The Road since Structure

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The Road Since Structure

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On this occasion, and in this place, I feel that I ought, and am probably expected, to look back at the things which have happened to the philosophy of science since I first began to take an interest in it over half a century ago. But I am both too much an outsider and too much a protagonist to undertake that assignment. Rather than attempt to situate the present state of philosophy of science with respect to its past — a subject on which I've little authority — I shall try to situate my present state in philosophy of science with respect to its own past — a subject on which, however imperfect, I'm probably the best authority there is.

As a number of you know, I'm at work on a book, and what I mean to attempt here is an exceedingly brief and dogmatic sketch of its main themes. I think of my project as a return, now underway for a decade, to the philosophical problems left over from the *Structure of Scientific Revolutions*. But it might better be described more generally, as a study of the problems raised by the transition to what's sometimes called the historical and sometimes (at least by Clark Glymour, speaking to me) just the "soft" philosophy of science. That's a transition for which I get far more credit, and also more blame, than I have coming to me. I was, if you will, present at the creation, and it wasn't very crowded. But others were present too: Paul Feyerabend and Russ Hanson, in particular, as well as Mary Hesse, Michael Polanyi, Stephen Toulmin, and a few more besides. Whatever a *Zeitgeist* is, we provided a striking illustration of its role in intellectual affairs.

Returning to my projected book, you will not be surprised to hear that the main targets at which it aims are such issues as rationality, relativism and, most particularly, realism and truth. But they're not primarily what the book is about, what occupies most space in it. That role is taken instead by incommensurability. No other aspect of *Structure* has concerned me so deeply in the thirty years since the book was written, and I emerge from those years feeling more strongly than ever that incommensurability has to be an essential component of any historical, developmental, or evolutionary view of scientific knowledge. Properly understood — something I've by no means always managed myself — incommensurability is far from being the threat to rational evaluation of truth claims that it has frequently seemed. Rather, it's what is needed, within a developmental perspective, to restore some badly needed bite to the whole

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notion of cognitive evaluation. It is needed, that is, to defend notions like truth and knowledge from, for example, the excesses of post-modernist movements like the strong program. Clearly, I can't hope to make all that out here: it's a project for a book. But I shall try, however sketchily, to describe the main elements of the position the book develops. I begin by saying something about what I now take incommensurability to be, and then attempt to sketch its relationship to questions of relativism, truth, and realism. In the book, the issue of rationality will figure, too, but there is no space here even to sketch its role.

Incommensurability is a notion that for me emerged from attempts to understand apparently nonsensical passages encountered in old scientific texts. Ordinarily they had been taken as evidence of the author's confused or mistaken beliefs. My experiences led me to suggest, instead, that those passages were being misread: the appearance of nonsense could be removed by recovering older meanings for some of the terms involved, meanings different from those subsequently current. During the years since, I've often spoken metaphorically of the process by which later meanings had been produced from earlier ones as a process of language change. And, more recently, I've spoken also of the historian's recovery of older meanings as a process of language learning rather like that undergone by the fictional anthropologist whom Quine misdescribes as a radical translator (Kuhn 1983a). The ability to learn a language does not, I've emphasized, guarantee the ability to translate into or out of it.

By now, however, the language metaphor seems to me far too inclusive. To the extent that I'm concerned with language and with meanings at all — an issue to which I'll shortly return — it is with the meanings of a restricted class of terms. Roughly speaking, they are taxonomic terms or kind terms, a widespread category that includes natural kinds, artifactual kinds, social kinds, and probably others. In English the class is coextensive, or nearly so, with the terms that by themselves or within appropriate phrases can take the indefinite article. These are primarily the count nouns together with the mass nouns, words which combine with count nouns in phrases that take the indefinite article. Some terms require still further tests hinging, for example, on permissible suffixes.

Terms of this sort have two essential properties. First, as already indicated, they are marked or labelled as kind terms by virtue of lexical characteristics like taking the indefinite article. Being a kind term is thus part of what the word means, part of what one must have in the head to use the word properly. Second — a limitation I sometimes refer to as the no-overlap principle — no two kind terms, no two terms with the kind label, may overlap in their referents unless they are related as species to genus. There are no dogs that are also cats, no gold rings that are also silver rings, and so on: that's what makes dogs, cats, silver, and gold each a kind. Therefore, if the members of a language community encounter a dog that's also a cat (or, more realistically, a creature like the duck-billed platypus), they cannot just enrich the set of category terms but must instead redesign a part of the taxonomy. Pace the causal theorists of reference, 'water' did not always refer to H_2O (Kuhn 1987; 1990, pp. 309-14).

