

## **TURNING NATURAL**

**Pascal Engel**

**Université Paris IV Sorbonne**

Review article

*(Review of Callebaut, The naturalistic turn, *Biology and philosophy*, 2000)*

In philosophy (and elsewhere) certain words which had a negative connotation during previous periods come to have a positive one during the next. “Naturalism” is obviously one such word. Early analytic philosophers reacted against XIXth century naturalism, which was accused of dissolving objective norms (logical, epistemological, ethical) into facts (biological, psychological, historical, social). Frege and Russell attacked naturalistic psychologism in logic and in the philosophy of mathematics. Moore denounced the “naturalistic fallacy” in ethics. The positivists were equally dismissive: naturalism meant the ignorance of such crucial distinctions as the logical vs the empirical and the analytic vs the synthetic. Today, however, to be a naturalist seems to be a Good Thing, and few philosophers would reject outright a characterization of their work as “naturalist”. Naturalism “returns” (Kitcher 1992). After a century of anti-naturalistic analytic philosophy, we seem to have come full circle.

This book is meant to document this naturalistic turn, and to assess its contemporary scope and influence, especially in the domain of the philosophy of science. It is made up of interviews of leading researchers in philosophy, biology,

psychology, cognitive science, history and sociology of science and science studies that Callebaut has met between 1985 and 1991. The actors are, together with the moderator, the philosophers of science William Bechtel, Robert Brandom, Richard Burian, Patricia Churchland, Ronald Giere, David Hull, Philip Kitcher, Elizabeth Lloyd, Helen Longino, Thomas Nickles, Robert Richards, Alexander Rosenberg, Michael Ruse, Dudley Shapere, Elliott Sober, and William Wimsatt, the biologists Richard Levins and Richard Lewontin, the psychologists Donald Campbell, Henry Plotkin and Ryan Tweney, and the social scientists Jon Elster, Karin Knorr Cetina and Bruno Latour. Their academic affiliations, however, do not indicate the scope of their interests, since most of them move back and forth between various disciplines, and have a strong background in several scientific fields. Instead of simply transcribing the individual interviews, Callebaut has combined bits and pieces of these by themes, so that on each theme of the book we have something which looks like a conversation between several individuals on a given topic. Sometimes the result is rather artificial and gives us a pseudo-dialogue. But most of the time this arrangement gives a happy result, where the opinion of each participant is brought to bear on a special topic in something which looks like a real dialogue. The choice of interviewees has been made by Callebaut on the basis of his personal connections and of his own interests in science studies. Although it is clear that other people could have been chosen, they are fairly representative of the various trends. As he says, his leading thread has been “serendipity”. Callebaut and his interlocutors have the gift of expressing themselves in a most straightforward manner, and often personal details are interwoven with highly theoretical matters, making it sound as if the reader were directly introduced into the offices and the daily life of research of (mostly) contemporary American universities. In addition, each interviewee gets a one-page presentation of his career and current work. This style is a bit irritating when it indulges in mutual

congratulations and laudative comments about how good, creative, and skillful each researcher is, but on the whole the book has a conversational charm and directness which is absent from many academic books.

In his introduction Callebaut aptly characterizes naturalism as the product of several strands or stances rather than as a unified doctrine. Although he does not explicitly do so, one can extract from his discussion the following divisions, from the most general to the most specific. One strand is *ontological*: naturalism is generally defined as the thesis that there are no other entities in the world than those that are postulated by the natural sciences. An extreme version of this view is physicalism, the thesis that there are no other entities than those posited by physics, taken as the fundamental science. As Callebaut notes, not all contemporary naturalists advocate such a reductive version of the doctrine; they side rather with a non reductionist one. Another strand is *epistemological*: Naturalism is the view that there are no other kinds of explanations than those which are provided by the natural sciences, presumably causal explanations. In this sense, naturalists reject the division between the *Geisteswissenschaften* and the *Naturwissenschaften*. On that score, contemporary naturalists do not seem, at first sight, to improve upon logical positivism, which was always associated with the doctrine of the unity of science. But this logical positivist doctrine was always meant as a *conceptual* or a *linguistic* one: the positivists attempted to reduce, by logical means, the language of psychology, of sociology, and of other disciplines to some basic language, physicalistic, phenomenistic, or otherwise, through what they took as a form of “rational reconstruction”. In this respect, their naturalism and their empiricism was largely an *a priori* and a philosophical thesis, not an empirical or a scientific one. This is not the path taken by most contemporary naturalists. They reject, following Quine, the analytic/synthetic distinction between

