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Laboratory Fictions

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I– Introduction

When philosophers consider fictions or fictional representations in science, we typically have in mind some canonical cases: idealized models, simulations, thought experiments, or counter-factual reasoning. The issue raised by scientific fictions may also seem straightforward. Sciences aim to discover actual structures and behaviors in the world, and to represent and understand them accurately. A role for scientific fictions provokes the question of how fictional representations, or more provocatively, misrepresentations could contribute to scientific understanding of how the world actually is.

I will address a different issue, concerning conceptual meaning and significance rather than truth or falsity. The two issues are closely connected, but it is a mistake to conflate them. Two decades ago, Nancy Cartwright collected some important essays under the provocative title, *How the Laws of Physics Lie*. Laws lie, she argued, because they do not accurately describe real situations in the world. Descriptions of actual behavior in real situations require supplementing the laws with more concrete models, *ad hoc* approximations and *ceteris paribus* provisos. Indeed, Cartwright argued that the fictional character of physical laws was analogous to literary, or more specifically, theatrical fiction; like film or theatrical productions, we might say, the genre of physical law demands its own fictive staging.

Shortly thereafter, I argued (1987, ch. 5) that Cartwright had mis-characterized the import of her concerns. Her arguments challenge the *truth* of law-statements only if their *meaning* were fixed in ways at odds with the actual use of such expressions in scientific practice. After all, the need for models, provisos, and *ad hoc* approximations to describe the actual behavior of physical systems in theoretical terms comes as no surprise to physicists. The models were integral to their education in physics, and the open-ended provisos and approximations needed to apply them was implicit in their practical grasp of the models. Thus, the “literal” interpretation of the laws that Cartwright once took to be false does not accurately express what the laws mean in scientific practice.

I would now express this point more generally. The meaning of an expression such as $F=ma$ or one of Maxwell’s equations is a normative matter, expressing a connection between the ways and circumstances in which that expression is appropriately employed in scientific practice, and the consequences that appropriately follow from its employment.¹ To that extent, of course, understanding laws and similar verbal or mathematical expressions cannot be easily disentangled from understanding the circumstances to which they apply. As Donald Davidson expressed the more general point, we thereby “erase the boundary between knowing a language and knowing our way around in the world generally” (1986, 445-446).²

¹ I adapt this two-dimensional account of conceptual meaning from Brandom 1994.

² Cartwright (1999) now recognizes that what was at issue in her concerns about whether and how laws accurately describe actual circumstances is not their truth, but their meaning (she interprets the relevant aspect of their meaning to be the scope of their application). Her revised view differs from mine in at least two crucial respects, however. First, her account is one-dimensional rather than two-dimensional: she determines the scope of laws based upon only the

Cartwright (1999) herself now makes a related point about the tradeoffs between truth and meaning. What concerns her is not the truth of laws, but their scope: which events should we actually take them to be informative about and accountable to? Moreover, she equates the scope of the laws with the scope of the concepts they employ. The laws of classical mechanics apply wherever the causal capacities that affect motions are appropriately characterized as “forces.” I am not here concerned with Cartwright’s answer to the question of which circumstances fall within the domain of the concept of “force.” I only want to insist that while questions of meaning and of truth are interconnected, they must remain distinct. We cannot ask whether a theory, a law, or any other hypothesis is true unless we have some understanding of what it says, and to which circumstances it appropriately applies.

To discuss conceptualization and meaning, I will consider a very different kind of case than the canonical examples of “scientific fictions.” I have in mind the development and exploration of what I once called laboratory “microworlds” (Rouse 1987), which Hans-Jörg Rheinberger (1997) has since characterized as “experimental systems.” I once described “microworlds” as “systems of objects constructed under known circumstances and isolated from other influences so that they can be manipulated and kept track of, ... [allowing scientists to] circumvent the complexity [with which the world more typically confronts us] by constructing artificially simplified ‘worlds’” (1987, 101). Some illustrative experimental microworlds include the Morgan group’s system for mapping genetic mutations in *Drosophila melanogaster*, the many setups in particle physics that direct a source of radiation toward a shielded target and detector, or the work with alcohols and their derivatives that marked the beginnings of experimental organic chemistry. These are not verbal, mathematical or pictorial representations of some actual or possible situation in the world. They are not even physical models, like the machine-shop assemblies that Watson and Crick manipulated to represent 3-dimensional structures for DNA. They are instead novel, reproducible arrangements of some aspect of the world.

My consideration of experimental systems in relation to the canonical scientific fictions may seem strange. Discussions of scientific fictions commonly take experimentation for granted as relatively well understood. Against this background, the question is sometimes raised whether thought experiments or computer simulations relevantly resemble experimental manipulations as “data-gathering” practices.³ I proceed in the opposite direction, asking whether and how the development of experimental systems resembles the construction and use of more canonical kinds of “scientific fiction.” The issue is not whether simulations or thought experiments can be sources of data, but whether and how laboratory work takes on a role akin to that of thought experiments in articulating and consolidating conceptual understanding. Philosophers have tended to exclude experimentation from processes of conceptual development. To caricature

circumstances of their application rather than the connection between circumstances and consequences. Second, she thinks that the only relevant circumstantial criterion is the empirical adequacy of the models that could connect law and circumstances, whereas I think empirical adequacy is only one among multiple relevant considerations. See Rouse (2002, 319-334).

