conceptualize \( b_{L} \)’s so-called “inertial reaction” \( m_L \cdot \frac{d^2x_L}{dt^2} \) as a kind of “force” (until recent times, it was quite common to employ the term “force” in this wider manner). Returning to our original “\( b_{R} \) supplying an external force to the group” case, we can codify our prediction in the guise: \( b_{L} \) will develop an inertial reaction “force” exactly equal in magnitude and direction to \(-F\). In this format, the reasoning of our previous paragraph can be extended: mechanical systems always maintain a kind of “equilibrium,” wherein certain members will counter any unbalanced forces upon them by forming the requisite “inertial reactions.”

The descriptive format just outlined (i.e., “figure out the conditions for static equilibrium first, then use these to compute inertial reactions”) was famously codified by Lagrange in his *Mécanique Analytique* (although most of its chief elements are of venerable heritage). The specific step of equating “unbalanced forces” with inertial reactions is usually called *d’Alembert’s principle*. The presumptions concealed in our thinking about “static equilibrium” here are usually codified in the *principle of virtual work*, which we shall discuss more fully in the next section.

All of this reasoning is well and good in a certain sense, except that (1) its notion of “force balance” has nothing to do with Newton’s third law as we have
interpreted it and (2) reasoning of this type properly requires the realm of rigid bodies for its firm support and can only be regarded as a rough approximative “lift” within the strict context of point mass mechanics. To see what has gone wrong, let us replace our array of pendulum balls with a lattice line of legitimate point masses. (I remember wondering in elementary physics class: “Where did these balls come from? I thought we talking about points”). To reconstruct a point mass substitute for the pendulum-like behavior of the balls, we need (1) some special force law 

\( F_{\text{rep}}(x_i, x_j) \) to generate repulsive force that point i will exert upon point j under close approach and (2) some outside source of attractive force \( F_{\text{att}}(x_i) \) to hold each point mass i within a neighborhood of its lattice rest position. Now apply a force F to the lattice point \( p_R \). What does our third law, as heretofore interpreted, demand? Only that \( F_{\text{rep}}(x_i, x_j) = - F_{\text{rep}}(x_j, x_i) \) and that the unspecified sources of F and \( F_{\text{att}}(x_i) \) should feel reciprocal forces upon themselves. There is absolutely no requirement that the summed forces upon our sundry lattice points i will “perform no work” upon them. In fact, this will generally be false: the initial blow will send waves of compression and expansion through the lattice, at each stage of which small amounts of work will be exerted on each i. It is only if \( F_{\text{rep}} \) and \( F_{\text{att}} \) forces of a very stiff character are posited that we will witness a lattice behavior similar to our pendulum ball expectations. In other circumstances; the blow at \( p_R \) might display negligible transmissive effects at \( p_L \) (e.g., we might see a point mass simulacrum for a line of pendulums composed of putty).

All of these specific requirements upon \( F_{\text{rep}} \) and \( F_{\text{att}} \) fall under our earlier heading of “constitutive modeling conditions.” How did we manage to overlook such constitutive concerns in our original reasoning about our pendulums? The answer is that we inadvertently punned on the term “force balance,” thereby lifting a local point mass requirement on \( F_{\text{rep}} \) and \( F_{\text{att}} \) to the level of visible “balls.” The main “wedge predicate” gambit that facilitates this shift lies in the innocent-looking invocation of a distinction between “external” and “internal” forces, where it appears as if all of the “internal forces” in the pack possess their required “third law” correlates, while the leftward force on \( b_R \) lacks a match, a lapse that \( b_R \) can only rectify through its “inertial reaction.”

There is a celebrated passage in Thomson and Tait which explicitly interprets “Newton’s third law” in this “lifted” manner:

[1] If we consider any one material point of a system, its reaction against acceleration must be equal and opposite to the resultant of the forces which that point experiences, whether by the actions of other parts of the system upon it, or by the influence of matter not belonging to the system. In other words, it must be in equilibrium with those forces. Hence, by the principle of
superposition of forces in equilibrium, all the forces acting upon the system form, with the reactions against acceleration, an equilibrating set of forces upon the whole system. This is the celebrated principle first explicitly stated, and very usefully applied, by d’Alembert in 1742, and still known by his name.\textsuperscript{51}

But if we do that, we abandon some of the original specifics that permit a ready pathway from Newton’s three laws as we interpreted them to the conservation of energy and the like.\textsuperscript{52}

What did Newton himself intend by his “third law”? I am no expert on such matters, but his examples suggest drifts in his own thinking, sometimes straying close to those of Thomson and Tait.

There are other potential “lifts” connected with a off-handed distinction between “internal” and “external” forces that operate in a more subtle manner than this, of which we will survey a few exemplars in the next section. The trick of substituting “rigid balls” for point masses commonly represents a characteristic ingredient within such gambits, as if the ontological adjustment altered nothing except to cloak a dry mathematics problem in a bit of descriptive color.

As indicated before, the strong requirements placed upon the third law are required to support the notion of “potential energy” (and, with it, energy conservation more generally) in a point mass setting. In these regards, the reader should be aware of a standard ambiguity in the notion of “potential energy” that is closely allied to the segregation of forces into “internal” and “external” categories. There are actually two significantly different notions in play, that I shall label as $V(q)$ and $V^*(q^*)$. The first is a notion of “potential energy” that is lodged within our ordinary 3-D Euclidean space, which I mark with the spatial coordinate $q$, while $V^*$, in contrast, represents a function over the complete set of $3n$ locations of the $n$ particles comprising a target system $S$ (in the mathematician’s usual jargon. $V^*$ “lives” in the configuration space of $S$, not in the normal 3-D space where the members of $S$ presumably dwell). A typical $V(q)$ candidate emerges from conventional Newtonian gravitation in the following way. Suppose that we are hoping to plot the movements of various small planets $a$, $b$, $c$ and $d$ within the vicinity of much larger masses $A$, $B$ and $C$ whose positions and strengths are unlikely to be much affected by the movements of the smaller units. The third law officially demands that if any of the $A$, $B$ and $C$ exert forces upon $a$, then $a$ must exert exactly reciprocal forces in return, causing $A$, $B$ and $C$ to wiggle slightly in
response. But if $A$, $B$ and $C$ are ponderous, such reactive perturbations will be quite minute and will become evident only over extremely long stretches of observation. From a mathematical perspective, tolerating that slight wriggling in our formulas renders them quite cumbersome, so it proves extraordinarily convenient to “turn off” the third law reciprocity in their case and treat $A$, $B$ and $C$ instead as “unmoved movers” in their effects upon $a$, $b$, $c$ and $d$. In other words, we artificially “freeze” $A$, $B$, and $C$ into fixed positions for short periods, despite the fact that our third law forbids such arrangements. The advantage we gain from this shift is that $A$, $B$, and $C$’s summed contributions to our problem can then be encapsulated into a simple function of position—$V(q)$—that registers the gravitational force that $a$ will feel if they venture into the location $r$ (the exact rule is $f_a(q) = m_a \, \partial V(q)/\partial x$). The values of $V(q)$ can then be computed from Poisson’s equation based upon the static characteristics of $A$, $B$ and $C$ while ignoring $a$ and company altogether. Because $V(q)$ is a scalar, we can also avoid struggling with the cumbersome force vectors that would be at issue if we hadn’t frozen the large bodies in place. It is common to call $V(q)$ “the gravitational field,” but, for reasons we shall soon discuss, this terminology is potentially misleading. The vital thing to note is, from our present point of view, any treatment of this ilk qualifies as merely an approximation to the “true gravitational physics” of our problem where $A$, $B$, and $C$ are not held fixed and obey Newton’s third law.

To make sense of energy conservation within a point mass setting, we require some notion of “potential energy” to store any apparent energetic loss when the total kinetic energy within our system alters due to the forces between the particles. But the $V(q)$ potential we have just surveyed cannot serve that role due to its direct $q$-dependency, which roots its “potential energy” storage directly to position within ordinary space. But to preserve our conviction that “classical physics” should act in a Galilean invariant manner (= identically composed isolated systems $S$ and $S^*$ should behave exactly alike if they move at a steady velocity relative to one another), we require a “potential function” $V^*$ that does not live at any conventional spatial location $q$. The standard resolution of this difficulty demands that the “true potential energy” function $V^*$ for an isolated system must be a function of time and the spatial separations of its components particles (viz, $V^*$ can only depend upon the factors $|q_a(t) - q_b(t)|$). The existence of such a $V^*$ is guaranteed by the third law requirements articulated above, particularly, proviso (3c). As such, $V^*$ becomes a rather abstract function living within a large and abstract “space” (a configuration space of $3n$ dimensions corresponding to the $3n$-tuple of numbers that comprise the vectorial $q^*$ inside $V^*(q^*)$). The old-fashioned $V(q)$ of the previous paragraph should be regarded as an approximative projection from this multi-dimensional
configuration space” into our familiar 3-D arena.

As such, we must beware of confusing \(V(q)\) with \(V^*(q^*)\) in a point mass setting—the latter represents an exact and “fundamental” quantity within the scheme, whereas the former merely qualifies as an approximate trick. Textbooks often loosely label \(V(q)\) as “the gravitational field” but, strictly speaking, no such “field” is truly compatible with the punctiform mechanics outlined here. In fact, true fields that live in real space don’t integrate well with our point mass ontology. The usual pretenders otherwise are merely pseudo-“fields” obtained from some approximating projection from \(V^*\) into ordinary space (there are a number of recipes for doing so). Prospects for confusion are rampant here, because true fields are permitted within a continuum physics approach (indeed, virtually every important object within that foundational scheme qualifies as a “field” in the true sense). If we are careless about maintaining a constant foundational basis, nebulous appeals to “potential energy” and “fields” can easily generate considerable muddle with respect to the third law requirements just outlined. The altering ontological qualifications of “fields” as we shift from point mass mechanics to continuum physics shows that one man’s “approximating object” can prove a “foundational verity” to someone else.

Historically, these issues have become further confused by the fact that the developmental route leading to our modern sense of the conservation of energy proceeded through the initial employment of approximate potentials such as \(V(r)\) as a productive mathematical “trick” skillfully exploited by Laplace, George Green and others. It took a while before our distinction between \(V\) and \(V^*\) “potentials” was clearly recognized.

There is an allied aspect of our \(V(q)/V^*(q^*)\) distinction that is worthy of remark, even if it unwinds somewhat tangentially to our main concerns. In the foregoing, we have tacitly assumed that the systems of point masses \(S\) under consideration can serve as “complete cosmologies” in the sense that a rich \(S\) might potentially serve as an all-encompassing model of an entire universe. But perhaps this “complete cosmology” assumption is a mistake; perhaps the universe is so complicated is that all we can ever do is describe some of its interior parts provisionally, subject to a “cut” where we temporarily distinguish an interior swarm \(S'\) from the collective effects of the remainder of the universe \(S\), whose contributions can only be captured in blurred \(V(q)\)-like terms. Often such “cuts” can be identified with geometrical divisions \(C\) that partition all of space into
“internal” and “exterior” portions. From this point of view, spatially situated potentials \( V^C(q) \) will enjoy a less eliminable role in our descriptive scheme than previously anticipated, because every formula we write down must mention \( V^C(q) \) (or one of its close cousins). To be sure, we can always improve the accuracy of a specific modeling by moving the requisite cut from \( C \) out to some further contour \( C^* \), thereby replacing \( V^C(r) \) in our equations with an improved \( V^{C^*}(q) \) (in the illustration, cut \( C \) isolates the earth/moon system from its two-sided “exterior,” whereas \( C^* \) moves its demarcation surface beyond the asteroid belt). But we should never expect, according to this new point of view, that any cut can be fully moved to spatial infinity, thereby obtaining a complete “cosmological description.” No; some localized variety of “cut” must be set in place before any descriptive work in physics can begin. If so, terms of \( V^C(q) \)-type must now appear in all of physics’ basic formulas, alongside regular potentials of \( V^*(q^*) \) type. The latter satisfy our third law demands but the cut-dependent \( V^C(q) \) are absolved of such requirements.

Indeed, a hoary controversy once raged as to whether “Newtonian physics” tolerates infinitely populated universes at all, worries that trace to “Olbers’ paradox”-like concerns.\(^{56}\) Insofar as the Euler’s recipe formalism surveyed in this section is concerned, the answer is “no,” simply because the ODE’s thus constructed are tacitly required to be finite dimensional. The most promising route to greater permissiveness is to work with progressive cuts in the manner suggested, so that a stable infinite universe can emerge beyond their expanding horizons as a limit. But rendering these progressive provisions mathematically coherent is not a trivial matter.

Whether or not physics requires descriptive “cuts” of this ilk emerges as a methodological theme in a number of contexts, such as:

1. the Kantian presumption that physics can only supply “regulative ideals” for gradually improving (but never completing) current modes of concrete description;
2. the Machian recommendation that the universe’s “compass of inertia” should be associated with the cut between \( S^E \) and \( S^I \) modelings\(^{57}\);
3. the Bohrian thesis that local quantum \( S^I \) modelings are intrinsically dependent upon cuts where some “complementary” swatch of reality \( S^E \) must be described classically.

It is not my intent to pursue these issues further, but, obviously, how one addresses these concerns will affect the manner in which one approaches “foundational questions” generally.

As indicated earlier, most physicists had firmly abandoned the point mass approach by 1850 or so, only to be revived in the twentieth century as offering the
easiest pedagogical bridge to quantum theory. Why did this happen? A number of salient considerations can be extracted from the wonderful articles that Clerk Maxwell composed for the celebrated ninth edition of the *Encyclopedia Britannica*. Many of this troubles trace to the simple fact that natural materials vibrate in the manner that spectroscopy indicates and can transmit waves. But attempts to construct point mass lattices capable of imitating the experimentally determined behaviors usually proved disappointing, whereas models constructed upon continuum or rigid body principles did much better. For example, in the 1820's Claude-Louis Navier had developed a celebrated point mass model for elastic materials leading to substances whose macroscopic behaviors are characterized entirely by their Young's modulus (more generally, Navier's methods give rise to what were then called "rari-constant theories" of elastic materials). Working from general principles in a top-down, continuum mechanics mode, Cauchy instead concluded that isotropic elastic materials require two independent constants (Poisson's ratio in addition to Young's modulus) to fix their behaviors rather than Navier's solitary value. These issues were of great scientific moment because the varieties of wave that can travel through an elastic material are intimately linked to these constants. After a long period of controversy, Cauchy's "multi-constant" predictions were eventually confirmed by experiment. By the end of the century, it was widely presumed that Nature was composed of continua of some sort, with its apparent point-like "particles" comprising whirlpool-like structures within an underlying continuous medium.

Cauchy didn't fully appreciate the methodological advantages of the approach he initiated (he sometimes worked in Navier's "bottom up" mode as well) but later writers such as Green and Stokes strongly emphasized the merits of the top-down approach, which eventually became the core construction within modern continuum mechanics (in a manner we shall survey in section (vi)). To this day, Cauchy's top down techniques generally supply more reliable models with respect to the materials of macroscopic experience. In fact, many of the celebrated philosophical percepts developed by writers such as Pierre Duhem and Ernst Mach in the late nineteenth century trace, in part, to their appreciation of the descriptive superiority of Cauchy-style methods. More recently, the rise of swift computers has rendered the project of working directly with point mass swarms in a bottom up manner a more viable enterprise, but the results obtained are generally more suggestive than accurate. Shortly after Cauchy's work, Poisson was able to reproduce the "two constants" predictions from a molecular model comprised of attracting spheroids rather than point masses. Likewise, one obtains better results within molecular simulation today by working with swarms of extended bodies.
rather than points (although the computational costs are much higher). But, from a foundational point of view, these modeling adjustments transport us into the realms of rigid body mechanics, which we shall canvass in the next section.

Some folks, however, become so smitten with point masses that they strive mightily to found “classical mechanics” upon that basis, no matter how physically implausible the constructions employed may appear. Thus we might theoretically piggyback upon Poisson’s “two constant” results by collecting large swarms of point masses into mock spheroids held together by strange attractive forces. But such assemblies bear no relationship to any structures present in real life materials (whereas Poisson’s spheroids often do). I’m not sure what one gains from seemingly vain enterprises such as this.

A logical observation is pertinent as well. When one strives to explain why modeling principles $\mathcal{P}^*$ work well at scale level $\Delta L^*$ based upon the principles $\mathcal{P}$ operative at $\Delta L$, one is further obliged to explicate why the $\mathcal{P}^*$ principles operate over the full range that they do. It is often easy to construct specific “toy models” at a $\Delta L$ level that will implement the desired $\mathcal{P}^*$ behaviors at the $\Delta L^*$ scale, but one little skirmish does not win a war. At best, one has merely built what the Victorians called a $\mathcal{P}$-principle analogy to the $\mathcal{P}^*$ events. To be sure, the construction ensures that some of $\mathcal{P}^*$’s ontological claims are technically compatible with $\mathcal{P}$, but this signifies comparatively little if the supportive “analogies” require such elaborate contrivances on a $\Delta L$ scale that they cannot serve as general underpinnings for the higher scale behaviors.

I would have presumed that this observation was so obvious that it is scarcely worth drawing, but several times in the past year I have heard philosophers proudly declare that they have “derived the Navier-Stokes equations” (or the like) upon a more elementary basis, when, in fact, they had merely concocted a weak and contrived analogy to such a system (by such standards, one can probably “found” the same equations upon The Pickwick Papers). There are many loose claims afloat within the philosophical world as to how the various branches of physics allegedly “reduce” to each other; readers should approach most of these with a wary eye.

There is a final issue we should survey before returning to our main themes. As I have explicated our Eulerian recipe, it fails as a modeling as soon as quantities like acceleration lose their required features. But this is exactly what happens if, e.g., a point mass runs into another point mass or into one of the “hard shell” barriers discussed earlier. From a strict mass point perspective, one shouldn’t
tolerate acceleration-destroying interactions of these kinds and writers like Boscovich hoped that someday smooth close range repulsive forces would be found that could prevent point masses from truly colliding. But fulfilling this ambition in a plausible manner is not easy (and we must furthermore tame the additional “blowup” problems that emerge in the Xia construction mentioned above). In real life modeling practice, “impactive” encounters between point masses are usually addressed through ad hoc remedies that temporarily relax our Euler’s recipe requirements, rather than searching for elusive “special force laws” according to Boscovich’s urgings. In fact, Newton’s own approach to billiard collisions implements this basic “turn off the laws temporarily” stratagem. He surrounds the center of each ball with a crisp finite boundary (so that the central mass point is credited with a “hard shell potential,” although utilizing that vocabulary is quite anachronistic in application to Newton). Whenever these radii contact one another (we shall only worry about the head-on collision case), Newton abandons the requirement that the “a” in “F = ma” must make sense and shifts his focus to the two balls’ incoming stores of linear momentum and kinetic energy (as we now dub them), together with a purely empirical factor called a “coefficient of restitution” (it governs how much the total kinetic energy budget will diminish post-collision). In effect, this treatment blocks out the crucial interval of time Δt where “F = ma” fails to sense and glues together the incoming and outgoing events exterior to Δt through a mixture of conservation principles\(^{60}\) (conservation of linear momentum) and raw empirics (“coefficients of restitution” extracted from experiment). Formally, tactics that patch over problematic intervals or regions in this manner are frequently called “matched asymptotics.”

Whenever I hear Newton’s views characterized as “billiard ball mechanics,” I am bemused, because the treatments he enunciated don’t tell us what really occurs inside such balls during the Δt interval (in reality, they compress and reexpand, but Newton’s treatment ignores all of that). If we desire a story about what happens inside Δt, we must look to continuum mechanics, where the processes turn out to be frightfully complicated. In effect, Newton has merely kicked the can of “the billiard ball problem” down the road to another time and another formalism. Crediting him with “solving” the problem is frequently the source of substantive methodological muddle.

As soon as objects possessing finite geometries (i.e., rigid bodies or continua) are brought onto the scene, the probabilities of impactive encounters that destroy derivatives become much greater. Most textbooks, old and new, include some discussion of “impactive integrals” allegedly to cover situations of these types, although what is often supplied is formalist mumble-jumbo. The better discussions
usually appeal to what are now called "weak" or "variational" reformulations of the basic laws. Let's take "\( F = ma \)" (in one dimension, for simplicity) and consider how its range of applicability might be enlarged through appeal to integration by parts. Specifically, multiply \( md^2x/dt^2 \) by some arbitrary function \( \delta h \) and integrate the result over a short interval \( L \). Integration by parts on the right then tells us that
\[
\int_L (md^2x/dt^2 \cdot \delta h) \, dx = [mdx/dt \cdot \delta h]_L^L - \int_L (mdx/dt \cdot d\delta h/dt) \, dx.
\]
Notice that one of the derivatives originally in \( md^2x/dt^2 \) has shifted over to \( \delta h \) in its second term. Now chose \( \delta h \) so that it vanishes at the endpoints of \( L \). Result:
\[
\int_L (mdx/dt \cdot d\delta h/dt) \, dx. \quad \text{(for all suitable \( \delta h \))}
\]
This is the expression we should employ to define the "impactive force" expended over the interval \( L \). Working with notions such as this (and its more sophisticated "distributional" cousins), one can reformulate point mass mechanics in a broader manner that can tolerate certain forms of impactive phenomena without cheating (e.g., blanking out critical time intervals). In doing so, we find ourselves relying upon significantly more sophisticated mathematical tools.

The one thing we don't want to do, if we take Hilbertian percepts on rigor seriously, is to regard our integration by parts manipulations as proof that the usual "impact avoiding" procedures for handling billiard ball impact logically follow from our original reading of Newton's laws. No: when we engage in such procedures, we are surreptitiously "lifting" our original set of doctrines into the arena of a potentially stronger formalism. Our "weak solution" formulas need to be viewed as extensions and corrections of our former doctrines, not as a "consequence" of them (our integration by parts exercise is merely suggestive of how the term "force" might be handled in a replacement format). The fallacy in presuming otherwise is easy to see. The particle trajectories governed by old "\( F = md^2x/dt^2 \)" must always possess two derivatives and so our manipulation merely shows what will happen to functions of that type over little time intervals \( L \). But the original purpose of our exercise was to determine what should happen when the trajectories of our masses lack second derivatives (as occurs when they bump into one another). Obviously, our old reading of Newton's laws can't possibly answer that question for us in the manner we seek, because their actual content claims, "But you can't have trajectories like that!" To which we can properly rely, "Yes, I know that you don't like trajectories and forces with fewer derivatives, but our little 'integration by parts' manipulation suggests that I might begin afresh with 'foundations' able to tolerate the condition you abjure."

This formal observation will become especially pertinent in the next section, because there we shall be centrally concerned with variational formula (i.e., equations with little \( \delta h \)'s in them). Such formula are almost always stronger in
import than comparable nonvariational schemes, although one would never know this from the way they are introduced within a typical physics primer, by “multiplying” already accepted formulas with sundry δh’s. Once again, the results in practice are applied to situations cases beyond the reach of the original principles. As such, such off-handed introductions disguise significant “lifts” in doctrine comparable to an increased capacity to cover impactive phenomena. In rigorous mathematics, you can’t get something from nothing (or even a few δh’s). Better books on the topic confess that they are actually starting over in their “foundations.”

Of course, a central reason why point mass modelers aren’t especially troubled by these “relaxations” in doctrine is that, in such applications, they have already adjusted their thinking tacitly to rigid bodies or continua: “Oh, we know that real billiard balls aren’t point masses with hard shell potentials; they genuinely fill space. So let’s see if we can’t adapt our old formulas to suit such purposes” (probably “representative point” percepts lodge in the back of their minds already). Finding an easy “lift” through integration by parts manipulation eases the pressure to set their ontological houses in sounder order. Likewise, the assimilation of Newton-style celestial mechanics (roughly equipollent to the point mass modelings considered here) to the physics of the billiard table across the innocent-looking bridge of a “coefficient of restitution” generates an illusion of wider common coverage than can be properly justified. But, as stressed before, if the broad banner of a shared “classical physics” had not been stitched together through these shady connective expedients, the composite could have never developed its vital—and genuinely merited—impression of broad “family resemblance” unity. The conglomerate’s triumph represents a great step forward in human thought, but one shouldn’t underestimate the blankets of philosophical misunderstanding that spring up like kudzu as the unintended side effects of these amalgamative efforts.

