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WHAT'S THE FUSS ABOUT SOCIAL CONSTRUCTIVISM?

The topic of this paper is social constructivist doctrines about the nature of scientific knowledge. I don't propose to review all the many accounts that have either claimed this designation or had it ascribed to them. Rather I shall try to consider in a very general way what sense should be made of the underlying idea, and then illustrate some of the central points with two central examples from biology. The first thing to say is that, on the face of it, some doctrine of the social construction of science must self-evidently be true. The notion of science as progressing through the efforts of solitary geniuses may have had some plausibility in the seventeenth century, but it has none today. Science is a massively cooperative, social, enterprise. And surely it is constructed. Scientific knowledge doesn't grow on trees; it is produced through hard work by human agents. Putting these two banal points together we conclude that science is socially constructed.

The second thing to say, on the other hand, is that a great many philosophers appear to think that social constructivism is not only false, but positively pernicious. Given the first point, this presents something of a paradox. Presumably more is taken as implied by this apparently harmless phrase than appears from my banal gloss. One such implication emerges from a less naïve interpretation of 'social': the question is not just whether science is cooperative, but whether the values or beliefs of a particular society influence scientific opinion. I shall return to this point shortly. Some anti-constructivist philosophers would also object at once that my first paragraph has begged the question by talking about scientific knowledge. For one interpretation of constructivism is that it somehow implies that what scientists say about the things they study is not true, or that the things they purport to describe do not exist. It is associated, in other words, with various forms of anti-realism. No doubt it is the case that some who claim to be social constructivists also subscribe to some brand of anti-realism, though as I shall insist it need not imply any such thing. Many things are, after they have been constructed, real. The problem, of course, is that the notion of construction may suggest that science is, in some sense, made up. Traditionalist philosophers want to insist that scientific truths are discovered, not in any sense made up.

What I want to suggest in this paper is that there is solid middle ground between the extremes of science as discovered and science as made up. Science, I take it, is produced by people in interaction with nature. Nature doesn't determine what science we produce, because there are indefinitely many sets of truths we could articulate about nature. Which we choose to articulate will depend on the interests of a particular investigating community. We want, as Philip Kitcher (2001) has recently put it, to distinguish the significant truths. And though truths are only significant for someone or some society, they may, nonetheless, be truths. I shall attempt to illustrate such a position by focusing on the determination of schemes of classification. While nature does not, I think, determine how we should classify phenomena, something that is in fact importantly guided by our interests, schemes of classification can be more or less effective vehicles for genuine illumination of nature. First I shall say a bit more in defence and explanation of this general view.

Consider the suggestion that if our scientific stories are constructed, made up even, they may be made up for reasons quite distinct from the traditionally assumed goal of simply describing how things are. Actual critics of actual constructivists do often assume something like this, and accuse constructivists of the view that scientific claims are devised largely to serve

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personal, social, or political goals of their authors. And it is certainly true that constructivists have tended to pay more attention to determinants of scientific belief of these kinds than have traditional philosophers of science. When it is further supposed that these secondary goals are sufficient to explain scientific belief on their own, this view is both implausible, and implausibly attributed to most serious thinkers who have embraced the term 'constructivist'. Few deny that scientific belief has some important dependence on interactions with the world. On the other hand scientists are people, and undoubtedly they do have personal, social, political, etc., goals. If, as many philosophers have insisted, appropriate interactions with nature are insufficient to determine scientific belief, it is hardly implausible that these other goals may have some role to play.

This, I think, gets us close to the genuine philosophical issue that underlies much of the controversy over social constructivism. Those most strongly opposed to constructivism are those who believe that interactions with nature completely determine scientific belief and, in connection with proper scientific methodology, determine true beliefs about the world. Of course there is a good deal of nuance possible to such views. No one supposes that all beliefs about nature ever held by honest scientists are true; it is acknowledged that science may be very difficult, and take many attempts. And it is generally acknowledged that there are, in principle, indefinitely many true facts about the world, so some account is needed of the process by which a finite set of these is selected as being worth including in the corpus of science. However such optimists will at least assume that science approaches the truth, and they are likely to believe that in some areas it has even reached the truth. They will also want to explain failures to achieve truth in terms of some kind of failure of scientific method—perhaps failure unavoidable at that point in history, perhaps excusable in many other ways, but nevertheless in some way a deviation from the ideal interaction with nature that will, in the end, lead to the true story of how things are. Thus one concrete issue that tends to divide constructivists from their critics is the universality of scientific knowledge. If nature will eventually tell us the true story, then all sufficiently diligent inquirers should converge on the same story. If knowledge involves also something that we contribute, then different communities of inquirers may contribute different things, and come out with different knowledge.

