Knowing Nature Without Mirrors:
Thomas Kuhn's Antirepresentationalist Objectivity

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"The picture which holds traditional philosophy captive is that of the mind as a great mirror."

– Richard Rorty,
Philosophy and the Mirror of Nature, p. 12

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Objective truth has traditionally been seen as the value or interest free correspondence of thought with the immutable world. Radically separated from this world since Descartes' *cogito*, the mind has been seen as a mirror containing knowledge that represents the world. Thomas Kuhn destroys the traditional positivistic view of science that rests, at bottom, on the traditional, Cartesian representationalist and dualistic epistemological paradigm and replaces it with an antirepresentationalist and evolutionary picture of science involving ruptures, different worlds and paradigms. While Kuhn's paradigm-based view of science is incompatible with the traditional *representationalist* conception of objectivity just mentioned, it does not preclude a meaningful notion of objectivity in which objects are independent of theory to the extent that they can conceivably "bring the theory down" or compel its restructuring. We will show that Kuhn's views do not, therefore, entail the reduction of objectivity to solidarity or of truth to consensus, as Richard Rorty would have it. Rather, Kuhn's conception of science *enables* a tenable and textured notion of antirepresentationalist objectivity and this essay aims to bring this largely implicit notion of objectivity out. With the help of John Haugeland's work, we will find that Kuhn provides a conception of objectivity that helps us move beyond a broken epistemological tradition.

>> ⬛1⬛ | | | The criterion for objectivity and the inadequacy of representationalism

On Kuhn's account, science is paradigm-based and therefore consists of both normal, cumulative periods and revolutionary ruptures that entail the reconstruction of previous theories from fundamentals. Subsequent paradigms may build on each other, but the movement between them consists not in the cumulative addition of facts but rather in a change in worldview in which some of the previous theory's basic tenets are altered. Thus, Einsteinian relativity is not simply a cumulative addition to Newtonian mechanics but a fundamental change in worldview; the move

between the two paradigms involved a "paradigm shift"[1]. Such a view departs from the positivistic

picture in which science develops linearly, teleologically and cumulatively by the piecemeal addition

of unvalued facts to a monolithic structure that increasingly accurately *represents* the immutable reality

outside the mind. For Kuhn, while subsequent theories better enable the solution of puzzles that

arise as importantly unsolvable in preceding theories, there is no theory-independent, neutral

language of arbitration that can be used to decide which theory is closer to the truth "out there." In

this way, two competing theories are incommensurable because they cannot be completely translated

into each other or into a third language without residue or loss and, thus, lack a common measure.

The scientist does not aim at producing fundamental novelties but rather solves the puzzles that

arise as pertinent against the background of the paradigm. The paradigm is the background against

which certain problems arise as puzzles needing to be solved and certain solutions become

acceptable. Most novelties or anomalies that arise in observation are disregarded and, as Kuhn says,

"only as experiment and tentative theory are together articulated to a match does the discovery

emerge and the theory become a paradigm"[2]. As we will see, Kuhn's evolutionary view of scientific

development exposes the problems of the Cartesian and positivist epistemological model that posits

an absolute split between given data, facts, and their man-made, value-contaminated interpretations,

theories[3]. Though Kuhn "find[s] it impossible to relinquish entirely this viewpoint" (Kuhn 1996/62,

126) in his early writing, he notes that "it no longer functions effectively" (Kuhn 1996/62, 126) and,

as we will see, his Darwinian conception of science moves beyond the Cartesian model.

Experiments are always a looking *for* something articulated prior to them in a hypothesis and they

therefore provide observations that are always already interpreted or valued in some way.

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[1] This incredibly vague term will not be used again in this paper.

[2] Thomas S. Kuhn, *The Structure of Scientific Revolutions,* 3E (Chicago: University of Chicago Press, 1996/first edition 1962), 61. Subsequent references to this text will be in the body of the paper and of the form (Kuhn 1996/62, [*pg. #*]).

[3] For more on the history of the fact-theory split, see Mary Poovey, *A History of the Modern Fact: Problems of Knowledge in the Sciences of Wealth and Society* (Chicago: University of Chicago Press, 1998), 8.

It might seem that scientific progress and objectivity necessarily disappear if theory choice is not seen as a neutrally rational process of judging which theory is closer to a human-independent reality and if, as Kuhn claims, "when paradigms change, the world itself changes with them" (Kuhn 1996/62, 111). Indeed, Kuhn's talk of world changes has prompted accusations that he relativizes and idealizes science and renders theory choice irrational, claims he rightly denies. Such accusations are predicated on the association of objectivity with theory-independence. If objectivity is seen as that which puts us in touch with the radically extra-human or extra-theoretical, Kuhn's picture does preclude it. However, while accepting Kuhn's incommensurability thesis does entail the denial of the traditional correspondence theory of truth and its correlative representationalist notion of objectivity, this representationalism is part of a broken epistemological paradigm. If theory and objective fact are radically separated and the former is said to *represent* the latter, it is unclear how objects affect their human-made representations and, thus, how idealism can be avoided. Indeed, if Reality is fixed and closed apart from the mind or any human involvement, our representations of it will always be distanced from it in a way that would seem to preclude objective testing. Furthermore, without a theory-neutral language—something Kuhn rightly argues does not and cannot exist—one theory cannot be judged to better represent Reality than any other theory.

As we have seen, Kuhn expresses as early as *The Structure of Scientific Revolutions* (*Structure*) that "the epistemological viewpoint that has most often guided Western philosophy for three centuries…no longer functions effectively" (Kuhn 1996/62, 126); he also writes in that book that he "find[s] it impossible to relinquish entirely that viewpoint" (Kuhn 1996/62, 126). The viewpoint in question originates with Descartes' ontological separation of mind from world and, consequently, fact from theory, object from subject, and body from mind. The corresponding epistemology is representationalism, which is the idea that the mind gets at truth by correctly representing the world with thought, as we have said. Since the first edition of *Structure* appeared in 1962, Kuhn deals, most

often implicitly, with the problem of "relinquishing entirely that viewpoint," which "no longer functions effectively." As we will see, his further elaboration of the concept of a paradigm as, in part, a lexicon and his emphasis on the various parallels between Darwinian evolution and the development and practice of science provide, at the very least, a foundation from which to build a notion of objectivity consistent with Kuhn's antirepresentationalism.

Kuhn turns in an antirepresentationalist direction in *Structure* and some of his early essays by expressing his misgivings with Cartesian epistemology, developing his (Kuhn's) notion of science as paradigm-based and arguing that science is learned and the scientist is acculturated into a paradigm through doing and studying exemplary problems rather than memorizing explicit correspondence criteria, which do not exist in most cases and could never be exhaustive. All of these ideas suggest that science is not a neutral, valueless, monolithic structure that can simply be learned by memorizing explicit facts. Rather, Kuhn's notions indicate that science is inextricably linked with its human practitioners, not simply in the obvious sense that scientific achievements cannot be made without scientists, but in the sense that the practice and results of science are valued. In other words, the knowledge science provides is not human-independent. Mind and world do not "correspond" to each other because they are always already inextricably connected. Thus, a paradigm does not correspond to an immutable Reality but is rather that through which the world shows up *as* something for scientists.

The crux of Kuhn's implicit notion of objectivity lies, as we will see, in what seems to most preclude it: his "different worlds" thesis, the idea that "when paradigms change, the world itself changes with them" (Kuhn 1996/62, 111). Kuhn explicitly rejects the claim that his different worlds thesis leads to an idealist construction or determination of the world by the mind, at one point

calling himself an "unregenerate realist"[4]. Though he never gives up his talk of different worlds, he does, as we will see, change the way he explains it. Rorty, a champion of most of Kuhn's ideas, does fault him for his talk of different worlds, claiming that it is an indication of his failure to abandon his "epistemological project"[5] in which the only way to avoid Cartesian realist dualism is to veer towards idealism. Rorty rightly argues that the world—construed as the given, immutable world—does not make beliefs true, that "'true' does not name a relation between discourse and the world"[6]. Indeed, the idea that the world makes our beliefs true is predicated on a representationalist or correspondence view of reality in which the purpose of our beliefs is to mirror the given, true reality. Whether or not Rorty knows it, Kuhn, antirepresentationalist that he is or at least becomes, would agree with Rorty's statement that

> "neither does thought determine reality nor, in the sense intended by the realist, does reality determine thought. More precisely, it is no truer that 'atoms are what they are because we use "atom" as we do' than that 'we use "atom" as we do because atoms are the way they are.' *Both* of these claims, the antirepresentationalist says, are entirely empty. Both are pseudo-explanations"[7].

Rorty acknowledges that the environment shapes our language and ourselves, but denies that the utility of a scientific term such as "atom" is explainable by its correspondence to what is really out there (Rorty 1991a, 5).

Rorty's brand of antirepresentationalism does, however, part ways with Kuhn's in its complete disposal of objectivity and truth. Rorty reduces objectivity to solidarity[8] and trivializes the

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[4] Kuhn, "Metaphor in Science" (1979) in *The Road Since Structure*, 203. Subsequent references to this text will be in the body of the paper and of the form (Kuhn 1979, [*pg. #*]).