Such issues are further complicated by the historical fact that Newton and most of his early followers did not conceptualize forces as acting in the smoothly varying manner required under our Eulerian recipe. They viewed gravity’s apparently continuous activity as the cumulative result of impactive pings similar to the hammer blows of elfin carpenters. From this vantage point, our dubious impactive forces assume pride of place as a central “foundational” construction. Accordingly, the occasional relaxation of “F = ma” demands will not seem a matter of great doctrinal moment (attitudes such as these prevailed far into the late nineteenth century). In so thinking, the underlying conception of “force” has been “dragged” to accommodate the contact forces that naturally emerge within the contexts of rigid bodies and continua (no such items trouble the pure action-at-a-
distance landscapes of Boscovichian physics). In the next section, we’ll see why “contact forces” secretly comprise creatures of a different stripe than the \( f(i,j) \) forces just surveyed. Many of the deepest conceptual tensions inherent in classical tradition trace to this fundamental disharmony.

Before we turn to these issues, let us extract the central morals of our discussion from the underbrush of specifics. The main descriptive “holes” within the point mass approach trace to the absence of the “special force laws” that would be needed to complete its Eulerian recipe for ODE model construction. It is hard to repair these lapses with any assurance because the missing laws concern the nature of close range cohesive forces and Nature offers few robust indications as to how a classical point mass modeler should tackle such phenomena. In consequence, our underlying “recipe” lacks many of the ingredients it would require before it could ratify, upon a purist mass point basis, the many non-punctiform modeling techniques that practitioners regularly employ at higher \( \Delta L^* \) scale lengths. In pedagogical practice, these lapses in “constitutive modeling” are frequently disguised by covert “lifts” to alternative approaches better suited to the \( \Delta L^* \) level techniques. But conceptual complications within those rival schemes encourage frequent retreats back to the stolid redoubt of point masses, where the conceptual setting—if not the livin’ itself—is easy. The result is an intellectual landscape pockmarked with easy lifts and quick escapes that can seem quite perplexing if your physics instructor assures you that everything you see view is rigorously wrought and intellectually beyond reproach.

\( (v) \)

**Rigid Body Mechanics.** Let us now investigate the foundational prospects for a physics resting squarely upon a basic ontology comprised of rigid bodies interacting through contact. There are a number of somewhat different treatments available in this arena, falling under the generic heading of “analytical mechanics.” Our plan is to no longer analyze such bodies as swarms of point masses, nor to allow them any internal flexibility (that will be the task of the continuum picture). Accordingly, when our ensembles of rigid parts flex, it must be through the internal realignment of completely stiff components, maintained as a coherent collection through an admixture of action-at-a-distance attractions and directly contacting linkages such
as hinges, pins, wires, etc.⁶⁴ The rigidity of any part is mathematically expressed by
the fact that its current location can be completely fixed by six numbers (three
Cartesian coordinates to locate a representative point within the body and three
angles to indicate how the figure has rotated about that point). Any contact
connection between such parts (e.g., part A is hinged or pinned to B or is forced to
slide within a slot carved in B) is usually called a “constraint” and are expressed as
“constraint equations” that interrelate the coordinates of the
sundry parts⁶⁵ (in many cases—but not always—these
constraint equations will be algebraic in character). Two
useful paradigms for the systems under review that I shall
often cite are (i) a bead sliding frictionlessly along a rigid
wire and (ii) the sewing machine mechanism illustrated.

I should here indicate, for later purposes, that in
practice the term “analytical mechanics” often tolerates a wider range of flexible
elements that can also redirect thrust in a controlled way, such as pulley ropes and
conveyor belts. It is likewise common to extend the formalism to embrace
formulaic forms of one-dimensional continua, especially Hookean springs. But
we’ll ignore these supplements for the time being, as they won’t materially affect
our discussion.⁶⁶

One doesn’t expect a normal lattice of point masses to remain completely
rigid when disturbed—the gentlest attempt to move them en masse is likely to send
little waves of disturbance across the swarm. But if we can safely assume that
substantial hunks of a mechanism remain approximately rigid in their gross
movements, we can potentially ignore a huge amount of internal complexity within
the device. Consider our sewing device, whose bottom eccentric link is turned by a
motor. If we were forced to model these arrangements explicitly as a swarm of
strongly attracting point masses, we would need to painstakingly plot how the
binding forces allow the inputted movement to gradually transmit itself from one
little piece to another across the mechanism. This story must surely involve very
complex processes in light of the branching causal pathways that initiate at the
motor. As observed earlier, it is scarcely evident that orthodox point mass
mechanics contains enough internal resources to provide an adequate simulacrum of
the expected behaviors. But once we are assured that the component pieces will
remain nearly rigid throughout all of the device’s ordeals, high school geometry can
compute exactly how much the needle will wiggle as the drum at the bottom gets
turned through an angle θ. Admittedly, this isn’t a trivial “high school” calculation,
but its demands are vastly simpler than computing how the whole point mass swarm
will behave under the same conditions. In other words, we can employ our upper
scale $\Delta L*$ knowledge that our sewing machine parts stay rigid and obey their connective constraints to avoid the very complicated mechanical relationships that hold amongst the device’s component masses at the $\Delta L$ level.

Or, at least, that is how our target mechanism would appear from a point mass perspective. But in the present section, we wish to consider “foundations” for classical mechanics in which notions like “rigid body” and “pinned constraint” comprise the mechanical primitives of the subject and are not introduced as convenient approximations to complex point mass underpinnings.

The convenient imposition of $\Delta L*$ rigidity further allows us to compute the mechanical advantage of our sewing machine mechanism in its present configuration (this varies according to its position $\theta$ within the full turning cycle of the bottom drum). That is, if our motor applies a torque of $X$ newton-meters to the drum through an (infinitesimal) angle of $\delta \theta$ degrees, then the needle can push a load of $Y$ newtons through an (infinitesimal) distance of $\delta m$ meters, where the values of each relate according to the “virtual work” relationship $X.\delta \theta = Y. \delta m$ (the last pair represents the outputted work exerted by the device over the relevant frame). This “work” relationship, of course, is the exact analog of those we discussed earlier with respect to levers and blocks-and-tackle. We shall discuss the “virtual work principle” more fully later.

All of this supplies analytical mechanics with a huge computational advantage over point mass based modelings. Thomson and Tait articulate these virtues as follows:

[T]he forces which produce, or tend to produce, [the actions] may be left out of consideration. Thus we are enabled to investigate the action of machinery supposed to consist of separate portions whose form and dimension are unalterable. 67

In discussing “generalized inertia” earlier, we likewise noted that it is hard to model, from a point mass perspective, the simplest forms of redirection of thrust, as occurs when a plug slides along a curved track. In our sewing machine, the “redirection” implemented is of a far cleverer design, but proceeds according to the same analytical mechanical principles.
To formulate doctrines of this type correctly, we generally need to capture the system’s current configuration in terms of so-called “generalized coordinates,” rather than the Cartesian x/y/z coordinates that are central to the mass point reading of the second law. Often good choices for these utilize “natural coordinates” in the sense mentioned earlier: quantitative measures of displacement that are closely correlated with the system’s available motions. For a plug sliding in a slot, its placement in terms of arc length along the slot represents the single “natural coordinate” we require to fix the plug’s position, whereas its three locations expressed in x, y and z terms don’t relate to the motion in any internally “natural” way. A second vital feature of the coordinates usually employed in analytical mechanics is that they are independently variable with respect to each other: any specific coordinate can be altered without necessarily disturbing the others. With a steam shovel, for example, we will want to employ the five decompositional movements illustrated to fix its configuration, rather than the regular Cartesian coordinates of its many parts because the latter are descriptively entangled in a manner prevents us from applying the usual forms of “virtual work” reasoning. Much of the practical success of analytic mechanics traces to the fact that suitable independent coordinates for a complex system can often be divined simply through experimentally determining how it wiggles under manipulation and the directions in which inputted thrust travel across its interior. Such data represents raw higher scale information about our system’s dominant behaviors.

As indicated earlier, as soon as material bodies genuinely fill finite volumes, a new type of “force” quietly enters the scene that eventually becomes a secret source of significant tensions within mechanical thinking. Since Bosovichean point masses are inherently zero dimensional in nature, they can be provided with a surrogate for normal “size” only by erecting rough “effective volumes” through a battery of strong, short range repulsive forces (as noted, there is no canonical procedure for doing so). But if our “fundamental” objects possess true sizes, then the contact (or traction) forces will arise upon the interface between two contacting bodies (and, in many natural situations, prove the most important of the forces applied). These new
items can no longer qualify as *action-at-a-distance forces* simply because no distance separates the embedded points where the transmission occurs\textsuperscript{68}. There are two grades of contact forces with we must eventually deal. The first are the *boundary traction forces* applied along the outside surface $\partial B$\textsuperscript{69} of a body $B$, such a loaded weight passively resting on top of a four bar mechanism or a hammer blow applied somewhere (the first is traditionally called a *static* or "dead" *load* and the second a *dynamic loading*).

These are the only contact forces at issue within rigid body mechanics. But when we turn to flexible continua, a second grade of interior contact forces emerge in the guise of the *traction forces* that appear across the boundary of (almost) any internal surface $S$ that we might mark out within the larger body $\mathcal{B}$. Such internal surfaces $S$ are commonly called *free body diagrams* or *Eulerian cuts*. Each such $S$ will bristle with an array of *traction forces* that point either inward or outward at each surface point-- it is then presumed in third law fashion that the material outside $S$ (often designated $S'$) will push or get pulled in the opposite direction at that same place. The most familiar exemplar of such traction vectors are the normal *pressures* acting within a non-viscous fluid, but the complicated internal pushes and pulls operative within other flexible bodies mandate the introduction of the more general notion of *stress*. The entire affair of internal contact forces raises a fair number of subtle issues which will canvassed in section (vi).

But once the interior of a body is claimed to be completely rigid, as we shall assume throughout the present section, then this interior grade of "contact force" becomes ill-defined in consequence, as do allied notions such as "internal pressure." So we will not worry about how to deal with such internal tractions now and will concentrate upon the surface forces that appear along the boundaries between contacting bodies.

It is common for elementary textbooks to vaguely claim that all forms of "contact force" really represent "short range cohesive forces between separate particles." This contention might be true insofar as "contact forces" represent *classical distillations of quantum processes* of roughly that character, but such asseverations can prove very misleading insofar our text appears to be concerned with classical processes exclusively, where it is not evident that plausible."short range cohesive forces"of a classical point mass character can ground, on a $\Delta L$ basis, the standard rigid body or continuum behaviors witnessed upon a $\Delta L$ scale (why? try to find a point mass modeling that can duplicate the surface pushes and pulls.
usually tolerated within an analytical mechanics framework). In fact, such off-handed appeals to “short range forces” often disguise the fact that fundamentally new issues on “how forces operate mathematically” appear on the scene as soon as “contact forces” are tolerated and need to be addressed coherently. Often their resolution requires that we reinterpret “Newton’s laws” in a significantly altered manner or turn to other forms of “foundational principle” altogether. In the face of these conceptual challenges, hazy appeals to “fictitious short range forces between point centers at a lower scale length $\Delta L$” merely serve as convenient escape hatches that allow authors to evade addressing these “foundational” issues squarely (these evasions become particularly troubling in the context of continua, as we shall later see). To be sure, the texts eventually stagger their way to the requisite $\Delta L^*$ level equations, but only along ersatz “derivational” pathways that are apt to confuse a critical student.

One of these difficulties—which arise even with the “exterior boundary” forces of rigid body mechanics—traces to the simple fact that there is a disparity in dimension between contact forces that act upon bounding surfaces $\delta B$ and forces such as gravity that act upon the localized points inside $B$ (the latter are usually dubbed “body”or “volume” forces to mark the distinction). As soon as our attention shifted to extended bodies, we should have properly stopped calling such items “forces” at all and instead considered “force densities” of dimensionally incompatible grades (thus I should have written of “surface force densities” rather than “surface forces”). The motives for these adjustment trace to the usual difficulties of making sense of continuously distributed quantities that date to the time of Zeno. Suppose we have a target with a bulls-eye and two archers: skilled Marian and inept Robin. What are Marian’s and Robin’s respective probabilities for hitting the exact center of the target $c$? Answer: most likely zero in both cases, because if the “hit $c$ exactly” answers were credited with any finite amount $\varepsilon$, then (under the assumption that points near to $c$ should be credited with probabilities close to $\varepsilon$) the summed probabilities of hitting any finite region of the target will become infinite (due to the infinity of individual points contained in such a region). But if Marian’s and Robin’s probabilities for hitting the target at any individual point are always zero, shouldn’t it follow that their summed probabilities of hitting any finite region $A$ also need to be identical (viz. zero), rendering them equally lousy marksmen? Obviously not, but the task of straightening out these riddles is the
business of the modern theory of measure which addresses the problem by crediting Marian and Robin with different probability densities with respect to the individual points in the target. To extract a proper probability from a “density,” one must “add up” (= integrate over) these densities over sufficiently large areas. Based upon their different archery densities, the true probability differences between Marian and Robin’s skills will show up only after sufficiently large expanses of target come into consideration. Getting all of this to work out correctly requires very careful mathematical preparation.\textsuperscript{50} Plainly, we need to adopt similar policies with respect to our new “forces”: considered at a point-size level we expect to see only non-zero force densities--true “forces” shouldn’t emerge until we have integrated these local densities over larger regions.

The awkward tension that segregates “surface forces” from “body forces” such as gravity stems from the fact that, considered properly as densities, their respective quantities must be dimensionally inharmonious. Why? In the case of the tractions pulling and pushing upon a boundary $\delta \mathcal{B}$, we expect to reach genuine resultant forces after we have integrated over finite stretches $\delta S$ of the full surface $\delta \mathcal{B}$. But with gravitation, we must integrate over volumes $V$ of points within $\mathcal{B}$ itself (not just along stretches of $\delta \mathcal{B}$) before we can assemble forces of comparable strength from gravitational attraction. Considered from the point of view of the normal “volume measure” on $\mathcal{B}$, any surface piece $\delta S$ will qualify as “of 0 measure,” so we can’t use this same measure in dealing with contact forces. In sum: genuine forces can be assembled from much smaller sets of points in the case of a contact force than gravity, for they need to reach the level of a “finite resultant force” more quickly in the former case.

It is only after these surface and volume resultants (the fat arrows in the diagram) have been obtained that we will possess gizmos (genuine forces, not densities) that can be meaningfully combined (how we should do this remains to be seen). As we’ll see, the problem of adjusting the dimensional disharmonies between surface and body force densities becomes particularly acute within a continuum mechanics context, where “traction force densities” must be summed along stretches of internal surfaces, in addition to the outer boundary integrations we must perform with respect to rigid bodies.

When we eventually consider the accelerative response (or “kinetic reaction”) of points within our body $B$ (i.e., $\rho \frac{D^2 \mathbf{r}}{Dt^2}$ where $\rho$ is the mass density
and $D^2/Dt^2$ is the so-called “material derivative”), this density is often conventionally classified as a “body force” as well, in light of that fact that these quantities must also be integrated over volumes before an “inertial reaction” comparable to the “ma” in our old friend $\mathbf{F} = ma$ is obtained. The wisdom or folly of grouping “kinetic reactions” together with “forces” is a delicate matter, because the policy encourages of altering the significance of “action = reaction” in the manner criticized earlier.

This dimensional disparity of our “densities” is not merely an awkward mathematical issue, for the fact that “surface forces” inherently overwhelm “body forces” within small regions plays a vital role in determining the logical character of vital notions like “stress.” We shall discuss these features in section (vi).

Let’s return to the problem of combining “body” and “surface” forces, now construed as densities. If our ensemble of body points can be welded into a wholly rigid body, Euler proposed a beautiful solution to our difficulties. Perhaps an analogy might help, before we consider the specifics. Suppose we have the task of heading a large herd of cattle along a plain. In the general case, persuading unruly steers to head in the right directions will require a lot of cowboys. But suppose that our steers always maintain a fixed formation relative to one another, so that they, in effect, move as a rigid group. If so, we only require two cowboys to manage our herd: cowboy #1 to push some arbitrarily selected central steer $A$ in the desired direction and cowboy #2 to turn some other cow to the desired orientation around $A$. The remaining cattle, forever loyal to their fixed formation, will accordingly follow the guidance of the two lead cows. Here we might call the line connecting cow $A$ to some other selected cow “the lever arm” of cowboy #2's turning efforts.

Translating these conclusions into mechanical terms, we find that two basic gizmos are needed to fulfill the roles that “total force” alone serves within point mass mechanics. We first require a dimensionally correct analog for the notion of “total force” which we now compute as the vector resultant of two density integrations $\int_S f_s \, ds$ and $\int_V f_b \, dv$ (where $f_s$ and $f_b$ are the surface force and body force densities respectively). Observe that these two integrations transpire over the requisite regions: $S$ for “outer surface” and $V$ for “interior volume.” In so doing, we are summing a large number of force densities that act in different locales, unlike in the point mass case where our “special forces” all act in the same place. But in composing our new notion of “total force,” we simply ignore these differences in point of application.
Using these new notions, we obtain an analog of Newton’s Second law suitable to isolated rigid bodies: \( (\int_V \rho \, d\mathbf{v}) \, \frac{d^2\mathbf{r}}{dt^2} = \int_S \mathbf{f}_s \, ds + \int_V \mathbf{f}_b \, dv \), where “\( \int_V \rho \, d\mathbf{v} \)” is the summation of the mass density over the entire rigid body \( \mathcal{B} \). But with which point in \( \mathcal{B} \) should the location \( \mathbf{r} \) in the term “\( \int_V \rho \, d\mathbf{v} \) \, \frac{d^2\mathbf{r}}{dt^2} \)” be computed? Answer: it doesn’t really matter because every one of them will display the same linear acceleration in any direction we look (due to the rigidity of the herd). Some writers link \( \mathbf{r} \) to the center of mass of the body, but there is no especial reason for doing so (especially when the center of mass is often not located inside \( \mathcal{B} \) at all, as in a doughnut).

Although the points in \( \mathcal{B} \) accelerate in the same way, they certainly don’t have the same velocities. Here’s where rigid body mechanics needs to bring an analog to cowboy #2 on the scene. This new notion is called the torque \( \tau \) (or turning moment) of the summed force densities acting upon \( \mathcal{B} \). Once again, this summation needs to be broken into two integrals that separately average the lever arm contributions of the surface and body forces with respect to some center \( \mathbf{A} \) within \( \mathcal{B} \) (it doesn’t matter where, although certain “centroids” can make the calculations easier). More exactly, \( \tau = \int_S (\mathbf{f}_s \times \mathbf{r}) ds + \int_V (\mathbf{f}_b \times \mathbf{r}) dv \), where \( \mathbf{r} \) represents distance to the chosen reference point \( \mathbf{A} \). Quite different distributions of force density across a rigid body can move it in identical ways as long as their averaged “total force” and averaged torque about \( \mathbf{A} \) are the same.

To complete our scheme, we must now quantify how our rigid body creates up an “inertial resistance” to an applied torque as well. Here we need to compute how far away from \( \mathbf{A} \) the mass density \( \rho \) within \( \mathcal{B} \) tends to lie on average, viz \( \int_V (\rho \cdot \mathbf{r}^2) dv \) (\( \mathbf{r} \) is squared to keep values positive). This new quantity \( I \) is called \( \mathcal{B} \)’s “moment of inertia” around \( \mathbf{A} \). Using it, we can express “Euler’s Second Law of Motion”\(^3\) for torques as \( \frac{d^2\theta}{dt^2} = \int_S (\mathbf{f}_s \times \mathbf{r}) ds + \int_V (\mathbf{f}_b \times \mathbf{r}) dv \), where \( \frac{d\theta}{dt^2} \) is the angular acceleration of \( \mathcal{B} \). Any textbook on rigid body mechanics will explain these notions quite adequately.

Working within a point mass framework, Euler’s second law is provable from Newton’s second law in conjunction with the restrictions on “action-at-a-distance forces” introduced in that context. But this dependence no longer holds as soon as the “forces” tolerated multiply into new varieties. In particular, Euler’s second law is required as an independent postulate to show that stress tensors must be symmetric within a continuum physics setting. Unjustified “lifts” from point
mass mechanics often disguise this crucial fact in many texts.

However, our two Eulerian principles alone don’t tell us how hinged assemblies of rigid bodies should act, which is our main objective in this section. A general answer to this question was supplied by Lagrange, who elevated some of the reasonings we have already canvassed into a general framing principle. Specifically, Lagrange maintained that, in any system of rigid parts characterized by n sites of impressed force, either (i) the device remains in equilibrium and the total virtual work associated with all impressed forces vanishes or (ii) the device moves with exactly the requisite inertial reactions at the n sites to compensate for the virtual work imbalance. Traditionally, consideration (i) is dubbed the principle of virtual work and (ii) is called d’Alembert’s principle (we have considered prototypes of both reasonings earlier). Combined into one formula, we obtain Lagrange’s principle:

$$\sum F_i \delta q_i = \sum m_i d^2 q_i / dt^2$$

where $\delta q_i$ represents a so-called “virtual adjustment” in the coordinate value $q_i$, leading to a measure of the work that the applied force $F_i$ would supply if it could be prolonged through that distance. In the shovel illustrated, applied forces $F_1$, $F_2$, and $F_3$ originate in the steam cylinders, whereas $F_4$ represents the load at the shovel. Note that the value of any specific virtual turning $\delta r_i$ can be calculated from that of the others, because they are geometrically constrained to act in concert. If the crane can maintain itself at rest with the forces indicated, the sum of virtual work at all the joints must vanish, for otherwise parts will begin to accelerate insofar as a force imbalance is present.

Lagrange’s formula is particularly useful if we have employed independent coordinates as our $r_i$, for then we can write down a formula that expresses how work supplied at, e.g., site $r_1$ gets transmitted across the mechanism to any other site on the assumption that the remaining sites can stay fixed in the process. Working out these rules for each pair of sites provides a collection of equations that can completely fix (modulo a set of initial conditions) how our crane will move (all mention of anything “virtual” drops out of these new equations by applying the usual Euler-Lagrange techniques from the calculus of variations). The formulas familiar from analytic mechanics that are couched in terms of “Lagrangians” or “Hamiltonians” simply represent these new equations rewritten reliant upon certain further assumptions about the nature of the forces at issue (viz, their derivability from potentials). It is also possible to derive the latter from restricted forms of variational principle such as “Hamilton’s principle of stationary action,” in which
one considers the variation of an “action” between two endpoints on the system’s eventual trajectory. Such principles loom large in modern physics but, in the context of applied classical mechanics, “Lagrange’s principle” as we have stated it, is considerably more general in its scope because it does not require independent coordinates for its valid expression (although it can be hard to employ effectively without them).75

Because terminological issues can prove very confusing, readers should appreciate that “Lagrange’s formula” qualifies as a variational principle because it contains all of those $\delta q_j$ terms, not because it considers any variation of path between endpoints (as one considers in setting up a “Lagrangian”76 for a dynamic system). There are many natural assemblies of rigid bodies that resist ready formulation in “varied endpoint” terms.

Many interesting geometrical problems are closely connected to generalized coordinates. The “configuration spaces” (= the high dimensional space corresponding to the 3n-tuples that specify the current locations of every member of the collection) of our earlier point mass swarms are comparatively uninteresting from a mathematical point of view. But if one considers the mobility space of a complicated mechanism like our crane, as determined by its varying generalized coordinates, we obtain a quite novel structure, largely because its coordinates are angle-like in character: they return to their starting values after a 360° rotation. That is, its “mobility space” should contain only the points corresponding to configurations that the crane can potentially assume while remaining linked together—any condition in which parts have become detached are excluded from the space. Mathematically, we obtain such “mobility spaces” by cutting out all of the “can’t be visited” regions from a regular Cartesian 3n space and gluing together the remaining edges according to the pathways of angle-like returns. The resulting substructures can prove very complicated geometrically and comprise a topic of great mathematical interest far beyond the limits of the kinematics of mechanisms (which is the traditional name for the study of machine mobility). Readers should be advised that many modern books ostensibly on “classical mechanics” are chiefly interested in what happens inside such restricted spaces.

From a point mass vantage, we are plainly skipping over a huge amount of internal structure. Let’s examine a small piece of our crane from a punctiform point of view. Clearly, strong cohesive forces $F_{ij}$ will be required to lock point $i$ into a lattice with point mass $j$ and some
kind of binding force $F_c$ will also be needed to keep our piece fixed to its pin. All mention of these have vanished from Lagrange’s principle. Why are we allowed to ignore these extra forces? Textbooks commonly argue as follows: (1) “The net work of the cohesive forces vanish because they occur in ‘internal force’ pairs where $F_{ij} = -F_{ji}$. Since their virtual displacements will be the same, their virtual work contributions will cancel each other out.” Or: (2) “The constraint force $F_c$ does no work because its action is orthogonal to the path of virtual movement $\delta r_c$.”

