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Author(s): Mohan Matthen

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BIOLOGICAL UNIVERSALS AND THE NATURE OF FEAR

How are emotional states such as fear to be defined? The problem arises out of a clash of methodologies used in the study of the emotions. In humans, some action-relevant states such as fear count as emotions, while others such as hunger, on the one hand, and respect for the law, on the other, do not. An examination of the ways in which we make the distinction leads me to the conclusion (section I) that we count as emotions only such states as are typically brought on by cognition and reasoning in humans; consequently, states like hunger, which have no such basis, do not count as emotions. But respect for the law does not count as an emotion either; and so I conclude that in order to be included among the emotions, a state must have a long evolutionary history and be something we share with animals.

These demarcation criteria seem to conflict. If the emotions have a long evolutionary history, they are unlikely to be *essentially* cognitive. When ethologists and psychologists say that animals such as geese and rats experience fear and other emotions, they are not suggesting that these states are the products of the kind of cognitive process that in humans typically precedes fear. So how to define fear? If we define it as cognitive in order to accommodate what seems to be an essential feature of human fear, we exclude fear in geese and rats and include respect for the law. If we let these animals in, we seem to include states like hunger which are essentially different from any emotion.

This is where “biological universals” come in. Evolutionary theorists define biological taxa such as species in ways that permit variation, both synchronic and diachronic. The species *horse*, for example, is understood in a way that permits us to recognize that contemporary horses may differ markedly from ancestral horses and that two

contemporary horses might similarly be very different in their properties, and also to hold that all of these individuals belong to the same species. I discuss how these variation-accommodating universals are to be defined (section II). The aim here is to preserve the well-established idea that terms like 'horse' are general terms with divided reference, while accommodating the antiessentialist program of those who insist that species must be individuals, since they evolve. I attempt to broaden this form of classification so as to accommodate the emotions, showing how we can solve the difficulties of section I, while also accommodating some much-discussed cases of variability among the human emotions (section III). My intention here is to be suggestive rather than definitive—it is my pessimistic conclusion that fear is not definable at the present time.

I. DEMARCATING THE EMOTIONS

(1) Emotions tend to cause a subject to respond to specific kinds of situations in a functionally appropriate way; but not all such states are emotions. I might feel hungry when my blood-sugar level gets low, and this would tend to make me look for food, a functionally appropriate response; but hunger is not an emotion. Respect for the law might tend to determine where I park and where I refrain from parking on different days of the week, but it is not an emotion either. There is a question, then, of how one might distinguish the emotions from other situationally specific states relevant to action.

Consider the following pair of situations.

Situation A: I go up in a hot-air balloon. As the balloon drifts upward to 1,000 meters, I glance downward over the edge of the gondola and look at the tiny objects on the ground far below me. Suddenly, I am overcome by panic: though I know that I am perfectly safe, I feel faint, I sweat, my heart flutters. This is fear-of-heights, or vertigo. When I duck my head back away from the side, the panic abates, and in time I can go about my piloting calmly.

Situation B: I go up in a hot-air balloon. As the balloon drifts upward, I resolutely stay away from the edge of the gondola, and look at the instruments instead. Glancing at the altimeter, I notice that I have drifted up to 10,000 meters. I recall that the safety limit of this particular balloon is only 7,000 meters. Suddenly, I am overcome by panic: I feel faint, I sweat, my heart flutters. This is fear—fear brought about by the reasoned cognition that I am too high for safety.

It seems intuitive to think that in situation B, I am experiencing an emotion. As described above, the state conforms, for instance, to

Wayne Davis's¹ specification of "experiential fear": it is a state of "involuntary arousal" resulting directly from *the occurrent propositional belief* that harm might occur to the believer.

But what about situation A? My reaction in situation A seems too automatic to count as such. Unlike the fear in situation B, which is brought about by the belief that I am too high for safety, hyphenated fear-of-heights bypasses belief altogether (and thus fails the Davis test).² Given merely the occurrence of the right kind of perceptual state (the sight of a chasm just beyond my feet), the state in situation A can occur on the balcony of a penthouse apartment or on a broad but lofty mountain path, even where I firmly believe that I am in no danger. No doubt, vertigo can be closely linked to fear proper in the absence of a definite belief that I am really safe; without such a belief, vertigo will often develop into fear (in part because it is likely to *engender* the relevant belief). But taken by itself, vertigo will persist (or else gradually fade away) independently of what I believe, whereas fear proper will always be linked to belief. In this respect, hyphenated fear-of-heights resembles hunger, cold distress, and the startle reflex: each of these states persists or can be triggered even in the presence of the clear and distinct cognition that the situation

¹ "A Causal Theory of Experiential Fear," *Canadian Journal of Philosophy*, XVIII, 3 (September 1988): 459-84. Davis's cognitive approach is typical of those to which I allude in this section, if more than usually explicit. The cognitive component of the definition is taken for granted by many philosophers. Kendall L. Walton has, for instance, called it a "principle of common sense" that fear involves belief—"Fearing Fictions," this JOURNAL, LXXV, 1 (January 1978): 5-26, esp. p. 6.

² In an article in which I otherwise find much to agree with, John Morreall says: "When some joker sneaks up behind us and pops a balloon or says 'Boo!' we do not have to think of a gunshot or rapist to be scared"—"Fear without Belief," this JOURNAL, XC, 7 (July 1993): 359-66, here p. 361. He takes such an occurrence to be an instance of fear, and uses it to argue that fear can be "objectless." Although I concur with Morreall's general conclusions in this article, I do not agree that the state described above is *fear*. Being startled can lead to fear given the right circumstances (for example, circumstances in which one is encouraged to think of gunshots or rapists), but surely the incident as described, if it occurred at a children's party, is not properly described as "objectless *fear*." The startle reflex is a lot more like vertigo, an automated modular response, and it is not connected to fear proper by cognitive complexes. I persist in this disagreement after having considered the arguments of Jenefer Robinson in an important recent article—"Startle," this JOURNAL, XCII, 2 (February, 1995): 53-74. Robinson says that "several (psychologists) have studied the startle reflex as an example of an emotion" (53), adding that most philosophers would find the suggestion "laughable." But her own descriptions of the opinions of psychologists suggests that they are, at the least, hesitant about classifying startle in this way. I shall say more later about my disagreements with Robinson. They are not relevant to my main line of investigation here.

does not warrant it. Most philosophers would agree that none of these counts as an emotion.

The reason why we make this distinction between fear proper and hyphenated fear-of-heights is, I suggest, that we think that there must be (possibly weak and defeasible) causal connections (a) between *believing* that some appropriate, action-relevant *triggering situation* (*S*) obtains and the subsequent onset of the emotion specific to that situation, as well as (b) between believing that *S* does not obtain and the cessation or diminution of that emotion. The states we have excluded are subject to no direct causal connections with belief, but rather to perceptual states of a specific sort. I might, for instance, experience vertigo simply because I am in a certain sort of perceptual state, whether or not that perceptual state leads to the appropriate belief. On the other hand, I shall not experience vertigo simply because I know that I am too high for safety, as in situation B.

Hunger and cold distress display an even more tenuous connection with cognition; they are triggered by bodily states that are not in themselves perceptual. I experience pangs of hunger simply because I am in a certain hormonal state, even when I know full well that I am not in need of food, and the reasoned belief that I am in need of food will not automatically make me hungry. In each of these cases, it is not of the essence of my state to be connected (however weakly) to reasoned cognition, that is, to the all-things-considered belief that the situation warrants it. This, I contend, is why vertigo and hunger do not count as emotions.

