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Disorder

(Desorden)

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ABSTRACT: This paper begins with some brief intellectual autobiography, recalling my first engagement with philosophy of biology. The substantive part of the paper then focuses on the plurality of possible classifications central to the theses of scientific disunity and metaphysical disorder developed in my early career. After discussing this in terms of biological classification, and introducing the reasons for thinking of classifications as typically value-laden, I discuss two sets of human classifications bearing on normatively vital questions, those around sex and gender and those involved in the distinctions between human races.

Palabras clave
Clasificación
Desunión de la ciencia
Desorden metafísico
Valores
Sexo
Género
Raza

RESUMEN: Este artículo comienza con una breve autobiografía intelectual, en la que se rememora mi primer contacto con la filosofía de la biología. La parte sustantiva del artículo se centra en la pluralidad de posibles clasificaciones central en las tesis de desunión científica y desorden metafísico desarrolladas en los comienzos de mi carrera. Después de discutir estas tesis en términos de clasificaciones científicas, y de introducir las razones para pensar en las clasificaciones como típicamente cargadas de valor, discuto dos tipos de clasificaciones humanas relacionadas con cuestiones de gran importancia normativa, las concernientes al sexo y el género y las involucradas en distinciones entre razas humanas.

1. Introduction

The organisers of the lectures on which these papers (this and Dupré 2025) are based asked that I present some reflections on my philosophical career. I shall take this literally and begin with a little intellectual autobiography.

Like so much in my life —and I think life generally— my becoming a philosopher of biology was a highly contingent matter. After interrupting my undergraduate studies with a brief and perhaps misguided idea that I would become a mu-

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sician, I returned to Oxford to complete my studies with a firm notion that I wanted to pursue a career in philosophy. This meant that I needed to apply to a PhD programme. The problem was that in the UK one is required to say, in such an application, what the subject of one's PhD research will be; and I had no particular idea what I wanted to work on.

I recall discussing this with one of my tutors, the late Gordon Baker, and he suggested a number of topics that he thought were interesting and topical. One of these was the philosophy of biology. I knew no biology at all. Indeed my schooling included just one term of biology provided, ironically as it later struck me, by a classical German morphologist. My only recollections of this experience were the class uncomprehendingly repeating morphological liturgies, such as stem, leaf, flower or sepal, petal, stamen, carpel and, rather unpleasantly, a practical session which began with the removal of the skin from a frog's leg, an operation referred to as "debagging a frog". On the other hand, I did have a longstanding interest in classifying organisms, in my early youth breeds of cows and species of beetles, and at that later time wildflowers. As this resonated more than any other suggested topic, I made the fateful decision to take up this branch of philosophy.

Further research followed, in which it appeared that there were only three prominent living exponents of the field: Michael Ruse, and David Hull, ambitious young philosophers working in America, and (much older) Joseph Henry Woodger, a British biologist and philosopher, then often reviled as a card-carrying positivist.[1] This at least made it easy to catch up with the field. Reading Ruse and Hull (and Woodger's attempt to axiomatize evolutionary biology, surely the low point of an exceptionally interesting and productive career) it became plain that the big question in the field was whether biology was reducible to physics, perhaps via biochemistry and one or two other intermediate stops.

So I devoted myself to this question, one that now I must confess seems extraordinarily pointless. Biophysics is an interesting but specialised area of biology, but apart from that, and some reflections on the relation between the mass of an animal and the likely effects of jumping off a tall building, physics and biology have only a modest amount to say to one another.[2] The question that really drove this debate was strictly one of metaphysics rather than philosophy of science. Was there some sense in which the smallest elements of the universe were all there were? If so, as many philosophers still seem to believe, ultimate causality could only reside in these fundamental entities, and the subjects of a so-called "special science" such as biology, must ultimately be fictitious and causally inert. Perhaps this is an important metaphysical question, but it is one that is almost wholly independent of any details of biological science.

Nonetheless, I spent a great many hours reflecting on whether biology might indeed be reduced to physics. Perhaps genetics provided a link between chemistry and biology that might lead to the desired reduction? Or perhaps advances in chemistry and physics would eventually enable us to subsume biology under their growing domain? Although the answers to these questions were, at least with hindsight, quite evidently negative, reflection on why this was so led me to learn some biology, which no doubt was highly valuable in my later philosophical ventures.

