must then be qualitatively different from one another. The distinctness of context C_1 from context C_2 must rest on the fact that property p_1 is somehow involved in C_1, property p_2 is involved in place of p_1 in C_2, and p_1 and p_2 exclude one another—no single context can feature p_1 and p_2 in just the same role. But the only way of spelling out "in place of" or "in the same role as" is to identify property bearers common to C_1 and C_2, property bearers that can have p_1 and can have p_2 but cannot, while remaining themselves, have both p_1 and p_2. This certainly seems to make the distinctness from one another, of distinct conversational contexts, logically posterior to the difference, in the case of these property bearers, between their accidental and their essential properties. But if so, essential status cannot be *merely* the projection of what it is correct to say in the various conversational contexts.

1.5 Escape from Paradox

Conventionalism, I contend, ultimately founders on its refusal to allow that any objects in the world possess mind-independent existences. On pain of paradox we must allow that at least human minds themselves have mind-independent existences. Almost certainly we must also allow that human brains and bodies have mind-independent existences, and that the various material objects with which we interact have such existences as well.

But to make out these claims we must hold that the essential*ness* of the properties essential to nature's kinds is independent of us—not a status for which we are responsible. And this returns us to the epistemological question: how do we manage to detect the essentialness of nature's essential properties?

2 The Epistemology of Real Natures

Conventionalism, I have argued, fails to give a believable explanation of how we come by our knowledge of properties essential to nature's kinds and stuffs and phenomena. And we do seem to have such knowledge. We know that gold necessarily has atomic number 79, that snow flakes by nature have symmetrical shapes, and that lightning is essentially an electrical phenomenon. To give examples just slightly more controversial, we know that hearts by nature have the function of pumping blood (see chapter 7) and that people by nature are organisms (see chapter 8).

Is the essential status that we know some properties to have, for one or another of nature's kinds, a status that they possess independently of us? In the previous chapter I argued that objects that have mind-independent existences—careers that begin and end at particular points, independently of how we think about those objects—must have essential properties whose status as essential is mind-independent. And everyone, I argued, must concede that at least some objects or entities have mind-independent existences. Proponents of even the most antirealist ontologies must assign mind-independent existences at least to minds and to elements of their physical or cultural surroundings.

A great many philosophers—perhaps most—are of course inclined to credit vastly more objects with mind-independent existences.

But then the question of how we can *know* certain properties to be essential to the objects belonging to this or that natural kind—or to the samples of a given natural stuff, or the instances of a given natural phenomenon—appears truly imposing. If objects are out there, tracing out mind-independent existences, surely one wants to allow that at least sometimes we can know *which* properties are essential to them—which properties it is, the disappearance of which marks the ends of their existences. But if we can sometimes know that certain properties have essential status, and if essential status is out there in the world rather than bestowed by us, how do we learn of it from the world? It is easy enough to see how we establish by induction that all samples of gold *are* composed of atoms having 79 protons in their nucleus. But how can we discover that samples of *that stuff*, of gold, *must* be so composed, *by nature* are so composed?

In this chapter I argue that there is an empirical test for essentialness that we do, and should, commonly rely on. That we do have such a test in our repertoire may seem an astonishing claim, given that most philosophers have for 220 years agreed with Kant that "experience tells us, indeed, what is, but not that it must necessarily be so" (Kant 1929, p. 42). But the explanation is simple. The test is one we run in several steps. No individual step is adequate to warrant a conclusion of essentialness. What has been overlooked is that a number of such steps together constitute a single, if protracted, test of essentialness.

Why has this been overlooked? I hazard this surmise: over the past 220 years philosophers have largely overlooked the

importance, stressed by Hegel and by Aristotle before him, of contrariety.[1] Any property's identity consists in—or at least crucially involves—its contrasting, to varying degrees, with its own proper contraries. That at least is what I shall argue in this chapter. If this starting premise is true, it follows that testing for essentialness is a multistepped affair. First, we must establish that *K*s are in fact uniformly characterized by properties in a certain cluster—say, by properties *f*, *g*, and *h*. Subsequently, we must discover that items generically akin to *K*s, and differing from *K*s by bearing some property (say, *f'*) contrary to a property that *K*s uniformly have, likewise uniformly bear properties contrary to others of the properties *K*s uniformly have (the generically similar kind will have, say, *g'* and *h'*). I call this "the test of flanking uniformities." It is the test which—without quite realizing it—we do rely on for judging that *K*s have *f* essentially. Because the starting premise is, as I shall argue, true—because any property's identity involves its contrasting with its own proper contraries—it is the test we should rely on.

2.1 Why Suppose That Essential Properties Occur in Clusters?

But in order to establish this position I must first address a simpler question: why suppose that essential properties need occur in clusters at all? Why might there not be natural kinds whose members are essentially characterized by just a single essential property? Philosophers who hold that essential status is mind-dependent can answer: "well, the only natural kinds that it is useful for us to recognize—the only ones about which we can come to make informative inferences—are ones characterized by multifaceted essential natures; indeed Mill had a point in thinking of natural kinds

as characterized by *indefinitely* rich essential natures."[2] But if essentialness is fixed not by our interests and classificatory practices, but by the way the world is, this answer fails to show that there might not *be* natural kinds, uninteresting to us, whose members were essentially characterized by just one property.

To answer this question I shall help myself to the assumption that all essential properties do have contrasting contraries; defense of this assumption will come in 2.3 and 2.4, where I will argue that any property must have contrasting contraries, since its very identity crucially involves its contrasting with them. Thus *having atomic number 79* contrasts with *having atomic number 80*, and more sharply contrasts with *having atomic number 19*; *having just that lattice structure*, as said of quartz crystals, contrasts with *having the arrangement of molecules in diamonds or in glass*.

Suppose then that the members of natural kind *K*—*Ks*—are essentially characterized *at least* by property *f*, which contrasts with contrary properties *f'* and *f''*. Can it be argued that *Ks* must essentially be characterized by other properties as well? The first step is to ask what is added, to the idea that *Ks* in fact have *f* uniformly, by the claim that *Ks* have *f* essentially. That *Ks* in fact uniformly have *f* entails that no *K* in fact bears *f'* or *f''*. That *Ks* have *f* essentially, necessarily, entails that *Ks* are *incapable* of having *f'* or *f''*. So we can know of any further object we discover that does have *f'* or *f''*—however great the similarity obtaining between that object and *Ks* themselves—that that object is different in kind from *Ks* themselves.

But now just what is this that we know of such an object? Just what do we infer, from the premise that this object differs by virtue of *f'* or *f''* from *Ks*, when we draw the conclusion that this object belongs to a different natural kind

from *Ks*? Not *just* that this object has *f'* rather than *f*—that is the premise of our inference, not its conclusion. Rather we infer some further or other separateness of this object from all *Ks*, some further exclusion of this object from the natural kind *K*. But kinds are individuated by their characterizing properties. So we infer some further or other *qualitative* difference between this object and *Ks*. We infer that this object differs from *Ks* not just in lacking *f* but in lacking some further property—or properties, plural—which *Ks* all have. It is (in part) in the lacking of these further properties that the differing-in-natural-kind consists. So it is in the *possessing* of these further properties that *Ks' belonging* to their own natural kind in part consists. These further properties are further *essential* properties.

2.2 What Holds Together the Properties in an Essential Nature?

So whether (as I deny) essentialness is mind bestowed, or instead is mind independent, the same holds true: essential properties by nature occur in clusters or packages. Where the properties in such a package come jointly to be instanced, there does an existence begin; where joint instantiation of the properties ceases, there does an existence end.

But the next important question is how, if at all, the properties in such a cluster are held together. Do all the properties in such a cluster crop up, in member after member of a given natural kind, because of the way the world works? Or do the world's workings leave it possible for one or several properties in such a package to disappear, even where all the rest remain jointly instanced? In the latter case the properties in such a package will "hold together" only in the

sense that *we* are unwilling to *allow* that a member of the natural kind in question can have neither quite ceased to exist nor cleanly continued to exist either. The properties will hold together, across members of the natural kind, only in the sense—and to the extent—that we refuse to classify something as *belonging* to that natural kind unless it presents the *full* complement of properties in the package.

But this latter answer seems to put us in the position of constructing the existences of the world's objects, just as surely as if we were responsible for the essentialness of their essential properties taken one by one. I shall take the arguments of the previous chapter as showing that such a position is not in general tenable.

The answer we must rather give, then, is that the properties composing an essential nature are held together by virtue of the laws of nature (more on this in 2.6). By virtue of these laws, some such properties individually, or several in combination, will ground the presence of other such properties. Turning the same point around, individual properties in an essential nature will, by virtue of the laws of nature, be necessary conditions for other properties in that nature—either for some one other property individually, or for one-or-another of several other properties.

But need there be—as the recent fixation on gold and water as sample natural kinds has suggested—some *single* property in each essential nature that somehow is responsible for the presence of *all* the rest? There is no warrant for thinking so, at least none provided by the traditional concept of a natural kind. Traditionally, a natural kind is a family of items over which attentive inductions will nonaccidentally turn out to be true—a family united by a common essential nature, not found among items outside the family. (That is why the basis of the induction must be an *attentive*

inspection of members *of the kind in question*.) So members of each natural kind must be characterized essentially by properties that, at least *in combination*, are found among members of no other kind. But need there be, for each natural kind, some one property that individually is found in members of no other kind? That *would* follow if each essential nature had to incorporate some one property which underlies, is responsible for, all the rest. But that requirement is unmotivated. All that is required by the traditional conception is that each essential nature incorporate enough properties to ensure a combination found in no other kind. The properties which do the underlying may be plural in number. They may be, individually, fairly indistinctive and run-of-the-mill. All that is required is that *in combination* they ensure, by virtue of the laws of nature, a package found in no other natural kind.

2.3 Contrast with Contraries as Crucial to Any Property's Identity

The idea that the properties in any essential nature are held together by the world—and hence incorporate some properties whose presence is a necessary condition for the presence of other properties in that nature—is the relatively uncontroversial premise in the argument that essentialness is empirically detectable via "the test of flanking uniformities." The more controversial premise is that any property's very identity is tied to its place in a range of contraries. The first premise entails that any essential nature is the subject of a counterfactual truth: that if such-and-such properties in a given essential nature were absent in a roughly similar essential nature, certain other properties in the given nature would be absent as well. The second premise casts light on

(e.g., iron) can have more than one valence (e.g., can bond in either the valence +2 or the valence +3 way). Still, even the heavier elements have only one maximally stable way of bonding, one maximally stable valence. So *maximally stable valences* are true contraries. If some atom has a (maximally stable) valence of +3, does it comprise a proper part that has a (maximally stable) valence of +2, and also a proper part that has a valence of +1? Not as a rule. A proper part that had in its own right a valence of +2 or +1 would have to take that valence with it when existing on its own. Yet some ways of breaking pieces off an atom can yield atoms having *higher* valence. Abstractly, indeed, this can happen in innumerable ways: just move up one row in the periodic table from the original atom, then move right for a proper part having higher positive valence, left for a proper part having higher negative valence. But separation of a higher-valence proper part does also happen in nature.[3] And while it is true that valence has come to be understood in a nondispositional way, with the development of atomic theory, the current understanding is of no help to Armstrong. Valence is now seen as the number of gaps in the outermost electron shell. But the problem here, for the "partial identity" view, is that electron shells do not in any clear sense have parts. If an outermost electron shell has three gaps, it does not follow that some part of that shell has two gaps.

