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Chapter 7: The Invisible Hand of Science

7.1 A Naturalistic Perspective

So far, we have looked at a few different ways in which to talk about the relation between science and the rest of the world: the first spanned chapters 4 and 5 and looked at what happens if we suppose that nature makes the final state of science inevitable (chapter 4) or, on the other hand, completely fails to play an explanatory role (chapter 5). Then, in chapter 6, we looked at an attempt to create a vocabulary for talking about science without dividing up the world in natural and human elements with their corresponding features.

In this chapter, we will look at naturalistic accounts of how the world influences science, namely at invisible hands accounts, including most notably the evolutionary account of science provided by David Hull. We have already seen in previous chapters that it is conceivable for nature to play a causal role in the history of science even if there is no logic that completely and unequivocally determines how what is given to us by nature gets processed by science. A naturalistic account of the relation between science and the world may do justice to that observation, by foregoing reference to transcendental rationality, idealism or laws of history, and looking only at causal relations.

In doing this, naturalistic accounts may also succeed in closing the ontological gap that is created by the division between ‘what science studies’ and ‘what science is part of’: in the previous chapter, we by and large agreed with Latour that to artificially cut all the connections between ‘Nature’ and ‘Society’ belies the fact that these categories are the result of constructive work and artificially inflates the problem of the relation between science and the world, by suggesting that they are different *kinds* of things. We did not agree with Latour that by deconstructing the self-evidence of the boundary between nature and society the question about the relation between science and the world collapses entirely: we may still make a distinction between a natural phenomenon and what a scientist (or a discipline, or a culture) believes about that natural phenomenon, and we may ask the question how these two facts are related. The point is that in answering this question, we can assume that both facts are about objects in the world: both natural phenomena and scientific beliefs and practices are particular and concrete entities, which may have a causal relation to each other.

A naturalistic approach, then, may help in treating both science and the world as referring to entities that are in the same causal nexus. Hull calls his approach ‘naturalistic’ if “naturalism is the ‘view that theories come to be accepted (or not) through natural processes involving both individual judgment and social interaction’”.⁵⁰¹ The intended message seems to be that what scientists do does not take place in complete independence of the world that their work is about; that the judgment they exercise and the interaction they engage in do not need to cross a broad river, a deep ontological gap, but that they are simply among the actions that take place in the world. Saying that individual and social actions are part of

⁵⁰¹ Giere (1988, 7), as cited (though with another page number) by Hull (1988b, 3).

nature is not the same as saying that they are the proper domain of the natural sciences; it can also simply be to counter the intuition that they are ‘made of radically different stuff’, and thereby to weaken the ‘bifurcation of nature’ that we saw Latour oppose in the previous chapter. What makes an approach naturalistic is then the notion that the social is a subset of the ‘natural’ in a broad sense of that word; not something different from it, but not necessarily indistinguishable from the *rest* of the natural either.

In this chapter, we will zoom in first on invisible hand accounts, and then on an invisible hand account that is also an evolutionary approach: the account of scientific development formulated by Hull. Of course, there are naturalistic approaches that are not invisible hand accounts, and there are invisible hand accounts of scientific development that are not evolutionary accounts – we will meet some examples of the latter. Also, it is well conceivable that there are evolutionary accounts that do not meet the criteria of invisible hand accounts. However, both invisible hand accounts and evolutionary accounts have properties that make them interesting from the perspective of a philosophy of history of science, and their combination in Hull’s account is therefore especially interesting, as we will see.

7.2 Invisible Hand Accounts

7.2.1 The Promise of Invisible Hands

One type of approach that tries to link science to the world by processes that are ‘natural’ in the sense described above can be filed under the label of ‘invisible hand explanations’.

The promise of invisible hand explanations lies in the extent to which they might harmonize belief in the authority of scientific opinions about nature with an emphasis on the thoroughly social nature of science. One thing that SSK and similar approaches have been very good at is ‘unmasking’ scientific ideologies, and bringing the actors in science back to worldly proportions: it has shown time and again that the persons whose aggregate actions constitute science are not ascetics motivated purely by a desire to find out and preach the truth about nature – that, in fact, science is all too human, and that we had better approach it not as if it were something pure, but rather “as if it was produced by people” who were, among else, “struggling for credibility and authority.”⁵⁰² Though we should not underestimate the extent to which previous generations of scholars in science studies were capable of apprehending this – was not Mertonian sociology doing precisely this? – this attitude is a genuine improvement over approaches that had to see scientists and science as disinterested and unattached to society,⁵⁰³ if only because a sustainable place *in* society for people who are genuinely (as opposed to mythically) detached from society is hard to find.

And so, to state it bluntly: if what scientists are systematically striving for is not finding out the truth about nature but pursuing their social interests, then perhaps science is

⁵⁰² Title of Shapin (2010).

⁵⁰³ E.g. Dubos (1950).

not about finding out the truth about nature after all, but about pursuing the social interests of the groups that do science. This blunt conclusion, however, is also rather cynical, to such an extent that it renders inexplicable the success of science in dealing with nature. SSK can set out to explain *belief* in the success of science, including our own belief, as a result of social processes; but because it leaves nature out and orients scientists upon society rather than nature, it has no way of making sense of the possibility that science is ‘actually’ successful in its attempts to investigate nature. Again, as has been emphasized already in chapter 5, this does not imply unbelief in the existence of an outside world, or idealism; it means rather that we are never in a position to link science to this outside world.

What invisible hand mechanisms may be able to do, now, under conditions that we will address shortly, is to recognize and affirm the demythologization of the people and institutions that make up and carry science – to say that scientists are fully the social beings that other people are, with the same drive for money and status as all of us (or at least within reasonable distance to the average on the same bell curve) – but to move back into the picture a notion of dealing with the world and even of success in dealing with the world for science as a whole, by presenting these not as matters of teleology and design, but as the emergent results of social structures in science. As Petri Ylikoski, one (critical) commentator, has summarized the reasoning behind invisible hand approaches to understanding science:

one can say that scientists are *humans without a great secret of success* (that is to say without the Scientific Method). So we might have to get rid of some [of] our usual ideas about the nature of science. Are the ideas of objective knowledge and of the cognitive authority of science among these? The idea of the invisible hand is supposed to save us from throwing them away along with other things. It refers to a naturalistically acceptable process, in a way that a naturalistic philosopher of science can accept it.⁵⁰⁴

An invisible hand process can be defined as a case in which the actions of individuals lead to a stable and understandable order that was not necessarily intended by them.

In this way, invisible hand explanations distinguish between immediate appearances – what people think they are doing, what they seem to encounter while they are doing these things – and the causal processes that actually lead to the results. For instance, people may all be interested in their own profit rather than in maximizing collective utility, and yet there may be mechanisms of which their own selfish actions are a part that ensure that the latter happens – mechanisms of which no individual needs to be aware and the working of which is not explained by individual intentions.

We can see how this is a promising answer to some of the arguments mentioned in the previous chapters (see, for instance, the section on Karin Knorr-Cetina) where it was said that scientists do not encounter nature in their everyday work in the laboratory, and are usually more occupied with ‘making things work’ in their dealings with all kinds of actors

⁵⁰⁴ Ylikoski (1995, 36).

and materials than with ‘nature’ or ‘truth’ (section 5.4.2). An invisible hand account may grant this, but add that it can replace this level of the immediate experience of the actors with a ‘deeper’ level, and provide us with mechanisms that ensure that the aggregate actions of these pragmatic individuals progressively lead to a better account of the natural world.