concepts and factual matters, between science and philosophy. As a result they do not, in general, try to reconstruct, by conceptual means, what an ideal kind of explanation would be; they rather try to *describe*, by empirical means, using the very resources which are provided by scientific investigation — biological, psychological, sociological, cognitive scientific, etc.— our scientific knowledge. In other terms, contemporary naturalism claims that *only science can describe science*. This implies, in particular, that the *normative* investigation about knowledge — whether our scientific reasonings and theories are justified, valid, or rational— has to be absorbed into a *descriptive* inquiry about what we actually know, as a matter of fact. Let us call this the *descriptive* stance. This is the stance which was taken, most obviously, by Quine when he proposed his famous thesis of the “naturalization” of epistemology, and its reduction to empirical psychology. By this Quine meant behavioral psychology, or possibly neurophysiology. But contemporary naturalists gave other versions of the appropriate description, and in this respect the naturalistic house contains many mansions : some think that it is biology, in particular evolutionary biology, which is apt to give us the governing body of information about the formation of knowledge, and propose various kinds of “evolutionary” epistemologies; others think that it is cognitive science and its subdisciplines which provide the appropriate descriptions; still others think that it is the history and sociology of science, perhaps helped by the results of other fields. The latest theme, however, leads to another strand as well. Since naturalism, presumably, starts with nature, either physicalistically, biologically or cognitively (so to say) described, it seems that nature is something *given* to us, from which our knowledge, commonsensical or scientific, derives. But the descriptive stance implies also that one takes into account the way in which scientific theories are formed, through history, as well as the way in which they are received in society, and shaped by social forces. And here we find something like the reversal of the

idea that nature is a given, or that reality, as science describes it, is something out there, which lies antecedently to our investigation: for scientific theories evolve, most are rejected in favour of new or improved ones. Our knowledge is not fixed; it is corrigible and fallible. Moreover, the history and the sociology of science presents us with a picture of science which is in direct opposition to the idea that nature, as it is described by science, lies outside our investigation. Theories are *constructed*, both by scientists and by the social and historical environments in which they live. There is, therefore, nothing really “natural” in the way knowledge is formed, and this leads to the thought that nature itself, the object of our scientific inquiry, is not “natural” either, but the product of a construction. As Callebaut says (p.3): “We, who are part of nature, help *construct* her.” Let us call this the *constructivist* strand. The theme is most present in science studies which rest upon historical and sociological analyses, and in philosophy it is often associated to pragmatism, and to the view that the traditional philosophical dualisms between mind and nature, norm and fact, the the manifest and the scientific image (Sellars) are obsolete. This theme, however is ambiguous: is the construction in question itself a natural process or not? In other terms, are the products of our knowledge constructions out of processes which are themselves natural (say through evolution of our cognitive capacities) or constructions out of forces which are essentially historical, social, i.e less than natural and somewhat artificial? And in what respect are the constructions descriptions of a unique reality, or alternative descriptions of a more or less arbitrary character? The answers to these questions depend upon the degree of freedom from nature itself that the naturalist is prepared to grant to historical and sociological determinants of scientific inquiry, and upon the degree of realism or of relativism about its subject matter that he is willing to admit. For instance, partisans of evolutionary epistemology and sociobiologists would not accept the idea that the social factors in science are

completely independent of their biological determinants, but many researchers in the history and the sociology of science would disagree with them. The later seem also to be more favorable to a broadly relativistic outlook about what science actually describes rather than to a realistic one.