Another approach to genuine disagreement here is through a classic statement of social constructivism, the symmetry principle famously associated with the Edinburgh School of the sociology of scientific knowledge. This principle states that there is no difference in principle between the explanation of scientific beliefs that we take to be true and those that we take to be false (Barnes 1974; Barnes and Bloor 1982). Though apparently quite innocuous, the principle in fact seems to block the anti-constructivist project of explaining the failures of scientific process to produce scientific truth.

There is surely something importantly right about the symmetry principle. What causes scientists to believe what they believe is at one level a psychological question. And it would be absurd to suppose that there is a systematic psychological difference between those scientists who got it right (as we now think) and those who got it wrong. Or, indeed, that there is some different psychological process at work when the same scientist gets it right or wrong—as if Newton must have been in a different psychological state when he asserted the existence of absolute space and when he proposed the laws of motion. This point is quite independent of whether we are inclined to believe that such explanations are more likely to be found in his relations with his mother during early infancy or in rational deliberation on the deliverances of experience.

Nonetheless, it will be said, there is one kind of explanation that is available for true beliefs but not for false beliefs, namely the explanation of the belief that p by appeal to the fact that p. Thus we may be tempted to say that the belief that there once were dinosaurs is most fundamentally explained by the fact that there once were dinosaurs. But of course this kind of explanation always requires a further step. Facts only explain beliefs via evidence or reasons. The belief that there were dinosaurs is explained by such things as observation and analysis of fragments of bone, the results of carbon 14 tests, and so on. These facts are no doubt explained in turn by the erstwhile existence of dinosaurs. But the dinosaurs themselves cannot explain the difference between the beliefs of the palaeontologist and the person who believes that the world was created in 4004 b.c. Neither has direct contact with the dinosaurs. One, sensibly, takes the bones as evidence for the existence of long extinct creatures, the other, less sensibly, as evidence for God's ingenuity in testing our faith. Even were I to travel back in time to the Jurassic and observe real live dinosaurs it is my observation of them rather than the dinosaurs themselves that explain my much firmer belief in their existence. I do not mean here to introduce a strange mental object, merely to note that it is in interaction with the world that beliefs about it are produced, and this interaction is more than purely passive reception of pre-interpreted data.

Hence both special creationist accounts of and evolutionary accounts of such things as the provenance of dinosaur bones are socially constructed in the stronger sense introduced at the beginning of the essay. We can very well ask which are reasonable, defensible, etc., constructs. And we can give general accounts of the way science works as part of the grounding for the distinction between reasonable and unreasonable epistemological constructs. It is then important to note that this distinction by no means maps exactly on to the constructs we now take to embody true knowledge and those we do not. Newton's mechanics was a brilliant and reasonable construct, even if we now take it to have been wrong in some very important respects. Democritus was perhaps right in certain respects in his atomic theory of matter, but it is open to question whether his beliefs were properly founded. Social constructivism is often taken to deny that there can be any distinction of epistemological merit between different epistemological social constructs (theories, let us say). What I have pointed out is that there is nothing in the idea of social constructivism that mandates this conclusion. And, as a matter of fact, I doubt whether many of those thinkers prominently associated with social constructivism hold any such opinion.

Before considering some examples in more detail, I need to say a bit more about the senses in which science is a 'social' activity. As already noted, it is largely uncontroversial that science is a highly cooperative, that is social, activity. However, social constructivism invokes different connotations of this term, in accordance with which it is supposed that various social and political agenda affect the outcome of science. In fact there is a huge range of actual and possible positions here. At one extreme it is possible to note the collaborative nature of much contemporary science with equanimity and conclude only that science has become more complicated and difficult. What could once be done by a solitary investigator now requires the contributions of several or many.

A growing number of philosophers, without going so far as to embrace the designation 'constructivist', have recently taken the social dimension of science much more seriously, however. There is a growing body of work under the rubric of 'social epistemology' specifically addressed to exploring the consequences for our understanding of knowledge from a social perspective (see, e.g., Solomon 2001). One particularly interesting project is Helen Longino's (1990, 2002) argument that objectivity can only be understood as arising from a dialogue between different interested perspectives, and that science needs to be organised socially in ways that will best promote such dialogue. In a somewhat similar vein, Philip Kitcher (2001), has recently argued for the importance of 'well-ordered science' science that is directed optimally at the production of the knowledge that will genuinely benefit (and, importantly, not harm) the members of a society. Well-ordered science, naturally, will tend to generate (socially) significant truth. At the most

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epistemologically radical, and explicitly constructivist, end of the spectrum, we might instance, for example, Bruno Latour's (1987) examination of the process by which a scientific claim comes to be generally accepted, a process Latour describes in terms of the recruitment and regimentation of allies.

