Abstract

I do not believe there are any "special sciences" in Fodor's sense. I think that there is a large group of sciences I will call them "historical sciences" that differ fundamentally from the physical sciences because they quantify over a different kind of natural or real kind than do the physical sciences. Moreover, the laws, or better, the generalizations, that these kinds support are not exceptionless. But heterogeneity is not characteristic of these generalizations. Indeed, I argue, the idea that there could be an univocal empirical science that ranged over multiple realizations of a functional property is quite problematic. For example, if psychological predicates name multiply realized functionalist properties, then there can be no single science that deals with all items having these properties: human psychology, ape psychology, Martian psychology and robot psychology are necessarily different sciences.

Historical Kinds and the "Special Sciences"

Ruth Garrett Millikan
University of Connecticut

The simplest way for one science to reduce to another is the way cooking laws reduce to chemistry. The cooks' law that if you mix baking soda with something sour it bubbles up is reduced to chemistry by identifying baking soda with $NaHCO_3$, identifying the sour taste with the presence of H^+ ions, adding enough about valences to derive that CO_2 will form if you mix these, and identifying CO_2 with what's in the bubbles. In an influential essay which he called "Special Sciences," Jerry Fodor claimed that there are various sciences that do not reduce in this simple way to more basic sciences (Fodor 1974=1981). Rather than each kind predicate of the special science being identified with a single kind predicate of a more basic science, Fodor claimed that each separate instance of a special science kind is identical to an instance of some lower kind, but not always to an instance of the same lower kind. That is, the kinds of the special science are, in current idiom, "multiply realized" in the lower science. "The physical mechanisms whereby events conform to the laws of the special sciences are heterogeneous" (Fodor 1981 p. 138). When their realizing mechanisms are heterogeneous, Fodor claimed, the laws of the special sciences are not reduced to "proper laws" of physical science. Suppose that the special science predicate Π is coextensive with the physical predicate *PwQwR* and the special science predicate *Σ* is coextensive with physical predicate *SwTwU,* and suppose that the law *...Πx...* | *...Σx...* holds because the laws *...Px...* | *...Sx...* and ...*Qx...* | *...Tx...* and ...*Rx...* | *...Ux...* each hold. That is, each of the various possible realizations of *Πx* involves a physical predicate that figures in the antecedent of a physical law whose consequent involves some physical realization of *Σx*. Then to the law *...Πx...* | *...Σx...* there corresponds the truth that if *(...Px...w ...Qx...w...Rx...)* then *(...Sx...w...Tx...w...Ux.....)*. But this lower level truth does not correspond to a "proper law," Fodor says, because disjunctive predicates do not correspond to "natural kinds", hence the law *...Πx...* | *...Σx...* is not reducible in the classical way.

The "special sciences," according to Fodor, have a second important characteristic. "I assume," Fodor says, "that the laws of basic science are strictly exceptionless, and I assume that it is common knowledge that the laws of the special sciences are not" (p. 141). In his "Special Sciences" paper, Fodor claims there is a close connection between these two properties of the special sciences. The reason the laws of the special sciences can have exceptions whereas the laws of more basic sciences do not is because it need not be the case that absolutely every lower level realization of *Π* is in fact an example of *P* or *Q* or *R* or of any other lower level predicate that leads lawfully to a realization of *Σ*. A reasonable question might be, what then would make *...Πx... | ...Σ...* be a "proper law" rather than a mere accidental generalization? If not all exemplifications of *Π* need be such as to cause exemplifications of *Σ*, can it be more than accidental that any of them do? Indeed, Fodor later changed his tack, claiming instead that special science laws are subject to exception because the lower level laws that explain them are themselves merely ceteris paribus or "hedged" laws. That is, typically, the laws *Px... | Sx...* and *Qx... | Tx...* and *Rx.... | Ux....* are each incomplete laws, making implicit reference to further completing conditions which would turn them into strict laws (Fodor 1991). On this reading, however, there is no obvious connection between the fact that special science laws are realized heterogeneously and the fact that they fail to be exceptionless Indeed, Stich, in (1991) suggested a tension between these two supposed properties of special sciences which it is not clear that Fodor was able to resolve (Mott 1992). But the idea that there does exist an important group of sciences exhibiting both heterogeneity and non-exceptionless laws, among which, paradigmatically, is intentional psychology, has stuck with us.

