Abstract: Attempts to arrange all of classical mechanics upon a self-contained basis encounter difficulties due to “the lousy encyclopedia phenomenon”: hard cases involving, e.g., billiard balls, often require that the standard treatments be abandoned in favor of conceptually different accounts. Worse yet, these chains of interdependence often travel in circular loops, where the practitioner is returned to formalisms that she had previously abandoned. However, behaviors of this sort are to be expected if classical doctrine is instead viewed as a “reduced variable” covering of quantum mechanics, which is the point of view that this essay recommends.

A Funny Thing Happened on the Way to the Formalism[1]

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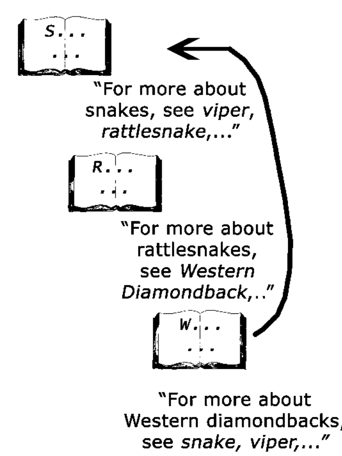
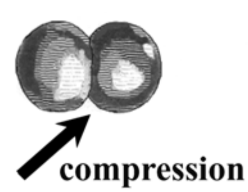
In reading a standard mechanics text, one often runs across an aggravating behavior that I call the *lousy encyclopedia phenomenon*, in honor of a regrettable "reference work" that my parents had been snookered into purchasing when I was a kid. I would eagerly open its glossy pages to some favorite subject ("snakes," say). The information there provided would prove utterly inadequate but hope always remained, for at the end of the article a long list of encouraging cross-references was appended: "for more information, see **rattlesnake**; **viper**; **reptile**, **oviparous** ..." etc. Tracking those down, I might glean a few pitiful scraps of information at best and encounter yet another cluster of beckoning citations. Oh, the hours I wasted chasing those informational teasers, never managing to learn much about snakes at all!



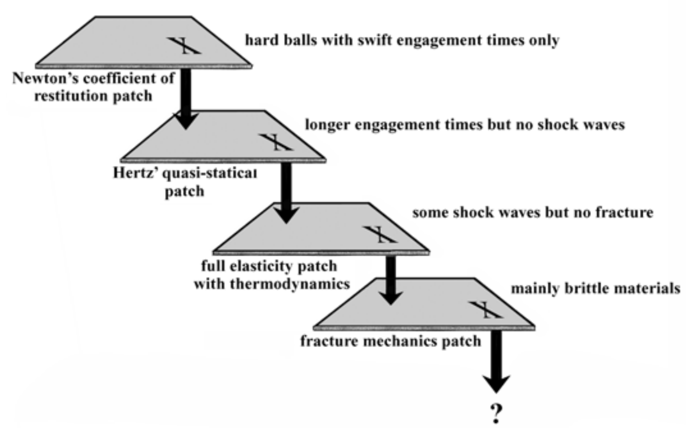
In a stock mechanics book, one will often read about a specific topic--let's say, billiard ball collisions--and realize that the treatment there outlined can't apply to *all* events of the expected type considered in fuller generality. Thus the text might appeal to an old treatment of Newton's wherein one treats the colliding balls as rigid throughout the collision and appeals to a so-called "coefficient of restitution" to govern how much of the incoming kinetic energy gets lost. And then one wonders, "Gee, this account isn't going to work if we happen to have three balls colliding at once. And don't real billiard balls sometimes *flex* when they collide?" Sure enough, you are likely to find a little footnote attached: "For more on this topic, see..." But when you look up one of those citations, you'll find some comment that implicitly overthrows the validity of the "coefficient of restrition" treatment you've just studied:



> *The initial approach* [historically] *to the laws of collisions was predicated on the behavior of objects as rigid bodies, with suitable correction factors accounting for energy losses. It is interesting to note that this concept has survived essentially unchanged to the present day and represents the only*

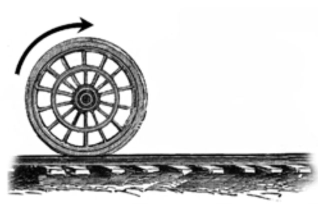
exposition of impact in most texts on dynamics.[2]

