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Sex, Gender, and Essence

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INTRODUCTION

The primary aim of this paper is to show why the assumption that real essences underlie the kinds we distinguish in scientific investigation is mistaken. I want to claim that this assumption is not merely empirically unwarranted, but necessarily at odds with a genuinely empirical approach to science. Briefly, unless one supposes the discovery of a kind to imply the discovery of an essence, there is nothing more to the discovery of a kind than the discovery of the correlations of properties characteristic of the members of the kind. Since I do not believe that essences are to be found so easily, I shall argue that the importance of the discovery of kinds to the progress of science is much less than is generally supposed.

Although I shall illustrate this argument with a fairly detailed discussion of some specific classificatory concepts, I would like to emphasize at the outset that the role of this discussion is just that—illustration. It would be possible to understand the basic thrust of this argument by reading only the introduction and the conclusion of the paper. However, the cases that I shall discuss should certainly assist in the understanding of the argument (as well, I hope, as having some intrinsic interest of their own). The significant point about the concept of sex is that although it is undoubtedly a concept that has major significance for biology, and although it is also a concept that divides the natural world into well-defined classes, the scope of the generalizations to which it gives rise is at every stage an empirical matter. In its briefest outline, the force of my argument will be that if a real essence is to serve any purpose, it must at least determine the scope of generalizations covering the entities that realize it. But, for a serious empiricist, there is never any reason to suppose that this can be done.

The case of sex is particularly revealing because despite the extreme sharpness, by biological standards, anyway, of the cleavages it makes in the organic world, it turns out that there is neither evidence nor reason to expect that it gives rise to any generalizations across the broad categories that it defines. The concept of gender, a

concept that has received a good deal of elaboration and clarity from the last fifteen to twenty years of feminist scholarship, has been developed in ways that demonstrate that one cannot assume that the intersection of biological sexual categories with some smaller biological category will give one the appropriate scope for generalization. Even as applied to one species, *Homo sapiens*, the scope of generalizations restricted to male or female is a purely empirical matter and, in most interesting cases, far narrower than that of the entire species.

I shall begin the paper with a brief explanation of what I understand by essentialism, or, at any rate, of the sense in which I shall claim that it is objectionable. I shall also mention some immediate difficulties that it presents in the context of biology. I shall, then, discuss first sexual categories, and then gender categories, as illustration of the impossibility of extrapolating from the existence of a kind to the scope of any generalizations about its members. In the discussion of the latter categories, I shall mention some of the reasons for confidence that the sex/gender distinction cannot be dissolved. In conclusion, I shall argue that the cases considered point to fundamental defects in the essentialist point of view, and that this point of view should be abandoned rather than modified.

ESSENCES

In thinking about essences, it is first necessary to distinguish two very different functions that they may be supposed to serve. First, essences are often conceived as properties that determine the answer to the question to what kind the object that instantiates them belongs. But, second, essences are also thought of as determining the properties and behavior of objects that instantiate them.

Within the first type of function, we can introduce the familiar Lockean distinction between real and nominal essences. The view that nominal essences determine the kinds to which objects belong amounts to little more than the introduction of a bit of technical terminology. A nominal essence is connected to a kind by some sort of linguistic convention. Since it is obvious that we could not refer generically to members of any kind without the existence of some linguistic convention determining the (at least approximate) limits of that kind, the existence of nominal essences as characterized above is not controversial. More detailed description of nominal essences may certainly commit one to more or less powerful semantic theses—for example, to the view that there must be necessary and sufficient conditions for the application of every classificatory term—but will carry no metaphysical commitments.[1] My concern in this paper will be solely with real essences.

To assert that there are real essences is, in part, to claim that there are fundamental properties that determine the existence of kinds that instantiate them. The existence of such properties will have profound metaphysical consequences: in particular, it will imply that the existence of kinds of things is as much a matter of fact about the world as is the existence of particular things. Such kinds are quite independent of our attempts to distinguish them, and their discovery is part of the agenda of science. It is consistent with at least the majority of modern usage to take the previous sentence as

providing a necessary and sufficient condition for the existence of a *natural* kind, and I shall hereafter use that expression in that way. It is important to note, however, that although the existence of a real essence is, then, sufficient to determine the existence of a natural kind (ignoring possible problems about noninstantiation), it does not follow that a real essence is necessary. As a matter of fact, I believe that there are natural kinds without real essences, unless perhaps in an almost vacuously attenuated sense.[2]

I would also like now to emphasize a point that will be central to the argument I have in mind against essentialism. This is the observation that even if a kind *is* determined by a real essence, it is hard to see what route there could be to the *discovery* of essences other than the prior discovery of kinds. This should immediately lead one to entertain serious doubts about the empirical credentials of such essences. In particular, if this epistemological point is correct, we should be very suspicious of any practical consequences that appear to follow from the existence of a real essence. In the remainder of this section I shall develop the concept of a real essence so as to try to show that if the existence of a real essence amounts to anything, it does, indeed, have practical consequences—specifically, in entitling us to anticipate the existence of laws governing the behavior of objects that partake of it. In the following two sections, I shall illustrate the fact that the differentiation of kinds entitles us to no such anticipation. The conclusion that will be developed in the final section of the paper is that discovering kinds does not involve discovering essences; and so, given that there is no other way of discovering them, nothing does.