[5] Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton, NJ: Princeton University Press, 1979), 324. Subsequent references to this text will be in the body of the paper and of the form (Rorty 1979, [*pg. #*]).

[6] Rorty, "Is Natural Science a Natural Kind?" (1988), in *Objectivity, relativism, and truth: Philosophical papers, Volume 1* (Cambridge: Cambridge University Press, 1991), 50. Subsequent references to this text will be in the body of the paper and of the form (Rorty 1988, [*pg. #*]).

[7] Rorty, "Introduction: Antirepresentationalism, ethnocentrism, and liberalism," in *Objectivity, relativism, and truth* (1991), 5. Subsequent references to this text will be in the body of the paper and of the form (Rorty 1991a, [*pg. #*]).

[8] Rorty, "Solidarity or Objectivity?" (1985), in *Objectivity, relativism, and truth*, 22. Subsequent references to this text will be in the body of the paper and of the form (Rorty 1985, [*pg. #*]).

notion of truth (Rorty 1988, 50) in a way that Kuhn does not. Thus, for Rorty, objective truth is nothing more than the upshot of rational discourse, where "rational" is understood as free and open and involving "tolerance, respect for the opinions of those around one, willingness to listen, reliance on persuasion rather than force"[9]. On Rorty's view, "our choice of elements will be dictated by our understanding of the practice, rather than the practice's being 'legitimated' by a 'rational reconstruction' out of elements" (Rorty 1979, 319). As we will see, such an emphasis on the primacy of practice as opposed to the accuracy of representations does not preclude a meaningful notion of *antirepresentationalist* objectivity. Rorty wrongfully assumes that Kuhn's talk of different worlds is a representationalist residue when, as becomes clearer in Kuhn's later writing, it indicates the irreconcilably different kinds that populate the experiences of people working in incommensurable paradigms or disciplinary matrices (of which more later). In extricating himself from accusations of relativism, Rorty identifies three meanings of the term "relativism:"

> "The first is the view that every belief is as good as every other. The second is the view that 'true' is an equivocal term, having as many meanings as there are procedures of justification. The third is the view that there is nothing to be said about either truth or rationality apart from descriptions of the familiar procedures of justification which a given society—*ours*— uses in one or another area of inquiry" (Rorty 1985, 23).

While Kuhn would certainly join Rorty in renouncing the first two views, Kuhn's views indicate a way of making the third view—the one Rorty accepts—more robust by giving it notions of objectivity and truth that, while being antirepresentationalist, have more critical bite than Rortian solidarity and consensus.

We must identify what the criterion for objectivity is so we can see if Kuhn's ideas meet it. Any notion of objectivity must be able to account for the independence of objects, the fact that they exceed the theory that discloses them and can, thus, prompt this theory's restructuring. To say that objects "exceed" the theory that discloses them is only to say that anomalies may arise in

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[9] Rorty, "Science as Solidarity" (1987), in *Objectivity, Relativism, and Truth*, 37.

observations of these objects that the theory cannot render intelligible. Thus, any conception of objectivity must be able to account for the possibility of objects "bringing down the system," so to speak. Our criterion for objectivity has nothing to do with a thought's correct correspondence with reality. Indeed, the idea that objects can bring down the system would seem inexplicable in representationalist terms since objects are radically separated from the theoretical "systems" that represent them. Our investigation of Kuhn's thought will reveal an implicit antirepresentationalist notion of objectivity that better accounts for the ability of objects to bring down the system than does a representationalist account. To bring out Kuhn's notion of objectivity, we need to explore in detail a number of his concepts—namely, paradigms, incommensurability, lexicons and his evolutionary view of science. We will start by elucidating his notion of a paradigm.

>> 2 | | | | Paradigms as disciplinary matrices and exemplars

Though the term "paradigm" is now about as vague as it is ubiquitous,[10] in later essays Kuhn adds considerable nuance to his rather confused discussion of the term in *Structure*. Kuhn distinguishes two meanings of the term paradigm, the one a subset of the other. Though its possession does not make a community scientific, a scientific community—i.e. the practitioners of a scientific specialty—must possess a disciplinary matrix. A disciplinary matrix, the more expansive of the two meanings of "paradigm" alluded to above, is, most broadly, the set of explicit or tacit commitments that the community members share. Community members agree, among other things, on the problems to be solved, the data to be sought and the meaning of the data as it relates

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[10] Kuhn blames the term's hyperbolic misappropriation on his own loose usage of the term in *Structure*. Indeed, Margaret Masterman claims that he uses the term in at least twenty-one different ways in the fewer than 200 pages that comprise that book. See Margaret Masterman, "The Nature of a Paradigm," in *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, London 1965,* vol. 4, ed. Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970), 59-89. See pp. 61-5 for the twenty-one uses Masterman identifies.

to these commonly accepted problems. More generally, community members share a worldview, which allows for relatively complete communication concerning matters within the community's scope. Members do not talk past each other because they are all on the same page conceptually, but communication *between* communities, though possible, requires a certain hermeneutic approach, or as Sharrock and Read put it in their book on Kuhn, "the work of attuning oneself *to a different sensibility*"[11]. This sensibility is essentially a scientific community's disciplinary matrix.

Kuhn distinguishes four important types of shared commitments that make up part—he does not make an exhaustive list—of the disciplinary matrix. First, there are *symbolic generalizations* such as Newton's second law, F=ma, which may be articulated in logical or mathematical form. These formal generalizations are not directly applied to scientific problems and say almost nothing by themselves, as we will see. Second, *models* provide both the heuristic analogies (e.g. light behaves *like* waves) and the metaphysical commitments (e.g. "all perceptible phenomena are due to the motion and interaction of qualitatively neutral atoms in the void"[12]) of the group (Kuhn 1974, 297-8). Thus, models determine which puzzles are important to solve and what the acceptable puzzle-solutions might be[13]. Third, there are *values* such as accuracy, scope, precision, simplicity and consistency that relate to the ultimate goal of normal science, which Kuhn sees as puzzle-solving[14]. Scientists of competing schools appeal to these values in choosing between competing theories. These values are typically shared across scientific disciplines but are applied in different ways by different scientists at different times. Kuhn departs from his predecessors in the philosophy of

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[11] Wes Sharrock and Rupert Read, *Kuhn: Philosopher of Scientific Revolution* (Cambridge: Polity Press, 2002), 151. Subsequent references to this text will be in the body of the paper and of the form (S&R [*pg. #*]).

[12] Kuhn, "Second Thoughts on Paradigms" (1974), in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), 298. Subsequent references to this text will be in the body of the paper and of the form (Kuhn 1974, [*pg. #*]).

[13] Kuhn, "Postscript-1969," in *The Structure of Scientific Revolutions*, 3E, 184. Subsequent references to this text will be in the body of the paper and of the form (Kuhn 1969, [*pg. #*]).

[14] Kuhn, "Afterwords" (1993), in *The Road Since Structure: Philosophical Essays, 1970-1993, with an Autobiographical Interview*, ed. James Conant and John Haugeland (Chicago: University of Chicago Press, 2000), 251. Subsequent references to the essay will be in the body of the paper and of the form (Kuhn 1993, [*pg. #*]).

science by arguing that these individual differences in value application, differences he attributes to individuals' personalities, their past experience as scientists and to broader social trends outside science[15], are irreducible to a universal algorithm of theory choice. As Rorty says in reference to this discussion on Kuhn, "the trade-offs between satisfaction of these various criteria provide room for endless rational debate" (Rorty 1979, 327). Fourth, *exemplars* are "concrete problem solutions, accepted by the group as, in a quite usual sense, paradigmatic" (Kuhn 1974, 298). The notion of an exemplar is the second meaning of the term paradigm—the first being that of a disciplinary matrix—and it is what Kuhn most often means to invoke by "paradigm."

As mentioned above, the formal symbolic generalizations shared by members of a scientific community are not applied directly to the puzzles of science. Rather, the concrete puzzles of science are solved by applying specific expressions of these generalizations that fit the situations. Kuhn argues that application criteria or correspondence rules articulating the necessary and sufficient conditions for the application of certain concrete expressions of a given symbolic generalization are rarely encountered in science, perhaps because it is unlikely that criteria could be developed that would be "nearly sufficient in number or force to account for the actual correlations between formalism and experiment made regularly and unproblematically by members of the group" (Kuhn 1974, 303). Analogously, as Kuhn says, a language metaphor "neither presupposes nor supplies a list of the respects in which the subjects juxtaposed by the metaphor are similar" (Kuhn 1979, 197). Criteria may be developed to explain past applications, but these criteria may not be able to account for future applications.