³ Humphreys 1994, Hughes 1999, Norton/Suppe 2001, Winsberg 2003.

quickly a complex tradition, the logical empiricists confined experimentation to the context of justification rather than discovery; post-empiricists and scientific realists emphasized that experimentation presupposes prior theoretical articulation of concepts; while reaction to the excesses of both traditions proclaimed that experimentation has a life of its own apart from developing or testing concepts and theories. None of these traditions has said enough about experimentation as itself integral to conceptual articulation.

As background to what I will say about experimental systems, consider briefly Kuhn's classic account of the function of thought experiments. Thought experiments become important when scientists "have acquired a variety of experience which could not be assimilated by their traditional mode of dealing with the world" (1977, 264). By extending scientific concepts beyond their familiar uses, he argued, thought experiments bring about a conceptual conflict rooted in those traditional uses, rather than finding one already implicit in them. He insisted upon that distinction, because Kuhn took the meaning of concepts to be open-textured rather than fully determinate. By working out how to apply these concepts in new, unforeseen circumstances, thought experiments retrospectively transformed their use in more familiar contexts, rendering them problematic in illuminating ways.

Thought experiments could only play this role, however, if their extension to the newly imagined setting genuinely extended the original, familiar concepts. Kuhn consequently identified two constraints upon such imaginative extension of scientific concepts, "if it is to disclose a misfit between traditional conceptual apparatus and nature": first, "the imagined situation must allow the scientist to employ his usual concepts in the way he has employed them before, [not] straining normal usage" (1977, 264-265). Second, "though the imagined situation need not be even potentially realizable in nature, the conflict deduced from it must be one that nature itself could present; indeed, ... it must be one that, however unclearly seen, has confronted him before" (1977, 265). Thought experiments, that is, are jointly parasitic upon the prior employment of concepts, and the world's already-disclosed possibilities; like Davidson, Kuhn found it hard to disentangle our grasp of concepts from "knowing our way around in the world." Against that background, thought experiments articulate concepts by presenting concrete situations that display differences that are intelligibly connected to prior understanding. In Kuhn's primary example, the difference between instantaneous and average velocity only becomes conceptually salient in circumstances where comparisons of velocities in those terms diverge. My question, however, is how scientific concepts come to have a "normal usage" in the first place, acquainting us not merely with what actually happens, but providing a grasp of possibilities, of the situations "that nature itself could present."

The novel circumstances presented in experimental systems or thought experiments are important because they make salient a conceptually significant difference that does not show itself clearly in more "ordinary" circumstances. Yet experimental systems sometimes play a pivotal role in making possible the conceptual articulation of a domain of phenomena in the first place. A post-empiricist commonplace rejects any "Whig" history of science that narrates a relatively seamless transition from error to truth. Yet in many scientific domains, earlier generations of scientists could not have erred, because the possibility of error was not yet open to them. In the most striking cases, scientists' predecessors either had no basis whatsoever for making claims within a domain, or could only make vague, unarticulated claims. In Hacking's

(1984) apt distinction, they lacked not truths, but possibilities for truth or error: they had no way to reason about such claims, and thus could not articulate claims that were “true-or-false.” A distinctive feat of laboratory science, then, is to allow new aspects of the world to show up as conceptually articulable.

II– Phenomena and Conceptual Articulation

To understand how the construction of experimental systems plays such a role in conceptual articulation, consider this remark by Hacking about how scientists come to “know their way around in the world generally”:

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. (1983,226)

Hacking’s conception of phenomena is now familiar. He was talking about events in the world rather than appearances to the mind, and he argued that most phenomena were created in the laboratory rather than found in the world. Experimental work does not simply strip away confounding complexities to reveal underlying nomic simplicity; it creates new complex arrangements as indispensable background to any foregrounded simplicity. Yet most philosophical readers have not taken Hacking’s suggested parallel between phenomena and laws as modes of analysis sufficiently seriously. We tend to think only laws or theories allow us to analyze and understand nature’s complex occurrences. Creating phenomena may be an indispensable means to discerning relevant laws or constructing illuminating theories, but they can only indicate possible directions for analysis, which must be developed theoretically. I will argue, however, that it is a mistake to treat laboratory phenomena in this way as merely indicative means to the verbal or mathematical articulation of theory, even if one acknowledges that experimentation also has its own ends. Experimental practice can be integral rather than merely instrumental to theoretical understanding.