However, to return to the main thread of the argument, it does seem that, barring the most radical and implausible nominalism, there must be *something* to the doctrine of real essences as so far described. Some kinds are, at the very least, more natural than others. The class of creatures with wings and feathers, for example, is more natural than that of creatures that are gray and over one foot long. This is so because when we know that a creature belongs to the first class, we can make numerous further reliable predictions about it—that it, or its female relatives, lays eggs, is warm-blooded, and so on. Membership of the second class carries no such benefits. Depending on how deeply we can explain such clustering of features, we can adduce more or less powerful characterizations of real essences. If, to take one extreme case, God had simply chosen to assemble creatures in the light of some preconceived ideas of which features went well together, the real essences might amount to no more than conjunctive, or perhaps partially disjunctive, descriptions of God's aesthetic preferences. Since these descriptions would still reflect genuine clusterings of properties, they would at least be natural kinds and would exhibit, in a sufficiently weak sense, real essences. However, we naturally believe that the discontinuities in nature admit of somewhat deeper explanations, and this leads us finally to the second, and much more problematic, function of real essences, that of explaining the nature of the members of the kinds that such essences determine.

The strongest possible notion of a real essence would be that of a property, or group of properties, that determined—and, hence, in principle could be used to explain—all the other properties and behaviors of the objects possessing them. Although such a notion *might* be defensible for individual essences (Locke seems sometimes to

have envisaged the microstructural description of an object potentially playing such a role), it cannot work for the type essences that are my present concern. This is for the simple reason that there is no kind (with the possible exception of the ultimate microphysical kinds), the members of which are identical with respect to all of their properties, even their intrinsic ones. I say "intrinsic" properties because it is obvious that the *behavior* of an object will typically depend on both its intrinsic properties and its external environment. Clearly, the strong view I am considering should claim only that essence determines behavior as a function of some set of external variables, or, in other words, determines precisely specifiable dispositions to behavior. However, variation in intrinsic properties requires a more fundamental retreat from the strong position. Specifically, some distinction between essential and accidental properties, that is, between properties that can and those that cannot vary between members of a kind, is unavoidable.

A promising and natural modification of the strongest conception of real essence, which provides a way of drawing just the distinction mentioned above, is the following: the essence of a kind determines just those properties and dispositions of its instances for which it is a matter of natural law that members of the kind will exhibit those properties or dispositions. The essential properties of members of a kind will then be, first, the real essence itself, and, second, those properties and dispositions nomically determined by the real essence. The rest will be accidental. Thus, for example, it is clearly no law of nature that squirrels are gray, since many are black. On the other hand, perhaps it is a law that squirrels have tails,and, hence, tailedness is an essential property of squirrels. An essentialist holding the position I am now suggesting would explain this by saying that the essence of squirreldom, perhaps a particular genetic structure, determined the growth of tails, but not a particular color of coat. The suggestion that the essence might be the genetic structure illustrates another important aspect of such a position, the way that the essence itself is to be distinguished from other essential properties. Presumably, the genetic structure causally determines the growth of a tail, and not vice versa. Thus, the essence itself is that property that is explanatorily primary among the set of essential properties.

Making such an essentialism applicable to any part of biology—and, probably, to most other parts of science—requires some important qualifications. To begin with, it is very difficult to find really sharp distinctions anywhere in biology; generally, there is a range of intermediate cases. Certainly, as far as taxonomic distinctions are concerned, sharp boundaries are the exception rather than the rule. Thus, a theory of essences would have to be considered as applying to typical members of kinds rather than to all members. Assuming that it remains desirable to attribute individuals to kinds despite their abnormality, the laws applying to such kinds could be only probabilistic. The probability that something has a tail, given that it is a squirrel, would then reflect the frequency of the abnormality of tail-lessness. The modified essentialist position could be maintained by insisting that there is, nevertheless, some standard genetic structure that constitutes the essence of squirreldom, and that anything that perfectly realizes this structure would, as a matter of nomic necessity, have a tail. Less-than-ideal squirrels would then be judged to be squirrels, or not, on the degree of

similarity of their genetic structure to this standard form. It will be apparent, however, that this unavoidable modification leaves the essential/accidental distinction rather more arbitrary than might have been wished. It is, at least, unclear whence the fundamental difference between blackness and tail-lessness of squirrels derives—apart from a patently question-begging appeal to the essential nature. If it comes to no more than a quantitative difference in frequency, then a fairly arbitrary decision is required to include one (or its genetic basis), but not the other.

Many philosophers of biology would wish to mitigate this difficulty by denying that taxonomic distinctions define kinds at all, and a fortiori, that they are natural kinds that could admit of real essences.[3] I myself believe that species (higher taxa I assume to be purely nominal kinds) are kinds rather than individuals. However, I doubt that the present problem will be much mitigated by denying that species are kinds. Whatever kinds one happens to favor in biology (e.g., ecological or evolutionary), one is unlikely to find the sharp boundaries that would evade the present difficulty. A striking illustration, which will be discussed in detail in the next section, is that of sex. It would be hard to imagine a more obviously natural division within biology than that between male and female. Yet in sexually dimorphic species there are typically variations with respect to sexually specific characteristics, and even genuinely intermediate individuals. At any rate, the distinction btween fairly sharp boundaries between kinds and absolutely sharp ones is itself an absolutely sharp one, so that the advocate of biological kinds that completely evade the present problem will have a difficult task.