Finally, consider a combination of genes that population genetics determines to have degree of stability n. That combination has a degree of stability that contrasts with, and is incompatible with, some lesser degree of stability m. Armstrong's position says these relations of contrast and incompatibility obtain in virtue of the fact that anything with degree of stability n comprises a proper part that itself has degree of stability m, together with yet another proper

part. But commonly this is just not true. Commonly a combination of genes comprises genes that individually have a higher degree of stability than the combination itself.

An atomist about properties, persuaded that Armstrong has pointed the way to an escape from the embarrassing questions about incompatibility among contraries, might insist that there really is no such property as horsepower, or even no such property as valence. But it would be implausible to claim that there are no properties at all that objects have only as wholes—without the parts having reduced versions of them. Indeed there seem to be many such properties. So there seem to be many properties for which atomism is untenable—properties for which contrariety is a self-standing phenomenon, not reducible to the intrinsic being of the properties involved. But if atomism is not defensible for *all* properties, just on account of their *being* properties, then there is no reason not to agree with my second main premise. The very being, the identity, of any property consists at least in part in its contrasting as it does with its own proper contraries.

2.5 The Test of Flanking Uniformities

Let us now return to the result established in 2.2: any essential nature includes some properties such that, were they to be absent in a roughly similar essential nature, certain other properties in the original nature would have to be absent as well. And now let f be such a property in the essential nature of *K*s. Just what is involved in f's being absent from the essential nature of another kind roughly similar to *K*s? Begin with what is involved in f's being *present* in the essential nature of *K*s, on the view advocated in the previous two sections. This is for *K*s to contrast with any and all items bearing

a range of other properties, say f', f'', and f'''—with some of them sharply, with others just mildly, but with items bearing each of these other properties to a unique and determinate degree. So for f to be *absent* from the essential nature of a roughly similar natural kind will be for there to be a *failure* of contrast with one of these groups—with items bearing f', or else items bearing f'', and so on. Or, to put it differently, it will be for items bearing this roughly similar essential nature—members of this roughly similar natural kind—to contrast with *K*s themselves. But such contrasting by nature occurs to one determinate degree or another. So the absence of f, from an essential nature roughly similar to *K*s', is never an undifferentiated, yes-or-no matter. It is a departure to one degree or another, by items bearing that roughly similar essential nature, from the f-ness present in *K*s.

But the idea put forth in 2.2 is that f's absence, in an essential nature roughly similar to that of *K*s, must go together with the absence there of some other property (say, g) likewise present in the nature of *K*s, because f is tacked onto g by the way the world works. The world itself is such that the presence of g ensures the presence of f. So if the absence of f in any roughly similar essential nature is really a matter of degree—a matter of greater or lesser departure from f-ness—the corresponding absence of g in such a nature, engineered by the way the world works, must be a matter of *corresponding* degree. It must be a matter of the bearers of this roughly similar essential nature departing, to a corresponding and commensurate degree, from the g-ness of *K*s.

In other words: members of any natural kind generically similar to *K*s, essentially characterized by some property f^* contrary to f, instead of by f itself, will likewise be characterized, uniformly, by some one contrary of g—a contrary g^* that contrasts as sharply with g as f^* does with f. This follows from the premise that f is a property whose presence in the essential nature of *K*s is ensured—required—by other properties (in this example, g alone) in the essential nature of *K*s. But *any* essential nature, 2.2 argued, will incorporate some properties ensured or required by others (individually, or in combination) in the nature.

It follows that there is an empirical test for essentialness. To gain evidence that f characterizes *K*s not just uniformly but essentially, see whether, among the members of (what seem to be) natural kinds roughly similar to *K*s, differing from *K*s by possessing some one property or another contrary to f, there are uniformly found *other* properties contrasting with *other* properties uniformly possessed by *K*s. In the simple case we have been considering—where the presence of f in the nature of *K*s is ensured by the presence there of g alone—one would expect to find, among all members of similar kinds characterized by one contrary or another of f, uniform presence of a contrary of g *commensurately* contrasting with g itself. But more complex cases are common. Commonly, that is, the presence of a given property f in the essential nature of a given kind *K* will be produced by the presence in that nature of a *combination* of other properties. So departures from f, in the natures of natural kinds roughly similar to *K*s, may not be accompanied by other properties that *individually* contrast to an exactly *commensurate* degree with other individual properties of *K*s.

But this much remains true. If f is an essential property of *K*s, then other kinds similar to *K*s, characterized uniformly by one contrary of f or another, will each differ in just one uniform way from *other* properties found uniformly among *K*s: the similar kinds will each select, throughout their membership, just some one contrary of another property, or of

each of several other properties, found uniformly among *K*s. That is "the test of flanking uniformities."

Thus it is warranted to judge that atomic number 79 is an essential property of gold because other physical elements, each characterized by one atomic number contrary to atomic number 79, also differ, always in the same way, from *other* properties found always in gold. Other metals select in all samples a particular melting point contrary to gold's melting point, a particular specific gravity contrary to gold's specific gravity, and so on. It is warranted to judge that quartz crystals *essentially* have a certain lattice arrangement among their molecules because other mineral formations, each characterized by a different molecular arrangement, likewise are characterized in all their instances by just some one contrary of other properties of quartz—by just some one scratch index, just some one density, just some one color, and so forth.

The test of flanking uniformities thus yields conclusions about the properties essential to nature's kinds and stuffs and phenomena strictly from what we learn from experience. It does not require that we know independently of experience—via a priori insight, or via armchair expression of our conventions of individuation—"template" truths about the kinds of kinds (physical elements, mineral formations, chemical compounds, etc.) into which nature's specific kinds fall. Rather it shows how we may *learn* such truths from experience. It shows how we may first establish that *this* chemical compound we call "water" has a certain molecular structure essentially, that *that* generically similar stuff we call "alcohol" has a contrary molecular structure essentially, that such-and-such an "acid" has yet another molecular structure essentially, and can then perform a metainduction over chemical compounds, thus establishing

that each has essentially whatever the molecular structure that observation determines it to have. Our observation of nature does indeed have to be supplemented by good luck, if not by nonempirical knowledge, in order for the test of flanking uniformities to yield conclusions. We must be lucky enough to find kinds generically similar to *K*s, and smart enough to recognize them as being generically similar, in order for the test to teach us anything about *K*s' essential properties. But most of the ways of learning about the world we wield are like that. They will not invariably yield the kind of knowledge that makes us favor them; they even may, in the short run, yield misleading conclusions which subsequent applications of them can correct. But only allegiance to verificationism can make us uneasy at the prospect that the ways the world is—including the ways the world must be—may outrun our abilities to learn of them.

2.6 But What if Laws Governing *K*s Fail to Hold in Worlds Containing *K*s?

But there is an objection to what I have said in 2.2—an objection that will by now have been bothering some readers for a long time. It runs this way: "The members of any natural kind *K* must retain all their truly essential properties in all possible worlds. But then the essential properties of *K*s cannot include some (e.g., *f*) that are cemented to others (e.g., *g*) by merely the laws of nature. For the actual laws of nature fail to obtain in some possible worlds. In particular, the laws actually bearing on *K*s fail to obtain in some possible worlds in which *K*s themselves are present. So there are possible worlds in which there really are *K*s, but the *K*s lack *f*. The test of flanking uniformities may *say* that *f* is an essential property of *K*s, but so much the worse for that test."

seems not at all an ad hoc trick when used to block "the sorites of decomposition."

Now for the causal exclusion arguments against mental causation.

4.1 Why Mental Causation Seems to Be Excluded

James decides that the best price today on pork chops is at Supermarket S, then James makes driving motions for twenty minutes, then James's car enters the parking lot at Supermarket S. Common sense supposes that the stages in this sequence may be causally connected, and that the pattern is commonplace: James's belief (together with his desire for pork chops) causes bodily behavior, and thereby causes a change in James's location. But many philosophers worry that such apparent mental causation is illusory (see, e.g., Heil and Mele 1993; or Macdonald and Macdonald 1995). Their worry stems from the close relation that evidently exists between James's arriving at the supermarket and an extremely complex event involving an enormous array of physical microparticles. This relation is in part a matter of co-location in space and time: exactly where and when James arrives by car at the supermarket, there and then do billions and billions of microparticles undergo billions and billions of motions and state changes. But the relation appears to be more than *just* co-location. It seems close enough, in particular, that whatever causes the extremely complex microphysical event, just there and then, thereby causes James's arriving at the supermarket itself.

Just what sort of event might be a cause of this enormously complex microphysical event? Another equally complex microphysical event, many philosophers reason, involving an equally enormous array of microparticles.

Quite possibly what went on in James's brain, when James decided about the best price on pork chops, is a large part of such an event: perhaps motions and state changes in those microparticles, given background circumstances including energy relations binding together the microparticles in James's car, sufficed to start a causal chain that eventuated in the complex microphysical outcome in the parking lot.[2]

But does this suggest that James's *deciding* as he did about the best price on pork chops did *not* cause his arrival at the supermarket? Some philosophers indeed discern no such suggestion. They maintain that the close relation between the complex microphysical outcome in the parking lot, and James's arrival at the supermarket, is simply *identity* (Davidson 1967, 1969). Similarly, these philosophers suppose, James's deciding and desiring as he did just *was* the complex array of motions and state changes involving microparticles in James's brain. So the thought that the microphysical event in James's brain caused the microphysical outcome in the parking lot hardly *imperils* the claim that James's decision caused his arrival; it *affirms* that claim.

But other philosophers do discern danger here. They worry that even if talk about James's deciding and desiring as he did picks out a complex microphysical event which, thanks to the laws of microphysics, was sufficient to ensure the complex outcome in the parking lot, such talk highlights features of that event that may have been causally inert. It may not have been *in virtue of* this event's instantiating mental properties, or propositional attitudes, that it caused what it did. James's mental life may enjoy only causal-efficacy-by-association—which would no more be *real* causal efficacy than guilt-by-association is real guilt (McLaughlin 1993). The impetus for this worry comes from the conviction that whenever an individual event *a* truly causes individual

event *b*, the succession of *b* upon *a* must instance, or be underwritten by, genuine laws of nature (cf. Davidson 1970). Now it seems undeniable that there are genuine laws of microphysics. Perhaps no one law of microphysics ties the general *sort* of complex microphysical event that occurred, when the microparticles in James's brain all moved just as they did, to the general *sort* of complex microphysical event which was instanced in the parking lot. (This is actually a controversial question, and I will return to it in 4.4.) But even so there will be particular laws of microphysics which tie particular elements of that initial complex event to subsequent events, those in turn to others, and thereby ultimately tie elements of the initial complex event to elements of the complex microphysical outcome in the parking lot. And these laws of microphysics are precise and exceptionless, or as close to preciseness and exceptionlessness as any that nature will yield. In contrast, the only "laws" that tie decidings and desirings, such as those James did, to actions like James's betaking himself to Supermarket S, will be imprecise and hedged by numerous *ceteris paribus* clauses. So, if a claim to having caused an outcome depends on the lawlikeness of the generalizations that are instanced, the complex microphysical event that occurred when James decided as he did will, in virtue of being the *microphysical* event that it was, have a very strong claim to having caused the microphysical outcome in the parking lot—and with it, James's arriving there itself. In comparison, this same complex event in James will, in virtue of being the particular *deciding* (*and desiring*) that it was, have only a very poor claim to having caused James's arrival at the parking lot.