Here is Donald Greenwood’s version of this last argument, presented in the course of “justifying” Lagrange’s principle from a point mass standpoint:

> Consider a body $B$ which slides without friction on a fixed surface $S$. The constraint force is normal to the surface at the tangent point $P$, but any virtual displacement of $P$ involves sliding in the tangent plane at that point. Hence no work is done by the constraint force $R$ in a virtual displacement.\textsuperscript{77}

But in our point mass frame, all forces are supposed to act from one point to another along the line connecting them. But our constraint force $R$ looks as if it starts and ends in exactly the same spot $P$, which wasn’t permitted under our old reading of the third law. Plainly, some new kind of “force” has been smuggled into Greenwood’s text, without adequate prior warning.

On virtually the same page Greenwood argues for Lagrange’s principle in a different setting as follows:

> Assume that two particles are connected by a rigid massless rod... Because of Newton’s third law, the forces exerted by the rod on the particles $m_1$ and $m_2$ are equal, opposite and collinear. Hence $R_2 = -R_1$ ... as shown. Furthermore, since the rod is rigid, the displacement components in the direction of the rod must be equal or $e \cdot \delta r_1 = e \cdot \delta r_2$ [where $e$ is a unit vector pointing in the direction of the rod]. Therefore the virtual work of the constraint forces is zero: $\delta W = R_1 \cdot \delta r_1 + R_2 \cdot \delta r_2 = 0$.

But by what right can we insert a “rigid, massless rod” in our system and still maintain that “Newton’s third law” equates $R_2 = -R_1$? It is not as if the two masses are directly exerting forces on one another, as our earlier reading of the third law expects. Indeed, suppose that the intervening rod is curved, rather than straight. We still want our reasoning to hold, yet plainly $R_2 \neq -R_1$ (the vectors point in quite different directions). In fact, we have already faced the problem of how a curved rod manages to redirect thrust earlier. We observed that quite complicated molecular arrangements must be
in place before a blow delivered to the endpoint \( m_1 \) will produce the requisite effect at \( m_2 \) (if such a transmission mechanism can be legitimately assembled within the framework of mass point physics at all).

Passages like these trade upon unnoticed elisions between section (iv)'s foundational sense of "isolated particle" and looser policies of talking of "representative points." By exploiting our alleged freedom to place "representative points" where we wish, Greenwood allows his "point masses" to sometimes sit on top of one another or locate themselves at the far ends of ghostly, massless rods. Through simple appeals of this character, we find ourselves miraculously "lifted" to the characteristic scale level of objects (e.g. steam shovel parts) far above the realm of the component "particles" (atoms, molecules, the tiny crystals in iron) that we originally sought to model as point masses.

In terms of our opening themes, observe that a doctrine that is essentially "philosophical" in nature ("scientists idealize their targets through selecting "representative points""") has been tacitly employed as a cover for a missing stretch of substantial mathematics ("how do point mass foundations support the principles of analytical mechanics?"). As we observed, one of Hilbert's stated objectives in his sixth problem was to study these "lifts" in a more rigorous spirit, although he didn't observe that stock textbook arguments like Greenwood's involve moves of this character. We also discussed the general manner in which constitutive modeling considerations get mysteriously bypassed in arguments of this character. With respect to our earlier example of stacked pendulums, we are quite aware that the anticipated transmission of thrust would have not occurred had the balls been composed of putty rather than steel. With the latter, we can safely assume that the most of kinetic energy inputted on the right will not become plastically stored as internal potential energy for any appreciable period of time nor converted to heat. 78 But we would never make comparable assumptions with respect to balls of putty. Why? Long experience with steel balls assures us that the worrisome forms energy dissipation mentioned will not prove a large factor with them, but we know this largely on the basis of macroscopic familiarity with such materials. But in this situation and the ones Greenwood discusses, we have somehow persuaded ourselves that we can derive salient predictions based upon "general laws" pertinent to point masses alone, without needing to supply any constitutive modeling that can explain why such setups would behave differently if they had been composed of putty-like parts.

In our earlier examples, such gaps with respect to constitutive modeling were disguised through various wedge predicate lifts based
upon unsupported distinctions between "internal" and "external forces" or "reaction forces" and "forces that do work." But in the Greenwood passages, some of the allied "lifting" gets accomplished under the cover of the "philosophy" of "representative point modeling." Within the context of nineteenth century continuum mechanics, allied forms of "philosophy" regularly served as camouflage for quite substantial forms of "lift," whose details we shall review in the next section. But the *justificatory role* that overt "philosophy" commonly plays in making standard textbook "derivations" appear palatable should be noted, for the historical entanglements between "philosophy" and dubious derivational practice continues to significantly affect the character of contemporary opinion in philosophy of science.

Thus standard "derivations" of analytical mechanics doctrines from point mass foundations are rarely cogent, if scrutinized from the point of view of Hilbertian rigor. But none of our considerations show that an alternative set of foundational principles can't be coherently framed that accepts rigid bodies as its "primitive objects," possibly in conjunction with point masses as well. In fact, the best modern writers on mechanics recognize that pretending that analytical mechanics can be adequately founded upon point mass foundations is simply a mistake. Cornelius Lanczos comments:

*Those scientists who claim that analytical mechanics is nothing but a mathematically different formulation of the laws of Newton must assume that Lagrange's principle is deducible from the Newtonian laws of motion. The author is unable to see how this can be done. Certainly the third law of motion, "action equals reaction," is not wide enough to replace Lagrange's principle.*

He particularly has in mind some of the "third law" shifts we discussed in connection with Thomson and Tait.

In criticizing derivations like Greenwood's for their lack of rigor, we should never forget that the *modeling techniques* they are intended to justify are of vital importance to working physics. For the import of "virtual work" schemes in practice is that they allow us to avoid working through an awful lot of difficult physics that runs the risk of introducing large errors into our calculations with little gain in predictive power. We've already discussed the advantages of working with *data drawn from a range of scale sizes*. If we already know how the principal patterns of thrust transmission operate within our crane at a large scale size $\Delta L^*$ (which we would not know if it were made of putty rather than steel), why not exploit that information to reduce the complexity of our modeling, even at the cost of a certain degree of approximation with respect to the point masses that comprise it at a scale $\Delta L$? What is the point of computing in a wholly "bottom up" way facts
about the crane’s potential movements that we already know (e.g., that its parts won’t bend much and will stay pinned together). The essential genius of “virtual work” and the other techniques of analytical mechanics lie in their ability to combine data types in this manner.

Undoubtedly, these ΔL* scale successes are rooted in lower scale considerations of “energy budget” in some fashion or other: we initially feed energy into the crane from its steam pistons and--due to the near rigidity of its parts--most of this input effort gets expended at the shovel end through lifting a substantial load. But allied principles of “energy maintenance” operate within quantum mechanic’s dominions as well and some of our difficulties in devising plausible “constitutive modelings” for our crane in purely point mass terms may trace to the fact that inherently quantum processes play a significant intervening role in the delivery of energy from the cylinders to the shovel. For such reasons we may justly feel that the “derivations” of analytical mechanics provided in textbooks like Greenwood’s “get things kinda right,” despite the fact that such arguments can’t possibly satisfy Hilbertian demands on rigor. The derivations are “ kinda right” in that they capture a roughly accurate sense of the energy transfer without it proving possible to implement the underlying processes classically in a satisfactory manner. Such considerations likely lie at the roots of the strong “family resemblance” ties that appear to unite most forms of classical technique.

For reasons we shall adumbrate in a moment, analytical mechanics, if stoutly set on its own feet axiomatically, should appear an odd choice81 for serving as a baseline “ontology” for classical mechanics due to the tremendous number of “descriptive holes” it contains. This section has devoted its attention to analytic mechanics’s prospects as a foundational enterprise largely because the subject commonly serves as a favored point of refuge when one encounters conceptual difficulties in pursuing our other basic “ontologies.” In fact, the position of “rigid bodies” within classical physics is much like that of the disreputable uncle who possesses most of the money in the family. You don’t fully admire his character but you appreciate all of the good things he can buy for you.

We have already examined several ways in which point mass mechanics commonly appeals to rigid body notions as “escape hatches” when it finds itself in descriptive hot water. Thus we invoke “massless rods” to hide the fact that we don’t know the “special force laws” that bind two mass points at a constant distance. Or we enlarge our point mass “planets” to become finite spheroids to avoid Xia-type blowups. Or, like Poisson, we correct the “one elastic constant” deficiencies of a material modeling by replacing the point centers with rigid ellipsoids. But these doors of conceptual escape swing both ways, for analytical mechanics commonly evokes the resources of its ontological rivals to sustain its
own reasonableness. In truth, the very phrase “reaction force” (and its cousin, “contact force”) is framed precisely to leach “force” respectability from its action-at-a-distance counterparts within the mass point framework. Let’s return to our old friend, the bead on a wire, and its “generalized inertia.” Consulting the diagram, it is easy to compute “reaction force” $F_{wb}$ that the wire must exert on the bead to hold it on the wire: it equals the mass of the bead times the acceleration needed to shift its velocity vector from its unconstrained heading to a direction lying tangent to the wire (presumably, a comparable “reaction force” $-F_{bw}$ must pull the wire towards the bead, albeit in an imperceptible manner due to its greater mass). But when we approach this situation employing Lagrange’s principle, such “forces” are not mentioned at all: it is merely observed that, relative to arc length along the wire, no “virtual work” has been performed upon the bead. Ergo, the bead will not display any acceleration relative to the wire (precisely as “generalized inertia” predicts). We only “see” the magnitude of the reaction force when we consider the situation in terms of Cartesian rather than “generalized” coordinates. From that vantage point, we realize, as observed in section (iii), that $F_{wb}$ must vary for beads that travel at different speeds, in contradiction to the prohibitions built into Newton’s third law as understood above.

And we should note that, with “forces of constraint,” the usual conceptual dependencies of point mass mechanics are reversed. In a gravitational system, we are supplied the rules for assigning force strengths to a configuration and, upon that basis, calculate the resulting motions. But with our bead, we first know its expected motion and calculate the “forces” upon it on an after-the-fact basis. Authors such as Poincaré have occasionally claimed that “$F = ma$” is a “mere tautology” on the grounds that one can always fit a “force” to any conceivable acceleration. This is a fairly apt description of how “forces of constraint” are calculated, but it is quite wrong when reactions governed by “special force laws” like gravitation become central. Because “reaction forces” display such different characteristics than interactions such as gravitation, the better writers on analytical mechanics treat them as belonging to intrinsically different classes of entity.

So why employ a common word for both and why should we bother with computing such after-the-fact values for “force” at all? The true motive for doing so, insofar as I can see, traces to the fact that we are rarely content to stay within the descriptive confines of a rigid body point of view. The after-the-fact values we calculate for our “reaction forces” become important precisely when we are posed to abandon our “rigid body” descriptive mode altogether. For example, if our bead is shot along the wire at a sufficiently high velocity, the wire will bend under the
duress of trying to hold the pieces together. A useful warning signal that significant distortion might be occurring is a high value for the “force of reaction” we calculate within a rigid body framework. So a large “force of reaction” can serve as a convenient trigger for escaping to another form of modeling, whether it is based upon continuum mechanics or approaching the wire as an assembly of mass points capable of lattice deformation. When that happens, our funny “forces of reaction” get replaced by forces of a more conventional nature.

A similar situation applies to the “rigid” collisions posited in our rack of pendulums. In reality, the colliding balls will distort and build up internal stresses and, if such quantities become excessive, they will shatter. The “reaction forces” computed within analytical mechanics supply a rough estimate of those initial stresses and thereby provide a signal that the more daunting realms of continuum mechanics need to be considered.

For later purposes, let us consider a final example. With a conventionally designed girder bridge, one employs “virtual work” techniques to calculate both the “forces of reaction” and the comparable “reaction torques” that will arise at the piers of the bridge whenever the bridge is burdened with loads as pictured. An engineer must ensure that these calculated “reactions” are less than the measured strengths of the piers, lest the structure collapse under the loads (engendering a dreadful situation in which continuum mechanics must again be applied).

So a central reason for calculating “forces of reaction” within analytical mechanics is that such computations serve as an internal check on the viability of modeling a system in rigid body terms: if these “forces” become too large, blow the joint! In my opinion, analytic mechanics could have never survived as a self-inclosed field of physical study except insofar as it can be naturally decked out with “escape hatch” indicators such as these. With respect to our colliding balls, we will be happy to model them as rigid shapes if we are assured, in the background, that richer frames of modeling are available to which we can turn when required and that we possess reliable internal signals for determining when such “escapes” should be executed.

These “crossover to a replacement frame” concerns address our wonderment as to why such formally different gizmos should bear the common label “force.” Each usage relies upon its neighbors for “escape hatch” assistance when local descriptive difficulties loom and it is helpful to mark the central locales of crossover
contact with a common word. These same considerations provide a strong rationale for structuring “classical mechanics” instruction in the patchwork manner of a “theory facade” rather than in the flattened axiomatic format that Hilbert sought, for the “facade” as a whole gains its descriptive strength and robustness through precisely the ready “lifts” that link its local modeling gambits together. As we’ve observed several times, such stitched together strength was essential to the historical triumph of “Newtonian physics.”

But efficiently framed linguistic schemes of this nature can occasion considerable confusion if the rationales for their “facade”-like characteristics are inadequately diagnosed. The fact that action-at-a-distance forces and “forces of constraint” appear conceptually incongruous lies at the source of some great controversies over classical matter. If we remain within a point mass framework and restrict forces in the strong third law manner we did, then it is easy to prove that the conservation of energy must hold (the restrictions are needed to make “potential energy” become well-defined). But “forces of constraint” do not obey these restrictions, so what happens to the conservation of energy? One tactic, already canvassed, contends that they are merely short range action-at-a-distance forces in disguise and hence satisfy the third law requirements (even though we cannot frame any “special force law” adequate to their empirical operations). But if we tolerate constraint forces on an equal footing with action-at-a-distance forces, questions of energy conservation can become rather tricky, especially in light of some issues concerning so-called “non-holonomic constraints,” which we’ll discuss in a moment. In so-called thermomechanics, where no attempt is made to reduce notions like “heat” and “entropy” to mechanical replacements, energy conservation needs to be introduced as an independent postulate.

Because our two “force” types mix in such an uneasy alliance, a range of historical attempts have attempted to purge this duality by privileging constraint connections over the action-at-a-distance relationships (Descartes can be credited as a chief progenitor of this point of view). But what should we do with the interactions that appear to operate directly across distances? Heinrich Hertz, in his celebrated Principles of Mechanics of 1894, saw the main task as one of explicating the potential energy storage displayed by such forces in terms of constraint connections. Here he borrowed some recent discoveries about so-called “cyclic coordinates’ from Routh and others. The central idea can be explicaded with a homey example. Suppose that we have a jack-in-the-box whose arms are hooked up to a concealed flywheel, in such a way that if we wiggle the arms of the puppet,
they will display a strong inclination to hover about their rest positions, much as the atoms within a stock action-at-a-distance lattice will do. If we look only at the little man, we will regard this “potential energy well” behavior as the result of attractive and repulsive forces acting directly across the breech between his hands. But when we consider the hidden connections inside the box, we find that this “potential energy” is actually being stored as the kinetic motions of the rapidly whirling flywheel. Its entanglements with the puppet’s arm movements depend only upon its angular velocities and not upon its current position within such turnings (this decoupling of “position” represents the formal requirement within the term “cyclic coordinate”), hence the hidden motions provide a perfect surrogate for the puppet’s apparent ability to store “potential energy” within the positions of its arms. Hertz’ program tried to repair our duality of force disharmonies through utilizing only constraint forces within his foundations, at the cost of needing to couple the original system (the puppet) to a larger system of “concealed masses.” The path to a clean “conservation of energy” is easy within such a purified framework.

As such, this proposal is now a mere historical curiosity, allied to other attempts to reformulate mechanics in unusual ways that emerged in the same period. But because Hertz’ book (or its “philosophical” introduction, at least) was widely influential and because he did not always explain his motivations clearly, it has left a philosophical legacy where it is commonly maintained that respectable physicists such as Hertz strived to “rid mechanics of forces on account of their mysterious nature,” even within contexts like point mass mechanics where they appear as absolutely central ingredients!

Defenses of dubious distinctions between “observational” and “theoretical” notions occasionally assume the character: “Recall that even the great Hertz was troubled by the ‘non-observationality’ of force.”

But a careful reading of Hertz reveals that he held philosophical views quite opposite to these. His primary focus was centered upon the foundational tensions engendered by the two families of “force” tolerated within conventional analytical mechanics.

Let’s now return to the chief limitations of conventional analytic mechanics. As matters now stand, our chief governing principle (Lagrange’s) for compounded rigid bodies has been formulated in variational terms. But such formulae have little practical utility until we can manage to get those “δq” terms out of there. If the geometrical constraints binding the system together can be expressed as algebraic relationships without derivative terms such as velocities (such conditions are called holonomic constraints), then we will be able to recast our system in generalized coordinates that can be varied independently of one another. Thus suppose we have
a coin that \textit{slides} along a slanted table top under the influence of gravity. Orient \(x, y\) and \(z\) coordinates with respect to the table. The constraint of “being attached to the table” is then captured by the simple equation \(z = r\), where \(r\) is the radius of the coin. If our disc can slide anywhere, it can be transported to spot A along arbitrary paths using an appropriate schedule of forces. Our freedom in plotting these paths to A possesses two degrees of freedom, captured by its freely variable \(x\) and \(y\) coordinates. Suppose that we now carve a groove \(G\) into the table leading to A and require the coin to slide along that curve. This new constraint demands that the \(x\) and \(y\) locations of our coin must satisfy the equation for the curve \(G\). Our coin’s allowed movements are now reduced to a single degree of freedom, naturally captured by its arc length placement along \(G\). In both cases, the applicable constraints are holonomic in nature, because the equations that express the constraints do not involve velocities. Under these conditions, we can remove the variational terms from Lagrange’s principle to obtain the standard Lagrangian and Hamiltonian formulas supplied in every competent textbook on classical mechanics.

But suppose that we allow our coin to \textit{roll} upright across the original table. It can still reach spot A, but can no longer do so directly, for the coin needs to sidle into that position in the manner that a car is parallel parked. This is because our coin must roll for a certain distance before it can turn its heading through any finite angle \(\theta\), for its \(x\) and \(y\) velocities are entangled in the \textit{non-holonomic} relationship \(\sin(\theta) \frac{dx}{dt} = \cos(\theta) \frac{dy}{dt}\). This formal wrinkle blocks the \(\delta\)-removing extraction policies that we employed with holonomic constraints. And problems of this new, “non-holonomic” stripe are often difficult and tricky, for their resolution turns upon quite subtle considerations of how the \(\delta q\) parameters can be validly removed from Lagrange’s principle under such condition. Conventional variational principles of a “least action” stripe supply patently incorrect answers for such problems.

In consequence, the treatments of “analytic mechanics” offered in most undergraduate textbooks (invariably resting upon “least action” principles or something similar) tolerate severe \textit{descriptive gaps} in the sense that such formalisms can deal deftly only with the physics of systems of type \(S\) and wax incorrect or inchoate when applied to systems of type \(S^*\), even though the \(S^*\) appear to be very similar to the \(S\) in their physical constitution (allowing a coin to \textit{roll} rather than \textit{slide} provides a case in point). Goldstein’s well-known primer contains interesting apologetics for such omissions, citing the fact that non-holonomic constraints lose their relevance as one approaches the quantum scale:

\begin{quote}
[T]he more vicious cases of non-holonomic constraint must be tackled individually and consequently in the development of the more formal aspects of classical mechanics it is almost invariably assumed that any constraint, if
\end{quote}
present, is holonomic. This restriction does not greatly limit the applicability of the theory, despite the fact that many of the constraints encountered in everyday life are non-holonomic. The reason is that the entire concept of constraints imposed in the system of wires and surfaces or walls is particularly appropriate only in macroscopic or large-scale problems. But the physicist today is primarily interested in atomic problems. On this scale all objects, both in and out of the system, consist alike of molecules, atoms or smaller particles exerting definite forces, and the notion of constraint becomes artificial and rarely appears. Constraints are then used only as mathematical idealizations to the actual physical case or as classical approximations to a quantum-mechanical property—e.g., rigid body rotations for "spin." Such constraints are always holonomic and fit smoothly into the framework of [least action-style classical] theory.\textsuperscript{90}

This aptly captures the tacit considerations that have allowed the “analytic mechanics of holonomic constraints only” to become the main topics highlighted within the vast majority of modern physics texts labeled “Foundations of Classical Mechanics” (observe that it is largely physicists who adopt this narrowed policy, not mechanical engineers) However, this clearly represents a conception of “foundations” that lacks the characteristics we anticipated when we began investigating the topic in Hilbert’s spirit. In terms of apologetic “lifts,” Goldstein has escaped from a rather large descriptive “hole” within “holonomic analytical mechanics” by retreating to the realms of quantum theory.\textsuperscript{91}

In any case, there are other descriptive gaps that no flavor of analytic mechanics (including Lagrange’s stronger principle) can adequately treat. The most obvious are situations where the applicable constraints suddenly alter, as when our coin falls off the end of a table. Point mass mechanics experiences no comparable difficulties with such events, because the forces applied to a point mass “coin” will adjust continuously as it passes the point masses at the table’s far edge. But if sharp constraints are active, gravitation must turn itself on discontinuously at such moments. It might be possible to extend the span of analytical mechanics by tolerating discontinuities in the spirit of the impulsive forces considered earlier, but such proposals are rarely pursued with any rigor.

However, the most egregious gaps in rigid body coverage trace to the fact that analytical mechanics contains no provisos for the rupture or fusing of rigid parts. Typically, one simply departs to another foundational landscape when such
problems loom. Let’s return to the girder bridge examined earlier and introduce a small change. In the previous diagram there are little wheels underneath the right hand side of the bridge symbolizing the fact that the bridge can shift freely at its right hand pier. But let us now attach the bridge firmly at both ends, as pictured here. From a mathematical point of view what change has occurred? In “free to slide” circumstances, the bridge can be modeled as a single rigid body and Euler’s rules for its equilibrium supply \textit{exactly the right number of equations} to fix its position completely. The magnitudes of the reaction forces at the piers can then be calculated unambiguously from those equations. Such assemblies of rigid bodies are called “determinant structures” and generally represent the designs that engineers hope to implement.

But as the sliding allowance is suppressed by fixing the second pier, an additional constraint equation gets thrown into our mix, resulting in an \textit{over-determined} set of formulas, in the sense that it now contains more independent \textit{equations} than independent \textit{variables}. Usually such over-constrained collections can’t be solved because they place inconsistent demands upon their variables. Accordingly, “something’s gotta give” and engineers “open up the internal degrees of freedom” by modeling the bridge as a \textit{flexible object}. In doing so, they can no longer work with Euler’s equations alone and must supply further “constitutive equations” that determine how the innards of girders bend when stressed at their endpoints (structures of this type are called \textit{indeterminate} due to this need for internal modeling). The mathematical character of the entire problem changes dramatically as soon as we “open up internal degrees of freedom” in this manner. In particular, the “forces” (= stresses) we calculate within our replacement setting assume a continuum physics character and lose the computed-after-the-fact characteristics of constraint forces. Once again we witness the “escape hatch” nearness of “forces of reaction” to more normal varieties of classical “force,” despite the fact that two classes of entity obey very different governing principles when considered in their own rights.

In sum: if conditions are appropriate (i.e., the bridge design is “determinate”), the “forces of reaction” required for engineering safety can be calculated without considering the \textit{interior physics} of how the iron comprising the bridge responds to heavy loads. But this ability to avoid internal complexity applies only in very special circumstances (it’s like Goldilocks and the three bears: if a bridge is neither over- nor under-determined, then its equational “soup” will taste just right). If our bridge is not perfectly “determinate,” we must retreat to more demanding mathematical landscape of continuum mechanics. In truth, it’s rare that we run across structures within Nature that meet the mathematical qualifications required in a determinate structure. We encounter so many in the terrain about us only because
we’ve put them there ourselves and have designed them that way!