I do not believe there are any "special sciences" in Fodor's sense. I think that there is a large group of sciencesSSI will call them "historical sciences"SSthat differ fundamentally from the physical sciences because they quantify over a different kind of natural or real kind than do the physical sciences. Moreover, the laws, or better, the generalizations, that these kinds support are not exceptionless. But heterogeneity is not characteristic of these generalizations. Indeed, I will argue, the idea that there could be an univocal empirical science that ranged over multiple realizations of a functional property is quite problematic. For example, if psychological predicates name multiply realized functionally defined properties, then there can be no single science that deals with all items having these properties: human psychology, ape psychology, Martian psychology and robot psychology are necessarily different sciences.

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In "Special Sciences," Fodor told us that a truth of the form, *if* (...Px...w ...Qx...w...Rx...) *then (...Sx...w...Tx...w...Ux...)*does not correspond to a "proper law" because disjunctive predicates do not correspond to "natural kinds." About "natural kinds" he said only, "roughly, the kind predicates of a science are the ones whose terms are the bound variables in its proper laws" (1981 p. 132) and about "proper laws," only that they govern relations between "natural kinds". We can begin where Fodor left off, by examining more closely the question what a "proper law" is and what sorts of kinds there are for proper laws to govern.

Proper laws must be distinguished in this context at least from non-empirical generalizations on the one hand and from accidental empirical generalizations on the other. Concerning the first, although the laws of logic and mathematics are certainly multiply realized, what Fodor had in mind as laws of "special sciences" are empirical laws. For example, general truths that might be logically derivable merely from the definition of a functionally defined predicate would not count as proper laws governing its instances in the sense required. I will return to this later.

Regarding the requirement that proper laws not be merely accidental empirical generalizations, I am going to bypass the traditional Hume-inspired question about the difference between true but accidental universal empirical generalizations and genuine natural laws, and simply assume there is such a distinction, in nature, the reality of which is unaffected by the fallibility of our epistemic capacities to track it. I will put this distinction intuitively (and circularly) by saying that in the case of a proper law, there is a reason why the corresponding empirical generalization holds, which reason lies in the natures of the antecedent and consequent conditions of the law, rather than in the accidental positioning of exemplifications of these conditions along with other things in the historical order. Similarly, I will assume that being projectible is a matter of the intrinsic nature of a property, to be distinguished from accidental continued coincidences in its occurrences. "If verificationist criticisms of talk about unobservables are rejected...then there is nothing more problematic about talk of causal powers than there is about talk of electrons or electromagnetic fields" (Boyd 1989). Assuming all this, I want to focus on another question concerning projectability which we might frame in this way: what is it for the domain over which the predicates and laws of a science range to be a domain over which predicates are projectible?

This brings us to a narrower and more traditional sense of the term "natural kind" than Fodor employed. In this more traditional sense, a natural kind corresponds not just to a projectible predicate, but must figure as the subject of many empirical generalizations. No science consists of a single generalization, nor of a heap of generalizations about different kinds of things. A science begins only when, at minimum, a number of generalizations can be made over instances of a single kind, for example, over instances of silver, or instances of humans, or instances of massive bodies, or instances of, say, moments in the American economy. For those disciplines systematic enough to be clearly labeled as sciences, the kinds studied typically belong also to some higher category being, say, kinds of chemical, kinds of animal, or kinds of national economy, and so forth, each higher category supporting generalizations of the same or similar types. For example, for the most part samples of each element and compound that the chemist studies are uniform with respect to melting and boiling point, specific heat, quantitatively expressed dispositions to combine chemically, tensile strength, color, odor, electrical conductivity and so forth. Similarly, for the most part each of the species that the zoologist studies is uniform with respect to approximate size of adults, color, variety and placement of internal organs, numerous physiological traits, behavioral repertoire, conditions that will sustain its life, and so forth. The result is that in the case of many sciences, observations need to be made of only one or a very few exemplars of each kind studied in order to determine that certain properties are characteristic of the kind generally. If I have determined the boiling point of diethyl ether on one pure sample, then I have determined the boiling point of diethyl ether. If the experiment needs replication, this is not because some other sample of diethyl ether might have a different boiling point but because I may have made a mistake in measurement. Similarly for determining the placement of the kidneys or the number of the chromosomes in Rana pipiens. Second order inductions of this sort underlie all of what Kuhn labeled "normal science."

In the 1989 Oberlin colloquium, Ian Hacking and Richard Boyd agreed that the term "natural kind" has historically been used to characterize kinds over which numerous reliable inductive generalizations can be made (Hacking 1991a, Boyd 1991). Hacking claimed that there is a variety of different kinds of natural kinds, distinguishing for comment (1) Russellian kinds, (2) Mill-Kinds (the capitalization is in Mill), (3) Peirce-kinds and (4) Leibniz-kinds. We can begin by looking at these various kinds, asking which ones can support genuine empirical sciences. To distinguish the empirical-science-supporting kinds from other sorts of natural kinds I will echo Fodor, calling them "proper natural kinds."