Beyond that, many philosophers think it strained and unmotivated to claim that the close relation between James's deciding and the complex microphysical event in his brain,

or between James's arriving and the complex microphysical outcome in the parking lot, is really simply *identity*. The reason for *hoping* that the relation is just identity is the thought that thereby one can save the causal efficacy of James's deciding—and that thought seems questionable in any case. So, many philosophers suppose, one might as well adopt the more intuitive idea that the relation is one of supervenience. James's arriving at the supermarket *supervenes*, at least weakly, on the complex microphysical outcome; that outcome *composes into* an arrival by James. Likewise James's deciding *supervenes on*, rather than *is*, the complex event occurring in James's brain. There are events on different levels. But once James's deciding is explicitly placed on this supervenient level, its causal inefficacy seems even harder to deny. The complex event occurring (largely) in James's brain causes the outcome in the parking lot, and therewith brings it about that James arrives. The supervening decision by James lodges a weaker claim to having brought it about that James arrives. Can we credit this weaker claim—can we believe in routine causal overdetermination of human actions? The more defensible response, many suppose or worry, is to rule that the weaker claim is *too* weak. James's deciding causes nothing.

I argue in this chapter that this worry gets the real situation exactly upside-down. In fact the complex microphysical outcomes, which mental events seem excluded from causing, are not caused at all. For they are either accidents, in something like Aristotle's sense (Sorabji 1980, pp. 3–25), or coincidences, in a sense that David Owens has recently sharpened (Owens 1992). Each individual microphysical event comprised within such a complex outcome does have a physical cause; but it does not follow, and is not true, that the complex "outcome" event as a whole does. Mental

causation, then, does *not* face competition "from below," from the microphysical level. Moreover, it may on its own level be perfectly genuine. For the outcomes that mental events appear to bring about—the motions of limb and larynx, and the changes in the agent's surroundings effected by these in turn—may have unified causal histories that the microphysical events subvening those outcomes do not.

4.2 A Suggested Analysis of Causation

Philosophers who insist or worry that mental causation is excluded by causation at the level of microphysics must suppose that the relata of "_____ is a cause of . . ." are fairly fine grained. They suppose after all that whatever qualifies as the microphysical cause of the complex microphysical outcome in the parking lot thereby also qualifies as a cause of James's arriving at the supermarket by car: the effects lie so close to one another that causing the former amounts to causing the latter. Yet as close to each other as they lie, there is a line of distinction so fine that it keeps them from being the same outcome. For it is the existence of an equally fine line of distinctness, at the opposite end of this causal transaction, that keeps the causal efficacy of the complex microphysical event involving James's brain from translating into causal efficacy on the part of James's decision.

Thus causal exclusionists must think of the relata of "_____ is a cause of . . ." as being states of affairs, or else Kim-style events—events with the structure, in the basic case, of object *o*'s possessing during time *t* property *p* (Kim 1969, 1980). I myself agree that the relata of "_____ is a cause of . . ." are thus fine grained, and to this extent I think the exclusionists entertain a picture that is entirely right-side-up.[3] Where my disagreement comes is over two rather

minor-sounding issues: whether "_____ is a cause of . . ." is agglomerative, and whether it is transitive.

But while the positions I take on those issues are controversial, the basic analysis of "_____ is a cause of . . ." from which I draw them is not, at least not markedly so. It is merely a modified version of Bennett's analysis in terms of NS conditions (Bennett 1988, ch. 3), and has elements in common with every main analysis currently on offer. There are to be sure differences between the analysis I favor and others currently defended, differences that can seem substantial the more closely one focuses on the array of puzzle cases, some of them quite fanciful, which has now become a staple of the literature. I will not undertake a detailed demonstration that the NS analysis deals better with those puzzle cases that really need to be addressed. If such a demonstration seems to be needed, please read this chapter as advancing a provisional claim: *if* a cause is a certain species of NS condition, *then* mental causation faces no competition from below. One could even think of this chapter as gesturing toward an "inference to the best explanation": this NS analysis yields a vindication of mental causation against causal exclusion arguments; we intuitively suppose that mental causation *is* genuine; so we should award a presumption of correctness to the NS analysis.

The basic idea then is that a cause of outcome *e* is a state of affairs *c* that figures indispensably in a set of circumstances jointly sufficient to ensure that *e* obtains. Typically a cause will not by itself compose all of such a set—that is, typically what qualifies as "a cause of *e*" will not strictly qualify as "the cause of *e*"—but will call on distinct circumstances, for example background conditions, for sufficiency to produce *e*. Also typically, the set in which *c* indispensably figures will be just one among several by which *e* could have

been produced. So Mackie (1965) had good reason to describe the typical cause as an *INUS* condition of its effect, as an *insufficient but necessary* part of a set of circumstances *unnecessary but sufficient* to produce the effect. At the same time it is unduly restrictive to *define* a cause as an INUS condition. A state of affairs that by itself ensures the occurrence of *e*, and is all that could ensure the occurrence of *e*, should qualify as a cause of *e*. A cause is fundamentally an NS condition (a necessary part of a sufficient condition) of its effect.

Now for details. First, it is crucial that the set of circumstances in which *c* is a key ingredient has the right *sort* of sufficiency for *e*. If *c* figures crucially in a set of circumstances *logically* sufficient for *e*'s obtaining—or in a set that is, so to speak, *constitutively* sufficient for *e*'s obtaining—it will be counterintuitive to bill *c* as a cause of *e*.

Here is an illustration of the first sort of counterintuitive result. I go to the agora with the intention of seeing a play, and my debtor goes to the agora for an unrelated reason, and by accident we arrive at the agora at the same time—a lucky accident, since it results in my recovering my debt. Aristotle, from whom the example is taken, holds that the accident of our arriving at the agora simultaneously has no cause, and soon I will argue that he is right. But for now my point is that it would be counterintuitive to hold that our arriving simultaneously does indeed have a cause, and that it is caused by my arriving at the agora (as in fact I did) at precisely 4:03. For causes must be distinct from their effects. Yet my arriving at 4:03 *is* our arriving simultaneously—or rather is, together with my debtor's arriving (as in fact he did) at 4:03, part of a set of circumstances that logically amounts to our arriving simultaneously.

For the other sort of counterintuitive result, consider the complex surging and swarming of microparticles that com-

poses into James's arrival by car at Supermarket S. Suppose that a crucial element in this sprawling microphysical event is the surging in a certain direction of so-and-so many carbon atoms configured in biochemical compounds, surrounded by so-and-so many iron atoms arranged in lattices that realize steel, all occurring at a certain distance above so-and-so many atoms of silicon bonded with other atoms in molecules that add up to pavement. The arrival of James's component carbon atoms in just that region should not count as a *cause* of James's arriving at the parking lot, again for the reason that causes must be distinct from their effects. *That* those atoms arrive in that region is, to be sure, a different fine-grained event (or a different state of affairs) from *that* James arrives in the parking lot. But their connection is still too close for the former to qualify as cause of the latter. The reasons for denying a causal connection here indeed go fairly deep. The whole worry about mental causation stems from the thought that the neural—ultimately, microphysical—states of affairs that compose into or subvene James's decision to go to S may really do all the causal work that James's decision appears to. But the causal work that the microphysical goings-on are thought to do does not include composing into or subvening James's decision itself. For their causal work is thought to be underwritten by the laws of physics, and the laws of physics do not quantify over decisions (Wittmer 1998). Subvening, composing into, is thought of as a *noncausal* relation between microevents and macroevents. Similarly noncausal then is the relation between the surging of James's carbon atoms and James's arriving in the parking lot.

A cause of *e*, then, is an indispensable component in a set of circumstances that jointly are *causally* sufficient—at any rate, not *logically* or *constitutively* sufficient—for the

occurrence of *e*. It would be better to say this in a way which does not use "causally" in the definiens, and perhaps this is the way to do so: a cause of *e* is an actually preceding event *c* that, *in virtue of the laws of nature*, is an NS condition for *e*. (I am assuming that there are no *laws of nature* that tie macroevents of a particular familiar type—e.g., arrivals-by-car-at-supermarkets—to the fathomlessly complex disjunction of microparticle events, each of which would compose into such a familiar macroevent [cf. Fodor 1997]. This assumption should be congenial to causal exclusionists. For if there were such laws of nature, someone could say we should just *identify* the familiar [type of] macroevent with the disjunction of complex microphysical outcomes. Then decidings—such as James's deciding about the best price on pork chops—could likewise be identified with disjunctions of complex microphysical events,[4] and the apparent causal efficacy of James's deciding would no longer be *threatened* or *rivaled* by the efficacy of the microphysical event which composes into it.)

Two other details must now be considered: first, that something Bennett calls "the continuity condition" must be added to the basic account of a cause as an NS condition; second, that "the continuity condition" cannot, despite what Bennett says, supplant that basic account.

I toss a lighted match toward the top of an open gasoline drum, Bennett's example runs, and thereby top off a set of conditions jointly sufficient for the house's being in ruins an hour later. But my toss does not cause the house to be in ruins, for a bomb lands on the house at the instant I launch the toss. Its causation preempts that of the match (Bennett 1988, pp. 45–46). What the possibility of such preemption shows, as Bennett rightly notes, is that at every moment *between* a putative cause and its would-be effect, a circumstance must obtain for which the immediately preceding circumstance was an NS condition, and which in turn was itself an NS condition of the immediately following circumstance. When the bomb intervenes, it assumes the role of NS condition for the next momentary circumstance in the series leading up to the house's being in ruins, but the bomb's presence is not itself anything for which any stage of my match toss was an NS condition.

But Bennett also thinks that we can now drop the requirement that *c* be itself an NS condition of *e*, and let the continuity condition do all the work of analyzing *c*'s causing *e*: it will be enough, Bennett says, that each intervening stage between *c* and *e* be linked NS-wise to its immediate predecessor and successor (ibid., pp. 46–49). This relaxation of the analysis, I maintain, yields counterintuitive results. For the continuity condition alone can be satisfied by the following sort of chain. P is an NS condition for Q and Q is an NS condition for R. But P calls on certain background conditions for causal sufficiency to produce Q, and Q in turn calls on *different* background conditions for causal sufficiency to produce R; moreover, the conditions Q calls upon get assembled later than the first set, and neither P itself nor the first set plays any role in bringing about the later set. Then so far as P's occurrence goes, it is a pure coincidence that somewhat later, R comes along. P cannot plausibly be said to cause R.