Though all of these philosophical projects consider science as much more socially involved than simply asking nature to answer our questions, none of them comes close to denying that there can be any truth to what is believed. Latour, no doubt the closest to such an outcome, may indeed be sceptical of scientific truth for a variety of reasons. But as far as the project just summarised, we need only note that among the 'allies' successful scientists recruit are such things as biological organisms or even inanimate objects. And the ability to control such allies surely depends on having some real insight into what they are and how they work.

On the other hand even the less radical positions delineated view science as a social enterprise in which there is space for the negotiation of scientific agenda and, to some extent, outcomes. Processes are distinguished that allow plenty of scope for social and political forces to have an impact on the scientific process. I don't have space here to examine these processes in detail. In the examples that follow, I shall rather attempt to illustrate a somewhat less controversial aspect of the construction of scientific knowledge through which, it may be argued, social agenda can have an impact on scientific belief. This will leave open the importance of such an influences. The aim will rather be to define an important part of the playing-field in which serious debates between constructivists and their opponents takes place.

More specifically, I plan to show that in biology, at least, the conceptualisation of phenomena, as displayed in schemes of classification, is not determined by nature (as a still important philosophical movement would have it) but is developed, or constructed, in the light of human goals and interests. As indicated above, however, I do not take this to be incompatible with a generally, if selectively and critically, realist attitude to the phenomena they describe. The broader thesis of which this is part, a thesis which will be unwelcome to both extreme positions in the Science Wars, but will seem commonplace to a growing number of moderate intermediate positions, is that science is a process of interaction between social agenda and often growing insight into the natural world. Since I can make no sense of the idea of a final complete insight into nature, I assume that science is likely to remain a domain of partial and partially interested insights into nature.

The Scientific Construction of Kinds

So much for global views of social constructivism. My conclusion so far is that there is no issue here that should be very controversial. Social constructivism is, if modestly construed, largely self-evident. Less modest anti-realist or sceptical positions sometimes associated with constructivism should be treated separately on their merits or lack of them. I turn now to a view which I wish to defend, and a view which is congenial to certain aspects of constructivism though certainly not to anti-realism or scepticism. This view is a thesis about the classificatory systems which are applied to domains of phenomena for purposes of scientific investigation, and the kinds that are distinguished by such classifications. The claim is that rather than being discovered in the course of investigation, delivered by nature to the properly diligent investigator, classificatory schemes are constructed. Though such construction is hardly oblivious to interactions with nature, it is also likely to be motivated in part by factors that may be described as social. Let me begin with two general points about this issue.

First, scepticism about the ability of nature to provide us with the classification of her products has nothing to do with any kind of anti-realism or scepticism about the things classified. I might, not very promisingly, begin a scientific investigation of the category of things that I particularly like. This might generate a miscellaneous assortment of people, kinds of

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food, pieces of music, and so on, which would not strike many as a category straightforwardly discovered in nature. But this casts no doubt on the existence of these things. There is, however, a much more elusive question about the reality, or perhaps the objectivity, of the category. In one perfectly legitimate sense a category is real if anyone bothers to distinguish it, provided only that it offers a more or less reliable criterion for inclusion within it. On the other hand there is a strong inclination to attribute a much more robust existence to, say, the category of acids than to the one defined by my peculiar tastes. One reason for this is that whereas there are both a range of typical characteristics of acids and a general explanation of these common characteristics, in the case of the things I like we might well doubt whether there was anything to be discovered about this class of things beyond the initial defining characteristic, my positive feelings about them. The greater reality of the former category has something to do with its having more robust and interesting properties. This also suggests that the latter category would be poorly chosen for scientific purposes: it is doubtful whether there are any general claims to be made about its members, let alone claims that might amount to scientific laws. And the reason that many philosophers hope that nature herself will provide us with categories is that they believe that only categories with this provenance are likely to figure in scientific laws.