As Hacking draws it, the distinction among Peirce-kinds, Mill-Kinds and Leibnitz-kinds is explicitly "epistemological", but we can easily project it onto the ontological plane. There Both Peirce-kinds and Leibnitz-kinds appear as having certain essential properties from which all the other properties of the kind follow by natural law, whereas Mill-Kinds do not have this structure. In the case of Peirce-kinds, the various properties of the kind are are explainable by reference to laws over relatively superficial properties, whereas the properties of Leibnitz-kinds are explainable by reference to an underlying structure common to members of the kind. (Some may find it easiest to think of these latter as "Putnam-kinds".)

So understood, Peirce-kinds and Leibniz-kinds are clear examples of proper natural kinds. This is because there is a reason for the various empirical generalizations holding over the members of the kind which lies in the nature of the kind. There is a reason why the members of the kind are like one another in a variety of respects. Contrast the Mill-Kind that is jade. As Putnam reminded us, jade is either of two minerals, nephrite and jadeite, which have many properties in common but not for any univocal reason. Rather, each has these properties for its own reasons. Similarly, Putnam's earth water (H_2O) and twinearth water (XYZ) were conceived as having numerous observable properties in common, but not in common for any univocal reason. Inductive inferences from the properties of samples of nephrite applied to samples of jadeite, when the conclusions happen to come out true, are not true for a reason grounded in a common nature. There is no ontological <u>ground</u> of induction underlying such inferences. Jade is not a possible kind for the generalizations of any empirical science to range over. Nor, if Putnam's twinearth story were true, would generic water, conceived to be multiply realized either as H_2O or XYZ, be a proper natural kind. At least some Mill-Kinds, then are not proper natural kinds, and it would be easy though, I will soon argue, mistaken, to conclude that none are.

For Peirce-kinds and Leibniz-kinds, the ontological ground of induction for the kind, that is, the reason for the samenesses among the instances, lies in the intrinsic natures of the members of the kind. Relative location in historical time and space plays no role in explaining the likenesses. I will call proper natural kinds of this sort "eternal natural kinds," later distinguishing them from "historical natural kinds." The various branches of physics and of chemistry concern eternal natural kinds exclusively, assuming that we take such disciplines as historical geography and cosmogony to be applications rather than branches of physics and chemistry. Astronomy, in so far as it deals with the various <u>kinds</u> of bodies in outer space (astrophysics) rather than with the placement and interactions among specific historical bodies, concerns eternal kinds. We can call these sciences "eternal sciences."

Hacking also makes a further distinction between "Russellian kinds" and others. First, Russellian natural kinds must have a number of properties in common. This requirement we have already affirmed for our proper natural kinds, adding that they must have these properties in common for a univocal reason. But not all members of a Russellian kind need have all of the properties characteristic of the kind. Although Russell began by characterizing a natural kind as "a class of objects all of which possess a number of properties..." Hacking remarks that Russell "was aware that his 'all'...won't quite do. Manx cats don't have tails." And he adds, "[Russell] made a rather charming comparison between natural kinds and topological neighborhoods, saying that the former may be thought of as intensional neighborhoods, in which every member is close to a great many other members according to some notion of closeness to be explained" (p. 112, referring to Russell 1948). Hacking next contrasts kinds of this "topological neighborhood" sort with kinds best characterized with "the metaphor of family resemblance, cluster, strands in a rope or whatever" (p. 115). He does not explain the contrast, but the suggestion, made more definite in his reply to Boyd (Hacking 1991b), is that "family resemblances" are traced out not by any objective clustering of all properties of the items seen to resemble, but by patterns of likeness of interest specifically for human purposes. If we define the notion "family resemblance" that way, then it is clear that family-resemblance kinds should not be included among our "proper natural kinds". But what about Russell's topological-neighborhood kinds? Unlike family resemblance kinds, these are characterized specifically as "natural kinds" by Hacking. They are clustered together by nature not us. Might any of them be "proper natural kinds" in our sense?

Hacking inclines to think that natural kinds of the topological neighborhood sort, including, for example, the various biological species, will not eventually figure in the "well developed" sciences. Why? He is not explicit about his reasons, but perhaps they are these. Topological neighborhood kinds cannot also be either Peirce-kinds or Leibniz-kinds, that is, eternal kinds. Eternal kinds are held together by universal and eternal laws of nature that determine the various properties of the kind from central intrinsic properties, say, from an inner structure common to all members of the kind, so all the true properties of the kind are had of necessity. If cats were an eternal kind, Manx cats would have to have tails. On the assumption that well developed sciences will deal only with kinds explainable from eternal natural laws, well developed sciences will not deal with topological-neighborhood kinds.