It may even be better to speak of three levels, since there is also a level at which scientists do see themselves as being occupied with realizing values like truth or objectivity. The point of ethnographers like Knorr-Cetina is that this is a misinterpretation of what ‘really’ happens in the laboratory, where truth and objectivity seem to play no causal role. An invisible hand explanation counters this reading by one where what seems to happen in the laboratory is itself not what constitutes the real order behind the working of science.

7.2.2 An Economic Account: Alvin Goldman

Invisible hand accounts of science are associated with neoclassical economical language,⁵⁰⁵ but they clearly do not overlap with laissez-faire economics.⁵⁰⁶ Alvin Goldman, for instance, who has developed theories that can excellently be classified as invisible hand mechanisms, has written rather critically about the extent to which pure market mechanisms favor epistemically desirable outcomes. We will look briefly at this argument in order to get a clearer and concrete picture of what invisible hand theories are and what they are not. Goldman, together with James Cox, investigates whether a free market for speech or ideas is the optimal solution for encouraging the production of true beliefs. The measure for truth possession, in their definition, is the number of true beliefs divided by the total number of beliefs. Under such premises, truth cannot be *defined* as the result of free competition;⁵⁰⁷ just like in other markets, the measure of the quality of the products needs to be independent in principle of market mechanisms. Like in other markets, economic theory does not imply that the products made under free competition are of the highest quality, but that they are produced most efficiently relative to production possibilities and consumer preferences. It does not categorically predict what these goods are.⁵⁰⁸

A complicating factor in the case of truthful information is that perfect information is usually regarded as a condition for market functioning, not as a result. Even markets for regular commodities can fail under circumstances of imperfect information, Goldman and Cox explain. If markets are unregulated, the costs of checking the truthfulness of information lie with the consumers.⁵⁰⁹

The most noteworthy aspect of this account is the strict requirement that truth be defined *independently* of the outcomes of the social and economic process. This is a major

⁵⁰⁵ Davis (1998).

⁵⁰⁶ Leonard (2002, 152-155).

⁵⁰⁷ Goldman and Cox (1996, 1-8).

⁵⁰⁸ Goldman and Cox (1996, 17-20).

⁵⁰⁹ Goldman and Cox (1996, 20-24).

point in Goldman's 'social epistemology', and one that he sees as the main point of difference with the 'social doxology' of scholars like Shapin, who are, according to him, uninterested in truth.⁵¹⁰ Goldman leaves no misunderstanding about his insistence that truth is about the world, that "only the world confers truth and falsity".⁵¹¹ A large part of his *Knowledge in a Social World* is devoted to substantiating this realist claim.

An article by Goldman and Moshe Shaked, which provides an economic model of scientific truth, is a good example of an exposition of invisible hand mechanisms and the extent to which they seem to need such a realist account of truth. The premise in this argument is that scientists try to maximize their individual expected utility, and that this utility is defined exclusively by professional success.⁵¹² Goldman and Shaked develop a rational choice model to formalize decisions of these credit-seeking agents about which experiments they are going to perform, given certain subjective probabilities regarding world states and given the subjective probabilities of other scientists, and assuming they try to maximize the expected credit they are going to get by modifying the subjective probabilities of others. They conclude that under most circumstances, the probability revisions will lead to more accurate beliefs; only if the initial subjective probabilities are absurdly inaccurate will the experiments lead to an increase of error.⁵¹³

Goldman and Shaked's is effectively an invisible hand account, which sees scientists as credit-seeking and the amount of credit they receive as determined by their influence upon the beliefs of others. Their interests are socially defined. But their actions in pursuing these interests are not understandable unless some notion of accuracy is involved: scientists believe things about the world – that is, they are able to attribute more or less explicitly certain probabilities to statements about the world – and in their experiments, the world gives clues as to how accurate these probabilities are; moreover, scientists' beliefs can be rationally adapted to these clues, so that the world plays (to the third-person spectator who knows what the world looks like and what the subjective probabilities of the scientists are) a predictable role in scientific belief formation, as scientific belief formation will respond predictably to the clues it received from its (no less predictable) investigations.

Now, how we evaluate the effectiveness of this invisible hand account depends on what we believe it aims to achieve. To the extent that it is an account of how scientists can be motivated by aims other than an accurate description of reality while reality nonetheless has explanatory value with regard to the formation of scientific beliefs, it works. It does not try to give a radically new role to reality, let alone a new conceptualization of it; but that is not its point. Its point is to show how science can be successful in what it aims to achieve, even if

⁵¹⁰ Goldman (1999, 7-9). For a critical assessment of Goldman's view of what SSK has to contribute to our understanding of science, see Kusch (2011).

⁵¹¹ Goldman (1999, 21).

⁵¹² Goldman and Shaked (1991, 31-32).

⁵¹³ Goldman and Shaked (1991, 40).

scientists are not structurally motivated by a desire to further science but rather by a desire to further their own professional interests.

However, some of the criticisms raised by SSK against earlier rationalistic philosophies of science certainly apply to it.⁵¹⁴ After all, is this not just correspondentism, together with some very doubtful suppositions about scientific rationality? Didn't we already agree that evidence did not bear upon belief in an unequivocal way? What about the contingency of scientific concepts and categories? If we endorse Goldman's account, are we not simply replacing an individualistic rationalism with an almost identical rationalism on a social level?

These are questions that need to be addressed: they are important to assessing in what sense, and under what assumptions, invisible hand accounts provide an alternative to other perspectives such as SSK. We will now take a closer look, therefore, at the role of truth and correspondence in Goldman's 'veritistic' social epistemology as described in his *Knowledge in a social world*.

According to Goldman, a correspondence theory of truth is indeed the most natural account of truth. Alternatives do not work: pragmatist or instrumentalist theories which define truth in terms of desirable outcomes run into problems regarding the fact that what is desirable differs by person; verificationist approaches, which identify the truth of a proposition with its justification, have the problem that exactly the same proposition can be true first and false later. Coherence theories run into similar problems.⁵¹⁵

Another possibility are deflationist accounts of truth, which explain statements about truth as performative actions, or instruments of semantic ascent, or see 'true' as a predicate while denying it is a substantive property. What these theories have in common is that there is no metaphysical relation between a statement and the world that 'makes' a statement true, but that truth is just a useful linguistic instrument.⁵¹⁶ Goldman, on the other hand, sees truth as something that requires *truth makers*. According to classical versions of correspondence theory, these truth makers lie in a structural isomorphism between fact and world, but Goldman claims that his theory doesn't need this.⁵¹⁷

If he is right about this, then the correspondence on which Goldman's invisible hand account rests may be able to avoid becoming a kind of 'mirroring', the kind of representationalism that SSK advocates often accuse their opponents of adhering to. Perhaps we can retain the realist intuition behind Goldman's invisible hand account while providing a more subtle analysis of what the role of 'reality' in this account actually entails. In the following I provide an interpretation of Bernard Williams' *Truth and truthfulness*, in which he does precisely that.

⁵¹⁴ Cf. Kusch (2011).

⁵¹⁵ Goldman (1999, 41-48).

⁵¹⁶ Goldman (1999, 51-53).

