My talk today concerns the contribution of experimental practice to conceptual understanding in the sciences. Understanding conceptual content was once a prominent topic in philosophy of science, reflecting the now widely recognized neo-Kantian roots of the Vienna Circle. Yet the logical empiricists’ semantic doctrines are now perhaps the most thoroughly repudiated part of their philosophical program. Moreover, some of their most influential critics, notably Quine and Putnam, framed their criticisms in ways that seemed to circumvent issues of conceptual content. This talk is not the place to review that history; I will only note that the relative eclipse of questions about conceptual articulation in science ought to strike us *prima facie* as odd. Enabling us to entertain and express previously unthinkable thoughts, and to understand and talk about previously unarticulated aspects of the world have been among the sciences’ most striking accomplishments.

Outside of philosophy of science, questions about conceptual understanding have attained renewed prominence in the work of John McDowell, Robert Brandom, John Haugeland, and others. McDowell strikingly formulates their closely related objections to a widespread failure to negotiate a critical philosophical passage. On one side of this passage loom the rocks of Scylla, where attempts to ground conceptual content on merely “Given” causal or experiential impacts run aground. On the other beckons the whirlpool Charybdis, where the mere intra-linguistic coherence of purported conceptual judgments falls into “frictionless spinning in a void.” Part of what experimentation contributes to theoretical modeling, I shall argue, is to enable scientific understanding to negotiate its way past both hazards in this philosophically perilous passage.

This suggestion runs against the grain of post-positivist philosophy of science. Work on theoretical understanding in the sciences has been rather more attentive to the influence of theory on empirical evidence more than to any empirical contribution to conceptual content. Quine’s (1953) famous image is instructive in presenting scientific theory as a self-enclosed fabric or field that only encounters experience at its periphery. On the resulting division of labor, experience and experiment provide occasions for conceptual development, but the work of conceptual articulation is an entirely linguistic or mathematical activity of developing and regulating inferential relations among sentences or equations. Yet that division of labor between “internal” conceptual development and “external”
empirical testing blocks any passage between Scylla and Charybdis. Or so I shall argue.

I — Experimentation and the Double Mediation of Theoretical Understanding

Philosophical work on scientific theory nowadays increasingly emphasizes the need for models to articulate the content of scientific theories. This shift is often traced back to the work of Ron Giere (1979, 1988) and Nancy Cartwright (1983), although there are surely earlier antecedents. A now-canonical claim in this tradition is Mary Morgan and Margaret Morrison’s (1999) characterization of theoretical models as partially autonomous mediators between theories and the world. Theories do not confront the world directly, but instead apply to models as relatively abstract representations of various phenomena; the models are often developed and used independently of specific theories; moreover, the models then sometimes serve as the proximal object of investigation, standing in for the phenomena themselves.

Discussions of models as mediators between theory and the world have generally paid more attention to the relations between theories and models than to the relation between models and the world. Nancy Cartwright (1983) early on recognized the need to “prepare” the descriptions of phenomena before even models of theory can apply, and such descriptive preparation for theory entry is in turn usually preceded by extensive data analysis and data modeling. For the most part, however, philosophical discussions of how theories and models relate to the world only consider this relationship after the descriptions have already been prepared. For example, Bogen and Woodward (1988) argue that the proximal target of scientific prediction and explanation is something already verbally characterized:

Well-developed scientific theories do predict and explain facts about phenomena. Phenomena are detected through the use of data, but in most cases are not observable in any interesting sense of the term. ... Examples of phenomena, for which the above data might provide evidence, include weak neutral currents, the decay of the proton, and chunking and recency effects in human memory. (1988, 306)

If we think about the relation between theory and the world in these terms, however, then it comes into philosophical purview only after the world has already been conceptually articulated. Michael Friedman (1975) highlighted this tendency even more clearly when he argued that in talking about the “phenomena” to which scientific theories are accountable, we are really already talking about scientific laws.
Since I am interested in understanding how the sciences allow various aspects of the world a location within the space of reasons in the first place, I cannot settle for this starting point for understanding the theory/world relation. Yet I agree that it is also crucial to avoid once again resorting to some version of the Myth of the Given. We should not think of data or any other observable intermediaries as “given” manifestations of the world science seeks to understand. Ian Hacking has introduced a different way of talking about “phenomena,” however, which I think can be useful here. Phenomena in Hacking’s sense are publicly accessible events in the world rather than linguistic (or perceptual) representations of them. His discussion also suggests a more subtle shift in emphasis, from what is observed or recognized to what is salient and noteworthy. Phenomena show us something important about the world, rather than our merely finding something in it.

Hacking’s use of the term also clearly has a normative significance, and thus cannot be understood as referring to something merely “Given.” Most of what happens in nature or the laboratory are not phenomena in his sense, for they show us little or nothing. Creating a phenomenon is thus an achievement. The focus of such achievements is, I suggest, the salience and clarity of a pattern against a background. Thus, in talking about the relative rarity of natural phenomena, Hacking suggested that

old science on every continent [began] with the stars, because only the skies afford some phenomena on display, with many more that can be obtained by careful observation and collation. Only the planets, and more distant bodies, have the right combination of complex regularity against a background of chaos. (1983, 227)⁵

Although he thus recognized that some natural events have the requisite salience and clarity, Hacking’s point in introducing the concept was to argue that most scientific phenomena have to be created in the laboratory, and that the creation of phenomena is one the important achievements of experimental practice.