Consider, as illustration, this lovely example from David Owens (1992, pp. 18–19). I contract a disease that will kill me in six months unless treated with drug A. But so noxious are A's side effects that A itself will, unless counteracted, kill me within a year. Desperate, I take A. After nine months someone discovers drug B, which suppresses A's side effects—but only for two years, at which point the patient

at length succumbs. I then take drug B, and two years after the original diagnosis, I am still alive. What causes me, two years after the original diagnosis, to be free from the clutches of death? Not that I took drug A, Owens maintains. On the contrary: it is mere coincidence that two years after taking A, I am still alive. I think this verdict is correct, though for slightly different reasons from those Owens gives.[5]

The central question is whether my taking drug A played a necessary part in a set of events *sufficient*, given the circumstances, for my being alive *today*. The answer is No. The set of circumstances in which my taking drug A played a part—the set my ingestion of A then engaged for its sufficiency to keep me alive nine months longer—included no circumstances at all connected with the development of drug B. Circumstances that would ensure development of B may then have obtained. But they were not causally involved in my living on for nine months more. Thus my taking A was not an NS condition of my being alive today—even though it was an NS condition of my being alive nine months later, and my being alive nine months later was an NS condition of my being alive today. So "_____ is an NS condition for . . ." is not transitive. Consequently, neither is "_____ is a cause of . . ."—a point of importance in 4.5.

4.3 How We Detect Causes, and Why an Accident Does Not Have a Cause

I have said I would argue that the complex microphysical mêlée that realizes James's arrival at Supermarket S must be viewed as either an "accident" in Aristotle's sense or as a "coincidence" in David Owens's sense, and that in either case it has no cause. Let me warm up for the needed argument by discussing simpler cases of what I mean by an

"accident" and a "coincidence," together with the reasons for thinking that these simpler cases lack causes.

The joint arrival at the agora of my debtor and me can be viewed as an amalgam of two specific states of affairs: my arriving at the agora at exactly 4:03, and my debtor's arriving there at exactly 4:03. Alternatively, it can be viewed as a unitary, relationally defined state of affairs: my debtor and I arrive at the same time as one another. The amalgam of two states of affairs is an example of what I call a "coincidence." The unitary relational state of affairs is an example of an "accident."

My focus in this section will be on accidents. So let us ask what, if anything, brings it about that my debtor and I arrive at the same time as one another. My arriving at the agora when I did was the product of a background of intentions and decisions peculiar to me, and my debtor's arriving there was the product of a set of psychological background conditions peculiar to him. Was there some previous event that ensured that these two sets of background circumstances would yield up their products simultaneously? Did one and the same event figure indispensably in both causal chains, and figure in such a way as to make each chain yield its product at the same time as one another? If so, what would such an event look like?

To answer this question as carefully as we can, it is worthwhile to digress briefly to ask how *in general* we identify causes. How in general do we tell, of a particular event *c* that in fact preceded event *e*, that *c* was, operating together with background circumstances then obtaining, causally sufficient for *e*? The evidence that experimentalists in fact generally take as indicative of causal sufficiency, James Woodward has shown, is something called "invariance" (Woodward 1992). To understand what invariance is we

must view events c and e as having structure. To pick a simple example, c might be a matter of object A's acquiring property g, and e a matter of object B's acquiring property f. (In the case where an object's acquiring one property causes *it* then to acquire another, object B will be the *same* object as object A.) "Invariance" is then the finding that as A-like objects acquire properties that contrast more and more sharply with g, relevantly placed B-like objects will display properties that contrast commensurately with f. In other words, values found in a B-like object of the determinable property which subsumes the f track, over a range of cases, values found in the A-like object of the determinable that subsumes g; the B-characterizing determinable reflects, seems tied to, the A-characterizing determinable; the B determinable fails to vary independently of the A determinable. Thus a low level of calcium in the diet is implicated as a cause of osteoporosis by the finding that, ceteris paribus, the more severe the porosity of the bones, the lower was the intake of calcium.

Of course it is fair to ask whether and why we *should* treat such "invariance" as evidence that c is, given the background circumstances, causally sufficient for e. The position on property identity staked out in chapter 2—namely, that any property's being itself is tied to its occupying the place it does in a range of contraries—appears to provide answers to these questions. For suppose that, in virtue of the causal laws that hold in the world, an A's acquiring g *is* causally sufficient for a relevantly placed B's acquiring f. Then that B's acquiring f will be a causally necessary condition for the A's having acquired g; if property f had been absent from that B, property g would have had to be absent from that A. But *what is it* for property f to be absent from that B? For f to be *present* in that B, on the position of chapter 2, is for that

B to contrast to varying degrees with other objects each characterized by one or another of f's contraries—by f' or f'' or f'''. So for f to be absent in that B is for one of these contrasts to fail to obtain. It is for B itself to contrast, to one degree or another, with items that do have f. In other words: the absence in that B of f is never an undifferentiated, all-or-nothing matter. It is rather a matter of the B's departing *to one determinate degree or another* from f-ness. And so too the absence of g in the A—which, we are supposing, would have *had to* accompany the absence of f in that B—will similarly be a matter of that A's departing to some determinate degree from g-ness, by coming to have g' or g'' or g'''.

But we are supposing it to be a function of the laws of nature that the B's departure from f-ness—had this occurred —would have had to be accompanied by a departure by that A from g-ness. We are supposing that a departure by the A from g would not merely have *coincided* with the B's departure from f, but would have *corresponded* to the B's departure from f. So we thereby are supposing that a *commensurate* departure by the A from g would have gone together with the counterfactual departure by the B from f. Had the B not acquired property f, and had it acquired instead the only slightly different property f', that could only have happened if the A had acquired, in place of g, the only slightly different property g'. To the B's having acquired the moderately different f'', there would have had to correspond the A's having acquired the moderately different property g''.

In short, selection by A-like objects of properties more and more different from g should correspond to—be found together with—selection by relevantly placed B-like objects of properties more and more different from f. This follows, given chapter 2's position on property identity, from the premise that A's acquiring g is causally sufficient (given

background circumstances) for B's acquiring f. We should indeed expect the experimental findings called "invariance," if c, that is, A's acquiring g, truly causes e, that is, B's acquiring f.

But—to return from this digression—how can invariance help us with the case at hand? Here our task is to identify a cause of the *simultaneity* between my arrival in the agora and my debtor's. And simultaneity is a relation, not a property. Here "event e" has the structure "simultaneous {my arrival, my debtor's}."

Even so, it seems easy to identify relations that contrast at first just mildly, then more and more sharply, with the actual simultaneity between my arrival and my debtor's. That is, it seems possible to identify relations that are proper contraries to the actual simultaneity between our arrivals. We may imagine first my arrival's having been just a bit earlier than my debtor's, or vice versa; and then that one of us arrived earlier by an even greater margin, so that one of us nearly missed the other in the agora; and so on.

So if there was a cause of the simultaneity between my arrival and my debtor's, we now know what such an event would look like. It must be some event previous to our arrivals, such that variations on that event, first mild and then sharp, must seem likely to have gone together with arrival relations more and more different from simultaneity. That is: to identify a *cause* of the simultaneity of our arrivals, we must find some event that set up a *relation* between the various background circumstances which were causally responsible for my arriving at the agora, and the quite different background circumstances that were responsible for my debtor's arrival; and this one event must have played an indispensable part both in the former circumstances and in the latter.

Here is an event that *would* meet our requirements—if only it had occurred! Suppose my debtor and I had simultaneously heard the town crier announce that nuggets of gold free for the taking had been dumped in the agora. This event would have set up a *simultaneity between* the starts of two causal chains that led, respectively, to my debtor's arrival and to mine. It would then have brought other relations between me and my debtor into play, causally. Suppose, for example, that my debtor and I live equally far from the agora, and are equally fleet afoot. Then the one event of the town crier's shout would have been an NS condition for our arriving simultaneously at the agora. It would have topped off two sets of background circumstances, involving me and my debtor respectively, such that the two would yield simultaneity in the arrivals. (Or the crier's announcement could have started out the chain that led to my debtor's arrival later than it started my chain—my debtor lives out of earshot of the crier, but the cry was repeated by an excitable child—and then my debtor's being more fleet afoot than I could still have led to simultaneity in our arrivals.)

But ex hypothesi there was no such event; ex hypothesi the events and circumstances that got me to go to the agora were unconnected with, disjoint from, those that got my debtor to go. There *existed* numerous relations between my debtor and me, but nothing brought them causally into play. So our arriving simultaneously, like all accidents, had no cause. No previous circumstance causally sufficed for its occurrence. The causal processes of the world, to speak metaphorically, did not grab our joint arrival by its simultaneity when they pulled it into existence. Rather they grabbed our joint arrival at two different points, independently, and pulled: the simultaneity just came along for the ride.

Now, at last, for the microphysical mêlée that realizes James's arrival at Supermarket S. It too can be viewed as a unitary, relationally defined outcome: microparticles that compose into James's car move on top of microparticles that compose the parking lot at Supermarket S while simultaneously surrounding, between them, microparticles composing into James. This may not indeed be the way of picturing that microphysical mêlée which first occurs to one. It may seem more natural to view that mêlée as an amalgam of innumerable individual states of affairs, that is, as what I call a *coincidence*—that such-and-such a microparticle is undergoing such-and-such a motion at that precise location, while such-and-such others are undergoing precisely that sort of motion in precisely that other location, while yet another microparticle is doing such-and-such there, and so forth. But just as there is an objective question of what caused my debtor and me to arrive *at the same time as* one another, regardless of whether I arrived at exactly 4:03 and whether he did, so there is a parallel question concerning the microphysics of James's arrival at S. There is an objective question concerning, not what caused exactly such microparticles as were present in that parking lot to undergo exactly such motions and state changes as they did, but rather why *some* microparticles or other, clustered together in one of the ways that would compose into a car, were collectively moving above *some* other microparticles configured in one of the ways that would compose into pavement, while collectively encompassing *some* microparticles that composed into James.

But would this relationally defined microphysical development be just another accident—could it be said to have a cause? To find a cause for the simultaneity of my arrival and my debtor's, we looked for a previous relation-making

event, variations in which would have gone together with variations in arrival relations. We had to identify an event relating my past to my debtor's, which when added (as a necessary element) to the background circumstances involving me and my debtor respectively, yielded a set sufficient for my arriving just when he did. This we could not do. But just so here.

For my opponent's aim, after all, is to use causal exclusion arguments to exclude from serious ontology James himself, James's car itself, and the parking lot itself. To prepare the ground for such arguments, he will have to operate *strictly* "from the bottom," from the level of the microparticles, to identify a cause for (what common sense calls) James's arrival at the parking lot. The question I have now raised is: what if anything caused microparticles composing into James's car to move above microparticles composing into the parking lot while simultaneously surrounding microparticles composing into James? The answer will have to identify some earlier relation-making event connecting microparticles in the first group to microparticles in the second and in the third—for example, that the microparticles composing into James's car *were* hurtling *toward* those composing the parking lot, while even then surrounding, collectively, those composing into James. This event will have to be such that variations on it can be expected to go with variations in the relative motions of these microparticles, and thereby with variations on James's arrival at the supermarket.