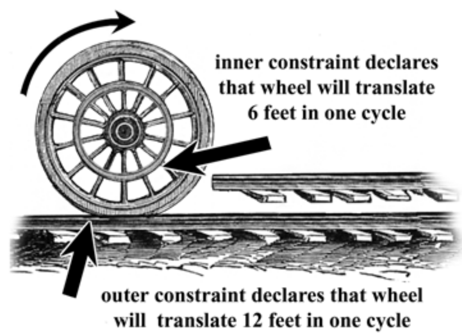
Often the treatments found in following the footnotes do not simply "add more details" to what we saw before in any reasonable sense of that phrase, but quite commonly overturn the old treatments altogether. In the case at hand, the entire mathematical setting gets replaced: specifically, the Newtonian treatment utilizes ordinary differential equations, whereas our specialist texts will usually employ partial differential equations of some class, which, from a mathematical point of view, represent an altogether different breed of critter and embody an ontology of flexible bodies, rather than the rigid balls we left behind. But if you scrutinize the new treatment, it is likely that you will notice some further holes that keep our new methods from being able to handle a generic collision adequately. For example, at a second stage of detail our balls will usually be treated according to a *quasi-statical* policy pioneered by Heinrich Hertz: the collision events are broken into stages that are assumed to relax into one another in a "finds a local equilibrium" manner. This method gives very nice results for an important range of cases, but there are plainly billiard ball events-- when wave movements initiate within the balls--that fall outside its range of application. When one drops into a patch better able to handle fast dynamic effects like this, one learns that internal shock waves often form inside our balls, which, from a mathematical point of view, means that our governing equations "blow up" and don't make classical sense anymore. Well, that eventuality drops us into yet another patch where our physics tolerates so-called "weak solutions" and we find, rather surprisingly, that we must evoke certain forms of thermo-mechanical principle to get our shock waves to move through the interiors of our balls properly. Likewise, high speed collisions at explosive velocities bring forward an entirely new range of untreated effects within our balls and certain cases will readily show that we have been heretofore treating the common boundary between our balls in an unrealistic fashion (permitted no sliding or cross-boundary transfer of wave motion). Nor have we introduced any mechanism that allows our balls to fracture. And so we keep going. Despite the popular stereotype of Newtonian mechanics as "billiard ball" mechanics. this chain of billiard ball descent never reaches bottom to the best of my knowledge: there



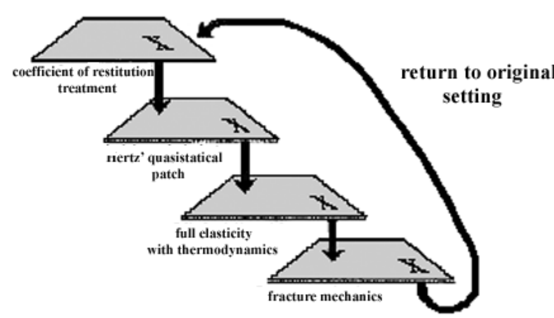
seems to be no universal theory of classical billiard behavior extant.

Here's another simple illustration of an important family of "lousy encyclopedia" exceptions that philosophers, at least, often overlook. Often they'll adopt some form of orthodox Lagrangian mechanics formalism as if it "fully embodies" the "content of classical mechanics," when, in fact, the formalism can only handle a wheel that *slides* along a rail, not one that *rolls* along it (that is, the formalism only accommodates holonomic constraints). Well, it isn't too hard to fix that hole, but now consider a wheel with *two* concentric hubs--"Aristotle's wheel" of antiquity--that eventually rolls into a configuration where the two hubs now lie upon two rails rather than one. When the inner hub rolls onto the upper shelf, the system shifts into *over-constraint* and we are forced to drop into a completely different arena of physical consideration to resolve the incompatible answers as to how far our wheel will roll in one revolution that our two constraints supply us. We are forced to open up a rather large suppressed can of worms pertaining to the *frictional processes* of sliding (and, usually, some bending) occurring at the two rail junctions (the computational advantages of a Lagrangian formalism lie in the fact that it allows to ignore such effects when they remain small). But a direct treatment of friction involves a lot of hidden physics that we haven't seen before (some of which carry us into a consideration of quantum processes).