As created artifacts, laboratory phenomena and experimental systems have a distinctive aim. Most artifacts, including the apparatus within an experimental system, are used to accomplish some end. The end of an experimental system, however, is not so much what it does, as what it shows. Experimental systems are novel re-arrangements of the world that allow some aspects that are not ordinarily manifest and intelligible to *show* themselves clearly and evidently. Sometimes such arrangements isolate and shield relevant interactions or features from confounding influences. Sometimes they introduce signs or markers into the experimental field, such as radioactive isotopes, genes for antibiotic resistance, or correlated detectors for signals whose conjunction indicates events that neither signifies alone. This aspect of experimentation reverses the emphasis from traditional empiricism: what matters is not what the experimenter observes, but what the phenomenon shows.

Catherine Elgin (1991) usefully distinguishes the features or properties an experiment *exemplifies* from those that it merely *instantiates*. In her example, rotating a flashlight 90 degrees merely instantiates the constant velocity of light in different inertial reference frames,

whereas the Michelson/Morley experiment exemplifies it.⁴ Elgin thereby emphasizes the symbolic function of experimental performances, and suggests parallels between their cognitive significance and that of paintings, novels, and other artworks. A fictional character such as Nora in *A Doll's House* strikingly exemplifies a debilitating situation, which the lives of many actual women in conventional bourgeois marriages merely instantiate. Yet Elgin still distinguishes scientific experimentation from both literary and scientific fictions. An experiment actually instantiates the features it exemplifies, whereas thought experiments and computer simulations share with many artworks the exemplification of features they instantiate only metaphorically.

Elgin's distinction between actual experiments and fictional constructions gives priority to instantiation over exemplification. Nora's life is fictional, and is therefore only metaphorically constrained, whereas light within the Michelson interferometer really does travel at constant velocities in orthogonal directions. Thought experiments, computer simulations, and novels are derivative, fictional or metaphorical exemplifications, because exemplifying a conceptually articulated feature depends upon already instantiating that feature. The feature is already 'there' in the world, awaiting only the articulation of concepts that allow us to recognize it. Unexemplified and therefore unconceptualized features of the world would then be like the statue of Hermes that Aristotle thought exists potentially within a block of wood, whose emergence awaits only the sculptor's (or scientist's) trimming away of extraneous surroundings.⁵

In retrospect, with a concept clearly in our grasp (or better, with ourselves already in the grip of that concept), the presumption that the concept applies to already-extant features of the world is unassailable. Of course there were mitochondria, spiral galaxies, polypeptide chains and tectonic plates before anyone discerned them, or even conceived their possibility. Yet this retrospective standpoint, where the concepts are already articulated and the only question is where they apply, crucially mis-locates important aspects of scientific research. In Kantian terms, researchers initially seek reflective rather than determinative judgments. Scientific research must articulate concepts with which the world can be perspicuously described and understood, rather than simply apply those already available. To be sure, conceptual articulation

⁴ Although I will not belabor the point here, it is relevant to my subsequent treatment of experimental systems as "laboratory fictions" that, strictly speaking, the Michelson/Morley experiment does not instantiate the constant velocity of light in different inertial frames, since the experiment is conducted in an accelerated rather than an inertial setting.

⁵ Aristotle, *Metaphysics* IX, ch 6, 1048a. Hacking's initial discussion of the creation of phenomena criticized just this conception of phenomena as implicit or potential components of more complex circumstances:

We tend to feel [that] the phenomena revealed in the laboratory are part of God's handiwork, waiting to be discovered. Such an attitude is natural from a theory-dominated philosophy. ... Since our theories aim at what has always been true of the universe—God wrote the laws in His Book, before the beginning— it follows that the phenomena have always been there, waiting to be discovered. I suggest, in contrast, that the Hall effect does not exist outside of certain kinds of apparatus. ... The effect, at least in a pure state, can only be embodied by such devices. (Hacking 1983, 225-226)

does not begin *de novo*, but extends a prior understanding that gives indispensable guidance to inquiry. Yet in science, one typically recognizes such prior articulation as tentative and open-textured, at least in those respects that the research aims to explore.

The dissociation of experimental work from conceptual articulation reflects a tendency to think of conceptual development as primarily verbal, a matter of gaining inferential control over the relations among our words. Quine (1953, 42) encapsulated that tendency with his images of conceptual schemes as self-enclosed fabrics or fields that accommodate the impact of unconceptualized stimuli at their boundaries solely by internal adjustments in the theory. Both Donald Davidson (1984) and John McDowell (1994) have criticized the Quinean image, arguing that the conceptual domain is unbounded by anything “extra-conceptual.” I agree, yet reflection upon the history of scientific experimentation strongly suggests the inadequacy of Davidson’s and McDowell’s own distinctive ways of securing the unboundedness of the conceptual.⁶ Against Davidson, that history reminds us that conceptual articulation is not merely intra-linguistic.⁷ Against McDowell, the history of experimentation reminds us that conceptual articulation incorporates causal interaction with the world, and not just perceptual receptivity.