As should become clearer later, the idea of having a complete list of criteria determining how theoretical generalizations relate to the world forecloses the possibility of objective incorrectness. If

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[15] Kuhn, "Objectivity, Value Judgment, and Theory Choice" (1973), in *The Essential Tension*, 325. Subsequent reference to this text will be in the body of the paper and of the form (Kuhn 1973, [*pg. #*]).

the way theory is applied is always determined in advance by a list of correspondence criteria, it is impossible for objects to exceed themselves, that is, for objects to bring down the system by arising anomalously. The existence of an exhaustive list of correspondence criteria would seem to imply that if one has applied a formal generalization correctly with reference to pre-established criteria, the result must be correct. However, we know that scientific theory is occasionally restructured or changed in light of results that do not fit. In order to account for the induction of theoretical restructuring, the attempt to explain scientific practice by appealing to correspondence criteria must be abandoned. Indeed, as Kuhn says, scientists rarely appeal to such criteria in their practice.

The ability of a scientist to appropriately apply formal generalizations unproblematically in the absence of explicit criteria indicates that the scientist shares the scientific community's "commitments." Members of a scientific community share a set of commitments—the disciplinary matrix—in a way analogous to the way organisms are "committed" to their biological niche: organisms see their *niche* as the *world* in which they live. In other words, the scientist sees the world in a certain way and, Kuhn argues, the way scientists are acculturated into their community's worldview is largely through doing textbook and laboratory problems, which are the exemplars or paradigmatic problems of the discipline. Continuing with the analogy to ordinary language metaphors, rather than providing a list of respects in which two subjects are similar, Kuhn says, "it is sometimes (perhaps always) revealing to view metaphor as creating or calling forth the similarities upon which its function depends" (Kuhn 1979, 197). As we have said, a symbolic generalization such as F=ma is rather useless on its own and translating it from "symbolese" into English as "force is equal to the product of mass and acceleration" does nothing to help. Rendering F=ma meaningful is not a simple matter of defining "force," "mass" and "acceleration." These terms are only truly salient from within the disciplinary matrix since they have meaning only in the interrelated web of formal generalizations and concrete applications that use them. Indeed, "mass" does not

mean the same thing in Newtonian as in Einsteinian physics since it is defined in terms of a different web of connections. As Kuhn notes, scientists don't learn to use terms like "mass" or, for that matter, "fish," by "acquiring a list of criteria necessary and sufficient to determine the referents of the corresponding terms" (Kuhn 1979, 197). Learning to apply formal generalizations to nature is not a matter of applying prior explicit criteria to a given case, but rather a matter of a more tacit knowledge that is the product, Kuhn argues, of working out exemplary problems. It is to this discussion that we now turn.

In doing an exemplary problem, the student of a particular scientific discipline, "discovers," as Kuhn puts it, "a way to see his problem as like a problem he has already encountered. Once the likeness or analogy has been seen, only manipulative difficulties remain" (Kuhn 1974, 305). Through doing problems, therefore, the student learns how to make analogies and, Kuhn suggests, this "acquired ability to see resemblances between apparently disparate problems plays in the sciences a significant part of the role usually attributed to correspondence rules" (Kuhn 1974, 306). In this way, the student learns things through doing exemplary problems that she does not learn by memorizing formal equations. In John Haugeland's parlance, she learns (acquires the skills) to consistently and resiliently follow the rules that constitute or prescribe what happens in the world of her scientific community[16] (we will return to this line of thought in our discussion of Haugeland below). Much like ordinary language metaphors, exemplary problems begin to call forth connections for the student that can only be made explicit *ex post facto*. The grouping of the world that is learned through the initial doing of exemplary problems becomes tacit and inseparable from perception so that members of a scientific community do not have to ask with respect to what two

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[16] John Haugeland, *Having Thought* (Cambridge, Mass.: Harvard University Press, 1998), 322. Subsequent references to this text will be in the body of the paper and of the form (Haugeland [*pg. #*]).

things are similar because no correspondence rule is needed (Kuhn 1974, 307)[17]. As Kuhn says, for

a while, "doing problems is learning consequential things about nature. In the absence of such

exemplars, the laws and theories he [the student] has previously learned would have little empirical

content" (Kuhn 1969, 188). In other words, in doing exemplary problems and thereby attuning

herself to see the world as the members of a given scientific community do, a student is learning

about nature. Thus, for Kuhn, the scientist's view is not relative to her solipsistic world, but is

rather accountable to *nature*. The nature to which the scientist is accountable is constituted by her

worldview, but this fact enables rather than precludes the production of objective knowledge about

nature, as we will see. Given the preceding characterization of an exemplar, Kuhn claims that

paradigms are "most fundamentally…accepted concrete examples of scientific achievements, actual

problem solutions which scientists study with care and upon which they model their own work"[18].

>> 3 | | | Incommensurability

With the preceding characterization of paradigms as disciplinary matrices and exemplars we

are now able to understand Kuhn's notion of incommensurability. The term "incommensurability"

has its origin in ancient Greek geometry and indicates the lack of a common measure for two

magnitudes. For Kuhn (as for Paul Feyerabend), different paradigms or disciplinary matrices,

whether separated by time or disciplinary specialty, are incommensurable in that "there is no

language, neutral or otherwise, into which both theories, conceived as sets of sentences, can be

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[17] The learning of science, the acculturation into a scientific discipline, is inherently disciplinary in the regulatory, pedagogical or developmental sense. It is not a matter of simple memorization, but attenuation, through the exercise of working out exemplary problems, to the world of the discipline. We will have more to say about this constituted "world of the discipline" later.

[18] Kuhn, "Comment on the Relations of Science and Art" (1966), in *The Essential Tension*, 351.

translated without residue or loss"[19]. Such a claim goes against the represenationalist view that

scientific development is marked by the closer and closer correspondence of scientific knowledge

with reality; there is no way to step outside of theory and judge whether one theory is closer to

reality than is another. Kuhn claims that a theory-independent language for comparing paradigms

has been sought since the modern (Enlightenment) separation of neutral sense data from their

human or valued interpretations all the way through the work of Karl Popper, Kuhn's most

illustrious and immediate predecessor in the philosophy of science. Although the search for "a

primitive vocabulary…consist[ing] of pure sense-data terms plus syntactic connectives"[20] had been

abandoned by Popper's time, it was still assumed that "theories [could] be compared by recourse to

a basic vocabulary consisting entirely of words which are attached to nature in ways that are

unproblematic and, to the extent necessary, independent of theory" (Kuhn 1970, 162). Kuhn's

incommensurability thesis comes from his claim that no such theory-independent language exists,

that all scientific discussion proceeds from a disciplinary matrix.

Kuhn's incommensurability thesis implies that translation from the language of one

paradigm into that of another is necessarily imperfect. The terms of a scientific lexicon are even

more strictly interdefined than are those of ordinary language (S&R 169) as our discussion of kind

terms below indicates and, as our discussion of exemplars shows, certain terms must be learned in

groups rather than learned individually with explicit correspondence criteria or definitions. Kuhn

provides a helpful example:

> "In learning Newtonian mechanics, the terms 'mass' and 'force' must be acquired together,
> and Newton's second law must play a role in their acquisition. One cannot, that is, learn
> 'mass' and 'force' independently and then empirically discover that force equals mass times

[19] Kuhn, "Commensurability, Comparability, Communicability" (1982) in *The Road Since Structure*, 36. Subsequent references to this text will be in the body of the paper and of the form (Kuhn 1982, [*pg. #*]).

[20] Kuhn, "Reflections on My Critics" (1970), in *The Road Since Structure*, 162. This essay is a response to the critiques of Kuhn's work given at the International Colloquium in the Philosophy of Science, London, 1965. The colloquium's proceedings are compiled in *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave, referenced above (p. 7, n. 9). Subsequent references to Kuhn's essay will be in the body of the paper and of the form (Kuhn 1970, [*pg. #*]).

acceleration. Nor can one first learn 'mass' (or 'force') and then use it to define 'force' (or 'mass') with the aid of the second law. Instead, all three must be learned together" (Kuhn 1982, 44).

Thus, contrary to the view of science as the linear and piecemeal accumulation of knowledge, as W. V. O. Quine puts it, "re-evaluation of some statements [in total science or, more generally, the totality of our knowledge] entails re-evaluation of others, because of their logical interconnections"[21]. Because of this interconnectivity, for instance, the physical referent of the Einsteinian concept of mass is not equivalent to the referent of the Newtonian concept of mass and, as Kuhn says, "only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same" (Kuhn 1996/62, 102). Newtonian mechanics is not a special case of Einstein's; rather, the two disciplinary matrices are incommensurable. Not only do a Newtonian scientist and an Einsteinian scientist mean different things by the term "mass," but there is no scientific lexicon that contains *both* of these meanings of mass. For this reason, it would be impossible to compare the two terms with reality simultaneously. In order to do science with either of these terms, one must be acculturated into one of the two communities so that one actually *sees* mass in nature as one's fellow community members do and is thus able to use it without necessarily having an explicit definition of it. As our discussion of exemplars shows, one cannot jump in and out of a disciplinary matrix at will by learning or dispensing with its definitions. The disciplinary matrix is one's tacit knowledge of the world; it is, as we will see, what allows the world to show up for us. Incommensurable disciplinary matrices do not admit point-for-point translation. Thus, there is no way for an Einsteinian physicist to understand mass the way a Newtonian one did without learning to *use* a web of interconnected and interdefined terms to structure nature in a fundamentally different way than he does currently. There is no concept in Einsteinian physics that is equivalent to Newtonian mass. Thus, communication between paradigms is necessarily incomplete.