2. *Classification, natural kinds, and promiscuous realism*

Somewhat to my surprise, my most productive pathway into philosophy of biology came from my existing, not especially intellectual, interest in classification, especially of plants. At that time, probably the liveliest topic of discussion in philosophy generally was the theory of direct reference promoted by Saul Kripke and Hilary Putnam. Putnam's (1970, 1975) application of this in the philosophy of science seemed finally to provide a solution to the vexing problem of meaning change with theory change. Then standard Fregean theories of language related meaning to descriptions of the entities referred to. But then, if theories change, so did the meanings of the words referring to their subject matters.

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1 There were, of course, other philosophers interested in biology. But long before the internet, this was as much as I was immediately able to discover of the field.

2 No doubt physics is relevant to chemistry and chemistry to biology. But relevance is not a transitive relation and in this case the inference that physics is relevant to biology seems questionable.

Given the vast changes that had occurred in theories of, say, electrons, since they were first postulated, it seemed that the story of these changes lacked any common subject matter. Putnam proposed to solve this problem by suggesting a direct interaction between the word and the referent when a name was introduced.

So when George Johnstone Stoney proposed the name "electron" in 1891 (O'Hara 1975), for a particle that had been extensively studied by scientists for some decades, there was a fair consensus about where the entities cold be found, what could be done with them, and so on. A link was established between a word and a kind of entity. As subsequent research produced increasingly divergent accounts of the nature of that entity, this link maintained the coherence of this developing narrative. Of course, the story only works if there is a kind of entity successfully "dubbed". And the dubbing is only successful in so far as other scientists accept that the entity exists and agree to use that name.

One thing that this notorious episode in philosophy brought into focus for me was the problem of a philosophy of science so solidly focused on physics. A whole host of problems around realism, to which the question of theory change is central, are really problems for physics. The theory of evolution transformed biology, but no one worried much about whether creationists and evolutionists were talking about the same animals. A monkey is a monkey, whether created or evolved. And no one apart from globally radical sceptics supposes that monkeys might not exist —though perhaps they may not for much longer.

Nonetheless, Putnam was happy to apply the theory immediately to biology. What does the word "lemon" mean? It is all and only members of the biological kind exemplified by the lemon first dubbed "lemon" (Putnam 1970). The assumption was that biological kinds, like atoms or subatomic particles, fall into discrete natural kinds exactly one of which every biological organism belonged to. Putnam assumed that these were demarcated by an essential property, the genetic structure of the lemon. But here my experience of biological classification sounded alarm bells. Plants, particularly, are a diverse lot. Members of a species differ greatly and are often almost impossible to distinguish from close relatives. One reason for this is that they hybridise a lot. There is no more reason to suppose that there is an unequivocal genetic basis for sorting them into discrete kinds than that there is a morphological basis for this.

Putnam's chosen example of a lemon illustrates this well. Greengrocers may be very good at distinguishing the various commercially grown citrus varieties —31, according to one specialist website— and since many of these are clonal, they may even be genetically distinct. But the biological origin of these fruits remains complex and even obscure. Simplifying slightly, experts currently trace a web of relations between four ancestral species, (the papeda (*Citrus micrantha*), the citron (*Citrus medica*), the mandarin orange (*Citrus reticulata*), and the pomelo, (*Citrus maxima*), and the various current varieties of lemon (Curk *et al.* 2016).

I had the great pleasure, when living in California, of owning a Meyer Lemon tree, a truly wonderful plant, which produces delicious lemons all the year round. Or maybe not lemons? The Meyer lemon is a cross between a citron and a hybrid between a mandarin and a pummelo.

In short, and as I argued in my first published paper (Dupré 1981) if there are natural kinds in biology, they certainly are not the kinds that Putnam needs for his account of the meaning of kind terms. Putnam wanted his dubbing story to provide an account of terms in ordinary language, and as the lemon example and many others I marshalled in that paper show, this it cannot do. In ordinary language a Meyer lemon is a lemon; but not all lemons share the same genetic essence. In fact, the situation is more serious than this. If God had created a world with distinct species they might have had distinct essences until citrus breeders and others messed things up a bit. But if, as I believe, evolution did the job, the project of assigning essences even to the biological kinds of science just won't work.