But are microparticles ever really influenced by such relations? In chapter 3 we noted that *individual* microparticles are almost never influenced even by the fact itself that they are contained in their host medium-sized object. In other words, it is almost never a necessary part, of what sufficed

for an individual microparticle's moving as it did, that *all* the microparticles within its host medium-sized object were respectively doing such-and-such. Far less then are individual microparticles ever influenced by relations obtaining *between* that medium-sized object and *other* whole medium-sized objects. Are things improved for my opponent if she speaks not of *individual* microparticles but of *whole collections* of them—for example, of that whole collection that composes into James's car, or into James himself, or into the parking lot? But there can *be* such collections in the world only if there is something that unites their component microparticles. Now if there are in the world James and his car and the parking lot, what unites the component microparticles can be their *being located within the borders* of these medium-sized objects. But if those objects do not in ontological strictness exist in the world, then as we saw in chapter 3, nothing unites these collections. In particular, nothing determines which microparticles are included across counterfactual scenarios in, say, "the collection of microparticles that composes into James's car." So there is no fact as to how this "collection" would have reacted if differently related to other "collections." In sum: we just cannot turn relations between James and his car and the road into circumstances that will prove causally influential at the level of microparticles, if microparticles are all there is in the world. The microphysical mêlée that realizes James's arrival, if viewed as a relationally defined development, has no cause.

4.4 Why a Coincidence Has No Cause

Then are the prospects for finding a cause improved if we view that mêlée as a coincidence? A coincidence, in the sense

I take from David Owens, is a compound outcome that divides into states of affairs caused independently of one another (Owens 1992, ch. 1). That is, for any one of the component states of affairs, no previous development that rounds out a set of circumstances causally sufficient for *it* also rounds out a set causally sufficient for any *other* component state of affairs. The example given earlier is the case in which I arrive at the agora at 4:03 and my debtor arrives at 4:03. For the developments that got me to go to the agora are ex hypothesi distinct from those that got my debtor to go.

Following Owens I hold that no coincidence has a cause, but the position could be disputed. One might reason as follows. *Something* caused me to arrive at the agora at exactly 4:03, and *something* caused my debtor to arrive there at exactly 4:03. But then there is a compound state of affairs composed of these two NS conditions taken together, and it is as a whole an NS condition for the compound outcome of my arriving at 4:03 and his arriving at 4:03. The compound circumstance, say, of my conceiving at 4:00 a burning desire to see a play *and* my debtor's conceiving at 3:45 a languid desire to buy a bracelet caused the compound outcome that he and I each arrived at 4:03.

But I suggest that this reasoning assumes too casually that "_____ caused it to be the case that . . ." (or "_____ is an NS condition for . . .") is agglomerative—that if individual event P causes individual event Q, and individual event R causes individual event S, (P & R) causes (Q & S). The question "What caused the left front tire on my car to go flat?" undoubtedly has an answer. The question "What caused the Indonesian economy to collapse?" likewise has, let us allow, an answer. But consider: "What caused my left front tire to go flat and the Indonesian economy to collapse?" The

question to which, later in this chapter, I shall give a positive answer.

So the position this chapter attacks is strictly *projectivism with respect to artifacts*. This chapter will locate the weakness of this position not where many contemporary metaphysicians would—in its affirming the reality of us projectors—but rather in a problem concerning the causes of our alleged projection. To the naive question "What gets us to believe that there are artifacts in the world around us?" the naive answer is that our interactions with artifacts themselves do this—we make artifacts, we use them, we observe them. To the less naive question "If strictly there are no artifacts in the world, what then causes us to believe in them?," the natural answer would be that our culture or conventions or customs do this; belief in artifacts is instilled by the sentences we hear at our mother's knee. But a true projectivist must be careful, in formulating an answer to this less naive question, to cite as acting upon us only such objects as are recognized by his artifact-free ontology. Quite possibly these objects do *not* include such things as customs or sentences at all. What objects *are* included? Let us allow that the projectivist recognizes all manner of *non*artifactual familiar objects, and recognizes people as well. Even so there is, I shall argue, a great gulf fixed between any answer to our less naive question that is available to a true projectivist, and the kind of answer that seems natural. For the realm of our culture, our conventions, and our language is bristling with copied kinds. Thus if the projectivist offers an answer rich enough to depict the action on us of items in this realm, he concedes that members of at least some copied kinds really act and really exist. Then he has no principled way of denying that at least some artifacts exist. If on the other hand the projectivist denies that there are in the world any copied kinds,

he denies that there are any objects which might plausibly be said to cause, by their action on us, the projection he believes in.

Strictly speaking, this chapter is an ontological vindication not directly of artifacts, but of copied kinds. Copied kinds *include* many kinds of artifacts, but more besides: kinds of biological devices, kinds of naturally selected behaviors (e.g., mating dances), kinds of customary performances (e.g., rain dances), and kinds of linguistic structure. Kinds of artifacts picked out by the sortals of ordinary language often amount to copied kinds, but not invariably: chairs do not compose a copied kind, and neither do neckties or nose rings (see 7.3). I will be content if I have staked out a place in ontology for at least *some* artifacts.

7.1 The Sorts of Properties That Essentially Characterize Copied Kinds

Artifacts do have a place in ontology if, in fashioning a desk, a carpenter does not merely set pieces of wood or bundles of cellulose into a different arrangement toward one another, but brings a new object into existence. So too do they have a place if, when the desk is crushed by a collapsing roof, what happens is not just that the pieces or the bundles get arranged differently again, but also that something is destroyed. But just what marks the difference between "substantial change," that is, change involving creation or destruction, and "accidental change," change involving mere alteration? Verbally the answer is easy: an object undergoes substantial change if and only if the properties that are lost (or acquired) jointly compose an essential nature. But just what would the essential properties of artifacts be? And how would we tell that they are essential?

My contention is that the artifacts that do have a place in ontology are just those that fall into one or another "copied kind." Let me therefore begin with the broader question of what the essential properties are that characterize any copied kind. First, the members of any copied kind are characterized by a particular qualitative make-up or "shape." This will *literally* be a shape in the case of artifacts or biological devices, for example, the household screwdriver or the double-lensed eye of the eagle; it will be a shape somewhat metaphorically in the case of reproduced behavior, for example, the mating dance of the stickleback fish or a ritual rain dance performed by a particular human culture; it will be a "shape" in a purely metaphorical sense in the case of linguistic forms or constructions, such as the indicative mood in a particular language. Second, the members of any copied kind are characterized by what Ruth Millikan calls a "proper function" (Millikan 1984, chs. 1 and 2, cf. forthcoming a). That is, the members are produced by a process or mechanism which copies them from previous members similarly shaped, and does so as a causal consequence of performances, by those previous members, of certain functions—productions by them of certain effects. The process is, in other words, such as to produce more copies of previous items that produced such effects than of previous items that produced *no* such effects, or more copies of items that produced a particular such effect *more often* than of different items that produced it less often, or more copies of items that produced a *more wide-ranging* such effect than of different items that produced one less wide-ranging. In consequence there is, in a historical sense, something that members of a copied kind are "for" doing, something current members are "supposed to" do.[1] Third, the members of any copied kind are characterized by what one might call a

"historically proper placement."[2] That is, the operations by past members, on which production of the current ones causally depends, were *co*operations with members of specific *other* copied kinds located alongside those past members. Past double-lensed eyes, in eagles long since dead, did something that causally contributed to the replication of eyes just like them in the eagles of today, but this "something" would not have helped eagles, nor contributed to the replication, if the eyes had not been accompanied by brains equipped to read the complex neural signals that the eyes sent. Screwdrivers have served to fasten objects together, but only because environed by screws suitably slotted and shaped.

Since "copied kinds" is my own coinage, I can simply stipulate that the members of any copied kind are uniformly characterized by a particular shape, a particular proper function, and a particular historical placement. But it does not follow that I can simply stipulate that the members of any copied kind are *essentially* characterized by three such properties. On the contrary my position is, as I have said in the introduction, that we must *learn* which of an object's properties are essential to it; claims of essentialness must be based on evidence. What sort of evidence, then, supports the claims I am making about the essential properties of any copied kind?

A conventionalist might answer that we learn which properties are essential to a given copied kind largely by tuning in to our own conventions for reidentifying kinds of artifacts and kinds of biological devices. We imagine, ensconced in our armchairs, various scenarios both realistic and not-so-realistic, and ask ourselves whether the items envisioned in them would still be household screwdrivers or eagles' eyes or stickleback mating dances. In the process we come to sense that it is our convention to individuate

artifact kinds and biological device kinds by a combination of a specific "shape" and a specific performance that members of that kind are supposed to do. Thus for the conventionalist shape and proper function get welded together, as elements of an essential nature, by our ways of thinking about the world. Empirical discovery about how the world works—specifically, about how the copying mechanisms work that produce members of such kinds—then teaches us that yet a third property is attached to these essential natures, namely, historically proper placement.

But on a realist understanding of what it is for properties to be essential, *all* properties comprised in an essential nature must get joined together by virtue of the way the world works. The world must weld together the distinctive package of properties found in member after member of a given natural kind. There need be no single property responsible for all the others, no single property found among members of no other kind in nature (see 2.2)—no, the properties essential to a given kind may individually be rather commonplace, individually found among members of various kinds—but it must be a function of the way the world works that around some pair (or triad, etc.) of such commonplace properties enough other properties cluster to yield a combination found in no other kind in nature. It cannot just be a function of how we think of the members of a copied kind that throughout its membership a particular shape is joined to a particular proper function. It must be a function of the copying process itself that produces the members of that kind, that in all such members a shape is joined to a function and to a historically proper placement, and quite possibly to a range of further properties as well, in such a way as to yield a cluster of properties found in no other kind in nature.

Now the properties essential to any copied kind typically *will* be properties that individually are "commonplace," capable of showing up in members of other copied kinds. The mating dance of the stickleback fish has the proper function of inducing female conspecifics to engage in reproductive behavior—in the case of sticklebacks, this means releasing eggs—and this proper function is in fact found in a wide range of other mating dances and behaviors. The "shape" of the stickleback's dance, its choreography, certainly *could* be found in copied behaviors selected for a different proper function, even if in fact no such other behaviors have yet gotten selected; it could be the shape of a threat display, for example.

But such commonplace properties can be *essential* properties of a copied kind nevertheless, if the way the world works—specifically, the way the copying mechanisms work that produce members of that kind—is such as to ensure that whenever a pair (or a triad, etc.) of the properties that uniformly characterize that kind are present, other characteristic properties will likewise be present, yielding an overall combination found in no other kind in nature. The nature of the copying process thus must make the combination of a particular proper function and a particular shape be a sufficient condition for the presence of a particular historically proper placement. Or else it must be such that *that* shape in a copied dance and *that* historically proper placement for the copying ensure that the dance had *that* proper function. Or else it must be such that *that* historically proper placement and *that* particular proper function are jointly a sufficient condition for—could have been present only if there had been—the presence of just *that* shape in the dance.