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[21] Willard Van Orman Quine, "Two Dogmas of Empiricism," *The Philosophical Review* 60, No 1, (January 1951), 20-43, 39.

If complete translation between paradigms is impossible theory choice might seem to be irrational. For Kuhn, "there is no neutral algorithm for theory-choice" (Kuhn 1969, 200) since the debate over theory-choice—i.e. what happens during a scientific revolution—"is about premises, and its recourse is to persuasion as a prelude to the possibility of proof" (Kuhn 1969, 199). Thus, theory-choice is not a simple matter of logical deduction because complete communication between the competing theories is impossible due to their incommensurability. As Kuhn says, "the proponents of competing paradigms are always at least slightly at cross-purposes. Neither side will grant all the non-empirical assumptions that the other needs in order to make its case" (Kuhn 1996/62, 148).

Far from rendering theory choice irrational, however, scientific values such as accuracy, broadness of scope, simplicity, consistency and fruitfulness provide good reasons to choose one theory over another. The idea that values only contaminate rationality is, as we will see, an untenable residue of representationalist epistemology. That these scientific values may be applied differently means that theory choice is not a matter of logical deduction, but these values do ensure that the new theory is better than its predecessor for solving the puzzles the group deems important, and puzzle solving is what "normal" science is about for Kuhn[22]. Though individuals in the community differ in how they apply these values, the community is the fundamental unit of theory choice (Kuhn 1970, 134). This does not mean that, as some of Kuhn's critics worry, he is arguing for "mob-psychology" in the sciences. Indeed, as Kuhn says, the values individual scientists share are also the values of the community, so the community's values cannot be said to bulldoze or

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[22] Saying that science is, by and large, "normal" science or puzzle-solving is simply to say that scientists pursue specific research questions or puzzles and aim simply to solve them and thus flesh out the accepted theory. As Kuhn says, "Normal science does not aim at novelties of fact or theory" (Kuhn 1996/62, 52) and, as Sharrock and Read point out, "the best way to make a fundamental contribution is to pursue normal science" (S&R 114). It is only with such focused work that the anomalies that prompt theory change may arise. This theory change is made with reference to scientific values and, thus, is more a conversion and re-acculturation than a choice for individual scientists (Kuhn 1973, 338). Insofar as it is fruitful, the new theory should "disclose new phenomena or previously unnoted relationships among those already known" (Kuhn 1973, 322).

change individual values as they would in mob-psychology. Furthermore, how these values are applied in theory choice is a function of the *particular* scientific community of experts, not of the masses (Kuhn 1970, 158).

>> 4 ||| Kuhn's evolutionary picture of scientific development

That scientific theories are characterized in terms of incommensurable disciplinary matrices and that theory choice is made in terms of scientific values that cannot be reduced to a neutral algorithm leads Kuhn to an evolutionary picture of scientific development that runs counter to the traditional epistemological view that sees science progressing linearly towards Truth by the piecemeal accumulation of value-neutral or theory-neutral facts. Kuhn models science on Darwinian evolution in three respects that will help us bring out his notion of objectivity. For one, scientific revolutions result in the specialization and speciation of disciplines. In other words, subsequent disciplinary matrices tend to be more and more specialized since they respond to only a few of the problems that arose in their predecessors as important but unsolvable. With this specialization comes a proliferation of scientific specialties that preserves science's breadth. In this way, science progresses in its ability to say more about the world. Second, science is evolutionary in that, as Kuhn says, "it must be seen as a process driven from behind, not pulled from ahead—as evolution from, rather than evolution towards"[23]. The idea here is that there is no immutable, predetermined, given, nonhuman truth out there to which science is pulled. Scientific development is better modeled by Darwin's theory of natural selection in which "no intelligence, divine or otherwise determines in advance the relative value of individual variations, and there is no ideal type

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[23] Kuhn, "The Road Since *Structure*" (1990), in *The Road Since Structure*, 96. Subsequent references to this text will be in the body of the paper and of the form (Kuhn 1990, [*pg. #*]).

of 'finch,' or essence of 'finchness,' towards which adaptive changes are leading"[24]. There is no

language that could serve as a yardstick for measuring the relative proximity of competing theories

to this static goal. Furthermore, this goal is not only unknown but, we might say, by virtue of its

being completely independent of the human mind attempting to know it, completely unknow*able*.

The third way in which science is evolutionary is that members of a scientific community relate to

their world or their part of the world as organisms relate to their biological niche. A discussion of

this last point, which is the most pertinent aspect of Kuhn's evolutionary analogy for our purposes,

must be deferred until later.

Kuhn's idea that scientific development is evolutionary and not teleological means that there

is no sense in which successive scientific theories are better approximations of what is really "out

there" (Kuhn 1970, 160). As Kuhn says, "comparison of historical theories gives no sense that their

ontologies are approaching a limit" (Kuhn 1970, 161). Kuhn writes,

> "I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that
> Einstein's improves on Newton's as instruments for puzzle-solving. But I can see in their
> succession no coherent direction of ontological development. On the contrary, in some
> important respects, though by no means all, Einstein's general theory of relativity is closer to
> Aristotle's than either of them is to Newton's" (Kuhn 1969, 206-7).

That is not to say that successive theories do not build on each other, but it is to deny that this

building is linear. As Kuhn puts it, "each stage in the evolution of a given field [is] built—not quite

squarely—upon its predecessor, the earlier stage providing the problems, the data, and most of the

concepts prerequisite to the emergence of the stage that follow[s]" (Kuhn 1993, 227). Kuhn rightly

claims that his belief that there is no coherent line of ontological development, except that which is

identified retrospectively in science textbooks, does not entail the loss of anything important to his

view of science including his view of scientific progress. Rather, his position simply denies

representationalism or what David Hoy calls "interpretation monism/world monism" in which our

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[24] Louis Menand, *The Metaphysical Club* (New York: Farrar, Straus and Giroux, 2001), 122.

interpretation of the world *represents* it and a complete description of the world is deemed possible[25]. Indeed, the whole idea that science should represent or correspond with a fixed reality is, as we have said, wrongheaded for Kuhn, who claims with regard to "the correspondence theory of truth, the notion that the goal, when evaluating scientific laws or theories, is to determine whether or not they correspond to an external, mind-independent world. It is that notion, whether in an absolute or probabilistic form, that I'm persuaded must vanish together with foundationalism" (Kuhn 1990, 95).

>> $\boxed{5}$ |||| Kuhn's pluralism and "different worlds" thesis

Kuhn's rejection of interpretive monism leads him to his much maligned claim that "when paradigms change, the world itself changes with them" (Kuhn 1996/62, 111), as we mentioned above. Making sense of Kuhn's "different worlds" thesis will prove, *pace* Rorty, to be indispensible to understanding Kuhn's conception of objectivity. First we must look at what his brand of pluralism might entail. Though there does seem to be a single world behind Kuhn's "different worlds," it is unclear how meaningful or important it is or whether it matters whether scientists live in different worlds or in different and incommensurable parts of the same world. If Kuhn does subscribe to what Hoy calls "interpretation pluralism/world pluralism" as opposed to "interpretation pluralism/world monism," he would certainly believe in what Hoy calls "interpretive non-voluntarism" (Hoy 387). One cannot proliferate or change disciplinary matrices at will because a period of acculturation is required. Furthermore, as Hoy says, pluralism "does not think that every interpretation is correct. Pluralism is the view that more than one interpretation can possibly be true. But it may also say that no given interpretation ever completely captures its entire domain. There is always 'more to say'" (Hoy 392). There is "the constant need to revise interpretations,

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[25] David Couzens Hoy, "One What? Relativism and Poststructuralism," in *Relativism: A Compendium*, ed. Michael Krausz (Forthcoming), 387. Subsequent references to this text will be in the body of the paper and of the form (Hoy [*pg. #*]).

which are never fixed and closed" (Hoy 392). These claims are even consistent with Rorty's thought. Kuhn's disciplinary matrices are, in some sense, particularly stringent and constitutive interpretative frameworks. Scientists working under a disciplinary matrix capture part of the world that matrix constitutes, while scientists working in other disciplinary matrixes glean other meaning or even *truth* from the world their matrix constitutes. These worlds are both different, in Kuhn's sense, and the same insofar as the scientists' results are made meaningful to a broader scientific group[26], which happens only occasionally, or to the public, which is rare. Furthermore, Kuhn is not one to denounce or render illegitimate the products of the arts, humanities and social sciences. There is a sense in which, though scientists do produce knowledge about nature, "there is always more to say" about it.