But why would well developed sciences deal only with kinds whose integrity is explainable from eternal natural laws? Well, the thought seems to be, any other uniformities must ultimately be accidental uniformities, arising from the accidental disposition of things in historical space and time. And developed empirical sciences do not deal with what is accidental, but only with what is naturally necessary. Therefore the rough uniformities that collect the topological-neighborhood kinds into groups must be, in part, accidental uniformities, hence will not be dealt with by developed sciences. I think something like that is the underlying thought, but I will argue that it is mistaken.

There is a modes tollens complement to this modes ponens argument, just as mistaken, which I suspect underlies Fodor's claims about the special sciences: since there are many perfectly good sciences that do deal with kinds that are not eternal kinds, it must be that perfectly good sciences can indeed deal with uniformities that are accidental. For example, here is Fodor at the end of his "Special Sciences" paper:

> A way of stating the classical reductionist view is that things that belong to different physical kinds ipso facto can have none of their projectible descriptions in common: that if x and y differ in those descriptions by virtue of which they fall under the proper laws of physics, they must differ in those descriptions by which they fall under any laws at all. But why should we believe that this is so? Any pair of entities, no matter how different their physical structure, must nevertheless converge in indefinitely many of their properties. Why should there not be, among these convergent properties, those whose lawful interrelations support the generalizations of the special sciences? (1981, p. 144-5)

That is, accidental historical convergences support laws of the special sciences.

J. S. Mill said, about his "Kinds" that "a hundred generations have not exhausted the common properties of animals or plants... nor do we suppose them to be exhaustible, but proceed to new observations and experiments, in the full confidence of discovering new properties which were by no means implied in those we previously knew" (from Hacking, p. 118). Are we to understand this confidence as grounded merely in accidental historical convergence? Clearly Mill has in mind that it is answered in nature by a supporting natural ground of induction. Mill's Kinds are supposed to be genuinely projectible kinds, not the result of accidental correlations, accidental heaps of piled up properties. How, then, can there be projectible kinds, suitable for building sciences on, whose integrity is not explainable as following from eternal natural laws, moreover, not all of whose members actually have all of the properties that characterize the kind? (Manx cats do not have tails.)

Richard Boyd began to answer this question in his comment on Hacking's paper at the 1989 Oberlin colloquium. Referring to Boyd (1989), he introduced what he termed "homeostatic property cluster kinds" which are such that "the property-cluster [compare topographical-neighborhood] which defines them is <u>causal</u> <u>rather</u> <u>than</u> <u>conceptual</u>" (1991 p. 141). Boyd's explicit example of these kinds are biological species, but he also suggests that social kinds may have a similar structure.

Boyd begins by saying of the co-occurrence of the properties in such a property cluster that it is "at least typically, the result of what may be metaphorically (sometimes literally) described as a sort of <u>homeostasis.</u> Either the presence of some of the properties...tends (under appropriate conditions) to favor the presence of the others, or there are underlying mechanisms or processes which tend to maintain the presence of the properties...or both...Imperfect homeostasis is nomologically possible or actual: some thing may display some but not all of the properties..." (1989 p. 16). So far (there is more to come) this sounds as if the lawful interdependence of various deep and\or surface properties of the kind was the glue holding homeostatic cluster kinds together, thus inviting assimilation of these kinds to eternal kinds. It sounds as if the same kinds we have in our world might be found in other nomologically possible worlds as well, for example, the persons on twinearth would also be members of the species <u>Homo sapiens</u>. In that event, the question would remain unanswered, how it is, where "imperfect homeostasis" is possible (where some members have only some of the kind's properties), that the degree of uniformity that does obtain across the kind can be more than accidental. Why are homeostatic kinds not either, on the one hand, "perfectly homeostatic," hence eternal Peirce-kinds, or on the other, the result of large scale accident in historical circumstance?

But Boyd continues. After various remarks about the naturalness and extensional vagueness of homeostatic cluster kinds, he claims that "the property cluster is individuated like a (type or token) historical object or process..." and speaks of "the historical development of the property cluster and the causal factors that produce it" (1989, p. 16). He tells us that "[t]he definitional role of mechanisms of homeostasis is reflected in the role of interbreeding in the modern species concept; for sexually reproducing species, the exchange of genetic materials between populations is thought to be essential to the homeostatic unity of the other properties characteristic of the species and it is thus reflected in species definitions" (1991, p.142). It seems that Boyd's homeostatic property cluster kinds are not eternal kinds after all, but historical. Can we say more clearly what holds them together if not an eternal essence?[1]

Notice that the role of interbreeding cannot be quite as central as Boyd suggests here, since nonsexual as well as sexual species remain stable in their properties over long periods of time, and so do species in the plant world despite the fact that hybrids can be readily introduced over vast ranges of different plant species. Combinations of other factors (not yet well understood) must be equally responsible for maintaining the separations between and continuities within the various species. What the reference to interbreeding <u>does</u> do, however, is effectively to confine each species to an historical location in this world. Similarly for the reference to lineage in all but the most radical cladists' attempts at defining both species and higher taxa. Cats must, first of all, be born of cats, mammals must have descended from a common ancestor, and so forth. Biological kinds are defined by reference to historical relations among the members, not, in the first instance, by reference to properties. Biological kinds are, as such, historical kinds.