Notice now that a lexical taxonomy of some sort must be in place before description of the world can begin. Shared taxonomic categories, at least in an area under discussion, are prerequisite to unproblematic communication, including the communication required for the evaluation of truth claims. If different speech communities have taxonomies that differ in some local area, then members of one of them can (and occasionally will) make statements that, though fully meaningful within that speech community, cannot in principle be articulated by members of the other. To bridge the gap between communities would require adding to one lexicon a kind-term that overlaps, shares a referent, with one that is already in place. It is that situation which the no-overlap principle precludes.

Incommensurability thus becomes a sort of untranslatability, localized to one or another area in which two lexical taxonomies differ. The differences which produce it are not any old differences, but ones that violate either the no-overlap condition, the kind-label condition, or else a restriction on hierarchical relations that I cannot spell out here. Violations of those sorts do not bar intercommunity understanding. Members of one community can acquire the taxonomy employed by members of another, as the historian does in learning to understand old texts. But the process which permits understanding produces bilinguals, not translators, and bilingualism has a cost, which will be particularly important to what follows. The bilingual must always remember within which community discourse is occurring. The use of one taxonomy to make statements to someone who uses the other places communication at risk.

Let me formulate these points in one more way, and then make a last remark about them. Given a lexical taxonomy, or what I'll mostly now call simply a lexicon, there are all sorts of different statements that can be made, and all sorts of theories that can be developed. Standard techniques will lead to some of these being accepted as true, others rejected as false. But there are also statements which could be made, theories which could be developed, within some other taxonomy but which cannot be made with this one and vice versa. The first volume of Lyons' *Semantics* (1977, pp. 237-8) contains a wonderfully simple example, which some of you will know: the impossibility of translating the English statement, "the cat sat on the mat", into French, because of the incommensurability between the French and English taxonomies for floor coverings. In each particular case for which the English statement is true, one can find a co-referential French statement, some using 'tapis', others 'paillasson,' still others 'carpette,' and so on. But there is no single French statement which refers to all and only the situations in which the English statement is true. In that sense, the English statement cannot be made in French. In a similar vein, I've elsewhere pointed out (Kuhn 1987, p. 8) that the content of the Copernican statement, "planets travel around the sun", cannot be expressed in a statement that invokes the celestial taxonomy of the Ptolemaic statement, "planets travel around the earth". The difference between the two statements is not simply one of fact. The term 'planet' appears as a kind term in both, and the two kinds overlap in membership without either's containing all the celestial bodies contained in the other. All of which is to say that there are episodes in scientific development which involve fundamental change in some taxonomic categories and which therefore confront later observers with problems like those the ethnologist encounters when trying to break into another culture.

A final remark will close this sketch of my current views on incommensurability. I have described those views as concerned with words and with *lexical* taxonomy, and I shall continue in that mode: the sorts of knowledge I deal with come in explicit verbal or related symbolic forms. But it may clarify what I have in mind to suggest that I might more appropriately speak of concepts than of words. What I have been calling a lexical taxonomy might, that is, better be called a conceptual scheme, where the "very notion" of a conceptual scheme is not that of a set of beliefs but of a particular operating mode of a mental module prerequisite to having beliefs, a mode that at once supplies and bounds the set of beliefs it is possible to conceive. Some such taxonomic module I take to be pre-linguistic and possessed by animals. Presumably it evolved originally for the sensory, most obviously for the visual, system. In the book I shall give reasons for supposing that it developed from a still more fundamental mechanism which enables individual living organisms to reidentify other substances by tracing their spatio-temporal trajectories.

I shall be coming back to incommensurability, but let me for now set it aside in order to sketch the developmental framework within which it functions. Since I must again move quickly and often cryptically, I begin by anticipating the direction in which I am headed. Basically, I shall be trying to sketch the form which I think any viable evolutionary epistemology has to take. I shall, that is, be returning to the evolutionary analogy introduced in the very last pages of the first edition of *Structure*, attempting both to clarify it and to push it further. During the thirty years since I first made that evolutionary move, theories of the evolution both of species and of knowledge have, of course, been transformed in ways I am only beginning to discover. I still have much to learn, but to date the fit seems extremely good.