If pluralism does not preclude the notion that some interpretations of a community's (part of the) world are better than others, we must examine how some interpretations might be said to be objectively true on Kuhn's pluralistic account. Kuhn only complicates matters in his early post-*Structure* writing by making a troublesome distinction between stimuli and sensations in an attempt to make sense of his different worlds pluralism. He claims that members of competing scientific disciplines form different sensations for the same given stimulus and says that this is what he means by the idea that they live in different worlds. While he argues that there can be no language of pure sense-data and that Descartes was wrong "in positing a one-to-one correspondence between stimulus and sensations" (Kuhn 1974, 308), he does not throw out the Cartesian distinction with the notion of a one-to-one correspondence between its two poles. Kuhn's notion of exemplars still makes sense when put in these terms since the connections made in working out exemplary problems can be said to rearrange neural connections prior to our explicit verbal interpretations so that the given is seen in a certain way. However, as Sharrock and Read point out, with his talk of

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[26] Kuhn acknowledges a multi-leveled taxonomy of disciplinary matrixes in the sciences (Kuhn 1969, 177-8).

stimuli and sensations, Kuhn has simply "relocated the interaction between 'the given' and 'the interpreted' by moving it outside the visual field and into the interaction between the nervous system and the causal input of nature" (S&R 181). On this interpretation in terms of stimuli and sensations, Rorty is right to say that Kuhn's talk of different worlds indicates his unwillingness to fully abandon his "epistemological project" (Rorty 1979, 324). Kuhn does seem torn here between the antirepresentationalist talk of different worlds and the so-called myth of the given.

Fortunately, Kuhn offers an evolutionary picture of science in his later work that helps him more fully shuck Cartesianism and make sense of his different worlds pluralism. We have already mentioned that Kuhn provides an evolutionary model of scientific development in which scientific specialties proliferate or speciate and move *from* past paradigms, not teleologically *towards* the immutable truth. Kuhn adds to this evolutionary conception of science a third facet that better makes sense of his different worlds pluralism than his stimulus-sensation talk. For Kuhn, as for Haugeland, "the world is not invented or constructed…[it] has been experientially given…Creatures born into it must take it as they find it. They can, of course, interact with it, altering both it and themselves in the process, and the populated world thus altered is the one that will be found in place by the generation which follows" (Kuhn 1990, 102). That scientists live in "different worlds" does not, for Kuhn, devolve into relativism or idealism. As he says,

> "Can a world that alters with time and from one community to the next correspond to what is generally referred to as 'the real world'? I do not see how its right to that title can be denied. It provides the environment, the stage, for all individual and social life. On such life it places rigid constraints; continued existence depends on adaptation to them; and in the modern world scientific activity has become a primary tool for adaptation. What more can reasonably be asked of a real world?" (Kuhn 1990, 102).

Kuhn claims, however, that it is not entirely correct to say that members of a disciplinary matrix adapt to their environment and neither is it completely accurate to say that the world adapts to these individuals. The world is not of our construction, yet we can fundamentally alter it, though only in

ways that involve simultaneously altering ourselves. Kuhn fruitfully models the situation by thinking

of scientists working within a disciplinary matrix as Darwin saw creatures living in a biological niche.

As Kuhn says,

> "Niches may not seem to be worlds, but the difference is one of viewpoint. Niches are
> where *other* creatures live. We see them from outside and thus in physical interaction with
> their inhabitants. But the inhabitants of a niche see it from inside and their interactions with
> it are, to them, intentionally mediated through something like a mental representation.
> Biologically, that is, a niche is the world of the group which inhabits it, thus constituting it a
> niche. Conceptually, the world is *our* representation of *our* niche, the residence of the
> particular human community with whose members we are currently interacting" (Kuhn
> 1990, 103).

Creatures simultaneously constitute their niche and are constituted by it as beings-in-the-world.

Analogously, scientific communities co-arise with the worlds constituted through their disciplinary

matrices. For Kuhn, "what permits the closer and closer match between a specialized practice and

its world is much the same as what permits the closer and closer adaptation of a species to its

biological niche. Like a practice and its world, a species and its niche are interdefined; neither

component of the pair can be known without the other" (Kuhn 1993, 250).

>> 6 |||| Co-constitution of skills and objects: Haugeland's objective truth-telling

Rorty argues that we should "switch over from a Cartesian-Kantian picture of intellectual

progress (as a better and better fit between mind and world) to a Darwinian picture (as an increasing

ability to shape the tools needed to help the species survive, multiply, and transform itself)"[27]. As

our discussion of Kuhn's evolutionary niche analogy indicates, Kuhn's project can and should be

seen in these terms. What we are suggesting, however, is that Kuhn and Haugeland do not believe a

reduction of objective truth to solidarity is necessary for this project's execution. Antirepresenta-

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[27] Rorty, "Introduction: Pragmatism and post-Nietzschean philosophy," in *Essays on Heidegger and others, Philosophical papers, Volume 2* (Cambridge: Cambridge University Press, 1991), 3.

tionalism does not bar them from a meaningful notion of objectivity, a notion, that must be able to, as Haugeland says, "give sense to the possibility of *everyone* being consistently wrong about something" (Haugeland 315), something for which Rorty's notion of consensus-based solidarity most assuredly cannot account. That everyone can be consistently wrong about something is essentially the criterion for objectivity mentioned above that objects must exceed what is said about them—exceed their constitution—and thus be able to bring down the system by which they are constituted. As we are beginning to see through Kuhn's analogy to biological niches, paradigms are accountable or beholden, to use Haugeland's term, to the world. Indeed, as Haugeland makes clearer than does Kuhn, the fact that scientific communities constitute their (part of the) world(s) actually enables rather than precludes objectivity.

In his essay "Truth and Rule-Following"[28], Haugeland, who co-edited *The Road Since Structure* compilation of Kuhn's work and is, therefore, no stranger to Kuhn's ideas, tries to elaborate the integrated structure of rules objective truth-telling presupposes (Haugeland 305). He says explicitly that he is trying to go beyond the pragmatic approach of someone like Rorty to recover a meaningful notion of objectivity, because for Haugeland, "genuine science is more than a social institution" (Haugeland 317). For him, "there must be two fundamentally distinct sorts of normative constraint: social propriety and objective correctness (truth)" (Haugeland 317). In science, unlike in biologically evolved normativity and socially instituted normativity, these two types of normative constraint—i.e. proper performance and getting things right—are distinguished. Whereas, to use his example, it is impossible for everyone in a given society to perform an indigenous folk dance incorrectly if it is that folk dance which they are setting out to do—because if

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[28] The ensuing discussion of Haugeland refers almost entirely to his essay "Truth and Rule-Following" in his book *Having Thought*, cited above (p. 11, n. 16), though reference is made at one point to another part of this book.

everyone did it one way, then that would be the correct way to do it—all scientists *could* be wrong about something.

There are, most generally, two types of rules: exhibited rules, which essentially describe what happens (e.g. the laws of physics), and governing rules, which dictate, delimit or prescribe what happens (e.g. driving directions or social norms) (Haugeland 305). Haugeland identifies two types of governing rules in the work of John Rawls and John Searle: on the one hand, there are rules regarding things that make sense independently of those rules and, on the other hand, rules that *constitute* that which they govern. Haugeland goes on to focus on these latter, constitutive rules and says that only constituted phenomena can be objects (Haugeland 325). This contention is crucial since any notion of *objectivity* must depend on what is meant by "object" and Kuhn's analogy to evolutionary niches seems to imply that the "different worlds" scientists inhabit are co-constituted with scientific communities. For Haugeland, to constitute is not to make or create an object, but neither is it to simply count or take an object as something. Such a sentiment echoes Rorty's claim that the world does not determine thought nor thought, the world or reality. To this extent, Haugeland, like Rorty and Kuhn, is an antirepresentationalist. For all three thinkers, the question of determinacy dissolves along with foundationalism, representationalism, Cartesian dualistic epistemology and the idea that there is a neutral scientific language unaffected by human values. On the one hand, the notion that objects are entirely of our making is solipsistic and idealistic and reduces objectivity to the internal coherence of the system[29]. On the other hand, holding that constitution is simply counting-as presupposes the objecthood of the objects one counts as objects (Haugeland 326). Attempting to transcend this problem, Haugeland develops the idea that "to

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[29] The term "system" should be understood broadly here to mean anything from the creation of rationalist, intellectualist or idealist systems (e.g. Hegel's historical dialectic) to structuralist semiology to, most pertinent for our purposes, a scientific theory or disciplinary matrix. Perception can only occur through some sort of reference frame and this conceptual scheme or system need not be closed and complete like Hegel's absolute. For more on the variety and character of reference frames, see Michael Krausz "Mapping Relativisms" in *Relativism: A Compendium* (cited above, p. 18, n. 25), 7-11.

constitute is to bring into being…[by] *letting be*” (Haugeland 325). Such a definition of constitution is decidedly Heideggerian in its relation to Heidegger’s talk of opening oneself or Dasein up to the disclosure of Being. Indeed, Haugeland identifies Heidegger as a fellow “anti-pragmatist” (Haugeland 317). Haugeland exemplifies his notion of constitution by talking of letting chess pieces “be” to discover that chess, or whatever game is being played, may be played with them. As Haugeland points out, chess pieces need not fit a certain physical description and the pieces in a chess set could be used to play a different game (Haugeland 297). This example reinforces the point that only constituted phenomena are objects and, thus, “what chess players perceive and move, the *objects* that stand as criteria for the correct exercise of their mundane skills [i.e. their ability to play the game according to the rules], are chess pieces, not figurines” (Haugeland 329) and they are only chess pieces because they have been *constituted* as such by the players not in an idealist or solipsistic sense, but by simply letting them be chess pieces.