Return now to the main question before us: how can there be domains over which predicates are projectible, domains suitable for building sciences on, whose integrity does not follow from eternal natural law, moreover, whose non-accidental characteristics are not universal over the kind? Here, I submit, is the answer. The members of these kinds are like one another because of certain historical relations they bear <u>to</u> <u>one</u> <u>another</u> (that is the essence) rather than by having an eternal essence in common. It is not just that each exhibits the properties of the kind for the same eternal reason. Rather, each exhibits the properties of the kind because other members of that same historical kind exhibit them. Inductions made from one member of the kind to

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[1] I am not at all clear how Boyd meant to put homeostasis and history together to produce his homeostatic cluster kinds. So I am not clear whether the next few paragraphs are best read as exegesis of Boyd or as criticism.

another are grounded because there is a certain historical link between the members of the kind that causes the members to be like one another. And what sorts of links, what sorts of reasons, might these be? The two most obvious reasons, these typically being combined, are (1) that something akin to reproduction or copying has produced all the various kind members from one another or from the same models (e.g. from genes replicated from the same gene pool) and (2) that the various kind members have been produced in or in response to the very same ongoing historical environment. Notice how different this is from saying that various properties and/or underlying mechanisms in each member of the kind produce other properties of that member, or stabilize one another in the individual. Homeostasis, when it is important, is so not by operating in the individual, but by operating in the gene pool over time, inhibiting the introduction of mutations that don't fit well with what is already there (Millikan 1984 chapter 17). Thus it keeps the reproducing or copying relatively faithful over periods of time, so that the kind does not do as Achilles' horse did and "run off in all directions" but remains relatively stable in its properties, maintaining its integrity as a kind.

A kind of this sort is not an eternal kind. As M.T. Ghiselin (1974) and David Hull (e.g. 1978) have said of the various animal species, they are not "spatio-temporally unrestricted classes" but more like big sprawling scattered individuals. But Hull was wrong to think that because species are historical entities, "their names function in no scientific laws." On a reasonable reading, a valid scientific law is just a true, well grounded, hence non-accidental, generalizationSSwell grounded, that is, not just in logic but in ontology. As Boyd observed in his Oberlin essay, the basic principles of good scientific induction are not found in logic alone; all inductive reasoning rests on a posteriori projectability judgments. Historical kinds are domains over which predicates are non-accidentally projectible: there are good reasons in nature why one member of an historical kind is like another, hence why inductions are successful over the kind.

On the other hand, historical kinds are unlikely to ground exceptionless generalizations. The copying processes that generate them are not perfect, nor are the historical environments that sustain them steady in all relevant respects. Moreover, as Boyd has argued, these kinds often have naturally and irreducibly vague boundaries.

Besides biological taxa, there are many other historical kinds. In (Millikan 1984) I spelled out why the 1969 Plymouth Valiant 100 was a real historical kind, there calling it a "secondary substance":

> ...in 1969 every '69 Valiant shared with every other each of the properties described in the '69 Valiant's handbook and many other properties as well. And there was a good though complicated explanation for the fact that they shared these properties. They all originated with the selfsame planSSnot just with identical plans but with the same plan token. They were made of the same materials gathered from the same places, and they were turned out by the same machines and the same workers...or machines similar and workers similarly trained [on purpose] ...[Hence all the Valiants] had such and such strengths, dispositions and weaknesses...placement of distributor... size of piston rings...shape of door handles....Valiants, like most other physical objects, are things that tend to persist, maintaining the same properties over time in accordance

> with natural conservation laws. ...Also, there are roughly stable prevailing economic and social conditions...in accordance with which working parts of automobiles tend to be restored and replaced with similar parts...
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> [The Valiant also] has an identity relative to certain kinds of conditional properties...For example, the fenders of the '69 Valiant that has not been garaged tend to rust out whereas the body stands up much better; the ball joints are liable to need replacing after relatively few thousands of miles whereas the engine...is not likely to burn oil until 100,000 miles... (p. 279-280)

