Background Considerations to “Wandering Significance”

Because I worried too much about scaring off “general philosophy” readers when I wrote WS, I probably didn’t provide my audience with enough early indication of the sorts of structures I would eventually advocate as important to understanding our linguistic practices. In truth, I was secretly motivated by the vast amount of work in approximation theory that indicates that successful calculation is best achieved by stringing together different “patches” of computation that best capture the locally dominant behaviors of a complex system (which are apt to differ from time to time and situation to situation). In this regard, there are strong affinities between my work and some of Bob Batterman’s. Furthermore, mathematicians have also learned that surprising global topologies will often emerge when an usage is built up by gluing one patch to another in a locally effective manner (as happens with many of the standard “special functions of mathematical physics” when they are prolonged to become meaningful over the complex numbers). I strongly feel that the many great successes facilitated in science employing terminology constructed in these glued together ways should impress upon the rest of us the great practical utility that semantic constructions of this sort often enjoy in all walks of life: their only cost is that we need to monitor our inferential practices with a certain degree of attention to local context.

So let me first provide some notes from a talk I recently gave about the hidden structure of classical mechanics, where I argue that, regarded as an enormously successful scheme for macroscopic approximation, the subject secretly rests upon a somewhat unanticipated structural basis (which I call a “theory façade”). Essentially, this same material appears in the latter part of chapter IV, but I was afraid to depend upon it too much in my exposition for the “general reader.”

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In his “celebrated Mathematical Problems” of 1899, David Hilbert 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.

Many commentators believe that the problem must be currently unanswerable, as quantum physics is not yet in a state where it can be profitably axiomatized. Indeed, Hilbert explicitly stated that one shouldn’t try to axiomatize any subject until it has reached some condition of relatively settled maturity. But the specific “mechanics” that Hilbert wished to clarify is that common corpus of “classical mechanics” doctrine upon which most contemporary workers seem to agree, in the same manner as we generally agree upon what “Euclidean geometry” amounts to.
In this regard, we shouldn’t necessarily expect that Maxwell’s electromagnetism or even gravitation will fit into this agreed upon “mechanical” scheme. Indeed, eight years before Hilbert wrote, Heinrich Hertz had attempted to clarify the proper extent of mechanical doctrine partially out of a desire to see whether Maxwell’s theory should properly considered as truly “mechanical” or not. So restricted, Hilbert’s problem looks like it should be solvable.

Indeed, viewed from this point of view, I think most philosophers would presume that the problem should be relatively easy and Patrick Suppes seems to have once claimed that he “had solved Hilbert’s problem” by producing a fairly trivial axiomatic scheme. But Hilbert knew better than this and recognized that there are greater unclarities about where one should start in formulating mechanics than arise in Euclidean geometry. In explaining his problem, Hilbert explicitly identifies the two out of the three most probable foundational contenders. These varying approaches differ in selecting different species of mechanical object to serve as “basic” and constructing the others as approximate methods for speaking about the “basic objects” chosen. Specifically, we have three choices that lead to quite different sets of fundamental axioms, each with their parochial problems:

1. point mass taken as basic → mass point mechanics

2. rigid body taken as basic → rigid body or analytical mechanics

3. flexible body taken as basic → continuum mechanics

Then part of our task is to properly locate the other elements within the framework chosen.

*Example:* following foundational choice 1, we might hope that talk of a continuous fluid or solid might be explicated as a *smoothed over approximation* to the behavior of a swarm of strongly interacting mass points.

Hilbert’s recommendations on this score later led to the development of *stochastic differential equations.*

*Warning:* Although we know that, in some sense, continuous fluids physically represent convenient approximations to *quantum swarms* of “elementary particles”, it should not be entirely evident that the expected mathematical behaviors of
classical continua can also be successfully mimicked by classical swarms of point masses.

In fact, continua often work better as the best microscopic approximations to quantum swarms. It has been known since the mid-nineteenth century that molecules cannot be adequately modeled as classical point mass swarms (see Maxwell’s article on “Atoms” in his collected papers).

When we shift instead to adopting flexible bodies as our “basic objects,” we must distinguish Hilbert’s desired “foundations of mechanics” from what might be called the “fundamentals of mechanics”—the stuff that one wants to first teach to beginning physics students. No one in their right mind would attempt to teach partial diffeo-integral equations to students before ordinary differential equations. But Hilbert correctly anticipated that the foundations of continuum mechanics require the use of the former (point masses only require o.d.e.s). In fact, the great twentieth century clarifications of continuum mechanics due to Hamel, Truesdell, Noll and others were the direct result of Hilbert’s own advocacy of integral equations. If one is forced to choose between point mass and continuum approaches, the latter undoubtedly does a better job in sustaining the body of classical modelings that have proved successful in practice.