⁵¹⁷ Goldman (1999, 59-62).

7.2.3 Accuracy and the Resistance of the World: Bernard Williams

Williams makes a broad philosophical argument for the idea that it is meaningful to want to find out the truth on an issue; if someone wants to do this, “we can say that this is equivalent to his wanting to get into the following condition: if *P*, to believe that *P*, and if not *P*, to believe that not *P*.”⁵¹⁸ According to Williams (referring to Goldman), some methods of inquiry have the property of leading to true belief (are truth-acquiring), and some have not, and this is what is meant with accuracy.⁵¹⁹ Williams goes on to say that it is important that there are external and internal obstacles to finding truths, and that this suggests a realist idea of truth, in the sense of “an independent order of things to which our thought is answerable. [...] It has often been recognized that the idea of a reality independent of us can involve an implication of resistance, resistance to the will.”⁵²⁰

Williams goes on to observe that this idea of resistance has usually been related to physical objects (which can resist our movements), but that it seems to be that “any case of necessity will be an example of radical resistance to the will.”⁵²¹ We cannot change truths about the past or about mathematics either; but Williams goes on to connect the notion of independent reality specifically to those states of affairs to which there is a conceivable alternative. This means that he can distinguish between the status of truths about, for instance, the past – of which we can wish that it had been different, but cannot begin to think that we can *do* anything that would make it different – and the status of mathematical truths – of which we cannot even conceive of what would be involved in other things being the case.⁵²² The Pythagorean philosophers may have wished that the square root of two was not an irrational number, but only if they did not think in a determinate or focused way about what this desire involved.⁵²³

I should remark at this point that this difference as based on Williams’ terms may be less clear than he suggests: do we really know precisely what would be involved in other truths about the past (or about other parts of reality that are independent of our will)?⁵²⁴ But this does not undermine Williams’ larger point, that “it is the sense of conceivable alternative that is particularly associated with realism. Realism invokes the idea of an order of things that is independent of us, where that means, in particular, independent of our will.”⁵²⁵ Accuracy, as a virtue, is resistance to subversion of truths about this independent reality by the wish.

Like the invisible hand theorists (to which Williams’ own genealogical account of the primary virtues of truthfulness bears some resemblance in its attempt to show how these

⁵¹⁸ Williams (2002, 133).

⁵¹⁹ Williams (2002, 126).

⁵²⁰ Williams (2002, 136).

⁵²¹ Williams (2002, 136).

⁵²² Williams (2002, 137-139).

⁵²³ Williams (2002, 139).

⁵²⁴ Cf. Rescher (2008, 113-126).

⁵²⁵ Williams (2002, 140).

desirable features of thinking can be thought of as having emerged out of non-intentional processes, though it goes further than just being a functional account),⁵²⁶ Williams believes that if science possesses the virtue of accuracy, this does not mean that scientists themselves live up to a Platonic ideal of personal disinterestedness, or that abstract natural science itself liberates from interestedness by transcending human affairs.⁵²⁷ The crucial issue is the question whether the thing scientists are interested in – even if this is a socially constituted good like prestige or power – depends on their succeeding in finding truths about nature, “just as those who in the ancient world or in the Renaissance sought fame through writing notable verse recognized that they would not achieve it without the notable verse.”⁵²⁸ The virtue of accuracy would be undermined if scientific recognition were itself a function of an antecedent social position; but that this is the case the sociology of knowledge has failed to demonstrate, Williams says.

“Science is, in game-theoretical terms, not a two-party game: what confronts the inquirer is not a rival will, and that is the key to the sense of freedom that it can offer. To be free, in the most basic, traditional, intelligible sense, is not to be subject to another’s will. It does not consist of being free from all obstacles.”⁵²⁹ The world, in Williams’ account, does resist; but the status of this resistance in Williams’ thought is completely opposed to that in Latour’s, since the resistance of the world and the objects it contains cannot be thought of as agency without the dualism between science and the world breaking down and the game becoming a multi-party game again.

7.2.4 The Limits of Normative Invisible Hands Accounts

These invisible hand accounts are not a solution to the problems posed by SSK concerning the relation between nature and society, let alone to the challenge of ANT; rather than answering these problems, they have to ignore them and ‘revert’ to a dualism between society and nature. This is not necessarily a problem in itself: as we have repeatedly observed, invisible hand accounts are there to show how nature can figure in the explanations of science even if it does not figure in the intentions or the immediate experience of scientists. This means that they are allowed to be conservative or commonsensical about the nature of the external world.

⁵²⁶ Williams (2002, 20-40).

⁵²⁷ Williams (2002, 141-142).

⁵²⁸ Williams (2002, 142). Cf. Brown’s (1989, 78-81) spin on the idea that scientists are motivated by peer recognition, by a comparison with capitalists who are motivated by making money: “But do successful capitalists only make money? Don’t they also make carpets and clothes pegs? And, in fact, don’t they make money because they make artefacts?”. A similar point is made by Papineau (1988, 53) within the context of an argument that naturalized epistemology can accommodate Edinburgh-style sociology of science and its symmetry principle, as long as the reliability of science is not considered to be explained sufficiently by exclusively social factors, but also by “the facts that scientific beliefs are supposed to be about” (51).

⁵²⁹ Williams (2002, 144-145). Cf. Dennett (1984, 50-73).

Both Goldman's and Williams' invisible hand accounts need to employ a concept of accuracy – something which, if it is supposed to say something substantive about the relation between nature and beliefs about nature, is closely related to correspondence and will in that case need to answer a whole range of skeptical and pragmatist objections.⁵³⁰ If accuracy does not pertain to the relation between reality and belief but purely to a virtue within science, this problem does not arise, but it becomes less clear what the conditions for this virtue are and how we can recognize them.

Goldman's and Williams' accounts are inevitabilist, in the sense that science converges to a more truthful account of what nature looks like. This holds especially for Goldman's correspondentism, if interpreted descriptively: as the claim that beliefs about the world will converge to a more truthful account of it if science works by and large according to Goldman's social epistemology, combined with the claim that science by and large works according to that epistemology. If this is the case, if we want to understand why scientists believe what they believe, we need to know only what the world they are studying looks like.

However, this is the case only if Goldman provides an account of scientific rationality in which the normative and the descriptive coincide: otherwise his mechanisms either lose explanatory value, or they shed light only upon fictional, idealized developments in science.⁵³¹ In fact, Goldman is clear about the fact that his project is a social *epistemology*.⁵³² It is not primarily an account of how actual science works; it is an account of how social belief-forming practices of a certain kind can lead to more accurate beliefs about an existing reality. Similarly, Williams is not providing a history of science; he is providing a genealogy of accuracy and sincerity as virtues of truthfulness, insisting that these virtues presuppose 'obstance' by an independent reality.⁵³³

The existence of these plausible fictional accounts could strengthen belief that plausible actual accounts based on similar presuppositions might also be within reach. Moreover, these descriptive accounts need not necessarily be inevitabilist. We will look at one project that explicitly identifies as an invisible hand account,⁵³⁴ but aims at the *explanation* of scientific beliefs rather than at their justification,⁵³⁵ and does so under the

⁵³⁰ E.g. Rorty (1982, xiii-xxii).