Take a tortoise and an oak tree. Trace the evolution of the tortoise back to its last common ancestor with an oak tree, something all evolutionists believe to be possible. Now go forward along the lineage that leads to the oak. We now have a series of species leading from lemon to oak in a fairly smooth series of changes. There are no (or anyhow few) sudden jumps at which a species with one essence is replaced by a new species with another. It is clear that an evolved biological world does not contain essences that separate every kind of organism from every other. And indeed, we now know that transfer of genes between species is not just something done by God-playing plant breeders, but happens all the time in nature. The biological world is much messier than Putnam assumed.

One thing that this excursion into taxonomy led me to see, then, was that popular theories of natural kinds with Lockean real essences dividing the things of the world into perfect classifications, made little sense in biology. But this opened the door to a surely more important insight: classification is always relative to a purpose. In my earlier paper I made this point mainly in relation to the differences between ordinary language and scientific language. Chefs, carpenters, gardeners and many others have reasons to distinguish kinds of organisms, and their interest need not coincide with those of scientists. Thus, for instance, for a carpenter cedar is a kind of wood with distinctive properties, notably its moth-repellence and rot-resistance, the former property making it popular for lining chests and closets. In Europe, this wood is generally from the genus *Cedrus*, a kind of pine. In the US, the same function is often served by the Eastern red cedar, *Juniperus virginiana,* a species of Juniper. The Western red cedar, *Thuja plicata*, a species of cypress, is, like its other namesakes, strongly rot resistant, and particularly suitable for many outdoor uses (Dupré 1981).

A number of philosophers have objected to this line of argument, that natural kinds were always intended as the kinds of science, and little, apart from Putnam's account of ordinary language terms, follows from the failure of the mundane aims of ordinary folk to sort the world in alignment with the kinds of science. Science has ultimately just one goal, discovering the truth. As it happens, however, and as has been increasingly apparent in investigations by philosophers of biology over the last forty years, the more technical scientific concept of a species presents similar difficulties. Current estimates suggest there are over two dozen species concept circulating among biologists, some based purely on evolutionary history, some on ecology, some on physiology, some on genetics. There is no reason to suppose that these will always coincide and, while some will no doubt prove more useful than others, no reason to think that one will serve all legitimate biological purposes. The solution to the so-called species problem, the problem of how to define the species, is that there is no solution to the species problem. Many different definitions serve different purposes (ecology and phylogeny, for instance) and are suitable for different domains of life (see, e.g., Dupré 1993, ch. 2).

I named the general picture to which these observations have led me "promiscuous realism". There are many ways of dividing the entities in the world into kinds (promiscuity), and these kinds may often overlap one another. But there is no reason to deny that these kinds —even, I argue, the kinds of ordinary language— are perfectly real. They reflect properties, often clusters of properties, that truly pertain to the objects to which they are applied, and identifying something as belonging to such a kind is often valuable for predicting how it will behave and the purposes, practical or intellectual, that it may serve.

Once essentialism is firmly dismissed, such a position is immediately appealing. If there is no privileged set of properties, the essential properties, that determine how things should be classified, why assume that any mode of classification will serve all the purposes for which we classify. Think, again, of biological species. The most ancient reason for classifying is simply to enable the recording and storing of information. If I see one biological entity devouring another, it is helpful for all sorts of reasons to be able to say that I saw, for instance, an orca eating a seal. There is a fair chance that this is a characteristic activity of things of that kind, the orcas; I should keep my pet seal away from orcas. This motive does point to the value of a general taxonomic system that aims to assign every organism to exactly one species. But the motivation for such a system is entirely pragmatic; it cannot hope to reflect some real structure of the living world.

3. Against reductionism

In this early work, reflection on the failure of essentialism led me also in a different direction, back to my concern with reductionism. Although the consensus was beginning to crack in the 1970s (Fodor 1974), even in the 1980s physicalist reductionism was still a widely assumed position. This was the idea that all science was really physics, that ultimately the findings of others would either be found in adequate or be interpreted in the language of physics. How was this interpretation imagined? The concepts of the "higher" level science, that is, the science that dealt with more complex objects, must be identified with arrangements of the objects of fundamental physics and the behaviour of the more complex objects deduced from the laws that apply to these physical parts. This seemed plausible enough for the reduction of chemistry to physics —atoms were understood as complexes of more fundamental particles, and molecules as arrangements of atoms— and the reduction of biology to chemistry seemed at least to have been begun by work in genetics and other parts of molecular biology.