Part of my reason for emphasizing this difficulty is to stress the detachability of a belief in natural kinds from a belief in essences. The belief that there are discontinuities in nature to be discovered rather than invented is quite independent of the question whether these discontinuities are sharp or gradual. Moreover, the relation of natural kinds to questions of explanation does not depend on a doctrine of essences. One might suppose, for example, that there was some optimal set of laws (perhaps maximally deterministic and/or explanatory) governing a domain, and that the classes of entities recognized by those laws should be considered as natural kinds. Such a view does not require that any fundamental distinction be drawn between the essential and accidental properties of the members of such kinds. Since it will typically be the case that the frequencies of such properties in a kind will vary continuously from almost one hundred percent to almost zero percent, such a distinction appears inevitably arbitrary. But, as I have tried to show, without this distinction, the point of essentialism becomes obscure.

I shall return to the idea that natural kinds should be treated strictly as derivative from the discovery of laws in the final section of this paper. But, for now, I shall move on to discuss more specific cases in detail. This will demonstrate some further and compounding difficulties with the essentialist perspective.

SEX

As promising an essential distinction as one is likely to find in biology is that between male and female. The distinction can be drawn successfully for a very large number of

organisms, and although, as I have suggested is true of almost any biological distinction, there are borderline cases, the vast majority of organisms of types to which the distinction applies can be assigned unambiguously to one category or the other.

Another relevant feature of the distinction, although now only for as long as we look at a particular type of organism, is that there are systematic differences between males and females at various levels of structural organization, and that these are causally and explanatorily related. More specifically, for most species, males and females differ genetically, physiologically, and behaviorally; and we are fairly confident that the genetic differences cause the physiological ones, and that the physiological differences cause the behavioral ones.

However, further consideration shows that the situation diverges greatly in certain respects from the essentialist scenario I sketched in the previous section. The properties that are causally fundamental in explaining sexual dimorphism between the members of a species are unquestionably not the properties that realize the real essences (if any) of maleness and femaleness. A microstructurally oriented essentialist might be inclined rashly to assume that the essence of maleness and femaleness for humans was/is the possession, respectively, of an XY or an XX chromosome. But many animals that can be divided into males and females as clearly as humans can have no XX or XY chromosomes. Indeed, this view would seem to imply that to say that there are both female humans and female geese would be a gross equivocation on the word "female," since in each case the word refers to a quite different microstructural property; and this would patently be absurd.

Surely the correct way of describing the situation is to say that *for humans*, having XY or XX chromosomes *causes* individuals to be male or female. What it is to be male or female, on the other hand, is a property at a higher level of structural organization, that of producing relatively large, or small, gametes. It is *this* distinction, based on the fact that most types of organisms have individuals of two kinds distinguishable by a major dimorphism in the size of the gametes they produce, that is referred to by the general categories of male and female, and that in particular species is caused by a particular genetic dimorphism.[4] Thus surprising, and even paradoxical, though it may seem, it is correct to say that physiological differences between the sexes, and any genetically determined behavioral differences that there may be, are not, in fact, caused by the sex of the organism; rather, these differences and the sex of the organism are joint effects of a common cause.

In this light, it is *not* surprising that the sexual categories have little explanatory power. It is very doubtful, that is, whether there are any very significant laws relating to males and females in general. It seems plausible that every generalization about a sexually specific characteristic is limited to some narrower group than that of all sexually dimorphic species. In some cases, there is a recognized taxon over which the generalization applies, either because the character concerned is an evolutionary novelty in a phylogenetically demarcated taxon, typically a species, or because that very character is used to define the higher level grouping, as with mammals, or placental mammals.

Although the possibility cannot be ruled out a priori that there might be some properties universally, or almost universally, correlated with large or small gamete production, there seems to be no reason to expect that this will be the case. This observation invites reconsideration of my claim that sexual categories are exceptionally promising candidates for biological natural kinds. The intuitive basis for that claim certainly has nothing to do with a knowledge of laws pertaining to males and females in general. It is based, rather, on two kinds of observation. First, that within any species, and often within much larger taxa, there are very pervasive sex-specific generalizations to be made. Men grow or shave beards, and women have breasts; males and females of large numbers of (related) species have relatively similar genitalia. And, second, for enormous numbers of species, it is possible to distinguish males from females. However, what these observations properly suggest is that sex is a very significant property that may be appealed to in the analysis of innumerable different taxonomic groupings but that, nevertheless, it is not a property that is sufficient to define any significant kind. Alternatively, if one wishes to insist that males and females *do* form natural kinds, then there are natural kinds with little or no explanatory power.

The fact that nature can be "carved at the joints" without yielding explanatorily significant categories is worth a moment's reflection. The explanation in this case is not hard to find, deriving from a very fundamental fact about biology: biological kinds reflect historical similarities as much as they indicate similarities of causal power. The divide between males and females, as general categories, derives not from characteristic properties or dispositions of the two classes but, presumably, from the existence of a very pervasive evolutionary tendency toward sexual dimorphism.[5] But it seems likely that the common evolutionary pressure may do no more than favor a simple dimorphism of gamete size, and that subsequent elaborations of the dimorphism may well be much more specific to particular evolutionary lineages, and not susceptible to large-scale generalization.