The contents of the book largely reflect these divisions and the potential tensions which exist between them. It is divided into two main parts, each divided in five chapters. In the first part, the interlocutors attempt to characterize the main features of the naturalistic turn in the philosophy of science and how, according to them naturalistic philosophy of science ought to be done (“Talking about it”). Much of this first part is devoted to a description of the changes which philosophy of science has undergone since the 60 onward, which have led to what I have called above the descriptive stance. Callebaut’s interlocutors explain in particular the importance of two major contributions for the reshaping of the field: Kuhn’s emphasis on the history of science, and Quine’s criticism of the analytic-synthetic distinction. In the second part are examined various positive proposals towards a “naturalized” philosophy of science which come from biology, evolutionary epistemology and cognitive science, i.e how it is actually done (“Doing it”). Most of these sections of the book deal with biology, the interpretation of Darwinian evolutionary theory and its present changes, and examine such topics as the nature of fitness, the unit of selection controversy, sociobiology, and the metaphysical output of contemporary Darwinism. A single important chapter is devoted to what may be the most important epistemological output of evolutionary biology, namely evolutionary epistemology. The remaining chapters deal with the way in which cognitive science models, from classical to connexionists one, can, together with current research in neuroscience, shed light on the analysis of natural and scientific knowledge. The final chapter deals with even broader topics such as the impact of

evolutionary theory on ethics, and in general the impact of science on society, as well as with the consequences of a feminist approach to these issues in the sociology of science. The book contains also extensive bibliographies, indexes, and lengthy footnotes, where the editor gives details about views only sketched in the conversations. Not all the sections are of equal interest, and given the mode of composition of the book, it is sometimes repetitive. Depending upon the reader's interests, some will find certain sections more interesting than the others. If I myself were to choose, I would say that I found that Wimsatt's reflexions on the problem of reduction and functional explanation, Kitcher's, Hull's, Ruse's, Sober's and Lewontin's views on the philosophy of biology are among the most illuminating, and Latour's reflexions the most irritating (see below).

Because these topics loom so large, and their treatment is so tied to the idiosyncrasies of each participant, it is impossible here to deal with all of them and to do full justice to the details of the views presented in the book. I shall try instead to discuss them through the framework of the various strands in naturalism mentioned above, with the hope of clarifying what exactly the commitments to naturalism are, and whether it can be a unified doctrine. As the classifications hinted at above and the variety of the views expressed by the participants to Callebaut's dialogues suggest, the answer to this last question is likely to be negative.

Let us take the various naturalistic stances in the reverse order, starting from the *constructivist* one. As it is usually characterized, the move away from positivistic philosophy of science since the sixties has led philosophers into two opposite directions. On the one hand the historical bias that philosophers of science have taken following Kuhn (and discussed here in the first chapter) has led them to

reject not only the idealised and history-free analysis of scientific statements attempted by the positivists, but also their reduction of the theoretical vocabulary of scientific theories to the observational vocabulary. They have emphasized the theory-laddenness of scientific terms, and accepted various versions of what is called the “meaning variance” conception of scientific theories, according to which terms like “electron” do not have the same meaning within each theoretical framework of theory. This has obvious *anti-realist* implications: the reality described by science is relative to each “paradigm”, and because paradigms are “incommensurable”, the reference of theoretical terms, which is determined by their variable meanings, is never unified nor fixed. On the other hand, a number of philosophers following Putnam (see here Shapere, p. 48-50) have reacted against the positivistic doctrine that the reference of scientific terms is determined by operational meaning-criteria, and have claimed that their reference is largely independent of their meaning, and therefore transtheoretical. This has obvious *realist* implications. Now those of the “naturalists” interviewed here who come from the most historically and sociologically oriented circles, such as Longino, Knorr Cetina, and especially Latour, when they emphasize the theme that science is but a social construction, that the making of scientific theories does not follow the Mertonian path of a community of researchers driven by an ideal of truth and responsibility, but is more the product of social forces and possibly of male biases, are certainly closer to the anti-realist theme. They remind us of the subjective, non progressive and arational character of scientific change. They are aware that their views may sound a bit too Feyerabendian, and so try to correct this impression by admitting some match between science and reality, but it is often hard to see how they differentiate themselves from full-blown relativism. Thus Longino (p.26-27) says that “scientific inquiry can’t be held to result in true theories or to be about some domain of reality totally independent of human, and mutable conceptual