But note that the proper arrangement of components looks exactly like the essential property of the higher level entity. If the properties of water, conceived as a mass of H_2O molecules, is to be inferred from the physical properties of its parts, then every H_2O molecule must have precisely the right structure, its essential property. So threats to essentialism looked likely to provide problems also for reductionism.

Perhaps at higher levels, such as the biology of organisms, eliminativism is the way to go. These sciences cannot be strictly true because their kinds are too heterogeneous to provide proper subjects for scientific laws. If organismic biology is irreducible so much the worse for organismic biology. But essentialism was beginning to look problematic even at much lower levels. Consider again water. The pure water made up only of identical H_2O molecules was nowhere to be found in the real world. Even allowing that we are not interested in sea water, pond water or even tap water, does a proportion of heavy water, deuterium oxide, disqualify a sample from being strictly water. But even samples of pure H_2O are problematic. It is now clear that hydrogen bonds between water molecules can form much larger transient structures within liquid water, which are important in explaining the highly anomalous properties of the fluid, and that also have important implications for biochemistry. These transient structures of water are crucial, for instance, in explaining protein dynamics and protein-protein interactions (Raschke 2006).

Considerations of this kind have convinced most scientists and many philosophers that reductionism as a practical exercise is wholly infeasible. Detail emerges in complex systems that could never have been foreseen from the perspective of so-called fundamental physics. But philosophers, at least, have generally held on to the idea that "in principle", something often spelled out in terms of what would be possible for the infinite mind of God, everything might be a deductive consequence of the laws of physics and the arrangements of physical parts. This may be understood most simply merely as the idea that fundamental physics is complete. The movements and behaviour of every fundamental physical particle is fully determined, it is said, by the arrangement of the physical particles and the laws that govern their behaviour. It may be practically impossible to explain or predict the behaviour of an animal from the laws of physics. But if the animal is composed entirely of physical particles, and the position of all of these is determined by physical laws, it seems that at least the position of the animal and any changes in that position must be a necessary consequence of the laws of physics. This claim has come to be expressed by the idea that higher level changes "supervene" on the underlying physics. Biological facts or mental facts (the main home of this thesis) depend wholly on underlying physical facts (Kim 1984).

Supervenience claims remain, pretty much, orthodoxy among philosophers. For my present purposes, it is perhaps sufficient to note that supervenience represents precisely the recognition that actual reduction is impossible. But I do think that, separated from its empirical origins in reductionism, supervenience quickly becomes hard to defend. I'll mention just one reason. A first intuition about supervenience is that the properties of, for instance, an animal, supervene on the physical structure of the animal. But then we must observe that the behaviour of the animal will often depend on features of its environment, such as the presence of food, possible mates, or opportunities for play. So the supervenient base, the physical material on which the animal's behaviour depends, must include much beyond the animal itself. And these elements of the environment will themselves often have a supervenient base that stretches beyond themselves. This suspicion has been reinforced in recent years by the thesis of externalism in philosophy of mind, that the mind extends beyond the confines of the minded individual. My memory, for instance, is now based as much in my computer and my phone as in my head. It begins to look as if there might be no limit to how much of the world may need to be included to exhaust the parts whose physical properties may be relevant to our target of interest. And this undermines any hope that there could be anything approximating empirical verification of supervenience.

This leaves the defender of supervenience with nothing but the belief in the completeness of physics. And given the impossibility of finding empirical evidence for this belief, it looks increasingly like an unmotivated dogma. In reality, physical laws are discovered and tested in very specific and highly controlled laboratory circumstances. As Nancy Cartwright (1983) has famously argued, it is quite unclear how these laws apply, if at all, to the messy conditions of the real world. Why should we suppose that the laws that apply to an electron in a cloud chamber apply just as well to an electron embedded in the complex metabolic processes of an organism?

The best that can be said for the completeness of physics is that it is a purely speculative thesis, that belongs to abstract metaphysics more than to science. As a believer in the kind of metaphysics —naturalistic metaphysics— that is answerable to scientific findings about the world, I am highly sceptical of such a thesis. But at this point it may be more useful to move on to something much closer to the science, with a return to biology. One thing that historians and philosophers of biology have come to a considerable degree of consensus about is that biologists generally have little interest in the formulation of laws[3]; their practice revolves rather about the construction of models. These models are often quite unique to a very specific phenomenon or problem.[4]

The realisation that biology deals in models, or families of models, rather than laws is a crucial one for a simple reason. Whereas laws are considered to be true, even universally true, models are generally seen not even to aim at exact truth. They are idealisations and/or abstractions, perhaps even fictions. The point is not to say exactly how the world is but to identify aspects of the complexity of the world that have a strong, perhaps even decisive, effect on what happens. But it is generally understood that other factors may, in a specific case, intervene to prevent the expected outcome. And it may even be that nothing particular intervenes but the outcome fails to occur; nature is increasingly recognised to be, to some variable extent, irreducibly probabilistic. In the actual science, then, in so far as this consists of models rather than laws, we do not even have candidates for the potential reduction to (the laws of) physics.