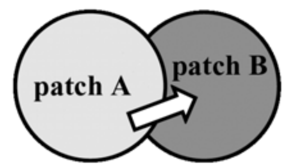


More generally, each drop in level in a lousy encyclopedia chain is apt to open up a lot of suppressed physics and this often forces considerable shifts in both mathematical setting and attended physical ontology (e.g., whether our basic entities are point particles, rigid bodies or flexible blobs). Worse yet, it often happens that some lower level in our chain will eventually return us to some apparently abandoned higher level, as, in fact, occurs when a detailed treatment of fracture asks us to once again treat portions of our billiard ball interior as a swarm of little rigid balls glued together by Newtonian attractions. I call this kind of circular behavior in our encyclopedia *foundational looping* and it

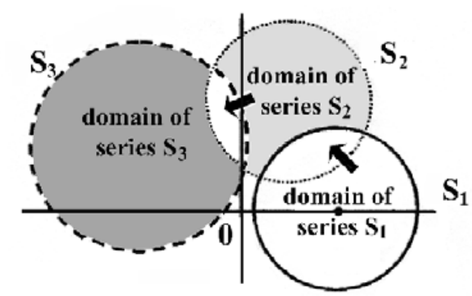


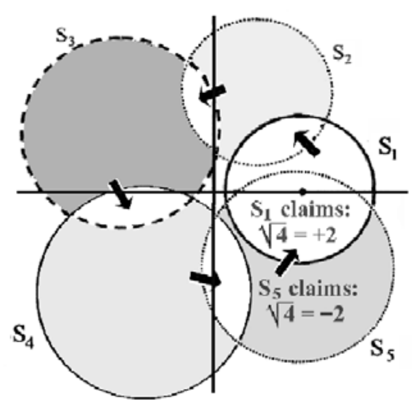
greatly puzzled scientists at the end of the nineteenth century who attempted to render the contents of classical mechanics clear. In fact, this is why David Hilbert set the formalization of classical mechanics on his famous list of problems that mathematicians should attack in the twentieth century.

Such looping introduces a *multi-valuedness* into our circumstances in the sense that our different patches will often describe the same physical system in mutually incompatible ways. Let us now explore some reasons why this odd inconsistency can sometimes result as the natural side effect of *wise* descriptional policies. I'll begin with a number of general remarks that pertain to situations of this type.



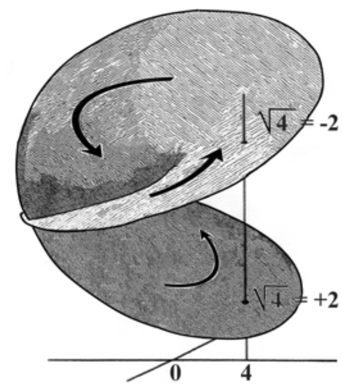
1. First of all, such multi-valuedness is often associated with linguistic expressions that gain their full domain of application through *prolongation from one local patch to another*. Standard examples can be found in the "analytic functions" of complex analysis, where we employ local power series expansions to calculate values for our expression off the real line. Let's consider / z as an example where we want / z to represent the positive square root of z over the real numbers. We can easily find a power series valid between 0 and 1 that can carry the significance of / z out into a little circle S_1 on the complex plane. But can we reach complex numbers that lie beyond the dominion of S? One of the pleasant features about power series calculations is that they can be recentered upon different values. So let c be some complex value just beyond the boundary of S_1. We can now center a new series S_2 upon c and explore where its new boundary δS_2 takes us. If we properly skirt blow ups and branch points, so forth, we will eventually construct a pattern of overlapping domains that covers the complex plane. But often an odd effect occurs as we pursue this building-through-local-prolonagtion program: starting from a region over z = 4, we can continue values for "/ z" completely around the origin, until we once more lie over 4 again. But now the power series we now employ will blithely informs us that, no, the proper value of "/ 4" is *not* +2, as we originally thought; it is actually -2! If we cycle a second time around the