For such reasons, I find it hard to consider analytical mechanics seriously as a plausible "foundation" for all of "classical physics." The physical systems it can model adequately appear precariously perched amidst an array of similar systems that require the descriptive tools of rival foundational programs for their governance. It thus seems (as writers like Leibniz have long opined) that true rigidity should not be tolerated within the foundational depths of "classical mechanics" and standard invocations of such characteristics should be regarded as merely excellent vehicles for effective "variable reduction" involving approximations at higher scale lengths.

Historically, however, this straightforward assessment could not be wholeheartedly embraced, for deep conceptual difficulties in articulating coherent principles for continuum physics continually drove practitioners back to rigid bodies for foundational assistance. We’ll review these concerns in the next section. Before we do, there are some final considerations regarding the notion of "machine" or "mechanism" that merit brief survey. Take a collection of rigid rods and pin them together in a random fashion, as illustrated in figures (a)-(e). Some of the results possess no ability to turn internally. Thus assembly (c) represents what we just called a determinate structure, while (e) is over-constrained in the manner of our second bridge. In contrast to these immobile frames, (a) and (d) can move freely with differing internal degrees of freedom (one and two, respectively). But, of all these possibilities, case (c) proves special in that its links form into a so-called closed kinematic chain and move with one effective degree of internal freedom. It is only arrangements of this last type that are considered to be mechanisms by the modern engineer. On the right, I’ve redrawn figure (c) as a standard four-bar mechanism which comprises a central object of study in works devoted to machinery. In such circumstances, input effort will be fed into the device at some particular point (say, the crank at its lower left) and extracted elsewhere (say, from the moving arm on the right).

To the best of my knowledge, it was the acknowledged founder of the modern theory of machines, Franz Reuleaux, who first isolated the special placement betwixt over- and under- constrained systems that provides the science of "closed kinematic chains" with unique mathematical characteristics. Here are some of these special features. (1) True mechanisms can execute clean force conversion, in that any input force applied to such a device will be modified or redirected into an output activity of a different character without significant loss of work capacity. This ability to redirect thrust in a controlled way is crucial for machines to work for us as they do. (2) Such conversion never creates additional work capacity and remains subject to the restriction that no perpetual motion device can be devised upon its basis. Nor should strict mechanisms lose any of this same work capacity to friction or allied
processes. (3) The efficiencies of the force conversions typically differ considerably from moment to moment in a device’s cycle. (4) Because of these shifting adjustments, appeals to virtual work (i.e., \( F \delta \theta \)) are required to frame the rules for the relation of input to output work correctly. (5) The same work capacity relationships apply when the locales where forces are applied and extracted become reversed (i.e., the input effort is supplied to the top of our four-bar linkage and the output work is performed at the crank handle). This important rule of reciprocal behavior is termed an inversion of the mechanism by engineers.

One route to appreciating the reasons why mechanisms display such special characteristics is to consider what occurs mathematically as we gradually subject a point mass structure to an increasing number of \( \Delta L \) level constraints. Each time we add a new proviso, we “turn off” many of the detailed forces that bind the mass points together in favor of the new equation. In the case of mechanisms, we carry out these policies of force suppression to the maximal degree possible, just shy of permitting the system no internal mobility whatsoever. In doing so, equations of geometrical constraint take over most of the substantive duties within our descriptive scheme, retaining only a thin flavor of energy conservation from the original point mass mechanics. So it isn’t any wonder that the central principles whereby mechanisms operate present a considerably different appearance than the foundational principles required for more general classes of Newtonian system. This startling realization was first clearly enunciated by Reuleaux in the introduction to his classic work.

Most readers are familiar with stereotyped characterizations of classical mechanics as supplying “clockwork universes.” But such loose claims are seriously confused, in that mechanical intuitions we extract from the specialized realm of machinery have become blurred with the un-machine-like behaviors displayed within, e.g., a point mass modeling of the solar system. The culprit ultimately responsible for these muddles is our old acquaintance rigid body, which doesn’t properly deserve a seat at the foundational table but proves so darned convenient as a tool of effective “variable reduction” that its presence is often welcomed there anyway (sometimes our best friends can make our lives very confusing). It is almost impossible to unravel the tensions within the long history of classical mechanics without appreciating the frequent intrusions of machine-derived
themes into a broader physical arena.

For later purposes, we should briefly comment on another manner in which “rigid body mechanics” is often presented in textbooks. Without much preface, one is confronted with situations as illustrated, where a target body $S$ is surrounded by a more complicated array of external manipulations than simple traction vectors (in the diagram, these generalized “manipulations” appear as tractions of dimensionally incongruent types (“continuous” and “concentrated”), along with primitive turning torques and constrained attachments to its surroundings). Such situations represent a considerable extension of our original “traction forces around a rigid body” problematic and can be approached most readily through virtual work considerations. Such diagrams often serve as the loci of significant surreptitious “lifts” when we consider $S$’s that are not rigid in the context of continuum mechanics.

(vi)

**Continuum mechanics.** If we could mark out the salient differences clearly (= remove the fogs of “idealization” associated with “representative point” thinking) and poll most of the prominent “classical physicists” of the past with respect to their favored choice of “foundational object,” the majority would undoubtedly select “continuous, flexible body.” Let us now try to articulate principles capable of governing the behaviors of continua coherently. We immediately confront a more difficult form of the surface/volume force coordination problem than we confronted in the case of rigid bodies. In the latter circumstances, the relevant traction forces operate only along the exterior surfaces $\partial B$ of the rigid body $B$ under consideration. But inside a flexible body, we can carve out infinitely many internal surfaces $\partial S$ able to support their own arrays of traction vectors as well. Furthermore, the tractions on each different $\partial S$ will generally differ from one another and from the exterior tractions applied along the outer boundary $\partial B$. Indeed, we anticipate that as we push and pull upon $\partial B$ in different ways, these exterior modifications will make themselves felt at an interior point $q$ through progressively altering the traction forces upon all of the interior surfaces $\partial S$ that surround $q$. Moreover, this process of inward transmission will require some time to complete: the tractions on $\partial S^*$ must alter before the
tractions on $\partial S$ can change (in a simple material $\mathcal{B}$, these processes support the notion of "the speed of sound within $\mathcal{B}$", i.e., the velocity at which waves of inward alteration will travel through the interior of $\mathcal{B}$). Inside a truly rigid body, however, such inner tractions and waves no longer make sense (for essentially the same reasons as the notion of "absolute pressure" becomes problematic when a fluid is assumed to be incompressible). Usually, notions of "rigid body" are regarded as incompatible with a continuum physics point of view (they can be, at best, approximated by bodies that are extremely stiff in response to applied forces and torques).

In the previous section, we summed the surface forces around the outer boundary $\partial \mathcal{B}$ and the volume forces inside $\mathcal{B}$ employing two integrations whose results we then added to get a resultant applied force. We then learned that we should also compute a combined torque (= "turning force") in a similar manner. With those two ingredients in place, Euler's two laws of motion could tell us how our rigid body will respond. But in that context we only had to contend with the body forces and traction vectors around the outer boundary $\partial \mathcal{B}$. How should we address the vast army of differing $\partial S$s that have now entered our stage in the entourage of flexible bodies? If we naively compute resultant forces and torques from these, we will obtain vastly different answers according to the inner surface $\partial S$ we select. This is a surface/body force coordination problem of considerably greater subtlety than we addressed earlier.

The eventual answer we shall provide invokes the notions of "stress" and "strain." Before proceeding further, a few words of warning concerning these innocent-looking words is in order. Most philosophers interested in physics have already run across those words—in the guise, say, of their close cousin, the "stress-energy tensor" of general relativity—without reflecting sufficiently on their conceptual oddities ("stress" is not "just a form of force" and "strain" is not "just a form of shape change"). Historically, it was not until the end of the nineteenth century that the true novelty of these constructions was adequately recognized. Some of this confusion traced to the thoughtlessness about "points" that "representative point" talk encourages. So let us reiterate that in dealing with interior points like $q$, we are not longer considering the isolated points of mass point physics: our new points come densely surrounded by infinitely many neighbors, situated as close to them as you'd like. And they should not to be identified with points in the ambient container space; our material points (their most common name) wander through that background space in a trackable way.

It is best, at this preliminary stage of our discussion, to conceptualize our
material points \( q \), not as bare geometrical entities, but as decorated points or physical infinitesimals that have temporarily parked themselves at various spatial points \( p \) (which are not "decorated"). In particular, if \( q \) forms part of a solid material like iron and the material in \( q \)'s immediate neighborhood is fully relaxed, we should picture its "decorated" condition as an infinitesimal little cube about the central point \( p \). But if \( q \) is subject to stress, infinitesimal traction vectors should appear on each of its faces, one to a side and pointing inward or outward in any direction desired. In response to these tractions, our little boxes will adjust their "decoration," by adjusting their infinitesimal volumes or shifting shape in a shearing pattern (or displaying combinations of these two basic alterations). In fact, the descriptive purpose of a stress tensor is capture the local pushes and pulls of the traction vectors on \( q \), while the usual (Cauchy-Green) strain tensor captures \( q \)'s degree of distortion from its cubic relaxed state. Considering our material over a broader scale, we realize that the material points \( q^* \) found at locations near to \( q \) must be "decorated" in a manner very close to—but not identical with—that of \( q \)—otherwise, the material would display fissures (this relationship amongst nearby infinitesimals is called compatibility\(^97\)). Because all of these modes of "decoration" occur on an infinitesimal scale, stresses and strains behave like the densities introduced earlier in connection with mass and force: such measures don't sum to become legitimate masses and forces until we consider finite volumes of material. But the rules whereby localized stresses and strains eventually sum to produce finite characteristics of the material stuff to which they belong are more complicated than the procedures used for simple densities (near neighbor compatibility is required for these rules to work).

I've highlighted the odd, "point decorated with sides" aspects of the "material points" of continuum mechanics to help readers properly appreciate the conceptual novelties that lie before us—we are no longer considering the familiar "isolated points" of the easy-to-comprehend mass point framework. And although we shall eventually appeal to various limiting procedures to persuade our stresses and strains to work together harmoniously, readers should not presume that the conceptual difficulties of continuum physics are merely matters of "explaining away infinitesimals" in the familiar \( \delta/\varepsilon \) fashion of elementary calculus courses. No: deep
questions of *physical principle* will prove central to our concerns; we are not simply striving to make hygienic sense of "infinitesimal points." Our foundational difficulties might be fairly dubbed "the problem of the physical infinitesimal," but our problems mainly belong to physics and are considerably more substantive than the comparable "problem of the mathematical infinitesimal" from freshman calculus. To be sure, philosophers have sometimes presumed otherwise, but only as a result of underestimating the pertinent physical concerns. It strikes me that many of the deepest worries about "matter" in our philosophical heritage trace, in one way or another, to our "problem of the physical infinitesimal."

With these warnings to not underestimate stress and strain in hand, let us now turn to the foundational ailments for which they will eventually provide part of the cure. Let's recall the rather complicated physical situation that pertains at the level of the complete blob $\mathcal{B}$ of material to which $q$ belongs. Its interior will be pulled upon by gravity $g$ and other action-at-a-distance forces of that ilk. But $\mathcal{B}$ will also be affected by the various twists and pulls that we exert directly upon its exterior surface $\partial \mathcal{B}$ as "contact forces." If the material inside $\mathcal{B}$ is perfectly rigid, in the previous section we saw that the basic problem of coordinating these two classes of "force" could be resolved fairly easily, by also computing resultant torques and applying Euler's two laws of motion for rigid bodies. But this simple policy works only because the material is *rigid* and every point $q^*$ inside $\mathcal{B}$ must display the same linear acceleration and the whole will rotate in exactly the same way no matter from which reference point its turning moment is gauged. However, if the matter inside $\mathcal{B}$ is not perfectly stiff (let $\mathcal{B}$ be a blob of jelly or water), then the response behavior immediately around $q$ will usually look quite different from the corresponding behaviors around $q^*$ (neither the local accelerations nor rotations will be the same). And it is these local differences within flexible bodies that allow them to carry interior waves, which rigid bodies cannot support (recall that notions like "internal pressure" are not well-defined inside perfectly stiff materials). When we twist and pull upon the external surface of a flexible body $\mathcal{B}$, we generate a lot of internal tractions, for the effects of our surface manipulations generate compression waves whose effects eventually reach $q$ by progressively altering the local tractions upon a contracting collection of surfaces $S_1, S_2, S_3,...$ surrounding $q$. It is these internal surfaces (or "Eulerian cuts," as we called them) and their shifting arrays of traction vectors that greatly complicate our earlier surface/body force coordination problem.
in the case of flexible bodies.

That difficulty, the reader will remember, traces to the dimensional disparity generated by the fact that contact tractions represent surface "forces" (properly densities) in the sense they must attach to some shell of surface $\partial S$ surrounding a point $q$ before they can be coherently integrated, whereas body "forces" (again, densities, marked in the diagram by spring-like lines) such as gravitation apply directly to simple points $q$ and need to be integrated over volumes. In the rigid body case, only the outer layer of exterior traction pulls needed to "coordinated" with its interior points $q$, but in flexible bodies we are confronted with a host of additional shells $\partial S$ to "coordinate," appearing as the interior "cuts" whose characteristics are continually altered by the waves that pass through them.

Plainly this represents a fairly complicated physical problematic. It is often remarked that "physics is simpler in the small," indicating that uncomplicated laws of material behavior can be elegantly formulated only at the infinitesimal (= differential equation) level. Will this methodology help us here? Well, let's consider the material at a point $q$, where it will display a local mass density $\rho$ and allied characteristics like charge. It will be pulled upon directly by gravitation and other possible long distance "body" forces, which can be summed to supply a local resultant vector $g^*$. We can presume our material will react to its full schedule of local pushes and pulls by manifesting an acceleration $D^2 q/DT^2$ (I'll later explain why our usual $dq/dt^2$ expressions require capital D's in the present context). Unfortunately, the compression waves passing through $q$ will also affect its "full schedule of local pushes and pulls" and it is these that make our "force coordination" problem so difficult. It is easy to understand how a passing wave will affect a shell of surface $\partial S$: run a tangent plane through any point on $\partial S$ and see which way the traction $T$ supplied by the wave locally points across the plane. So to understand how the compress waves will affect $q$, we should set up a little shell around $q$ and compute the traction vectors on $\partial S$ created by the passing waves. All we need to do, it would seem, is to compute how the resulting surface "force" summation $F^*$ should compare to the body force summation $g^*$ acting at $q$. But wait a minute: no part of $\partial S$ is actually located at $q$ and, in fact, we can easily carve out a smaller cut $\partial S^*$ inside $\partial S$ whose integrated tractions may
differ considerably from those on \( \partial S \) itself (why? because \( \partial S^* \) is affected by different wave movements than \( \partial S \)). And we can draw an even smaller cut \( \partial S^{**} \) inside \( \partial S^* \) where the same phenomenon reappears. And so on, \textit{ad infinitum}.

In short, we have gone \textit{smaller in our physics}, but nothing has become \textit{simpler!} (in the spirit of Leibniz, I'll call this \textit{the labyrinth of the continuum paradox}). The regress traces, of course, to the fundamental \textit{scale invariance} of homogeneous classical continua. Whatever characteristic length \( \Delta L \) we choose, volumes of such materials will always behave exactly alike in terms of the principles they obey (of course, one can also consider \textit{composite continua} where various sectors obey different rules, but these raise further difficulties, which we shall discuss later).

Somehow we must arrest this chain of unprofitable descent if we hope to get anywhere in continuum physics. But how can we do this? One can't blithely say, "Oh, just take a 'limit' as you shrink to \( q \)," for it is not at all apparent what should happen to our traction forces when the cuts on which they live shrink to nothingness at \( q \) itself. (1) Will the result be merely a simple \textit{pressure}, which operates to expand or contract our element in terms of its volume. (2) Can such local "pressures" pull differently in different directions? (3) Can the directionalities of our tractions lean sideways in a manner that can shear an infinitesimal blob \( S \) without altering its volume? (4) If so, will they act differently upon different planes around \( S \)? (5) How differently? (6) If so, how much latitude can they display with respect to these variations? (7) Will turning torques also leave a residual infinitesimal turning moment within \( S \)?

The standard (although not invariable) answers to these questions are: (1) no; (2) no; (3) no; (4) yes; (5) yes; (6) they must interrelate in the manner of a 3D vector space; (7) no. But few of these should seem entirely obvious. Internal pressures, for example, can vary considerably across a fluid--mightn't these longer range inequalities deposit an unbalanced pulling upon our small blob \( S \) as a local residue? Prior to the time of Cauchy, the greatest practitioners of mechanics answered "no" to (3), often on the basis of the way in which they correctly answered "no" to (7).\textsuperscript{98} Although the conventional textbook response to (7) is "no," there are well-developed theories of "directed media" in use that address this question differently (I'll discuss these briefly below). The fact that it is hard to augur intuitively how \textit{infinitesimal portions} of a continuous medium should behave helps explain why the old controversy between rari- and multi-constant theories of elasticity took so long to resolve.
Such questions concern only the static responses of materials. Once time and
dynamics come into play, a wider range of difficult questions emerge. Can our
infinitesimal elements retain long term “memories” of their previous history?
Certainly, macroscopic media often behave in this way: two identical looking paper
clips made of the same material may respond differently to bending pressures
because clip A has been flexed many times in the past but clip B hasn’t (this is called
an “hysteresis effect”--clip A “remembers” its previous mutilations). Can an
infinitesimal blob $S$ display allied “memories” as well, or must such processes
emerge due to complicated interactions between finite portions of a composite
system? Likewise, might our “infinitesimals” display “delayed memories” in the
sense that a blob $S$ might respond to altered conditions in a
non-immediate manner? Again, toothpaste acts like this: it
gradually “remembers” its shape back in the tube and
tardily reverts back to it. Such questions lay behind the
twentieth century revival of interest in the “foundations” of
classical continuum mechanics, scientists who confronted
with new industrial substances needed guidance as to how
such complicated materials might be reliably modeled.

Similar problems affected the Victorians as well, because of the close linkage
between traction forces and waves. Figures like Kelvin and Rayleigh wanted to
know how many different waves could arise in an earthquake or travel through the
hypothetical aether in which light passes. And the answers often turned on what is
possible at the infinitesimal blob level.

Ultimately, the answers to all of these questions depend upon physics because,
insofar as mathematics is alone concerned, such issues can be resolved in many
different ways. That is why our “problem of the physical infinitesimal” (which is
equivalent to answering our questions coherently) is not mainly an issue in $\delta / \varepsilon$
rigorization.

In sum, we are confronted with a serious conceptual regress: materials whose
complicated behaviors never seem to become simpler no matter how small the
portions we consider. How can we halt this unprofitable descent into what Leibniz
called “the labyrinth of the continuum”? I’ll first sketch two traditional answers and
then the modern view. Perhaps at some minute scale length $\Delta L$, the volumes $S$
around $q$ will “stiffen” enough that we will see a simpler physics there. We won’t
want our infinitesimal $S$ to become totally rigid, less we never recover any flexibility
in the larger bodies \( B \) to which it belongs, but perhaps \( S \) might move like a little *mechanism*, so that some of the techniques of the previous section become applicable. Suppose we want to understand the physics of a thin drumhead. Perhaps "in the small" it can represented as a raft of infinitesimal blocks tied together by elastic cords that allow these blocks to shift vertically up and down but not migrate sideways. The rigidity of these little blocks allow us to easily solve our surface/body force coordination problem, for the resultant of the four cord pulls tells us what the magnitudes of the up and down accelerations at the center of each block \( (\partial^2 h/\partial t^2) \) will be (we needn't worry about any turning moments, because the cords and close packing prevent that). Taking a limit where the width of these blocks is shrunk to zero, we can obtain the standard equation for a linear plate without much fuss (= the wave equation in two dimensions: \( k \partial^2 h/\partial t^2 = \partial^2 h/\partial x^2 + \partial^2 h/\partial y^2 \)).

As a second example, a standard weighted beam can be assigned a small scale "mechanical element" that eventuates in the stock Bernoulli-Euler equation for such structures. In this situation, our element is allowed to turn about its centroid (= neutral axis), as well move up and down in a plane, although a series of springs sets up an internal resistance to turning. In addition, gravity now acts in the center of the element according to the weight \( W \) it bears. In this situation, Euler's rules for torque play a role in the derivations.

Except in early works\(^99\), it is fairly rare to see presumed "mechanical elements" decked out in blocks and springs quite like this. But there are several alternate mode of presentation that can achieve comparable results, through invoking the "controlled virtual work" behaviors that we briefly discussed in the previous section. Thus we might see our Bernoulli-Euler element presented as illustrated, where we have an "element" that is intrinsically flexible, but responds to contact tractions only at specific sites (viz. along the "fibers" indicated, whose combined torque balances, in a virtual work manner, the gravitational body force pulling downward). As stated before, such restrictions represent a diagnosis of how applied thrusts are expected to transmit themselves through the element. It is evident that we get our required "simplification in the small" through locating these sites of controlled thrust; otherwise, we would be simply looking at a small section \( S \) of the original blob \( B \) we began with, displaying exactly the same
behavioral complexities as where we started.

There is a curious variant on these techniques that is worth mentioning, because it appears rather often even today. It is usually called "the principle of rigification" or something similar (I will follow Thomson and Tait's exposition here). Consider a flexible blob of putty $S$ at times when it is maintained in equilibrium through an outside schedule of forces, torques and constraints (I reproduce the diagram we introduced at the end of the last section to illustrate the generalized ways in which $S$ might be be affected by its surroundings). They assume--this is where the "rigidification" comes in--that because $S$ is held fixed by its battery of forces and constraints, that equilibrium won't be spoiled if we magically render $S$ completely stiff--e.g., we transmute $S$ from putty to iron. Here the underlying thinking is that if a frame house can stay in place under certain loads, it won't hurt to nail on some additional cross beams. But a rigid body $S^*$ can remain motionless under the proscribed schedule of generalized forces if and only if such manipulations obey the virtual work principle or, less directly, Euler's two laws for a rigid body at equilibrium (this is how Thomson and Tait address the matter). Ergo the same relationships must hold amongst the applied manipulations even when $S$ is not rigid, but made of putty.

Such arguments often discomfort readers, and rightly so, for a significant "wedge predicate lift" has just been invoked. Note that our argument begins: "if our putty $S$ could be maintained in equilibrium by the applied forces illustrated, then a rigidified replacement for $S$ would as well." But real putty, acting under gravity, will plainly ooze the constraints and can't maintain an equilibrium state at all. Or, to render the difficulty even more vivid, suppose that $S$ is a drop of water in interstellar space. Won't we require constraints around every inch of perimeter to hold our water fast (e.g., put it a corked bottle)? Thomson and Tait begin with a dubious "if we could" situation and convince themselves that they are considering flexible blobs quite generally. It seems that some "break the infinite recess" assumption that drops of water $S$ can be held in equilibrium balance if they are "made small enough" has been smuggled in.

I'm not sure why Thomson and Tait find such arguments more pellucid than simply applying Lagrange's principle of virtual work directly to the "generalized forces" acting upon $S$, in our earlier manner. Perhaps reducing their operative modeling principles to non-variational claims (Euler's laws) strikes them as
somehow “safer,” despite the oddity of the “rigidification” appeal itself. Quite commonly, older scientists liked to articulate their “laws” X in the format “If X doesn’t hold universally, then Y would be feasible,” where Y provides some physically preposterous condition such as a perpetual motion device. The reasons for such preferences are unclear to me: perhaps they trace to some intuitive conviction that energetic considerations cut deeper to the bone of Nature than straightforward theses as to how forces act.\textsuperscript{101}

In any case, modern books in engineering--more exactly, the sophisticated ones--no longer follow these old policies, which trade upon \textit{rigid body mechanics} as an intermediary doctrine step in sundry ways (as we’ll soon see, large amounts of significant \textit{philosophy} have sprung from this innocent-looking methodological seed). From a practical point of view, such presentations leave us rather confused as to which behaviors are possible--and which are not possible--within a continuous material. Consider, for example, the blocks-and-cords model for a drumhead that we considered earlier. Its little elements have been linked together in such a way that they can only move up and down, but not horizontally. Translating those limitations into “wave” terms, this means that such membranes can transmit only \textit{transverse waves} (like surface water waves) but not \textit{compression waves} (like sound). But are such materials really possible, except in coarse approximation? This is the kind of \textit{inductive guidance} with respect to the behavioral capabilities of materials that we’d like continuum mechanics to provide.