Both points are highlighted by examples in which experimentation opened whole new domains of conceptual articulation, where previously there was, in Hacking’s apt phrase, “just complexity.” Think of genes before the Morgan group’s correlations of crossover frequencies with variations in chromosomal cytology (Kohler 1994); of heat and temperature before the development of intercalibrated practices of thermometry (Chang 2004); of interstellar distances before Leavitt’s and Shapley’s tracking of period-luminosity relations in Cepheid variables; of the functional significance of cellular structure before the deployment of the ultracentrifuge and the electron microscope (Bechtel 1993, Rheinberger 1995); or of sub-atomic structure before Rutherford targeted gold leaf with beams of alpha particles. These features of the world were less ineffable than the “absolute, unthinkable, and undecipherable nothingness” that Hacking (1986) memorably ascribed to anachronistic human kinds. They nevertheless lacked the articulable differences needed to sustain conceptual development. What changed the situation was not just new kinds of data, or newly imagined ways of thinking about things, but new interactions that articulate the world itself differently. For example, surely almost anyone in biology prior to 1930 would have acknowledged that cellular functioning requires a fairly complex internal organization of cells in order to perform their many roles in the life of an organism. Yet such acknowledgment was inevitably vague and detached from any consequent program of research (apart from the identification of some static structures such as nuclei, cell walls, mitochondria, and a few recognized *in vitro* biochemical pathways). Without further

⁶ I have developed these criticisms of Davidson and McDowell more extensively in Rouse 2002.

⁷ Davidson himself would argue that the “triangulation” that the interpretation and articulation of concepts involves is not merely intra-linguistic. Yet he also sharply distinguishes the merely causal prompting of a belief from its rational, discursive interpretation and justification. For discussion of why his view commits him despite himself to understanding conceptual articulation as intra-linguistic, see Rouse 2002, ch. 2 and 6.

material articulation of cellular components, there was little one could say or do about the integration of cellular structure and function.

The construction of experimental microworlds thus plays a distinctive and integral role in the sciences. Heidegger, who was among the first to give philosophical priority to the activity of scientific research over the retrospective assessment of scientific knowledge, forcefully characterized the role I am attributing to some experimental systems:

The essence of research consists in the fact that knowing establishes itself as a “forging-ahead” (*Vorgehen*) within some realm of entities in nature or history. ... Forging-ahead, here, does not just mean procedure, how things are done. Every forging-ahead already requires a circumscribed domain in which it moves. And it is precisely the opening up of such a domain that is the fundamental process in research. (1950, 71; 2002, 59, translation modified)

The creation of laboratory microworlds is often indispensable to opening domains in which scientific research can proceed to articulate and understand circumscribed aspects of the world.

III– Laboratory Fictions

What does it mean to open up a scientific domain, and how are such events related to the construction of experimental systems? Consider first that experimental systems always have a broader “representational” import. It is no accident that biologists speak of the key components of their experimental systems as model organisms, and that scientists more generally speak of experimental models. The cross-breeding of mutant strains of *Drosophila* with stock breeding populations, for example, was not a phenomenon of interest for its own sake, but it was also not merely a peculiarity of this species of *Drosophila*. The *Drosophila* system was instead understood, rightly, to show something of fundamental importance about genetics more generally; indeed, as I shall argue, it was integral to the constitution of genetics as a research field.

I suggest that we think of such domain-constitutive experimental systems as “laboratory fictions.” It may strain normal usage to talk about real circumstances as “fictions,” even when their creation and refinement are unprecedented. They may seem to present rather than represent a phenomenon. Yet recall Elgin’s insistence upon the symbolic character of experiments that exemplify rather than merely instantiate their features. Domain-opening experimental systems “exemplify” in an even stronger sense, because they help constitute the conceptual space within which their features are intelligible as such.

Thought experiments and theoretical models may seem “fictional” in an especially strong sense because they display unusual, idealized, or even impossible situations. Ideal gases, two-body universes, or observers traveling alongside electromagnetic waves nicely exemplify this apparent contrast between fiction and fact. Yet while experimental systems do present possible ways the world might be, they need not be typical or representative, and need not even instantiate the features they exemplify. Consider *Drosophila melanogaster* as an experimental organism. As the preeminent model system for classical genetics, *Drosophila* was highly atypical: as a human commensal, it is relatively cosmopolitan and genetically less-diversified than alternative model organisms. More important, however, for *D. melanogaster* to function as a model system, its atypical features had to be artificially enhanced, removing much of its residual “natural”

genetic diversity from experimental breeding stocks (Kohler 1994, ch. 1, 3, 8). *Drosophila* is even more anomalous in its later incarnation as a model system for evolutionary-developmental biology. *Drosophila* is now the textbook model for the development, developmental genetics, and evolution of animal body plans (Carroll, et al. 2001, esp. ch. 2-4), and yet the long syncytial stage of *Drosophila* development is extraordinary even among Arthropods.

Two interconnected features of the experimental systems that bring an entire field of phenomena “into the open” or “into the space of reasons” lead me to conceive them as laboratory fictions. First is the *systematic* character of experimental operations. Fictions in the sense that interest me are not just any imaginative construction, but have sufficient self-enclosure and internal complexity to constitute a situation whose relevant features can be identified through their mutual interrelations. Second, fictions *constitute* their own “world.” The point is not that nothing outside the fictional construction resembles or otherwise corresponds to the situation it constructs, for many fictional constructions do have “real” settings. Even in those cases, however, they constitute a world internally rather than by external references. In this sense, we can speak of Dickens’s London much as we speak of Tolkien’s Middle Earth.