With Hacking’s concept in hand, my response to Morgan and Morrison is that we should treat theoretical understanding as doubly mediated. The creation and differential reproduction of “phenomena” in Hacking’s sense, I shall argue, mediate in turn between the kinds of models Morgan and Morrison (and many others) are talking about, and the world outside the laboratory, in ways that allow for its conceptual intelligibility. Hacking himself may have had a similar concern in mind. He indicated the philosophical significance of creating such phenomena in a remark that has not received sufficient philosophical comment:
In nature there is just complexity, which we are remarkably able to analyze. We do so by distinguishing, in the mind, numerous different laws. We also do so, by presenting, in the laboratory, pure, isolated phenomena. (Hacking 1983, 226)

The kind of “analysis” he has in mind, it seems to me, is to make the complexity with which the world confronts us intelligible, by articulating it conceptually. His claim thus counters the widespread philosophical presumption that laws (or models) and experimental phenomena play very different roles in science. On this presumption, only laws or models make the phenomena intelligible, by explaining them. A noteworthy phenomenon may suggest possible laws or theoretical models, and may even be an indispensable means to discerning them. Yet on this presumption, the phenomena only become intelligible when they are brought explicitly within the space of reasons via a law, a model, or a theory.

To take Hacking’s suggestion seriously, however, we need to understand how the recognition or creation of natural or experimental phenomena could amount to a distinct mode of scientific “analysis,” complementary to theoretical or nomological representation. That requires us to understand how a laboratory phenomenon by itself could show something intelligibly. Catherine Elgin (1991) makes an instructive distinction in this respect between the properties an event merely instantiates, and those it exemplifies. Turning a flashlight in different directions instantiates the constant velocity of light in different inertial reference frames, but the Michelson/Morley experiment exemplifies it. Homeotic mutants likewise exemplify a modularity of development that normal limb or eye development merely instantiates. What accounts for the difference?

Consider Elgin’s example of the Michelson/Morley experiment. The interferometer apparatus allows a light beam traveling tangential to the earth’s motion to show a difference, or the absence of any difference, between its velocity and that of a beam traveling perpendicular to the earth’s trajectory. Any such difference will be manifest in the interference of a light beam in one direction that is out of phase with another part of the same beam that was redirected perpendicularly and then recombined by carefully placed mirrors. The juxtaposition of the returning beams, and the absence of interference fringes on an appropriate detector, clearly show what in other circumstances happens indiscernibly.

The occurrence of this manifestation of the constant velocity of light is thus contextual in a way that belies the comparative abstraction of its verbal representation. We need not mention the beam-splitters, mirrors, compensation
plates, or detectors used to produce the Michelson/Morley phenomenon, in order
to talk about the constant velocity of coincident light beams that traveled the same
distance through the same medium in different directions relative to the earth’s
motion. We often represent phenomena in this way, abstracting from the requisite
apparatus, shielding, and other surrounding circumstances. Moreover, such
decontextualized representations are precisely what Bogen and Woodward or
Friedman meant by “phenomena.” Hacking thinks that such decontextualizing
talk can be importantly misleading, however. Using an example, he claimed by
contrast that

The Hall effect does not exist outside of certain kinds of apparatus. ... That
sounds paradoxical. Does not a current passing through a conductor, at
right angles to a magnetic field, produce a potential, anywhere in nature?
Yes and no. If anywhere in nature there is such an arrangement, with no
intervening causes, then the Hall effect occurs. But nowhere outside the
laboratory is there such a pure arrangement. (1983, 226)

Hacking’s point, I take it, was that such apparatus is not merely a contingently
useful means to produce an isolated phenomenon. The apparatus that produces
and sustains such events in isolation is instead an integral component of the
phenomenon itself, as much a part of the Hall effect as the conductor, the current,
and the magnetic field. The apparatus is integral, that is, if we understand a
“phenomenon” to be the manifestation of a meaningful pattern of events in the
world.

Yet this very irreducible contextuality, and hence particularity, of
experimental phenomena might give us pause. Surely conceptual understanding
must transcend such particularity. To the extent that laboratory phenomena are
contextually particular, they could only instantiate concepts. In order to articulate
the more generally applicable content of those concepts, the concepts need to be
expressed in other ways. Should these considerations then lead us to reject
Hacking’s suggestion? Should we conclude, that is, that the creation of
phenomena can at best be materially and psychologically instrumental to
conceptual articulation, but that conceptual understanding itself must be expressed
in more abstract and instantiable laws or models?