How in general can one *tell* that the combination of two properties, wherever yielded by the world's workings, is a

sufficient condition for yet a third property? The test of flanking uniformities (2.5) begins by turning this question around, namely, as a question about a necessary condition: how does one tell that for that third property to be absent, in some closely similar kind, one or the other of the first two would likewise have to be absent? The test then notes that for that third property to be absent is for there to be a failure of contrast with one or another of that third property's own contraries. Thus the idea, that the absence of that third property would require the absence of one or another of the first two properties, gets converted into the thought that a determinate departure from that third property would go with an answering departure from one or the other (or both) of the first two.

Thus in the case of a copied kind one would ask: would a choreography differing from that of the stickleback's dance in some one fixed way have uniformly gone with either a particular difference in the historical audience of that dance, or a particular difference in the function that led to its getting replicated? And the answer is Yes. Among species other than sticklebacks, dances differing in choreography *do* go with correspondingly different historical placements—with females in *those* species that are wired to respond with reproductive behavior of their own—if the proper function of the dance is still that it is a mating dance. Among sticklebacks themselves, dances differing in choreography certainly *could* have gotten selected for and copied time and time again, *if* they had had the correspondingly different proper function of being threat displays, or if they had historically gotten shaped by the presence of females correspondingly different in their dispositions to respond by laying eggs.

Or consider the familiar household screwdriver. Does it follow from the nature of the copying process that produces

members of this copied kind that *that* distinctive shape and *that* distinctive proper function together guarantee that the historically proper placement of the copying was an environment containing standard slotted screws? The test of flanking uniformities turns this question about sufficiency into a question about necessity: was that historical placement a *necessary* condition for that combination of shape and proper function? If items generically akin to simple screwdrivers had instead gotten produced alongside screws bearing a particular *different* sort of slot—say, a cross-shaped slot rather than a straight slot—would that difference have required, thanks to the nature of the copying process, a difference in either the shape or the proper function? The answer is Yes. In fact that very difference does go along with a commensurate uniform difference in the shape of the blade: that is, where the historical placement incorporates screws with cross-shaped slots, and the copying process still produces items with the proper function of affixing fasteners, it produces *Phillips* screwdrivers. Of course the same sort of copying process, in that altered placement, could still have produced items very similar to simple screwdrivers, that is, items still bearing flat blades—but only if it had happened upon a different proper function with which to endow those items. In sum, to the change in historically proper placement there really *would* (sometimes there *does*) correspond a change in either shape or in proper function.

In the case of typical copied kinds, then, the three features I have outlined are shown by the test of flanking uniformities to cluster together in just the manner of properties that jointly compose an essential nature. There is *realist* evidence for judging that any copied kind essentially is characterized by shape, proper function, and historically proper

as I will presently argue, to have copied the eyes themselves in today's eagles from the eyes of ancestor eagles. But a competent craftsman who fashions a screwdriver, on the model of previous screwdrivers that have proven effective, copies consciously and deliberately. And between these extremes there may seem to be a spectrum of interestingly different copying processes. The current generation of an indigenous people may deliberately copy its ritual rain dance from the dances of previous generations, but with no clear understanding of the benefit to social cohesion, which is the real reason (let us suppose) for the dance's continued existence. An automobile manufacturer might stay in business only because its automobiles replicate the design of pollution-free prototypes developed by a competitor, but may thus design its automobiles out of concern for profit alone; the replication of a pollution-free design may be not unconscious, but not intentional either.

But there is a crucial similarity among the copying processes that produce these seemingly disparate items. They are all causally sensitive to the performance, by the past tokens that figure as "originals" in the copying process, of certain sorts of functions—perceptual or behavioral or physiological functions among "originals" embodied in animals, functions of fitting and turning and bending among "originals" embodied in tools, functions affecting performance and ease of use among commodities. The processes are such as to copy for a longer time, or in greater numbers, previous items that have served some such function than previous items that served none; or previous items that served such a function more often or more effectively than items that served the same function less well; or previous items that served a more urgent such function than items that served one less urgent. The copying processes or

mechanisms are not confined by the ways they work to copying items of just *that* qualitative make-up found in the items *currently* produced. They will have copied qualitatively different items, to a lesser degree. Their histories will have warranted the claim that if originals *more* functional than the current products had historically been available for copying, those more functional originals *would have* gotten copied instead.

This is why it is indeed legitimate to speak of natural selection as copying, not just genotypic configurations from generation to generation, but also the phenotypic traits that express those genotypes. Directly, of course, it is only genes that get copied. The offspring of an amputee do not inherit wooden legs. But often what causes a particular genotype to get replicated more and more widely, in generation after generation, is not random genetic drift, but the adaptational (and hence reproductive) success of the phenotypic trait for which it codes. In such circumstances the consequent spread through the gene pool of the underlying genotype in turn causes a spread through the species of that phenotypic trait. Hence often, the successes achieved by earlier tokens of a phenotypic trait cause the production of later tokens. There is a process that produces eyes in present-day eagles that resemble eyes in ancestor eagles, and it is causally sensitive to the successes scored by those ancestor eyes. There is a mechanism responsible for the presence in present-day beavers of dam-building behavior, and it is causally sensitive to the successes achieved by past tokens of just such behaviors. In short, while what *directly* gets copied from generation to generation are genes, it is also true that *indirectly* phenotypic traits get copied across generations, copied as a causal consequence of functions served in the past. In just this sense the dams made by present-day beavers

can be said to be copies of dams made by ancestor beavers; as Dawkins (1982) points out, the dam is as much a part of the beaver's naturally selected phenotype as is the beaver's tail.

Items produced by such success-sensitive copying processes, then, are the subject of my present contention. The contention is that where a particular copied "shape," a past performance causally responsible for the copying (i.e., a proper function), and a historically proper placement all come together, further properties will typically cluster with them. Inferences from examined samples will non-accidentally hold true for copied kinds, just as for natural kinds more familiar in philosophical discussions. These further properties fall into three main categories. There are properties connected with material composition; there are functional peculiarities of the design that is copied; and there are specific propensities for historical change when and if the proper placement should alter.

First, then, the members of a given copied kind can warrantedly be expected of be made of the right sort of stuff.[3] This is obviously true for artifacts and kinds of phenotypic hardware; it is true in a transposed sense for even reproduced behaviors. Household screwdrivers, for example, can warrantedly be expected to be made of fairly firm materials. For the screws they turn must be firm enough to penetrate the materials to which they are applied, and the screwdrivers themselves must turn the screws without being bent in the process. The materials composing a beaver dam must be firm enough that, when woven together in the characteristic design, they do not snap or dissolve under the pressure of the impounded water. But they must not be so firm or dense that beavers cannot grasp pieces of them with their jaws. The mating dance of a particular species of fish

must not have a choreography so acrobatic that almost no male can dance it, nor so complex that almost no female can recognize it.

Second, the members of any copied kind will embody a particular design solution to what might broadly be termed an engineering problem, and with that solution will go particular excellences and liabilities. The mechanism in humans for localizing sounds has a simple, "low cost" design, but a recurrent and predictable failing: it commonly fails to differentiate a sound emanating from a source 30° to 60° removed from "straight ahead" from a sound emanating from 30° to 60° removed from "straight behind." The stereoscopic visual systems found in mammalian predator species embody a solution to the task of achieving depth perception, but one achieved at the cost of a narrowing of the visual field. Human rituals involving sacrificial offerings embody solutions to the challenge of meeting social and emotional needs, but in times of famine predictably entail suffering and disruption as well.

Finally, the members of at least some copied kinds will have propensities to shift in their qualitative makeup, or a history of having actually done so, in ways that coincide with changes in their historically proper placement. The hunting behaviors in a predator species will alter as the customary prey species acquires new routines of evasion and escape, or dies out and gets replaced by other prey species. New strategies for responding to social defection may develop in a given population as defection comes to be more common. Mating dances or plumage may become more stylized and exaggerated in a given species, when females start favoring by their responses the more colorful of the dances or plumages originally on offer. The syntactically significant suffixes and markers in a language will shift as the

phonemes of that language come to be typed differently by its speakers.

There are then reasons for thinking that copied kinds will be characterized by essential properties beyond those that form the core of their essential natures—beyond the properties of shape, proper function, and historically proper placement. Copied kinds will at least often have rather rich essential natures, just as is the case with the natural kinds more often discussed in the literature—for example, chemical kinds such as water, physical elements such as gold. But the scope of this point should not be exaggerated. Some copied kinds may have thin essential natures, and some may even be characterized only by a distinctive combination of shape, function, and placement. Even they will have genuine essential natures, as 7.1 argued, but natures that are certainly less interesting.

7.3 Classes of Artifacts That Are and Are Not Copied Kinds; Coinciding Objects

Let us now focus on the particular case of artifacts. I have so far argued that copied kinds in general are characterized by clusters of essential properties; thus that where the properties in such a cluster arise or cease to obtain, *substantial* change occurs; thus that members of such kinds exist in ontological strictness. But what follows about the ontological status of the artifacts that common sense recognizes? Is every kind of artifact for which there is a sortal in common usage—for example, chairs and tables and sweaters—a copied kind in its own right? If not, what marks the division between the kinds of artifacts that may be admitted to our ontology and those that must be treated as mere projections of our language and culture?

In this section I defend and refine the position that broad and inclusive kinds of artifacts are less likely to constitute true copied kinds than are kinds more specifically delimited. Chairs are less likely to compose a copied kind than are desk chairs, and desk chairs are less likely than Eames desk chairs of the 1957 design. But this is not to say that where one kind of artifact is a specific version of some broader kind, *only* the more specific can claim to be a true copied kind. Given a modicum of specificity, *both* may be perfectly genuine as copied kinds. The difference may be only that the more specific kind is characterized by a richer, more interesting cluster of properties.

The basic rationale for this position is obvious: kinds as broad as chairs and tables can barely be said to have any one "shape" or qualitative character in common at all. Moreover, they have no well-defined historically proper placement: there are dining room chairs, electric chairs, birthing chairs, and camping chairs. The challenge lies not in finding reasons for thinking that artifact kinds must be *fairly specific* to qualify as copied kinds. It lies rather in defending the claim that a fair degree of specificity is *enough*—that where one artifact kind is a specific version of another, the former need not always usurp the latter's claim to being a copied kind. For suppose that one artifact kind is a specific version of some broader artifact kind, that both do amount to copied kinds, and that some one artifact is a member of both. Suppose, to make it concrete, that some one chair is both a desk chair and an Eames 1957 desk chair. Then we seem to be faced with "the problem of coinciding objects." Exactly where that chair is located there is an object that essentially has the characteristic Eames shape, and an object that does not essentially have that shape. But if object A differs in its essential properties from object B, A and B are distinct.