My first point, that scientific experimentation typically requires a more extensive experimental system rather than just individual experiments, is now widely recognized in the literature.⁸ Ludwik Fleck (1979) was among the first to highlight the priority of experimental systems to experiments, and most later discussions follow his concern for the justificatory importance of experimental systematicity: “To establish *proof*, an entire system of experiments and controls is needed, set up according to an assumption or style and performed by an expert” (Fleck 1970, 96, my emphasis). Hacking’s (1992) discussion of the “self-vindication” of the laboratory sciences also primarily concerned epistemic justification. The self-vindicating stability of the laboratory sciences, he argued, is achieved in part by the mutually self-referential adjustment of theories and data.

I am making a different claim: it is primarily by creating systematically intraconnected “microworlds” that new domains are opened to contentful conceptual articulation at all.⁹ “Genes,” for example, were transformed from merely hypothetical posits to the locus of a whole field of inquiry (“genetics”) by the correlation of cross-over frequencies of mutant traits with visible transformations in chromosomal cytology, in flies cross-bred to a standardized breeding

⁸ Notable defenses of the systematic character of experimental systems and traditions include Rheinberger 1997, Galison 1987, 1997, Klein 2003, Kohler 1994, Chang 2004.

⁹ Fleck, at least, was not unaware of the role of experimental systems in conceptual articulation, although he did not quite put it in those terms. One theme of his study of the Wassermann reaction was its connection to earlier vague conceptions of “syphilitic blood,” both in guiding the subsequent development of the reaction, and also thereby articulating more precisely the conceptual relations between syphilis and blood. He did not, however, explicitly connect the systematicity of experimental practice with its conceptual-articulative role. Hacking was likewise also often concerned with conceptual articulation (especially in the papers collected in Hacking 2002 and 1999), but this concern was noticeably less evident in his discussions of laboratory science (e.g., Hacking 1983, ch. 12, 16; 1992).

population. Carbon chemistry (as distinct from the phenomenological description of organically-derived materials) likewise became a domain of inquiry through the systematic, conceptually articulated tracking of ethers and other derivatives of alcohol (Klein 2003). Leyden jars and voltaic cells played similar roles for electricity. What is needed to open a novel research domain is typically the ability to create and display an intracommunity field of reliable differential effects: not merely creating phenomena, but creating an experimental practice.

My shift in concern from justification to conceptual articulation and domain-constitution correlates with a second difference from Hacking's account. Hacking sought to understand the eventual stabilization of fields of laboratory work, as resulting from the mutually self-vindicating adjustment of theories, apparatus, and skills. I consider the beginning of this process rather than its end, the *opening* of new domains for conceptual articulation rather than their eventual practical and conceptual stabilization. Yet my argument puts Hacking's account of self-vindication in a new light. We need to consider not only the self-vindicating justification of experimental work, but its scientific significance more generally. However self-referential and self-vindicating a complex of experimental phenomena and theory may be, its place within an ongoing scientific enterprise depends upon its being informative beyond the realm of its self-vindication. Hacking's emphasis upon self-vindicating stabilization thus may subtly downplay the intentional and conceptual character of experimental systems.¹⁰

To understand the intentionality of experimental systems, I turn to the second feature of "laboratory fictions," their constitutive character.¹¹ The constitution of a scientific domain accounts for the conceptual character of the distinctions functioning within the associated field of scientific work. Consider what it means to say that the *Drosophila* system developed initially in Morgan's laboratory at Columbia was about *genetics*. We need to be careful here, for we cannot presume the identity and integrity of genetics as a domain. The word 'gene' predates Morgan's work by several years, and the notion of a particulate, germ-line "unit" of heredity emerged earlier in the work of Mendel, Darwin, Weismann, Bateson and others. The conception of genes as the principal objects of study within the domain of genetics marks something distinctive, however. Prior conceptions of heredity did not and could not distinguish genes from the larger processes of organismic development within which they functioned. What the *Drosophila*

¹⁰ One might go further, in the spirit of McDowell's (1994) criticism of Davidson. On such an analogous line of criticism, Hacking's account of self-vindication would refute skeptical doubts about the epistemic justification of experimental science at the cost of losing one's grasp of the semantic contentfulness of the concepts deployed in those self-vindicating domains. If this criticism were correct, then Hacking "manages to be comfortable with his coherentism, which dispenses with rational constraint on thinking from outside it, only because he does not see that emptiness is the threat" (McDowell 1994, 68).

¹¹ John Haugeland (1999, ch. 13) suggests the locution "letting be" to explicate what we both mean by the term 'constitution'. Constitution of a domain of phenomena ("letting it be") must be distinguished from the alternative extremes of creation (e.g., by simply *stipulating* success conditions), and merely taking antecedently intelligible phenomena to "count as" something else.

system initially displayed, then, was a field of distinctively genetic phenomena, for which the differential development of organisms was part of an apparatus that articulated genes as relative chromosomal locations and characteristic patterns of meiotic crossover, as well as phenotypic outcomes.