Haugeland’s notion of “existential commitment” is crucial to his discussion of constitution as “letting be.” For Haugeland, existential commitment differs from deontic commitment—i.e. “a socially grounded obligation or duty…a way one is supposed to behave” (Haugeland 341) on pain of rejection—in that the latter is a socially grounded obligation while the former is a first person stance that is “a dedicated or even a devoted way of living: a determination to maintain and carry on…Such commitment is not ‘to’ other players or people, or even to oneself, but rather to an ongoing, concrete game, project or life” (Haugeland 341). These two types of commitments entail corresponding deontic and existential responsibilities and, thus, as Haugeland says, “the normative authority of objects, by virtue of which they can stand as criteria for the correctness of mundane results [within a “game”]—and thus as binding on judgements and assertions—devolves upon them from the commitment to the standards in accord with which they are constituted” (Haugeland 343). In this way, Haugeland speaks of mundane performances—i.e. ones analogous to moves *within* a

game—as normatively *beholden* to constituted objects for their correctness (Haugeland 348). As we have indicated, however, Haugeland is importantly not arguing for the correspondence theory of truth that Kuhn and Rorty reject and he is not proposing the possibility of a neutral language. Rather, he says that "what we want is a way to acknowledge that all objective checking (testing, validation, confirmation, and so on) is 'internal' to its domain—at least in the sense of not being alien to it—while maintaining that objective correctness is importantly independent of the practices constituting that domain" (Haugeland 345). Objective correctness requires "external" constraint— i.e. constraint that is neither a construction of the individual mind nor a group creation unconstrained by nature—even if we cannot say that that external constraint is the real truth "out there," but only that it is a constituted phenomenon. We are beginning to see, then, that saying that correctness depends on constitutive practices need not preclude a tenable notion of objectivity.

Haugeland fruitfully interprets Kuhn's talk of different worlds by saying that "the 'worlds' are different because the constituted events are different; that is, they belong to different domains" (Haugeland 352). This reading is in line with Kuhn's talk of scientific niches and Haugeland confirms the felicity of this connection by saying that "in a constituted domain…skills and objects are *made for each other*: the objective skills [i.e. essentially the abilities allowing us to abide by governing rules correctly and reliably (Haugeland 322)] are *co-constituted* along with the objective phenomena, and the latter are precisely what the former respond to (and otherwise deal with)" (Haugeland 346). One has an existential commitment to one's niche and the constituted phenomena within it are the *objects* that populate one's world and to which one's constitutive skills respond and alongside which they develop. Despite this connection, Haugeland claims that Kuhn cannot explain the fact that, though constituted objects co-arise with the objective skills that respond to them, these constituted objects also have the power to bring the disciplinary matrix down. Such an accusation may seem a strange one to levy against the father of "scientific revolutions," but it is not clear from what we

have said so far that it is entirely unfounded. Clearly, then, this accusation must be dismantled if Kuhnian objectivity is to be recovered since the ability of objects to "bring down the system" is the necessary condition of objectivity we identified above. In order to account for everyone being objectively wrong about something, Kuhn must be able to explain how constituted objects can bring down their co-constituted practice. Thus, it is to elucidating and dismantling this accusation that we will turn after a brief discussion of Kuhnian lexicons, another way he makes sense of his concept of paradigms. Understanding what he means by lexicons and their component kind terms will help us understand the process by which objects force the restructuring of the system.

>> 7 | | | | Lexicons and kinds

A lexicon in Kuhn's parlance is "the module in which members of a speech community store the community's kind terms" (Kuhn 1993, 229). Possessions of scientific communities, kinds, whether physical, social or otherwise, "populate the world as well as divide up a preexisting population" (Kuhn 1993, 229). Thus, we see the interconnected nature of a scientific lexical community—i.e. a community with a disciplinary matrix—and its world. Kuhn is not proposing a nominalistic view in which kinds simply divide up at will a world that consists of real, given individuals out there. Such a view lends itself to the traditional empiricist monism Kuhn rejects. Scientific practice not only entails the further specification of the same kind terms but also periods of lexical reconstruction. Furthermore, many kinds, such as "force" cannot be seen as individuals and kinds do not just divide the world up, but populate it as well (Kuhn 1993, 229). Our lexicon is that through which we "let our world be" what it is for us. It neither makes the world *ex nihilo* nor simply illuminates it as if the world were given, predetermined and fixed. Thus, as Sharrock and Read point out, "Kuhn is with Wittgenstein in seeing nature not as a standard of and for

comparison, except as 'within' a paradigm" (S&R 170). A statement's truth-value depends on and can only be evaluated within a lexicon (Kuhn 1989a, 77).

Kind terms are learned in use through exemplars and, what is most important for our purposes, "by the time the learning process has been completed, the learner has acquired knowledge not only of the concepts but also of the properties of the world to which they apply" (Kuhn 1993, 230). Again, acculturation into a disciplinary matrix is learning about *nature*, not an individual or social construction. Kind terms are also, as Kuhn puts it, "projectible: to know any kind term at all is to know some generalizations satisfied by its referents and to be equipped to look for others" (Kuhn 1993, 230). In other words, the projectibility of kind terms is what enables induction[30]. Thus, learning kind terms entails learning things about nature, but this learning is not reducible to the acquisition of an exhaustive list of generalizations about the world and the correspondence criteria that allow these generalizations to be correctly applied to represent it. Rather, acquiring a lexicon compatible with the community's lexical structure or disciplinary matrix is essentially acquiring "the world in which members of the culture [or community] live" (Kuhn 1993, 247). In other words, the student learns a worldview, which *opens her up to the world.*

A lexicon opens a scientist up to the world in that its kind terms are that in terms of which the world is constituted. Such an assertion runs counter to the traditional view that natural phenomena, the objects of science, are static and absolute. Kuhn contends that the sciences deal objectively with the real world. However, as he says, "their truths (and falsities) are thought to transcend the ravages of temporal, cultural, and linguistic change. I [Kuhn] am suggesting, of course, that they cannot do so" (Kuhn 1979a, 75). For Kuhn and Haugeland, natural phenomena— i.e. Haugeland's constituted phenomena or objects—which are the referents of a lexicon's kind

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[30] Kuhn, "Possible Worlds in History of Science" (1989), in *The Road Since Structure*, 75. Subsequent references to this text will be in the body of the paper and of the form (Kuhn 1989a, [*pg. #*]).

terms are not constant across scientific communities separated by time or by space. Kuhn claims that the heavens were different for the ancient Greeks from what they are for us now because "our celestial taxonomies are systematically different"[31]. For example, looking at what we call the cluster of stars that make up the Milky Way galaxy in which Earth is situated, the ancient Greeks saw "meteors," where their concept of "meteors" included such things as we now call rainbows (Kuhn 1989b, 218-9). For Kuhn, this difference is *not* simply a matter of seeing the same stable, immutable objects in different ways. As he contends in talking about learning through working out exemplary problems and in discussing the projectibility of a lexicon, "though anyone who understands a concept must know *some* salient features of the objects or situations that fall under it, those features may vary from individual to individual, and no one of them need be shared to permit the concept's proper application" (Kuhn 1989b, 219). Just as there is no exhaustive set of explicit criteria prescribing the proper application of formal generalizations, there is no exhaustive set of explicit criteria determining the referents of a concept or a kind term. Indeed, the concept is what allows objects to show up as something. What Kuhn is arguing against here is the idea that, though the Greeks called a meteor what we now call a star, an ancient Greek and a modern astronomer could *point* to the same thing as an instantiation of their corresponding concepts and thereby both be instantly clear about what the other means. The problem is that there is no one-to-one correspondence relationship between the Greek concept of a meteor and our concept of a star or, more generally, between concepts in incommensurable lexical structures. As Kuhn says,

> "until identity can be made out, there is nothing to be learned (or taught) by pointing. As in the case of equity or negotiation [i.e. social as opposed to natural concepts], neither the presentation nor the study of examples can begin until the concept of the object to be exemplified or studied is available. And what makes it available whether in the natural or social sciences, is a culture [which we can see as a community with a lexical structure], within

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[31] Kuhn, "The Natural and the Human Sciences" (1989), in *The Road Since Structure*, 218. Subsequent references to this text will be in the body of the paper and of the form (Kuhn 1989b, [*pg. #*]).

which it is transmitted by exemplification, sometimes in altered form, from one generation
to the next" (Kuhn 1989b, 220).