These considerations tend to support a cognitivist (more specifically a *doxastic*) view of the emotions; but cognition is not sufficient. Other demarcation criteria suggest that the emotions must be *primitive* states that we share with animals. I have already mentioned respect for the law as a state that is situation specific and action relevant, but unlikely to count as an emotion. Intellectual curiosity is an even more striking example. When one is intensely curious about something—for instance, something one has just discovered in the library or the laboratory—one may become physically aroused—one's heart rate may become elevated, one's breathing labored, and competing physical demands such as sleepiness or hunger may be blocked. Nevertheless, intellectual curiosity is not usually regarded by philosophers as an emotional state: one does not, for instance, find great European savants declaring that excessive curiosity is a sign of the overwrought and primitive condition of some class of humans, as they have held that excessive emotion is a weakness in women and savages. This suggests that it is actually a part of the way in which we

conceive of the emotions that they are states or capacities that we share with animals.

(2) The specific problem I want to tackle here arises directly from the employment of cognitive triggering and of ancient origin as demarcation criteria for emotion in humans. Many scientific studies of behavior stipulate that we share some emotional states with animals; psychologists cite comparative studies of "fear" or "anxiety" in mice and other animals; cognitive ethologists talk of "aggression" in primates, birds, and even insects; primatologists and ornithologists investigate social hierarchies and pair bonding. There is no reason to believe, however, that all these animal states satisfy the cognitive criterion. When psychologists talk of fear in rats, for instance, they are talking about unconditioned responses, such as panic in the presence of signs of predators, or conditioned responses as to a flashing light that signals the onset of an electric shock. There is no reason to think that either of these sorts of response is triggered by anything remotely like *reasoned* cognition or belief. These states are triggered in ways much more like our vertigo or cold distress than like the belief-mediated state in situation B.³

This yields something of a paradox:

In humans, fear is cognitive.

Fear is the same thing in humans as it is in rats.

In rats, fear is noncognitive.

Or:

In humans, fear is an emotion.

Fear is the same thing in humans as it is in rats.

In rats, fear (being noncognitive) is not an emotion.

Are these triads inconsistent? Some think that they are; for if fear is the same in rats as in humans, how can it be cognitive in humans, but not in rats? As a result, some philosophers are inclined to reject one or other of the statements they incorporate. Those of an *affectivist* bent would generally be averse to the idea that the human emo-

³ I am assuming the untenability of basing thought processes, such as those involved in situation B above, on classical conditioning. A rat may freeze when confronted with a stimulus that it has been conditioned to associate with an impending electric shock. This is nothing like the process by which I become afraid in situation B. Morrell makes a similar point with respect to fear in the presence of *unconditioned* stimuli: "A greylag goose that has never seen a hawk, and has never experienced harm, responds with fear when a hawk shaped piece of cardboard is passed overhead, but lacks the experience necessary to form any belief about the object of its fear" (*op. cit.*, p. 363).

tions are cognitive, and they would cite the continuity with animal states as evidence in their favor. *Cognitivists*, on the other hand, tend to oppose the idea that animals incapable of ratiocination could be subject to the same states as are humans; regardless of other similarities, they say, animals simply are not capable of fear or anxiety, properly conceived.⁴ Both positions are subject to difficulties: affectivists are unable to say why hunger and vertigo fail to be considered emotional states, cognitivists exclude animal states.

The problem I am posing is one even for those who conceptualize the distinction between emotions and other situation-relevant states in a way that differs quite radically from mine. Jenefer Robinson (*op. cit.*) insists that the startle reflex is an emotional response, and includes (more tentatively) hunger, pain, and sexual desire; and she allows that a "*passionate* interest in symbolic logic" might count as "an emotional response" (*op. cit.*, p. 66). So her judgments about demarcation are in these cases the opposite of mine. This is because she conceptualizes emotion as a bodily state which renders some feature of the environment salient and which mediates a behavioral or communicative response to the same. This approach to the emotions might be regarded as functional. Mine, on the other hand, is evolutionary: the principle I follow is that the emotions are grouped together because they possess common characteristics that have enabled them to evolve in parallel ways. Thus, I am interested in (though I do not discuss here) the physiological characteristics that make them, but not hunger, open to doxastic triggering. Despite this difference, Robinson does allow (a) that emotions are typically linked to belief in humans (though she does not think that this defines them); (b) that emotions occur in animals, though they are hardly ever, if at all, triggered by beliefs in animals; and (c) that there are action-relevant states in humans that are too intellectual to be counted as emotional. This combination of beliefs suffices to generate, in broad outline, the problem I wish to tackle here.

⁴ Jeffrey Moussaieff Masson and Susan McCarthy—*When Elephants Weep: The Emotional Lives of Animals* (New York: Dell, 1995)—say rightly that "Of all the emotions animals might feel, fear is the one that skeptics most often accept and one of the few that comparative psychology investigates" (p. 46). They quote a number of skeptics concerning the other emotions. A biologist is quoted as saying: "[H]uman love differs quantitatively from animal love...[A] human mother is unique because she has an abstract idea of the Good and that therefore human love, unlike animal love, has its ontogenetic beginnings in a spiritual bond between mother and child" (p. 65). The intention of such utterances is to combat the so-called "anthropomorphism" that describes animals in terms of categories that ought allegedly to be restricted to humans.

II. DEFINING SPECIES

(1) I shall be arguing that fear and the other emotions are "biological universals." I shall now attempt to characterize this category by reflecting on some recent treatments of *species*.⁵ Michael T. Ghiselin has suggested that species are individuals; David Hull agrees and concludes that species terms must be proper names.⁶ I shall attempt to accommodate the scientific concerns of Ghiselin and Hull, while still maintaining that species are a special category of universal and species terms are common names.

Through most of the history of biological taxonomy, it seemed that the most striking thing about species was that the organisms that belong to them were united by observable similarities they bear to one another, and distinguished from organisms belonging to other species by differences more marked than those which prevail within the group. A lion, for example, seems to be more like other lions in qualitative terms than like any tiger or household cat. If one imagined organisms arrayed in a multidimensional space, the distance between any two being proportional to their *dissimilarity*, the organisms of a single species would cluster close together, it was thought, and there would be a definite separation between the clusters that constitute different species.⁷ Species were conceived, therefore, as *similarity-based universals*, identified with the region in this "quality space," arrived at by abstraction from its member organisms, and defined by a formula that descriptively specifies the region.⁸

⁵ I am most directly indebted to Ernst Mayr's classic work, *Animal Species and Evolution* (Cambridge: Harvard, 1963). For a great deal of help with this section of my paper, I am grateful to my colleagues, Bernard Linsky and Alexander Rueger.

⁶ Ghiselin, "A Radical Solution to the Species Problem," *Systematic Zoology*, xxiii (1975): 536-44, and "Species Concepts, Individuality, and Objectivity," *Biology and Philosophy*, ii (1987): 127-43; and Hull, "Are Species Really Individuals?" *Systematic Zoology*, xxv (1976): 174-91, and "A Matter of Individuality," *Philosophy of Science*, xlv (1978): 335-60.

⁷ Notice that defining the kind *lion* using the similarity of lions one to another is implicitly underwritten by the greater difference between any lion and (for instance) any tiger: for if this did not obtain, the similarity criterion would gather in tigers as well as lions. Nevertheless, similarity has been historically more important in defining kinds than discontinuities. Criticizing this, Mayr argues that such unity is illusory. Differences within species can be very marked: age differences, sexual dimorphisms, and so on, can result in a qualitative spread greater than that found between "sibling species" which display a very marked similarity to one another. Because inanimate kinds such as minerals are generally much more unified than biological kinds, Mayr says: "[T]he most characteristic attribute of a species...is that it is separated by a gap from other[s]. The gap that surrounds a species is the core of the species concept" (*op. cit.*, p. 18). According to Mayr, it is not *overall* similarity or difference that is important, but the fact that for any two species there will always be some parameters that display significant differences.