I start from points familiar to many of you. When I first got involved, a generation ago, with the enterprise now often called historical philosophy of science, I and most of my coworkers thought history functioned as a source of empirical evidence. That evidence we found in historical case studies, which forced us to pay close attention to science as it really was. Now I think we overemphasized the empirical aspect of our enterprise (an evolutionary epistemology need not be a naturalized one). What has for me emerged as essential is not so much the details of historical cases as the perspective or the ideology that attention to historical cases brings with it. The historian, that is, always picks up a process already underway, its beginnings lost in earlier time. Beliefs are already in place; they provide the basis for the ongoing research whose results will in some cases change them; research in their absence is unimaginable though there has nevertheless been a long tradition of imagining it. For the historian, in short, no Archimedean platform is available for the pursuit of science other than the historically situated one already in place. If you approach science as an historian must, little observation of its actual practice is required to reach conclusions of this sort.

Such conclusions have by now been pretty generally accepted: I scarcely know a foundationalist any more. But for me, this way of abandoning foundationalism has a further consequence which, though widely discussed, is by no means widely or fully accepted. The discussions I have in mind usually proceed under the rubric of the rationality or relativity of truth claims, but these labels misdirect attention. Though both rationality and relativism are somehow implicated, what is fundamentally at stake is rather 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. What replaces it will still require a strong conception of truth, but not, except in the most trivial sense, correspondence truth.

Let me at least suggest what the argument involves. On the developmental view, scientific knowledge claims are necessarily evaluated from a moving, historically-situated, Archimedian platform. What requires evaluation cannot be an individual proposition embodying a knowledge claim in isolation: embracing a new knowledge claim typically requires adjustment of other beliefs as well. Nor is it the entire body of knowledge claims that would result if that proposition were accepted. Rather, what's to be evaluated is the desirability of a particular change-of-belief, a change which would alter the existing body of knowledge claims so as to incorporate, with minimum disruption, the new claim as well. Judgements of this sort are necessarily comparative: which of two bodies of knowledge — the original or the proposed alternative — is *better* for doing whatever it is that scientists do. And that is the case whether what scientists do is solve puzzles (my view), improve empirical adequacy (Bas van Frassen's), or increase the dominance of the ruling elite (in parody, the strong program's). I do, of course, have my own preference among these alternatives, and it makes a difference (Kuhn, 1983b). But no choice between them is relevant to what's presently at stake.

In comparative judgements of the kind just sketched, shared beliefs are left in place: they serve as the given for purposes of the current evaluation; they provide a replacement for the traditional Archimedean platform. The fact that they may — indeed probably will — later be at risk in some other evaluation is simply irrelevant. Nothing about the rationality of the outcome of the current evaluation depends upon their, in fact, being true or false. They are simply in place, part of the historical situation within which this evaluation is made. But if the actual truth value of the shared presumptions required for the evaluation is irrelevant, then the question of the truth or falsity of the changes made or rejected on the basis of that evaluation cannot arise either. A number of classic problems in philosophy of science — most obviously Duhemian holism — turn out on this view to be due not to the nature of scientific knowledge but to a misperception of what justification of belief is all about. Justification does not aim at a goal external to the historical situation but simply, in that situation, at improving the tools available for the job at hand.

To this point I have been trying to firm-up and extend the parallel between scientific and biological development suggested at the end of the first edition of *Structure*: scientific development must be seen as a process driven from behind, not pulled from ahead — as evolution from, rather than evolution towards. In making that suggestion, as elsewhere in the book, the parallel I had in mind was diachronic, involving the relation between older and more recent scientific beliefs about the same or overlapping ranges of natural phenomena. Now I want to suggest a second, less widely perceived parallel between Darwinian evolution and the evolution of knowledge, one that cuts a synchronic slice across the sciences rather than a diachronic slice containing one of them. Though I have in the past occasionally spoken of the incommensurability between the theories of contemporary scientific specialties, I've only in the last few years begun to see its significance to the parallels between biological evolution and scientific development. Those parallels have also been persuasively emphasized recently in a splendid article by Mario Biagioli of UCLA (1990). To both of us they seem extremely important, though we emphasize them for somewhat different reasons.