The second point is that the question of the social component in categorisation is likely to be a local and variable one. There is no reason why nature should provide us with categories for social science even if she is generous enough to do so for the purposes of particle physics, for example. It is very widely supposed, in fact, that the basic kinds of physics and chemistry are purely discovered and that the kinds of particles recognised by physicists and the atoms distinguished by chemists are the uniquely correct kinds in terms of which physics and chemistry must be developed. I shall say nothing here for or against this view. On the other hand it is almost as widely believed that the classifications used in social science are chosen to serve particular needs and interests and are, for that reason, contingent. This idea is taken a stage further by Ian Hacking (1999) in his development of the idea that the choice of human kinds, at least, may have profound effects on the humans who are the members of those kinds, an idea captured in his conception of 'looping' kinds. What seems indisputable is that the social and human sciences will deploy a variety of classificatory schemes, and that these will crosscut one another. A particular individual might be, for instance, a manual worker, a member of the Labour party, homosexual, an atheist, and so on, classifications which he would share with distinct further sets of people. Any of these classifications may be relevant for some social scientific project.

Biology, generally seen as lying somewhere between the physical sciences and the social sciences on many relevant dimensions, is therefore a particularly salient dimension in which to consider the respective roles of natural and social factors in the determination of classificatory schemes. Before turning to this, I shall illustrate some of the central themes I want to develop with an example from the social sciences.

Consider the human category of criminals. It is hard to imagine that social science could do without this category, and indeed there is a whole discipline, criminology, based on it. Yet it is quite obvious that the category is in important respects constructed by society. Who counts as a criminal is determined by the laws currently enforced in a particular society, and as legislation is introduced and withdrawn the extension of the kind 'criminal' changes. Clearly the category must be interpreted relative to a specific society: a twenty year old drinker, for example, is a law-abiding member of most European societies, but a criminal in the U.S.A. The staggering incarceration rates in Russia and the U.S.A. (about 1 person in 200) do not reflect an unusual prevalence of a particular natural kind, but social decisions about what counts as serious criminality. The complexity of the processes of criminal formation and of the social motives underlying these processes have

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been famously and brilliantly explored by Foucault (1977).

Despite its central category being socially constructed and relative to a particular society, criminology has some true things to say. There are established links between, say, criminality and poverty, and statistically true psychological generalisations may well be discovered about criminals. I say 'statistically true' because there can be little doubt that in every respect apart from their criminality, criminals will surely be a heterogeneous group. Think only of the stereotypical images of pickpockets, pimps, Mafia hitmen, corrupt executives of multinational corporations or politicians engaged in launching illegal wars. Without endorsing the stereotypes, there is surely not much in common between these groups beyond the necessary contempt for social norms. The diversity of cross-cutting kinds will, I suppose, inevitably put quite modest limits on the possibilities for generalisations about kinds of humans. The question for biology, then, is whether we should see biological kinds as more like human kinds or chemical kinds or, perhaps most plausibly, somewhere in between.

Let me now turn to two biological examples which show quite different patterns of interaction between elements of social construction and elements of the naturally given. The first of these is a topic with which biologists and philosophers of biology have been concerned for many years, the classification of organisms into species. The second is a more recent concern, but one that is increasingly central to theoretical debates in biology, the classification of parts of the genome into genes.

Species

The so-called species problem has been a concern of biologists and philosophers at least since the general acceptance of Darwin's theory of evolution, and in different forms, since at least the Greeks. The modern version centres on the attempt to reconcile a conception of biological kinds generally, and species in particular, with the theory of evolution. A natural point of entry is with the thought that if evolution implies that one species can, over time, turn into another, there can be no sharp divisions between species of the kind that appears to exist between, say chemical kinds. Of course, it is now possible to transmute one element into another, but this is a discrete process and does not entail the existence of a range of kinds intermediate between the two elements.

The assumption that species are the entities which are the primary subject of the theory of evolution has led many philosophers and some biologists to conclude that species are not kinds at all, but concrete, if dispersed, individuals. I shall not be concerned with this issue in this paper. This is because whether or not it may sometimes be necessary to treat species as individuals, it is also necessary for many purposes to classify organisms (see Dupré, 2003, ch. 4). This will be true, for instance, for many purposes in ecology or for the measurement of biodiversity. The question that will concern me is whether such a classification is something provided by nature or, rather something in part constructed for particular human purposes. The former answer would be consistent with the position that in classifying individuals we were in fact distinguishing parts of individuals somehow constituted as such by nature.