⁸ Darwin is frequently quoted as a proponent of a form of this view: "I look at the term species as one arbitrarily given for the sake of convenience to a set of individuals closely resembling each other...it does not essentially differ from the term variety which is given to less distinct and more fluctuating forms"—*On the Origin of Species* (London: Murray, 1859), p. 52.

The idea that species are similarity-based universals falls immediately afoul of evolutionary biology; for

- (a) Species evolve, and change their characteristics over time. Regions of quality space do not change with time.

and

- (b) The condition for species membership is at least partly concrete and causal: the reproductive offspring of organisms belong to the same species as they. Since this condition applies independently of any resemblance that the offspring might bear to its ancestors, it shows that the definition of species by a set of resemblances is inadequate.

These facts do not controvert the idea that species are groupings of similar organisms; they are, though the similarities that prevail within a species change with time and evolution. They also do not absolutely prevent us from *defining* species in terms of characteristics shared by the organisms that belong to them; for in order to accommodate evolution, one could make the species definition an indefinitely long disjunction of time-bound conditions. For example, one could say that while at time t an individual would have to satisfy condition F in order to belong to species S , at time t' it is the distinct condition F' that determines membership in the same species. This makes the definition a temporal succession of time-independent conditions tracking evolution. Similarly, to accommodate the conditions of descent, one might build a "closure condition" into a species definition, which states that when a belongs to S , the reproductive descendants of a also belong to S .⁹

These maneuvers preserve the definability of species in terms of descriptive formulas; but they miss an essential point. The qualitative homogeneity and distinctness of species is real, but not fundamental in the way that similarity-based definitions suggest. Rather, they are the effects of underlying evolutionary processes. When we take account of the evolutionary facts that constitute the true unity of species, it turns out that species fall into a special class of universals; permitting and explaining similarities, but defined and understood without reference to them. Because they are so defined, the diachronic variability brought on by evolution, as well as synchronic

⁹ For more on such maneuvers, and on the confusions that have arisen through a lack of awareness of their availability, see Philip Kitcher, "Ghostly Whispers: Mayr, Ghiselin, and the 'Philosophers' on the Ontological Status of Species," *Biology and Philosophy*, II (1987): 184-92.

variation, are accommodated without the need for disjunctive definitions.

(2) Before we consider the implications of evolution, let us consider another point. As we shall see below, species figure in explanatory generalizations and predictions concerning individuals. This threatens the idea that they are similarity based. If 'tiger' meant 'large striped cat', it would not be right to say that Sher Khan is striped *because* he is a tiger, no more so than to say that I have children *because* I am a father. So 'tiger' cannot be defined in terms of the features that being a tiger is supposed to explain. Rather, it must be defined by reference to features that *cause* tigers to possess these features. *Explanatory universals* are defined, then, in a more demanding way than similarity-based universals—that is, in terms of shared *explanatory* features.

The distinction between similarity-based and explanatory universals is central to the treatment of "natural kinds" pioneered by Hilary Putnam and Saul Kripke¹⁰ in the early 1970s. As the reader will be aware, Putnam argued that no set of observable features would serve to define the application of the term 'water': for no matter how closely a liquid with a different molecular structure ('XYZ', in Putnam's famous example) might resemble water in other respects, it would not be water. Only something with chemical composition H_2O counts as water. This argument presupposes that, in the terms discussed above, *water* is an explanatory universal. We do not construct the universal by abstraction; we do not count things as water because they are colorless, tasteless liquids, or similar to other samples of water in just any way. We include them under the designation if and only if they possess the molecular structure that accounts for these and other water features (even if, as a consequence, we end up admitting some samples of water that are not very similar to most others).

This leads us to a problem: the meaning of 'water' seems to have been fixed long before it was known that its chemical composition is what made it water. How can this have been so? Putnam and Kripke proposed that the *extension* of a general term could be fixed independently of any identification of common properties. They proposed that a "natural kind" such as water is defined by ostending a specimen, and stipulating that *water* comprehends *everything that belongs to the same natural kind as that*. Putnam and Kripke thought that

¹⁰ Putnam, "The Meaning of 'Meaning'," in K. Gunderson, ed. *Minnesota Studies in the Philosophy of Science*, Volume VII (Minneapolis: Minnesota UP, 1975), pp. 131-95; and Kripke, "Naming and Necessity," in D. Davidson and G. Harman, eds., *Semantics of Natural Language* (Dordrecht: Reidel, 1972), pp. 253-355.

the meaning of 'water' did not change subsequent to this stipulation. To accommodate this supposed constancy of meaning, they posited that the *same-kind* relation would be pretheoretic, so that there would be a continuity between ancient uses of the term 'water' and our own.

It is questionable that this could be right, for two reasons. First, it is doubtful that the governing intuition of the Putnam argument, that XYZ is not water, could have been shared, or even formulated, by an ancient like Aristotle, since it was no part of Aristotle's own scientific framework that observational properties must be explanatorily derivative. Secondly, there is not one, but many kinds to which a sample of water belongs: hydrogen compound, hydroxide, liquid, and the like.¹¹ To define water, therefore, we need a more specific same-kind relation than the one that defines these other kinds. I do not know how this difficulty is to be resolved. I note, however, that, if we use not *same kind*, but *same chemical compound*, the problem would not arise. The latter relation is theory laden, however, and it would not maintain the constancy of meaning for 'water' that Putnam and Kripke wanted. I am going to let the constancy condition go here—I shall not worry about how Aristotle could have defined 'water'. Abandoning this consideration, I shall take advantage of the fact that using a theory-laden *same-kind* relation is an effective way to fix the extension of kind terms at the right level.

As molecular chemistry made it clear that for chemical substances 'same kind' means 'same molecular composition', it became evident that the actual molecular composition of water, H_2O , could be used to define its extension. Thus, the ostensive-relational fixing of the extension of 'water' could be replaced by a definition in terms of an explanatory feature possessed by all samples of water. This is an *intrinsic* feature of these samples, something that a sample of water could possess even if no other such sample existed. Generalizing from this example, Putnam, Kripke, and those following their lead assumed that as science uncovered the nature of the same-kind relation in other areas, *intrinsic* properties would become available to define natural kinds.

The proponents of the Putnam-Kripke approach to natural kinds generally assumed that the same conclusions would apply to biological species such as *tiger*.

First, it is an explanatory universal, rather than similarity based. This seems right: many biologists seem committed to the idea that

¹¹ Here, I am indebted to conversations with Ali Akhtar Kazmi.

something is striped *because* it is a tiger, rather than the other way around. "An individual belongs to species *X* not because it has the same species-specific characters as other members of *X*"; says one, "it has these species-specific characters because it is part of the species."

Second, species terms cannot be defined in terms of observational similarities. This conclusion is supplemented by the considerations presented in the last subsection. Since species comprehend variety, and evolve with respect to common features, both observational and explanatory, the species term cannot be defined in terms of the properties it possesses at a given time. Further, an animal's reproductive offspring count, for that reason, as belonging to the same species, and it is always possible that the offspring of members of a species will lack any given species-specific character.

Third, the extension of species terms can be fixed by the ostensive-relational method: that is, by ostending a sample and including everything that bears the same-kind relation to this exemplar. I argue below that this is correct, too, provided that the relation used to fix the extension gets it at the right level: *same species* rather than *same genus* or *class*. To ensure that this is so, we shall need to appeal to modern biological systematics to specify the relation.