However, in practice, we generally refrain from choosing one format definitively over the other and the unsettled rationale for this practice is the “funny thing” I want to highlight here (the talk from which this was taken was called “A Funny Thing Happened on the Way to the Formalism”).

In this regard, it is important to recognize that physical processes that are easy to understand from a point mass point of view can become quite tricky to treat from a continuum viewpoint (and vice versa). Here are two examples, which both trace to the fact that standard partial differential equations never alter the topology of the system under consideration.

Example 1 (droplet formation): using just p.d.e.s one can theoretically get an agitated fluid to form into two blobs connected by a very thin thread but the p.d.e. will never tell the droplet to completely sever its umbilical connection to its base liquid. Within orthodox continuum mechanics, practitioners usually put in the expected severance by hand: they just start their modeling over with two bodies rather than one. Of course, this situation is very easy to understand from a point mass point of view.
Example 2 (wake closure): According to orthodox continuum mechanics, when one drags a knife into a fluid, it pushes the top surface of the fluid ahead of its edging, stretching this layer greatly as it goes. For short periods of time, this mode of description is quite reasonable, as the fluid on each side of the cut may be in quite different states. But soon conditions on both sides of the wake should equalize and the dragged boundary should disappear in the natural manner we would expect if we had modeled our fluid as a swarm of molecules. But orthodox continuum mechanics has no means for closing the resulting gap and maintains that, theoretically, the dragged upper surface never heals itself inside the fluid.

Now there are modern schemes for evading this problem but they are rather strange and unnatural (albeit useful)—see my “Beware of the Blob” for some details. But in most applications of classical mechanics like this, practitioners commonly switch from one mode of modeling to another, appealing to atomist models when their continuum modeling occasions difficulties like this.

One needs to be careful about whether the meanings of physical vocabulary alter when one switches from one species of foundational object to another and Hilbert explicitly warns of this danger in “Mathematical Problems”

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.

Here’s an important example of such terminological shifting (which I call “property dragging” in WS).

Example: a bead sliding without friction along a rigid wire. A condition like this is usually called a constraint in mechanics. Traditionally, the wire is said to exert a “reaction or constraint force” upon the bead that pulls it away from its natural trajectory (most textbooks introduce this notion without any apology at all). But how must these “constraint forces” behave in order to produce this motion?
Answer: f must be velocity sensitive because the wire must exert a stronger “reaction force” f to pull the bead exactly to the right point B on the bead as the bead’s velocity v becomes greater.

Accordingly, these kinds of “forces” must be significantly different from those produced by gravitation and the like. Instead of calculating the strength of the force to find the motion (as in celestial mechanics), we must find the motion before we can calculate the strengths of the “constraint forces.” Worse yet, the standard textbook pathway from “Newton’s Third Law” to the conservation of energy becomes blocked if we allow “forces” like this. This realization seems to have prompted Heinrich Hertz to write his (once) famous Principles of Mechanics in 1892. However, Hertz’ proposed solution to this difficulty was rather strange, and most thinkers conclude that classical systems can never strictly obey constraints. In other words, both our point mass and our continuum physics viewpoints require that, strictly speaking, our bead must instead wobble about the wire as it moves forward.

If we accept this conclusion, then we must rule out rigid bodies as fully acceptable “classical objects.” Indeed, analytical mechanics itself must be seen merely as providing rules for a convenient set of approximations. Indeed, formalisms that employ rigid bodies as “basic objects” typically contain all sorts of descriptive holes (they allow balls that slide, but not roll, for example) that preclude such formalisms from supplying “complete accounts of nature” (this fact comprises a running theme in my book, largely because “rigid body” is considered to be a paradigm of a “clear and distinct concept” within traditional philosophy).

If we don’t notice that some secret shift has taken place with “force” when we apply constraints, we are apt to be confronted later with puzzling paradoxes when the inherent tensions between the employments come to light. I think that lots of standard philosophical puzzles trace to this source as well (which is why I highlight these in the opening chapters of WS).