There is a notoriously large number of opinions on what distinguishes organism as members (or parts) of particular species. By far the most popular, however, are those based on a criterion of reproductive isolation or reproductive coherence, and those based on genealogy. The trouble with a criterion of reproductive isolation is its limited applicability. The context in which it has greatest intuitive plausibility is for a species in which a range of genetic and behavioural constraints limit reproductive relations to a well-defined set of relevantly similar organisms. Reproductive isolation can also be seen as potentially serving the important function of maintaining the integrity of a coherent and successful genotype. But whereas this seems to apply very well to many species of birds and mammals, for instance, in many parts of the biological world it is much less successful. To begin with, many species are asexual. The attempt to apply this conception of the species to asexual organisms would imply that every distinguishable clone of such a species, being reproductively isolated, was a distinct species. This would lead to a massive proliferation of species and, ironically, imply that the vast majority of species were local clones the application to which of this criterion would seem lacking in intuitive plausibility. At any rate, for a large proportion of the history of life all species were asexual and, incidentally, appeared to show very little in the way of reproductive isolation. Increasing realisation of the extent of lateral genetic transfer between apparently quite distantly related organisms in prokaryotes is beginning to problematise even the assumption of a localised individual genome.

It is very widely the case that the criterion of reproductive isolation fails to deliver intuitively satisfactory categories. A classic example is a group of American oaks between which there is considerable gene flow, but which have retained morphological distinction over very long periods of time (Van Valen, 1976). The theoretical conclusion, given the conception of the species in terms of reproductive isolation, is that this has been discovered to be a single and variable species. But for the forester or ecologist who has reason to distinguish these different types, the criterion of reproductive isolation fails to provide the required kinds. At the other extreme, there are morphologically homogeneous species that exist in a number of geographically distinct populations. These may be reproductively isolated one from another for long periods of time, and therefore would appear to qualify as distinct species. But such a conclusion is driven solely by theory, and serves no useful practical purpose.

The second, and currently most popular, conception of the species ties species directly to evolutionary history by treating the species as a distinct part of the genealogical nexus. Cladism, the dominant version of this idea, requires that species be monophyletic, which is to say that they should include all and only the descendants of a particular ancestral grouping. An immediate difficulty for this approach is that monophyletic groups are likely to occur at many different scales, and it is unclear how to decide which of these is a species. Isolated populations dying out in marginal habitats will form monophyletic groups as will a whole series of increasingly inclusive groups. This difficulty has led a number of theorists to conclude, plausibly enough, that the extent of the species is a matter of convenience and hence, in certain sense is determined by scientists rather than by nature. But the requirement of monophyly itself presents serious conflicts with intuitive judgements about what organisms should be classified together. This is most familiar and striking at the level of larger groupings. Since birds are believed to have descended from a species of dinosaur any monophyletic group that includes all the dinosaurs will also include the birds, showing, apparently, that birds are a kind of dinosaur. It is often reported that science has discovered that birds are dinosaurs, and if the monophyletic criterion of classification is accepted, then this is a correct statement. On the other hand given that birds have diverged rather significantly from there dinosaur ancestors another plausible conclusion is that the requirement of monophyly is either misguided or shows that scientific classifications are of limited use for the intuitive project of classifying like with like.

A final difficulty, alluded to above, is that inheritance is not limited to the transfer of genetic material from parent to offspring. It is increasingly clear that in many groups of organisms there is substantial lateral transfer of genetic material through a variety of mechanisms. This phenomenon raises a doubt as to whether the criterion of monophyly is even intelligibly applicable in a general way, since a group of organisms may have a degree of descent from a variety of perhaps distantly related ancestors. The general strategy of classification into monophyletic groups assumes the traditional view of evolution as generating an always divergent tree. Lateral genetic transfer suggests that a better representation may be a densely connected net of hereditary relations. This seems to be increasingly the case for prokaryotes and to a lesser extent even for

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eukaryotes. This move threatens to undermine the entire project of phylogenetic classification.

I don't propose to examine in detail here the various pros and cons of various strategies for biological classification some of which I have not yet even mentioned (for example morphological and ecological classifications). The important point, which I have argued in much more detail elsewhere, is that there is no criterion discoverable through biological investigation that provides us with a unique and privileged system for organising biological diversity. Particular criteria break down in many cases and, more relevantly to the present topic, they present us with classifications often poorly suited to the applications to which a variety of scientific and non-scientific users may wish to put them. If there were, nonetheless, some unequivocally given classificatory system one might look at this failure of usefulness as merely regrettable. But given the controversy over which system is the best, and given the deep flaws that each of them has in some areas, no such conclusion is reasonable. A more reasonable response is to see particular classifications as selected in the light of particular goals. And this, of course, is to say that classifications are constructed by people to serve their interests.

The observation that even within properly scientific biology principles of classification must be selected to serve particular theoretical or practical ends removes any serious reason for doubting that even thoroughly non-scientific activities may legitimately develop their own classifications of biological entities. Elsewhere I have suggested that such practices as gastronomy, forestry, herbal medicine, and so on may all legitimately divide organisms in ways that do not coincide with those found most useful for scientific purposes (2003, chs. 1, 2). There is also no doubt that such interests have left their mark on canonical scientific classifications. At the very least, degree of interest has a major effect on fineness of classification. So, for instance, the various closely related Rosaceous fruits (apple, pear, quince, medlar) would surely have been assigned to the same genus but for their economic salience (Walters, 1961).