Two responses to the preceding argument need to be considered. First, I have so far ignored a trend in contemporary biology that *does* want to maintain the general explanatory power of sexual categories. By this, I mean a major area of sociobiology. And, second, one may accept the general conclusion that I have argued for above and yet explore the possibility of defining narrower, but still explanatorily powerful, sexually delimited kinds. I shall now briefly discuss these positions. The second will lead conveniently into the topic of gender.

I cannot hope to give an adequate treatment here of the highly problematic and controversial discipline of sociobiology.[6] However, one major area of sociobiological theorizing does assert precisely what I have said there is no reason to believe: that the simple fact of gamete size dimorphism strongly disposes species to certain subsequent evolutionary developments, specifically, to quite well-defined behavioral dimorphism. At its most general, the theory asserts that those organisms with smaller gametes (i.e., males) will tend to develop behavioral strategies that maximize the dispersion of their gametes, while the females will develop strategies that tend to increase the chances of successful development for those offspring that they are able to assist. At this very general level, the theory is based simply on the idea that a large gamete is

a more significant investment of resources than a small one, and this will give disproportionate encouragement to strategies that tend to further its development. If there is any force to this argument, there is obviously a lot more to it when the reproductive physiology of the organism requires that much larger investments of resources are demanded for the female to have any chance of reproduction, as is the case of viviparous animals or animals that lay large eggs. Additionally, in such cases it is argued that when the offspring, or egg, is produced a substantial time after fertilization and requires further care to have any chance of survival, the female will find herself playing an evolutionary game with no cards. The male, it is argued, will by then have taken off to attempt to impregnate more females, and the only way that a female can expect to have any reproductive success at all will be to provide at least the essential minimum of parental care.

The most obvious defect with this argument is that the predictions to which it gives rise do not turn out to be true. Many species, even of birds and mammals, are quite monogamous in both sexes, and there are many species in which the male provides as much parental care as the female, or even more. But I shall not attempt any evaluation of the general force of this sociobiological argument, since the preceding simple observation is sufficient to demonstrate the conclusion I wish to draw for my present purpose. This is simply that however significant a *force* in evolution these arguments may indicate, that is all they indicate. Clearly, if there is such a force, it is one capable of being overridden by other forces that operate in an opposing direction; otherwise, there could not exist the many exceptions just mentioned. (The same point can, and will, be made in connection with alleged systematic differences beetween men and women.) It might be thought that since I have allowed that dispositions common to members of a kind suffice to give that kind explanatory power, the above concession would be sufficient to constitute sexual categories as natural kinds. But this would be a confusion based on the failure to distinguish historical from causally explanatory categories. It may be that in every species there has been an evolutionary tendency for males to acquire dispositions to promiscuity and females to acquire dispositions to parental care. But in many cases those dispositions have not been actualized; and, hence, the members of many species do not have those dispositions. A drake, say, may have no disposition whatever to desert his mate. And it would be absurd to say that he must have such a disposition merely on the grounds that his ancestors had some, in fact unrealized, tendency to evolve such a disposition. So, in short, whatever the force of these sociobiological arguments, though they may help to explain the particular behavioral dimorphisms in particular species, they do nothing to make males or females into genuinely explanatory kinds.[7]

The second response I described above was to accept that sexual categories are not themselves explanatory kinds but to argue that more narrowly defined sexually specific categories might, nevertheless, be so. Thus, male and female mammal, goose and gander, and man and woman may constitute sex specific natural kinds with explanatory force regardless of whether male and female are themselves such kinds. Two general points should be made about this proposal. First, assuming, which I would be very reluctant to do for any taxonomic level above the species, that the taxon

that is being sexually restricted is a genuine kind, this is not a case of the intersection of two kinds, but one of the subdivision of one kind. This is simply the application of the main conclusion of the present section about general sexual categories. But second, there is certainly no a priori objection to the internesting of natural kinds. There is nothing incoherent, for instance—though there is, almost certainly, something false—in conceiving biological taxonomy in this way. Metal and iron provide one plausible example. Human and woman might be another.

It is worth mentioning that the viability of this proposal will depend on accepting that some taxonomic groupings are, indeed, natural kinds. For one who believes that species are individuals (see note 3), it would be quite extraordinary to suppose that these individuals might be formed from the union of two kinds. However, this is not the place to pursue that issue.

It seems that there is nothing deeply wrong with this idea provided one registers some important qualifications. In particular, it would be absurd to suppose that man and woman, say, were "better" kinds than human. To admit that species are kinds is to admit that kinds may encompass very considerable variation and, hence, license only probablistic nomic generalization. Moreover, as I have argued earlier, it is to admit that kinds can be considered as defined by essences only in the most attenuated sense of "essence." It would, again, be absurd to suppose that the essence of woman was any more clearly definable than the essence of human. But, in fact, one would predict the opposite. Since the similarities between men and women are vastly more numerous than the differences, one would expect the latter kinds to be "worse." And the problems with defining an essence of woman must surely, then, be more severe than those of defining an essence of human.