So in that location there are two objects. Each of them is a chair. Yet if the 30-pound desk chair and the 30-pound Eames desk chair are both placed on a scale—which can be done, *mirabile dictu*, in a single motion—the scale reads "30," not "60."

The problem of coinciding objects has indeed been lurking in the wings since this chapter began. It is the main reason why some contemporary metaphysicians judge that artifacts do not really exist in the world (see Rea 1997). For artifacts of many familiar kinds can readily be supposed to coincide with matter-objects that differ from those artifacts in their modal properties. A statue of Goliath, for example, might be thought to coincide with a particular lump of gold; but this lump would surely be able to survive getting flattened, while the statue could not. It seems to follow that the statue, if real, is a distinct object from the lump. But the presence of these two objects in the same volume is undetectable by scales and other instruments of observation. Some contemporary metaphysicians infer that one of these objects must go, namely, the statue (e.g., Zimmerman 1995).

Now the problem of coinciding artifacts seems to me genuine, and I will return to it presently. The problem of coincidence between any artifact and a matter-object is another matter. Why need we suppose that there is some one "matter-y" thing, possessed of a spatiotemporal career of its own, which at present composes the statue, but may later not do so? Our ontology must, to be sure, admit that there is such a stuff or substance as gold; gold, like water and bronze, is what Aristotle called a secondary substance, one that by nature occurs in spatially localized quantities. Our ontology must also recognize the individual atoms that between them compose any localized quantity of gold, and the molecules that compose any sample of water. But why need we say that in addition to the one stuff of which a homogeneous artifact is made, there is some one object that composes that artifact?

Just what nature are we to think of such a matter-object as having—just what features should we think of as marking out its career? One answer sometimes discussed is that the object is the *aggregate* of gold atoms now within the statue. *This* matter-object by nature survives just as long as those very individual atoms continue to exist, and just where they come to be; unlike the statue, it can survive radical dismemberment, but also unlike the statue, cannot survive the destruction of even one of those atoms. An alternative answer is that the matter-object in question is a *parcel* of gold, defined by its having exactly that statuesque shape. When even a small chunk is clipped from Goliath's ear, the statue continues to exist, albeit in damaged condition, but the parcel is no longer.

There is a third answer as well, a more promising answer, and I will consider it in a moment. The problem with these first two matter-objects is that they are said to have, essentially, properties that do not test out *as* essential on any test of essentialness that is even remotely appropriate, provided we adopt a realist stance toward essentialness (Elder 1998a). Now if we adopt a conventionalist stance toward essentialness, things may indeed be different. It cannot be said that *people in general* wield conventions for reidentifying aggregates and parcels—for tracing their careers across space and time—but there are *philosophers* who coin and adhere to precisely such conventions. And then if it is true that our conventions are constitutive of essential status—if the features that our conventions take as cues for reidentification, whether of individuals or of kinds, eo ipso *are* essential properties of individuals or of kinds—then aggregates and

lumps have just those essential properties that the present "problem of coinciding objects" supposes them to have. But conventionalism about essentialness, I argued in part I, yields an incoherent ontology. And if we adopt a realist stance toward essentialness, the only appropriate tests for essentialness must look to ways properties cluster together reliably—across all members of some class, even in counterfactual scenarios—in virtue of the way the world works. The test of flanking uniformities is one such test (and, so far as I can tell, the only such test).

But set the specifics of flanking uniformities aside. Could *any* test show that the properties essential to an aggregate of gold atoms, or to a parcel of gold, cluster together with other properties, in virtue of the laws of nature? Begin with the case of the aggregate. It essentially has the property, supposedly, of *being-composed-of-numerically-those-atoms-of-gold*. But could *being-composed-of-numerically-those-atoms-of-gold* engage the laws of nature in such a way that yet other properties will cluster together with it? No, since the laws of nature are never engaged by bare numerical identity, by haecceities. They apply to things by virtue of the things' properties or circumstances or relations—by virtue of repeatables. (The same reasoning shows that origin, i.e., being-derived-from-numerically-that-matter or being-derived-from-numerically-that-source, also cannot be essential; I will return to this point presently.)

Turn next to the parcel of gold coincident, supposedly, with the statue. It is said to have essentially the property of being of exactly *that* extent or size or mass. But, with rare exceptions such as piles of Uranium-235, that a sample of some stuff is of one precise size or another makes no further difference, under the laws of nature, to what other properties it has.

Then might we think of the matter-object with which Goliath appears to coincide in yet a third way—as just *that sample* of gold, that expanse or chunk of gold? The persistence conditions for this matter-object would be more loosely defined than for either of the first two; they indeed vary with different conversational contexts. Sometimes asking "Just where is that sample of gold now? Does it still exist?" will amount to asking whether 90 percent of the atoms in the original statue are still joined together, sometimes just to only whether half or more of them are joined together, sometimes just to asking whether some percentage of them still now exist. My response is that all such questions are perfectly genuine. But they are questions about many objects, in the plural—many gold atoms—not questions about some one object.

At the same time, the problem of coinciding *artifacts* does seem perfectly genuine. Artifacts belonging to one copied kind often do, it seems, exactly coincide with artifacts belonging to some other copied kind—typically another kind more specific, or less. An Eames desk chair, 1957 design, occupies exactly the same volume as does some desk chair; and, as in Sidelle's example (1998), a single long piece of woolen yarn, itself an artifact, might compose the whole of a sweater. How then can two distinct artifacts—which differ, after all, in their essential properties—be wholly present at exactly the same place?

The sting of this question seems to come precisely from the realist position on essentialness which I so vigorously endorse. If essentialness is really *out there in the things*, it seems, a thing must have essentially those properties that *are* essential to it strictly in virtue of its own material makeup, its being composed of just *those* atoms. And then if thing A and thing B have exactly the same material

composition, they cannot differ in respect of their essential properties (cf. Heller 1990, pp. 30–31).

But what this chapter has argued is that, in the case of copied kinds, essentialness can be *out there in the things* in virtue of the histories of function that lie behind the causes that produced the things. The long piece of yarn springs from a copying process long underway, continued over generations because of successes its earlier products scored at composing primitive socks and mittens and cords as well as sweaters; the "shape" in virtue of which it figures as product of this process involves its thinness and the crisscrossing of wool fibers within it, not the sweatery form it currently assumes. Eames desk chairs spring from a copying process that began long after the copying of *some* desk chairs *or other*, and that process continued because of special features unique to its products—their exiguous and sinuous shapes, their bright color, and so forth.

Because the Eames desk chair and the desk chair possess different essential properties in virtue of their different *histories*, and not in virtue of any difference in *material composition*, it is unsurprising that when the two are put on the scale, the scale still reads "30." The two are composed of exactly the same matter! Now true, this answer would prolong our difficulties about coinciding objects, rather than resolve them, if expressed as the claim that *the parcel* of matter which composes, for example, the Eames desk chair also composes the desk chair, or if expressed as a parallel claim about *the aggregate* of atoms that composes either. But it need not be expressed that way. It can rather be expressed as the claim that every atom found within the boundaries of the Eames desk chair is found in the boundaries of the desk chair, and vice versa. (Refinements may be needed to reflect the fuzziness of the boundaries of either object. But they

reflect the fact that either object is compositionally vague, not the problem of coinciding objects.)

7.4 "Historical Kinds" and Biological Species

My position, in sum, is this. Commonly recognized kinds of artifacts that are very broad and inclusive are unlikely to constitute copied kinds; fairly specific familiar kinds of artifacts are all likely to do so (more on this in a moment); and among these *fairly* specific kinds the *more* specific will in general be the more interesting copied kinds, the ones that display richer clusters of characteristic properties. Eames 1957 desk chairs are a more interesting copied kind than are desk chairs in general. But now why is that, exactly? Ruth Millikan has argued that for the special sciences, "historical kinds" are especially likely to sustain a rich range of inductive inferences (Millikan 1999). "Historical kinds" are defined as ones whose members not only bear qualitative resemblances to one another but derive from numerically the same historical process of copying as one another. Are Eames 1957 desk chairs a richly characterized artifact kind because they all stem from numerically the same originals in the Eames's studios? Is it true in general that the most interesting copied kinds are historical kinds?

Millikan's contention seems to me to give distorted expression to an important truth. By speaking of historical *kinds*, not just *groupings*, she suggests that there would be a difference in essential nature between, say, an Eames 1957 desk chair and another chair qualitatively just like it that were derived from a historical copying process just like the one that produced the Eames chair. But this difference between the genuine Eames chair and its look-alike would be a difference that made no difference, that entrained no

further properties in either chair; the laws of nature simply are not sensitive to bare numerical identities. So "kinds" is an exaggeration. But there is an important truth here. It is that in studying highly specific copied kinds, we should *act as if* part of what constitutes membership in that kind is a descent from numerically just *that* historical copying process. For in this way we will focus our study on individual copied items that may bear to one another qualitative similarities we did not originally know to look for. Copied items that stem from numerically the same copying process may resemble each other in *many* details of historically proper placement, or of copied qualitative "shape," some of which we did not initially recognize.

My mention of Millikan's "historical kinds" has a second motivation as well. Part of Millikan's motivation for endorsing historical kinds comes from a conviction that biological species must, in some way, be genuine kinds in nature. I have announced my sympathy with this conviction; at the start of this chapter, I indicated I would try to defend the idea that human beings compose a natural kind unto themselves, and so likewise for other biological species. But Millikan is right to suppose that some unexpected philosophical argument is needed to defend such a claim. The protracted criticism of "essentialism" in biology has shown that there are no qualitative phenotypic traits that we can warrantedly expect to crop up in all and only the members of Homo sapiens, or of any other biological species (Dupré 1981; Rosenberg 1985, pp. 180–225; Hull 1992; Sober 1992). Nor does it help, at least not in any straightforward way, to look to the genomes of members of our lineage. There are not even any genotypic features—at least, no features specifiable in qualitative biochemical terms—that crop up in all and only the members of Homo sapiens. Can we

defend the idea that human beings compose a natural kind by noting that they compose one of Millikan's *historical* kinds? No, since *coming-from-numerically-just-that-origin* could not be tied, by laws of nature, to any other properties incorporated in an essential nature. Could we argue that human beings compose a *copied* kind? No, for reasons I have elsewhere identified (Elder 1996, pp. 200–201). But there is an unexpected philosophical move that does succeed, I believe. The "working" genotypic parts of all genomes in our lineage—the parts that are not just "junk DNA"—all do have something distinctive in common. This "something" is not qualitative but dispositional. These parts can to a striking degree be randomly combined with the working parts that are found at other loci, within the gene pool of our historical lineage, to produce viable organisms. Now the mention here of "our historical lineage" may seem to turn Homo sapiens into a historical kind after all, but really does not do so, since its function is just reference fixing. Compare the picture that some readers take from Kripke (1972) and Putnam (1975): namely, that we fix the reference of "water" by saying "water is just *that* physical stuff which shares the microstructure of the stuff that happens to be present in *those* samples in *those* locations." This may render it a necessary truth that water is H_2O; it does not render it a necessary truth that water is found just where those samples are.