4. Values in science

I want now to go back to kinds, and some important ways in which the abandonment of the essentialist view of kinds really matters a great deal. I have argued that there are many ways of classifying the world into kinds and the correct, or best, way of doing so depends on the purposes for which our classification is intended. There is no single, objectively best, classification that science can provide for us. But this opens up a whole range of normative questions about science. What are the goals underlying specific classificatory schemes? And may there be social, political or ethical values implicit in the way we do science? After drawing together my arguments on essentialism and reductionism in my 1993 book, *The Disorder of Things*, these questions were my central philosophical concerns for the next decade or so.

Let me begin with the general question of values in science. This has taken many fascinating directions in recent philosophy of science. We have an important body of work on inductive risk, drawing attention to the necessity of weighing up the costs of getting our scientific conclusions right or wrong (Douglas 2000). Finding out experimentally whether a chain reaction might propagate through the oceans is a bad line of research to pursue, even if the undesirable possible outcome, the total destruction of our planet, is judged as having a very low probability. More difficult questions of this sort arise in social contexts. If, for example, we feel the need to explore the possibility that certain groups of people are less intelligent than others, we should consider carefully the costs of getting a false positive answer.

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[3] Whether there might nonetheless *be* laws is a trickier question, since it depends what you mean by "law". John Beatty (1995) robustly rejects biological laws; Sandra Mitchell (1997) dilutes the idea of a law sufficiently to make biological laws possible. Almost everyone agrees that the universal, necessary, laws of nature once assumed to be central to all science, play no important role in biology.

[4] There is a large philosophical literature on models. For a good overview see, e.g., Downes 2021.

Another major research program in this direction has been the concern with epistemic justice, mainly derived from Miranda Fricker's (2007) influential book of that name. This concerns the unjust exclusion of many groups of people from the production and application of knowledge.

I shall return to epistemic justice in a moment. But there are more immediate applications of the plurality of possible classifications to values in science. Consider some fundamental concepts in macroeconomics, for example inflation. The casual observer might imagine that the change in price level was just an objective fact about an economy. But as economists are well aware, it is nothing of the sort. Not all prices go up or down at the same rate, so the inflation rate will be measured differently according to what so-called "basket of goods" we look at. Could we not look at all of them, weighted according to how much is spent on them? Something like this could have its uses, but it would not solve the problem. Not everyone, probably no one, spends exactly the average amount on every kind of good. And these differences are systematic in certain ways. Luxury yachts and private jets have a disproportionate effect on such a measure, disproportionate in that their high cost multiplies their effect but also makes their price irrelevant to all but a tiny minority of consumers. In general, however inflation is measured for a whole economy, it will be different for different consumers, or categories of consumers, depending on what exactly they buy[5].

If the price of such luxury items is falling, but the price of essentials such as food and housing is rising, poorer people may experience a higher inflation rate than the general average for a nation. Their wages, pension or social security benefits, when indexed to this average rate, will then leave them poorer. This is a clear case of epistemic injustice: the results of scientific measurement, in which the poor typically have little voice, turn out to disadvantage them.

I don't propose to speculate here on whether there are malign motives at work. The general point is that there is no way of measuring inflation that does not involve making decisions that affect different people differently. Values —whose goods matter most— are impossible to exclude from the measure. Inflation, in short, is not a natural kind. Similarly with a global measure such as gross domestic product, or GDP. This is generally supposed to measure the productive activity of an economy. This is hard to measure. The normal procedure is to measure such activity in terms of the money paid for the results of this activity. Since GDP is a policy target of most governments, how different activities will affect this quantity affects the ways that governments encourage and reward different activities. But suppose instead of measuring exchange value, we tried to measure GDP in terms of use value. One might imagine that lower use value might be attached to luxury yachts, designer clothes and perhaps nuclear weapons and cigarettes, than to food and housing. Even true believers in the all-knowing powers of markets should know that well-known market distortions caused by widespread monopoly power limit the benign effects of this supposed omniscience[6].