Such considerations raise the specter of what I call the lousy encyclopedia phenomenon, named in honor of a horrible reference work my parents bought in the 1950s. I was very interested in snakes and so I eagerly turned to the article on “Snake” but it was very thin gruel indeed, telling me very little about my subject. But there was still hope, for at the end of the article was a rather lengthy list of further information.
pertinent articles. So I steadfastly looked up every one, with disappointing results yet with the allure of even further citations I might pursue. Eventually, of course, I found myself referred back to “Snake” itself, having learned very little about my beloved reptiles in the cycling.

Indeed, the following situation often occurs in physics textbooks. In the main text, we are taught how to model some common physical situation using a modeling based upon choice A of “fundamental objects.” But it is apparent that the suggested treatment won’t work for all natural cases and so we find a little footnote saying “For more details, see….” When we actually consult the cited text, we find that the problematic situations are there modeled utilizing a completely different ontological basis B.

Example: billiard ball impact. Virtually, every elementary physics text will usually treat this problem in Newton’s “coefficient of restitution” manner, in which we actually never consider any change of shape in the balls. Our “basic objects” are therefore rigid bodies. But the book’s footnotes will usually warn us that the balls actually compress upon impact and that such behaviors are addressed in texts on continuum mechanics. As we do so, we should notice that we have switched our “basic objects” from rigid bodies to flexible continua (actually, there are a variety of different degrees of approaching the problem from a continuum point of view, but I’ll not go into those subtleties here).

But when we finally consult a continuum text, it will inform us in a footnote that its equations often break down at so-called “shock fronts” and that those events are often better addressed using rigid body or point mass models! (there is a celebrated trick due to Riemann and Hugoniot that delays the need to model the interior of the shock front in this manner, but it is not a permanent fix for the problem). Prima facie, it appears as if we’re witnessing a “lousy encyclopedia” cascade within “billiard ball mechanics” that never supplies us with a permanent choice of “basic element”.

Now in 1899 it was reasonable to suppose that these problems could find a reasonable resolution, possibly by modeling molecules as continua made of a material not prone to shocks. However, the “funny thing” that happened on the way to this potential formalism comes from the fact that we now appreciate that, at very small scales, classical behaviors must shift over to becoming intrinsically
quantum behaviors. Specifically, molecules gain palpable “effective volumes” only through the exclusion principle and other means and hence don’t “occupy space” in any of the standard classical manners. So let us consider the varying tradeoff points (=size scales) where Nature first permits us to approximate the underlying quantum behaviors through classical modeling techniques with some measure of descriptive success. In other words, at what size scale do we find that we can reasonably model chemical or physical phenomenon X with some form of “classical modeling”? Depending upon the phenomenon at hand, such scale choices are apt to differ rather widely (the cutoff is much higher for electrical phenomena than for stretching and contracting, for example) and different tradeoff levels often select different choices of classical “basic object” as their favored modeling device (as we noted, fracture phenomena often prefer point mass modelings or the like, whereas normal fluid flow usually calls for a continuum-based treatment).

If we now erase the quantum supports underneath these locally advantageous classical approximations, we are left with a linked “lousy encyclopedia” structure glued together as pictured. Now from the point of view of modern approximation theory, this is probably a good thing, for we understand that the best approximation schemes attempt to capture the dominant behaviors of a complex system in a localized and patch-like manner, whose predictions are then pieced together into a full treatment of the system across a range of localized behaviors. From this point of view, the “lousy encyclopedia” structure of classical modeling technique can appear quite optimal considered as a tractable approximation technique suitable to a quantum world. From this computational vantage, we positively shouldn’t wish to tie “classical mechanics” to a fixed ontology; instead, we should rather encourage “ontology shifting” as a means of obtaining locally optimal approximation results.

Mathematicians know that when we build up large structures through patch-to-patch continuations like this, global arrangements often emerge that display multi-valuedness and funny topologies like Riemann surfaces (which I illustrate below). In fact, recent studies in effective variable reduction indicate that such behaviors are rather common. The picture illustrates Barry Simon’s
explanation of the natural anholonomy pertinent to the Foucault pendulum (which I discuss in my reply to Bob Brandom). Although I did not know of this example when I wrote my book, its construction almost exactly mirrors those I examined in the book.

In any case, the ultimate result is that “classical mechanics,” taken in its traditional entirety, should be regarded as an optimally useful set of patches for macroscopic descriptive purposes that only looks like a single integrated theory if you don’t inspect its sundry joins quite closely. I call it a “theory façade” because the results resemble a “theory” in the same manner that a bunch of cardboard cutouts on a Hollywood sound stage might resemble ancient Babylon.

And that, I submit, represents the fundamental problem with Hilbert’s 6th problem on mechanics: it seeks to smooth out an effective descriptive structure that inherently refuses to lie flat.