⁵³¹ See Kusch (2001, esp. 188-190), a criticism of Goldman (1999), where Kusch says that Goldman does not consider "what kinds of belief or knowledge are necessary for norms or social institutions to exist in the first place" (188).

⁵³² Goldman (1999, 1-9).

⁵³³ Williams' employment of a 'genealogy', incidentally, belies the assumption made by Bevir (2008) that genealogies are to be associated with radical historicism.

⁵³⁴ Hull (2001b). In Hull (1982, 273), he says that "I find a scientific theory of sociocultural evolution a vastly more significant goal than an evolutionary epistemology" Cf. also Hull (1988b, 12-13).

⁵³⁵ Hull (1988b, 12-13). According to Munévar (1988, 211), Hull cannot maintain that his goal is restricted to explanation rather than justification if he speaks of rationality, but he also fails to deliver an evolutionary epistemology. For similar reasons, Grantham (2000, 449) says about Hull what I have just said about Goldman: that his account "blends descriptive and normative claims" (cf. also 455-457). I

supposition that science is by and large very good at “realizing its manifest goals”.⁵³⁶ This is the project of David Hull, who has tried to look at scientific development as analogous to biological evolution, and subject to similar selective mechanisms.

7.3 David Hull’s Evolutionary Invisible Hand Account

7.3.1 Science as a Process

As said, Hull’s project is to give an evolutionary account of scientific development, rather than an ‘evolutionary epistemology’.⁵³⁷ That it is an evolutionary account does not mean that it tries to extend a biological vocabulary to conceptual developments.

Though Hull obviously proceeds from some presuppositions about the natural inclinations of humans as a species with regard to curiosity about their environment,⁵³⁸ he makes it clear that strictly biological accounts fail to explain the kind of conceptual developments that science exhibits: after all, our natural tendencies when it comes to, for example, classification of plants, are patently unscientific, and wrong.⁵³⁹ Hull, then, emphasizes that he does “not propose to extend a gene-based biological theory of evolution to include conceptual development in science. Instead, I provide a general analysis of selection processes which is intended to apply equally to both biological and conceptual change.”⁵⁴⁰

Thus, Hull sees selection processes as something that can be defined independent of the specific substrate on which they operate.⁵⁴¹ The notion of a ‘gene’ is not just made into a metaphor which is subsequently applied to the notion of a scientific ‘concept’;⁵⁴² rather, we could say that both are involved in instances of a ‘selection process’ that can be abstractly defined – there is a weak hint of idealism here in what is otherwise a thoroughly naturalistic metaphysics, which, as we will see shortly, dispenses with essentialism about science in a very useful way.

The abstract terms involved in selection processes are those of replicators and interactors – a replicator being “an entity that passes on its structure largely intact in successive replications”, an interactor being “an entity that interacts as a cohesive whole

believe this relies on a confused image of what Hull is doing: the point is not to be able to justify scientific theories and methods on the basis of an epistemology built on evolutionist presuppositions, but to provide an explanation of how the social organization of science leads to an effective realization of its goals. Though this may in some contexts ‘justify’ this organization, Hull does not seem to want to provide such a justification. I believe, with Hull, that the distinction between Hull’s explanatory aims and normative epistemology can be upheld here. See also Griffiths (2000, esp. 306-307).

⁵³⁶ Hull (1988a, 98).

⁵³⁷ Thus, Hull fits neither of Bradie’s (1986, 403) characterizations of evolutionary epistemology.

⁵³⁸ Hull (1988b, 14).

⁵³⁹ Hull (1988b, 284).

⁵⁴⁰ Hull (1988b, 20).

⁵⁴¹ Hull (1988a, 98).

⁵⁴² Hull (1988a, 109).

with its environment in such a way that this *causes* replication to be differential.”⁵⁴³ A process that differentiates between the fates of different interactors in such a way as to differentiate between the replication of different replicators, is a selection process.⁵⁴⁴

Selection processes influence the temporal change of *lineages*, a lineage being “an entity that persists indefinitely through time either in the same or an altered state as a result of replication.”⁵⁴⁵ Hull’s ‘lineage’ is a key concept to grasp, as it provides an excellent point of overlap between philosophy of biology and philosophy of history; it is through this concept that historical entities can become the center of scholarly attention. This is of immediate relevance to history of science in more than one way.

In *Science as a process*, Hull deals with the controversies concerning the applicability of the term ‘scientists’ before the modern period, judging that “the terminological convention being suggested by purists is so patently silly that it hardly warrants refutation.”⁵⁴⁶ The larger issue at stake here, however, Hull identifies as being the choice between calling everyone a scientist who performed activities that we would recognize as science, or treating terms as referring to particular times and places. This issue he rephrases in turn (the rephrasing effecting a slight change in content) as “similarity versus descent.”⁵⁴⁷ Hull decides to look upon science as something general, and Western science as an instance of this.⁵⁴⁸ ‘Western science’ is a particular instance of science in a very general sense, and as such we do not need to specify its formal characteristics; we just need to be able to identify it as a lineage. For purposes of delineation, its uniqueness or its similarity to Chinese or Greek science is neither here nor there, just like the question whether whales look like fish is irrelevant if we want to define whales as a species in the sense of a lineage.

What is the relation between lineages and selection processes? It is not tautological: the shape of lineages can result from something other than selection processes.

Lineages are historical entities formed by replication. Differential perpetuation caused by interaction is not necessary for something to count as a lineage. In fact, differential perpetuation itself, regardless of its causes, is not even necessary for something to count as a lineage. However, when the interplay between replication and interaction causes lineages to change through time, the result is evolution through selection.⁵⁴⁹

This makes Hull’s thesis that science is a lineage that evolves through selection into a synthetic claim, rather than the analytic one that it could have been.⁵⁵⁰ It would have been an analytic claim if Hull had held, for instance, that in order for something to be part of

⁵⁴³ Hull (1988a, 109).

⁵⁴⁴ Hull (1988a, 110).

⁵⁴⁵ Hull (1988a, 110).

⁵⁴⁶ Hull (1988b, 75).

⁵⁴⁷ Hull (1988b, 77).

⁵⁴⁸ Hull (1988b, 81).

⁵⁴⁹ Hull (1988a, 111).

⁵⁵⁰ Hull (1988b, 280-281).

science at all, it needed to be subject to selection pressures. A definition of science in terms of ‘conjectures and refutations’, or something similar, could lead to the claim that science evolves through selection being analytic. But Hull has not defined science as a class in any but extremely broad terms; and the ‘lineage’ of Western science has not been defined formally, but pointed to. This lineage changes over time – scientists believe, write and do different things now than before – and since historical change may be the result of something other than the interplay between replication and interaction, the question whether Western science has evolved through selection remains open.

7.3.2 Selection Pressures and the World

Much will depend, then, on the mechanisms that Hull identifies behind this supposed selection process. What is the thing that is being replicated differentially, what are the interactors, and with what do they interact?

Hull answers all these questions: the replicators are “elements of the substantive content of science – beliefs about the goals of science, the proper ways to go about realizing these goals, problems and their possible solutions, modes of representation, accumulated data reports, and so on”.⁵⁵¹ Conceptual replication is “a matter of ideas giving rise to ideas via physical vehicles, some of which also function as interactors. Replicators are generated, recombined, and tested by scientists interacting with the relevant portion of the natural world.”⁵⁵²

So to rephrase only slightly: the replicators are more or less abstract entities, which can exist in different physical vehicles; these physical vehicles, some of which are scientists, interact with the natural world, presumably in a differential way so as to cause differential proliferation of the scientific ‘ideas’.