What the *Drosophila* system thus did was to allow a much more extensive inferential articulation of the concept of a gene. *Concepts* are marked by their possible utilization in contentful judgments, which acquire their content inferentially.¹² For example, a central achievement of *Drosophila* genetics was the identification of phenotypic traits (or trait-differences) with chromosomally-located “genes.” Such judgments cannot simply be correlations between an attributed trait and what happens at a chromosomal location, because of their inferential interconnectedness. Consider the judgment in classical *Drosophila* genetics that the *Sepia* gene is not on chromosome 4.¹³ This judgment does not simply withhold assent to a specific claim; it has the more specific content that either the *Sepia* gene has some other chromosomal locus, or that no single locus can be assigned to the distinctive traits of *Sepia* mutants. Such judgments, that is, indicate a more-or-less definite space of alternatives. Yet part of the content of the “simpler” claim that *Sepia* is on chromosome 3 is the consequence that it is not on chromosome 4. Thus, any single judgment in this domain presupposes the intelligibility of an entire conceptual space of interconnected traits, loci, and genes (including the boundaries that mark out what is not a relevant constituent of that space).¹⁴

My point about the relation between scientific domains and entities disclosed within those domains can be usefully highlighted by a brief comparison to Hanna and Harrison’s (2002) discussion of the conceptual space of naming. Proper names are often regarded as the

¹² My emphasis upon inferential articulation as the definitive feature of conceptualization is strongly influenced by Brandom 1994, 2000, 2002, with some important critical adjustments (see Rouse 2002, ch. 5-7). Inferential articulation is not equivalent to linguistic expression, since conceptual distinctions can function implicitly in practice without being explicitly articulated in words at all. Scientific work is normally sufficiently self-conscious that most important conceptual distinctions are eventually marked linguistically. Yet the central point of this paper is to argue that the inferential articulation of scientific concepts must *incorporate* the systematic development of a domain of phenomena within which objects can manifest the appropriate conceptual differences. The experimental practices that open such a domain thereby make it possible to form judgments about entities and features within that domain, but the practices themselves already articulate “judgeable contents” prior to the explicit articulation of judgments.

¹³ The distinctively revealing character of negative descriptions was brought home to me by Hanna and Harrison 2004, ch. 10.

¹⁴ Classical-genetic loci within any single chromosome are especially cogent illustrations of my larger line of argument, since prior to the achievement of DNA sequencing, any given location was only identifiable by its relations to other loci on the same chromosome. The location of a gene was relative to a field of other genetic loci, which are in turn only given as relative locations.

prototypical case of linguistic expressions whose semantic significance is directly conferred by their relationship to entities bearing those names. Hanna and Harrison's counter-claim, which I endorse, is that proper names themselves refer not to persons, ships, cities, and the like, but to "nomothetic" objects ("name-bearerships") disclosed within a larger framework of practices that they identify collectively as the "Name-Tracking Network"¹⁵:

To give a name is ... to reveal, in the ordinary way of things, a label that has been used for many years, through occurrences of tokens of it in the context of many naming practices, to trace, or track, one's progress through life. Such tracking operates by way of a variety of practices: the keeping of baptismal rolls, school registers, registers of electors; the editing and publishing of works of reference of the *Who's Who* type, the inscribing of names, with attached addresses, in legal documents, certificates of birth, marriage, and death, and so on. Such practices are mutually referring in ways that turn them into a network through which the bearer of a given name may be tracked down by any of dozens of routes. (Hanna and Harrison 2002, 108)

We use names to refer to persons, cities or ships, but we can do so only via the maintenance and use of such an interconnected field of practices. Such practices constitute trackable "name-bearerships" through which names can be used *accountably* to identify human beings or other entities.

Experimental systems play a role within scientific domains comparable to the role of the Name-Tracking Network in constituting name-bearerships. They mediate the accountability of verbally articulated concepts to the world, which allows the use of those concepts to be more than just a "frictionless spinning in a void" (McDowell 1994). Morgan and Morrison (1999) have compellingly characterized theoretical models as partially autonomous *mediators* between theories and the world. I am claiming that scientific understanding is often *doubly* mediated; experimental systems mediate between the kinds of models they describe as instruments, and the circumstances to which scientific concepts ultimately apply. It is often only by the application of these models within the microworld of the experimental system that they come to have an intelligible application anywhere else. Moreover, in many cases, the experimental model comes first; it introduces relatively well-behaved circumstances that can be tractably modeled in other ways (e.g., by a *Drosophila* chromosome map).