schemes.” She is prepared to admit a certain amount of “match” between our descriptions and natural processes, but claims it is so only because it is “local”, and because “it works”. Knorr Cetina says roundly that “scientists do not interact with the world directly; but with ...what other scientists have said about the world.” (p.183) Latour is both bolder by admitting that he, like “the French”<sup>1</sup>, is convinced that “the facts don’t speak for themselves” and “theory is always there” (p.65), and more subtle, or more cautious, when he claims that his description of scientific practice is not meant to “expell rationality” (p.118), but that he wants to describe knowledge in general from a standpoint which would be beyond rationalism and anti-rationalism, neither modern nor post-modern.<sup>2</sup> No wonder that Latour is reluctant to call himself a naturalist. What is particularly baffling is the fact that such writers try systematically to escape such commonsensical questions as: why is it that science works so well? could it be successful if it were not true? Could

---

<sup>1</sup> Latour feels obliged, often pressed by Callebaut, to represent the “French” point of view on the philosophy of science, and to be the spokesman for the views of Bachelard, Canguilhem, Foucault, etc.. But it’s unlikely that we take his word for that.

<sup>2</sup> In general, most of the interventions by Latour bear the mark of his obscurity and, to say the least, sheer conceptual confusion, and contrast in this respect with the others. The last word upon him may well be Kitcher’s, who, after quoting King Lear ( “I shall do such things/ I know not what they are, but they shall be/ the terror of the earth”) : “ I often get the same feeling when I read Latour. It’s wonderfully unentertaining, but the talk of a new program in which philosophy of science has withered (Why?) and traditional philosophy (What traditional philosophy?) enjoys a new relationship^(What new relationship?) with empirical studies seems to me like emphatic advertising from a master salesman.” (p.219) . No

Latour sit on his chair in front of his computer at the Salk Institute if all the technology made possible by science were only the product of a jumble of political tricks performed by tribes of laboratory workers willing after power?

Not all of those who emphasize the historical turn, however, commit themselves to these extreme forms of anti-realism and constructivism. Burian (p.46,187-189) and Shapere (p.67-68, 175), for instance, are both willing to accept a transcendence of reality with respect to inquiry together with some form of relativity of the reference of scientific terms. But most of the people interviewed here who advocate some form of biological account of knowledge, or some form of epistemology based on the findings of cognitive science, are naturalistic *realists* (Giere, p.170; Kitcher,p.129, Hull, p.316, 284, Wimsatt,p.173). It is not difficult to understand why. Realism seems to need naturalism, for if one insists that only realism — the thesis that our theories are true or approximately true— can explain the reliability and success of science, and if this reliability is not guaranteed *a priori* , but discovered by empirical means, then we need to be committed to a naturalist account of knowledge, which would compare, through the descriptive means of natural science, the kind of reliable processes that we have. The converse implication seems to hold as well, since if our scientific knowledge is the product of causal (biological, psychological) natural processes which are in general reliable, then the causal relation has better be a relation between our cognitive equipment and objects which are out there in the world, and which we can know. Moreover, a Darwinian naturalized epistemology seems to be perfectly fitted to the view that scientific knowledge, which has evolved, is cumulative and progressive, and hence that constructivism is false. In different ways, this what Popper, most notably, but also Campbell and Kitcher claim. The equivalence between a realist and a naturalist