Things are complicated because science is a social process; Hull’s account gets its subtlety from dealing with the social structure of the scientific process, analyzed through the concepts of *credit*, *use*, *support* and *mutual testing*.⁵⁵³ Scientists act the way they do, not because they get the rewards for adequate ideas about nature directly from nature itself; they do not physically die sooner (or fail to procreate) if their theories about nature are inadequate.⁵⁵⁴ Rather, what they strive for is recognition by other scientists;⁵⁵⁵ they are

⁵⁵¹ Hull (1988a, 116).

⁵⁵² Hull (1988a, 117). For a discussion of in what sense the environment of science in Hull’s model is the non-conceptual world, and the role of interpretation, see Gross (1988).

⁵⁵³ Hull (1988b, 281).

⁵⁵⁴ For Munévar (1988, 210), this is a sign that scientific change is, under Hull’s assumption, not actually the result of a selection process. Cf. also Campbell (1988, 176), about the question whether selection in science does not work much more directly upon the replicators.

⁵⁵⁵ Hull (1988a, 282).

differentially successful *as* scientists, in interacting with *other* scientists; and the extent of their success in this regard determines their ability to spread the ideas they hold.⁵⁵⁶

If the social (scientific) success of the scientists-interactors is dependent on the content of the replicators to which they are connected, the conditions for evolution through selection are realized. This is the case if science is organized in such a way that the goods that make scientists successful are in the end rewards for the content of the ideas that they hold. This is why it is crucial that scientists make use of each other's ideas, and test them.⁵⁵⁷

For this to be the case, it seems the community as a whole needs to interact with the natural world. At least Hull strongly believes that this is the case. Strictly, it is not necessary; it has been noted that Hull's analysis is applicable to any community of experts in which these experts are simultaneously sellers and buyers of the goods they produce.⁵⁵⁸ These experts might be theologians just as well as physicists. In that case, the supposedly crucial role of the world drops out. It still makes sense to attribute differential success of theological opinions in the Middle Ages to the differential social success of the theologians carrying these opinions. But is it necessary to suppose that the successes of the interactors can be related to the content of the replicators; that the opinions of the more successful theologians bear a different relation to the Supreme Being than those of their less successful counterparts?

It seems not. There may be a different 'outside world' against which these theological opinions were tested by peers, of course; the texts of the Bible and the Church Fathers, for instance. But perhaps not even this is necessary, and perhaps we can just say that the differential success of different theologians holding different opinions can be related rather to particular historical circumstances – social, political, cultural – to which these opinions were better suited than those of competing schools. This keeps the entities causally relevant to the selection processes in the realm of society; it would be the church-historical equivalent of SSK. Since the social, political and cultural circumstances would of course be somewhat less stable than the Deity about which the medieval theologians intended to write, we will not see these theologians converging to stable opinions and reaching stable goals, even though the evolution of the lineage(s) of theological thoughts involves differential replication through differentially successful social interaction.

In order to follow Hull, similarly to Goldman and Williams, we need to believe already that it is possible for the success of scientists to depend on those features of the

⁵⁵⁶ The duality and trade-offs between recognition and support resonate with the paradox that Latour's actors face according to Shapin (1988, 537-538).

⁵⁵⁷ Criticism of Hull's use of the concepts of replicators and interactors in science is often based on the idea that the relation between replicating scientific entities and interacting scientists is too unlike that between genotype and phenotype in biological evolution. Cf. Sterelny (1994, 50-52) on genes building their interacting vehicles: "An electrical engineer is not a voltmeter's way of making another voltmeter." (50) I think the analogy holds and is actually instructive: if usage of voltmeters increases the fitness of the engineer *as* engineer relative to other instruments, this will lead to a differential proliferation of voltmeters and other instruments.

⁵⁵⁸ Kantorovich (1988, 200).

substantive content of their theories which pertain to their relation to the natural world. Again, this is not necessarily a problem; we have established that the arguments that say we should completely omit the natural world from our account of science are unconvincing, and that it is very plausible that features of the natural world can be of explanatory value for the history of science. The question is where the natural world comes in and how its causal influence works.

Hull gives an example of causal links between the non-conceptual, non-social, natural world on the one hand, and scientists on the other:

because I see a ball accelerate as it rolls down an inclined plane, I come to hold beliefs about the motion of balls as they roll down inclined planes.⁵⁵⁹

This is straightforward enough, if we don't read the condition here as a sufficient condition, and make no trouble about the 'seeing a ball accelerate' being hardly 'non-conceptual'. Observations can cause us to believe certain things, depending on how we are disposed to respond to these observations. Here I would like to call into mind what we discussed in chapter 3: that observations of nature can cause us to form certain beliefs is not the same as saying that nature on its own forces these beliefs upon us. After all, what kinds of beliefs we form when we see a ball roll down an inclined plane depends very much upon our previous beliefs, and upon our judgment of this observation, et cetera. From this quote, there is no reason to think that Hull does not realize this or that he disagrees; he is simply pointing out that there are obvious causal links between what we see and what we believe.

However, he also speaks about the role of the world in a more inevitabilist register: he finds himself believing, not just that events in nature are partial causes of opinions in science, but also that science gets progressively better at what it tries to do, and that this is because the things it is looking for in nature actually exist.⁵⁶⁰ Laws of nature, for instance, are really there to be found:

Conceptual evolution, especially in science, is both locally and globally progressive, not simply because scientists are conscious agents, not simply because they are striving to reach both local and global goals, but because these goals exist. If scientists did not strive to formulate laws of nature, they would discover them only by happy accident, but if these eternal, immutable regularities did not exist, any belief a scientist might have that he or she had discovered one would be illusory.⁵⁶¹

The claim about the role that nature plays in science according to this quotation, I hope it is clear, does not follow from that in the previous quotation, though it also does not contradict it. In the gap between the two lies the question how nature gets to be not just a cause of

⁵⁵⁹ Hull (1988a, 117).

⁵⁶⁰ Cf. the discussion by Henson (1988).

⁵⁶¹ Hull (1988a, 124-125).

beliefs, but also something that beliefs in some way converge toward. How precisely do we need to read this, and how does this come about?

This puzzle, as Donald Campbell states it, is how it happens that “the beliefs of physicists come to fit the physical world they refer to”.⁵⁶² Selection processes are an answer to this question, similar to the case in which we are puzzled by the whiteness of the polar bear which so well fits the whiteness of the surrounding terrain.⁵⁶³ If we are not convinced of, or interested in, a striking ‘match’ between science and its ‘surrounding terrain’, like SSK scholars; or if we think science and its environment have come to be in the same movement, so that their correspondence is not a puzzle, like Latour, then the reference to scientists interacting with the physical world in order to test the ideas-replicators of other scientists becomes uninformative, an answer to a non-existing question.