To understand the significance of this claim, we need to ask what "well-behaved circumstances" means here. Cartwright (1999, 49-59) has introduced similar issues by talking about mediating models in physics or economics as "blueprints for nomological machines." Nomological machines are an arrangement and shielding of various components, such that their capacities reliably interact to produce regular behavior. I am expanding her conception to include not just regular behavior, but conceptually articulable behavior more generally. I nevertheless worry about the metaphors of blueprints and machines. The machine metaphor suggests an already determinate purposiveness, something the machine is a machine *for*. With

¹⁵ Hanna and Harrison sometimes seem to contrast nomothetic objects, of which chess pieces and name-bearerships are prototypical, to natural objects. By coming to recognize the "nomothetic" character of scientific domains, my argument shows the coincidence of the domains of their "nomothetic objects" and objects more generally.

purposes specified, the normative language that permeates Cartwright's discussion of nomological machines becomes straightforward: she speaks of *successful* operation, running *properly*, or arrangements that are fixed or stable *enough*. Yet where do the purposes and norms come from? That is the most basic reason to think about experimental systems as laboratory fictions mediating between theoretical models and worldly circumstances: they help articulate the norms with respect to which circumstances could be "well-behaved," and nomological machines (or experiments with them) could run "properly" or "successfully." Scientific concepts, then, both articulate and are accountable to norms of intelligibility, expressed in these notions of proper behavior and successful functioning.

For theoretical models and the concepts they employ, Cartwright and Ronald Giere (1988) have each tried to regulate their normativity in terms of their empirical adequacy, or their "resemblance" to real systems. In discussing the domain of the concept of 'force', for example, Cartwright claims that,

When we have a good-fitting molecular model for the wind, and we have in our theory ... systematic rules that assign force functions to the models, and the force functions assigned predict exactly the right motions, then we will have good scientific reason to maintain that the wind operates via a force. (1999, 28)

Giere in turn argues that theoretical models like those for a damped harmonic oscillator only directly characterize fictional, abstract entities of which the models are strictly true, whose relation to real systems is one of relevant similarity:

The notion of *similarity* between models and real systems ... immediately reveals— what talk about approximate truth conceals— that approximation has at least two dimensions: approximation in *respects*, and approximation in *degrees*. (1988, 106)

To answer my concerns, however, empirical adequacy or similarity come too late. What is at issue are the relevant respects of possible resemblance, or what differences in degree are degrees of. Such matters could be taken for granted in mechanics, which serves as the proximate example for both Cartwright's and Giere's discussions, because the relevant experimental systems have long been established and stabilized, in mutual adjustment with the relevant idealized models. The phenomena that constitute the domain of mechanics, such as pendula, springs, free-falling objects, and planetary trajectories, and their conceptual characterization, could already be presumed.

Such is not the case when scientists begin to formulate and explore a new domain of phenomena. Mendelian ratios of inheritance obviously predated Morgan's work, for example, but spatialized "linkages" between heritable traits were novel. The discovery that the white-eyed mutation was a "sex-linked" trait undoubtedly provided an anchoring point within the emerging field of mutations, much as the freezing and boiling points of water helped anchor the field of temperature differences. Yet as Hasok Chang (2004, ch. 1) has shown, these initially "familiar" phenomena could not be taken for granted; in order to serve as anchors for a conceptual space of temperature differences, the phenomena of boiling and freezing required canonical specification. Such specification required practical mastery of techniques and circumstances as much or more

than explicit definition.¹⁶ Indeed, my point is that practical and verbal articulation of the phenomena had to proceed together. Likewise with the development and refinement of the instruments through which such phenomena could become manifest, such as thermometers for temperature differences or breeding stocks for trait-linkages.

One might object that conceiving domain-constituting experimental systems as fictions renders the constitution of these domains as merely stipulative, and thus not as answerable to empirical findings. Do they merely institute normative standards for the application of a concept that are not accountable to any further normative considerations? Chang's (2004) study of the practices of thermometry shows one important reason why such norms are not merely stipulative. There are many ways of producing regular and reliable correlates to increases in heat under various circumstances. Much work went into developing mercury, alcohol, or air thermometers (not to mention their analogues in circumstances too hot or cold for these canonical thermometers). Yet it would be insufficient merely to establish a reliable, reproducible system for identifying degrees of heat (or cold) by correlating them with the thermal expansion or contraction of some canonical substance. Rather, the substantial variations in measurement among different putatively standard systems suggested a norm of temperature independent of any particular measure, however systematic and reproducible. Such a norm, if it could be coherently articulated, would introduce order into these variations by establishing a standard by which their own correctness could be assessed. That the development of a standard is itself normatively accountable is clear from the possibility of failure: perhaps there would turn out to be no coherent, systematic way to correlate the thermal expansion of different substances within a single temperature scale.

Chang (2004, 59-60) identifies the deeper issue here as “the problem of nomic measurement”: identifying some concept X (e.g., temperature) by some other phenomenon Y (e.g., thermal expansion of a canonical substance) presupposes what is supposed to be discovered empirically, namely the form of the functional relation between X and Y. But the more basic underlying issue is not the identification of the correct functional relation, but the projection of a concept to be right or wrong about in the first place. The issue therefore does not apply only to quantitative measurement. It affects non-quantitative concepts like the relative location of genes on chromosomes or the identification of functionally significant components of cells as much as it applies to measurable quantities like temperature or electrical resistance.¹⁷

¹⁶ Chang (2004) argues that in the case of state changes in water, ironically, ordinary “impurities” such as dust or dissolved air, and surface irregularities in its containers, helped *maintain* the constancy of boiling or freezing points; removing the impurities and cleaning the contact surfaces allowed water to be “supercooled” or “superheated.” My point still holds, however, that canonical circumstances needed to be defined in order to specify the relevant concept, in this case temperature.