Fourth, (though no very confident assertion was ever made about this), it was generally assumed that because species are explanatory universals, the ostensive-relational fixing of their extensions would be replaced by a definition in terms of an intrinsic explanatory feature common to all members of the species. Followers of Kripke and Putnam seemed to suggest that this nonrelational "real essence" would be microstructural, as it is in the case of *water*—perhaps some string of genetic code possessed by all and only tigers. It was thought that this feature would constitute a instancehood condition for *tiger*, as *having the molecular composition H_2O* is the instancehood condition for *water*.¹²

We shall see that this last assumption is mistaken. Although species are explanatory universals defined by ostensive-relational method, they are associated with no nonrelational real essence.

(3) I begin my argument by considering another sort of grouping of similar organisms, also important to evolution: the *deme* or local population. Demes consist of individuals that have access to one another for purposes of mating and reproduction. This access is minimally, if at all, restricted by genetic factors. Thus, a deme is sometimes thought

¹² The work most often cited in this connection is Stephen Schwartz, "Natural Kinds," *Behavioral and Brain Sciences*, iv (1981): 301-02.

of as a "gene pool" out of which new individuals are created by recombining genes already present.¹³

The organisms that belong to a deme tend to become and stay qualitatively homogeneous through the operation of a variety of causal factors. Within the deme, the same genes get recombined time and again and this sets a limit to the amount of variety that can be introduced when new individuals are created.¹⁴ Natural selection is a homogenizing force as well. Well-adapted characteristics originate in a small number of individual organisms, and then spread through the population because these individuals are more fit than others. Natural selection will also force a qualitative separation between a deme of one species and demes of other species present in the same location. The reason is that, if two populations are competing for precisely the same resources, then the one able to use those resources better will normally drive the other into extinction. Consequently, competing demes tend to evolve in different directions, and to exploit the environment in different ways; that is, they will tend to occupy different ecological "niches." In this way, demes will tend to be both internally homogeneous as well as qualitatively distinct from other demes present in the same place.

A deme is a group of similar individuals, but it is not a universal. It is not an attribute at all, but is more aptly regarded as a collection of interacting organisms, or even as a complex individual which has other individuals (genes or individual organisms) as constituent parts.¹⁵ *Being a member of a given deme*, however, is a shared attribute of the individual organisms that participate in its reproductive structure. Is it an explanatory universal? Can one say, for example, that

¹³ Mayr defines a deme as "a group of individuals so situated that any two of them have equal probability of mating with each other and producing offspring, provided...they are sexually mature, of opposite sexes, and equivalent with respect to sexual selection" (*op. cit.*, p. 136). The notion of mating is odd here: it does not make a lot of sense that I could mate with another male if only we were "of opposite sexes." It is the conception of recombination that is crucial. My genes are as likely to get recombined with those of a given member of my deme as with those of any other. Mayr allows that this conception of a "panmictic unit" is an idealization.

¹⁴ The so-called fundamental theorem of natural selection, propounded by R.A. Fisher, says that the rate of evolution in a population is proportionate to the variance of fitness in it. This is a good example of facts about evolution being explained by population-level variables.

¹⁵ Ghiselin and Hull take the position that such collections are individuals. Mayr—"The Ontological Status Species: Scientific Progress and Philosophical Terminology," *Biology and Philosophy*, II (1987): 145-66—argues that "population" is a better characterization, while G. Ledyard Stebbins—"Species Concepts, Semantics and Actual Situations," *ibid.*, pp. 198-203—suggests "systems." This particular controversy is not of interest to me in the present context.

membership in a particular deme *explains why* this finch has a beak of a particular shape? Clearly, membership in a deme has no *concurrent* influence on the features of the individuals that belong to it—if an individual were to move in or out of the deme, its qualitative features would not change. The deme does not maintain these features or create them in already formed individuals.

Nevertheless, membership in the deme does play another sort of explanatory role. As the influences mentioned above work on a population, the frequencies of genes it contains changes. Such attributes of the deme are among the causal factors that shape the process by which a deme gets homogenized and the member organisms get their attributes. In general, the causal relations that obtain within a deme and those which work on it, and its prior gene frequencies, will be parts of the explanation of evolution. In this sense, membership in the deme explains why its individual members have many of the properties they do. Take an extreme case. A finch has a beak of a certain shape because, as a result of past selection, there are no other genes available in the deme in which it was created. Mutation or immigration aside, there is no other possibility for the individuals produced by the deme. Thus, the finch has this sort of beak because it was produced by this deme. In other cases, the probability may be considerably more distant from certainty, but gene frequencies support an explanation nevertheless. This man has sickle-cell anemia because his ancestors belonged to a deme in which the sickle-cell allele provided some protection against malaria. This tiger is an albino, because breeders ensured that the gene pool that produced it contained as many albino genes as they could manage.

Membership in the deme is an explanatory universal, then, not because it denotes some common nonrelational feature of its individual members, but because the composition of a population, the forces that impinge on it, and the ways in which it produces new individuals predict and explain the types of individual it creates.¹⁶

(4) A species, too, is a gene pool, though more extended than a deme. It, too, is a composite of individuals that produces new individuals and it, too, is one of the factors responsible for the evolution of property distributions among the ensembles of individual organisms it creates.

A species can be thought of as a collection of demes spread out through space and time, and connected by gene flow. Individuals

¹⁶ We are not obliged here to insist that there are no features shared by all members of a deme (I was assuming above that the beak was an instance of just such a common feature), or even that there are no shared microstructural features. The point is that membership in a deme can be used to explain individual characteristics without appealing to common intrinsic features.

migrate from one synchronic deme to another, carrying their genes with them, and there is gene flow from temporally earlier to later demes by inheritance. A species is the extended entity within which such gene flow occurs. A species is, moreover, guarded from gene flow from other species by so-called "isolating mechanisms," genetic factors that restrict cross-species mating.¹⁷ A species is thus a self-contained unit. Take an individual organism and include all the others—past, present, and future—with whose genes its genes could recombine by a natural process of sexual mating, and you have a class that constitutes the membership of the species to which the original individual belongs.¹⁸ Thus conceived, a species is, to combine the characterizations of Theodosius Dobzhansky and Ernst Mayr, "the largest and most inclusive...reproductive community of...actually or potentially interbreeding natural populations."¹⁹

The confusing thing about species is that, notwithstanding the above, they are used in biology as archetypical universals. In fact, their role in biological taxonomy is strongly analogous to that of structural classifications in chemistry. It is not possible to discuss the point in any detail here, but I summarize. First, in most areas of biological science, species categorizations are the primary vehicle for explanatory generalizations and predictions concerning individual organisms.²⁰ Further, species are unlike demes in that an individual

¹⁷ These isolating mechanisms evolve as species and their demes come to occupy characteristic niches: the qualitative distinctness that this demands results eventually in reproductive isolation, and thus in the creation of a group that maintains its qualitative distinctness from other such groups and evolves independently. A new species has been created when morphological change within a subgroup of a pre-existing species minimizes gene flow in or out, so that the subgroup becomes isolated from genes of the type present in the ancestor group.

¹⁸ Kitcher gives a more complicated definition along the same lines (pp. 187–88). As he notes, the reproductive compatibility criterion is vague.

¹⁹ Mayr quotes Dobzhansky (p. 19). The definition does not apply to asexually reproducing species. The following discussion is meant to apply only to Mayr's "biological species."