All these possibilities arise from the fact that nature appears to have underdetermined the taxonomy of her products, an outcome that is easy to understand when one reflects on the process, evolution, that we take to have generated those products. However, it is important to stress that the claim is not that there are no natural divisions to be found between kinds or organisms. Rather, there are too many. We have to choose which to focus on, and such choice will inevitably and appropriately be constrained by the theoretical ends that our taxonomies are designed to serve. In this respect the situation is parallel to the social case. There are countless divisions we could emphasise between humans, and many have actually been emphasised. Which are emphasised, investigated, and perhaps thereby deepened, will depend on our interests and goals. These interests drive the production—or construction—of the great range of biological categories we distinguish and in turn make possible the various kinds of knowledge that different groups of us develop about the biological world. This range of categories, however, represents only an infinitescimal fraction of the distinctions that could, in principle, be made.

Genes

The project of identifying genes within the organismal genome is a very different one from the project of identifying and classifying organisms. The best point of entry to the former is historical. Whereas organisms have been classified probably since the dawn of language, genes have been with us for about a century. The word 'gene' is generally attributed to Willhelm Johannsen, in 1909. The basic idea for which it has most generally come to stand is that of particulate units of inheritance, and is standardly traced to the experiments of Gregor Mendel, an Austrian monk, in the 1860s. Mendel's famous experiments on peas disclosed numerical ratios in crosses between strains with different characteristics that suggested the existence of factors responsible for the characteristics that were inherited intact, without blending or dilution, to subsequent generations. These factors were what later came to be called genes, and were the subject of an intensive research programme in the first half of the nineteenth century, most famously the experiments on the fruitfly, Drosophila melanogaster, by Thomas Hunt Morgan, Hermann Muller and others. Genes were identified in this period entirely by the observable trait to which they gave rise. The theory developed that these units of inheritance were arranged along chromosomes and measures of the frequency with which particular traits were inherited together enabled inferences to be drawn about the order of this arrangement. The first genetic map, including half a dozen genes for eye colour, wing shape, and suchlike, was published by Alfred Sturtevant in 1913.

There was a natural hope that the physical basis of these particulate causes of inheritance would be found, and with the famous discovery of the structure of DNA in 1953, it was widely supposed that just this had been achieved. Whereas work in the first half of the century had involved inference down from properties of the phenotype to properties and relations of genes, it now became possible to work from the bottom up, attempting to move from chemically characterised bits of DNA to the phenotypic properties that they were held to produce. Unfortunately, however, these top down and bottom up projects have failed to meet in the middle. Long before 1953 and its sequel, it was well known that the relation between genes and phenotypes was generally a complex one, and that genes interacted with one another to produce varying effects. One gene might be correlated with a wide variety of phenotypic traits and, on the other hand, most traits required a variety of genes for their manifestation. However as the mode of action of DNA became clear, namely as providing a template for the production of protein sequences, it seemed possible that genes could be unequivocally identified in terms of the protein sequence the production of which they directed. But even this hope has proved to be far too optimistic.

A first point is that very little of the genome has turned out to be composed of even prima facie candidates for being genes. Most of the genome appears to code for nothing and has even thought to be lacking any function at all (though it is reasonable to suspect that this may merely reflect presuppositions about the kind of function being sought). Of the parts that are known to be functional, a large proportion does not code for protein production, but serves a variety of regulatory and other functions. But more importantly, even those parts of the genome that do code for proteins, typically bear no simple relation to the products they are involved in constructing. Coding sequences may be read in different ways, depending on where their transcription begins, and may be part of overlapping transcribed sections. Some sequence may even be read in both directions. Hence there may be several possible immediate products form a particular bit of sequence. I say 'immediate product' because the RNA which is first transcribed from a DNA sequence is only the beginning of an often complex sequence of interactions that will finally produce one or several functional proteins. The RNA may be edited, cut, or spliced onto other fragments before it is translated into amino acid sequence which, in turn may undergo similar modifications before final functional proteins are produced. (For more detailed exposition of these complexities and their philosophical significance see Moss [2003] and several of the essays in Buerton [2000].)

The processes leading from DNA to phenotype, then, are extraordinarily complex from the very start. To expect in general that identifiable bits of the genome will have privileged relations to particular traits of the phenotype, given that they do not typically even have unique relations to particular functional proteins, would be hopelessly unrealistic. The notion of the genome as composed of a series of genes 'for' particular phenotypic traits has gone the way of phlogiston.