The strangeness of our drumhead’s hypothetical capacities can be made quite vivid if we consider its one-dimensional analog, an oddity that lies concealed within the basic “equation for a vibrating string” discussed in every classical physics primer. In its derivation we tacitly posit that, in its stretching each section of string “remembers” its rest position well enough to remain constantly above it, never veering left or right in the manner of the gray arrow. How can a dumb piece of string achieve this remarkable feat (think of the equipment that a flock of helicopter pilots would require in order to execute the proscribed flight pattern)? In the drumhead case, we surreptitiously employed the “rigidity” of the blocks to enforce the vertically only movements, inserting “cords” to allow each “element” to become effectively longer as it does so. But our string lacks any comparable enforcement mechanism of this kind. Should we conclude that no continuous material can truly behave as our textbook model prescribes or simply that it is unlikely, except in crude approximation?

In fact, nearly all of the standard continuum models studied in undergraduate primers contain some hidden dimension of “unlikely behavior” of this ilk: they
continually ask beams to bend in a plane, say, but to not bulge outward as they do so. But see if you can find a real material that will be so obliging.

On the other hand, real materials do display odd abilities to "remember" their earlier states—vide the paper clips and tooth paste discussed above (and stranger memory capacities are known as well). If we attempt to find an infinitesimal mechanism-like element that duplicate these capacities, we are likely to require strange, Rube Goldberg-like devices.

One can’t understand many of the historical struggles that classical physics has witnessed without a sympathetic appreciation for the inductive problems presented by continuous materials. Lord Kelvin once remarked:

*I am never content until I have constructed a mechanical model of the subject I am studying. If I succeed in making one, I understand; otherwise I do not.*

For this, he was vigorously flogged by Pierre Duhem in *The Aim and Structure of Physical Theory* for falling prey to the “visualizing propensities of the English” rather than displaying the “abstract mind” of French thinkers such as himself (all of this enfolded in unpleasant xenophobia with respect to the “psychologies” of different nations). But recall that questions of the waves that a medium can transmit are intimately linked to questions of how inputted thrusts can be processed within the infinitesimal elements that lie at the heart of continuum physics. “Abstract principles” of the sort Duhem favors may declare that a certain wave behavior X is possible but should one wholeheartedly trust such assurances in the absence of a more concrete grip on how its “possibilities” find instantiation?

So, at base, our “problem of the physical infinitesimal” is one of delineating, with some measure of confidence, the full range of infinitesimal behaviors that can be legitimately expected of the points q within a continuous body.

The great twentieth century investigations into the “foundations” of continuum mechanics led by Clifford Truesdell and his school decided that traditional approaches of the character we have surveyed had jumbled together three basic tasks that should be kept distinct. The first of these chores—"Task A"—is to establish the local existence (and occasional non-existence) of stress, strain, rate of deformation and allied tensors within a continuous body. The second task—"task B"—supplements these guiding principles with “constitutive relationships” that capture why a material like iron differs so greatly from putty or water. Finally, task C exploits empirical determinations of the dominant patterns of thrust propagation within a medium in order to render the results of tasks A and B more tractable mathematically. According to this modern reassessment, the policies pursued by the great nineteenth century masters of continuum physics (Kelvin, Stokes, et al.) had mixed *approximative considerations* properly reserved for task C together with the “general theoretical principles” required for Tasks A and B. Such blurring made it
impossible to answer our "what range of infinitesimal behaviors are possible?" question with any confidence (as already remarked, such questions had assumed considerable practical urgency as industrial chemists attempted to model the strange behaviors of paint and rubber).

To get a better sense of what's at issue here, let's return to our old problem of how to combine the traction vectors acting upon a surrounding shell $\partial S$ modelings with the body forces (including accelerations) that act inside $S$. In particular, let's carve out a finite internal volume $S$ of a body $B$ with an imaginary Eulerian cut. As before, sum (= integrate) all of these actors as resultant forces $F^*$ and torques $\tau^*$ over $S$ or $\partial S$ according to need, just as we did with rigid bodies. But where inside $S$ do $F^*$ and torques $\tau^*$ act? What "representative point" should be appropriate for the finite volume $S$? In the case of rigid bodies, the answer didn't matter because of the rigidity, but lumps of putty will act quite differently according to where $F^*$ is placed. Once we establish how $S$ as a whole behaves, we might be able to assign it some reasonable "representative centers" (its center of gravity, perhaps) but, right now, such "centers" move around inside $S$ considerably according to how the blob is affected by the outside forces. It is at this stage that traditionalist approaches invoke "rigification" or "little mechanisms" within $S$'s that are sufficiently firm to allow our $F^*$ and $\tau^*$ to work upon them in a more determinant manner. But to gain this firmness, the traditionalists invoke constraints and other modeling restrictions that our modernists regard as approximative and wish consigned to the "simplify the mathematics" purposes characteristic of task C's portfolio.

Accordingly, the modern approach advises us to overlook these "how do we halt the regress" concerns for the moment and assures us that we can nonetheless regard Euler's two basic laws of motion (or "balance principles," as they are usually called in this context) as fully applicable to (almost) any Eulerian cut $S$. we might choose. This is a rather abstract claim to accept, due to the fact that we possess little concrete sense of where or how $F^*$ and $\tau^*$ will operate upon $S$. "Have patience, children" our modernists advise, "we'll trap it eventually." Crudely speaking, the proposal is that if we continue shrinking $S$ to ever smaller dimensions, in the final limit, we'll recover those infinitesimal cubes $C$ we considered earlier. In fact, these $C$'s are so small that they no longer qualify as Eulerian $S$'s at all (the $S$'s possess
finite volumes, whereas our \( C \)'s comprise "decorated points"). Due to their minute character, such \( C \)'s will possess one traction vector on each face and only one (summed) body force vector and acceleration inside. Furthermore, the tractions upon opposing faces must be diametrically opposed lest our \( C \) cube find itself subject to an infinitesimal turning moment. The net effect of these forces is to make \( C \) either alter its volume or shear, or some combination of the two. Once we know what happens here, then we can determine what happens in cuts with larger volumes \( S \) by simply integrating all of the infinitesimals \( C \)'s that comprise it.

These procedures probably sound obscure (or even mystical) due to the fact that I've framed the proposal in the language of infinitesimals. So let's purge those notions from my presentation using tensorial objects instead (the basic technique for doing so is rather abstract but beautiful). To do this we must understand how \textit{stress} and \textit{strain tensors} function. I'll begin with the latter, conventionally designated by \( \varepsilon \). Take a point \( q \) inside a finite blob \( S \) and run an oriented reference plane through it (the orientation is supplied by the little gray arrow). Our strain tensor intuitively provides, in the guise of a matrix of nine numbers, how much the corresponding face (or "response plane") of an infinitesimal cube at \( q \) has expanded or contracted (according to whether the center of the response plane has moved outward or inward from the reference plane) and also the degree to which the response plane has become tilted with respect to that original orientation (obviously it can tilt in both \( x \) and \( y \) directions). In other words, a \textit{strain tensor} is a gizmo that maps planes through points \( q \) to new planes (this is part of its proper definition). Employing this strain tensor information about the response planes through \( q \), we can, in effect, reconstruct our original strained infinitesimal \( C \) by calculating the dilation (= compression or expansion) and reorientation experienced by various choices of reference frame as we run them through \( q \). Now there needs to be a gradualist coherence amongst our answers for we want our reconstructed "infinitesimal" to turn out to be a skewed cube and not, say, a skewed dodecahedron. We enforce this coherence amongst our answers through the standard "vector space" qualities demanded of any tensor. The upshot of all of this is that the strain tensor attaching to \( q \) can be fairly characterized.
as "the ghost of a vanishing shape"--the technique captures the data that we need to have installed at \( q \) in a manner that explains why we are intuitively inclined to picture \( q \)'s strained state as an infinitesimal cube with sides.\(^{104}\)

We employ the same techniques to make sense of the stress tensor \( \sigma \) at \( q \), except that \( \sigma \) now places a tilted traction vector \( F \) upon our reference plane. The component of \( F \) that runs normal to the reference plane represents the pressure (compressive or dilatory) that strives to alter the volume of \( S \); the planar component of \( F \) captures its shearing capacities.

Because we normally don't want to deposit any unbalanced torques on \( S \), we require the \( F \) on the other side of our cube (= a reference plane with a reversed orientation) to be equal and opposite in magnitude. Operationally, these requires that matrix of numbers corresponding to \( \sigma \) must be symmetric, with only six independent values.

In any case, our \( \varepsilon \) and \( \sigma \) tensors provide with the basic information we require within our infinitesimal cubes\(^{105}\), while eschewing any talk of "infinitesimals" per se. I hope it is evident that, while the tensorial method for eschewing infinitesimals is quite clever, most of the entangled difficulties within our "physical infinitesimal" packet have been left untouched, for they largely concern the question of the local traits that need to be deposited at \( q \) for continuum mechanics to work coherently.

Once those physical issues have been resolved, any "infinitesimal" proposal can be easily reworked into a collection of tensors or allied objects. Indeed, we'll later see that, within various modern extensions of our subject, physical considerations place an interesting array of rather strange "local objects" on points beyond the stress \( \sigma \) and strain \( \varepsilon \) of mechanical tradition.

Using this language, the result of enforcing Euler's two laws of motion upon (almost) every cut \( S \) we can carve out of a body \( B \) tells us that stress and strain tensors will be locally defined at (most) points \( q \) inside \( B \) and will, furthermore, obey Cauchy's celebrated law of motion:

\[
pD^2q/Dt^2 = \nabla \sigma + \rho \mathbf{g}.\]

Here the divergence operator ("\( \nabla \)"") evaluates how the stress field varies in the vicinity of \( q \) and provides us with a vectorial assessment of where the greatest changes in \( \sigma \) lie. This provides with a density vector that can be meaningfully summed with the body force densities that act at \( q \). Observe that Cauchy's principle looks very much like Newton's second law as it appeared within our point mass setting and many authors identify it as such (although that can be only regarded as a rather diffuse "family resemblance" claim, in that we are plainly dealing with a considerably more sophisticated construction now).
Indeed, it is a mistake—although many elementary textbooks encourage the opposite point of view—to assimilate notions like stress too glibly to more straightforward notions like force (I devoted a fair amount of space to their proper mathematical nature for this reason). Thus many writers will assure their readers that stresses “reflect the short range forces within a material,” which is true in some loose “stresses reflect information about such arrangements” sense (in a fashion that encourages us to conceptualize the underlying material in molecular terms). But there is no ready recipe that converts these molecular “short range forces” into the numerical values that belong to the stresses assigned to points \( q \) within a continuum modeling of the situation.

Perhaps this last point can be clarified with a specific example. The “short range forces” active within most real materials rarely bind them into perfect lattices, but tolerate the irregularities known as dislocations. Large numbers of these lattice defects can emerge at scale lengths that need to treated as “short range” and can affect the macroscopic qualities of a material in significant ways. How, in a classical continuum modeling of our material, should its dislocational properties be registered? A lot of recent work in extended continuum mechanics (I’ll discuss some of this later) has supplied a variety of answers to this question. In some of these schemes, the dislocations are not captured in the material’s strain tensor at all, but within other mathematical constructions attributed to the point \( q \) (e.g., to a torsion within the underlying manifold on which \( q \) lives). Such a torsion answers to the “short range forces” within the material just as ably as does its conventional strain, but follows a different coding scheme.

In truth, when we casually parse stresses as “short range forces,” we are tacitly making a “lift” from continuum mechanics into a different conceptual arena within which the tricky notion of “stress” can be “rationalized” through a rough alignment with a more readily understandable form of material structure. Such “lifts” (which are a common occurrence in continuum mechanics) are fully in accord with the “theory facade” character of “classical mechanics overall, but they can obscure the fact that, considered in their own terms, tensor fields are novel mathematical constructions with their own spectrum of characteristics (indeed, mathematicians didn’t isolate the notion clearly until the end of the nineteenth century). Indeed, it is precisely these special qualities that allow modernists to halt our “labyrinth of continuum” regress in a novel way: they claim that the traction vectors around shrinking \( S \)’s will deposit a localized residue on \( q \) in the form of a stress tensor. With the help of a simple divergence computation, we can then extract a vector to add to the body force and acceleration in a mathematically coherent manner.
There is an additional subtlety connected with Cauchy’s principle that we might note, as it possesses some philosophical salience (although we cannot devote great space to these issues). It concerns the meaning of the mass density $\rho$ that appears in the equation. Within a point mass setting, material points are rarely allowed to alter their masses and hence expressing the doctrine of mass conservation becomes quite trivial. But continua alter their mass densities on a regular basis and it is not obvious how one should enforce mass conservation within a compressible gas that expands and contracts as it moves. The canonical modern resolution employs a so-called reference manifold to keep track of where shifting allotments of flexing matter originally came from. More exactly, we are supplied a map that determines, at each moment in time, to which portion $S^*$ of the reference manifold any finite sized blob $S$ corresponds. Over time, finite-sized blobs that map back to the same $S^*$ must be accorded the same mass. This proviso allows the points $q$ inside $S$ to alter their densities as $S$ shifts its shape (a process we now interpret as “at $t_0$, $q$ is surrounded by shape $S$ and at $t_1$, it is surrounded by shape $S'$, but $S$ and $S'$ map to the same $S^*$ in the reference manifold”). Philosophers concerned with “identity through time” often frame incorrect presumptions about how physics addresses such questions, based upon simple procedures that work for mass point physics. But within continuum mechanics and allied settings, “identity through time” needs to be introduced as fundamental primitive in the manner of our “reference manifold” construction.

Closely associated with such concerns is the question of what a “derivative of position $q$” means in such a context as well. Are we considering a position in the background container space or a position of the moving matter as tracked by the reference configuration? Generally, it is the latter we desire, although, in dealing with fluids, positions in space are much easier to deal with (the standard textbook distinctions between “Eulerian” and “Lagrangian” coordinates trace to this source, as well as the funny capitalized “$D^2/\partial t^2$” notation I employed in Cauchy’s law--it is the so-called “material derivative”). The reader can find a discussion of these matters in any competent book on continuum mechanics.

If we survey the conceptual framework just sketched, we realize that none of the fundamental principles employed directly concern points $q$, but instead talk, in sometimes very abstract ways, about how finite volumes $S$ behave. Thus Euler’s two laws of motion hold only of finite “cuts” $S$ extracted from a body $B$; they don’t
make sense for individual points $q$. Conservation of mass, likewise, concerns how finite blobs $S$ relate to the reference manifold. And so on. Mathematically, such principles need to be expressed by integral differential equations (= equations involving integrals over finite volumes), not as localized differential equations per se.\textsuperscript{107} Cauchy’s law, to be sure, is of the latter class but it has been derived from fundamental integral principles, it has not been posited as “basic.” One of the reasons for doing so, which we’ll discuss later, is that we sometimes want Cauchy’s law to fail at special locales within a flexible body.

Indeed, the “basic law” structure of continuum mechanics is rather complex (we are not yet finished with it!) and runs counter to some of the demands that aprioristic philosophers sometimes place upon their formal structure. What is still missing?

In the “shrinking down to $q$” techniques we’ve just surveyed, we have expunged from the process any appeal to the “little mechanisms” or “rigidification” central in older approaches. Thus we have introduced no hypotheses whatsoever on how thrust gets transmitted across our infinitesimal cubes. So we can expect that Cauchy’s law enjoys great universality with respect to all continuous media—from water to putty to iron--, although that same generality makes “Euler’s laws, as we’ve interpreted them here, uncomfortably abstract in their import. But how do we introduce the particularities of specific materials into our modelings? How do we express that a beam of iron is before us and not a puddle of water?

The answer is that Cauchy’s law can only serve as a framework upon which a certain recipe for building a continuum physics modeling can be assembled, rather as “$F = ma$” serves as the framing for Euler’s recipe in a point mass context (for this reason, I’ll call our new techniques “Cauchy’s recipe,” even though Cauchy himself didn’t always adhere to its strictures). And inspecting the equation itself, we can easily determine that we haven’t been supplied with enough information to construct an adequately “closed” equational system, because its lefthand side contains the position vector $q$ whereas the right hand side mentions sundry force-related quantities ($\sigma$ and $\rho g$). Well, $g$ is usually a simple function of $q$, but we’ve not yet linked $\sigma$ to $q$ in any way at all. And this the “task B” I indicated earlier: the individuality of the material we are considering needs to be captured by constitutive principles that link stress $\sigma$ to other quantities in a way that generates a closed equational system. The most common way to do this, for pure solids, is to link stress $\sigma$ to strain $\varepsilon$ in some direct way. A rough prototype for such a relationship can be
found in Hooke’s familiar law for a pinned spring: \(-m \, d^2x/dt^2 = -k \, (x - x_0)\), where \(x_0\) marks the spring’s rest position. Here “\(x - x_0\)” marks the degree to which the spring is currently strained while “\(m \, d^2x/dt^2\)” marks the restorative stress that push the endpoint in the direction of its rest position (often overshooting in the bargain). But that expression trades upon the artificial one-dimensionality of the modeling and, in the modern approach, we are not allowed to drop dimensions in this manner (doing so is reserved for the “Task C” stage of our enterprise). Nor can we utilize non-local relationships over finite spans such as \(x - x_0\) (a proper strain measure should measure the conditions locally at \(q\), for we want an infinitesimal scale replacement for Hooke’s law to allow spring to transmit compression waves as alterations in the local strain). But the closest linear analog to Hooke’s law for a three-dimensional material is a formula of the form \(\sigma = M \varepsilon\) where \(M\) is a 81 term matrix comprised of sundry “material constants.” The fact that the stress tensor must be symmetric forces many of these constants to be the same and if we further assume that our material is isotropic (= compresses and stretches by the same rules in all directions), we obtain a stress-to-strain relationship that (in component terms) assumes the form: \(\sigma_{ij} = K(\varepsilon_{kk} \delta_{ij}) + 2G(\varepsilon_{ij} - 1/3 \varepsilon_{kk})\) where \(\varepsilon_{kk} = \sum \varepsilon_{ii}\). Note, in reference to our earlier discussion, the two elastic constants \(K\) and \(G\) characteristic of the Cauchy’s “multi-constant” treatment of elasticity. In fact, the modern approach to continua bases its Task A approach to stress and strain upon the “top down” techniques that Cauchy pioneered and further copies him in reducing the \(M\)-tensor to a simpler form exploiting isotropy in exploring possible stress-to-strain relationships under its Task B. In fact, the chief innovation of the modern approach traces to its insistence that the general principles derived under Task A not become confused with the “constitutive equations” (like our improved “Hooke’s law” we explore wearing our Task B modeling hats.

There are zillions of ways in which equational sets can be closed through “constitutive equations” other than following this simple Hookean scheme. Packets of a pure fluids, for example, do not care in the least if they are strained--they are “happy to go with the flow,” as the cliché says. What they don’t like (at least those with any viscosity to them) is being sped up relative to their neighbors. To capture this behavior, we assign a viscous fluid a rule that links its sheer stress to its rate of deformation (a different tensor in the general “strain measure” family). If this relationship is simple, we obtain the celebrated Navier-Stokes equations.\(^{109}\) Viscoelastic materials such as paint and asphalt are sensitive to both strain and rate of deformation. To model materials with “memory” (such as our toothpaste), we require stress/strain relationships that also reflect the material’s previous strain history (I’ll come back to some of these “memory” effects later).

Within the point mass setting of section (iv), particular materials were credited
with behavioral individualities through (i) choosing the number of particles present within the system and (ii) assigning them "material constants" (mass, charge, etc.). These constants then "turned on" an appropriate set of "special force laws" in the modeling such Newton's law of universal gravitation. Modeling specifications of this character we called a "constitutive assumptions," secretly borrowing terminology from a continuum context. When we have successfully assembled a closed equational system by these procedures, we were say that we have thereby followed Euler's recipe. Although this portrait of modeling technique within physics is both simple and appealing, in point of brute fact one forever finds practitioners evading the recipe's dictates through appeal to $\Delta L^*$ level constraints and allied modes of "physics avoidance." Indeed, those methodological intrusions have become so pervasive in practice that most point mass modelers appear to forget that they have any obligations to track down a full set of "special force laws" at all. But the appeals that typically displace "special force laws" within such contexts scarcely seem "lawlike" in their own right: "X is a rigid rod" does not sound much like a "law of nature." In that sense at least, it is misleading to insist that such physicists and engineers are "seeking to find the laws to which Nature conforms."

Within our current continuum mechanics program, we are not allowed to invoke constraints in setting up our fundamental modeling equations (such procedures are entirely consigned to the Task C agenda we shall survey later). But what is the present analog to our former "special force laws"? Answer: the stress/strain constitutive assumptions we have just examined. "But there are zillions of these," we might protest. "Don't workers in continuum mechanics attempt to reduce their multitude to a smaller collection?" And the answer is: no, they don't; they merely try to sort the possibilities into general classes, so that the simplest forms of stress/strain behaviors can be studied first. In other words, they structure their textbooks to supply a gradualist taxonomy of possible constitutive behaviors, but no reductive listing of "special force laws" is ever offered. Indeed, in typical modeling practice, the "constitutive principles" assigned to a material are generally determined through direct experimentation on large hunks of the material in the laboratory (we study how the toothpaste flows in a set of carefully designed experiments). In this manner, in a Cauchy recipe modeling, its core "constitutive equations" reflect a projection of behaviors witnessed experimentally at a large $\Delta L^*$ length scale down to an infinitesimal scale.

Should the complicated stress/strain relationship that toothpaste obeys (or, more exactly, the relationship that a particular brand of toothpaste obeys) qualify as a "law of nature"? Practitioners in the subject are often happy to dub it "the constitutive law for toothpaste" (vide "Hooke's law," which is of the same equational type), but many philosophers would demur, considering our toothpaste principle as
no more a “law of nature” than “this bar is rigid” is. I won’t attempt to adjudicate this dispute here, but our reflections should inspire a higher degree of descriptive caution than many philosophers manifest. It is very common for philosophers to write glibly of “the laws of classical physics,” but it is not evident, on the basis of the considerations we have scouted here, that they could concretely sift through a college textbook and confidently sort out the “laws” from the “non-laws.” It might be reasonable to hope that a “better physics” might supply a better set of succinct “laws” than continuum mechanics offers, but we should be careful of converting hopes into verities without a more careful discussion (just because one has heard others frequently speak of “the laws of classical physics” doesn’t mean that there is any clear answer as to what the full collection might look like).

If we do accept constitutive principles as “laws,” then the “law”-structure of modern continuum physics is rather unusual, in that its Task A set of principles directly concern finite volumes of a material, while its Task B claims (usually) operate at the wholly infinitesimal level.\textsuperscript{110}

I can’t explore such issues properly here, but the great debates over the “proper mission of science” that one finds in nineteenth century authors such as Ernst Mach and Pierre Duhem relate intimately to these organizational concerns. In fact, Duhem himself, in his scientific work, can be fairly credited as an important forebear of the “modern approach” to continuum mechanics outlined here. Although both men made unwise (and, for their ultimate purposes, somewhat unnecessary) complaints about “atoms,” insofar as one of their chief objectives was to argue for a non-reductive approach to physics, their expectations have been largely vindicated in the way that continuum mechanics developed over the course of the twentieth century. I’ll return briefly to some of these matters near the essay’s end.

Let us finally turn to our “Task C.” According to the modern program under review, we should not invoke constraints of any sort in setting up our basic “constitutive modelings.” But this methodological prohibition is commonly violated within traditionalist presentations of continuum mechanics. Consider again the illustrated “infinitesimal element” for a Bernoulli-Euler beam. Note that the applicable pushes and pulls upon the element are assumed to balance along fibers running across the material. But what kind of assumption is that? Well, it represents a constraint upon permissible behavior, of the same general character as was examined in the previous section. According to Cauchy’s modeling recipe, we should instead supply constitutive equations of a Hooke’s law ilk able to insure that stresses will be largely conveyed across the element in this fashion. Great—but see if you fill out a matrix of coefficients that will do this. And the sad truth is that this task isn’t at all easy, for more or less the same reasons as finding a point mass modeling that could approximately support the constraint of “frictionless sliding”
proved elusive.