Hull, like the other invisible hand theorists, believes that science is trying to do something and that it is successful at doing so. What is so appealing about Hull’s approach, however, is that he foregoes the rationalism inherent in, for instance, Goldman’s epistemology; he does not provide an account of what it is about the theories selected by science that made them fit for selection rather than their alternatives. Following Laudan,⁵⁶⁴ Hull embraces the idea that it is not just scientific theories that change over time, but methodologies and goals as well – “the nature of science is constantly under negotiation”.⁵⁶⁵ It is not that the theories possess some ahistorical value like truth; it is not even that the scientists holding some specific theories excel at some transcendent virtue like ‘accuracy’;⁵⁶⁶ no, the way to speak about this is to say that scientists are trying to increase their ‘conceptual fitness’, and this is a wholly contextual term, depending on what counts as ‘fitting’ to the relevant communities, and what counts as a successful test. Hull affirms the primacy of the use scientists make of terms over the philosophical analysis of these terms.⁵⁶⁷

Hull’s sympathetic mention of Laudan and his insistence that the ‘nature’ of science is dynamic and under constant negotiation suggest that he believes that the goals of science are themselves part of the evolutionary process, rather than something that remains stable throughout the scientific lineage. This is indeed more in tune with a naturalistic approach than retaining the notion that science has always had the same final goals – except of course if it be in the broad sense needed to identify a particular lineage of opinions as ‘scientific’, which might involve a minimal requirement related to finding out things about nature. It is certainly not necessary for science to try to find eternal and immutable laws of nature; and if Hull (in the passage quoted above) really means to say that the progress of

⁵⁶² Campbell (1988, 175).

⁵⁶³ Campbell (1988, 175).

⁵⁶⁴ Laudan (1977). Laudan considers it useful to investigate the rationality of these developments over time (see Laudan (1977, 167-170) on the relation between this and Lakatosian ‘rational reconstruction’).

⁵⁶⁵ Hull (1988b, 297).

⁵⁶⁶ Cf. Downes (2000, 436-438), who employs Hull’s account in a critical discussion of the link between truth and selection.

⁵⁶⁷ Hull (1988b, 298).

science he seeks to explain is progress towards a stable goal, namely the discovery of eternal laws of nature, then this belief would be hard to square with a belief that the axiology of science is under constant negotiation.⁵⁶⁸

This belief would also be just a bit too much like the belief that ‘current polar bears are better at fur colors than their ancestors’. Polar bears as a lineage don’t try to be white; they try to survive (– in fact, not even that claim is conceptually necessary; selection processes might explain the whiteness of the polar bear’s fur without any polar bear ever really trying anything). Similarly, we are supposed to believe that scientists don’t try to find eternal laws of nature; they try to increase their conceptual inclusive fitness (or at least, they fare better as scientists if that is what they do).⁵⁶⁹ No or very little explanatory value is supposed to lie in their intentions to solve problems.⁵⁷⁰ Under certain circumstances, convincing other scientists that you have found a law of nature, or testing or using claims by other scientists to this effect, may be the best strategy to increase your conceptual fitness. But these circumstances are not universally present: the beauty of Hull’s account is precisely that it is so permissive when it comes to strategies that scientists can employ, and that it can recognize that the aggregate effects of these strategies can be a ‘redirection’ of not just the theories, but also the goals of science.

The ‘progress’ we are trying to explain, then – our observation that current science is so good at what it tries to do – is progress from *our* point of view; the point of view of the current aims and methods of science. This takes away some of the magic. If the substantial goals of science (as opposed to the attempt of scientists to increase conceptual inclusive fitness, which is potentially a general feature of scientists – or otherwise it is something that successful scientists turn out to have done⁵⁷¹) have developed together with its other features, then these goals may be the result of strategic adaption to what is possible. In that case, we are like polar bears that realize how good we are at being as white as our environment, and who are puzzled by this fact. The solution to this puzzle is not the Latourian solution that our environment and ourselves result from the same process (because this is not the case); but part of the solution may lie in the realization that our *valuing* of being-white (which is a useful trait specifically in our snowy environment) is not causally independent of our *being*-white (which is the result of evolution through selection in our snowy environment).

To say this more bluntly: that we find our science to be so good at doing what we expect of it is not only because it has found better ways to do the things we expect of it, but also because our culture has grown to expect of science the things it simultaneously found out how to do. This is one reason why we reformulated the question of contingentism and

⁵⁶⁸ On this issue, cf. also the very lucid treatment by Grantham (1994).

⁵⁶⁹ Hull (1988b, 282).

⁵⁷⁰ Hull (1988a, 123).

⁵⁷¹ Heyes (1988, 194) notes that Hull’s empirical claims suffer from a sampling problem, since most historical databases show only those scientists that succeeded in gaining recognition.

inevitabilism at the beginning of this thesis (section 2.1), to free our questions about historical causality and path dependence from our criteria of success.

This co-evolution of means and goals is a concept that will be especially appealing to historians who embrace the historicist intuition that scientists in different times and places are playing different ‘games’,⁵⁷² with goals that cannot be easily translated into each other. It may be the case that the underlying aim of early modern natural philosophy was the understanding and praise of the Creator, whereas modern scientists *qua* scientists usually do not have this aim. However, the essential incomparability of scientific goals should not be a postulate of history of science. Goals (and scientists or science need not have had just *one* goal at any time) may have been stable for some time in some respects: the ancient Greeks have never tried to split the atom, for obvious reasons, so in that sense they were not engaged (as a scientific community) in an intentional activity identical to that of some modern research communities; but on another level of abstraction (‘trying to identify the fundamental elements of physical entities’) they may have been pursuing the same goal. The verdict of progress will differ according to the ways the goal can plausibly be phrased, and according to our measure of technical progress in the realization of those goals. We are never just better at ‘science’ than the Greeks, just like polar bears are not better at ‘fur colors’ than their ancestors; we always need to find a plausible way of interpreting the purpose of what went on, then and now.

Much of what I have been saying here, including this last point, suffers from an equivocation of functionality and intentionality. The complex interplay we see developing here between individual intentions, the functioning and goals of science, and the blind forces of selection pressures, certainly merits further attention.

7.3.3 Scientists as Agents

If we are to provide an account of historical change in science in terms of evolution by selection, then we should be able to distinguish processes that cause mutations in replicators from processes that select these replicators.

Hull has emphasized that his account is not Lamarckian.⁵⁷³ This he has done in answer to objections to analogies between biological and conceptual evolution: it is intuitively plausible that conceptual change is a directed process, since it is carried out by intentional agents. A Darwinian process is, by definition, ‘blind’; usually this is taken to mean that genetic mutations are random. In a slightly less strict sense, it can be understood to mean that interactions with the environment do not influence the mutations of the replicators; their differential interaction with the environment (caused by differences in their corresponding replicators) only causes the differential proliferation of different replicators, and has nothing to do with the processes that lead to the differentiation of replicators itself. In biology, this means that those accounts fail to be Darwinian that fall short of ‘hard

⁵⁷² Cf. the discussion of Cunningham in section 3.3.

⁵⁷³ Hull (1988a). See Smith (1988, 216); Bechtel (1988); Heyes and Plotkin (1989, esp. 154-157).

heredity': if acquired characteristics can be genetically passed on, then the independence of replicators and interactors gets compromised.

It seems that most of the things that happen in culture are the passing on of acquired characteristics, and this may seem like a complication for a Darwinian account of scientific development. However, this is not necessarily so. According to Hull, the intentional agents are not the replicators, but the interactors; scientists are not reproducing themselves, but they are, through their interactions with the social and natural environment, causing differential reproduction of replicators. The memes are replicators, analogous to genes in biological evolution.⁵⁷⁴ Just like the fact that some biological entities have intentions does not negate the Darwinian nature of biological evolution, the fact that scientists have intentions does not negate the Darwinian nature of cultural (scientific) evolution.