My final point follows, once again, from the fact that male and female are not themselves explanatory kinds. The explanatory significance of sexually specific kinds must be wholly empirically determined. No systematic differences between the males and females of a particular species can be assumed beyond those that are used to distinguish the sexes. This does need slight qualification. Sometimes one can appeal to higher-level generalizations. If one discovers a new species of mammal, one will reasonably anticipate that dissection will reveal an approximately familiar and sexually dimorphic type of reproductive physiology characteristic of mammals. However, I know of no other type of property for which, in the case of higher animals at least, such broader generalizations would be of any use. Certainly, there are none in overt morphology beyond similarities of external genitalia in related taxa; and more importantly, there are none in the area of behavior, or again, none that extend beyond very narrowly defined phylogenetic groups. And, as I have insisted, none can be deduced from the mere fact of subsumption under the broad sexual categories. Accepting, then, the possibility, if highly qualified, that species as kinds may be subdivided into sexually specific subkinds, it is now time to look in more detail at the human case and to turn to the topic of gender.

GENDER

The term "gender," as it has been developed in contemporary feminist theory, refers to the sexually specific roles that are occupied by men and women in various societies.

The most obvious reason for insisting on a sharp distinction between sex and gender[8] is that whereas whatever properties may follow from the sex of their bearers, such as reproductive physiology and secondary sexual characteristics,[9] must be equally prevalent in all societies, it is quite clear that gender roles, on the contrary, are highly variable and culture specific in many respects. So even if man and woman as biological categories are modestly explanatory natural kinds, it is clear that much of the behavior encompassed under gender roles is no part of what they explain.

It is not altogether easy to assess the *extent* of variability in gender roles. One reason for this is that a great deal of the relevant research is particularly susceptible to the kinds of problems to which feminist critics of science have drawn attention; if there is any part of science for which the accusation of distortion by male-biased preconceptions seems particularly plausible, this is surely it.[10] But both anthropological and historical evidence leaves little doubt that such variability is extremely widespread.[11] Some particularly noteworthy areas are those that have been of special prominence in attempts to reduce gender specific behavior to a causal consequence of sex. Promiscuity, and the extent to which it is a male prerogative, provides one important example. Also of interest is the variability in the extent to which the generally socially approved form of gender specific behavior is adhered to or insisted upon. The prevalence of, and attitudes toward, homosexuality and incest, both subjects that have received a great deal of attention from sociobiologists, appear to be highly variable. The prima facie evidence seems to be that in most of the aspects of behavior that suggest sexual dimorphism in the context of a particular culture, there is a great deal of cross-cultural variation.

Before continuing this discussion, it will be useful here to recapitulate a little, and explicitly to reintroduce the topic of essentialism. One traditional view might be the following. Both humans and males constitute natural kinds with a certain essential property. To be a male human is to partake of both the relevant essential properties, and much of the behavior of a male human can be explained by reference to the causal powers of one or both of these essential properties. Against this I have argued that the most that can be sustained is the claim that male humans form a subkind of humankind. If this kind has an essential property, it is presumably a combination of the essential genetic structure of humans with the specifically sex-determining genetic features of male humans. The reason that the essentialist is forced into this specific, and, I suspect, rather unpromising, form of genetic determinism is precisely that maleness in general is not an explanatory category, and the only available candidate for an explanatory essence for the kind of human males must be their distinctive genetic features. Unpromising or not, there are certainly those, certain sociobiologists providing their theoretical wing, who want to maintain a position of this kind and trace the behavioral differences between men and women to the genetic.

A major thrust of the feminist research that has emphasized the historical and anthropological variability of sexual differences in behavior has been explicitly directed against positions of this kind. Its aim has been to establish that these differences are to be understood in terms of social forces, which are fairly specific to particular

cultures. It has also offered alternative schemes of explanation, perhaps the most influential and interesting of which are those that trace these differences primarily to the action of economic forces and conditions.

It would be an oversimplification to suppose that feminist scholarship fits uniformly into this agenda. To begin with, there are some feminists who would pretty much accept the essentialist structure that I have just outlined, while objecting only that the details have been filled out in a way revealing profound male bias. Most noteworthy in this category are a small number of feminist sociobiologists.[12] But more significantly, a markedly essentialist flavor has often been detected in a good deal of more mainstream feminist thought.[13] Indeed, it may even be suggested that the very intelligibility of feminism depends on construing women as a natural kind and, hence, on accepting essentialism. Although there may be some feminist projects that do, indeed, depend on this assumption, in most cases such suspicions are ill grounded. It will be worth a short digression to indicate why this is so.

It is easy to overestimate the prevalence of essentialist assumptions in feminist writing by failing to identify its primary goals. A great deal of emphasis in feminist work has been accorded to one observation that strongly appears to be a cross-cultural universal, namely, that men seem invariably to have achieved a position of domination over women. I do not think it would be unfair to say that this is often seen as the central theoretical problem of feminism. And if the central theoretical problem is one of explaining a universal fact about the relation of men and women, it is not surprising that much of the writing has a rather essentialist flavor.