Thus, for Kuhn, natural phenomena are *not* concept or lexicon independent. They appear as kinds

in our lexicons. The ancient Greek astronomical lexical structure is incommensurable with ours and,

thus, there is no meaningful way in which we are simply pointing at "the same thing" and labeling it

differently. To point at what we now call the Milky Way presumes we have a certain concept or

kind term that we have learned through exemplification. Pointing is always already a pointing *at*

something and the something at which one points is *not* independent of the concept that allows one

to point at that something (*as something*). It follows from this that that which scientists take to be

facts depends on the theory espoused (Kuhn 1973, 338).

>> 8 | | | | Bringing down the system 1: Haugeland's "excluded zone"

If objects and, thus, observational facts depend on theory in the sense of being constituted

through the lexicon of the disciplinary matrix, it might seem that Kuhn cannot account for the

ability of objects to bring down the constitutive lexicon or "system." Haugeland claims that his,

Haugeland's, account of constitution entails a consistent explanation of the ability of objects to bring

down the system, while Kuhn's account of science does not. In explaining how Haugeland can

account for the "externality" of objects and, thus, for objective correctness, we will show that

Kuhn's account of science as characterized above is entirely consistent with and even helps elucidate

Haugeland's account.

For Kuhn, seeing and being able to work with the constituted objects of a particular

scientific discipline presumes that a scientist has been acculturated into the discipline's disciplinary

matrix—a "constituted domain" for Haugeland—and a lexicon, which is structurally congruent with

those of other members of the discipline. That this acculturation is a process of education that

discloses (part of) the world means for both Kuhn and Haugeland that, as Haugeland, writes, "constitution, making sense of objects, is not free" (Haugeland 353). Like Haugeland, Kuhn sees the intelligibility of the discipline's (part of the) world as depending on the constituted objects, the kinds referred to by the lexicon's kind terms, which are known by "the correct exercise of constitutive skills" (Haugeland 353). As we have said, for Haugeland, "honest objectivity…would seem to require some *external* constraint" (Haugeland 345) and this constraint comes from the constituted objects. It is crucial here that objects in some sense *exceed* their constitution, which is to say that they exceed themselves as the objects they are in the lexicon. The world is never entirely captured once and for all by the lexicon, which means that anomalies may arise in observation that the lexicon cannot render entirely intelligible and that seem to defy the lexicon in some way. Haugeland calls the realm in which the conceivable exceeds the possible the "excluded zone." Constitution is at once an enabling and a limiting of possible meaning. It discloses the world of a given practice or discipline, but the intelligibility it enables is contingent on there being an excluded zone marking the theoretically empty space between the possible and the conceivable. Anomalies that arise in the excluded zone are, in the lexicon in question, impossible and, thus, not true or false, but rather unintelligible or ineffable in some way. As we have said, the constituted domain's intelligibility depends on the constituted objects. These constituted objects are, in turn, only intelligible insofar as they follow the domain's constitutive standards and, thus, do not appear in the excluded zone by exceeding the possible. As Haugeland says, phenomena in the excluded zone are conceivable and, thus, "the crucial requirement that they don't in fact occur is subject to empirical test" (Haugeland 333). That the acceleration of an object near earth's surface is not 9.8 meters per second per second is conceivable and would show up on an empirical test, but it is not *possible*—it does not make sense in terms physical theory. To say that it is *conceivable* that a given stone's acceleration in free fall near the earth's surface is, say, 10.3 m/s^2 is not to say simply that it might

occur in a dream but that it would be recognized if it actually occurred in a scientific experiment. It

is not *possible* because it is not in line with the constitutive standards of modern physical theory.

With Haugeland's conception of the excluded zone in place, let us characterize his critique of

Kuhn in greater detail. Haugeland contends that constitution

> "depends on an equilibrium among a number of constitutive and mundane skills—an
> equilibrium which, since it excludes the bulk of what it renders conceivable (testable), is
> empirically precarious. But, for the same reason, and for as long as it lasts it is also an
> empirical achievement. The constituted objects participate in that achievement, deriving
> determinacy and normative status from it. But they also have an ability to resist that
> transcends that participation, because it is they, and they alone, upon which the equilibrium
> itself rests; by not cooperating they have the power to bring it down. Even though in so
> doing they must annihilate themselves as what they are, nevertheless they can" (Haugeland
> 353).

We have already seen, through his evolutionary niche analogy, that Kuhn seems to share

Haugeland's conception of constituted objects and domains. Despite this seeming agreement,

Haugeland claims that Kuhn cannot explain the "potentially resistant yet normatively significant role

for the objects themselves" (Haugeland 354). Turning to Kuhn's discussion of theory change

should, however, show that Kuhn does believe along with Haugeland that the normative force of

anomaly does indeed come from the (anomalous) objects themselves. Such an argument would

support Kuhn's well known claim that excessive anomaly can lead to scientific revolutions and

theory change, which seems to jibe with Haugeland's conception of the excluded zone.

>> ⁹ | | | Bringing down the system 2: Kuhn's "no-overlap" principle

For Kuhn, members of a particular scientific community possess structurally congruent

lexicons, which contain the kind terms that both populate the community's world and divide up

preexisting populations. There is a strict "no-overlap" principle in Kuhn's notion of a kind. As

Kuhn puts it, "no two kind terms, no two terms with the kind label, may overlap in their referents

unless they are related as species to genus" (Kuhn 1990, 92) as, for example, "human" is to

"mammal" [32]. Not all terms are kind terms for Kuhn and part of what distinguishes a kind term

from other terms is the no-overlap principle. In ordinary language some terms exhibit this

principle—e.g. "there are no dogs that are also cats, no gold rings that are also silver rings" (Kuhn

1990, 92)—but there are many sets of ordinary language terms that have overlapping referents and

are not related as species to genus. For instance, not all atheists are police officers and not all police

officers atheists, but there are atheists that are also police offers and their existence does not, for

most people in our society, elicit strain on our conceptual vocabulary. There is, however, no overlap

in the referents of natural kind terms. For instance, in the lexical structure of modern chemical

theory, the referents of elemental carbon do not overlap with those of elemental iron. On the other

hand, mass nouns such as "combustible" or "brown" have overlapping referents and, thus, are not

kind terms.

A kind term is *not* equivalent to the idea of a "primary substance" found in Aristotle's

Categories[33]. *Contra* the causal theory of reference, natural kind terms do not have absolute and

essential "Ur-properties," properties that distinguish them once and for all (Kuhn 1989a, 78). Hilary

Putnam, in defense of the causal theory of reference, suggests that if scientists were to go to "Twin

Earth," a planet otherwise identical to Earth, and find that the substance with all the properties of

water at normal temperatures and pressures had a long chemical formula, abbreviated XYZ, as

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[32] The following quote from Kuhn is useful to keep in mind in our discussion of the no-overlap principle and kind terms generally: "Kind terms supply the categories prerequisite to description of and generalization about the world. If two communities differ in their conceptual vocabularies, their members will describe the world differently and make different generalizations about it. Sometimes such differences can be resolved by importing the concepts of one into the conceptual vocabulary of the other. But if the terms to be imported are kind terms that overlap kind terms already in place, no importation is possible, at least no importation which allows both terms to retain their meaning, their projectibility, their status as kind terms. Some of the kinds that populate the worlds of the two communities are then irreconcilably different, and the difference is no longer between descriptions but between the populations described. Is it, in these circumstances, inappropriate to say that the members of the two communities live in different worlds?" (Kuhn 1993, 233).

[33] Aristotle, *Categories*, in *Aristotle: Selections*, ed. and trans. Terence Irwin and Gail Fine (Indianapolis: Hackett, 1995), margin number 2a15ff.

opposed to H_2O, they would report back to Earth that "On Twin Earth the word 'water' means XYZ" (Kuhn 1989a, 80, quoting Putnam). This view cannot account for lexical restructuring or, therefore, objectivity, since the discovery of something with all the properties of H_2O save the chemical formula would simply be accepted as a different chemical because it has a different Ur-property, i.e., a different chemical formula. All the shared properties are deemed superficial since the Ur-properties differ. Kuhn counters that "modern chemical theory…is incompatible with the existence of a substance with properties very nearly the same as water but described by an elaborate chemical formula" (Kuhn 1989a, 80). Thus, the discovery of XYZ would show up in the excluded zone of chemical theory and entail a restructuring of that theory to make it explainable. To call something H_2O requires the acquisition of a group of lexically interdefined terms that allow the scientist to *use* the term in practice. One does not simply learn an explicit and absolute definition. For Kuhn, "the so-called superficial properties are no less necessary than their apparently essential successors" (Kuhn 1989a, 83) since the properties are interdefined within the lexicon. Observing all the properties of H_2O should allow scientists to predict that they are observing H_2O and, if that expectation is disappointed, the lexicon might have to be restructured. If all properties of a kind but one were superficial, one could not have any expectations about the objects and the existence of the excluded zone depends on the constitutive expectations that demarcate the possible from the conceivable.