More important still is a problem with GDP eloquently explained decades ago by Marilyn Waring (1988). Many kinds of productive work happen outside the monetary economy altogether. Notable among these is domestic work such as childcare, cooking, small scale food production and care of the aged. In most countries this work, invisible to GDP, is both unpaid and overwhelmingly performed by women. The standard procedures for measuring GDP are, therefore, a massive source of epistemic justice to a vast proportion of the human population. And as Waring explained in some detail, this has real and massive effects to the disadvantage not only of women, but to children and often even to men. It would, admittedly, not be easy to measure GDP in terms of use value. But the attempt to do so, making it impossible to conceal the highly political, value-laden nature of the concept, would surely be salutary. Note finally, that I am not saying that there is anything epistemically wrong with measuring GDP in terms of exchange value. It is part of one scheme with which we can provide conceptual order on the chaotic array of economic activity. But we should recognise that it is not the only such scheme; that it carries with it particular goals and particular values; and that it has real consequences on the way we all live.

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5 The relevance of this example to values in science is discussed further in Dupré 2007.

6 I do not mean to imply that they actually do. Recent leaders of the British Conservative party, most spectacularly and disastrously the ephemeral Prime Minister, Liz Truss, provide some obvious counter-examples.

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5. Sex and gender

Another classificatory question has become massively contentious in recent political debate, the definitions of sex and gender. It is hardly within the scope of this paper to resolve these questions here, and I shall not try. But at least the beginning of wisdom is to recognise the purpose relativity of classification. If we are interested in general questions in evolutionary theory, for example, there is no question that sex, generally defined as production of large or small gametes, eggs and sperm, is an unavoidable classification. We should note first, however, that not all organisms have sexes. Second, and more important, as Paul Griffiths (2021) has argued in recent work, sex is not an essential property of any organism understood, as it should be, as an entire life cycle. At many stages of their life, organisms do not produce gametes, large or small. Some organisms, for example the Eastern Blue Groper (*Achoerodus viridis*), produce small gametes at an early stage in their life and large gametes later on. It also changes from brown to blue when it comes to satisfy the biological definition of being male. Ocellaris clownfish (*Amphiprion ocellaris),* begins life as a male. When the female in a community dies, the highest ranking male transforms into a female. Sex is, nonetheless, in these and similar cases, an important biological property that organism possess at stages of their life cycles. Griffiths (2021) argues that an immature organism that has yet to develop the ability to produce gametes is, in this sense, neither male nor female.

Humans are not classified by sex because of our overwhelming interest in evolution or gamete production. There are many institutional functions that are served by this classification, but we may very reasonably inquire which of these are important and what the appropriate criteria are for applying them. That the latter question is only contingently related to the biological distinction just discussed is clear from the fact that a substantial proportion of humans, for various reasons, do not at any particular time have any capacity to produce gametes.

Of course, most people think of sexual difference in terms of genital morphology and secondary sexual characteristics such as beards and breasts, rather than gamete size. Here it is important to be aware that sexually dimorphic development is a complex developmental process with a huge array of different outcomes, loosely clustered around what are thought of as the paradigmatically male and female (Dupré 2017). Within and beyond these clusters there is great diversity and no universal correlation between the various genetic, hormonal and physiological features that constitute the loose clusters. When it comes to the social and behavioural characteristics more commonly thought of as constituting gender, these are enormously diverse. The degree of clustering around norms of employment, domestic work, styles of dress and so on is now seen to be overwhelmingly due to cultural norms, and in a society that values individual freedom of choice, there is no possible reason to object to the wishes of the many gay, non-binary or transgendered people who prefer to diverge from these norms. I shall say a bit more about this in my second lecture.

6. Race

My final example will also be a controversial one, though a declining controversy from a biological perspective, the question of racial classifications. No biologist now thinks that the human species has well distinguished subspecies, and even the biological term "race" which has no formal definition in taxonomy, has no clear application to *Homo sapiens*. The species is genetically unusually homogeneous, and reproductively panmictic, that is to say there are no biological barriers to interbreeding between any identifiable subpopulations.