²⁰ Hull and Ghiselin would disagree on the grounds that there are no laws of nature of the form "All Ss are F," where 'S' stands for a species name. As Alexander Rosenberg—"Why Does the Nature of Species Matter? Comments on Ghiselin and Mayr," *Biology and Philosophy*, II (1987): 192–97—explains: "[T]he generalizations about particular species on which taxonomic decisions rest are full of exceptions, and there is no background theory that will enable us to eventually eliminate, reduce or even explain these exceptions" (p. 195). This is most likely correct, but to my mind it is beside the point. Certainly, generalizations of the form 'Most tigers are striped' support counterfactuals such as 'If this had been a tiger, it *probably* would have been striped', and explanations like 'This is striped because it is a tiger'. Why do probabilistic generalizations not count as "laws of nature?" Why do such generalizations even have to be "laws of nature" in order for the terms that occur within them to be counted as explanatory universals?

member cannot join or leave a species as it can a deme—since the species is inclusive of all demes within the reproductive, ecological, and ethological barriers that isolate it from outbreeding, movement from one deme to another still leaves the individual within the species. This permanence of species membership supports the explanatory and predictive value of species taxa. Moreover, the logic and grammar of species terms seems to dictate that the relationship of an organism to its species be treated as that of instance-hood. We say “This *is* a tiger,”²¹ and if it is also true that “the tiger is a cat,” we are entitled to conclude that the thing ostended is a cat. Species are thus associated with the logicolinguistic practices of “*is-a*” predications, which again seems to indicate that they are treated within biology as important categorizations of individuals, categorizations to which explanatory properties are attached. Lastly, species terms have divided reference: they take the plural and are quantified. It makes sense to say: “There are ten tigers in the zoo.”

It seems, then, that while species are populations just as demes are, they need to be treated analogously with universals with respect to their taxonomic-explanatory and semantic roles. Our treatment of demes suggests how to accommodate this duality. In the case of demes, we distinguished between the deme itself, a causally structured collection of individuals, and *membership in the deme*, a universal. Here, we need to make a distinction between *species populations* and *species universals*. Species populations are composite individuals, the extended reproductive communities of Mayr’s definition. The species universal corresponds to *membership in the species population*.²² The linguistic practices of biologists make the distinction explicitly in the case of demes; with regard to species, they do not.

(5) I return now to the third and fourth characteristics of species universals enumerated in subsection (2) above. The extension of a species term can be fixed by the ostensive-relational method. But the

²¹ Ghiselin disagrees: “Parts are not instances—we do not say ‘This thumb is a Michael Ghiselin’...to some people it is not obvious that it is equally wrong to say ‘Trigger is an *Equus caballus*’”—“Species Concepts” (p. 128). I suppose that he is playing on the infelicity of using an article with the Latin species term without interposing ‘member of’. Does he think it wrong to say “Trigger is a domestic horse,” or “There are thousands of domestic horses on Earth?”

²² Here, I do not mean to close the door on Kitcher’s suggestion that we should be “pluralistic” with regard to species concepts—“Species,” *Philosophy of Science*, LI (1984): 308-33. See also the comments of Elliott Sober in “Sets, Species, and Evolution: Comments on Philip Kitcher’s ‘Species’,” *Philosophy of Science*, LI (1984): 334-41. Kitcher proposes that some species classifications might correspond to similarity-based universals (as I call them) or to real essences. As I stipulated in footnote 19 above, my proposal concerning universals is meant to apply only to “biological species,” that is, the extended reproductive families discussed above.

pretheoretical "same-kind" definition does not distinguish between species and the other taxa to which the organism belongs—genera, classes, and the like.²³ As we saw earlier, the most sophisticated theoretical investigation of species reveals that organisms are conspecific if they belong to same extended reproductive community. Thus, we may define a species population Σ as follows:

- (a) This (ostended) organism belongs to Σ .
- (b) If x belongs to Σ , and y belongs to the same extended reproductive community as x , then y belongs to Σ .
- (c) Nothing else belongs to Σ .

The species term S is then defined:

$$S(x) =_{df} x \text{ belongs to } \Sigma^{24}$$

The species term thus applies to any individual that stands in a certain relation (*belonging to the same extended reproductive community*) as the originally ostended individual; this relation is given to us by theory. For two reasons, it cannot be redefined in terms of an intrinsic feature shared by every species member: first, though the members of a species may in fact have certain intrinsic features in common, they need not and in the future may not; second, membership in the species is prior from an explanatory point of view.

The status of species as universals and of species terms as common nouns is saved by defining them in terms of membership in a population. Membership in a population is a relational property: an individual possesses this property as a consequence of the relations it bears to other individuals. I have argued that membership in a population can figure in a form of explanation concerning the properties

²³ On the nature of higher taxa and their definability, see particularly Kevin de Queiroz and Jacques Gauthier, "Phylogeny as a Central Principle in Taxonomy: Phylogenetic Definitions of Taxon Names," *Systematic Zoology*, xxxix (1990): 307-22; and de Queiroz, "Phylogenetic Definitions and Taxonomic Philosophy," *Biology and Philosophy*, vii (1992): 295-313.

²⁴ Hull is scornful of designating the species property in the way I have. "One way of salvaging essentialism is to claim that being a member of a class is the essence of the class; e.g.,...being a horse is the essence of horse.... If there was ever reason to suppose that essentialism was no longer a viable metaphysical position, this means of salvaging it is it"—"Are Species Really Individuals," see p. 176. My move is not so vacuous: (a) because there is a distinction between the species population and the species universal (thus there is a difference between defining the universal in terms of the population, and defining the species in terms of itself); (b) because the defining characteristic of belonging to the population is relational, whereas most essentialisms require essences to be intrinsic features; and (c) because it is the status of the population in evolutionary theory that gives us the route to a definition like the one offered. As argued in the text, the extension of chemical terms have no such status in chemistry.

of the members of that population, even though membership corresponds to no common intrinsic feature. Because this is so, explanation by appeal to species membership does not demand the replacement of the relational characterization of species population by one in terms of an intrinsic real essence. This is important. Variability in terms of intrinsic features is a precondition of evolution. Even though populations display common features, any such feature may always be modified in a new organism, and subjected to selective pressures. So, the only way in which the theory of evolution can be reconciled with the traditional conception of species as explanatory universals is if there is a way in which membership in a relationally defined population can be explanatory.

There is, then, a crucial difference between biological systematics and chemistry which makes the transition to a nonrelational real essence unnecessary in the former. The difference is that, in biology, the extension of species terms corresponds to an entity, the population, that enters into explanatory causal relations. In chemistry, the extensions of terms like 'water' have no such ontological status.

I argue in the next section that universals like *fear* possess the same characteristics as those outlined above. Obviously, there are differences: the instances of fear do not, taken together, form a population of interbreeding organisms. Nevertheless, emotions are an inheritable characteristic in respect of which organisms differ, and as a consequence the emotions evolve. They, too, are definable only by the ostensive-relational method. Like species, they have no nonrelational real essence capable of replacing the relation by which their extension is defined. In short, they are biological universals.

III. FEAR AS A BIOLOGICAL UNIVERSAL

(1) The problem that we are trying to solve has variation at its heart. Consider our "paradoxes" above. Fear is something that, because of evolution, takes different forms in different species (indeed in different individuals, and even different occurrences). The first paradox reflects this variability. The doxastic triggering of fear is a qualitative characteristic. However typical this characteristic might be with respect to occurrences of fear in humans, it would be a mistake to build it into the definition in such a way as to exclude all nondoxastically triggered states. Rats are different from humans in this respect.