Let’s begin by considering Hacking’s own subsequent account of the
relation between theoretical models and experimental phenomena in terms of
“self-vindication.” Hacking (1992) introduced this term to highlight the stable co-
evolution of models and phenomena, in response to Kuhnian accounts of
conceptual change and the Quine-Duhem thesis of underdetermination. Hacking
concluded that,

The several systematic and topical theories that we retain, at different levels of application, are true to different phenomena and different data domains. Theories are not checked by comparison with a passive world with which we hope they correspond. ... We [instead] invent devices that produce data and isolate or create phenomena, and a network of different levels of theory is true to these phenomena. Conversely we may in the end count them as phenomena only when the data can be interpreted by theory. Thus there evolves a curious tailor-made fit between our ideas, our apparatus, and our observations. A coherence theory of truth? No, a coherence theory of thought, action, materials, and marks. (1992, 57-58)

Hacking is onto something important in recognizing the mutual co-adaptation of theoretical models and experimental phenomena, and I will return to what is right about his account later in the talk. We nevertheless cannot understand the empirically grounded conceptual content of theoretical models in terms of a self-vindicating co-adaptation with their data domain. Hacking’s proposal steers directly into McDowell’s Charybdis, with an account of conceptual understanding that renders it empty in its splendidly coherent isolation. Hacking envisages stable, coherent domains, in which scientific claims gradually become practically irrefutable, by only applying to those well-defined phenomena they have already been shown to fit. He offers that exemplar of conceptual stability, geometrical optics, as illustration:

Geometrical optics takes no cognizance of the fact that all shadows have blurred edges. The fine structure of shadows requires an instrumentarium quite different from that of lenses and mirrors, together with a new systematic theory and topical hypotheses. Geometrical optics is true only to the phenomena of rectilinear propagation of light. Better: it is true of certain models of rectilinear propagation. (1992, p. 55)

The problem with Hacking’s proposal is that, in supposedly securing the correctness of such theories each within their co-adapted domains, he renders them empty (and thus bereft of content about which to be correct). In particular, he helps himself to a presumption that his own account makes unintelligible, namely (in the case of geometrical models of rectilinearity) the sense in which this is a theory about optics. It is one thing to say that geometrical optics only accurately describes some of the phenomena in its domain, and hence is limited in its effective range of application and/or its approximation to accuracy. It is another thing altogether to confine its domain to those phenomena for which it seems to
work. The fine structure of shadows is directly relevant to geometrical optics, and thereby displays the theory’s empirical limitations. But it is only through its openness to such empirical challenge that the theory even purports to be a theory about the propagation of light. We should thus say of Hacking’s account of the self-vindication of the laboratory sciences what McDowell concluded about Davidson: Hacking “manages to be comfortable with his coherentism, which dispenses with rational constraint upon [conceptual thought] from outside it, because he does not see that emptiness is the threat” (1994, 68).

Nancy Cartwright makes a similar move, which nevertheless clarifies the problem by improving upon Hacking’s account of self-enclosed and supposedly self-vindicating conceptual domains. Cartwright argues that concepts like force in mechanics are limited in their scope of appropriate application. Force, she argues, is an abstract concept that requires more concrete “fitting out.” Just as I am not working unless I am also doing something more concrete like writing a paper, teaching a class, or thinking about the curriculum, so there is no force among the causes of a motion unless it can be at least approximately specified by a force function like $F=-kx$ or $F=mg$ (1999, 24-28, 37-46). Those causes of motion that are not mathematically tractable in this form, or at least do not constitute “nomological machines” whose functioning can be decomposed into component “forces,” consequently do not fall within the domain of classical mechanics.

Cartwright’s effort to limit the scope of scientific concepts does better than Hacking, because she allows a limited open-endedness to these conceptual domains. The concept of force extends beyond the models for $F=ma$ that we actually have in hand, to incorporate any events for which models could be successfully developed to some reasonably accurate degree of approximation. This extension is still not sufficient, however. First of all, the domain of mechanics then becomes highly gerrymandered. Apparently similar situations (e.g., the motions of various objects in gravitational free fall in the earth’s atmosphere) fall on different sides of its borders. Second, this gerrymandered domain then empties the concept of force of conceptual significance, and hence of content. The reason is that Cartwright thereby conflates two distinct dimensions of conceptual normativity. A concept expresses a norm of classification, with respect to which concept users may then succeed or fail to show how various circumstances accord with that norm. Typically, we have some understanding of how and why it matters to apply this concept, instead of or in addition to others, and to group together these instances. The difference it makes expresses what is at stake in our success or failure to grasp things in accord with that concept (for
example, by specifying a force function appropriate to those circumstances). Both dimensions of conceptual normativity are needed: we need both to specify what the concept is “about” such that we know what is within its domain, and we need to understand what it would be for it to apply successfully or correctly within that domain. By defining what is at stake in applying a concept like force in terms of criteria for its successful empirical application, she removes any meaningful stakes in that success. In doing so, of course, she thereby undermines both dimensions of conceptual normativity, since, as Wittgenstein famously noted, where there is no room to talk about error, there is also no room to talk about correctness.

II — Salient Patterns and Conceptual Normativity

To understand the constitutive role of experimental phenomena in conceptual articulation, despite their particularity and context-dependence, we thus need to think further about phenomena in Hacking’s sense. A crucial feature of such phenomena is the manifestation of a meaningful pattern in the world. Such patterns stand out against a background. This “standing out” need not be anything like a perceptual gestalt, of course. As Hacking noted, a few astronomical phenomena are visibly there for anyone who looks, but others require more effort; the strikingly elliptical pattern of planetary orbits, for example, could only be recognized via extensive tracking and analysis. Experimental phenomena in turn require actually arranging things in ways that manifest a significant pattern, even if that pattern is subtle, elusive, or complex. As my co-symposiast Karen Barad noted, for example,

It is not trivial to detect the extant quantum behavior in quantum eraser experiments. The experimenters must be clever enough to design an experiment that will detect the entanglement. ... [Zajonc and Greenstein 1997] point out that ... in the quantum eraser experiment the interference pattern was not evident if one only tracked the single detector [that was originally sufficient to manifest a superposition]. ... What was required to make the interference pattern evident upon the erasure of which-path information was the tracking of two detectors simultaneously. (2007, 348-49)