Nor are kind terms *absolute* demarcations. Putnam claims that water always meant H_2O even before the discovery of hydrogen and oxygen. Kuhn points out, however, that whereas H_2O refers to ice, liquid water and steam, the liquidity of water was seen as an essential property of it 250 years ago (Kuhn 1989a, 81-2). Delimiting and distinguishing natural kind terms is a matter over which debate is never ultimately settled since this debate must proceed from within a lexicon, which, because it opens up rather than forecloses possibilities of meaning, has an excluded zone.

Employing yet another biological analogy, Kuhn says that "even individuals who are unproblematically members of the same species have differently constituted sets of genes. Which sets are compatible with membership in that species is a subject of continuing debate" (Kuhn 1989a, 84, n. 30). Kuhn likens the individual's lexicon to a particular organism's set of genes, which come from the "gene pool" of the community's, or species', lexicon, which is, in turn, what constitutes the species (Kuhn 1993, 242-3). Analogously, kind terms do not refer to some static object referents once and for all. Rather, kind terms depend on the disciplinary matrix of which they are a part and, indeed, their referents are co-constituted with them. As has been said, all perception presumes the existence of a constitutive reference frame (see p. 23, n. 29 above), which may be extended, elaborated or restructured, but which also *limits* the possible overlaps. This limiting, which is a necessary condition for perception, is what produces Haugeland's excluded zone. In order for observations to show up in the excluded zone, the lexicon must be conceived as a system in which expectations can be disappointed and one can thus be objectively wrong, rather than as a closed system of kinds with absolute, given and explicit Ur-properties or definitions.

Thus, the lexical contingency of natural phenomena, on Kuhn's account, seems to enable objectivity by both enabling and limiting possible meaning and, thus, creating an excluded zone. When anomalous observations, such as the discovery of XYZ, occur in the excluded zone, lexical structures are occasionally "brought down" or altered to accommodate these new observational data. Recall that one does not become a practicing member of a given scientific community by learning definitions or correspondence criteria. Rather, one learns to use the terms of a discipline's lexicon through doing exemplary problems. Similarly, the process of theory change is not a matter of simply adding new concepts, such as "XYZ," to the existing theory. Contemporary chemical theory cannot answer the question of whether XYZ is water. As Kuhn puts it, "such circumstances, if they endure for long, call forth a locally different lexicon, one that permits an answer but to a

slightly altered question" (Kuhn 1989a, 72). In other words, the restructured theory would be able to say whether or not XYZ is water, but water would mean something different from what it had meant in the old theory. The kind term and its referent have changed. In this way, "the new lexicon opens new possibilities, ones that could not have been stipulated by the use of the old" (Kuhn 1989a, 72). In this way, the objects can, on Kuhn's account, bring down the structure and, with it, themselves as the objects they are in it. Therefore, the constituted objects (as opposed to Rorty's consensus) are, for Kuhn as well as Haugeland, the loci of incompatibility when our moves in a given "game" do not accord with the constitutive standards that govern all the phenomena in that game (Haugeland 336-7).

>> 10 | | | | Conclusion: value-based objectivity

"Science…deludes itself about the fundamental interests to which it owes not only its impetus but the conditions of possible objectivity *themselves."* [34]

> – Jürgen Habermas

We have attempted to bring out the largely implicit antirepresentationalist notion of objectivity in Kuhn's work. We have seen that Kuhn is rightfully resolute about the fact that, as he puts it, "science is cognitive, that its product is knowledge of nature, and that the criteria it uses in evaluating beliefs are in that sense epistemic" (Kuhn 1993, 243). As our interpretation of Kuhn's different worlds thesis suggests, scientific knowledge and theory choice depends on the *community* nature of science, and progress occurs in terms of scientific *values*. The shared lexical structure of community members' lexicons allows for the complete communication necessary for the rather cumulative progress of normal, inter-revolutionary science, which is almost entirely that in which science consists. Kuhn's evolutionary notion of different worlds indicates, as we have stressed, that

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[34] Jürgen Habermas, *Knowledge and Human Interests*, trans. Jeremy J. Shapiro (Boston: Beacon Press, 1971/original German text, 1968), 311.

nature is not independent of the scientific community, in the sense that this community is co-constituted with its objects of study. That fact helps account for the way in which learning to see in the way of the scientific community can be learning about nature. Charles S. Peirce claims that "reality is independent, not necessarily of thought in general, but only of what you or I or any finite number of men may think about it"[35]. For Kuhn too, if in a somewhat different way, "science is intrinsically a community activity. Methodological solipsism, the traditional view of science as, at least in principle, a one-person game, will prove, I [Kuhn] am quite sure, to have been an especially harmful mistake" (Kuhn 1993, 243). For Kuhn, scientific communities "should…be regarded as the units which produce scientific knowledge" (Kuhn 1970, 148).

The supposedly "subjective" or idiosyncratic features of theory choice and of scientific practice in general are, for Kuhn, indispensible to it. He says that "objectivity ought to be analyzable in terms of criteria like accuracy and consistency" (Kuhn 1973, 338), criteria that are, for him, *values*, not absolute or neutral rules. During theory choice, scientists, who appeal to these values idiosyncratically, are making *judgments*, not simply expressing *tastes* about which there can be no debate (Kuhn 1973, 337). As Kuhn says, "where factors dependent on individual biography or personality must be introduced to make values applicable, no standards of factuality or actuality are being set aside" (Kuhn 1973, 337). Rather, as we have said, objectivity should be analyzable in terms of these values. Indeed, for Kuhn, science is in no way immune to values or, on a broader level, to other so-called "subjective" influences like politics and power. As he says,

> "the pursuit of puzzle solving [i.e. normal science] constantly involves practitioners with questions of politics and power, both within and between the puzzle-solving practices, as well as between them and the surrounding nonscientific culture. But in the evolution of human practices, such interests have governed from the start. What further development has brought with it is not their subordination but the specialization of the functions to which they are put. Puzzle solving is one of the families of practices that has arisen during that

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[35] Charles Sanders Peirce, "How to Make Our Ideas Clear" (1878), in *Pragmatism: A Reader*, ed. Louis Menand (New York: Vintage Books, 1997), 45.

evolution, and what it produces is knowledge of nature. Those who proclaim that no interest-driven practice can properly be identified as the rational pursuit of knowledge make a profound and consequential mistake" (Kuhn 1993, 252).

Knowledge is never free from interests or values, scientific or otherwise. Scientific practice within a community is co-constituted with its constituted domain or world. We have shown that this constitutive view of the world or, in Kuhn's parlance, of the different worlds of different disciplines, enables an antirepresentationalist objectivity in which theory does not mirror nature, but can still be brought down by its constituted objects. This objectivity depends on the constitution of a discipline's world in that constitution enables and limits the possible meanings of the things that populate that world. There is always an excluded zone between the possible and the conceivable. Thus, knowledge about nature is necessarily open-ended. As Hoy claims with regard to interpretations and meaning, there is always more to say. That does not mean that all things that are said are correct or that science cannot be associated with getting at how things are. It does mean that "how things are" is inextricable from the scientific practice that is constantly opening itself up to it or letting it be.

>> A | | | | Appendix: Kuhn and Rorty reference key

Full bibliographic information is included in the first footnote referencing a given source. Subsequent references to texts are given parenthetically with multiple texts by the same author being distinguished by date. Though this information is in the footnotes, I am providing this table summarizing, for all the Kuhn and Rorty texts, the parenthetical abbreviation used and the page number and note number of the first note referencing the text where the reader can find full bibliographic information.

Parenthetical abbreviation	Title	Page number of first note	Note number of first note
Kuhn 1996/62	*The Structure of Scientific Revolutions*	2	2
Kuhn 1966	"Comment on the Relations of Science and Art"	12	18
Kuhn 1969	"Postscript-1969"	8	13
Kuhn 1970	"Reflections on My Critics"	13	20
Kuhn 1973	"Objectivity, Value Judgment, and Theory Choice"	9	15
Kuhn 1974	"Second Thoughts on Paradigms"	8	12
Kuhn 1979	"Metaphor in Science"	5	4
Kuhn 1982	"Commensurability, Comparability, Communicability"	13	19
Kuhn 1989a	"Possible Worlds in History of Science"	27	30
Kuhn 1989b	"The Natural and Human Sciences"	28	31
Kuhn 1990	"The Road Since *Structure*"	16	23
Kuhn 1993	"Afterwords"	8	14
Rorty 1979	*Philosophy and the Mirror of Nature*	5	5
Rorty 1985	"Solidarity or Objectivity?"	5	8
Rorty 1987	"Science as Solidarity"	6	9
Rorty 1988	"Is Natural Science a Natural Kind?"	5	6
Rorty 1991a	"Introduction: Antirepresentationalism, ethnocentrism, and liberalism"	5	7
Rorty 1991b	"Introduction: Pragmatism and post-Nietzschean philosophy"	21	27