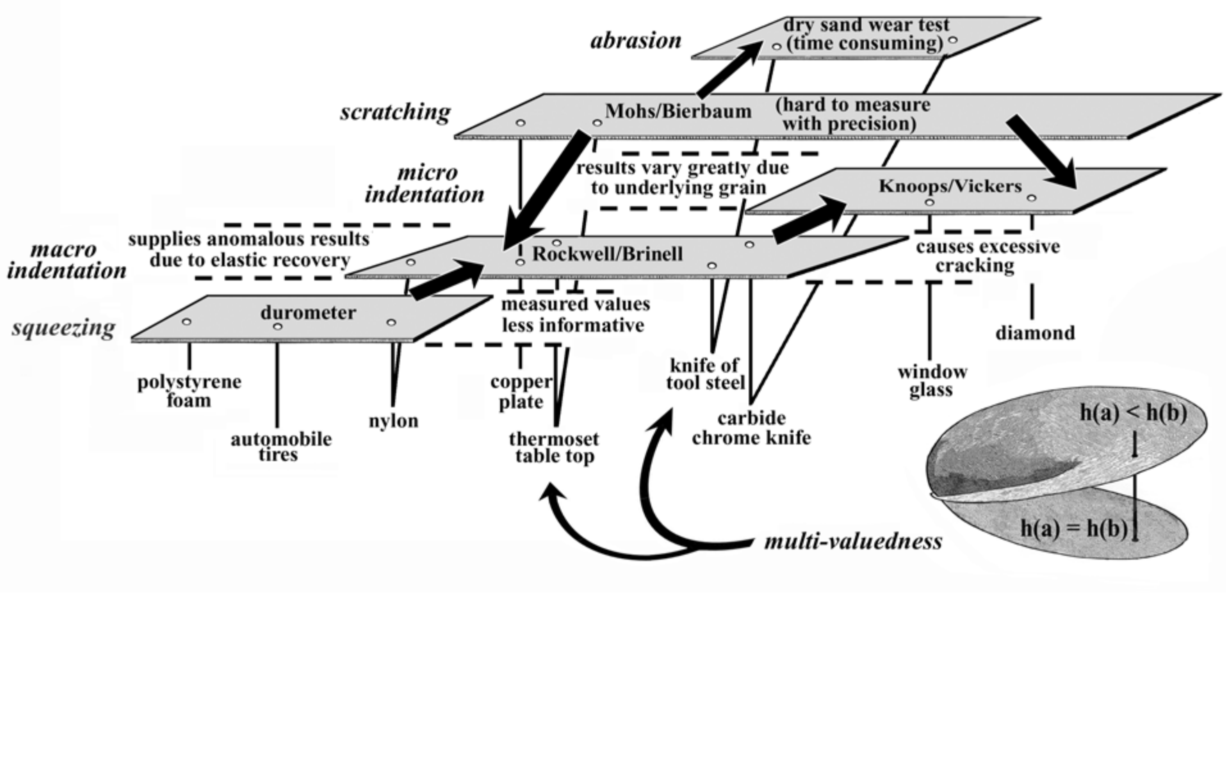
origin using the same kind of continuation, "√4" recalculates more happily as +2 once again. The mere fact that each individual power series supplies unique values to a functional expression *locally* does not guarantee that it will also display unique values *globally.* But this tacit expectation often proves mistaken. This, of course, doesn't ruin the utility of "√4"; it merely means that we need to be careful in how we reason with the expression.

And Riemann provided us with an evocative picture of the twisting that √z evinces: imagine a ramped parking lot with two floors in which we can drive around forever without running into anything (the topology of such a *Riemann surface* cannot be realized as an ordinary spatial shape within three dimensions). While we are driving on level one, the correct value of √4 looks as if it should be clearly +2 but, as we motor onto level two, the value -2 begins to seem preferable. And so on.



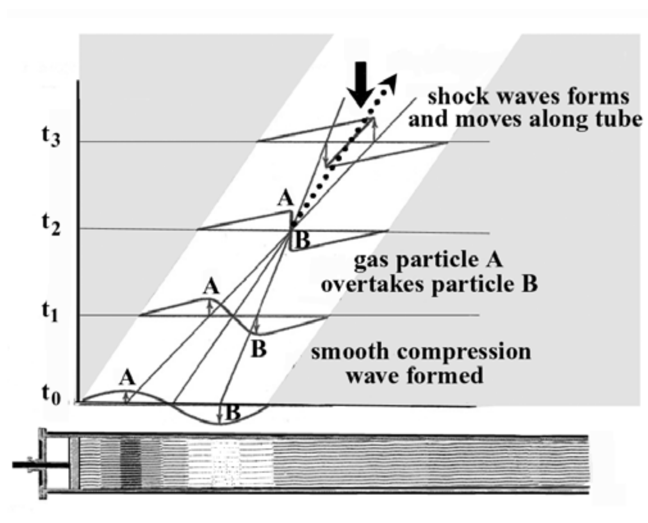
In my book I study a number of linguistic systems that grow through prolongation and sit upon multi-valued Riemann-like surfaces as a result. And the general moral is that they constitute reasonable--and often unavoidable--descriptive systems that are wholly satisfactory as long as we are careful in not exporting data carelessly from one sheet of the surface to another (even if the same rules are valid locally). I call assemblies of this sort *facades*, for a reason I'll explain in a moment.

Here's a very simple example. When industry investigates a material for hardness, it employs a number of different kinds of test designed to be suited to the



particular material at hand--to whether it is a metal, plastic, rubber, ceramic, etc. And, in general, such tests prolong into one another fairly smoothly along their boundaries. Nonetheless, when we piece the whole facade together, certain mild forms of multi-valuedness appear, although they cause no harm because we are rarely inclined to transport data between such distanced forms of test.