Given this situation, how are we to understand the apparent successes of Mendelian genetics and the continuing relevance of something similar in medical genetics? Actually, one answer to this question is relatively straightforward. Although it makes no sense to identify bits of the genome with bits of the phenotype, changes to the genome will often have quite predictable effects on the cascade of developmental processes, and lead to predictable changes in developmental outcome. Most often this involves a dysfunctional effect on a protein deriving in part from the altered protein. A classic example is what is often treated as a paradigm of human Mendelian genetics, blue and brown eyes. Blue eyes result not from a blue pigment, but from the absence of brown pigment. Various mutations may result in the failure to produce this pigment and will result in blue eyes (ignoring, for the sake of simplicity, various other genes that affect eye colour). Since in high latitudes there is no significant loss of fitness to blue eyes, such defects have tended to accumulate in high latitude populations. Thus there is no gene for blue eyes, and not even a specific localised gene for brown eyes. Nonetheless, the mutations that disrupt the pigment producing processes will exhibit classical Mendelian patterns of inheritance. For example, if neither parent has the capacity to produce brown eye pigment they will reliably produce blue-eyed children. Essentially the same story can be told for most or all cases of interest to medical genetics. What most typically exhibits Mendelian inheritance patterns is a harmful mutation or one of a set of harmful mutations. It would be curious to refer to such a mutation as a gene for a disease, and just wrong to refer to the undamaged sequence as a gene for the absence of the disease. Familiar Mendelian phenomena outside the realm of pathology—double-jointedness, and suchlike—are simply alterations in the genome with functionally harmless effects on the developmental process leading to the phenotype.

Where does this leave the concept of the gene, and the question to what extent this concept is socially constructed? One way of telling the story would be to trace the divergence of two histories of genetics. On the one hand there is a popular development of genetics, which has led to the central cultural role of genes as naturalising inheritance and, perhaps more importantly, grounding various accounts of the inflexibility of developmental outcomes. On the other hand there is the story I have briefly summarised, leading from genetics to genomics, one plausible conclusion from which would be that genes have turned out not to exist at all. In the process of discovering and describing the genome we have failed to find any place within it for the genes that originally led us to it.

A natural reflection on the relation of these two stories would be to draw a strongly anti-constructivist moral. While society has embraced the gene concept and reconfigured earlier concepts of inheritance in a new naturalistic way by appeal to the gene concept, science has gradually deconstructed the very same concept. Of course, scientists have constructed contemporary genomics in the banal sense noted at the beginning of this essay, but the direction this construction has taken has been substantially driven by quite unexpected findings about the processes investigated. Science ultimately corrects the errors that derive from social influence. This thought should, however, be qualified by the reflection that the situation described is at a provisional and very possibly unstable stage. The insights into cellular function that I have briefly described are very recent and their effects both on science and on public reception of science remain to be seen. Science has provided powerful resources for destabilising social understandings of inheritance, but we should not assume that it will quickly or easily do so. Scientific undermining of essentialist views of species have been available for much longer than have genomically based critiques of traditional genetics, but it is doubtful whether essentialism has declined much outside very narrow and specialised discourses.

But there are also rather different ways of thinking of the story. One might think of the molecular genetics leading from 1953 to contemporary molecular biology as a spin-off from, rather than a continuation of, genetics. In favour of this view it might be noted that Mendelian genetics has continued to this day with empirical inheritance studies, in theoretical population genetics, in medicine, and no doubt other areas. Consistency with molecular

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genomics requires that its central concept receive a somewhat more instrumental and less realistic treatment than might have been supposed, but it has also turned out that this is not a fatal flaw in the project. Why has genetics continued apparently so oblivious to the undermining of its foundations? Presumably because its concepts, genes for biological properties that matter to us, have been constructed to serve a huge range of our interests in things biological. Contemporary genomics may show us limitations to its utility in addressing these interests, but in the absence of some more useful substitute we are likely to persevere with classical genetics as the best thing going.

Does contemporary genomics provide a substitute for classical genetics? Evidently not, precisely because it refuses to offer concepts connected closely with the phenotypic properties we care about. Of course genomics has notoriously been advertised as exactly a successor to genetics, but a more powerful one that will lead us to cures for the diseases classical genetics traces, and enable us to produce the organisms, and perhaps the babies, we have sought much more slowly with traditional breeding and eugenics.