Our second paradox, too, is the result of biological universals cross-cutting against qualitative specifications. Above, we arrived at a qualitative characterization of human emotions, namely, that they are triggered by cognitive states. At the same time, we wished to maintain that in some other animals the same states—fear and the like—could

be noncognitive. How are we to accommodate both positions? In one of two ways. We might want to say that though fear is an emotion in humans, it is not truly so in other animals. Such a position would allow us to retain a cognitivist characterization of *emotion*, while allowing that each of the types of occurrence so designated occurs in forms that do not satisfy this characterization. Alternatively, we might hold that the term 'emotion' applies to those dispositions which have a certain characteristic as they occur in humans. Fear in rats is an emotion not because it is different from such nonemotions as fear-of-heights or hunger *tout court*, but because it is the same state as one that is different from these states *in us*. It may very well be the case that the states so designated have a common characteristic (for example, that they are routed through the frontal cortex of the brain), but this feature is interesting only because they permit the doxastic triggering that interests us with regard to human emotion. By the same token, fear-of-heights would not be counted an emotion even in rats, not because its intrinsic character is all that different from that of other avoidance modules, but because of its subsequent history—that is, because it did not become sufficiently connected with cognition and learning.

The paradoxes are the result of evolutionary change. They are no more difficult to accommodate than individual change in a theory of personal identity.

(2) The variable character of fear across species shows us that cognitive characterizations, such as Davis's, are inadequate. The young greylag goose's fear of a hawk-shaped piece of cardboard is not a cognitive state; nor is the chimpanzee's fear of snakes and chimpanzee corpses.²⁵ Davis's definition fails even within the human species. The child's fear of the dark or of loud noises is not cognitive; there need be no "apprehension of hurt"²⁶ in it. Even among adult humans, the definition fails. Adults as well as children can be afraid of being alone, of the dark, (like chimpanzees) of snakes, and of dead bodies. They can be neurotically afraid, or even incapacitatingly terrified, of sex or of members of the other sex. Such fears are not mediated by cognition; they may indeed be accompanied by the firm belief that there is no impending hurt in the situation. Nevertheless, they are involved in cognitive complexes characteristic of

²⁵ D.O. Hebb, "Emotion in Man and Animal: An Analysis of the Intuitive Processes of Recognition" and "On the Nature of Fear," *Psychological Review*, LIII (1946): 88-106 and 259-76.

²⁶ Robert Kraut quotes Collingwood as citing "a woman's fear of a mouse or a boy's fear of the dark" to controvert Thomas Hobbes's view that the "the opinion of Hurt from the object" is an essential part of fear—"Feelings in Context," this JOURNAL, LXXXIII, 11 (November 1986): 642-52, see p. 643.

fear as we know it: as is shown, for instance, by the fact that the fear of any object can be heightened (or even created) by associating it with the dark or with the presence of a corpse. It seems, then, that a number of the modules that are found unconnected in some animals are found bundled together in humans, and triggered by cognitive processing and learning. In a similar vein, the emotions we (humans) experience in response to works of fiction are not doxastic; they occur in the full knowledge that the cognitive attitudes that trigger them are false. Pace Kendall Walton (*op. cit.*),²⁷ these fearful reactions can be real.

What this variability shows is that it is a mistake to attempt a definition of emotions in terms of shared intrinsic characteristics. It is, no doubt, an extremely valuable generalization concerning the human emotions that they are often, or even typically, doxastically triggered—indeed, I have alleged that it is the human capacity for such triggering that demarcates the emotions from other action-relevant states. But this should not lead us to make cognitive conditions essential to their very occurrence by building such conditions into their definitions. Such attempts are defeated by variation within or across species, and by the explanatory primacy of the collections of things within which natural selection operates.

It has been suggested that a more plausible alternative to a cognitivist definition is a functionalist one. Robert Kraut, for instance,

²⁷ To be fair to Walton, the bulk of his discussion is concerned with showing that in such cases we are not afraid *of the fictional object*. Walton's proponent, Charles, is not afraid of the green slime he sees in a movie, because he knows that it does not exist. So far, I sympathize. But one might ask: If he is not afraid of the green slime, is he really afraid?—Is there anything else to be afraid of? Walton thinks not, and makes his famous move: "The fact that Charles is *quasi*-afraid as a result of realizing that make-believingly the slime threatens him generates the truth that make-believingly he is afraid of the slime" (*op. cit.*, p. 14). Quasi-fear outside the scope of the make-believe operator, fear inside. But I do not accept the 'quasi'. It is make-believe that Charles is afraid *of the slime*. He is, however, afraid period. Perhaps *fear* is an example that gives the Walton example a degree of plausibility: as with being startled by a popping balloon (footnote 2, above), Charles is *scared* or *afraid*, but is he *really* fearful? But this a point irrelevant to Walton; and there are examples less favorable to his analysis. Can we not be truly envious when we read of the enormous wealth of the (fictional) families portrayed by Evelyn Waugh? Are we not truly indignant when we read of the murderous ways of upper-caste Indian landowners in Vikram Seth's *A Suitable Boy* or Rohinton Mistry's *A Fine Balance*? It may be that in these cases, we are led to emotional involvement with real situations, by reflection on the situations fictionally portrayed, and their resemblance to real life. Nevertheless, our make-believe involvement with the fictional case does engage our real emotions: I do not find it plausible to say that the *only* emotion evoked by our reading of Seth and Mistry arises out of our engagement with the *reality* of caste injustice: someone could be indignant even if they had never heard of the caste system.

thinks that emotions are "feelings in context," and considers a "functionalist strategy," which treats of the feelings as states which "by virtue of their systematic interconnections and functional organizations, realize the functional roles definitive of the emotions" (*op. cit.*, p. 643).²⁸ Following this strategy, fear might be defined as a feeling that enables one to respond appropriately to a dangerous situation. This definition is not explicitly cognitive, and so it is inclusive of animal states. Allegedly, it also enables us to accommodate the "non-standard" emotions such as those we experience in response to fictions. Such states admittedly do not enable us to respond to dangerous situations, but it does not follow that they do not have this function, since not everything is able to achieve its function in every situation. Kraut says: "What renders *F* a feeling of [fear]...is the presence of certain features...in the situations with which *F* covaries under optimal conditions," but he stipulates that under suboptimal conditions, a feeling might occupy the emotional role without these covariant conditions being satisfied (*op. cit.*, p. 649). Optimally, a feeling of fear will enable me to respond appropriately to a dangerous situation, but when I experience fear in response to a movie, or when I am left alone in a funeral parlor at night, it is because conditions are suboptimal.

Does the functionalist strategy really solve the difficulty? How is a suboptimal occurrence of fear to be identified as such, if it lacks central properties of normal fear? Kraut seems to rely on something like Robert Cummins's²⁹ identification of function with causal role; but since fear of fictions does not occupy the same causal role as "normal" fear, the applicability of Cummins's analysis here is moot. It might be thought more plausible, therefore, to use an analysis of function that tackles nonperformance head-on. According to Ruth Millikan's³⁰ analysis, an item has the "proper function" of enabling an animal to respond appropriately to dangerous situations if (i) it is a "reproduction" of some prior item that enabled an animal to respond appropriately to dangerous situations, and (ii) we now possess the item because that prior item did so. So understood, a particular occurrence of fear may in fact possess the *proper function* of responding to dangerous situations even though, in fact, it does not manage to do this. It has this proper function because it is a reproduction of an item that...and so on. The difficulty is: What makes an item like fear-in-response-to-fictions a "reproduction" of the prior item? After

²⁸ Kraut abandons the functionalist strategy in the end.

²⁹ "Functional Analysis," this JOURNAL, LXXII, 20 (November 20, 1975): 741-64.

³⁰ "In Defence of Proper Functions," *Philosophy of Science*, LVI (1989): 288-309.

all, it fails to perform the very service by which we identify that prior item.