That there is a pattern that stands out in an experimental phenomenon is thus crucially linked to capacities and skills for pattern recognition. As Dennett once noted,

the self-contradictory air of “indiscernible pattern” should be taken seriously. ... In the root case, a pattern is “by definition” a candidate for pattern recognition. (1991, 32)
This link should not be understood to confer any special privilege upon our own capacities for discernment, however. Perhaps the pattern in question shows up through the use of complex instruments, whose patterned output is discernible only through sophisticated computer analysis of the data. What is critical to the notion of recognition, however, is its normativity. To speak of pattern recognition is to allow for the possibility of error. And so the patterns that show up in phenomena must not merely indicate a psychological or cultural propensity for differential responsiveness to them. There must be some way in which our responsiveness to them, our taking them as significant, is open to assessment. Of course, the sciences provide many examples of what were once taken to be revealing patterns in the world, only to be later rejected as misleading, artifactual or coincidental. From the earliest attempts to discern significance in the layout of the constellations, to cold fusion, some seemingly salient patterns have not borne out the efforts to understand them as significant. The challenge is to understand how and why those initially salient patterns lost their apparent significance, and for that matter, why that shift in response is not itself merely a change in our de facto responses rather than a correction of an earlier error.

What makes natural and experimental phenomena meaningful, I suggest, is that the pattern they embody is understood to refer beyond itself, in ways that are informative about a broader range of actual or possible occurrences. Consider, for example, the Morgan group’s initial correlations of differences in cross-over frequencies of mutant traits with visible differences in chromosomal cytology. This phenomenon would have been of no scientific significance (and in that sense, I would argue, not a phenomenon after all) if it turned out to be just a peculiarity of *D. melanogaster*, or worse, of this particular group of flies. Rather, its salience as a phenomenon from the outset depended upon the sense that it showed something important about the cross-generational transmission of traits and the chromosomal location of “genes” as discrete causal factors, in a much wider set of organisms. To this extent, the salience of natural or experimental phenomena indicates their broadly inductive significance for scientific understanding.9

Yet we should not understand this point as an issue of how to reason inductively from a telling instance of some already linguistically-articulated concept to its wider applicability. If we are to understand the role of experimental phenomena in the articulation of conceptual understanding, we need to think less about the inductive-inferential acceptance of determinate judgments, and more about reflective judgment in the Kantian sense. The question, that is, concerns how we articulate and understand relevant conceptual content rather than how we
justify specific judgments that employ it.\textsuperscript{10} The two points are, of course, connected, but they must remain distinct.

Marc Lange (2000) has made an important contribution to this issue with his revisionist conception of natural laws. Lange argues that laws have an expressive significance in scientific practice. A law expresses what it would be for unexamined cases to behave in the same way as cases already considered. In taking a hypothesis to be a law, we thereby commit ourselves to a set of inductive strategies in context that will vindicate the reliability of the inference rule corresponding to the hypothesis. Yet in so doing, I note, we also commit ourselves to the inductive projectibility of the concepts employed in the law.

The familiar difficulty, of course, is that many inference rules are consistent with any given body of data. Lange’s response is to ask which of these possible inference rules is salient within this context. The salient inference rule would impose neither artificial limitations upon its scope, nor unmotivated bends in its subsequent extension. In his example, inferring from local electrical experiments that “all copper objects in San Francisco are electrically conductive” would be an inappropriately narrow scope limitation, for in the absence of any further considerations, geographic location is not a salient feature of these events. In the other direction, “grue” and “quus” are infamous examples of unmotivated bends. The salience of the relevant inference rule, Lange argues, is not something psychological, concerning the way our minds work, ... [Rather] it possesses a certain kind of justificatory status: in the manner characteristic of observation reports, this status [determines] ... what would count as an unexamined [case] being relevantly the same as the [cases] already examined. (Lange 2000, 194)\textsuperscript{11}

Lange’s comparison to observation reports is instructive, but I want to shift his point from reports about what one observes to commitments about what the salient pattern of a natural or experimental phenomenon shows.\textsuperscript{12} Its normative status as a salient pattern meaningfully articulates the world, which thereby helps render intelligible those aspects of the world that fall within its scope, albeit defeasibly so.

Such a role for meaningful patterns in the world to articulate conceptual content does not bring back an empiricist foundationalism that would steer onto the philosophical rocks of Scylla. The salient patterns displayed in natural or experimental phenomena are nothing Given, but instead indicate the defeasibility of both the pattern itself, and its scope and significance. One partial indication of the normativity of such patterns is the role background assumptions can play in
shaping what is salient in an experimental phenomenon. An example from Lange
(2000) illustrates this point especially clearly. Consider the pattern that emerges
from correlated measurements of the pressure and volume of a gas at constant
temperature. In the absence of other considerations, the linear inverse correlation
between pressure and volume suggests its indefinite extension, for all gases,
yielding Boyle’s familiar law. Yet couple this same phenomenon with two further
assumptions — first, that the relevant volume is not the volume of the container
but instead of the free space within it between gas molecules, and second, that the
pressure those molecules exerts is affected by intermolecular forces among the gas
molecules which diminish very rapidly with distance — and the pattern extension
that becomes salient is instead the van der Waals law. What it would be for this
pattern to continue “in the same way” at other volumes and pressures has shifted,
such that Boyle’s Law actually incorporates an “unmotivated bend.” Moreover,
with yet other assumptions, all general patterns dissipate, and must be replaced by
multiple patterns specific to the chemistry of each gas.