I would like to strongly stress that my argumentation here only applies directly to the sorts of syntactic structuring that one expects to find in macroscopic “reduced variable” circumstances and allied expectations should not be automatically carried over to the quantum level per se (to be sure, some of the observations I make in the book may prove germane in that arena, but not in the basic “theory façade” mode that I explore in the book). I have found that careless readers of WS often over-generalize my observations about “facades” to suit non-”reduced variable” circumstances in a loose manner that smudge my doctrines into a loose “anti-realism” analogous to Nancy Cartwright’s, even though doing so would make the “scientific realism” within which I operate quite incoherent. Indeed, I would argue, pace Cartwright et al., that we need to gradually develop a general sense of how façade-like structures fit in with more straightforward forms of descriptive vocabulary in a manner that makes coherent sense of our overall inferential practices (I believe in the “unity of science” in a much stronger manner than most of my philosophy of science peers—it is just that this “unity” needs to be expected in the manner common with modern applied mathematics, not in the “logic”-based manner than philosophers usually follow). These are, essentially, the issues under review in chapter VIII and connect to my sundry observations about the importance of “correctness proofs” in applied mathematics and all that.
Turning to an allied theme, WS also predicts that allied patchwork structures will often emerge as an originally inexact and relatively unstructured body of calculations gets refined into greater accuracy. This is the point of my critical remarks about Ruth Millikan’s “tuning to natural kinds” in chapter VI. Here’s portions of a talk I gave on this general topic:

Consider this celebrated maxim from Hilary Putnam: “Cut the pie any way you like, meanings just ain't in the head!” But what, exactly does that gnomic pronouncement mean? I will suggest that Putnam himself took a wrong turn in exploring the potential salience of the kinds of consideration he was raising.

In his earliest work, Putnam tried to investigate, in a fairly straightforward way, how the apparent “references” of scientific vocabulary behave over time, regarding such histories as excellent laboratories in which hypotheses in the philosophy of language might be tested. But by the time of his “The Meaning of ‘Meaning,’” Putnam had aligned these early musings with Saul Kripke’s picture of how the logical behaviors of a certain species of quantified modal logic could be supported in a conception where “natural kind” reference is fixed through initial baptismal acts.

And Kripke’s logical requirements demand that “natural kind” predicates must behave in a rather simple way in the long term, in the familiar “attachment to a natural kind” manner illustrated in the attached cartoon. It has long been my view: that this alignment of Putnam’s original investigations with Kripke’s project represented a mistake on Putnam’s part because

(1) the posited alignment prejudges the historical situation by requiring language to develop along rigid and improbably tidy developmental pathways;

(2) it contains a covert commitment to “fierce propositionalism.”

In WS, I argue that a wide range of considerations suggest that it is unlikely that many predicates of macroscopic classification will naturally “tune” themselves to unique physical properties (I call this “the myth of natural kinds”). Here’s a worry I had back in the days when “The Meaning of ‘Meaning’” first appeared (it comes from my “Predicate Meets Property”). Consider the denotation of the term “Gray’s zebra,” first applied to a group of animals on the west coast of Africa. They seemed quite unlike the animals found directly on the assessable other side of
Africa and so the term “Gary’s zebra” was not extended to fit them. However, moving around the horn of the continent, one finds a more gradual shift in zebra population characteristics and I augured that, if exploration of Africa had followed the black path rather than the red path, “Gray’s zebra” would have naturally extended its range to cover the sub-species on the right. So “tuning to natural kinds” can’t be quite as straightforward as Kripke and Putnam suppose.

However, it is hard to argue for such counterfactual claims about language development convincingly. So I asked myself, “Can we witness the hand of non-unique developments operating within occurrent forms of linguistic process?” Perhaps different potential developments might simultaneously appear within different niches of localized context. The resulting usage would comprise a patchwork of differently specialized local applications. And such speculations align nicely with the applied mathematics considerations sketched above: patch-formation is likely in real life terminological application because, in dealing with nature effectively at a macroscopic level, we must find adequate “reduced variables” so that we describe our environment using a language of manageable size. As we observed, the “reduced quantities” that will prove locally optimal are apt to shift their focal concentration slightly as we move from patch to patch, even when we fancy that “we are using language in the same way” as we do so.

This suggests the following prediction about how words will align themselves with the world in patchwork framework circumstances: As language evolves, the patches it will naturally form with respect to a given word are often subject to “property dragging” (= shifted alignments of predicates with physical properties). Here’s a simple example.