The plausibility of analyses like Millikan's (as a basis for functional definitions) rests on their application to items like hearts and eyes. These items are morphologically, and even histologically, distinct from surrounding body parts, and occur in a relatively predictable place relative to other parts of the body. Thus, even a defective heart or eye is easily identified as such. Such cases make it easy to think that we are given a class by perception, the problem being to state what its essential defining characteristics are. (Note, however, that when we do identify a defective heart as such, we do so not by the function but by morphological or other such criteria.) But not all body parts are so easily identified either across or within species. Something like a skeletal muscle is compositionally similar to other muscles, and can only be identified by its attachment to particular bony structures, that is, relationally. Other anatomical elements—for example, the carotid artery—have no definite boundaries, and are identified in an even fuzzier way.³¹ A glance at any textbook of comparative anatomy shows how theory-laden identifications of parts are when applied across species. Speaking of pterosaurs, one such text says:

...the fourth finger [was] tremendously elongated to form the principal support for a wing membrane.... The fingers in front of the fourth digit were reduced to small hooks.³²

Thus, similarity is not necessary for identification, and it is not sufficient.

Animals without jaws...can develop movable plates around the mouth opening that serve after a fashion as weak "jaws"...the appearance of jaws in vertebrates was brought about by a transformation of anatomical elements [namely, gill arches] that had originally performed a function quite different (*ibid.*, p. 30).

Distorted fingers that do not function as such, structures that perform the function of jaws but are not jaws, gill arches that have turned into jaws.... It is clear that the principles used to make these identifications of body parts are neither trivial nor easy to come by. The "same" part differs from species to species in both form *and*

³¹ For a taxonomy of such cases, see Bruce A. Young, "On the Necessity of an Archetypal Concept in Morphology: With Special Reference to the Concepts of 'Structure' and 'Homology'," *Biology and Philosophy*, viii (1993): 225-48.

³² E.H. Colbert and M. Morales, *Evolution of the Vertebrates* (New York: Wiley-Liss, 1991, 4th edition), p. 177.

function. Positional relations, similarities, compositional discontinuities, and ontogenetic criteria have to be used to establish the sameness of part across organisms and across species. Thus, it is not necessarily clear when we find, in an organism of our own or another species, a vessel of a very different shape and function in the place of the carotid artery, whether we are to say that that part has changed its form and function or is simply absent.

When it comes to identifying a fleeting mental occurrence as an instance of *fear* when it is not performing its supposed function, we are in uncharted territory. The analyses of function summarized above are useful enough in attributing functions once the class of fear occurrences has been identified; they are of no use in identifying the class. In responding to dangerous situations, one individual may freeze, another may attack, yet another may warn or get together with members of the same group. Some individuals will be afraid of the dark, some of fictions, some of corpses, some of sex. Is this a case of different emotions, or of the same directed to different objects, leading to different actions? The analyses of function offered by Cummins and Millikan do not offer us an answer. Because they do not, it seems dubious that a *functional* definition of fear is possible, and even more dubious that such a definition will help us identify *optimal* or *normal* instances of fear.

Why is it thought plausible that there are normal as well as subnormal instances of fear (or quasi-fear, as Walton calls it)? Because it is thought that there is something like a real essence of fear, a characteristic that explains its typical manifestations. As Elliott Sober³³ has shown, one maintains this hypothesis in the face of variety by positing interfering "forces" that on occasion prevent or frustrate the working of the essence. This is what the subnormality assumption attempts: it claims that fear sometimes manifests itself in peculiar ways because nonstandard conditions intervene to interfere with its real essence. The biological universal model gives us an idea of how fear can be an explanatory universal without making such an assumption.

This mental event is triggered by the "apprehension of hurt," *this other* event is not. We may wish to explain the characteristics of both by reference to the evolutionary pressures operating on some class of occurrences to which both belong, including (a) the fact that events like the first tend to keep us out of hurt-causing situation, (b) selection favoring this tendency, and (c) various similarities and connec-

³³ "Evolution, Population Thinking, and Essentialism," *Philosophy of Science*, XLVII (1980): 350-83.

tions between the two kinds of events that might tend to preserve the second despite its failure to display the selected property. It would be a mistake to think that this kind of explanation, functional though it might be, consists in the attribution to the second event a lesser degree of some capacity that the former possesses in full. Like explanation in terms of biological universals, it reaches down to the individual only through some class to which it belongs, and the variation present within this class. But in order to do this, the occurrences must first be identified as belonging to the class. By the very nature of the case, functional specification is not going to help us do this.

(3) The variability of body parts and of the emotions discussed above suggests that, like species, they are biological universals. As we have seen, the Putnam-Kripke method of ostensive-relational definition helps us fix the extension of universals in cases where characterizations in terms of common properties do not serve. In the case of fear, one would need to ostend (successfully) an occurrence of fear, and stipulate that everything else of the same kind is also fear.

It is here that the so-called "optimal" or "normal" cases are useful. We are interested in the kind of fear described in situation B above: cognitively triggered, leading to classic symptoms, and occurrent in a human. Fear is then the class of occurrences defined by some *same-kind* relation with respect to the exemplar. Provided it is properly specified, this relation will gather in some of the other cases that we have discussed—fear in rats because though not cognitive, it is of the same kind as the exemplar. If we are right in thinking that fear is a biological universal—that is, that it defines a class on which selection has worked, and continues to do so—this relational characterization of fear will not be replaced by a nonrelational characterization, a specification of a characteristic possessed by all and only occurrences of fear. Fear will be understood as constituted by membership in the class characterized as above.

I argued above that a pretheoretical characterization of the same-kind relation would not be sufficient to fix the extension of a term if it did not pick out a kind at the right level. There are many kinds to which an occurrence of fear belongs; we are interested in only that *same-kind* relation that binds the exemplar together with other occurrences of *fear*, not some broader or crosscutting classification. We need a more demanding characterization of the requisite *same-kind* relation if the definition is to do its job. It is the business of empirical science to investigate both the same-kind relation and the class of events defined by it. But it does not seem that empirical science has succeeded in giving us a good way in which to understand homol-

ogy. The situation is not unlike that which obtained sixty years ago with respect to species. Since that time, the notion of a *biological species* has become much clearer. The same could happen for homology. At present, however, it appears that the same-kind relation is not well understood as it applies to items of this sort—the difficulties alluded to above have not been resolved to the satisfaction of all. The *same-kind* relation is thus a placeholder awaiting further clarification. Until an adequate theoretical understanding of the same-kind relation is achieved, as it applies to body parts and “mental” events, no adequate definition of these things will be possible, even within the framework of ostensive-relational definitions.

David Lewis³⁴ has argued that mental kinds should be defined functionally, but that in different “populations,” they might coincide with different morphological kinds in a way that admits of variation both across and within populations. As test cases, he imagines a “Martian” whose pain is functionally similar to that in humans but physiologically very different, and a human “madman” whose physiologically standard form of pain is functionally aberrant. According to Lewis, the case of the Martian shows that different states can occupy the functionally defined role of pain in different “populations.” The case of the “madman” is different: “the state that occupies the role (of pain) for the population (that is, humans) does not occupy it for him” (*ibid.*, p. 127). My argument tells against the functionalist definition of pain, and the considerations of the next subsection tend to tell against the idea that the Martian suffers *pain*. I reject the notion of defining pain by its causal role, and the practice of using this definition to identify the states that in each population occupy that role. But the idea that mental kinds like pain or fear may coincide with morphological kinds within particular biological taxa is one that I can accept, given the proviso that they will do so in a way that admits of variation. Provided that one allows for change, that is, for the possibility that fear might come in the future to coincide with a different morphological kind, the approach that I am taking does support the at-least approximate co-extensiveness of biological universals with some morphological characters. But (much as Lewis intends for his functional definitions) the ostensive-relational definition will be explanatorily prior to these shifting and uncertain liaisons with morphological kinds.