In fact, we've canvassed this same problem already, in the humble form of the vibrating string. The constraint critical to the simple "derivations" found in most college textbooks maintains that string "elements" forever hover infallibly above their original rest positions. But try to find a set of stress/strain relationships that can simulate this behavior approximately within a three dimensional material. This is a quite daunting task.\textsuperscript{111}

Plainly, adhering to the foundational clarity demanded within our modern approach places ghastly burdens upon beginners in continuum physics, for the path to the one-dimensional wave equation has become strewn with knotty mathematical thorns. So this sets up a rationale for setting up an approximative division of continuum mechanics that investigates how our strict Task A/Task B modeling requirements can be profitably circumvented through the wise exploitation of $\Delta L^*$ level constraint information. Doing so frames our final Task C: to develop "mixed level" modeling techniques that relate to the strict "constitutive modeling" requirements of "foundational" continuum mechanics in the same manner as the evasive techniques of analytical mechanics relate to Euler's recipe.\textsuperscript{112} From this point of view, it isn't surprising that the characteristic emphases of analytical mechanics make a strong reappearance for we've already noted that this branch of mechanics is best understood as practicing the art of employing $\Delta L^*$ scale constraints to isolate the pathways of dominating activity within a complex medium. As a result, traditional continuum mechanics becomes riddled with innumerable "lifts" into rigid body mechanics, as the demanding modeling requirements of Cauchy's recipe get tempered with the of more tractable mathematical structures we have now consigned to Task C approximative work.

Although from this point of view, any Task C exploitation of constraints represents an approximative procedure, it is often only an "approximation" in the aspirational sense that we discussed with respect to point mass modelings: we may not know of any strict modeling to which our Task C approach to a string or beam actually approximates. It may very well be that there isn't any and that physical considerations that underpin our modelings trace to other sources (e.g. quantum mechanics). For this reason, task C modeling techniques (which, after all, compose most of what one finds in the literature) float rather freely above our concrete ability to rationalize such techniques in "fundamental continuum physics" terms.

On the other hand, one shouldn't dismiss the fastidious cavils of the modern school as stemming entirely from fussbudget impulses, for, as we've already seen, the price of a convenient
constraint is often a nettlesome set of subsidiary oddities that sometimes create serious headaches. And this phenomenon occurs with distressing frequency amongst the task C modelings pursued in practice. Whenever they can, engineers drop dimensions freely, appealing to some loose constraint as "the membrane or string is pretty stiff in a horizontal direction, so all of the important action must occur in a vertical direction." Sometimes such policies merely engender relatively harmless puzzles (such as "if a bit of string never moves horizontally, how does it manage to become longer when it rises above its rest position?"), but sometimes they led to genuine trouble. If we merrily "approximate" two neighboring plates in an architectural dome in locally convenient ways, we may easily find ourselves with a combined model that leads to numerical disaster when fed into a computer (the entire theory of plates and shells is riddled with "surgery" problems of this sort).

Related problems often arise in attempting to fit a continuum modeling to the "boundary conditions" that appear to hem in the material in Nature. Thus a boundary condition appropriate to a string or drumhead will reflect how the material attaches to the nut or drumhead. And usually the formal conditions we invoke for this purpose ("the string always remains motionless at the nut") are quite simplified representations of physical circumstances that are quite complicated at a molecular level. And sometimes our natural simplifications of the interior of a material clash with our natural simplifications of the physics prevailing at its boundaries. A celebrated exemplar of these troubles can be found in the annals of aerodynamics. The viscosity of air is extremely low and for a century and a half physicists presumed that such terms could be dropped, in canonical Task C fashion, from the pertinent equations without serious loss. Unfortunately, when the molecules of most fluids (including air) meet a solid boundary, they stick there, engendering a "no slip" situation on the continuum mechanics level (no slip = the tangential velocity of the fluid at the surface must be zero). But if we've incautiously discarded viscosity terms, then our "reduced variable" modeling equations can't satisfy such conditions. And that tiny mismatch leads quickly to all sorts of erroneous conclusions, including the predication that heavier-than-air flight is impossible. It was only at the beginning of the twentieth century that Ludwig Prandtl realized that the omitted terms play a crucial role in allowing fluid to match its boundaries properly (and, with that, the appearance of lift and drag in an airplane wing).

Traditional workers in continuum mechanics often experienced a great deal of difficulty in unraveling these paradoxes due to the haphazard manner in which they freely mingled the Task C exploitation of constraints with more basic modeling necessities. In particular, they realized that our "labyrinth of the continuum" regress needed to be arrested in some manner that would permit them to assign stress and strain tensors to most points inside a continuous body. As we observed, the modern
approach fulfills this chore in a clean way through the Task A part of its program. But traditional modelers often appealed to infinitesimal constraints to halt that regress instead, largely because such restrictions would be eventually needed before modeling sets of any practical utility could be assembled. But mixing up Task A and Task C requirements in this manner (and suppressing many Task B requirements along the way) left our traditionalist with no easy route to unraveling the conflicts and paradoxes that represent the natural side effects of effective "variable reduction."

Plainly, continuum modeling could have never gotten on its feet historically without the temporary assistance of rigidified infinitesimals and "little mechanisms." If d'Alembert, the author of the first PDE for a vibrating string, had felt obliged to deal with matrix equations containing 21 independent constants, continuum technique would have been abandoned as stillborn at birth. All of this merely underscores the lessons we have noted with respect to the secret contribution of "lifts" with respect to classical mechanics' triumphant hegemony.

But the specific constraint-assisted lifts that helped traditional continuum modelers on their way had the curious effect of encouraging themes within the philosophy of science that continue to reverberate strongly even to this day. They trace to the following factors. In order to block the "never simplifying" regress created by flexible materials that behave identically on all size scales, traditional modelers assumed that small portions of a material will behave that resemble little or mechanisms. In doing so, they tacitly credited the lower scale lengths of a material with characteristics that they didn't believe they really possess. It then appears that we can't set up coherent "foundations" for flexible bodies without injecting patent descriptive fictions to arrest an otherwise vicious regress. And so the thesis emerges that physics can't begin its descriptive tasks until it has first indulged in a preliminary degree of essential idealization: smallish portions of materials must be credited with patently incorrect characteristics. After we reach a completed modeling, we can throw away the idealized ladder we have climbed, for our final equations will describe materials that behave identically at every scale. But on route ther, we we must accept, in the physicist J.H. Poynting's phrase, a fictive "scaffolding from without."

While the building of nature is growing spontaneously from within, the model of it we seek to construct in our descriptive science, can only be constructed by means of scaffolding from without, a scaffolding of hypotheses. While in the real building all is continuous, in our model there are detached parts, which must be connected with the rest by temporary ladders and passages, or which must be supported till we can see how to fill in the understructure. To give the hypotheses equal validity with the facts is to confuse the temporary
scaffolding with the building itself.\textsuperscript{113} Observe that Poynting presumes that the materials found in nature are continuous in their behaviors and that our intervening "scaffolding" of point masses and little mechanisms misrepresent their lower scale structure.

I believe that the assumption that some form of "essential idealization" must be invoked to arrest the continuum physics' "labyrinth of the continuum" problem has played an important, if often unacknowledged, role in shaping the doctrines of the philosophers who pondered the problems of classical matter carefully: Leibniz, Kant, Duhem, Hertz, Mach et al.\textsuperscript{114} Its enduring legacy is the lingering presumption that\textit{ intentional misdescription} represents a commonplace activity in scientific activity. In retrospect, however, this philosophical thesis seems to have engendered by the "lifts" required to link Task A/Task B modeling demands with the more relaxed standards required in practical work of a Task C cast.

At a number of points, I have rather mysteriously qualified our Task A objectives: "to deposit stress and strain tensors at (almost) all points \( q \) within a body." But why did I include that "(almost)" qualifier? Well, real materials possess external boundaries, cracks, joins and other forms of interface where ordinary stresses and strains can't make sense. To appreciate the difficulties, let's discuss one of the most remarkable forms of internal interface: the\textit{ shock wave}. If we set a pulse of gas into motion inside a long tube, the region will move to the right and, if one portion moves faster than the rest, a big pile up will eventually occur akin to a traffic jam. This is our "shock wave" and the center of it will continue moving through the gas affecting the density of the regions through which it passes in characteristics ways. A supersonic bullet will force the air before it into a "shock bow" and wind tunnel shows how dramatically such bows affect the regions of air through which they move. From a molecular point of view, their formation is easy to rationalize in a "traffic pile up" sort of way. But within a continuum framework, these molecular events register as serious mathematical difficulties, because our continuum gas equations become inconsistent at such moments, due to the fact that our flowing gas is forced to assume\textit{ incompatible densities} in the places where shock wave occur. Technically, the continuum equations undergo a mathematical "blowup," somewhat akin to the one we discussed in connection with Xia's gravitational problem.\textsuperscript{115}
So a straightforward continuum approach to our gas breaks down at such moments, which is unfortunate for both computational and foundational reasons. Computationally: because one scarcely likes to abandon the convenient framework of continuum modeling for the arduous details of what occurs amongst the individual molecules within our piled up swarm. (although such details are sometimes intensively investigated). Foundationally: because our modeling has developed a big internal hole at just the moments when we'd like it to plot the gas's continuing behavior. At this point, Riemann suggested a rather surprising technique for extending continuum mechanics' modeling capabilities beyond these moments of equational breakdown. In certain ways, his suggestion harkens back to Newton's approach to billiard ball collisions. Recall that, rather than delving into the lower scale $\Delta L$ compression events that actually transpire when such balls meet, Newton simply erases the problematic temporal interval and replaces it with an "impulsive" crash on the $\Delta L^*$ level. In mathematical terms, Newton replaces a very complex history of smoothly varying ball events with a brute singularity in which we no longer assign our balls determinant velocities (i.e., we can't determine if they're compressing or expanding). Perhaps, Riemann suggests, we might likewise excuse the shock wave region from any obligation to fully satisfy the demands of our underlying gas law? But we will still need some rule to tell our new shock fronts how to move, given that they're no longer obliged to satisfy the old gas law. Recall that Newton drives his billiard balls through their moments of collision by demanding that they conserve basic quantities such as momentum and kinetic energy across the omitted interval. So perhaps we only need to require our shock wave to obey comparable requirements from continuum mechanics: fresh energy or mass can never be created when a shock wave moves through a region and so forth?

We certainly want to place such conservation demands upon our shock waves (they are called "jump conditions"), but they will unfortunately not prove strong enough to fully fix how the waves should move. And here we should observe that PDEs, in general, represent rather tightly organized affairs mathematically. If we ask them to tile a room and we start them off with a fixed pattern on three sides, they can usually complete the job without a hitch.\textsuperscript{116} But if we draw a curve $C$ in the middle of the floor and declare
“break your pattern here,” our PDEs usually won’t know how to restart their tiling pattern on the other side of \( C \). And that’s the difficulty that the toleration of shock fronts poses for continuum mechanics. We can extract sundry jump conditions pertinent to the tiling from our Task A principles, but these requirements allow the PDE’s wide latitude in how they commence their tiling afresh. Purely “mechanical” jump principles aren’t able to interconnect the two sides of a shock wave in a sufficiently robust manner that our PDEs know how to move the shock wave through its surrounding gas (moral: if you relax the requirements of a differential equation “law,” be prepared for the anarchist consequences).\(^{117}\)

Riemann made the startling suggestion that the answer could come from thermodynamics: passing through a shock front disorganizes a gas and leaves it in a state of increased entropy afterward. Indeed, with that supplementary information, the manner in which the gas density distributes itself behind the shock front becomes determinant.\(^{118}\) We’ll return to the doctrinal ramifications of this surprising intrusion of thermodynamics into what appeared to be a “purely mechanical” setting, but in a moment let’s concentrate on what Riemann’s proposed extension signifies for our tensorial resolution of our “labyrinth of the continuum” regress.

In the naïve form we articulated, our Task A approach to mechanics presumes that Euler’s two laws hold true, in an abstract manner, for any contracting sequence of cuts \( \mathcal{S}, \mathcal{S}', \mathcal{S}'', \ldots \) surrounding a target point \( q \). And this presumes that their respective perimeters \( \partial \mathcal{S}, \partial \mathcal{S}', \partial \mathcal{S}'' \), can carry full complements of traction vectors. But this demand is too strong, partially because some \( \partial \mathcal{S} \) are too irregular to bear such measures, but also because such requirements need to fail when \( \partial \mathcal{S} \) cuts through a portion of shock wave surface (some irregularity must prevent the contracting cuts \( \mathcal{S}, \mathcal{S}', \mathcal{S}'', \ldots \) from installing stress and strain tensors upon these problematic points).

In point of fact, the canonical modelings of traditional mechanics have long tolerated similar “funny spots” (= singularities) upon their boundaries. For example, take a notched rod and pull upon its two open faces with an uniform tension. The result is an impossibly large (= infinite) twist along the base of the cut. One is tempted to evade the problem with simple “Oh, you really can’t have perfect notches in nature,” but we shouldn’t wish to discard most of the results of classical continuum research in such an offhanded matter (those lovely models supply us with exceptionally salient information about the behaviors of real life materials). Nor should we wish to automatically privilege the “correctness” of the inner description of a flexible body over the “boundary conditions” that seem to fit it. Accordingly,
modern treatments of continua employ rather fancy tools from functional analysis ("trace operators" and all that) to bring the inner and boundary descriptions of continuous bodies into better mathematical accord. Very subtle considerations with respect to energy storage typically lie in the background of such interior/boundary "harmonizations."

Let me here inject a quick comment on the term boundary condition, for it frequently serves as a locus of significant philosophical misunderstanding (for reasons now lost in the mists of logical empiricism, philosophers are trained to employ this stock classification of applied mathematics improperly). Typically when one assembles a PDE model (following Cauchy’s recipe, say), one attempts to capture within ones equations only the physical processes that are actively at work within the interior of a body \( B \). In most circumstances, quite different physical processes are active within the outer molecular layers near the bounding surface \( \partial B \) (a fact that we’ve already noted in regard to “frictionless sliding”). Cornelius Lanczos observes:

> And here we have first of all to record the fact that from the physical standpoint a “boundary condition” is always a simplified description of an unknown mechanism which acts upon our system from the outside... Imposed boundary conditions are merely circumscribed interventions from outside which express in simplified language the coupling which in fact exists between the system and the outside world.\(^{19}\)

For such reasons, traditional continuum mechanics has always allowed singularities and other oddities to sit upon the boundaries of its models, in a Bob Dylanish mode of “something is happening there, but we don’t know what it is” (\( = \) the singularities register the interior-relevant effects of complex surface/interior interactions in some manner doesn’t need to be unraveled to resolve the problem at hand).

In fact, in their “foundational” work, writers like Truesdell largely confined their attention to “universes” no larger than the interior of a non-composite blob, partially on the grounds that framing appropriate relaxations of our \( S, S', S'', \ldots \) demands become harder to draw as complicated forms of interface (such as shock waves or cracks) become tolerated. More recent work, however, has explored ways of modeling wider structures carrying significant interfaces within a continuum mechanics frame, generally through employing clever variants upon Riemann’s proposal for shock waves. We’ll survey a few of these later.
But such limitations on the “universes” of standard continuum mechanics are plainly at odds with our intuitive expectations with respect to “the possible worlds of classical physics.” What kind of “world” is it if the relevant discipline refuses to contemplate composites as simple as a jelly doughnut? How can it happen that observers have overlooked the obvious fact that orthodox continuum mechanics is very restrained in its modeling ambitions? And the answer lies in the fact that we commonly escape from strict continuum mechanics' conceptual confines to other modes of classical mechanics whenever significant questions of “how do the various flexible bodies found in our universe fit together?” emerge. And often we make our getaways without noticing that we’ve left the original territory. Here's an example of this phenomenon that I regard as particularly telling. Pull a knife through some water, drawing the top layer of the water with it. Intuitively, we expect that, after a certain period of mixing, the waters on the two sides of the cut will soon fuse together. But according to the story that the PDEs of continuum mechanics tell, this wound can never heal, for differential equations cannot alter the topologies of the flows they track. But these descriptive limitations entail that, without some significant alteration, the mathematical framework of orthodox continuum mechanics can model neither the fusion nor the fracture of ordinary materials (which is why the subject traditionally confines its attention to non-composite blobs in circumstances where they are unlikely to suffer fracture or fission.

To anyone who has not scrutinized the standard “lifts” of mechanical tradition in the critical manner of this essay, this claim will seem outrageous: “Of course, classical mechanics can readily handle mixing: the molecules from each side of the cut rapidly intermingle until it becomes impossible to determine where the dividing boundary had been.” Yes, but observe that in this rationalization we have escaped into a reentologized ΔL domain governed by point mass mechanics or something similar. It’s “classical mechanics” all right, but it’s not the same continuum mechanics with which we started. Due to these readily available “lifts,” one can learn a substantive amount of fluid mechanics without realizing that ones PDE tools are limited in this way.
For the same "can't change topology" reasons, conventional continuum mechanics cannot model the fracture of materials either. But, of course, the shelves of any engineering library are crowded with hefty volumes on "classical theories of fracture." And if one looks into these, one finds that most of them follow a prototype that was pioneered by A.A. Griffith in the context of brittle materials. He employs a technique that we might dub "monitored model switching." It works like this. Subject an unsullied specimen \( B \) of glass to its predicted PDE evolution according to conventional continuum mechanics. If we twist its boundaries too severely, internal stresses will soon mount beyond the laboratory breaking strength of the material, although those critical values rarely supply an accurate prediction of how the slab will shatter in other configurations. So Griffith suggests that we should simultaneously run a set of auxiliary simulations upon a host of altered blobs \( B^C \) that differ from \( B \) in containing cracks \( C \) of microscopic length. In addition, we must supplement our \( B \) modeling with rules that determine when these \( C \) will absorb or release energy in the form of surface strain. With this additional data, Griffith proposes the following criterion for predicting when the original \( B \) will shatter. For each \( B^C \) possibility, determine, as energy is gradually fed into the glass through increased sheering, whether it becomes energetically advantageous for \( B^C \) to store some of this burden in the form of surface energy within a longer crack \( C^* \), rather than lodging everything as conventional strain energy within the unblemished parts of \( B^C \). Whenever the answer becomes "yes," Griffith predicts that an unmarred \( B \) will probably fracture within one of the regions where it becomes energetically advantageous for small cracks to enlarge. We usually can't make a fully deterministic prediction of where the breakage will occur because a range of distinct \( C \) might be able to reduce the overall strain on the glass in equally effective ways. After the fracture event, one begins with a new continuum modeling problem, now working with a reframed \( B^* \) with cracks in it.

Again, this technique is all "classical mechanics," but it represents a "classical mechanics" that considers the relationships between a variety of different modelings, rather than following any single model straight through. As such, it represents a "lift" of a somewhat different character than we've examined before, but it is equally capable of disguising the brute mathematical fact that the tools of conventional
continuum mechanics cannot handle fracture or fusion by themselves (we’ll examine several ways in which an extended continuum mechanics can perform somewhat better in these regards).

Although I’ve here discussed such issues in rather formal terms, many of the great historical philosophers of matter (e.g., Locke, Leibniz, Kant) commented upon the fact that the everyday processes of cohesion and disassociation appear very mysterious from a mechanical point of view. Only the mass point approach handles such topics with any satisfaction yet it is unable to equip materials with the characteristics they need when they’re not about the business of breaking or fusing. The only route to a satisfactory coverage of common forms of everyday material behavior is to weld together a “classical mechanics” from different descriptive platforms assessable to one another along suitable “escape hatch” ladders.

These modeling limitations have led most contemporary experts in continua to see the strict Task A/Task B/Task C program as outlined here as excessively “purist” in its contours. In the present form, these demands were articulated by Clifford Truesdell and his school, partially in the hope that continuum mechanics might prove a crisply delineated mathematical discipline in the manner that Hilbert sought. Today’s experts fully agree that it was necessary to untangle the confusions of methodological purpose that led traditional modelers to employ unrealistic rigidified “elements” as a means of alleviating their “labyrinth of the continuum” worries. But many practitioners now reject Truesdell’s vision of a pure “Task B” taxonomy that might anticipate and categorize every conceivable “constitutive modeling” potentially wanted in the modeling of a real life material. 

Reasons for their hesitancy can be illustrated if we consider some of the “memory effects” mentioned earlier (such as the paper clip that “recalls” its previous history of bending). Within Truesdell’s purist framework, such examples indicate that stress/strain relationships of a widely encompassing “functional dependency” class should be investigated. But most modern experts find Truesdell’s proposed tolerances too unfocussed in their generality and complain, “Why don’t we instead take our clues from Nature as to what kinds of stress/strain relationships are worth studying?” And often those “clues from Nature” derive from lower scale AL understandings of what transpires within the material. Among the many exotic forms of memory effect, there is an intriguing class of strange materials such as NiTi that can remember two prior configurations in which it had been originally molded, depending upon the temperature. If the temperature is higher
than T*, such “shape memory alloys” elastically rebound to a straight rest configuration after they are manipulated, but will instead favor a curved rest condition once the temperature falls below T* (such materials are useful in microscale medical assembly, because they unfold into different configurations at different temperatures). If we inspect the grain of the NiTi upon a ΔL level, we can qualitatively understand why this occurs, roughly as follows. Little inclusions in the bar of a nickel/titanium alloy undergo a phase change below T*, making its minute crystals prefer a “twinned” rather than cubic structure (the first phase is called “martensite” and the second, “austenite”). The newly formed martensite then rearranges itself and grows in skinny slivers through the rest of the material, causing the bar to bend overall. When the material is reheated, the NiTi reverts to its cubic configuration and the bar straightens out. Since these patterns of recrystallization always occur in the same way, our bar displays its odd “dual memory.”

A modern continuum modeler observes this ΔL scale structure and seeks a means for capturing its gross effects in ΔL* terms (so that an engineer can deal with large chunks of NiTi in a profitable manner). Sometimes the proposed ΔL* continuum physics representation will conform to Truesdell’s “memory functional” expectations, but in other cases more radical Riemann-like innovations work better. For example, dislocations force a lattice to capture more strain energy within its webbing than it would require if it were a perfect crystal, in a manner that the lattice can’t easily rectify (just as a ring welded under pressure has no means of releasing the tensions trapped within). A number of observers have observed the similarity of a dislocation on a ΔL scale to a torsional incongruence within differential geometry and have experimented with registering the dislocation energy at the continuum ΔL* level as a geometrical distortion within the manifold upon which the material lives.

Once modeling endeavors are viewed in this “look to the ΔL level for inspiration” mode, it becomes less obvious that our fundamental Task A tenets should be treated as utterly sacrosanct. In particular, Euler’s two balance laws prevent any residual turning moment from becoming deposited directly on q, resulting in a symmetrical stress tensor. Usually, this is an empirically justified assumption. But suppose that q contains internal resources for resisting such turning. From a ΔL point of view, this proposal gains some plausibility, for liquid crystals (= the stuff in the digital displays of electronic equipment) consist of needle-like molecules that reduce strain energy in a magnetic field by twisting in alignment with it (thus producing the visual numbers one sees when the juice is turned on). When we we shrink down to a q-level in such cases, perhaps we should allow some residue of turning response to be deposited upon q itself. A. C. Eringen writes:

_The concept of microcontinuum naturally brings length and time scales into_
field theories. The response of the body is influenced heavily [by] the ratio of the characteristic length $\lambda$ (associated with the external stimuli) to the internal characteristic length $l$. When $\lambda/l \gg 1$, the classical field theories give reliable predications since, in this case, a large number of particles act collaboratively. However, when $\lambda/l \approx 1$, the response of constituent subcontinua (particles) becomes important, so that the axiom of locality underlying classical field theories fails... Nature abounds with many substances which clearly point to the necessity for the incorporation of micromotions into mechanics. Suspensions, blood flow, liquid crystals, porous media, polymeric substances, solids with microcracks, dislocations and disclinations, turbulent fluids with vortices, bubbly fluids, slurries and composites are but a few examples which require consideration of the motions of their microconstituents, e.g., blood cells, suspended particles, fibers, grains, swarms of liquid crystals, etc.\textsuperscript{124}

On his approach, we add “internal variables” to capture the “micromotions” in a fiber bundle construction (allied ideas were proposed by the Cosserat brothers in the early twentieth century).

Operationally, such “look across size scales for inspiration” tactics can deflect continuum physicists from fulfilling the “purist” project of delineating all Task B possibilities in a sharp and definitive manner, in the same manner as “impure” borrowings from $\Delta L^*$ level constraints led point mass modelers to shirk their “foundationalist” obligations to deliver a full complement of “special force laws” required for an adequate point mass mechanics. But why should a practical scientist accede to such “foundationalist” demands, if they merely encourage wasted effort with respect to excessively permissive stress-strain relationships, while simultaneously discouraging the parallel exploration of Riemann-like tricks for integrating novel representational elements like torsions into the continuum mechanics frame? In such “impure” attitudes (which strike me as entirely reasonable), some of the “little mechanism” inclinations of nineteenth century workers in continuum mechanics receive a partial vindication, in the guise “seek modeling guidance from $\Delta L$ scale structures.” But to a modern modeler, these inspirational $\Delta L$ level structures don’t need to be classical models in any obvious sense and may operate according to intrinsically quantum mechanical guidelines.