But it is crucial not to overlook the fact that feminism, perhaps more than any other area of academic interest, is at least as much a political movement as a theoretical inquiry. From a political point of view, the universality of male domination is clearly of paramount importance. The political achievement of feminism may be described without exaggeration as the discovery and definition of an entire political class ignored by traditional theories. Nevertheless, the political significance of patriarchy should not blind us to the fact that from the point of view of the purely theoretical task of understanding sex and gender categories, this fact is anomalous rather than central. A brief consideration of the reasons for this will also help to forestall any tendency for the universality of male domination to serve as a motivation for a crudely biological theory of gender differences.[14]

Although I would readily concede that if male domination is a universal or near universal phenomenon throughout human societies, it is a phenomenon well worth theoretical study, there is no reason whatsoever for taking this as contradicting the basic variability in human sexually differentiated behavior. In the first place, it is only one case to set against many. But even this way of putting the case is misleading; male domination is a phenomenon on a higher level of abstraction than is the characterization of particular forms of behavior in particular societies. There is no reason to suppose that the exercise of male domination is itself something that has always been implemented by the very same kinds of behavior. On the contrary, the kind of labor that women perform for men is quite different in, say, feudal societies, hunter-gatherer societies, and modern industrial societies; and the social institutions and personal

interactions that enforce such performance are equally variable. Hence, the *implementation* of male domination—which is what we should consider, rather that its mere existence, if we are evaluating the plasticity of behavior—far from contradicting the variability of gender roles, graphically illustrates it. Analogously to my conclusion for the case of sex, there may be good reason to suppose that human sex differences give rise to forces that have some tendency to bring about male supremacy. But, as in the previous case, although the existence of such a force may be of use in explaining the genesis of a particular gender-differentiated society, it does not pick out any property that characterizes the present state of that society. In this case, even if male supremacy is genuinely universal, the enormous variability in the form that it takes indicates extensive interactions with more specific forces that, in turn, show that there are no grounds for assuming that even the abstractly characterized consequence is in any way inevitable.

Returning now to the main theme of my argument, as in the broader case of sex in general, the question we should consider is whether even man and woman (in their biological sense) are genuinely useful explanatory categories. I have conceded that as purely biological kinds, they are largely unobjectionable; it may be allowed as a modest nomic generalization, for example, that humans born with penises will tend to grow facial hair later in their lives. But there is a very powerful tendency to extend the relevance of explanatory categories beyond their empirically determined limits—a tendency, I am suggesting, that derives philosophical nourishment from the idea that when one has distinguished a kind, one has discovered an essence. If, in fact, the empirical significance of the kinds man and woman does not go beyond some systematic, if quite variable, physiological differences and the observation that men appear to have achieved a dominant position in all or most societies, the kinds distinguished seem of very modest significance. Certainly, nothing in those empirical facts provides the slightest motivation for thinking that these categories should be accorded fundamental importance in explaining the particular forms of behavior found in very different social systems, whether such explanation is motivated by sexist apologetics or (misplaced) feminist ardor.

The conclusion I want to defend might be stated as follows. Just as the concept of sex in general will do very little to explain why peacocks, but not turkeys, have long tails, or why the prairie chicken, but not the goose, is polygynous, so the notion of woman will do nothing to explain why Oriental women once had their feet mutilated, or why twentieth century Western women are more likely to become nurses than doctors. In principle, the same move is open to one as was suggested at the conclusion of th discussion of sex in the previous section. It would be possible to suggest that the appropriate explanatory categories were again to be narrowed, so that for behavioral explanations, the relevant classes would be as specific as female 'Kung or male Spaniard. At this point, however, the claim to have identified even the most attenuated natural kinds would be impossible to sustain. The claim to have identified a natural kind must involve the idea that the behavior of its instances depends, in some cases, on intrinsic properties of the individual characteristic of members of that kind. But it would be hard to find even the most bigoted racist nowadays prepared to assert that the

social interactions characteristic of a man raised in, say, rural Spain would have been just the same if that individual had been brought up in a wealthy California suburb. To concede that explanations must appeal to kinds with that degree of specificity is to concede beyond serious argument that it is local, presumably cultural, factors that determine the relevant forms of behavior.

MORALS FOR ESSENTIALISM

I would like to take the discussion of the preceding two cases to illustrate a general argument against essentialism. The main thrust of this argument is to plea for complete empiricism with regard to the explanatory potential of particular kinds. My suggestion is that a belief in real essences either is vacuous or violates this demand. In partial reaction to this point, I shall also suggest that attention be drawn away from the attachment of fundamental importance to the delineation of kinds, and directed toward the identification of properties, dispositions, and forces. To connect these points, what makes a kind explanatorily useful is that its instances share the same properties or dispositions and are susceptible to the same forces. But since we have no way of deciding how much of such concomitance to expect in any particular kind, the discovery of a kind adds nothing to the discovery of any correlations that may turn out to characterize it. An essence, as I characterized it in the first section of this paper, can be seen as a promissory note on the existence of such correlations. It is a promissory note that empiricists should reject. I take the preceding discussion to illustrate this point in the following way: it is easy enough to distinguish classes at many different levels of generality—males, male vertebrates, men, Irishmen, and so on—but there is nothing in this process of differentiating classes that provides any basis for predicting the extent to which its members will be amenable to lawlike generalizations. Finally, this in no way impugns the theoretical significance of the *properties* on the basis of which such classes are differentiated (I shall elaborate on this remark with regard to sex below).

The most powerful example I have offered in support of this plea is the case of sex in general. As I said in the course of discussing sex, what we see is that there are major seams in nature that not only fail to distinguish robust natural kinds, but also fail to distinguish classes that realize any general lawlike regularities. The explanation in this case is simple enough: the seam reflects a presumably uniform type of historical process rather than the discrimination of any causally uniform type of entity. But it is hard to see what could be the basis for postulating the existence of a natural kind, in the strong sense of a set of common possessors of a real essence, except either the perception of a natural seam among phenomena, or the discovery of one or more laws satisfied by a class of phenomena. In the first case, as the example of sex shows, the inference to a natural kind would be illegitimate; and in the second case, unless it constituted a quite ungrounded assumption that further hitherto undiscovered laws were in the offing, it would be wholly redundant.