To indicate what is involved I must revert briefly to my old distinction between normal and revolutionary development. In *Structure* it was the distinction between those developments that simply add to knowledge, and those which require giving up part of what's been believed before. In the new book it will emerge as the distinction between developments which do and developments which do not require local taxonomic change. (The alteration permits a significantly more nuanced description of what goes on during revolutionary change than I've been able to provide before.) During this second sort of change, something else occurs that in *Structure* got mentioned only in passing. After a revolution there are usually (perhaps always) more cognitive specialties or fields of knowledge than there were before. Either a new branch has split off from the parent trunk, as scientific specialties have repeatedly split off in the past from philosophy and from medicine. Or else a new specialty has been born at an area of apparent overlap between two preexisting specialties, as occurred, for example, in the cases of physical chemistry and molecular biology. At the time of its occurrence this second sort of split is often hailed as a reunification of the sciences, as was the case in the episodes just mentioned. As time goes on, however, one notices that the new shoot seldom or never gets assimilated to either of its parents. Instead, it becomes one more separate specialty, gradually acquiring its own new specialists' journals, a new professional society, and often also new university chairs, laboratories, and even departments. Over time a diagram of the evolution of scientific fields, specialties, and sub-specialties comes to look strikingly like a layman's diagram for a biological evolutionary tree. Each of these fields has a distinct lexicon, though the differences are local, occuring only here and there. There is no *lingua franca* capable of expressing, in its entirety, the content of them all or even of any pair.

With much reluctance I have increasingly come to feel that this process of specialization, with its consequent limitation on communication and community, is inescapable, a consequence of first principles. Specialization and the narrowing of the range of expertise now look to me like the necessary price of increasingly powerful cognitive tools. What's involved is the same sort of development of special tools for special functions that's apparent also in technological practice. And, if that is the case, then a couple of additional parallels between biological evolution and the evolution of knowledge come to seem especially consequential. First, revolutions, which produce new divisions between fields in scientific development, are much like episodes of speciation in biological evolution. The biological parallel to revolutionary change is not mutation, as I thought for many years, but speciation. And the problems presented by speciation (e.g., the difficulty in identifying an episode of speciation until some time after it has occurred, and the impossibility, even then, of dating the time of its occurrence) are very similar to those presented by revolutionary change and by the emergence and individuation of new scientific specialties.

The second parallel between biological and scientific development, to which I return again in the concluding section, concerns the unit which undergoes speciation (not to be confused with a unit of selection). In the biological case, it is a reproductively isolated population, a unit whose members collectively embody the gene pool which ensures both the population's self-perpetuation and its continuing isolation. In the scientific case, the unit is a community of intercommunicating specialists, a unit whose members share a lexicon that provides the basis for both the conduct and the evaluation of their research and which simultaneously, by barring full communication with those outside the group, maintains their isolation from practitioners of other specialties.

To anyone who values the unity of knowledge, this aspect of specialization — lexical or taxonomic divergence, with consequent limitations on communication — is a condition to be deplored. But such unity may be in principle an unattainable goal, and its energetic pursuit might well place the growth of knowledge at risk. Lexical diversity and the principled limit it imposes on communication may be the isolating mechanism required for the development of knowledge. Very likely it is the specialization consequent on lexical diversity that permits the sciences, viewed collectively, to solve the puzzles posed by a wider range of natural phenomena than a lexically-homogenous science could achieve.

Though I greet the thought with mixed feelings, I am increasingly persuaded that the limited range of possible partners for fruitful intercourse is the essential precondition for what is known as progress in both biological development and the development of knowledge. When I suggested earlier that incommensurability, properly understood, could reveal the source of the cognitive bite and authority of the sciences, its role as an isolating mechanism was prerequisite to the topic I had principally in mind, the one to which I now turn.

This reference to 'intercourse', for which I shall henceforth substitute the term 'discourse', bring me back to problems concerning truth, and thus to the locus of the newly restored bite. I said earlier that we must learn to get along without anything at all like a correspondence theory of truth. But something like a redundancy theory of truth is badly needed to replace it, something that will introduce minimal laws of logic (in particular, the law of non-contradiction) and make adhering to them a precondition for the rationality of evaluations (Horwich 1990). On this view, as I wish to employ it, the essential function of the concept of truth is to require choice between acceptance and rejection of a statement or a theory in the face of evidence shared by all. Let me try briefly to sketch what I have in mind.

Ian Hacking, in an attempt (1982) to denature the apparent relativism associated with incommensurability, spoke of the way in which new "styles" introduce into science new candidates for true/false. Since that time, I've been gradually realizing (the reformulation is still in process) that some of my own central points are far better made without speaking of statements as themselves being true or as being false. Instead, the evaluation of a putatively scientific statement should be conceived as comprising two seldom-separated parts. First, determine the status of the statement: is it a candidate for true/false? To that question, as you'll shortly see, the answer is lexicon-dependent. And second, supposing a positive answer to the first, is the statement rationally assertable? To that question, given a lexicon, the answer is properly found by something like the normal rules of evidence.