“Property dragging” with “force”: A common method of “reducing variables” treats the track and ball as completely rigid. But when we do this, a component of the mechanical work required to move the ball along the longer path secretly shows up inside the local denotation of “frictional force” term. Nobody recognized the magnitude of this shifting effect until the 1950’s. It is related to the shift in “force” we witnessed in the bead case above.
The same behavior is displayed in a more sophisticated way in the Foucault Pendulum example mentioned above.

The basic claim of WS: over time profitable language use often naturally splits into patches where terms carry locally adjusted forms of physical significance. The diagram above shows some of the patches natural to “x is harder than y,” as that term is utilized across materials science (see WS, chap VI). Note that this patchwork is multi-valued in places, a common side-effect of building descriptive structures through patch-to-patch assembly. If Putnam had not prematurely wedded his conclusions to Kripke’s program, he might have observed that such a patchwork is often the more probable result of linguistic evolution than simple “natural kind” alignment.

Obviously, such “property dragged” patches create a potential risk for error if data is carelessly exported from one patch into another. But applied mathematics has firmly taught us that such problems can be cured through installing a suitable system of “border controls” between patches. In fact, mathematics offers a fantastically rich object lesson in how a sublimely useful a multi-valued linguistic object can be if we merely practice simply borderline controls: the square root “function” \( x \). As Riemann showed, its computational behavior naturally sits on a funny surface above the complex plane. To avoid paradox, one merely
needs to attend to the “sheet” of the Riemann surface on which one is currently calculating. With that simple supplement, "\sqrt{x}\" becomes a wonderfully useful thing. So applied mathematics strongly supports my key moral: multi-valued patchwork linguistic arrangements are not intrinsically bad. Although such “reduced variable” practices can cause confusion if transfers of data between patches are not monitored appropriately, patchwork word/world alignments can represent close to optimal designs for computationally effective language use at a macroscopic scale as long as appropriate contextual controls have been put in place. Because of this optimality, we should expect that as we strive to describe the complicated world about us in an increasingly effective manner, tacit strategies for controlling data transfers between localized descriptive patches will gradually emerge slowly within our improving practices, often forged largely through trial-and-error experimentation without explicit recognition of their functionality.

Here I should mention that Riemann surfaces and allied objects are themselves constructed in mathematics through patch-to-patch gluing and it is a general fact that one can obtain multi-valuedness and other odd global behaviors when one builds up a usage through the local prolongation of one descriptive patch into another. It is this fact I am trying to stress in the opening pages of chapter VI; it forms part of my “theoretical reasons” for supposing that the natural evolution of linguistic pattern will often move towards patchwork-based “facades.” As such, it predicts a more finely adjusted form of linguistic evolution than Millikan’s “tuning to natural kinds.”

So here’s how I would recast Putnam’s slogan that “meanings just ain’t in the head”: A good design for the effective employment of macroscopic classificatory words needs to be shaped as much by environmental opportunity as prior mental intent. Here “environmental opportunity” means the localized adaptation of effective “reduced variables” for the local patch/task at hand, usually adapted from syntactic materials already operative in neighboring forms of “descriptive patch” (that is why I imitate the mathematicians in saying that a usage “prolongs itself” from one patch into another).

Note that such considerations suggest a quite anti-Kripkean attitude towards our ability to found the rules of logic upon a permanently trustworthy basis. Our sundry patches will be generally patched together by sundry non-logical inferential rules that I call “strands of practical advantage” in the book (I have in mind various sorts of mathematical techniques that are commonly adapted to suit fresh circumstances). As such, these prolonging
inferential policies are rarely “logical” in character. Indeed, the emerging importance of our effective “higher order” patterns of reasoning often warns us to not travel along sundry logically approved pathways. Thus first order logic may assure us that the red arrow reasoning path is sound if the blue arrow path is, but actually following the red arrow path may lead to inferential disaster in real life.

**Analogy:** left to its own devices, logic plots the streets of reasoning in tidy, Midwestern regularity, but the patch building requirements of useful “higher order” reasoning restrictions often creates a situation where some unpleasant giant of predictive failure has warned us to not venture into his shadow lest we get stomped (i.e., we’d better only follow the inferential routines close to the blue arrow in the other diagram). In this manner, we should be wary of excessive claims about the “generality of logic” in philosophy and its supposedly forever trustworthy qualities (I once wrote an essay called “Can We Trust Logical Form?” about this topic).