This is surprising to many people, as they are often brought up to classify most people immediately as belonging to specific racial groups, and experience this as easy to do[7]. What are we doing when we classify people, on the basis of a few

[7] There is a substantial and growing literature on the philosophy of race. For a survey and extensive further citations, see James and Burgos (2024).

superficial, but to us salient, physiological traits? There is a long tradition of racial science, almost all of which has been thoroughly discredited. But the conviction of the reality of race has been reinforced —to many people if not biologists— by the recent announcement that races can reliably be distinguished by genetic tests[8]. What are these tests testing, if not distinct races?

Why do people from equatorial climates generally have dark skin? This is quickly answered. Melanin in the skin, the chemical which causes dark coloration, protects from sun damage. Why do people from cooler climates generally have lighter skin? Again there is a readily available answer. Humans use sunlight to make vitamin D, and melanic skin is less able to use sunlight. Strong climate-based selection has rapidly optimised skin colour as human populations moved from hot to cold climates and vice versa (Jablonski and Chaplin 2000). Such adaptation to local conditions is found throughout life. Often, especially in plants, such local adaptation is purely developmental. For example, the North American lake cress, *Rorippa aquatica*, produces entirely different leaf shapes according to whether or not it is submerged in water, and also in response to increasing temperature. Animals tend to be less developmentally plastic, but genetic and epigenetic switches can allow rapid evolution of environmentally sensitive traits, and this seems to be the case for human skin colour.

The genetic tests just mentioned do not, of course, merely confirm that people have genetic tendencies to more or less melanized skin, something that can be done without much specialized equipment. Rather they claim to sort people into distinguishable populations of some kind. The first thing to note about this research is that the programme used by Rosenberg and colleagues (2002) to analyze genetic data in this way, *structure*, does not tell us how many human populations there are. Rather, it is given a number, and on the basis of a measure of genetic similarity it provides the best clustering it can find into that number of groups. The genetic data it uses does not track functional genes for skin colour or hair texture, but what are believed to be random mutations on non-functional genes. Since these are (or so it is believed) not subject to selection, they accumulate over time, and thus measure the distance in time that groups of humans have been reproductively separated from one another. It is thus a way of exploring human migration patterns.

As the number fed into the programme increases, new such groups are added by splitting existing groups. N = 2, separates Africa and America; n = 3 separates Eurasia from America; n = 4 separates East Asia from Eurasia; and n = 5 separates Oceania from East Asia. This all fits quite well with traditional views of continentally based racial groups. N = 6, however, distinguishes the Kalash people of Northwest Pakistan, population about 4,000, and not an obvious candidate for a distinct race.

As I noted, this methodology measures migratory history rather than any significant differences between the groups distinguished. As the authors acknowledge, the vast majority of variation between humans exists within not between these populations. And anyhow, the genetic features used are specifically chosen not to mark functional differences. Nonetheless, it is striking that these markers do in fact still provide a fairly reliable criterion for deciding the continental origin of an individual. Why is this?

As I have mentioned, there are no systematic biological barriers to interbreeding between the groups of people distinguished by *structure*. The human species is highly migratory, as we all hear much too much about nowadays, and major migrations of the past can be traced through these methods. But given the possibility of interbreeding, it is perhaps surprising that these marks of ancestry have not been more thoroughly distributed through the population.

Some of my ancestors a mere 350 years ago, were Huguenots, who migrated en masse from France in 1685 to avoid religious persecution. But in the intervening dozen or so generations the amount of interbreeding with, in my case, German and English ancestors makes it unlikely that I could be genetically distinguished as a Huguenot. The ancestor who bequeathed me my family name was only one of a few thousand ancestors since that time, though perhaps a few others

8 The most famous such report, and the basis for discussion below, is Rosenberg *et al.* (2002).

left France with him. Some groups of people have been a lot less mobile and liable to dispersal than others, and distinguishing genetic markers will be proportionately more concentrated.

But probably most significant is something quite different. I said there are no biological barriers to interbreeding between human groups, but there are plenty of social barriers. Broadly speaking, these are what we call racism. I use this term here very broadly, not in a necessarily ethical sense. There are more or less problematic ways of retaining the identity of human groups by preferential inbreeding, all of which require making distinctions that are (broadly) racial. Nonetheless, without any such preferences, and given the level of human mobility that currently exists, these markers of ancestral geography would dissipate very rapidly. For this reason, I conclude that far from racism being a response to the perception of the objective fact of racial difference, racial differences are largely a consequence of racism. Race does not cause racism; racism causes race.