Having recognized the inherent normativity of pattern recognition in
experimental practice, we can also recover the requisite two dimensions of that
normativity that I criticized Cartwright for conflating. Recall the difficulty
confronting both Hacking’s attempt to secure the mutual “self-vindication” of
theoretical models and experimental phenomena, and Cartwright’s willingness to
acknowledge “forces” only where there were empirically tractable models of force
functions. The cost of defining the scope and content of scientific concepts in
terms of their successful application was to extract significance or content from
the supposed success. Yet they were right to look to the back-and-forth between
experimental phenomena and their theoretical models as a locus for the
articulation of conceptual content in scientific understanding.

John Haugeland’s (1999, ch. 11) reflections upon the ontological
significance of patterns and pattern recognition can begin to point us in the right
direction. Haugeland distinguishes
two fundamentally different sorts of pattern recognition. On the one hand,
there is recognizing an integral, present pattern from the outside—outer
recognition .... On the other hand, there is recognizing a global pattern from
the inside, by recognizing whether what is present, the current element, fits
the pattern—inner recognition. The first is telling whether something (a
pattern) is there; the second is telling whether what’s there belongs (to a
pattern). (1999, 285)

What does it mean for a pattern to be there as a candidate for outer recognition? It
means that what stands out as salient in that context actually points beyond itself toward some larger set of relationships that are informative about the world. The apparent pattern is not just an isolated curiosity or spurious association, but instead shows something significant. Consequently, there is something genuinely at stake in how we extend this pattern, such that it can be done correctly or incorrectly. Only if it matters to distinguish those motions that are caused by forces from those that are not (if there were any) is there anything in classical mechanics to be right or wrong about.\(^\text{13}\)

What does it mean, in turn, for something to be a candidate for inner recognition, such that we understand how it belongs as an element in or continuation of a larger pattern? To be sure, inner recognition is only at issue if there is some larger pattern there, such that there is something at stake in getting it right. At that point, inner recognition involves understanding just how to go on in the right way, consonant with what is thereby at stake. So for classical mechanics, inner recognition is involved any time we need to identify the forces at work bringing about some interconnected motions, and to calculate their contributions to the outcome. But of course, the existence of a pattern accessible to outer recognition is ultimately dependent upon the possibility of recognizing how it applies in any given case. For as I cited Dennett previously, “the self-contradictory air of “indiscernible pattern” should be taken seriously” (1991, 32). To assert that the world has a genuine pattern of conceptual intelligibility which cannot actually be identified is to take back with one hand what one allegedly gave with the other. Haugeland thus concludes, also rightly, that

> What is crucial for [conceptual understanding]\(^\text{14}\) is that the two recognitive skills be distinct [even though mutually constitutive]. In particular, skillful practitioners must be able to find them in conflict— that is, simultaneously to outer-recognize some phenomenon as present (actual) and inner-recognize it as not allowed (impossible). (1999, 286)

Both dimensions of conceptual normativity, outer and inner, are needed to sustain the claim that the pattern apparently displayed in a natural or experimental phenomenon actually enhances the world’s intelligibility. There must be something genuinely at stake in following out that pattern, and any issues that arise in how to follow it out must be resolvable without betraying what was at stake.

### III— Conceptual Articulation and a New Scientific Image

So far, I have talked primarily about the experimental creation and recognition of phenomena, and have said little about their relation to modeling.
Yet without addressing that question, we will not yet grasp what conceptual understanding is, such that the salience of natural and experimental phenomena actually contributes to it. And it is here that I will, as promised, return to what is right in Hacking’s and Cartwright’s discussions of the relation between laboratory phenomena and theoretical modeling.

Cartwright’s work seems to provide an especially powerful challenge to the account of scientific understanding gestured toward in my talk so far. The heart of much of Cartwright’s work from How the Laws of Physics Lie to The Dappled World could be formulated as a challenge to the compatibility of inner and outer recognition in physics, at least in the terms in which Haugeland and I are construing it. In her earlier work, Cartwright (1983) argued that the explanatory patterns expressed in the most fundamental laws and concepts of physics are mostly not candidates for inner recognition, since most events in the world cannot be accurately articulated in those terms except by extensive ad hoc phenomenological emendation and ceteris paribus hedging. Later (1999), she argued instead that we should regard the alleged universality of the explanatory power of fundamental laws as illusory. The scope of their concepts is restricted to those situations (nomological machines) which actually generate more-or-less lawlike behavior, and to the broader tendencies to behavior expressed in underlying natures or causal capacities that are nevertheless regularly overridden by other causal factors in most real situations. In much of the dappled world we live in, we must seek other, less precise concepts and laws that can be made empirically adequate and practically beneficial without reference to the most fundamental concepts and laws. Scientific understanding is a patchwork rather than a unified conceptual field.