2. Let's now switch to another observation which I'll eventually combine with what we've just noted. To render a particular physical situation mathematically tractable, we usually need to *reduce the number of variables* that we actively track. If we can get away with it, a popular scheme for achieving this is to sweep the most difficult parts of the physics into regions we do not attempt to describe accurately: we might call this a policy of *physics avoidance*. And the general rationale is this: if we can examine a situation from several sides and discern that some catastrophe is certain to occur in a certain region, we needn't describe the complete internal details of that calamity in order to predict *when* it will occur and what its likely aftermath is likely to be (it's analogous to being told that "There's going to be a war here and the country will be destitute thereafter": we don't need to know much about the details of the war to calculate what the country will be like thereafter). This is exactly the policy enforced within one of the great paradigms of "physics avoidance": Riemann and P. H. Hugoniot's celebrated approach to shock waves.[3]

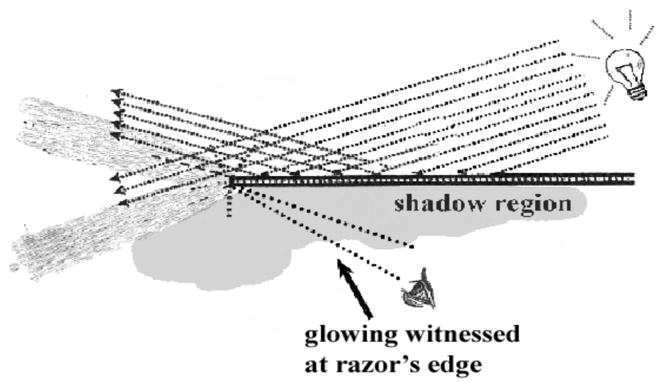


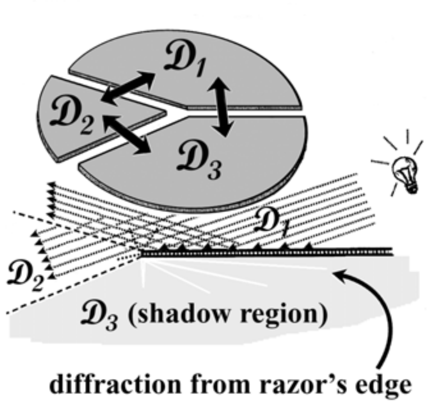
Suppose we put some gas in a long tube and give it a violent shove on one end. There is a simple equation that describes our gas as a continuous fluid, subject to a little viscosity. But if the initial impulse is strong enough, the faster molecules in the pulse will eventually overtake their slower moving brethren ahead and create a *shock wave* pileup, like the traffic snarl that would occur if our molecules had been automobiles. From the point of view of our continuous gas equation, this situation develops a *descriptive inconsistency*, for our equation actually predicts that our gas must display *two* distinct velocities at exactly the same spot and time (in the jargon, its characteristics cross). Prima facie, one would expect that this apparent contradiction in the mathematics will force us to abandon our smoothed out fluid description and turn to the complex details of how discrete gas molecules will interact when forced into such close quarters. "Don't be so hasty," Riemann and Hugoniot advise us. "We can accurately predict from the gas's ingoing behavior when the shock wave is going to arise and how much gas momentum will be funneled into that event. Moreover, by appealing to thermodynamics, we can also predict how the gas on the other side of the shock front will flow smoothly away from the event. By piecing this two-sided information together, we can predict exactly how fast the shock wave will move down the tube, without needing to know the complex details that actually occur inside the shocked region." Thus the Riemann-Hugoniot policy sweeps what, in real life, represents a narrow but still finite region of churning air currents into a two-dimensional boundary that separates regions of smoother gas. The treatment descriptively collapses a finite area of great complexity into a *singularity*: a point or lower dimensional boundary separation. Riemann and Hugoniot do not attempt to write a "law" to directly govern the shocked area's behavior; they instead employ simple "boundary condition" stipulations to dictate how its two neighboring smoother regions piece together.

The fact that a complicated region can be *descriptively avoided* in this manner does not indicate that it is therefore unimportant: the condition at the shock front represents the most important physical event that occurs in our tube. It is merely that we can keep adequate track of its overall influence in a minimal descriptive shorthand, just as "a terrible war between North and South occurred in 1861-5" may supply sufficient information to appreciate the Civil War's long term effects upon our country adequately enough. Indeed, the whole idea of *variable reduction* or descriptive shorthand is that we are able to locate some shock-like receptacle that can absorb complexities and allow us to treat its neighboring regions in a simplified fashion. The basic Riemann-Hugoniot moral sounds like a methodological paradox when stated bluntly: a good recipe for achieving descriptive success *papers over* the physical events most responsible for the phenomena we witness! But that, in fact, is the manner in which successful variable reduction typically works. And it should warn us philosophers of science that much of the action in a physical explanation may not be supplied within its explicit “laws” alone, but may lie hidden within the tacit singularities and boundary joins.