Relatively few historical kinds furnish subject matter for science, however, partly because relatively few (unlike animal species and chemical kinds) fall neatly into higher kinds that furnish general <u>a posteriori</u> principles of induction. Relatively few are such that one can tell in advance details about which determinables can be projected over the kind. Moreover, relatively few have numerous and interesting properties in common, or have these with high regularity. For example, consider chairs. Surely it is not reasonable to project a science or sciences of furniture, but there are historical reasons why historical chairs are much alike in a number of respects. They have been designed to fit the physical dimensions and practical and aesthetic preferences of humans, who are much alike in relevant respects for historical reasons. Moreover, the majority of chairs have not been designed from scratch, but copied from previous chairs that have satisfied these requirements.[2] They thus form a rough historical kind: there are reasons that go well beyond (mysteriously agreed on) points of definition why one knows roughly what to expect when someone offers to bring a chair. Similarly, when someone offers to take one to see a Romanesque church. In a similar vein, Crawford Elder has argued for a collection of natural kinds based on copying, historical context and teleofunction, examples being household screwdrivers and stickleback mating displays.[3] One might argue that even Californians form a very rough or vague historical

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[2] The example is from Frank Keil (1989, p. 46-7).

[3] Elder describes these kinds as though they were eternal kinds, brought together by their <u>type</u> of history rather than by the historical relations of the members to one another. Kinds of this sort, however, could not have any natural boundaries, since

kind. They are of the same species, many have copied behavioral patterns from one another, they have been subject to social and physical environmental influences from the same sources, hence certain rough generalizations can be made over them for good reason. There is a long, graded continuum, then, between historical kinds suitable to project sciences overSS"proper historical kinds"SS and a great variety of less interesting historical kinds that are nonetheless not nominal but "real."

At the Oberlin colloquium, Hacking and Boyd drew our attention to kinds that interest social scientists, for example, ethnic, social, economic and vocational groups. Boyd claimed that members of such groups sometimes exhibit properties characteristic of the group largely as a causal result of being classified into these groups rather than conversely, but that this does not compromise these social kinds as possible scientific objects. Such groups are, indeed, proper historical kinds. Their members are likely, for example, to experience similar training handed down from member to member in the group (copying), to participate in the same customs handed on from the group, to experience social and/or legal pressures toward conformity originating from the same sources (including pressures that result from being considered a group member), in general, to be molded by what is relevantly numerically the same environment. On the other hand, in so far as social scientists sometimes generalize across radically different cultures, not just, say, across Western cultures, the common historical thread in studying kinds of social groups is mainly just human psychology, the common psychological dispositions of the historical species Homo sapiens.

Let us return then to the science of psychology, queen of Fodor's designated "special sciences," on which foundation surely all the other social sciences are built. What is its subject matter? Over what do its laws quantify? What sort of kinds serve to ground its generalizations? What makes successful inductions over its instances non-accidentally possible?

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all possible sets of fine grained properties, historical context types, and teleofunctions surely merge imperceptibly into one another. Compare the argument against a kind including both Swampman and yourself in (Millikan 1996, part 2).

The teleofunctions of those historical kinds that have them could indeed be viewed, as Elder says, as "essential properties" of the kind, for the selection processes that define a teleofunction, parallel to the homeostasis in the gene pool of a biological species, is what prevents inaccurate copying from dispersing the kind in all directions. It is the glue that holds the kind together.

Let us assume, with Fodor, that psychological predicates name functional (perhaps traditional functionalist, or perhaps teleofunctionalist) properties, these being, in principle, multiply realizable. A tempting mistake is to reason as follows: psychology concerns lawful relations among instantiations of psychological properties; psychological properties are multiply realizable; so psychology concerns lawful relations among properties which may be multiply realized. But where has it been demonstrated that there is single science that concerns psychological properties wherever found? Indeed, is it at all plausible that there is a single science that ranges over the domain of all objects that have psychological properties?[4]

By a single science we must mean, of course, a single empirical science. Suppose, for example, that psychological properties typically have complex functional definitions, best stated in complicated Ramsey sentences making explicit their causal relations to other psychological properties. Then objects having these properties will each have to fall under a great many empirical laws, basic and derived, and there might be a discipline that traced out what all these laws were. But such a science would be a priori not empirical. For each object having such psychological predicates it would be independently necessary that it conform to all the laws in order to be counted as having any of the predicates. That would be a matter of definition. But the fact that each of several objects conforms to one and the same set of functionally defined laws does not by itself entail that there is any non-accidental ground underlying this similarity, that there is an univocal reason why they do. It does not entail membership in a single proper kind, hence that anything can legitimately be projected about the behavior of any of these objects by observing the behavior of others. Compare, for example, various disciplines that apply the same mathematical models to different subject matters, say, the mathematics of economic bargaining models applied to the evolution of animal behaviors. The fact that parts of the mathematical structures of two sciences are isomorphic does not make empirical generalization possible from one science to the other. Similarly for isomorphisms in functional structure.