Yet I think Cartwright’s conclusion about the scope of “fundamental” physical concepts only follows if we accept a familiar but untenable account of what it is to grasp a concept and apply it to worldly situations. On this view, grasping a concept is (implicitly) grasping what it means in every possible, relevant situation. On this point, Cartwright is in agreement with her “fundamentalist” opponents who insist on the universal scope of the most basic physical laws. They agree, that is, that $F = ma$, the quantum mechanical formalism, and other theoretical principles provide fully general schemata for what it means to apply their constituent concepts in any circumstances within their scope; they only differ (quite dramatically) on how far that scope extends.

Mark Wilson (2006) offers a telling alternative account of what it is to grasp and apply empirical and mathematical concepts. Wilson’s approach can begin to
reconcile Cartwright’s concerns with my insistence upon maintaining simultaneously the broader stakes that confer content upon scientific concepts, their empirical manifestation in the salient patterns of worldly phenomena, and their detailed explication in practices of theoretical modeling. Cartwright emphasizes the dappled, patchwork character of the world, which is not amenable to smooth, systematic inclusion within the supposedly regimented universality of either classical or quantum mechanical concepts and their model applications. Wilson, by contrast, insists that the articulation of empirical concepts is itself an only loosely unified patchwork of “facades” (Wilson suggests the image of binding together the various pages of an atlas). As a telling example, he suggests, Consider the popular categorization of classical physics as billiard ball mechanics. In point of fact, it is quite unlikely that any treatment of the fully generic billiard ball collision can be found anywhere in the physical literature. Instead, one is usually provided with accounts that work approximately well in a limited range of cases, coupled with a footnote of the “for more details, see ...” type. ... [These] specialist texts do not simply “add more details” to Newton, but commonly overturn the underpinnings of the older treatments altogether. (Wilson 2006, 180-181)

Wilson then calls attention to a series of layered treatments that provide more detailed, sophisticated, and complicated models of the supposedly paradigmatic case of billiard ball collisions. One aspect of this layering includes sequences of added complexity in the physical features of billiard balls that are taken into account (such as the sequence from point masses, to rigid bodies, to almost-rigid bodies requiring corrections for energy losses, to elastic solids that distort upon impact, to the emergence of shock waves moving through the ball, to explosive collisions at high velocities, and so on). Another form of layering within some of these layers breaks down the flexible response of the balls upon impact into stages, each of which are modeled in somewhat different ways, albeit with some gaps and overlaps between their ranges of application. Wilson concludes that, “to the best I know, this lengthy chain of billiard ball declination never reaches bottom” (2006, 181).

I cannot summarize here Wilson’s other fascinating, rich case studies of the many ways in which conceptual facades can be linked together, or the various forms of “property dragging” through which the concepts involved undergo subtle (and sometimes not-so-subtle) shifts in how and where they apply. I will only note that, alongside the kind of intensifying development exemplified by the increasingly complex layered articulations of the mechanics of billiard ball
collisions, there is the kind of extensive articulation required to apply familiar concepts in unfamiliar circumstances. Wilson thus objects to what he calls “tropospheric complacency,”

Our naive inclination to picture the distribution of properties everywhere across the multifarious universe as if they represented simple transfers of what we experience while roaming the comfortable confines of a temperate and pleasantly illuminated terrestrial crust. ... We readily fancy that we already “know what it is like” to be red or solid or icy everywhere, even in alien circumstances subject to violent gravitational tides or unimaginable temperatures, deep within the ground under extreme pressures, or at size scales much smaller or grander than our own, and so forth. (2006, 55)

We typically do not know in advance “what it is like” for situations to fit (or not) our scientific concepts in sufficiently unfamiliar circumstances, or in those aspects of their current circumstances that have not already been modeled explicitly or exhibited in the laboratory. There is no substitute for further empirical and theoretical work if we are to understand what our familiar concepts actually mean under those circumstances.

Wilson’s work offers a useful lesson for Cartwright’s argument. We can endorse her criticism of the idea that in grasping a general, fundamental law-schema we have understood what is happening in more complex or less accommodating settings, while also rejecting her proposal to limit the scope of application of the relevant concepts. Instead of restricting their scope, we should instead recognize that our concepts commit us to more than we know how to say or do, in ways that would typically require both intensive and extensive conceptual articulation to vindicate. ‘Force’ or ‘gene’ should be understood as dappled concepts, rather than as more simply projectible concepts whose scope is limited in a dappled world. Bob Brandom (1994, 583) offers an especially telling analogy here between conceptual understanding and grasping a stick. Even though we make only very partial acquaintance with a concept in the vicinity of our grasp, we take hold of the entire stick from that end. Moreover, when we use the concept for various purposes, we are also accountable for the sometimes unanticipated consequences of that use at the other end and in between. The same is true, I would argue, for the pattern recognition involved in creating and working with experimental phenomena. These patterns can be inductively salient in ways that extend far beyond what we know how to say or act upon.

That is why I talk about inner and outer recognition of conceptually salient patterns in the world in terms of what is at issue and at stake in the use of those
concepts. ‘Issues’ and ‘stakes’ are fundamentally anaphoric concepts. They give us ways to refer to the scope and significance of a pattern, a concept, or a practice (what is at stake there), and what it would be for things to go on in the same way under other circumstances or more stringent demands (what is at issue), even though those issues and stakes might be contested or unknown. The point of such talk is especially evident in Lange’s discussions of inductive salience. He argues that the normative status of inductive salience presupposes a widespread agreement among qualified observers about what it would be for a salient pattern to go on in the same way. That supposed presupposition is too stringent, however. What is needed is only the ability to identify, articulate, and eventually reason about how that pattern extends, i.e., for qualified observers/practitioners to be engaged with and accountable to the same issue, even if they disagree. In a similarly problematic way, Lange argues that what I’m calling inner recognition can be shaped by disciplinary interests.