In this reformulation, to declare a statement a candidate for true/false is to accept it as a counter in a language game whose rules forbid asserting both a statement and its contrary. A person who breaks that rule declares him or herself outside the game. If one nevertheless tries to continue play, then discourse breaks down; the integrity of the language community is threatened. Similar, though more problematic, rules apply, not simply to contrary statements, but more generally to logically incompatible ones. There are, of course, language games without the rule of non-contradiction and its relatives: poetry and mystical discourse, for example. And there are also, even within the declarative-statement game, recognized ways of bracketing the rule, permitting and even exploiting the use of contradiction. Metaphor and other tropes are the most obvious examples; more central for present purposes are the historian's restatements of past beliefs. (Though the originals were candidates for true/false, the historian's later restatements — made by a bilingual speaking the language of one culture to the members of another — are not.) But in the sciences and in many more ordinary community activities, such bracketing devices are parasitic on normal discourse. And these activities — the ones that presuppose normal adherence to the rules of the true/false game — are an essential ingredient of the glue that binds communities together. In one form or another, the rules of the true/false game are thus universals for all human communities. But the result of applying those rules varies from one speech community to the next. In discussion between members of communities with differently structured lexicons, assertability and evidence play the same role for both only in areas (there are always a great many) where the two lexicons are congruent.

Where the lexicons of the parties to discourse differ, a given string of words will sometimes make different statements for each. A statement may be a candidate for truth/falsity with one lexicon without having that status in the others. And even when it does, the two statements will not be the same: though identically phrased, strong evidence for one need not be evidence for the other. Communication breakdowns are then inevitable, and it is to avoid them that the bilingual is forced to remember at all times which lexicon is in play, which community the discourse is occurring within.

These breakdowns in communication do, of course, occur: they're a significant characteristic of the episodes *Structure* referred to as 'crises'. I take them to be the crucial symptoms of the speciation-like process through which new disciplines emerge, each with it own lexicon, and each with its own area of knowledge. It is by these divisions, I've been suggesting, that knowledge grows. And it's the need to maintain discourse, to keep the game of declarative statements going, that forces these divisions and the fragmentation of knowledge that results.

I close with some brief and tentative remarks about what emerges from this position as the relationship between the lexicon — the shared taxonomy of a speech community — and the world the members of that community jointly inhabit. Clearly it cannot be the one Putnam (1977, pp. 123-38) has called metaphysical realism. Insofar as the structure of the world can be experienced and the experience communicated, it is constrained by the structure of the lexicon of the community which inhabits it. Doubtless some aspects of that lexical structure are biologically determined, the products of a shared phylogeny. But, at least among advanced creatures (and not just those linguistically endowed), significant aspects are determined also by education, by the process of socialization, that is, which initiates neophytes into the community of their parents and peers. Creatures with the same biological endowment may experience the world through lexicons that are here and there very differently structured, and in those areas they will be unable to communicate all of their experiences across the lexical divide. Though individuals may belong to several interrelated communities (thus, be multilinguals), they experience aspects of the world differently as they move from one to the next.

Remarks like these suggest that the world is somehow mind-dependent, perhaps an invention or construction of the creatures which inhabit it, and in recent years such suggestions have been widely pursued. But the metaphors of invention, construction, and mind-dependence are in two respects grossly misleading. First, the world is not invented or constructed. The creatures to whom this responsibility is imputed, in fact, find the world already in place, its rudiments at their birth and its increasingly full actuality during their educational socialization, a socialization in which examples of the way the world is play an essential part. That world, furthermore, has been experientially given, in part to the new inhabitants directly, and in part indirectly, by inheritance, embodying the experience of their forebears. As such, it is entirely solid: not in the least respectful of an observer's wishes and desires; quite capable of providing decisive evidence against invented hypotheses which fail to match its behavior. 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. The point closely parallels the one made earlier about the nature of evaluation seen from a developmental perspective: there, what required evaluation was not belief but change in some aspects of belief, the rest held fixed in the process; here, what people can effect or invent is not the world but changes in some aspects of it, the balance remaining as before. In both cases, too, the changes that can be made are not introduced at will. Most proposal for change are rejected on the evidence; the nature of those that remain can rarely be foreseen; and the consequences of accepting one or another of them often prove to be undesired.