It is also fairly evident that such policies of squeezing into a singularity are not going to always work, and that we will sometimes need open up the internal details of what actually transpires amongst the molecules within our shock front. When this happens, we witness a typical “lousy encyclopedia drop” into a different realm of physical formalism.

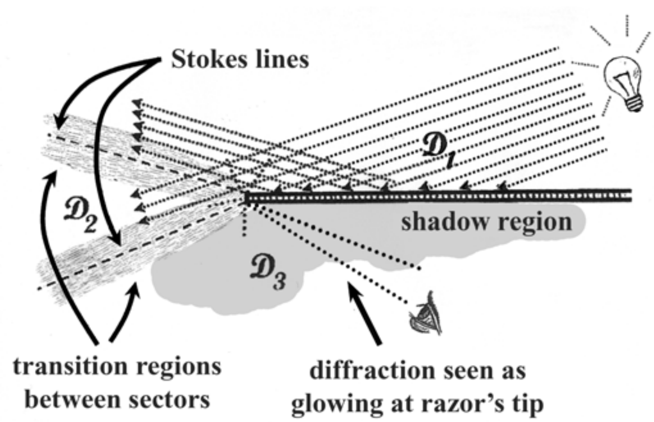
3. Observations (1) and (2) fit together naturally as follows. When we attempt to descriptively cover a single complex situation or handle a wider range of related situations, it is advantageous to adopt different local policies of variable reduction over different sectors of domain and then piece them together along their boundaries by some policy of extrapolation. Here is a classic illustration. Suppose that short wavelength light from a distant light bulb strikes a completely reflective razor blade and we want to calculate how the light will reflect from its surface. We know roughly what will occur: some of the incoming light will miss the mirror, but some will be reflected and mingle with the former and, if we view the razor from the shadow region below, diffraction effects will make it appear to glow as if a flourescent light had been placed there. Now Arnold Sommerfeld, in famous investigations of 1894,[4] found several exact expressions for the kind of analytic function that solves this problem exactly, including a series in Bessel functions. However, these representations prove quite impractical because computing acceptable values upon this basis requires an enormous number of operations. However, Sommerfeld also found that, by dividing the plane around the razor into three sectors $\mathcal{D}_1$,$\mathcal{D}_2$,$\mathcal{D}_3$ and ignoring two extremely thin sectors of complicated behavior along their boundaries, he could replace his slow-to-converge Bessel function series with three series utilizing exponentials and square roots that provide useful values with a quite astonishing reduction in computational complexity (perhaps by a degree of as much as 15,000 to 1). And then we can anticipate roughly what happens in the inbetween regions by extrapolation.





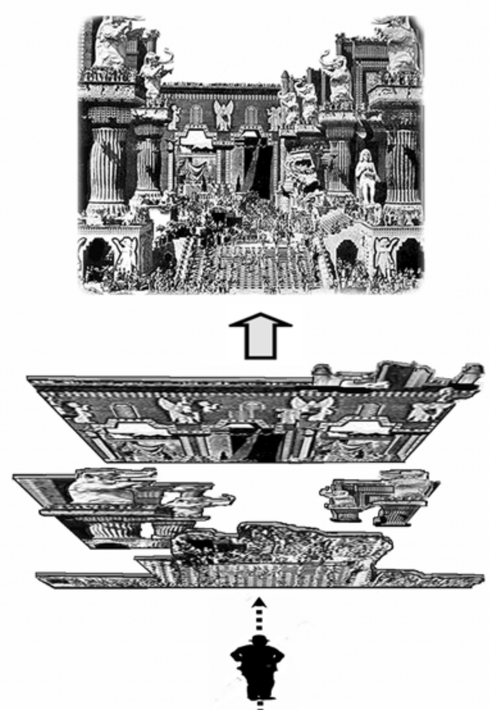
But there is a price to pay for the convenience of our new descriptive reduction. The formula we use to compute values of light intensity around the blade must follow a sectorized policy: in region $\mathcal{D}_1$, we trust formula $\mathcal{F}_1$ to give us correct values, but once the boundary line into $\mathcal{D}_2$ is crossed, our allegiance needs to be shifted to formula $\mathcal{F}_2$ which is obtained from $\mathcal{F}_1$ by altering its coefficients in a certain way and a similar handoff must occur when we move into sector $\mathcal{D}_3$ (this odd behavior is called the *Stokes Phenomenon* (after its discoverer, George Stokes) and the lines that divide our sectors are called *Stokes lines*). In fact, our replacement series obtains its advantages through practicing *physics avoidance* and ignoring the complicated light behaviors that occur within the little slices near the Stokes line boundaries. This policy lets us employ exponential terms to characterize the *dominant behaviors* that occur inside each of the $\mathcal{D}$ patches in very simple terms and to handle the complicated Stokes line regions by shock wave-like patching together. But in effecting this changeover in representational language--that is, moving from Bessel term factors to exponentials and square roots--our descriptive language undergoes what I like to call an *alteration in inferential personality,* allowing those square roots in our representation places our descriptive language upon a Riemann-like parking lot. And this alien element then prevents us from using computational rules that work uniformly everywhere across our razor: we must instead divide our domain of treatment into sectors if we want to obtain reasonable and self-consistent values. So, in my terms, we wind up covering our razor blade circumstances with a reduced variable patch-work facade. In a mathematician's terms, such a facade is the natural structure that arises when one approximates the locally dominant behaviors below using *asymptotic approximation*.