A discipline’s concerns affect what it takes for an inference rule to qualify as “reliable” there. They limit the error that can be tolerated in a certain prediction ... as well as deem certain facts to be entirely outside the field’s range of interests.... With regard to a fact with which a discipline is not concerned, any inference rule is trivially accurate enough for that discipline’s purposes. (2000, 228)

Lange’s basic point is correct, but what matters is not what members of a discipline are de facto interested in, but rather what is at stake in the practices, achievements and concerns that are the focus of their work. They can be wrong about what is at stake in their own work, and hence which considerations matter to the proper extension of their own concepts, models, and salient phenomena. Moreover, those stakes can shift over time as the discipline develops, in ways that can call into question its prior circumscription of its domain.

In this context, we can now see what was right about Hacking’s account of the self-vindication of the laboratory sciences, even though it could not secure such self-vindicating domains from empirical challenge. New domains of phenomena, and new aspects of familiar domains, are often brought into the space of reasons by creating and refining new kinds of phenomena. As merely one example, Wilson (2006, 181) notes in passing that the phenomena made available through photographic depiction with fast film brought the distortions of seemingly rigid spheres upon impact into mechanics. The facade-like character of various theoretical models often reflects their development in response to new experimental phenomena, and vice versa. It is perhaps pushing Wilson’s metaphor
only a little too far to extend his image of theoretical models as facades bound together in an atlas by suggesting that the creation of new phenomena and the development of new models are often two sides of the same page. Yet the binding together of various mutually adapted models and phenomena cannot be overlooked, even if the models that apply a concept in new circumstances involve very different treatments only patchily overlaid with others, and shift its inferential involvements in the ways suggested in Wilson’s title, Wandering Significance.

Yet we should also further amend Hacking’s notion of phenomena, which suggests fairly static patterns of salience, to reflect the inter-connected dynamics of ongoing experimentation and model building. Hans-Jörg Rheinberger (1997), Karen Barad (2007), and I (1987, 1996, 2002, 2008, forthcoming) have each in different ways emphasized the importance of this shift. Thus far, I have spoken of the role of experimental phenomena in conceptual articulation as if experimenters merely established a significant pattern in the world, whose conceptual role was then further articulated by model building. That impression drastically oversimplifies the role of experimentation along at least two dimensions. First, we should think not of isolated experimental phenomena, but of systematically interconnected experimental capacities (what I have called experimental “micro-worlds” in Rouse 1987, and Rheinberger 1997 describes as “experimental systems”). Salient patterns revealed in experimental practice bring into the open whole domains of conceptual relationships rather than single concepts (Rouse 2008). Moreover, what matters is not a static experimental situation, but its ongoing differential reproduction, as new, potentially destabilizing elements are introduced into a relatively well-understood system. Such ongoing shifts in experimental practice also reconfigure what is at issue and at stake in its ongoing reproduction (what Rheinberger calls the “epistemic thing,” although that description offers no analog to my differentiation of issues and stakes as inner and outer recognition).

Examination of the shifting dynamics of conceptual articulation in the differential reproduction of experimental systems and theoretical model-building suggests a recognition that all scientific concepts are dappled, that is, always open to further intensive and extensive articulation in ways that may be only patchily linked together. That is not a deficiency. The supposed ideal of a completely articulated, accurate and precise conceptual understanding is in fact very far from ideal. Lange’s example of the gas laws, and the conceptual relations among temperature, pressure and volume, clearly shows why not. Neither Boyle’s nor van der Waals’s laws yield a fully accurate, general characterization of the
correlated variation of these macro-properties, or of the inferential articulation of the corresponding concepts that is needed to account for the details of those correlations. Yet there is a real pattern in the world that each law brings to the fore, despite the noise that the law cannot fully accommodate. Moreover, increasingly precise specifications of the correlations require abandoning any conceptual relationship among these properties that is applicable to gases generally; confining ourselves to such conceptual precision thus erases some important ways in which the world can be intelligibly disclosed.

The patterns already disclosed and modeled in various scientific fields are often sufficiently articulated with respect to what is at stake in those inquiries, despite extant possibilities for further extension or intensive articulation. The situations where inner recognition of those conceptual patterns might falter if pushed far enough do not matter to scientific understanding in those cases, and those divergences can be rightly set aside as noise. Often enough, such articulations remain sufficient for purposes of scientific understanding even when more refined and detailed experimental systems and theoretical models may be needed in engineering or other practical contexts. Yet at other times, seemingly marginal phenomena, such as the fine-grained edges of shadows, the indistinguishable precipitation patterns of normal and cancerous cells in the ultracentrifuge, the discrete wavelengths of photoelectric emission, or subtle cross-generational shifts in the kernel patterning of maize only recognizable to an extraordinarily skilled and prepared eye, turn out to matter in ways that require the conceptual re-organization of whole domains of inquiry. It thus remains crucial to recognize that the scope of our scientific concepts extends further and deeper than the domains in which their application can be accurately modeled, even if for the moment we rightly regard their current level of articulation as sufficient to what is at stake in our understanding them.