I believe that the tolerant attitudes that the modern worker in continuum mechanics brings to her subject matter offer valuable lessons with respect to “the pursuit of rigor” within a wide variety of topics far removed from “classical physics.” I’ll return to this theme shortly.
But, for the moment, let's examine a second surprising aspect of Riemann's approach to shock waves: the fact that the thermodynamic entropy of the gas flow needs to be invoked to reach correct answers on how the gas will behave after a shock wave has formed in its interior. We started with gas equations that traffic entirely in "purely mechanical" notions (stress, strain, etc.) and have discovered that, after a finite lapse of time, those formulas can no longer reach a satisfactory equational closure on their own terms—the docket of "primitive notions" they employ must be enlarged to include "heat" and "temperature" along with equations that handle their behaviors. This realization forces us to reconfigure our Task A endeavors so that, in shrinking down to the point level \( q \), local measures of new qualities like "internal energy" \( Q \) can (usually) become installed there, along with \( \varepsilon \) and \( \sigma \) (\( Q \) codifies the density of energy stored as heat at \( q \)). Because energy can now appear in \( Q \)'s non-mechanical guise, energy conservation can no longer be established as a consequence of "mechanical" principles alone and must posited as an independent general law of mechanics. And further thermodynamic tenets will be needed to govern the production of entropy within an evolving process (the most common candidate for this role is the "Clausius-Duhem inequality," which captures the somewhat hazy "Second Law of Thermodynamics" in a more pungent mathematical form). Using this, we can uniquely predict the manner in which gas densities will distribute themselves behind a moving shock wave. Generalizations of traditional continuum mechanics in this vein are sometimes called thermomechanics, a term I will employ in the sequel. Some disagreement remains as to what its exact doctrines should comprise, for reasons we shall survey in a moment.

Insofar as the macroscopic world around us goes, there are plenty of reasons for being interested in thermomechanics beyond the "equation closure" oddities displayed by shock waves. The stress/strain relations displayed by most everyday materials show a palpable sensitivity to temperature: it is easier to stretch an iron bar if it's hot than cold. And temperature often couples strongly to mechanical movement: passing vigorous waves through a medium can heat it up. Duhem and Mach tried to steer physical research in such a "thermomechanical" direction in the late nineteenth century, through methodological urgings of a character: "Shouldn't we be directly responsible to Nature's materials as we find them in real life?" And once the door has been opened to primitive stress/temperature couplings, shouldn't similar allowances be made for the factors that drive chemical alteration and phase...
change (as when water freezes to ice)? To get this new zoo of critters to cooperate sensibly with our old mechanical menagerie, the most direct route works with “virtual work” pairings (e.g., TδS) of the sort surveyed in the context of analytical mechanics (Duhem rather oddly views their thermomechanical centrality as a return to the glories of Aristotelian physics).

Truesdell hoped that a developed thermomechanics of this ilk could demonstrate the self-contained conceptual closure expected within a Hilbert-style axiomatized framework (certainly of the pretenders to the throne “classical mechanics,” thermomechanics can offer the best foundational credentials in its favor). Insofar as I can determine, most contemporary experts in the field now believe that Truesdell’s ambitions were overly optimistic in these respects (and, probably, unduly driven by a faulty philosophical conception of what a “real scientific theory” should look like). There are a wide range of useful flavors of “extended thermodynamics” in development, but they rarely pretend to adequate coverage of every anticipated application. Such completeness difficulties aren’t particularly surprising because thermodynamics ipso facto applies notions like temperature and entropy to rapidly changing dynamic circumstances (e.g., sound waves passing through a medium). But our lower scale ΔL understanding of disordered kinetic behavior warns that small blobs of material can’t be assigned coherent “temperatures” until they have been allowed sufficient “relaxation time” to reach states of local equilibrium. Principles such as the Clausius-Duhem principle can turn dodgy or ambiguous when a material’s rates of significant mechanical change become comparable to its relaxation times. So modern continuum modelers usually chasten their ΔL* scale descriptive ambitions by consulting the processes active at smaller ΔL (and Δt) scale lengths.

If we consider “the world of physics” as it seemed to appear circa 1890, we realize that a rather unexpected regress has emerged with respect to the nascent kinetic theory of heat. A large number of empirical considerations suggest that the phenomena that appear as “heat” upon a ΔL* scale represent the handiwork of large swarms of molecular blobs operating upon a ΔL scale. It is for this reason that Machian and Duhemian proposals to organize “fundamental physics” within a thermomechanical vein were resisted. Yet we’ve already seen that those molecules appeared to be flexible bodies of some kind (they must display regular spectral signatures, inter alia). Moreover, the behaviors of most natural materials are non-linear in character and, as such, prone to develop internal shock waves if jostled hard enough. If so, mustn’t our banished thermal ideas make a reappearance on the ΔL level, to govern the inevitable shock fronts inside our molecules? And so any “purely mechanical” version of continuum mechanics seems to face a problematic
regress. When we attempt to extract methodological morals from the debates that transpired over "atomism" during the nineteenth century, led by skeptics such as Mach and Duhem, it should be remembered that a "thermomechanics" of the contours they favored has become the working core of modern engineering practice, despite the fact that they proved deeply wrong with respect to the atomic view of matter.

Although it is beyond the scope of this essay to survey such endeavors adequately, much recent research in continuum mechanics has explored the possibilities for handling internal interfaces within a generalized thermomechanics using Riemann-like supplements. For example, the dividing line between ice and water often moves through saturated soil as an advancing front (such problems are usually called "Stefan" or "moving boundary value" problems--they are usually difficult to treat from a mathematical point of view). The shape and velocity of these moving surfaces will be affected by the condition of the soil through which they move, so one would like to introduce "configurational forces" into our continuum picture to capture interactions between front and medium in a manner that doesn't move masses in any conventional way (per standard \( F = ma \) expectations). Such constructions tend to be rather sophisticated. In an allied vein, molten metals often cool into dendrites that naturally suggest that their phase interfaces might be profitably modeled as fractal curves. But depositing suitable mechanical measures on the latter can prove a subtle business.

With respect to the limitations that classical PDE's display with respect to altering boundary topologies, the possibility that the "weak" parts (= regions possessing fewer classical derivatives than normal) of "weak solutions" to generalized PDEs might serve as internal signals that some fissure has appeared within the affected region have been explored. Such modeling techniques might alleviate fracture mechanics' need to employ "monitored modelings" in its endeavors.

In considering these extensions, one should not fancy that such innovative proposals can be profitably pursued on an utterly ad hoc basis. On the contrary, the inductively established lessons of "family resemblance" classical mechanics with respect to energy balance, work capacity and so forth place strong limitations on how our new interfaces, "weak solutions" and "configurational forces" can behave. Great mathematical skill is often required to weld old and new elements into a coherent working package.
**Conclusion.** In sum, if we go searching for the "foundational core" of classical physics practice in Hilbert’s manner, we are likely to feel as if we’ve become trapped in a novel by Kafka, with particular branches of a vast bureaucracy claiming greater authorities than they truly possess and, when challenged, shunting us off to other departments that assist us no further in our quest. And the most maddening aspect of these unsettled convolutions is that the resulting interconnections appear, when evaluated from the perspective of brute pragmatics, as exceptionally well plotted in their organizational architecture, for the intricate interwebbing we call “classical mechanics” comprises as effective a grouping of descriptive tools as man has yet assembled, at least for the purposes of managing the macroscopic aspects of the universe before us with well-tuned efficiency.

In the final analysis, our lengthy investigations provide us with a richer understanding of why “family resemblance” structures often possess great pragmatic utility. The crucial point to observe is that the frequent “lifts” that populate the pages of college textbooks do not function as the “derivations” their authors suppose them to be, but instead provide “task C” guidelines for how difficult modeling problems can be evaded through the exploitation of data (e.g., rigidity or principal directions of thrust propagation) extracted from observation along a mixture of scale lengths.127 So while we have been critical of such textbook “lifts” when evaluated from a Hilbertian point of view, these same passages perform a crucial pedagogical purpose in directing a modeler’s efforts to locally effective results. In the final analysis, it is the astounding success of these well-tuned models with respect to the macroscopic world that insure that “classical physics,” as an important intellectual activity, will probably remain with us forever. So while it is important to recognize, from a methodological point of view, that the routes whereby standard textbook prose stitches the fabric of “classical mechanics” into a well-engineered facade rarely comprise “derivations” in a proper sense, the good offices they perform for us should not be devalued in rendering that judgement. I trust that many readers had the uneasy sense, when we criticized worthy textbooks earlier for failing to satisfy Hilbertian standards of rigor, that somehow our target authors were “doing the right thing” in their presentations regardless. Yes, but such passages serve a different organizational purpose than we have been led to expect.

Although we cannot properly explore the possibilities here, deeper answers are still wanted as to why the characteristic ingredients of “classical mechanics” bind together into a facade as effectively as they do. Although Wittgensteinians
sometimes claim otherwise, our remarkable capacities to sort human faces into "family" groups wants explaining: the brain must perform some rough form of statistical analysis over facial features when it computes its groupings, although the psychological mechanisms involved don’t appear to be well understood at present. Just so: the strong feelings of "family resemblance" with which every student of classical physics is familiar merit probing in the same vein. The Victorian physicist P.G. Tait invokes the phenomenon well:

[All who have even a slight acquaintance with the subject know that the laws of motion, and the law of gravitation, contain absolutely all of Physical Astronomy, in the sense in which that term is commonly employed:--viz., the investigation of the motions and mutual perturbations of a number of masses (usually treated as mere points, or at least as rigid bodies) forming any system whatever of sun, planets, and satellites. But, as soon as physical science points out that we must take account of the plasticity and elasticity of each mass of such a system, the amount of liquid on its surface,... [etc.], the simplicity of the data of the mathematical problem is gone; and physical astronomy, except in its grander outlines, becomes as much confused as any other branch of science.]^{128}

Here Tait expresses his conviction that point mass physics best encapsulates the elusive "central core" to classical mechanics, although he realizes that this "core" must be dressed within the confusing garments of flexible bodies before reliable empirical results can be obtained. But what is the true nature of this "central core"? I believe that any reasonable answer must come from a deeper understanding of how our classical descriptive tools sit on top of quantum mechanics: the ways in which we usefully track macroscopic "work" and "energy" at the $\Delta L^*$ level must somehow trace to the $\Delta L$-importance of correspondent notions within the quantum domain.

There has been interesting mathematical work of late on issues such as this, although they sometimes suggest that Tait was wrong to locate his presumptive "core" within point mass mechanics. Although this essay granted analytical mechanics of a holonomic stripe little "foundational" credit in our survey due to its patently incomplete ontological coverage, it remains a patent fact that the subject comprises the central focus of modern day physicists (as opposed to engineers). Why? I don’t believe that the answer provided by Herbert Goldstein in the quotation above--that the ODEs of classical practice provide the right preliminaries for the PDEs of quantum mechanics--is fully adequate. Barry Simon articulates the central reason nicely:

[I]t seems to me that there has been in the literature entirely too much emphasis on quantization (i.e., general methods of obtaining quantum mechanics from classical methods) as opposed to the converse problem of the
classical limit of quantum mechanics. This is unfortunate since the latter is an important question for various areas of modern physics while the former is, in my opinion, a chimera. Indeed, Simon and a large number of other mathematicians have worked on an interesting answer to his “converse problem.” In the sorts of “limit” Simon has in mind, we hope to situate a classical-looking phase space over an underlying quantum configuration space. The basic problem is that, although the latter possesses a natural cotangent bundle of its own, this construction won’t look much like the classical “phase space” we hope to see (i.e., it lacks the position/momentum pairings characteristic of such constructions within classical analytical mechanics). So Simon and cohort employ a clever trick. They first decompose the quantum wave function into what is called a “coherent phase” representation, which holds the uncertainties in position and momentum roughly in balance. This representation looks rather like a blurry portrait of some unknown object. So they then apply “focusing techniques” akin to the ones that computers employ to digitally filter away the “noise” in an old photograph. Blurry areas convert into sharp points that now appear to move around in a new space hemmed in by exactly the position/momentum pairings familiar from classical analytical mechanics. Under such a story, it emerges that, among all of the classical “ontologies” surveyed in this article, Nature prizes analytical mechanics foremost amongst her many “classical” children, simply because that discipline best emphasizes the mathematical structures that appear on a macroscopic level when we “clean up” the fuzzy photographs she supplies on the quantum scale.

Such a diagnosis, of course, only explains the salience of “classical mechanics” tools within close-to-quantum applications (as arise in molecular modeling circumstances); not the descriptive successes that “classical mechanics” achieves at much higher scale levels (e.g., in “virtual work” analyses of machinery, where considerations of transmitted “work capacity” become central). Plainly, any family resemblance “core” that emerges across this patchwork cannot be expected to display the sorts of “internal conceptual closure” that we anticipated when we initially went scouting for Hilbertian “foundations.”

However such speculative issues resolve themselves, “classical mechanics, as studied here, offers many valuable lessons to philosophy as a whole: in particular, that well-wrought conceptual structures can be assembled as “facades” tied together through “look across size scales” linkages. But to praise a “family resemblance” fabric in this manner is not to deny that its organizational patterns can be accorded rational underpinnings. On the contrary, we should scrutinize “lifts” and “escape hatches” within a facade with formal care so that their operative strategies of “physics avoidance” become accurately identified and their empirical outreach
accordingly improved. As a prerequisite to those diagnostic endeavors, we must first recognize that the "derivations" provided in elementary textbooks rarely satisfy Hilbertian demands on "rigor" but instead fulfill the "look across scale sizes" offices that allow the basic terminology of "classical mechanics" to cover wide swatches of macroscopic experience with an admirable efficiency. In these respects, current work in continuum mechanics provides an excellent paragon of how a useful base scheme can be profitably extended to wider applications once its conceptual supports have become viewed without methodological illusion.

As it happens, I firmly believe that the profitable words of everyday, non-scientific usage -- "red," "hard," "solid" -- gain their own descriptive efficiencies through interwoven arrangements not unlike those investigated here. But such topics would take us far afield.
Endnotes:


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

3. Due to the notorious “back action” between charged masses and fields, it is difficult to persuade the two items to work together in coherent harmony (*cf.* Stephen Parrott, *Relativistic Electrodynamics and Differential Geometry* (New York: Springer, 1986)). It is worth recalling that, in its original contours, Maxwellian electromagnetism conceptualized charges and current in a quite different fashion that better suited the tenets of orthodox continuum mechanics (*cf.* Bruce Hunt, *The Maxwellians* (Ithaca: Cornell University Press, 2005)). Tracing these conceptual issues would carry us along a different trajectory than we shall follow here.

   In an allied manner, the “physical ontologies” favored in the early classical period often involved flexible bodies penetrated by various forms of “imponderable fluid.” Making clear sense of such “imponderability” is tricky and requires thinking about “mass” in a different way than we shall presume. Such largely historical issues will not be further discussed here, although they are vital to many controversies arising in the early years of classical tradition.

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

5. Modern investigations have shown that true ODEs and PDEs are usually the resultants of foundational principles that require more sophisticated mathematical constructions for their proper expression (integro-differential equations; variational principles, weak solutions, etc.). We shall briefly survey some of the reasons for these complications when we discuss continua in (vi) (although such concerns can even affect point mass mechanics as well). For the most part, the simple rule “ODEs = point masses or rigid bodies; PDEs = continua” remains a valuable guide to basic mathematical character.
6. Often internal variables such as “spin” are tolerated in these ODEs, even though they lack clear counterparts within true classical tradition.

7. Horace Lamb, **Dynamics** (Cambridge: Cambridge University Press, 1923), pp. 345-6

8. We look forward to Michael Friedman’s big book on these issues.

9. More exactly, there were a number of logical empiricist attempts to axiomatize a simple point mass approach (e.g., P. Suppes, J. C. C. McKinsey and A. C. Sugar, “Axiomatic Foundations of Classical Particle Mechanics,” *Journal of Rational Mechanics and Analysis*, 2 (1953)). Such essays were invariably composed without an adequate appreciation of the vital problems of the subject. For an excoriating yet fair review of such literature, see “Suppsian Stews” in C. Truesdell, **An Idiot’s Fugitive Essays in Science** (Berlin: Springer-Verlag, 1984).


13. Alternatively, the phrase may connote a sophisticated interest in the geometrical objects implicit within analytic mechanics (vide R. Abrahams and J. Marsden’s **Foundations of Mechanics** (Redwood City: Addison-Wesley, 1987) or V.I. Arnold, **Mathematical Methods of Classical Mechanics** (New York: Springer, 1987)). But this sense of “foundations” will not prove pertinent here. In these respects, a brief methodological comment may be in order. I have run into a large number of young philosophers who have learned the “calculus on manifolds” techniques taught in these books and, because the learning curve in doing so is fairly steep, fancy themselves complete masters of “everything about classical mechanics.” But this is scarcely so (and none of the authors of those great books would have pretended otherwise). The great accumulation of lore that we loosely call “classical mechanics” remains of vital philosophical interest as both a living descriptive practice (especially within engineering) and a subject deeply enmeshed within our philosophical ancestry. It is my hope that the considerations surveyed here will provide readers with a better sense of where the basic
controversies of the subject originate.


16. For clarity’s sake, we shall not much discuss the “mixed ontologies” one can assemble from the three basic types of mechanical elements introduced in section (i), although such conglomerates play important roles in actual modeling practice. Such admixtures are generally resistant to rigorous treatment.


18. I do not have the space to survey such modern studies here, which attempt to, e.g., recover the tenets of rigid body mechanics from continuum principles by allowing certain material parameters to become infinitely stiff (thus “degeneration”). Generally the results are quite complex, with corrective modeling factors emerging in the manner of Prandtl’s boundary layer equations. Sometimes efforts are made to weld our different foundational approaches into unity through employing tools like Stieltjes-Lesbeque integration.

    More generally, a “homogenization” recipe *smears out* the detailed processes occurring a wide region $\Delta W$ in an “averaging” kind of way, whereas “degeneration” instead *concentrates* the processes within $\Delta W$ onto a spatially singular support like a surface (the Riemann-Hugoniot approach to shock waves provides a classic exemplar).

19. After a sufficient range of mechanical considerations has been surveyed in later sections, we shall be able to sketch a more favorable view of the useful offices that standard textbook “lifts” provide. I should also add that we shall generally consider our “$\Delta L$ to $\Delta L^*$ lifts” in two simultaneous modes: (1) as a modeling shift from one *finite scale length* to another (e.g., from $\Delta L^g$ to $\Delta L^o$ in our steel bar example) and (2) as a mathematical shift from a *lower dimensional object* (a point mass or line) to a higher dimensional gizmo such as a three-dimensional blob. Properly speaking, these represent distinct projects, although, in historical and applicational practice, they blur together.

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


22. But not always: later in the paper we shall discuss the Riemann/Hugoniot trick of tolerating degenerative singularities along the shock front locations.


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


26. This is historically a bit too simplistic, because scientists in Hilbert's time often presumed that mathematical treatments of nature could only resolve its details up to a choice of scale length $\Delta L$. If so, incompatible forms of "mechanics" might fulfill these "resolve to a scale length" obligations equally well. Such "scale length" assumptions lie at the root of familiar "underdetermination of theory" claims that originally became popular in this period.

It should also be observed that no immediate lessons with respect to Hilbert-style axiomatics for quantum theory should be extrapolated from our discussion, for our conclusions are intimately entangled with the organizational features desirable in a system of "reduced variables" that operates at comparatively long scale lengths.

27. I am particularly struck by recent work on "geometrical phases." For a brief discussion, see Mark Wilson, "Of Whales and Pendulums: A Reply to Brandom" in Philosophy and Phenomenological Research 82, 1 (2011).

29. Ludwig Wittgenstein, *Philosophical Investigations* (New York: John Wiley and Sons, 2009), §67. As a curiosity, it was with Horace Lamb that Wittgenstein studied when he enrolled in aeronautics at Manchester.


33. Our modern renderings of “virtual work” principles are due to John Bernoulli and Lagrange, but allied ideas underwrite ancient thinking. Historically, it was common to write of “loads” rather than “forces” and the subtle considerations that mandate the “virtual” qualifier were clearly articulated by Descartes (see Pierre Duhem, *The Origins of Statics* (New York: Springer, 1991).


39. Like many early authors, Thomson and Tait employ “material point” in a manner that confuses isolated point masses with the “continuum physics infinitesimals” discussed in section (vi).


44. In many circumstances, it is natural to borrow Coulomb's law from Maxwellian electrodynamics, but, strictly speaking, this rule only suits static circumstances. Accommodating dynamic circumstances within a "classical physics" frame, we must normally introduce a foreign element (the electromagnetic field) which carries us beyond the limits of our point mass framework. Indeed, no one has yet figured out a wholly satisfactory way to amalgamate classical point masses with such a field.

45. Sometimes this phrase is tacitly restricted by further requirements on the locations \( q(i,t) \): it seems strange to say that we have supplied a "constitutive modeling" for a cuckoo clock if we are willing to consider that "modeling" in a condition where its component masses are scattered across the wide universe!

46. From a syntactic point of view, an adequately completed "Euler's recipe" modeling should provide us with a pure set of coupled ODEs that are "formally closed" with respect to the dependent variables they contain. The most common "constraints" sully this purity with various *algebraic* relationships.


48. For a more detailed discussion of these issues, see Mark Wilson, "Determinism: The Mystery of the Missing Physics," *British Journal for the Philosophy of Science* 2008. I might add that the common technique of *dropping dimensions* (e.g., confining point masses to a plane with no specification of the forces that keep them there) should be considered as a further variety of "Euler's recipe avoiding" policy (such moves should be scrutinized with a close methodological eye whenever they are invoked).

49. That is not to say that there isn't a lot of very important work now going on the behavior of composites that better combine the virtues of both approaches.


52. These difficulties are closely related to those discussed in section (iii) in relation the quotation from Donald Greenwood.

53. If one retains an interest in how the rapid movements of a and b will produce long term shifts in the positions of A, B and C, our “frozen potential” answer can provide the first stage in an improving “perturbative” calculation that eventually takes into consideration the actual interactions between the large and small bodies. Such a division of computational labor is typical of a huge class of “multi-scale” techniques where the movements of a complicated system are factored into different “epochs” which serve as the preliminary ingredients in a fruitful attack on the system’s otherwise intractable complexities.

54. A common projection of this ilk considers the force that an object a would feel in the presence of other masses A, B, C, ... under the assumption that a exerts no reciprocal influence upon A, B, C,... (this construction is often dubbed “*the other field*” of a). Such “other fields” will exist for every particle within a system but they cannot be integrated together to form a single “gravitational field” of the sort conventionally expected. Hartry Field’s *Science without Numbers* (Princeton: Princeton University Press, 1980) provides a paradigmatic illustration of such a confusion of “fields” within a philosophical context.

55. To be sure, earlier writers such as Leibniz maintained that some fundamental quantity akin to “energy” was conserved through all interactions, but they generally presumed that any “energy” lost to friction must show up on smaller scale lengths as “moving energy” (*vis viva*). The notion that energy could be stored in a primitive manner as a “potential to perform work” belongs largely to the nineteenth century. Perhaps the novel aspects of this “work capacity storage” idea conception can be strikingly underscored by the consideration that many nineteenth century religious thinkers hoped that a solution to the mind/body problem had finally come in view: when we think, we are merely juggling non-embodied “energy” as it resides in its “potential” form.

57. This ploy is sometimes utilized in order to geometrize the notion of "material frame indifference." Cf. Mark Wilson, "There's a Hole and a Bucket, Dear Leibniz," Midwest Studies in Philosophy 18, 1 (1993).

58. Cf. the entries "Constitution of Bodies," "Atom" and "Attraction" in J.C. Maxwell, Collected Scientific Papers, Ivan Niven, ed. (New York: Dover, 1952). Maxwell also worried that point mass swarms could not remain structurally stable when vigorously shaken nor explain the fact that the world's wide variety of materials only display a very limited palette of spectra. Reint de Boer, Theory of Porous Media (New York: Springer, 2000) provides a good capsule summary of these developments.


60. If no kinetic energy is lost to heat (a so-called "purely elastic collision"), then we possess enough "conservation laws" (energy and linear momentum) to guide two colliding point masses uniquely through a collision (as every elementary college text demonstrates). But these principles alone aren't adequate to three-way collisions, energetic losses or to more oblique modes of scattering.