(4) Although we do not know exactly how fear will be defined, we do know that the definition will respect descent; that is, some states

³⁴ “Mad Pain and Martian Pain,” in *Philosophical Papers*, Volume 1 (New York: Oxford, 1983), pp. 122-32.

will be included because they are phylogenetically connected with the human exemplars, even though they lack some of the characteristics that seem salient in these cases. In such cases, evolutionary taxonomy identifies elements as “the same” across species when they are homologous, that is, when they have a common evolutionary origin.³⁵ In the above mentioned cases of fingers and jaws, the homology consideration trumps form and function. That a particular structure has a very different shape in pterosaurs does not count against its being recognized as the “fourth digit,” that something performs the function of jaws in some *Agnatha* does not make it a jaw. What counts is common origin: the elongated digit has an “ancestor” in common with the corresponding structure in other vertebrates, and the jaw-like structure has a function but no ancestor in common with our jaws. (How “ancestors” are identified in the case of bodily parts is, as we said, a contested question.) It seems possible that the emotions, too, will be regarded as the same or different across species on the basis of homology. This possibility is rich in consequences for the philosophy of mind, some more intuitive than others, and I conclude this essay by gesturing briefly toward some of these.

Emotions are mechanisms that cause an animal to respond to specific situations in specific ways. But it is possible that, just as some creatures use nonjaws to perform some jaw functions, similar responses to a given situation could be mediated by different mechanisms. It is thus not inconceivable that, even if an animal belonging to a distantly related taxon attempts to avoid danger, it might do so by the operation of something other than fear. Within functionalism, it is hard to make sense of this claim: functionalists identify elements as the same when they have the same functional role within a system.

There is an interesting passage in *When Elephants Weep* in which Jeffrey Moussaieff Masson and Susan McCarthy describe a dying mother spider enfolding her young to herself when the entire brood was immersed in alcohol.

³⁵ Philosophers are sometimes inclined to favor a more restrictive criterion than this. In an otherwise interesting article, Crawford L. Elder, for example, makes the claim that kinds like *lung* cannot have unequivocal application across species—“A Different Kind of Natural Kind,” *Australasian Journal Of Philosophy*, LXXIII, 4 (December 1995): 516-31, here p. 526. I suppose that this would rob me of one of the main motivations of my argument, namely, that fear occurs in many species. But I cannot think what would motivate such a restriction; the proper provenance for a kind based on Millikan’s reproductions—Elder’s “different kind of natural kind”—is common origin, not sameness of species.

Can a spider love her babies? Was it a mere reflex that caused the...spider to reach for her young?... It is so hard to have insight into a spider's mind that it is almost impossible to guess.... Yet spiders have evolved to produce complex venoms and digestive fluids.... Building a spider's web is an extremely complicated behavior. One can argue that a spider is not really a simple organism and that the development of maternal love might well be a shorter evolutionary step than web building (*op. cit.*, p. 68).

Masson and McCarthy think that the question is one of evolutionary development: Is the spider *advanced enough* for us to say that it feels love? But there is another relevant parameter which they ignore, and it might be important to the way we answer their questions. Is the evolutionary development along the same path or along parallel paths. Do the two instances of maternal instinct have a common origin, or did they originate independently?

Masson and McCarthy continue:

What if it was discovered that when a mother wolf spider sees young spiders, her body is flooded with a hormone whose presence is associated with feelings of love in higher animals? Would that be evidence that the spider loves her young? What if it was a hormone peculiar to spiders? Would that mean it wasn't love (*op. cit.*, p. 69)?

The focus here concerns moral questions (broadly conceived). So let us stipulate right away: as far as certain moral evaluations and moral consequences of maternal behavior are concerned, the identity of the hormone that floods the spider's body makes no difference. From the point of view of biological explanation, however, it might make a difference. Because parental investment is an important reproductive strategy, it has evolved independently several times. If spider maternal behavior evolved independently of human, that is, if there is no common ancestor of both forms of behavior, then it may well make sense to say that though the spider's state has the same *moral* significance as love, it is something different.

What sort of consideration would motivate us to make such a distinction? Look again at the hormone question. Functionally comparable items that are not homologous will share functionally relevant features. The dying spider reached out her legs and folded her dying babies to herself in an attempt to protect or comfort. A human mother might do something very similar. The similarity of the behavior is accounted for by its functionality. But there are other accompaniments of human love that are less functionally relevant: characteristic bodily feelings, modes of expression peculiar to hu-

mans and closely related species, genetically induced emotional associations, common learning patterns. Some of these may be caused by the hormone secreted when the mother sees its child, or by some other functionally irrelevant characteristic: for example, the location of the neurological substrate of maternal feelings within the brain. So these accompaniments might be there, not because they are functionally relevant, but because they are caused by the hormone in question. Now, homologues are more likely, as a result of their common origins, to share functionally irrelevant characteristics. So, where functionally irrelevant characteristics are relevant to demarcating kinds, the kinds are more likely to be based on homologies.

Suppose that the spider's behavior, though just as advanced in evolutionary terms as human mother love, is caused by a different hormone and neurophysiologically realized in a different way. This might have the consequence that the spider has feelings that are qualitatively quite different from the human, even though the feelings are functionally the same. Suppose that maternity is differently wired up with respect to the spider's brain. This might result in differences of expression. Another point: suppose that human mother love is shaped in a way relevant to human evolutionary history and spider maternity correspondingly so. This might mean that while the functional characteristics of the emotions were comparable, since the salient ecological challenges are comparable, many side manifestations would be different. For example, it might be that spiders cease to feel for their young when they reach a certain age, and care for all little spiders without differentiation until then. Humans, on the other hand, might continue to care for their offspring forever but not for all little humans undifferentiatedly. Humans might care for their grandchildren, spiders might not. Humans might get sentimental when they see pictures of small children, spiders might not. Would these differences not count as evidence that these are different mental states, even though comparable with regard to a central function? It is reasonable to think so. But this means that, to some extent, functionally irrelevant or functionally peripheral characteristics are important to the identification of emotions or feelings as the same or different. This provides evidence that the emotions are indeed homologies.

Opponents of functionalism have long claimed, largely on grounds of intuition and ordinary language, that irrelevant accompaniments of various mental states, including the emotions, are important to their identification. "Neurophilosophers," for quite different sorts of reasons, make the same sort of claim: for instance, they

choose between computationally equivalent models of mental functioning on the grounds that some are more "biologically real" than others. Without wishing to pronounce on the merits of these moves, it is surely significant that the categorization of fear and other emotions as homologies would give such claims a scientific foundation.

Of course, it should not be assumed that the ultimate definition of fear in terms of homologies will necessarily be intuitive in every respect. It might be that in some species a homologue of fear will serve a completely different function: perhaps it would serve primarily to increase the attachment of the young to established social hierarchies. To aid in the performance of this function, such a species could have evolved a special mechanism to defeat the freezing response characteristic of fear. Thus modified, the state might lead members of this species straight into dangers when it is necessary to do so to defend their hierarchies. This would be a case of modified fear causing an animal to seek danger rather than to avoid it. Would it still be fear? Assuming that fear is defined in terms of homologies, the answer might well be "yes." It really is the business of empirical science to furnish the basis of a biological definition of fear, and the result may well not be a perfect fit with the intuitions of philosophers.

MOHAN MATTHEN

University of Alberta