That is, as long as the evolution of scientific concepts is not *directed* in any sense by the actions of scientists.⁵⁷⁵ Peter Skagestad, an early critic of evolutionary epistemology (which is, again, not the project Hull is engaged in, but the criticism is relevant to his project as well), has attacked the application of a model of purely blind variation and selective retention to science held by Campbell (mentioned above in connection to the polar bear analogy).⁵⁷⁶

For this attack, it is not enough simply to say that scientists direct conceptual change because they base adaptations of previous theories on heuristic methods. This might provoke the reply that these heuristic principles are themselves the product of blind variation and selective retention, and that their existence and what it implies must be taken to be part of the already-acquired knowledge; any acquisition of knowledge that is really new might still be a result of blind variation.⁵⁷⁷

However, Skagestad adds to this that the accumulated tradition works to decrease the "range of permissible guesses".⁵⁷⁸ This is a crucial point. "Prior adaptation in biological evolution *raises* the probability of further adaptation, while the prior guessing embodied in an intellectual tradition may as often *lower* the probability of further progress through a novel, correct guess."⁵⁷⁹

It may be worth to drive this point home. After all, in one sense the possible further development of phenotypes in biology is limited by the previous evolutionary history of the organism, just like the possible development of an intellectual tradition is limited by its previous history: evolutionary change is path dependent in both cases. In biological

⁵⁷⁴ Hull (1988a, 144).

⁵⁷⁵ Cf. the discussion by Grantham (2000, 452-454). For Grantham, this problem suggests that Hull's social explanations of science (especially based on its demic structure) are to some extent independent of his selectionist account, since the former can be accepted without the latter. Cf. also Wray (2000), which focuses on 'hidden hand' explanations of scientific institutions rather than theories.

⁵⁷⁶ E.g. Campbell (1960). See esp. *ibid.*, 392-295 for Campbell's responses to anticipated objections.

⁵⁷⁷ Campbell (1974, 421). Cf. also Stuart-Fox (1999, 43).

⁵⁷⁸ Skagestad (1978, 615).

⁵⁷⁹ Skagestad (1978, 615).

evolution, however, genetic mutations that are physically possible are not ruled out by the makeup of the phenotype such as it is as a result of prior adaptation. In cultural history, on the other hand, not all the changes that are conceptually possible are always equally 'culturally possible': it may be possible for one scientist to replace a falsified heliocentric theory about the solar system by a geocentric one, while for another the only viable option is to replace it by another heliocentric hypothesis.⁵⁸⁰

The differential viability of heliocentric and geocentric hypotheses may simply mean that we should expect a differential proliferation (*as* scientists) of scientists holding heliocentric and geocentric theories in different ages, and that this differential proliferation will lead to a differential success of heliocentric and geocentric theories in different times. Moreover, Hull does not require that the social environment of scientists is only a proxy to nature; he allows for differential and culturally determined historical influences on theory selection.⁵⁸¹ But the objection, as I interpret Skagestad and as I would maintain myself, is that the likeliness of a heliocentric theory being 'altered', in one generation, into a geocentric rather than a heliocentric theory is itself not stable, and dependent on a historical context – the same historical context with which the scientists-interactors are confronted. The same forces that would operate to influence the selective retention of proposed theories may be anticipated by the scientists, and influence the ways they develop and publish their theories.⁵⁸²

Cecilia Heyes has called it problematic that in Hull's account individual scientists have so much agency, since: "it would be unfortunate if an evolutionary analysis of scientific change were crucially dependent on our understanding the beliefs and motivations of individual scientists since [...] the content of these states is very difficult to specify."⁵⁸³ She proposes to drop the idea that scientists themselves function as interactors; rather, if they conform to certain specified cognitive characteristics, they can allow other entities (such as texts, diagrams, and gestures) to function as interactors.⁵⁸⁴ Needless to say, if scientists and their interactions with other scientists and with experiments influence not just the selective retention of theories but also the way in which they develop, then the role of individual scientists will become even greater, much to the dislike of those who find the understanding of their beliefs and motivations so cumbersome to deal with. It is my position that this understanding is, indeed, necessary.

I agree with Hull, however, that scientists do not, on a large scale, foresee what history will do to the conceptual change they have carried.⁵⁸⁵ The reasons why a scientist develops and publishes a theory need not bear a clear and direct relation to the forces that

⁵⁸⁰ Cf. Cain (1988) on the emphasis in Hull's work on selection rather than the problem of variation.

⁵⁸¹ Cf. Skagestad (1978, 616-617).

⁵⁸² Cf. Thagard (1980).

⁵⁸³ Heyes (1988, 198). On the complicated role of scientists (and, in particular, of their social and conceptual relatedness) in Hull's system, see also Griesemer (1988, 181-182).

⁵⁸⁴ Heyes (1988, 199).

⁵⁸⁵ Compare e.g. Ghiselin (1988, 178).

allow her to increase her conceptual inclusive fitness through this theory and thereby allow her theory to flourish. Mendel need not have anticipated the circumstances that led to the eventual success of his findings. Newtonian mechanics may owe its spread to its service to lot of purposes that Newton did neither intend nor consider desirable.⁵⁸⁶ I think it is, in general, a safe bet to say that no early modern scientist was consciously striving towards the current state of science – or laboring against it, for that matter.

This is in line with the invisible hand motif running throughout this chapter: we are looking at mechanisms that potentially transcend the scope of individual intentions. There may be something understandable about the dynamics of theory acceptance and rejection in science that does not at any stage need to be traced to individual intentions.

Nonetheless, I think it is important to have established – and the possibility that scientists anticipate at least some of the forces that influence the survival potential of themselves as scientists given their commitment to a certain theory, is only one way in which this may happen, albeit a conceptually important one – that there is something to be understood about the dynamics of conceptual change as well; and that these dynamics are historically conditioned in a way not captured by the notion of blind mutation.⁵⁸⁷

7.4 Adaptation, Realism, and the Necessity of Understanding

Hull's proposal for an explanatory account of the development of the 'lineage' of Western science in terms of evolution by selection is very elegant and sympathetic. We ought to keep in mind that the goals in this process have evolved along with the means and that this evolution has therefore not necessarily been one of linear progress or convergence. If we do so, Hull's account delivers all that it has promised: it explains scientific beliefs while undergirding the intuition that science is, by and large, good at realizing its manifest goals. The trick is that science is adaptive: that is has historically come to be structured in such a way that it evolves to accommodate new input from nature or changes in epistemic goals and methods.

We have also seen that Hull considers it essential to his invisible hand account that the things natural scientists are orienting their activities to – for instance, when they set out to discover laws of nature – actually exist. The question is to what extent we need to go along with this. For instance, do we need to be metaphysical realists when we say that science *adapts* to nature? This might seem to be so, since in this case we grant to nature a status independent of what science says about it – and is this not simply an instance of granting to the external world a status independent of what we think about it?

In fact, we need to be realists in this case no more than we need to be realists when we say that animal species (or lineages) *adapt* to their environment: what we need to believe is that we can intelligibly and meaningfully speak of 'natural entities' as something distinct

⁵⁸⁶ See e.g. Jorink and Zuidervaat (2012) and the other essays in Jorink and Maas (2012) on the reception of Newton in the Netherlands, and its complicated relation to Newton himself.