Now it is a striking feature of Sommerfeld's approximation facade (he noted the fact himself) that its components correspond quite tightly to the traditional world of *geometrical optics*--the venerable assumption that light travels in rays--, for our three sectors correspond to its different regions of ray behavior. But we obtain a splendid bonus in addition: our asymptotic policies also supply a ray-like approach to the *diffraction pattern* witnessed around the blade's edge. Now diffraction is not a phenomena that is easily handled within an the old-fashioned ray picture (its traditional explanations were quite strained), but it emerges as a natural companion of the regular rays *once we view our geometrical optics world as arising as a asymptotic facade over an underlying wave domain*. Indeed, from this point of view, it seems natural to investigate whether we can't extend the old ray picture in more detailed ways by considering further terms in higher order



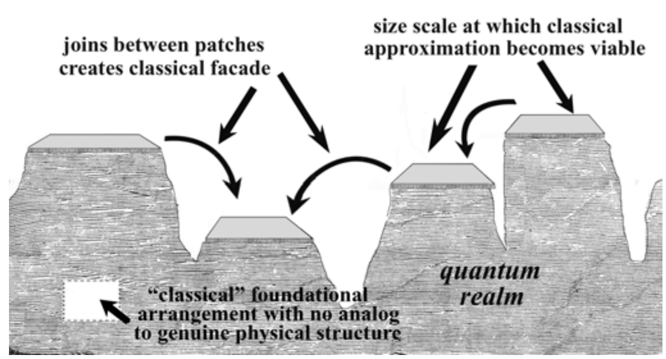
expansions of our asymptotic expansions (the physicist J.B. Keller was a pioneer in this work). If we do this, we find ourselves adding some rather strange ray-like structures to a traditional geometrical optics picture, such as the odd creeping rays that spin off glass globes as they were luminous lawn sprinklers and sundry *imaginary waves* that we can't directly see, but control the intensity of the light we do witness. In this way, we create a very useful structure of *improved ray optics*[5] that floats above the wave account of light in the manner of a patchwork facade. In fact, this is the "world" in which modern optical designers usually work--you can't design a telescope very ably if you attempt to struggle directly with unreduced wave optics. However, if we mistakenly approach the jumble of elements found in our facade as simply a "physical theory" in its own right, we are likely to become puzzled by its "imaginary rays" and all that. Viewed more properly as simply a facade covering of wave optics, these strange locutions make good sense

However, if we don't go so far as to add anything quite so radical as "imaginary rays" to our geometrical picture, our variable reduction procedures can easily create a doctrinal set that looks "kinda like a theory," but with puzzling elements in it. And this observation suggests a useful analogy. In the days of old Hollywood, fantastic sets were constructed that resembled Babylon in all its ancient glory on screen, but, in sober reality, consisted of nothing but pasteboard cutouts arranged to appear, from the camera's chosen angle, like an integral metropolis. When we look at traditional classical mechanics in its full extent, I believe that we witness sheets of mechanical assertion that do not truly cohere into unified doctrine in their own rights, but merely appear as if they do, if the qualities of their adjoining edges are not scrutinized too scrupulously. In short, classical mechanics, taken across its full extent of expected application, represents a patchwork of globally incongruent claims that might very well pass for a unified theory, at least, in the dark with a light behind it. And, borrowing from my Hollywood analogy, that is why I like to say that it is really comes structured as a *theory facade*, rather than a proper "theory" per se.