This leads me back to my main theme. Race turns out to be a wonderful illustration of the necessity of asking what a classificatory system is for. Historians have long explored the ways in which racialist ideas have been used to justify appalling exploitation of people from other parts of the world. Whatever the intentions of early racial classifiers, this seems to have been the main purpose that it served. And it is not hard to argue that this is the purpose that it still serves, though the issue is complicated by the argument that we now need racial classifications to identify the people to whom reparations are owed for past injustice. But even if we accept this last point (and, less probably, act on it), the ultimate goal should be the end of racial categories even if there are processes of reparation that will be needed before this goal can be properly obtained. Racial kinds are not interesting biological kinds, they are social kinds; and social kinds that have been used for almost exclusively malign ends.

7. *Conclusion*

It is time to summarise. The philosophical journey I have been sketching began with a question seemingly quite internal to science, even a quite technical question. What is the relation between the sciences of complex things and the sciences of the simpler parts of which they are composed? A commonly held view at the time my work began was the answer summarised as reductionism: ultimately we should explain the behaviour of anything in terms of the behaviour of its parts. This led me to the consideration of how we begin to impose epistemic order on things at any level of organisation, the theory of classification. The standard reductionist view was that there was an objective answer to this question, the scientific search for the natural kinds into which the world was organised. But a careful investigation of biological classification showed there were no such natural kinds. Kinds were only natural in so far as they best served particular goals of enquiry. Classifiers look to maximise homogeneity of their kinds not absolutely, which is impossible, but in respect of characteristics relevant to a particular kind of question. In biology, such questions might be evolutionary, ecological, physiological, genetic and so on, and there is no guarantee that these will coincide. And, for that matter, the interests of the gardener, the chef, the carpenter or the forester may also quite legitimately motivate their own distinctive modes of classification.

This observation demolished one central plank of the reductionist programme, since it showed that we did not have the natural kinds that would provide the links between sciences in the hierarchy of complexity. There is no unique answer to how we should define a human or a banyan tree in terms of its smaller components. But the realisation of the purpose relativity of enquiry points to something much larger that takes us beyond the internal understanding of science.

If classification, even scientific classification, is relative to human purposes then science itself must be shaped in fundamental ways by our interests. One influential idea that cannot survive this insight is the ideal of science as wholly value free. But if this is right, then we must also look at the ways that science influences human life, and does so in ways that depend on the values that were incorporated in the production of science. An obvious consequence of this is that science will tend to reinforce and validate the social and ethical ideas that have been fed into it from the outset. The deeper and more significant the social ideas involved, the stronger this reinforcement is likely to be, so that the science based on concepts —classificatory concepts— that most generally shape our lives, such as the science of sex and gender or the science of race, are most seriously in need of critical scrutiny. This, I am pleased to note, is something that has become an increasingly central part of the work of philosophers of science in the last few decades.

This talk has also illustrated a broad feature of my work that remains controversial within the philosophy of science, the deep interconnections between science and metaphysics. I started with an idea that has traditionally been considered a question central to metaphysics, whether the world is composed of natural kinds defined by essential properties, and used my answer to reach some broad conclusions about science. But this is not the metaphysics traditionally conceived of as an a priori activity that precedes science, but a metaphysics grounded in reflection on the findings of science; the conclusion about natural kinds and essences was based on exploration of scientific (and indeed extra-scientific) empirical knowledge. Just as science is partly grounded in human values and human values are further shaped by science, so with metaphysics. The metaphysics I am concerned with, sometimes called naturalistic metaphysics, is grounded in science, but such a metaphysics can also redirect and improve the way we do science.

In the next paper I turn to a more ambitious metaphysical thesis that has shaped the last two decades of my work, the claim that we live in a world not composed of discrete and sometimes interacting things, but of inextricably intertwined processes. Again, I shall hope to show how this thesis best provides coherence for the findings of the various sciences, and how at the same time it reshapes much of how we should do science, and how we should do science in ways better able to serve our human purposes. To quote the concluding sentence of my 1993 book, "like other human products, the only way [science] can ultimately be evaluated is in terms of whether it contributes to the thriving of the sentient beings in this universe". I hope that the ideas summarised in this paper and the next can be seen have been shaped by the hope of making a contribution, however small, to this goal.

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