⁵⁸⁷ Cf. Sterelny (1994, 59-60) on the less ambitious options open to evolutionary theorists.

from ‘things scientists say about nature’. It is possible to deny that we can do this, and in a simplification of Latour’s position, we could say that he comes close to denying this; but *not* denying this does not mean that we are holding the metaphysical position that the world exists completely independently from our minds, let alone that our minds somehow have access to this mind-independent world.

The analogy with biological adaptation illustrates this. The opinion that biological entities adapt to their environment is not restricted to metaphysical realists. The analogy is complicated, of course, by the fact that, contrary to the case of biological adaptation, in which we have access to descriptions of the environment independently of our access to descriptions of biological entities, we do not always have access to nature independently of the science that we study.⁵⁸⁸ Sometimes we do, but when we study the historical development of our *own* scientific opinions we do not.

Then still, the notion of an independent world can be something other than ‘unnecessary metaphysics’; Philip Kitcher has characterized it as a result of extrapolation: “Our purchase on the idea that some objects are independent of some of us (although observed by others) suffices to make intelligible the thought that some objects are independent of all of us.”⁵⁸⁹ In our case, we can extrapolate to our awareness that some objects in nature exist independently of what other scientific cultures and traditions have said about them, to an awareness that some objects in nature exist independently of what any scientific tradition has said about them – including our own. When we say of successful ways of dealing with the world that they are approximately correct, Kitcher says, we do not make a jump from “things-as-they-appear-to-us” to “things-as-they-are-in-themselves”,⁵⁹⁰ but from a situation we observe to a (possibly counterfactual) situation which we do not observe: we say that if we hadn’t been present to see this person or this culture dealing with a world that we observe to be independent of her, their actions would have been just as successful because the same causal relations apply.

Kitcher makes his move from success to accuracy based on his idea that “we rely on our common experience of likely success rates with accurate and inaccurate representations”.⁵⁹¹ I am not sure that it is necessary or desirable to make this step in history of science: though accuracy may be a virtue or value central to current science, it is only indirectly relevant to history of science, as a potential aspect of the explanation of the historical development of scientific beliefs. And in history, as Laudan has argued,⁵⁹² we have to recognize as successful at least some theories that we are also bound to call inaccurate – sometimes perhaps even less accurate (to our knowledge) than less successful theories. At the very least, the relation between accuracy (which I understand to mean a degree of

⁵⁸⁸ Cf. Skagestad (1978, 618).

⁵⁸⁹ Kitcher (2001, 25).

⁵⁹⁰ Kitcher (2001, 28).

⁵⁹¹ Kitcher (2001, 28).

⁵⁹² Laudan (1981).

structural similarity between a scientific theory or model and the external world) and success is not linear.

The reason I refer to Kitcher's argument about the notion of an independent world is not because of his point about accuracy, but because it serves to show that the fact that we cannot think about 'the external world as independent of how we think it is' as something substantially different from 'the external world as we think it is' – it would be paradoxical to say that we believe these two things to have different properties (e.g. one containing laws of nature but not the other) – does not prevent us from conceiving of this external world as independent of what any third person thinks about it, and extrapolate to its independence of what *we* think about it. That is, the idea can make sense that *our* scientific culture has to some extent 'adapted' to a nature that is independent of it even if it is only known to us *through* it.

Now, in a thoroughly Darwinistic world, the natural environment will be the main explanatory factor for the makeup of a species (conceived of as a lineage) at a certain time, in combination with the preceding temporal parts of this lineage: the lineage as it is will adapt to fit the environment in one of the optimal ways. We cannot simply translate this to say that nature as it is will be the main explanatory factor for the makeup of science; and not just because this would be Whiggish and circular. It is also not possible because 'nature' does not exhaust the 'environment' of science; or phrased more precisely, the set of things a scientific discipline seeks to describe does not exhaust the set of things that constitute the environment to which it adapts, since the environment with which it interacts also contains other people and objects. An evolutionary account of (a particular discipline in) science predicts that it will not converge to theories and models which fit the *objects* it studies best – if this would even mean anything – but that it will adapt to fit its environment as a whole best.⁵⁹³ An easy way to make sense of this is, if we follow Hull in identifying the substance of science as being primarily its conceptual content, the recognition that this content needs to be of such a nature as to be amenable to being handled by humans with the perceptual and cognitive capacities such as they are biologically given, in numbers such as the social and economic structure of a society can provide, and with the categories and prejudices that their culture has imprinted upon them. Even if we believe that a geocentric cosmology is 'less accurate' than a heliocentric one – which would be the closest thing to 'less well *adapted* to the actual state of the solar system' – it can, at a certain time and place, be the doctrine *best* adapted to the historical context as a whole. The goal of history of science would be to describe this context in such a way that we can see how this is the case.

We have also seen, in the previous section, that the mechanisms behind the evolution of scientific concepts necessarily fall short of pure Darwinism, because the likeliness of possible mutations within the pool of scientific entities is influenced by cultural and other historical factors, adding a stage to the process before the actual mutations go

⁵⁹³ Cf. Giere's (2006) use of 'fitness' in his perspectivist account of science (esp. 71-72).

through the selection process through interaction with the environment. It is crucial to understand how this happens.

These things together ensure that the thesis that science develops through the mechanism of evolution through selection, though it deals successfully with many problems regarding the relation between science and the world, cannot sidestep the demand for a more detailed understanding of the historical context by appealing to the environment from which the selective forces that work on science proceed.⁵⁹⁴ The word ‘understanding’ is, at this stage of the argument, not intended to be contrasted to the ‘explanatory’ activity of evolutionary mechanisms simply by means of a terminological divide; I do not want to suggest a fundamental distinction between these two aspects of the explanation without arguing for it, so until further notice, by ‘understanding’ the historical context I simply mean clarifying the *explanatory* function that this context serves.

However, I do think that there is some crucial work to be done in exploring the relevance for history of science of ‘understanding’ with its further connotation of ‘interpretation’; and that a philosophy of history of science, where history of science is the discipline engaged in understanding science by grasping it in its ‘environment’, cannot be complete without further reflection upon what it means to understand science in the world. This is the aim of the final chapter.

7.5 Conclusions

From the preceding discussion, we can conclude:

- 1) David Hull’s view of science as a lineage provides a historically fruitful alternative to a view of science as defined normatively or as a kind.
- 2) A naturalistic view of science as evolving in continuous interaction with the world, where this interaction causes a differential proliferation of scientific theories and practices, provides a very plausible account of the role of the world in science that can in principle harmonize historical causal explanation of scientific developments with an explanation of why science seems successful.
- 3) There is no reason why such an account would be globally inevitabilist, since the entities that science studies do not constitute the whole of its selective environment, and scientific goals, methods, instruments, and theories are also part of each other’s selective environment, which as a whole develops historically.

Since the selective environment influences not only the selection but also the mutations in scientific goals, methods, instruments, and theories, the evolution of science is not properly Darwinian and an explanation of historical change in science requires an understanding of the whole local environment in which change comes to be proposed.

⁵⁹⁴ See also the criticism of evolutionary accounts by Jardine (2004, 272-273).