---

wonder that Sokal could integrate a number of Latour's declarations in his famous "hoax".

view of science, however, is not guaranteed. For one may be an anti-realist, and reject, for instance on the grounds of the “pessimistic induction” from the past failures of science, the view that science is a cumulative approximation to the truth, while at the same time accepting the view that we cannot explain the successes of science otherwise than by empirical means. Thus Larry Laudan is, in this sense, an anti-realist *naturalist*, or, as he calls himself, a “normative” naturalist.<sup>3</sup> Still, it does not follow, for Laudan, that constructivism and relativism are true, and indeed he rejects these views.

Such considerations lead us to examine the claim, put forward by many naturalists, that their investigations are purely *descriptive* of scientific practice. For if such a claim were taken at face value, analyses of scientific knowledge should ban all normative considerations about the rationality or non rationality of science, or about whether science justifiably matches an independent reality. This purely descriptive aim of naturalized epistemology was explicitly endorsed by Quine (1969) when he claimed that the only work left for epistemology was to examine the relationship between the “meager input” of external causes on our sensory apparatus and the “torrential output” of scientific knowledge. This implies, Quine says, that there should be no place left for traditional epistemology as an investigation upon the nature of knowledge, or for an attempt at his definition. A number of participants here seem to admit this consequence: Campbell (p.294), Bechtel (p.352), Giere (p.293) for instance. Patricia Churchland too (p.16), who is

---

<sup>3</sup> Unfortunately, Laudan is not a participant to these conversations, also he often gets cursory references from the other actors of Callebaut’s play. On his views, and for an illuminating account of the various relationships between naturalism and realism, see in particular Rosenberg 1996.

most famous for her and Paul Churchland's eliminativism, denies that we need any account of the *concept* of knowledge. But it is not evident that this naturalist descriptive stance has completely undermined all normative accounts for knowledge. A number of writers who advocate this point of view still seem to feel obliged to use such notions as justification. Campbell, for one, accepts that we should "pass the justificatory buck to biology": but what kind of buck is it? Moreover, not all naturalist here agree that we can dispense altogether with normative questions (see especially Hull, p.99, on the limits of the descriptive view; Richards, p.439: "Even as an epistemological naturalist one must be concerned with problems of justification"). There is a good reason for that, which comes often as a major objection to Quine's version of naturalized epistemology: if all there is to epistemology is a purely scientific account of knowledge, this account will be provided by science, but what will, in the end justify our confidence in science itself? So the account is to be circular. Even if one answers that the circularity is not vicious, and that *science justifies science* (Neurath's ship), this pragmatist answer will hardly be free from use of the very notion of justification. A number of contemporary naturalistic epistemologists are quite aware of this, and do not think that their attempt to build "reliabilist" theories of knowledge can dispense them with a normative inquiry about the definition of such notions as knowledge, justification, and rationality. In particular Goldman (1986), who is a notable absent from these conversations, does not think that epistemology has ceased to be a normative enterprise by becoming naturalistic. One could also quote other naturalizers, like Dretske (1996), who are ignored in this book, who do not think that the *philosophical* problems of causal theories of intentionality and of knowledge can be evaded. Bechtel (p.352) is scornful of such approaches, which he finds too "philosophical" and he himself pretends to be interested only in the way "cognitive systems work to produce reliable information", and not in how we

could differentiate belief from knowledge. But by what criteria does he counts “representations” as beliefs or as knowledge, and as reliable? He does not say. As it is often remarked, Churchland’s eliminativism balks at the fact that, in spite of her rejection of “folk psychological” notions such as beliefs, she certainly has to *believe* her own theory. The same is true of naturalism itself: is there a naturalistic account of the naturalistic thesis? This sheds some doubts about the pretensions of many of the interviewed writers to be doing science and to dispense with philosophical accounts of knowledge. The naturalist’s discovery how all the beauties of science and of the way their undermine *a priori* and conceptual analyses is certainly a good thing, and much of the conversations here reveal how exciting it is for these explorers, but often the conceptual lightness of their accounts calls for philosophy’s revenge.