Having rejected the idea that general sexual categories could provide a basis for lawlike generalization, I then considered the possibility that far more restricted sexually specific categories might still constitute natural kinds in the strong, essentialist

sense. I do not mean to claim that this possibility has been—or, for that matter, could have been—rigorously explored in the space I have allowed. However, the issue of gender shows, at least, that it cannot generally be assumed that such kinds are discoverable. Here again, it is not difficult to see what is going on. Many factors affect human behavior, including behavior that is gender differentiated. Looked at in this obvious way, it would be foolish to assume that there were forms of behavior that were determined simply by the agent being, say, a human male—still less by the agent being merely a male. Nevertheless, unless we are careful to restrict the import of our categories to the empirical, we are in danger of being led into just such an assumption.

I should emphasize that I do not take myself to have shown any particular limits on the nomic significance of sexually defined categories in general. In the case of humans, there are good reasons for doubting whether this significance extends beyond the purely physiological. In many other species, there are undoubtedly good generalizations to be made about sexually dimorphic behavior. My point is not that sex is a scientifically useless concept, but rather that from a conceptual standpoint that seeks kinds and their underlying essences, one is very likely to misrepresent that significance. I should now, therefore, say something very briefly about how I do understand its significance.

To begin with, nothing I have said contradicts the idea that sex is a highly significant *property*. By that, I mean that sex is a property that, in a sufficiently specific context, is frequently susceptible of lawlike generalization. At a certain time of year, for instance, the males of a particular species of bird produce very characteristic and predictable noises. If you know the time of year and the species of bird, what you additionally need to know, if you want to predict whether it will make that noise, is what sex it is.

No doubt of more theoretical interest are the ways that sex connects with evolution. At the most theoretical level, there is the question of the origin and maintenance of sexual reproduction. Since sex seems, prima facie, such an extraordinary waste of reproductive energy from the point of view of most females, this is a very baffling question. On the other hand, this very problematic nature of the phenomenon makes it likely that there is some powerful evolutionary process at work. At a less general level, as Darwin emphasized at great length, the existence of sex can have profound effects on the particular course of the evolution of a species. Thus, I am far from denying the biological interest of sex. What I want to claim is that the way in which the basic sexual categories—male, female, neuter, hermaphrodite—divide the natural world tells us nothing about either the extent to which such categories will give rise to general laws or, more importantly, what will be the scope of whatever interesting laws do involve those categories. It is the denial of this latter point that, I believe, is required to provide any motivation for an essentialist position and that, I have argued, is very difficult to reconcile with the range of phenomena I have been discussing.

Let me conclude with a word about natural kinds. There is certainly no harm in calling a set of objects that are found to have a substantial number of shared properties a natural kind. I want to insist that the discovery of such a kind provides no basis for the supposition that some particular property or properties can nonarbitrarily be singled

out as essential. But, as I remarked earlier, there is no reason why the term "natural kind" should be wedded to essentialism—or, anyway, no more reason than an accident of linguistic history that can readily be rectified. With this proviso, I am quite happy to refer to species as natural kinds. This case is unusual, in that we do have reason to expect that members of species will share a large number of properties, this reason being that we suppose the members of the species to have come about through an extremely homogeneous historical process. However, this in no way contradicts my insistence that the extent of homogeneity within a kind should be treated wholly empirically. Members of a species, as I have remarked, also vary greatly. And we cannot know a priori how variable any particular feature will turn out to be.

The only thing that could provide grounds for dispensing with this empirical stance would be if we were somehow to know that the members of certain kinds were completely homogeneous in all respects. Many people seem to believe that this is true of the kinds distinguished by physics and chemistry, though I find this doubtful.[15] If it is, physics and chemistry are in a very important respect different from biology. But even if this is the case, it is surely an empirical fact, not anything that could be known a priori. It is surely possible to conceive of a world composed of indivisible atoms, each as different from one another as one organism is from the next. Such would not appear to be the case; but *how* homogeneous physical or chemical particles may be remains an empirical matter. If this is correct, even microphysics cannot provide a hiding place from the categorial empiricism that I am advocating.[16]

Notes

1. This claim is, of course, controversial in the light of the well-known views to the contrary of Kripke (1972) and Putnam (1975). I have argued against these views elsewhere (1981). The possibility of deriving essentialism from semantic considerations has also been attacked at length by Salmon (1981).

2. This is a reasonable way of interpreting the conclusion I formerly defended about species (1981).

3. Classic statements of the view that species should be treated as individuals have been made by Hull (1976) and Ghiselin (1974). An excellent sense of the present state of the debate can be gleaned from Kitcher's (1984) attack on the view and Sober's (1984) reply.

4. An interesting paper by Michael Lavin (unpublished), primarily addressed to some philosophical problems that arise from gender reassignment surgery, includes a persuasive argument for the view that what we mean in ordinary language by "male" and "female" has nothing to do with either genetic or general biological considerations, but is derived wholly from considerations of gender, that is, of socially constructed conceptions of what it is to be male or female in our society. Although I am entirely sympathetic to this view, I hope it is clear that it is these more technical considerations that are relevant to my present discussion.