I conclude this discussion of the conceptual significance of experimental practice with a provocative suggestion. My distinguished commentator Bas van Fraassen (1980) some years ago proposed a dramatic reorganization of Wilfrid Sellars’ (1963) account of the manifest and scientific images. Sellars had argued that, although the scientific image of the world only emerged from within the manifest image of ourselves as persons accountable to rational norms, the scientific image eventually gains epistemic and metaphysical primacy from its explanatory power. Our understanding of ourselves must ultimately be explainable in terms compatible with the scientific image. Bas presented two central considerations that would fundamentally revise Sellars’s account of both
the scientific image and its relation to the manifest image. He first argued that we should not regard explanation as the fundamental accomplishment of the scientific image, to which the manifest image of ourselves in the world must be held to account. Explanation is instead only a pragmatic virtue that is responsive to our contextually specific questions and concerns. Second, he argued that despite the greater conceptual reach of the theories employed in the sciences, the scientific image that we ought to believe remains tethered to its rational accountability to human observation. Accepting Bas’s constructive empiricism would restore priority to the manifest image as the source of epistemic norms to which the scientific image must answer.

I think the themes developed in my talk point toward a different revision of the scientific image and its relation to our own capacities and concerns. This revised image would place conceptual articulation at the heart of the scientific enterprise. The sciences expand and reconfigure the breadth and depth of the space of reasons. At their best, they enhance and bring relevant focus to those aspects of the world that are within the scope of what we can say, reason about, and act responsively and responsibly toward. The sciences do so in part by creating phenomena, extending those phenomena beyond the laboratory, and building models that develop concepts and allow the world to show itself in inferentially relatable terms. Of course, applying concepts also requires reasoned scrutiny of what we say and do, and holding our performances and commitments accountable to evidence. Justification is more than just a dispensable virtue. Yet justification nevertheless remains contextual, and dependent upon what is at issue and at stake in various circumstances. We should not envision a general conception of empirical adequacy as the telos of scientific theorizing (in place of Sellars’s emphasis upon explanation), since empirical adequacy can be assessed at various levels of conceptual articulation, and relative to what is at issue and at stake in various settings. Empirical adequacy is also contextual and pragmatic.

Such a reconception of the scientific image also revises yet again the relation between the scientific image and our manifest image of ourselves as persons accountable to norms. The result would privilege neither a conception of nature as seemingly utterly indifferent to norms, nor a humanism that subordinates all understanding to parochial human capacities or interests. Karen Barad (2007) aptly captures such a re-conception of the outcome of the Sellarsian clash of images in the title of her own important contribution to such revision: “meeting the universe halfway.” Working out how to meet the universe halfway, by displaying our scientific and ethical understanding as both part of scientifically
articulated nature, and also as responsive to issues and stakes that are not just up to us, requires a very long story that must be reserved for another occasion. Yet I think that is ultimately what is at stake in understanding the role of experimentation in conceptual articulation.
REFERENCES


NOTES
1. There are significant counter-examples to the eclipse of attention to conceptual content, most notably Wilson (2006), but also Chang (2004), among others. Lange (2000) is ostensibly about laws, but as we shall see, it equally concerns the scope and content of concepts employed in inductive reasoning and explanation. Likewise, those philosophers who have attended to the interrelations between models and experimental practice, e.g., Hacking and Cartwright, have also at least implicitly addressed conceptual content.

2. McDowell 1984 actually invokes the figures of Scylla and Charybdis in a different context, namely what it is to follow a rule. There, Scylla is the notion that we can only follow rules by having an explicit interpretation of what the rule directs, and Charybdis is regularism, the notion that rule-following is just a blind, habitual regularity in what we do. I adapt the analogy to his later argument in McDowell 1994, with different parallels to Scylla and Charybdis, both because the form of the argument is similar, and because the figures of Scylla and Charybdis are especially apt there. It is crucial not to confuse the two contexts, however, since when McDowell 1984 talks about a “pattern,” he means a pattern of behavior supposedly in accord with a rule, whereas when I talk about “patterns” below, I mean the salient pattern of events in the world in a natural or experimental phenomenon.

3. I would call attention to the postscript to Kuhn (1970) as importantly anticipating later work on models. I think this conception of models should be kept quite distinct from the semantic account of theories developed by Suppes, van Fraassen, and others; although both emphasize the role of ‘models’, the term is used in very different senses.

4. See Galison (1997) for instructive discussion of some of these forms of data processing in experimental high energy physics.

5. The term ‘regularity’ was probably an infelicitous choice in this context, however. The philosophical prominence of this term stems from Hume’s attack on more robust conceptions of causality. Humeans locate scientific understanding in the recognition of regularities, and our habitual expectation of their continuing recurrence, precisely because they think single occurrences have no salient intelligibility. I think Hacking had a very different conception in mind, whose challenge to Humeans was nicely expressed by Nancy Cartwright:

   Once a genuine effect is achieved, that is enough. The [scientist] need not go on running the experiment again and again to lay bare a regularity before our eyes. A single case, if it is the right case, will do. (Cartwright 1989, 92)

   There are, admittedly, some phenomena for which ‘regularity’ seems more appropriate, such as the recurrent patterns of the fixed stars or the robustness of normal morphological development. The phenomenon in such cases is not the striking pattern of any of the constellations, or the specific genetic, epigenetic, and