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?

In the penultimate sentence, above. the word 'adaptation' is clearly problematic. Can the members of a group properly be said to adapt to an environment which they are constantly adjusting to fit their needs? Is it the creatures who adapt to the world or does the world adapt to the creatures? Doesn't this whole way of talking imply a mutual plasticity incompatible with the rigidity of the constraints that make the world real and that made it appropriate to describe the creatures as adapted to it? These difficulties are genuine, but they necessarily inhere in any and all descriptions of undirected evolutionary processes. The identical problem is, for example, currently the subject of much discussion in evolutionary biology. On the one hand the evolutionary process gives rise to creatures more and more closely adapted to a narrower and narrower biological niche. On the other, the niche to which they are adapted is recognizable only in retrospect, with its population in place: it has no existence independent of the community which is adapted to it. (Lewontin 1978.) What actually evolves, therefore, is creatures and niches together: what creates the tensions inherent in talk of adaptation is the need, if discussion and analysis are to be possible, to draw a line between the creatures within the niche, on the one hand, and their "external" environment, on the other.

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.

The world-constitutive role assigned here to intentionality and mental representations recurs to a theme characteristic of my viewpoint throughout its long development: compare my earlier recourse to gestalt switches, seeing as understanding, and so on. This is the aspect of my work that, more than any other, has suggested that I took the world to be mind-dependent. But the metaphor of a mind-dependent world — like its cousin, the constructed or invented world — proves to be deeply misleading. It is groups and group-practices that constitute worlds (and are constituted by them). And the practice-in-the-world of some of those groups *is* science. The primary unit through which the sciences develop is thus, as previously stressed, the group, and groups do not have minds. Under the unfortunate title, "Are species individuals?", contemporary biological theory offers a significant parallel (Hull, 1976, provides an especially useful introduction to the literature). In one sense the procreating organisms which perpetuate a species are the units whose practice permits evolution to occur. But to understand the outcome of that process one must see the evolutionary unit (not to be confused with a unit of selection) as the gene pool shared by those organisms, the organisms which carry the gene pool serving only as the parts which, through bi-sexual reproduction, exchange genes within the population. Cognitive evolution depends, similarly, upon the exchange, through discourse, of statements within a community. Though the units which exchange those statements are individual scientists, understanding the advance of knowledge, the outcome of their practice, depends upon seeing them as atoms constitutive of a larger whole, the community of practitioners of some scientific specialty.

The primacy of the community over its members is reflected also in the theory of the lexicon, the unit which embodies the shared conceptual or taxonomic structure that holds the community together and simultaneously isolates it from other groups. Conceive the lexicon as a module within the head of an individual group member. It can then be shown (though not here) that what characterizes members of the group is possession not of identical lexicons, but of mutually congruent ones, of lexicons with the same structure. The lexical structure which characterizes a group is more abstract than, different in kind from, the individual lexicons or mental modules which embody it. And it is only that structure, not its various individual embodiments, that members of the community must share. The mechanics of taxonomizing are in this respect like its function: neither can be fully understood except as grounded within the community it serves.

By now it may be clear that the position I'm developing is a sort of post-Darwinian Kantianism. Like the Kantian categories, the lexicon supplies preconditions of possible experience. But lexical categories, unlike their Kantian forebears, can and do change, both with time and with the passage from one community to another. None of those changes, of course, is ever vast. Whether the communities in question are displaced in time or in conceptual space, their lexical structures must overlap in major ways or there could be no bridgeheads permitting a member of one to acquire the lexicon of the other. Nor, in the absence of major overlap, would it be possible for the members of a single community to evaluate proposed new theories when their acceptance required lexical change. Small changes, however, can have large-scale effects. The Copernican Revolution provides especially well-known illustrations.

Underlying all these processes of differentiation and change, there must, of course, be something permanent, fixed, and stable. But, like Kant's *Ding an sich*, it is ineffable, undescribable, undiscussible. Located outside of space and time, this Kantian source of stability is the whole from which have been fabricated both creatures and their niches, both the "internal" and the "external" worlds. Experience and description are possible only with the described and describer separated, and the lexical structure which marks that separation can do so in different ways, each resulting in a different, though never wholly different, form of life. Some ways are better suited to some purposes, some to others. But none is to be accepted as true or rejected as false; none gives privileged access to a real, as against an invented, world. The ways of being-in-the-world which a lexicon provides are not candidates for true/false.

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