5. The nature of this pressure, however, remains surprisingly obscure. Excellent sources on the problem are Williams (1975) and Maynard-Smith (1978).

6. The classic text on sociobiology is Wilson (1975); a highly readable popular introduction is Dawkins (1976). The enterprise has come under devastating attack from Lewontin, Kamin, and Rose (1984) and, perhaps a little more sympathetically, from Kitcher (1985).

7. Kitcher (1985, especially 166–76) shows clearly the internal weakness of this sociobiological argument.

8. Some feminists, notable among them Alison Jagger (1983, 112) now want to resist drawing such a distinction between sex and gender, on the grounds that it erroneously suggests that the sexual side of the dichotomy is rigid and unchanging; and that, in fact, there is a continuous dialectical interaction between

cultural and biological aspects of gender differentiation. Nancy Holmstrom (1982) develops a similar position and defends the conception of a distinctively female nature, on the basis that "nature" should be understood in a way that encompasses both biological and culturally determined aspects, since she also denies that these can be intelligibly disentangled. Although I do not want to take issue with this view and willingly disavow any implication that there is some readily distinguishable set of immutable biological differences between men and women, I believe that my appeal to this distinction in the present context is both useful and harmless.

9. Secondary sexual characteristics—for example, the distribution of body hair—in fact show considerable geographic variability. If Darwin was right in attributing the majority of geographical variations among humans to a process of sexual selection (see Darwin [1981], especially chs. 7, 19, 20), this is hardly surprising.

10. See, e.g., Longino and Doell (1983); Reed (1978).

11. A good illustrative source is the collection of essays in Ortner and Whitehead (1981). It should, perhaps, be mentioned that these authors have more interesting and ambitious goals than merely establishing gender role variation. Ortner and Whitehead's introduction *begins* with the sentence: "It has long been recognized that 'sex roles'—the differential participation of men and women in social, economic, political, and religious institutions—vary from culture to culture." Nevertheless, for anyone who doubts this claim, these essays include ample evidence.

12. See, e.g., Hrdy (1981). At a more popular level, an entertaining feminist answer to Desmond Morris is Morgan (1972).

13. Jagger (1983) suggests that a commitment to biological determinism is a characteristic defect of the school of feminist thought she describes as "Radical Feminism."

14. As that, e.g., of Steven Goldberg (1973). Unfortunately, it is also my impression that some feminists have been led by the same observation in the same direction though certainly those who, like Goldbertg, see male aggressiveness as the crucial, and even biologically grounded, factor are likely to point out that aggressiveness is not necessarily an unqualified virtue.

15. I have briefly defended this claim elsewhere (1983, 326–27).

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References

Darwin, Charles. 1981. *The Descent of Man, and Selection in Relation to Sex*. Princeton.

Dupré, John. 1981. "Natural Kinds and Biological Taxa." *Philosophical Review* 90:66–90.

———. 1983. "The Disunity of Science." *Mind* 92:321–46.

Ghiselin, Michael. 1974. "A Radical Solution to the Species Problem." *Systematic Zoology* 23:536–44.

Goldberg, Steven. 1973. *The Inevitability of Patriarchy*. New York.

Holmstrom, Nancy. 1982. "Do Women Have a Distinct Nature?" *Philosophical Forum* 14:25–42.

Hrdy, Sarah. 1981. *The Woman Who Never Evolved*. Cambridge, Mass.

Hull, David. 1976. "Are Species Really Individuals?" *Systematic Zoology* 25:174–91.

Jagger, Alison. 1983. *Feminist Politics and Human Nature*. Totowa, N.J.

Kitcher, Philip. 1984. "Species." *Philosophy of Science* 51:308–33.

———. 1985. *Vaulting Ambition: Sociobiology and the Quest for Human Nature*. Cambridge, Mass.

Kripke, Saul. 1972. "Naming and Necessity." In *Semantics of Natural Language*, edited by D. Davidson and G. Harman, 253–355. Dordrecht.

Lavin, Michael. Unpublished. "On the Moral Permissibility of Sexual Reassignment Surgery."

Lewontin, Richard, Steven Rose, and Leon Kamin. 1984. *Not in Our Genes: Biology, Ideology, and Human Nature*. New York City.

Longino, Helen, and Ruth Doell. 1983. "Body, Bias, and Behavior: A Comparative Analysis of Reasoning in Two Areas of Biological Science," *Signs* 9:206–27.

Maynard Smith, John. 1978. *The Evolution of Sex*. Cambridge and New York.

Morgan, Elaine. 1972. *The Descent of Woman*. London.

Ortner, Sherry, and Harriet Whitehead. 1981. *Sexual Meanings*. Cambridge and New York.
Putnam, Hilary. 1975. "The Meaning of 'Meaning'," "Is Semantics Possible?" and "Explanation and Reference," In *Mind, Language, and Reality, Collected Papers, Vol. II*. London and New York.
Reed, Evelyn. 1978. *Sexism and Science*. New York and Toronto.
Salmon, Nathan. 1981. *Reference and Essence*. Princeton.
Sober, Elliott. 1984. "Discussion: Sets, Species, and Evolution: Comments on Philip Kitcher's 'Species'." *Philosophy of Science* 51:334–41.
Williams, George C. 1975. *Sex and Evolution*. Princeton.
Wilson, Edward O. 1975. *Sociobiology*. Cambridge, Mass.