But the truth is that genomics has so far done little to realise these goals, and perhaps is not well equipped to do so. It is (though this is not something that anyone will want to advertise in the present utilitarian and short-termist climate) currently very close to a project of pure inquiry. The classificatory division of the genome within genomics proper, therefore, is one driven very much by theoretical considerations, and is little effected by social factors in the interesting sense of 'social'. If genomics eventually gives us a good understanding of development, then we might expect to derive real abilities to control developmental outcomes, human and otherwise. But given the demonstrable complexity of development and of its joint dependence on internal and environmental factors, the task is a daunting one. It is entirely possible that traditional genetics methods will remain more effective for traditionally understood goals for the foreseeable future. So whether the story will develop as a demonstration of how nature can eventually dictate the shape of a science, rather, as an illustration of a strongly constructivist thesis, remains to be seen.

It may perhaps be good to state explicitly that the historical narratives I have offered are grossly oversimplified, rationally reconstructed even, with the goal of presenting some extreme positions on the issue of constructivism. A more realistic and nuanced history of how these understandings were gained would unquestionably emphasise much more of the contingency of the process and of the personal interests and goals that motivated the contributors to it. As I have indicated, I take the reality of science to be a variable interaction between social construction for human goals, and a partly recalcitrant nature. If my narrative suggests, for instance, that genes are social constructs whereas genomes are real things, this should only indicate the caricatured nature of the historical narrative.

Conclusion

I have suggested in this article that social constructivism is in some ways a fairly banal doctrine, and that the controversy that has surrounded it derives from further claims that are wrongly alleged to follow from it. Indeed the reminder that science is a social process, something done by human beings for a variety of reasons is a thoroughly salutary one. Nonetheless we can accept this reminder, and embrace such reasonable consequences of it as the symmetry principle, without any threat to the possibility of adopting a robustly realist attitude to some parts of science.

The greatest danger of social constructivism, perhaps, is one that it shares with most sweeping isms about science, that it should harden into a general and restrictive set of claims about science in general. As I have argued extensively elsewhere, I can see no reason to suppose there is any true such set of claims. Science is an extraordinarily diverse set of activities. The ways these activities are shaped by social forces are diverse, and the plausibility of adopting a realist attitude to the claims made within these activities is highly variable.

I have tried to give a sense of this diversity by looking at one of the central ways that science

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is shaped, whether by nature or society, the decision on how to classify the objects within the domain of the science. Unsurprisingly, social forces dominate this process in cases of highly politically charged social categorisations, and internal scientific processes have a much greater role for the case of quite technical concepts in quite technical sciences. The terminology of genetics is a particularly fascinating case because it is at the same time a terminology that has been developed in a highly technical scientific domain, and one that has from the start been absorbed into highly contentious public discussions. As should be clear, I remain undecided about how best to conceive this general area of enquiry. Lying uneasily between these technical and political contexts, genetics/genomics is a paradigm field for exploration of the interaction between broadly social and technically scientific forces.*

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Bibliography

Barnes, Barry and David Bloor (1982). 'Relativism, Rationality, and the Sociology of Knowledge', in Rationality and Relativism, ed. M. Hollis and S. Lukes. (Cambridge, Mass.: MIT Press.)

Beurton, P, Falk, R., and Rheinberger, H.-J. (2000), The Concept of the Gene in Development and Evolution. (Cambridge, Cambridge University Press.)

Bloor, David (1976). Knowledge and Social Imagery. (Chicago: University of Chicago Press.)

Dupré, John (2003). Humans and Other Animals. (Oxford: Oxford University Press.)

Foucault, Michel (1977). Discipline and Punish: the Birth of the Prison, translated by Alan Sheridan (New York: Vintage Books.)

Hacking, Ian (1999). The Social Construction of What? (Cambridge, MA.: Harvard University Press.)

Kitcher, Philip (2001). Science, Truth, and Democracy. (New York: Oxford University Press.)

Latour, Bruno (1987). Science in Action. (Cambridge, MA.: Harvard University Press.)

Longino, Helen (1990). Science as Social Knowledge: Values and Objectivity in Scientific Inquiry. (Princeton: Princeton University Press.)

Longino, Helen (2002). The Fate of Knowledge. (Princeton: Princeton University Press.)

Moss, L. (2003), What Genes Can't Do. (Cambridge, MA: Bradford Books/MIT Press.)

Solomon, Miriam (2001). Social Empiricism. (Cambridge, Mass.: MIT Press.)

Van Valen, Leigh (1976). 'Ecological Species, Multispecies, Oaks', Taxon, 25: 233-9.

Walters, S. M. (1961). 'The Shaping of Angiosperm Taxonomy', New Phytologist, 60: 74-84.

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