¹⁷ One might argue that chromosomal locations in classical genetics were quantitative properties also, either because they were assessed statistically by correlations in genetic crossing over, or because spatial location is itself quantitatively articulable. Yet crossover correlations were only measures of relative location, and because locations were only identifiable through

The most dramatic display of the normativity of experimental domain constitution, however, comes when domain-constituting systems are abandoned or transformed by a constitutive failure, or a re-conceptualization of the domain. Consider the abandonment in the 1950's of the *Paramecium* system as a model organism for microbial genetics.¹⁸ *Paramecium* was dealt a double blow. Its distinctive advantages for the study of cytoplasmic inheritance became moot when the significance of supposed differences between nuclear and cytoplasmic inheritance dissolved. More important from my perspective, however, is the biochemical reconceptualization of genes through the study of auxotrophic mutants in organisms that could grow on a variable nutrient medium. Despite extensive effort, *Paramecium* would not grow on a biochemically characterizable medium, and hence could not display auxotrophic mutations. In Elgin's terms, the cytogenetic patterns in *Paramecium* could now only instantiate, but could no longer exemplify, the newly distinctive manifestations of genes.

A different kind of failure occurs when the "atypical" features of an experimental system become barriers to adequate conceptual articulation. For example, the very standardization of genetic background that made the *D. melanogaster* system the exemplary embodiment of chromosomal genetics blocked any display of population-genetic variations and its significance for evolutionary genetics; Dobzhansky had to adapt the techniques of *Drosophila* genetics to *D. pseudoobscura* in order to manifest the genetic diversity of natural populations (Kohler 1994, ch. 8). More recently, Jessica Bolker (1995) has suggested that the very features that recommended the standard model organisms as laboratory models of biological development may be systematically misleading. Laboratory work encourages using organisms with rapid development and short generations; these features in turn correlate with embryonic pre-patterning and developmental canalization. The choice of experimental systems thereby *materially* conceives development as a relatively self-contained process. The reconceptualization of development as ecologically-mediated may therefore require exemplification in different experimental practices, which will likely employ different organisms.

IV— Conclusion

I have been considering experimental systems as materialized fictional "worlds" that are integral to scientific conceptualization. The experimental practices that establish and work with such systems do not just exemplify conceptualizable features of the world, but help constitute the fields of possible judgment and the conceptual norms that allow those features to show themselves intelligibly.

The sense in which these systems are "fictional" is twofold. First, they are simplified and re-arranged as "well-behaved" circumstances that make some features of the world more readily manifest. The world as we find it is often unruly and unarticulated. By arranging and maintaining more clearly articulated and manifest differentiations, we create conditions for conceptual understanding that can then be applied to more complicated or opaque circumstances. Second,

internal relations on a specific chromosome map, I would argue that these were not yet quantitative concepts either.

¹⁸ For detailed discussion, see Nanney 1983. Sapp (1987) sets this episode in the larger context of debates over cytoplasmic inheritance.

these artificially regulated circumstances and the differentiations they manifest are sufficiently interconnected to allow the differences that they articulate to be systematically interconnected. The resulting interconnections, if they can be coherently sustained and applied to new contexts and new issues, demarcate norms for conceptual intelligibility rather than merely isolated, contingent correlations. What might otherwise be merely localized empirical curiosities instead become scientifically significant because they allow those situations to manifest an intelligibly interconnected domain of conceptual relationships. By articulating relevant conceptual norms, experimental systems join theoretical models in doubly mediating between scientific theory and the world it thereby makes comprehensible.

To talk about laboratory “fictions” in this way does not challenge or compromise a scientific commitment to truth. On the contrary, these “fictional” constructions help establish norms according to which new truths can be articulated (and, correspondingly, erroneous understanding can be recognized as such). Moreover, as I have argued, such “fictional” constitution of norms is not stipulative or voluntaristic, but is instead itself normatively accountable to the world. Conceptual articulation through experimental practice is vulnerable to empirical failure, but such failures are manifest in conceptual confusion or incoherence rather than falsehood: in Hacking’s (1984) terms, they challenge the “truth-or-falsity” of claims made using those concepts, rather than their truth. Haugeland (1997, ch. 12) has rightly emphasized that such normative accountability in science requires both resilient and reliable skill (that allows scientists to cope with apparent violations of the resulting conceptual norms, by showing how they are *merely* apparent violations), and a resolute willingness to revise or abandon those norms if such empirical conflicts cannot be adequately resolved. Yet these crucial skills and attitudes cannot come into play unless and until there is a conceptually articulated domain of phenomena toward which scientists can be resilient and resolute. Laboratory fictions help open and sustain such conceptual domains, and thereby help make the attainment of scientific truth and the recognition of error possible.

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