Could the same empirical science then encompass, say, the psychology of humans, of Martians, and of some variety of intelligent robots? Assuming, that is, that the principles of the robots' design were not copied from the humans or from the Martians? A suggestion has recently been made both by Papineau (1992) and by

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[4] Both Enç (1983) and Schwarts 1992) have argued that, as Enç puts it, "the proper objects of psychology are human beings" (p. 290), but both seem to take it that what psychology ranges over is a matter of stipulation of the meaning of psychological terms rather than a substantial question about what sort of empirical sciences are possible.

Macdonald (1992) that multiple realizations of a functional property can be expected to arise exactly when the function has been selected for. Papineau and Macdonald formulate the following problem and offer the same solution. Functional properties are not defined merely in terms of their relations to other functional properties but also by their relations to physically specified inputs and outputs of the system they characterize. But how can we account for the strange coincidence that a certain kind of physical state S_1 always leads to another S_2 yet this connection gets made via entirely different mechanisms on different occasions? The answer, they claim, is that these different mechanisms have each been selected, either by natural selection or by trial and error learning, precisely to make it possible to move from S_1 to S_2, however, the materials at hand to be selected from were different in each case.

Surely this is the reason why different mechanisms sometimes subserve the same functions in the biological world, for example, why some animals achieve sight with lens eyes while others have compound eyes, and why astonishingly similar swimming motions are achieved by the fish, the penguin, the alligator and the otter using entirely different bones and muscles. This explains how it might happen that certain functional properties become multiply realized. But our question is not how a variety of different objects might come to exhibit the same functional property, but whether these objects would then form a proper natural kind over which inductions to further functional properties would be grounded. That a variety of objects all exhibit the same functionalist property for the same reason would not seem, by itself, to imply that they are alike in any other respects.[5]

[5] Papineau remarks that every hot water heater has a thermostat but that these thermostats are constructed in a variety of different ways. The wintertime hot water heater I grew up with did not in fact have a thermostat, being but a special water tank built over our coal furnace, nor did the hot water heater that automatically turned on and off with the hot water tap in the kitchen in our English flat have one. In camp we heat the water for showers in a black plastic container made for that purpose and placed in the sun. Possibly none of these devices would now be advertised in the yellow pages under the label "hot water heater" (though in England I am not so sure), for in that context, having a way to keep the water at constant temperature may nowdays be taken for granted by everyone. But if all hot water heaters have thermostats in that context, it is a matter of definition in that context, not empirical law.

Is the idea, perhaps, that there are proper kinds formed as a result of inhabiting exactly the same sort of ecological niche, being subject to the same quite specific selection pressures, demanding the development of certain exact whole sets of functionally defined properties if they are to avoid extinction? Besides the implausibility of including intelligent Martians in the same ecological niche with humans while excluding all other terrestrial animals, this idea suffers from a misunderstanding of the role of an evolutionary niche. An evolutionary niche is not something that a species finds itself in and must then respond to, but something it creates for itself as it evolves by random mutation. (Why aren't house flies in the same evolutionary niche that we are? Why don't they respond and get smarter?) Pairs of unrelated species in similar niches often do display some analogous characteristics, presumably for good reason, but occasional illuminating comparisons across species are not laws about the causal powers of niches. (Frogs do not swim underwater with the same motions as fishes.)

The fact that psychological properties are multiply realized does not imply that the laws of any science that concerns psychological properties are multiply realized. Nor does the Papineau-Macdonald suggestion help us to see how a single science of psychology, a single set of non-accidental psychological generalizations, could extend across any hostorically unrelated species such as humans and Martians. That there could be a single science of psychology that stretched across rational beings merely <u>as such</u> is quite out of the question.

An entirely different question concerns whether there is reason to think that psychological properties are multiply realized in humans, and if so, whether a single set of grounded generalizations could cover all their instances. According to Fodor, "it is entirely possible that the nervous system of higher organisms characteristically achieves a given psychological end by a wide variety of neurological means" (1981, p. 135). He made the same claim much more elaborately in (Fodor 1968). In neither case, however, did he offer any empirical support. The claim was argued for as a logical possibility, not as an empirical fact, or even as a reasonable empirical hypothesis. Is it in fact empirically plausible?