Now from this point of view, it is natural that our mechanics facade will display the puzzling features we discussed earlier: the *lousy encyclopedia phenomenon* and *foundational looping*. In fact, our survey suggested strategic reasons for expecting these two features to emerge as the natural concomitants of the policies of variable reduction and squeezing complexities into singularities that underwrite the descriptive successes provided in our covering facades. But if we can't eliminate multi-valuedness and looping from our overall set of classical mechanics doctrines, we can't expect that any reasonable closed-unto-itself set of assertions will be able to duplicate our facade in descriptive power, anymore than any self-contained doctrine can weld together the strange ingredients found in modern "improved geometrical optics" into a self-contained world view.

Putting the point in a different way, let's consider how the puzzling descriptive patterns we find in classical organization might look as an asymptotic covering of the quantum domain. That is, let's ask ourselves *from a quantum mechanical perspective*, "At what length scale will quantum effects supply molecules with a sufficiently robust notion of ***shape*** that classical modeling techniques will begin to provide useful answers?" We will soon discover that the quantum/classical handoff occurs at many different levels depending on the particularities of the system studied. That is, molecules (or, quite often, matter collected into bundles of a higher scale of organization) must be first supplied with a trackable "shape" before any form of classical treatment is applicable and the size scale at which these tradeoff points are permitted can vary greatly. And closer inspection will show that both that the kinds of "classical molecules" we require (i.e., point-like, extended or blobby) and the physical data we squeeze into singularities will differ enormously from one classical situation to another. The net effect of this bumpy support makes the set of classical doctrines sitting above the quantum world look like a suit of armor welded together from a diverse set of stiff plates. Considered solely on its own terms, its organizational rationale will seem elusive, but, regarded as outer fitting suitable for a quantum mechanical knight underneath, the entire affair makes complete strategic sense as an efficient asymptotic covering. To dogmatically assume that this jumble of hinged doctrine can be regularized into a self-contained axiomatized format that employs only Newtonian terminology misdiagnoses the true nature of its descriptive successes: they are effective precisely because their sundry routines of physics avoidance neatly cover the



quantum realm like an excellently tailored fabrication of buckler, breastplate and shin guard. In other words, if we purify the contents of the predicates that repose upon our facade into complete internal coherence, we will find ourselves sitting within the land of quantum mechanics, and no longer in classical mechanics at all.

But, of course, it is entirely understandable why David Hilbert and the physicists of his day would not have anticipated this assessment and would have looked to other means for resolving the surface oddities of classificatory use that puzzled the Victorians. Who might have then conceived that it is through *quantum mechanics* that classical doctrine would find its "unity"?

Let me extract a few quick philosophical observations from our discussion. First of all, it should be clear that facades can represent very extremely important descriptive systems (I think this moral carries over into everyday descriptive technique as well). However, unlike the intimations of, say, Nancy Cartwright on topics such as this, the phenomenon should suggest neither sweeping instrumentalist conclusions nor murky mysticism about causation's role in physical understanding. Instead, we learn that certain bodies of doctrine find their "coherence" as asymptotic coverings of other realms, instead of displaying the internal closure expected within traditional philosophy of science pictures. The manner in which improved geometrical optics sits over wave theory constitutes our basic model for the phenomenon and I suggest that many of classical mechanics' oddities can be explained in an allied way.

And this also suggests popular assumptions in philosophical circles that "theories" inevitably support *globally defined models* is far too rash, as the lousy encyclopedia behavior readily shows. It seems to me that we will learn a lot more about how both good science and effective descriptive language operate if we scrutinize in more careful detail the strategic advantages that we extract through suppressing detail within somewhat artificial "boundary conditions" and the like, rather than continuing to blithely appeal to ill-defined "models," "possible worlds" and the like.

1. The materials in this talk (given at the University of Maryland in February, 2006) are largely extracted from my Wandering Significance (Oxford: Oxford University Press, 2006). It collects some of the book's philosophy of science themes materilas into a more compact form.

2.Werner Goldsmith, Impact (Mineola: Dover, 2001), p. 1.

3. James N. Johnson and Roger Chéret, Classic Papers in Shock Compression Science (New York: Springer, 1998).

4. Arnold Sommerfeld, Mathematical Theory of Diffraction, Raymond Nagem, Mario Zampolli, Guido Sandri, trans. (Boston: Birkhäuser, 2004).

5. D.A. McNamara, C.W.I. Pistorius and J.A.G. Malherbe, Introduction to the Uniform Geometrical Theory of Diffraction (Boston: Artech House, 1990).