7.5 Useless Artifacts and Useful Copyings

My main concern in 7.3 and 7.4 was with a question of specificity. Is every kind of artifact for which there is a sortal in common usage a copied kind in its own right, or is a sortal more likely to pick out a true copied kind, the less its extension is sprawling and diverse? My overall contention was

that in general, the more crisply defined classes of artifacts are more likely to be copied kinds. But let me now balance that suggestion with a cautionary note about certain specifically delimited classes of copied items.

We copy from one another, half knowingly and half unwittingly, a thousand minor details of personal behavior—turns of phrase, bodily gestures, styles of dress and articles of personal ornamentation. Much of this copying is entirely uninfluenced by any history of function that the items copied may have. The psychological and social mechanisms that underlie the copying are either sensitive to past functionality only sometimes—perhaps mainly in larger and more consequential aspects of behavior—or are distinct from the mechanisms that underlie function-sensitive copying of cultural items. Or, indeed, the copying may occur because there is a function served by the copying itself—for example, that it affirms group affiliation—rather than by the items copied. In any case familiar artifacts such as neckties, high-heeled shoes, and nose rings are very unlikely to amount to copied kinds. The behaviors of wearing such personal articles may fall into copied kinds, but the articles themselves probably do not.

The main reason for this is that members of true copied kinds have a characteristic shape—in a literal or metaphorical sense—and replication of that shape causally depends on something that previous members of the kind did in consequence of having that shape. Now neckties (for example) do literally have a typical shape: a necktie typically is shaped like two elongated kites joined at the tail. But what causes that shape to get replicated, in one bolt of silk after another, is not some performance that earlier neckties were disposed by their shape to carry out. The causes that produce new neckties have nothing to do with

performances that past neckties, as physical objects, effected. That is why neckties can vary widely in width, can have parallel sides, can get fashioned from a wide variety of materials, and why inferences from the shape of this year's neckties to the shape of neckties in 2010 will only accidentally be accurate. In contrast, the ways in which neckties get knotted around the neck, and the circumstances in which neckties thus knotted get displayed, actually *are* matters over which we may run inferences that non-accidentally succeed. The reason why is that it is *wearings of* neckties that form a true copied kind. These have a characteristic physical and social "shape," and get reproduced because, in the historically proper placement of a specific dress code, they have afforded their agents social access or acceptance. Ontologically, there are manufactured materials such as silk and cotton yarn, themselves both secondary substances and copied kinds; these materials exist in spatially localized quantities, and of these there are some shaped like two elongated kites joined at the tail; and there are wearings of neckties. That is all. The expanses of silk or of cotton yarn (etc.) that satisfy the sortal "neckties" do not have essential properties distinct from those of any other parcels of these materials. Like any such expanses, they essentially are characterized only by the properties essentially characteristic of silk and of cotton themselves. These expanses do not amount to unitary matter-objects that trace out spatiotemporal careers of their own.

7.6 The Problem with Projectivism: Customs and Conventions

If there really are in the world instances of copied kinds, there are in the world at least some artifacts. So any

philosopher who holds that artifacts do not, in ontological strictness, exist, must deny that copied kinds are instanced in the world. At the same time such a philosopher must allow that we *project* onto the world existences of artifacts—creations of artifacts, courses of existence which they trace out, destructions of artifacts. What elements in the world act on us to cause this projection, according to such a philosopher? The only plausible answers must cite our customs or conventions or linguistic practices. But the arena of custom and convention and language is rife with copied kinds, as I now briefly shall argue. If this is correct, the ontology of *projectivism with respect to artifacts* is incoherent.

Consider, to begin with, some typical customs. It is customary among many peoples to mark national holidays with public spectacles or the singing of patriotic songs. There are customary ways of preparing meals, there is a custom of taking a siesta, and there is a custom of bringing casseroles to the homes of people recently bereaved. It seems hard to doubt that at least part of what causes such patterns of behavior to get copied from person to person, and from generation to generation, is some function that the patterns have repeatedly, if not invariably, served. Thus such copied patterns have not only a characteristic "shape" but also, it seems, a proper function. It is no objection to this claim that different patterns or practices could have served the same function as well. So long as we are confident that the mechanisms that copied these behaviors would have copied them (perhaps actually *did* copy them) more widely than behaviors that served no function, or served the same functions less well, or served functions less useful, the attribution of proper function is warranted. Moreover, these customary behaviors serve functions only when and as cued to

customarily recognized settings—to holidays, to meal times, to members of the family of the deceased—and can therefore be said to have historically proper placements. At least many customs, it seems, are copied kinds.

To call a copied pattern of behavior a "convention," in contrast, often is to suggest that it *lacks* a proper function. It is a convention in countries other than England, Australia, and Japan to drive on the right side of the road. But obviously right-side driving is not intrinsically useful, nor is driving on the left intrinsically a poorer practice. It is a convention to say "hello" when answering the phone, to extend one's right hand when greeting someone, and to call a chair "a chair"—but in all of these cases the intrinsic content of the act confers by itself no benefit or gain. But coincident with every case of such conventionally copied behavior there is something that does have a proper function and is a member of a copied kind. It is that same behavior relationally described—that behavior *as* a copying, *as* a replicating of conventional behavior. *Replicating* right-side driving, where right-side driving already has the status of a widespread behavior, copied from person to person over long periods, is indeed extremely useful. Replicating an *expected* sound by saying "hello"—as opposed to producing just that sound for its own sake—is indeed useful. *Followings of* conventions have specific shapes, they often have proper functions, and they have historically established conventional settings. They too are then copied kinds.

Finally, a word about linguistic practices. There has been considerable debate as to whether, and in what sense, language is governed by conventions (see Millikan forthcoming b). Whatever the outcome of this debate, it seems

ontology—there must also be that to which The Austere appears as it does. But this additional element is not a transcendental subject, nor several, but a linguistic community of naturally selected minds. So it is false that in the beginning there was (only) The Austere. In the beginning—on the ground floor of ontology—there is the splendidly, marvelously rich.

Notes

Chapter 1

1. This is a slight oversimplification. Strictly, chromium is also found in Turkey and in the Phillipines.

2. As to Sidelle, this is only his *initial* formulation of what we know about chemical kinds. "I have proceeded by giving the conventionalist's story in the material mode," he then remarks; "... The conventions, of course, are in the first instance rules governing the use of terms, or kinds of terms, and I may have gotten myself into some trouble by proceeding at the object level" (1989, p. 43). Sidelle's preferred formulation, for reasons I make clear below, is that the extra premise is something we know about the proper use of *terms for* chemical kinds.

3. Sidelle 1989, p. 55n., p. 57; 1998, pp. 441–444; cf. Jubien 1993. "World-stuff" is from Hawthorne and Cortens 1995.

4. Michael Rea puts forth much the same paradox in Rea 2002, ch. 7. But Rea's paradox concerns temporal priority, not logical priority, and it is presented as a paradox confronting *naturalists*—for Rea argues that the only tenable position a naturalist can take on modality is conventionalism.

Chapter 2

1. Hegel 1975, secs. 89–98, or Hegel 1969, pp. 109–137 and pp. 600–622; Aristotle 1966, *Physics*, Bk. I, ch. 5, or Bk. V, ch. 1 and ch. 5. Recent philosophers who have *not* overlooked the importance of contrariety include

Chapter 7

1. Any historical account of proper function, like the one I take over from Millikan, faces a "poser" concerning the very first item from which a copied kind comes to be copied. An example: didn't the very first telephone, fashioned by Alexander Graham Bell, already have a proper function (Plantinga 1993, p. 203)? From Millikan's perspective (to which I subscribe) the answer is "Yes and No." The first telephone had no direct proper function, but it did have an adapted and derived proper function—that of enabling remote conversation. In just the same way, if a chameleon turns a shade of puce unprecedented in chameleon history, its skin color has an adapted and derived proper function—that of matching its puce surroundings (Millikan 1984, ch. 2). "Derived" here means that the telephone or the skin color inherits its proper function from that of the program in Bell, or the device in the chameleon, which produced it. In Bell's case, the derivation probably extends further still: beyond the program that underlay production of the telephone, to a program for forming such programs, and perhaps to a capacity for forming programs for forming programs. The derivation ends at a device that operates independently of Bell's conscious intentions, and which has a direct proper function. This brings up the "poser" concerning the proper function of the first item from which a *biological* copied kind comes to be copied. Suppose the first wings (tokens) arose as a result of a single, massive mutation. Didn't those very first wings already have a proper function? But there is no intuitive pressure on Millikan to answer Yes. The onset of (direct) proper function, she can plausibly reply, depends on the intensity of selectional pressure on the gene pool. It depends on how soon the capacity for flight, bestowed by early wings, conferred replicative advantage on the genes which coded for wings—and replicative disadvantage on the alleles. This is a causal question. The answer to it—and to the question where (direct) proper function begins—may be somewhat vague. But it would be poor practice to throw out causation, or the theory of natural selection, out of preference for a neatly segmented universe.

2. I say "historically proper *placement*" rather than "environment" because the latter suggests a *broad* cross-section of the historical surroundings; placement is a matter of co-location, and consequent cooperation, with tokens of *specific* other copied kinds.

3. Much the same point is made by Ned Block (1997) in his discussion of "the Disney Principle."

Chapter 8

1. I infer that this view has proponents from the fact that it is the intersection of two widely held views: the belief that all there is in the world, in ontological strictness, are the microparticles posited by physics; and the belief in mereological universalism (also known as unrestricted mereological composition). Alan Sidelle reviews the considerations that lead philosophers to the former belief in section Sidelle 1998, § V, and adds that "these philosophers are not small in number" (p. 440). A representative defense of mereological universalism is Rea 1998.

2. One good place to see a representative problem cropping up for Husserl is Husserl 1970, sec. 61. One good place to see a similar representative problem for Kant is in the first *Critique*, at the bottom of A 534 / B 562, when compared with the second sentence on A 541 / B 569, and with A 545 / B 573 ("*Nun tut ihm . . .*"), and with the very start of A 550 / B 578 (in the Kemp Smith translation, p. 465 as compared with pp. 469, 471, and 474).

3. See Jackson 1998. Jackson contends that his position, viz. that terms such as "water" have "A-extension" in addition to the more familiar "C-extension," is distinct from the description theory of reference. But his contention seems to me unsupported.

4. The speaker does have to know the right *sorts* of questions to ask, in order for the tokens of a term in his sentences to corefer with tokens of that term as uttered by fellow speakers. In Millikan's parlance, he must wield a "template" of the item referred to, an outline of the *sorts* of properties with respect to which it is stably and determinately characterized (Millikan 2000, ch. 5). But note that the idea of a "template" is *not* the idea of a description (via determinables) that all intelligent users of a term associate with the term, and which affords a priori knowledge about the referent. For some or all elements of a template are revisable in the face of experience (ibid., p. 30), and differences in personal experience may lead one speaker to associate a detailed template, another a more sketchy template, while yet both utter tokens of a common term that are perfectly coreferential (ch. 5).

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