As a preliminary, notice that contrary to Fodor's remarks in "Special Sciences," the non-strictness of psychological laws does not bear on whether their realizing mechanisms are heterogeneous. Generalizations over historical kinds are probably never strict. Psychological generalizations quantifying over humans are about dispositional properties of humans. Offhand one might suppose that they would be no more likely, or unlikely, to be universally realized in the species than various physiological dispositions. That is, some would nearly always be realized and others less often, or realized in varying degrees. This would be so whether or not the mechanisms of realization for each disposition were heterogeneous.

Why suppose then that these mechanisms would be heterogeneous? Are human physiological mechanisms multiply realized? When they exercise, do different people's hearts beat faster and do they sweat for different reasons? Are their knee jerk reflexes explained by different principles, or do they occur in the same person for different reasons on different days? Do different people digest their food with different enzymes? Why should the various functions of brains be multiply realized more than the functions of the rest of the body?

Crucial here is what "multiple realization" is supposed to mean in this context. For example, is liver function multiply realized in humans because some people's livers are larger than others and have more cells? Are verbal abilities multiply realized if the neural structures responsible for them, though operating in exactly the same manner, occur more bilaterally in some people than others? In a well known passage, Putnam (1975) analogized the multiple realization of functional properties to the multiple configurations of colliding atoms that might cause a square peg to refuse to go through a round hole. Did he actually intend this as an example of multiple realization of a functional property? Then, it would seem, having a low center of gravity would be a multiply realizable functional property too? Jackson and Petit (1990) analogize existentially generalized properties, such as some of its atoms are decaying, to functional propertiesSSbecause any of various individual atoms might be the ones that are decaying. Are Newton's laws also multiply realized because sometimes it is atoms of gold and sometimes of lead that make up the masses to which they apply? Or because sometimes the atoms are arranged in crystalline structure and sometimes not? Or because it might be the atoms named Sally and Mike that help make up the mass or it might be the atoms named Betty and Michael?

Clarification is surely needed in this area, nor will I attempt much of it here. But something like the following distinction seems to be required. Sometimes different mechanisms that accomplish the same operate in accordance with different principles; other times they represent merely different embodiments of the same principles. Or we might say, sometimes looking more closely at the mechanism helps to explain how it works; sometimes it reveals only what stuff it is made of. It is only the former kind of difference that makes interesting "multiple realizability." What then is the argument that the same functional properties are realized in accordance with different principles in different humans, more so for psychology, say, than for physiology?

The only argument I can see returns us to Papineau and Macdonald's claim, but narrowed now to encompass only human learning, that is, to exclude natural selection. Different individual humans often learn to accomplish exactly the same things using quite different methods. They use different methods to recognize the same objects, relying on different properties of these objects (dramatic example: Helen Keller). They learn how to perform the same physical manipulations, such as writing with a pencil, using different methods, different grips. They learn to get what they want from fellow humans in different ways, some through charm, some through behavioral vestments of authority, some through veiled or unveiled threats, some through tears. They learn how to get to New York by different routes, some using the Merritt parkway, some the interstates, some by train, some by bus. When faced with how to get the children to soccer at the same time they are due at the dentists, they will hit on different solutions, indeed, the same person might well have hit on different solutions different days, depending on small incidents bringing this or that possibility to mind. And different people perform multiplications in their heads in different ways, depending on what "math facts" they remember easily and what general strategies they find most natural.

Clearly examples can be multiplied indefinitely. Humans do things, including mental things, as they have accidentally learned to do them, given the materials and chance accidents of experience available to them, or they do things the ways they happen to think of doing them, given somewhat random access to their memories, using a generate and test method of projected behavior design. Given this, the same desires harbored by different persons may lead quite reliably to the same results but by different mechanisms. Add to this that humans, as a single historical kind, are born with certain broadly similar functional goals built in. They come into the world with the same sorts of desires, responsive to roughly the same "primary reinforcers". Because of this, they have certain very broad behavioral and psychological dispositions in common, such as the disposition (figure out how) to procure food when hungry, to procure shelter when cold, to procure company when lonely, to develop economic systems of various kinds under appropriate circumstances, and so forth. But these basic dispositions are filled in only through experience, learning, and other stocastically influenced mechanisms, hence are realized in a wide variety of ways.

Multiple realizability, yes, but I think not of the sort Fodor had in mind when he wrote "Special Sciences." There he was thinking, for example, that how modus ponens was done might vary significantly from brain to brain. Interestingly, what we have stumbled on here accords better with his much later views in The Elm and the Expert, where he suggests that the firmest laws of psychology may be wide rather than narrow, and very rough. I have added that they will be laws for humans, not for rational beings generally. [6]

[6] Thanks to Crawford (Tim) Elder for a very helpful reading of this manuscript.

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