**Moral:** Tidy logical pattern and the productive forms of “higher order” reasoning do not always mesh together easily. Sometimes, we must set logic’s inferential permissions on the back burner until we can reconcile these discrepancies. As my hero Oliver Heaviside sagely observed, “Logic is eternal; it can wait.”

But Kripke’s views on “natural kind” reference are precisely framed to guarantee that his rules for modal logic manipulation can be trusted at all times and across all contexts, even during historical epochs when we know little about the true nature of our “natural kind” references. This constancy in “logical form” is the Kripkean theme in which I think Putnam should not have acquiesced.

On a related note, profitable notions of “possibility” (= “ways that things might have been”) are rarely universalist in character, but are instead contextually adapted to the local patch in which one is presently working (surprisingly, the “father of possible worlds” (Leibniz) believed the same thing). Kripke’s semantic presumptions require that “natural kind” predicates must provide totally defined maps from n-tuples selected from every possible world to truth-values (these maps comprise “intensions” in Carnap’s sense). Such thinking quickly leads to an embrace of “fierce propositionalism”: the contents of our sentences can be usefully characterized by their alignment with the cosmic maps that supply propositional truth-values for sentences across all “possible worlds.” Many present day
semanticists see their job as one of providing these “fierce proposition” alignments for natural language.

But I think we should be quite leery of such assumptions. Not only should we not expect that predicates of macroscopic classificatory utility will invariably “tune” themselves to univocal “natural kinds,” we should also recognize that it will actually prove sub-optimal linguistic engineering point of view to attempt to forge unitary designation links within a macroscopically well-adjusted language. We should instead allow enough “free play” in our language that our evolving predicates can scramble to find more effective patterns of patchwork alignment through, inter alia, trial-and-error exploration.

My general interest in the advantages of contextual control of data trace to these concerns, because one needs such tools to operate properly upon Riemann surface-like platforms. Quite independently, I am struck by how swiftly we make context-controlled shifts in representational frame within our humblest forms of everyday thinking. The very swiftness and frequency of these shifts helps disguise from us the extremely context sensitive nature of our actual employment of language. This is a theme I develop further in chapter VI: I’m trying to explain why we often don’t notice the complexity of our everyday descriptive practices (in science, such complications often come to light when we try to program computers to duplicate the complex inferences that a skilled engineer instinctively follows).

In the first part of my book I gather together sundry ordinary language examples that have traditionally occasioned philosophical puzzlement with the anticipation I can later unravel some of their perplexities as the natural expression of hidden global topologies in the manner just indicated. And my central criticism of traditional views of concepts is that they are simply too stiff to easily allow usages structured in this way to grow naturally. But it would have probably helped if I had gotten my alternative models out a little sooner in the book.

So it is my suggestion that philosophy of science-inclined readers might wish to skip most of chapters II, III and V after skimming the preface and chapter I. The basic complaint expressed in the earlier chapters is that standard philosophical approaches to “concepts” and “meanings” (whether of a “classical” or “anti-classical” stripe) implicitly install excessively stiff structures into language that make the natural evolution of language into my patch-like facades seem unlikely. That is, most traditional views require that, when we understand a “concept,” we will have “grasped” semantic constraints that have to be violated if we carelessly ever allow a “facade” to form later. But I think that rather simple
computational considerations suggest that it is quite unlikely that human brains, taken either singly or in “division of linguistic labor” collaboration, can store enough data to completely anticipate how a term should usefully prolong itself to future circumstances (the arguments about interplanetary explorers in chapter V are intended for this purpose). Nor should we expect that environmental factors of a “tuning to natural kinds” pattern to turn the trick either (for the reasons canvassed above). So the overall task of WS is to scale back our prevailing philosophical expectations of the extent to which we can fully anticipate how macroscopic language will usefully adapt itself to novel circumstances in the future (Kripke’s futuristic demands strike me as a good example of the sort of linguistic predictions we should not be making). I believe that we want to develop a view of language’s ties to the world where the façade-like developments I have traced in the evolution of classical mechanics doctrine will appear as an expected commonplace within the historical evolution of descriptively effective vocabulary, rather than striking us as a deviant and unexpected anomaly in the manner of more traditional views of “concepts” and “meanings.”

But the primary interests of a philosophy of science-inclined reader will be directed towards the structural features that I impute to classical mechanics and the like and these ruminations begin largely in chapter IV. In the book I’m currently writing, I’m going to let most of the “general philosophy” stuff go and concentrate solely upon the descriptive philosophy of science!