61. Specifically, many textbooks cite similar integration by parts computations as "proofs" that Lagrange's principle qualifies as a consequence of weaker, non-variational postulates. In truth, such "proofs" function validly only in the reversed direction: to show that the non-variational posits follow from Lagrange's principle. For an excellent discussion of such methodological misapprehensions, see the Antman article cited previously.

Incidently, I believe that curing the collision singularities of point mass physics through "weak solutions" and all that represents the methodological equivalent of dispatching house flies with a bazooka. But such repair methods become a more serious concern when we need to deal with shock waves and other phenomena that arise within a continuum mechanics context. I'm employing our point mass problem as a simple setting in which the basic strategy behind the "weak solution" technique can be readily explained.

Another strategy for achieving impactful tolerance utilizes integral principles rather than weak solutions (the two policies are intimately connected). We shall later resolve the shock wave problems of continuum mechanics by allied means.
62. The writings of Gallavotti and Papastravridis, *op cit.*, are particularly lucid on these points. We'll later see that many of statements labeled by physicists as "theorems" truly serve as indicators of profitable pathways for conceptual prolongation.

63. Indeed, kudzu was introduced in the American South in order to bind together the loose top soils of its roadside embankments.

64. Constraint relationships are sometimes maintained through factors external to the device (such the pressures of an ambient fluid or the gravitational attraction that binds a cam to its follower), in which case the device is said to be *force closed*. Descartes, for example, essentially dissected the universe into component mechanisms, but they were usually held together through force-closure rather than internal pinning.

65. Such contacts are further classified as "higher or lower pairs" according the contacting geometry they implement.

66. These specific supplements do not alter the required mathematical; setting much. In the next section, when we survey traditional methods for installing little mechanisms inside the "infinitesimals" of continuum physics, we will often find that allied flexible elements are often employed in that guise as well. The hope is that we will be able to adequately the huge range of flexible behaviors that we encounter upon a macroscopic scale using a very small collection of stereotyped elements such as springs on the infinitesimal level. This program is an *illusion* generated by the wide applicability of "finite element" methodology, as we shall briefly discuss later.


68. Indeed, it is not evident to which body the contact point "belongs" (one needs to beware of making simplistic assumptions about "how points belong to bodies" in such circumstances).

69. It is common to designate the external closure of a body $B$ with the notation "$\delta B$."

70. The "theory of measure" is a subtle subject and prone to oddities, such as the celebrated Tarski-Banach "paradox." It might be noted that Hilbert's Sixth Problem (which also concerns probabilities) played a vital role in prompting the
necessary mathematical developments.

71. On the other hand, the fact that flexible bodies distort when accelerated according to exactly the same rules as when they are pulled upon by a comparable array of "real" forces is an important behavioral symmetry (often called *material frame indifference*) that motivates lumping the two quantities into a common class.

72. Contexts occasionally arise where one wants to allow finite values of, e.g., mass to sit upon lower dimensional structures such as points or surfaces. To formulate these tolerances properly, one must resort to Stieltjes-Lebegues integration and allied devices. Efforts to capture point mass mechanics in a common framework with continuum mechanics sometimes appeal to these expedients, which will not be further discussed here.

73. In this context, "Euler's First Law" is often viewed as simply "Newton's Second Law" in application to rigid bodies. Credit for regarding the \( \mathbf{F} = \mathbf{ma} \) scheme as a framework upon which "recipes" for differential equations for both forms of mechanics can be built is historically due to Euler, not Newton. As we shall see, the analogous recipe for continua relies upon a formula traditionally called "Cauchy's Law" which many writers regard as yet "another version of \( \mathbf{F} = \mathbf{ma} \)" (although it actually employs the tricky notion of stress which Cauchy originated). The similarities of these three "recipe" formulas supports the strong "family resemblance" character of "classical mechanics." Terminological issues become more confusing within the context of continua, in which analogs of Euler's two laws are also applied to the *sub-bodies* in the interior of container blobs. In such contexts, these analogs are often dubbed the "balance principles" for momentum and angular momentum. In the context of rigid bodies, once specific values for moments of inertia *et al.* have been computed with respect to such entities, these values remain the same, allowing the import of Euler's principles to be expressed as equations of ODE type. Within flexible bodies, in contrast such values fluctuate as they flex and so PDEs are required to capture the requisite relationships.

74. Although I have quoted Lagrange's principle in its standard textbook form, it conceals a subtle ambiguity, specifically as to whether the "\( \mathbf{r} \)" cited is a true position coordinate or rather represents something "generalized" like an angle. If the latter (which is usually what's needed), then the corresponding "mass" terms "\( \mathbf{m} \)" must be read as moments of inertia *et al.* Presumably, we require some instruction in how these "generalized inertial terms" are to be found. Such unnoticed shifts are often sites of significant "lifts" (and sometimes outright errors,
which are common in this branch of mechanics).

The restriction to “virtual variations” is necessary because the mechanical advantages of most mechanisms continuously adjust as they move through their cycles. This means that input forces \( F_1, F_2, F_3 \) on our crane will not be able to balance quite the same output force \( F_4 \) when the machine stands in a different configuration. But the “instantaneous work” performed by the input forces will always equal the “instantaneous work” expended at the outputs, which is the key idea that we need to capture in our “virtual work” formula for static situations.

75. Papastavridis, op cit, p. 387, notes that d’Alembert himself probably had a less far-reaching principle in mind. Historically, there has been much confusion over the exact contents of the popular tenets of “analytical mechanics.” S.K. Soltakhanov, M.P. Yushkov and S.A. Zegzhada, Mechanics of Non-Holonomic Systems, pp. xxi-xxxii (Berlin: Springer Verlag, 2009) provides a capsule history of these struggles.

76. Nomenclature becomes very confusing here, due to the fact that Lagrange and Hamilton contributed so much to this general corner of mechanics.

77. Donald T. Greenwood, op cit, pp. 16-8. I do not intend these remarks to be as critical as they may presently seem. Eventually, we come to see Greenwood’s “proofs” as functioning, not as derivations proper, but as “Task C” indicators of profitable ways to avoid \( \Delta L \) constitutive assumptions through the exploitation of knowledge of a material’s \( \Delta L \)-behaviors (specifically, its apparent “rigidities”).

78. Real balls must store energy as stresses momentarily while compression waves move across their diameters, although there is no way to register this obvious fact as long as we pretend that our balls can be modeled as point masses carrying repulsive hard shell potentials. Likewise does our scheme supply any method for capturing the heat loss that pertains in circumstances like these.

79. One might proceed in the manner of the geometries that treat lines, points and surfaces as coequal “primitive” objects. However, attempting to tolerate point masses alongside three-dimensional rigid bodies would entangle us in various tricky considerations of measure that we can skirt if we retain only linked rigid bodies as our “primitives,” in the customary manner of most presentations of the subject. In practice, of course, “mixed ontology” modelings (vide a point mass sliding on a plane) are quite common, but rigorously justifying such “dimensional drops” is invariably fraught with difficulties (cf. Diarmuid Ó Mathúna, Mechanics, Boundary Layers, and Function Spaces (Boston: Birkhäuser, 1989)). Such “drops”
frequently disguise serious "foundational" gaps, as we shall observe later in connection with one-dimensional "stress." With respect to present concerns, the crucial observation is that, once $\Delta L^*$-scale bodies become accepted as "primitives," then a governing formalism no longer needs to supply internal "constitutive modelings" to explain their behaviors.


In his Rational Kinematics (New York, Springer-Verlag, 1988), pp. 1-2, Jorge Angeles comments:

A popular practice that has gained many adherents in the last decades, in connection with classical dynamics, consists of first deriving the dynamical equations concerning the motion of a system of particles. From these, the equations governing the motion of rigid bodies are derived by regarding the rigid body as an aggregate of particles. Since the derivation of the Lagrangian dynamical equations is rather simple---no rotation is involved---, those of rigid bodies are usually derived by sheer summation of forces over a set of infinitely many particles... Kirchhoff appears to be the first one to introduce this approach, which is by no means free of flaws. The popularity of this approach is surprising, for the founding fathers of classical dynamics... treated rigid bodies as continua, and not as aggregates of an infinite number of particles.

In truth, due to the intrusion of "essential idealization" and other doctrines with respect to infinitesimals, it is often hard to determine how a classical author is reasoning (Poisson, for example, sometimes seems to proceed in the manner proscribed). But Angeles is right that the logical underpinnings of the procedure are invariably left obscure. Papastravridis, op cit., includes a good discussion of these matters on p. 390 (his thinking is influenced by the foundational work within continuum mechanics by Noll and others, just as my own remarks are).

For reasons I don't fully understand, Angeles also finds approaches based upon virtual variations (such as "Lagrange's principle") obscure and prefers to set up rigid body mechanics through specializing upon continuum physics in the manner sketched in section (v). This procedure raises delicate questions of whether the imposition of rigidity is strictly compatible with the constitutive assumptions tolerated within that framework. This tension with strict constraints is not as blatantly problematic as it is within a mass point setup, but still seems to require, at a minimum, an extension of basic principles to account for the fact that usual stresses etc. become ill-defined or ambiguous in the face of superimposed requirements such as rigidity or incompressibility.
My own opinion, in harmony with that of Papastravridis, is that the variational approach is best suited to rigid bodies as such, as long as one takes that ontology seriously (i.e., we do not view rigid bodies as convenient approximations to other varieties of “classical object”).

81. I personally feel that analytical mechanics is more aptly regarded as an approximation scheme that exploits mixed level data deftly. But I’m trying to avoid dogmatism on such issues.

82. I remember being confused by such claims as a student—isn’t real work performed in pulling the bead down to the wire? Yes, but we’re not supposed to count it as such now.

83. In more complex settings, the usual technique for calculating “reaction forces” employs “Lagrange multipliers,” which calculate the “forces” required to make the “virtual work” performed in maintaining the constraints vanish. This merely represents a fancier scheme for implementing the same after-the-fact “force” calculations.

84. Henri Poincaré, Science and Hypothesis (New York: Dover, 1952), Ch. 5.


86. Alternative analogy for philosophers: discovering large “forces of reaction” is like noticing clues within a nightmare that indicate that we are merely dreaming and that we should wake up (= escape to another form of modeling).

87. Heinrich Hertz, The Principles of Mechanics, translated by D.E. Jones and J.T. Walley (New York: Dover, 1952). Hertz’ proposal greatly troubled the contemporary “energist” Pierre Duhem and may have served as an inspiration for his celebrated “underdetermination of theory” thesis. Specifically, Duhem favored a form of thermomechanics in which many non-mechanical forms of potential energy storage are tolerated (e.g., potentials that drive the formation of chemical compounds). But he also believed that every form of physical description is correct only with respect to a resolution to some implicit scale length ΔL, in the manner discussed previously. Hertz’ results then suggest that any behavior that Duhem can model with his potentials upon a scale length ΔL, Hertz can faithfully imitate without potential through positing suitable “hidden motions” at a lower scale. Cf. Pierre Duhem, The Evolution of Mechanics, J.M. Cole, trans. (Alphen, Sijthoff and Noordhoff, 1980), pp. 78-9 and Mark Wilson, “Duhem Before Breakfast,” PhilSci Arch (2007).
88. An influential argument of this type can be found in Bas van Fraassen, *The Scientific Image* (Oxford: Oxford University Press, 1980). Max Jammer’s *Concepts of Force* (New York: Dover, 2011) is a prime repository of erroneous misunderstandings of “force”’s scientific history. I might also mention that d’Alembert’s often cited rejection of forces as “metaphysically unnecessary” actually seems to represent a disinclination to employ the term except in cases where transfers of momentum through direct impactive contact occur. Cf. Thomas Hankins, *Jean d’Alembert* (Oxford: Oxford University Press, 1970), Ch. 7.

89. Better answers derived from Lagrange’s principles can appear counter-intuitive as well because, according to them, the coin merely rolls in peculiar cycles and never reaches the bottom of the slanted plane. However, because we’ve not included any friction in our modeling, it’s hard to augur “intuitively” what we should expect in such cases. As a curiosity, the “wrong answers” provided by “least action” computations are still important, because they plot the *pathways of minimal exertion* that the coin should follow if we are allowed to *control* its orientations (such computations are important in gauging the path a rocket should follow in reaching a target destination with the least expenditure of fuel). A large amount of recent research has been devoted to these subtle issues. Cf. Jerrold E. Marsden and Tudor S. Ratiu, *Introduction to Mechanics and Symmetry* (New York: Springer, 2010).


91. Goldstein nowhere mentions Lagrange’s principle as such (physicists are often unmindful of the hidden strength of “virtual work” principles), although he later traces some of the key steps one would employ to reach manageable formula for rolling coins from such a perspective. It is unclear whether he incorrectly presumes that Lagrange’s principle somehow “follows” from the point mass foundations he lays down. Certainly primers similar to Goldstein’s often “prove” Lagrange’s principle by multiplying by the variations, integrating and finally employing integration by parts to remove the variations. But one can’t legitimately *strengthen* principles through arguments such as these.

92. Both circumstances are labeled *structures* in the engineering literature, but I’ve avoided this technical usage here, as we often need the word for more general, well, structures. But the official subject of “structural mechanics” concerns itself only with systems that display no internal mobility from a rigid body point of view.

94. Thus "Newtonian mechanics" is aptly characterized as neither "clockwork" nor "billiard ball physics." I suspect that "inversion of the mechanism" intuitions play a substantive role in making "relationalism about space" appear obligatory to thinkers such as Descartes.

95. The expectation that continuous materials support waves is closely allied to requirement that the most basic equations of a physical subject should be *hyperbolic* in character. We'll later see that a certain circumspection is required with respect to these arrays of traction forces in the face of shock waves and allied phenomena.

96. Cf. Clifford Truesdell, "The Creation and Unfolding of the Concept of Stress" in *Essays in the History of Mechanics* (Berlin: Springer-Verlag, 1968). One needs to be wary of framing ones conception of these notions from one-dimensional continua such as strings or lamina, for in such reduced contexts "stress" does appear like a simple force density. In the main text, I'm trying to bring forth the funny kind of three-dimensional structuring that is inherent in the notion of a "tensor."

97. This proviso is enforced within a PDE modeling through the Saint-Venant compatibility equations.

98. One can witness some of this struggle in Kant's *Metaphysical Foundations of Natural Science* (Cambridge: Cambridge University Press, 2004) where he is plainly aware that some source of sheer is needed to make sense of conventional "solidity," but can't find a way to incorporate such a quantity into his descriptive framework.


to connote any scheme for halting our regress through “shrunked boxes,” although the alternatives often embrace blobs controlled by a finite number of “lumped” sites along their perimeters or utilize stereotyped continua such as springs along with their rods and cams. The fact that such tactics were so universally regarded as necessary before the twentieth century explains an otherwise puzzling feature of the old debates over “underdetermination.” Specifically, in The Evolution of Mechanics, Duhem claims that “experiment can never decide” between his thermomechanical approach to foundations and that of a stricter mechanist such as Hertz (cf. My earlier comments on this dispute). But the formalisms that he and Hertz individually favor only tolerate a finite number of degrees of freedom, whereas flexible bodies are inherently infinite dimensional in their behaviors. But Duhem clearly believed that nature was composed of continua (and I believe that Hertz did as well). The fact that both parties presumed that “physical infinitesimals” needed to descend from shrunked finite systems explains why the old debates were conducted in terms of these reduced frameworks, rather than with respect to continuum mechanics proper.

101. Given the sometimes wobbly nature of “force”’s significance, one can appreciate why such indirect checks might seem important.


104. Philosophers new to the peculiar world of continuum physics parlance should prepare themselves for phraseology such as “dimensionless point cube” (J.D. Reddy, An Introduction to Continuum Mechanics (Cambridge: Cambridge University Press, 2008), p. 126--an excellent book, by the way).

105. A.N. Whitehead did some foundational work in mechanics at the turn of the twentieth century and his “method of extensive abstraction” was later popularized

*A subtle point:* when we combine our stress and strain information, should our resultant vectors situate themselves on the reference or the response planes? This matter becomes important in non-linear elasticity and requires the careful delineation of different "strain tensors" that one finds in modern textbooks.

106. \( \mathbf{g} \); it will be recalled, captures the summed body forces acting upon \( \mathbf{q} \). In following this standard representation, we are tacitly ignoring the third law demands that persuaded us to distinguish \( V(\mathbf{q}) \) from \( V^*(\mathbf{q}^n) \) earlier (the mathematics of continua is rough enough without fussing about that!). It is important to realize that the accelerative term behaves mathematically very much like \( \mathbf{g} \) and is often called an "inertial force" as a result (some of the third law ambiguities surveyed earlier trace to this drift in the significance of "force"). And an important symmetry with respect to constitutive equations is relevant as well: materials (usually) respond to an applied schedule of accelerations by exactly the same rules as they react to an comparable array of genuine forces (this requirement is called "material frame indifference" or "objectivity").

107. Hilbert played a central role in the development of integral equations and it is likely that some of his interest in mechanics traced to the expectation that such equations might find fruitful application there.

108. The pictured alignment of Hooke's original law with stress tensors that only vary along a one-dimensional line encourages the hazy perception that stresses and pressures are "just a kind of force."

109. Observe, however, that these formulas are often supplemented with a constraint of compressibility, a convenient appendage that usually needs to be regarded as a Task C excrecence from a foundational point of view (recall that true pressure (= stress) becomes undefined in such circumstances).

110. To be sure, there are certain further limitations that can be reasonably imposed upon materials in general (e.g., the "material frame indifference"
mentioned earlier) but they tend to be relatively feeble in scope. And we need “compatibility equations” to insure that our local strains and stresses assemble into finite forces and displacements.

111. For a vivid illustration of the divergence between traditional methods and the approved “modern” approach, see Stuart S. Antman, “The Equations for the Large Vibration of Strings,” American Mathematical Monthly 87 (1980). Drops in dimension through appeal to “symmetries” usually act in the manner of constraints.

112. As we’ve seen, traditional modelers commonly appealed to little mechanisms as a means of introducing Task C simplifications into their modelings, so that analytical mechanics serves as a convenient house of refuge for continuum mechanics as well.


114. As a case in point, a key document within the rise of “anti-realism” is Karl Pearson’s once influential The Grammar of Science (London: Thoemmes Continuum, 1992), which is very explicit in its continuum mechanics roots, commingled with a variety of neo-Kantian themes.

115. To be more precise, an analysis of how the relevant equations operate indicates that densities must travel through the gas along so-called “characteristic curves.” As shock waves form, these curves cross and place inconsistent demands upon the equation’s solutions. This is a somewhat different mechanism for inducing “blowup” that we witness in the Xia example. Serge Alinhac provides a valuable discussion of these issues in his Blowup for Nonlinear Hyperbolic Equations (Boston: Birkhäuser, 1995).

116. Readers familiar with numerical methods will recognize that I am describing the operations of a typical finite difference scheme here. Such representations often give one a good sense of the potential difficulties that PDEs face.

117. Such “jump” information can be extracted from our Task A laws because they have been formulated as integral equations (i.e., pertaining to volumes \( S \) rather than points \( q \)). In shrinking to the point \( q \) level, one engages in calculations very similar to the manner in which one clears the \( \delta \) symbols from a variational principle. If the process is not blocked, the ingredients needed for Cauchy’s law
will be deposited upon \( q \); if not, one is left with jump conditions on \( q \)'s two sides (relative to an interface \( C \)) that insure that basic quantities like mass and momentum will be preserved in crossing \( C \), even though the tensorial qualities that usually transport these quantities become temporarily meaningless at points along \( C \).

The role that integral equations play relate to a substantive mathematical difficulty that hindered many of the early attempts to develop classical mechanics. Thinkers like Descartes and Leibniz largely proposed conservation principles of a first-order character, but Nature herself prefers stronger connective "laws" of a second-order (acceleration-based) stripe. Usually the added strength of integral or variational considerations is required to help bridge this critical gap.

But even here one must be careful. Appeal to thermodynamics principles provides valuable supplementary instruction as to how the surroundings of a shock curve \( C \) should be tiled, but will it be enough? The answer may depend upon the dimension of the problem and the number of variables involved (it's not yet known whether our "entropy conditions" can resolve our problems completely in 3-D, for example). Analogous difficulties affect Newton's approach to billiards, for the prescriptions he offers supply enough equations to only resolve the head-on encounters of two balls; they can't resolve a triple collision adequately at all. Readers should be advised that drops into lowered dimensions often have a tendency to hide foundational problems in this way. Many elementary texts leave the impression that they have provided their readers "with all the tools you'll ever need" for "billiard ball physics," an exaggerated claim that a glance at any advanced text on "contact mechanics" swiftly disproves. Indeed, as noted before, major ontological adjustments are usually made in those works, as the rigid balls and point masses of the elementary presentations become displaced by the flexible spheres of continuum physics.

118. Riemann actually proposed the wrong thermodynamic resolution, which was corrected by Rankine and Hugoniot. Appeal to thermodynamics principles provides valuable supplementary instruction as to how the surroundings of a shock curve \( C \) should be tiled, it is not clear that they will prove sufficient in all natural circumstances (it's not yet known whether our "entropy conditions" can resolve our shock problems completely in 3-D, for example). Analogous mathematical difficulties affect Newton's approach to billiards, for the prescriptions he offers supply enough equations to only resolve the head-on encounters of two balls; they can't resolve a triple collision adequately at all. Readers should be advised that drops into lowered dimensions often have a tendency to hide foundational gaps in
this manner. Many elementary texts leave the impression that they have provided their readers “with all the tools you’ll ever need” for “billiard ball physics,” an exaggerated claim that a glance at any advanced text on contact mechanics swiftly disproves. Indeed, as noted before, major ontological adjustments are usually made in the advanced works, as the rigid balls and point masses of the elementary presentations become displaced by the flexible spheres of continuum physics.

119. Cornelius Lanczos, Linear Differential Operators (New York: Van Nostrand, 1964), p. 505. In applications, typical “boundary conditions” differ greatly from typical “initial conditions,” which generally represent temporal slices through a material in a manner that leaves its modeling principles operational along the slice. But the space-like slices employed as boundary conditions commonly reflect regions where the interior principles no longer apply and some other strongly dominating process has taken over (at the nut of a vibrating string, these are the strongly binding contact forces between wood and wire). In fact, we generally align our “boundary conditions” with those special “opportunities in nature” where the locally governing physics can be adequately approximated by crude rules, in the manner that Lanczos highlights

120. The discussion in Richard E. Meyer, An Introduction to Mathematical Fluid Dynamics (New York: Dover, 2007) brought home the point to me.

121. With this passing mention of “indeterminism,” we should observe that there are many canonical branches of “classical physics” that approach their subjects in an intrinsically probabilistic fashion (of these, Norbert Wiener’s celebrated treatment of Brownian motion comprises the great paradigm). It is impossible to survey these accounts here, but they commonly provide another vital class of “escape hatches” to sustain the classical “facade.” In his articulation of his sixth problem, Hilbert clearly recognized the centrality of such constructions and his urgings played a significant role in the development of what were later called “stochastic differential equations.”


124. Microcontinuum Field Theories I (New York: Springer Verlag, 1999), pp. xi-xii and Reint de Boer, Theory of Porous Media (Berlin: Springer Verlag, 2000). In
fairness, it should be indicated that I have sketched the methodological contrasts in
the main text in a somewhat starker manner than they were in real life, for
Truesdell encouraged the development of micro-polar and allied extensions of his
central approach.

Thermodynamics (Berlin: Springer-Verlag, 1993) and Ingo Müller, A History of
Thermodynamics (Berlin: Springer, 2007), ch 8.

126. Although Duhem and Mach both worked with shock waves (the gas speed at
which they form is named after Mach), I’ve never found this consideration
explicitly employed as weapon against the “mechanists” in their writings. On the
other side, many of the latter favored a “vortex atom” approach to molecular blobs
which could resolve our regress through the special capacities of the aether in
which such vortices float.

127. In the applications considered here, only two characteristic scale lengths are
generally relevant, but Batterman’s essay in this volume surveys some of the
exciting recent work that promises a capacity to intermingle data extracted from a
wider array of scale sizes.


Sinai, Classical Nonintegrability, Quantum Chaos Basel: Birkhauser Verlag, 1997).
We should observe that such studies only rationalize the parts of the classical
“facade” that lie nearest to the points of quantum/classical crossover; they don’t
really explain why engineering mechanics needs to be as it is.

130. The claim that everyday classificatory words operate along organizational
principles similar to those surveyed here comprises the chief argumentative burden
of my Wandering Significance, op cit.