Fortunately, more philosophical moments are not absent from the book. They occur in particular in the conversations with Wimsatt, Sober, Elster, and Hull on such fascinating topics as the problems of reduction and functional explanation, and of the nature of selection and evolution. I gather that the latter, and the very interesting considerations on the sociobiology and creationism debates made by Kitcher, Levins, and Lewontin are likely to attract most attention from the readers of this journal, but I lack the space and competence to deal here with them appropriately. One very important theme which emerges from these passages of the book is the utter complexity of the issues raised by Darwinism today, and how much it should invite to prudence those who are tempted by evolutionary accounts of knowledge, morality, society, language and culture. In addition to leading his battle against the uses and abuses of sociobiology, and against “adaptationism”, Lewontin (1989) has reminded us how little we know about the evolution of cognition, and Sober (1981, 1995) how

difficult it is to extrapolate from Darwinian considerations to claims about the evolution of human rationality. The main problem here, for the partisans of an evolutionary epistemology, is that we lack a proper understanding of what would be the “missing link” (p.403) between evolutionary theory for genes, organisms, and populations on the one hand, and for ideas, beliefs, theories, and knowledge in general. We might start things from bottom to top, and analyse ideas and beliefs through genetic models (as in Dawkins’s “memes”) or through some “epidemiology of representations” (Cavalli-Sforza, Sperber). But this has notorious shortcomings: Memes, if they can be isolated, are just too unspecific to account for the contents of particular beliefs. We could, on the other hand, start from top, and envisage the missing link to be an evolutionary *psychology*. But here too we are far from an understanding of the applications of evolutionary biology, as it is understood for lower levels, to specific mental processes at higher levels: presumably evolution has selected, for instance, the cognitive mechanisms by which humans reason, but it is quite another thing to say that it has selected the specific rules of inference that they use (Lloyd here has interesting remarks about Cosmides and Tooby’s program of evolutionary psychology, p.409, see also Engel 1997). The same can be said about attempts at understanding morality through the theories of reciprocal altruism. The lesson that Lewontin, Wimsatt, Hull, Sober, Richards and Callebaut himself agree to draw from these difficulties is that naturalism, evolutionary or otherwise, has to be *anti-reductionnist*, and abandon the hope of fitting the mental, cultural and knowledge facts into a single an unified account from top levels to bottom levels (see in particular p.147-168). None of the participants espouses the extreme thesis of physicalism (not even of biologicalism) according to which all the cognitive facts that there are can be explained completely through reduction to the bottom levels (and biological facts to chemical facts, and then to physical facts). But granted that no such reduction is

forthcoming, the hard problem is: for the naturalistic thesis to have some bite, one must at least agree that, although they are autonomous from them, higher level facts must nevertheless *depend upon* some sorts of naturalistic lower level facts. Sober (p.151-154) appeals to the philosopher's notion of *supervenience*, which he illustrates with the notion of fitness: when fitness varies in a set of organisms, there is a physical explanation of why one is fitter than another (for instance one organism has stronger legs than another, and is thus able to escape from predators more quickly), but there is no general physical property common to all the organisms which explains why they are fit. This is in line with the functionalist theory about the mind-body relationship: mental properties can be characterized at the functional level, but they are not uniquely "realizable" in physical or other lower level properties. Here again, not all protagonists agree. Some, like Rosenberg and Wimsatt (p.153) say that supervenience is only a "metaphysical solution" to the reductionism problem, which allows us to give a characterisation of higher-order properties with respect to a set of properties determined by "an apocalyptically complete" lower level theory, but that it cuts no methodological ice, because it does not tell us what *kinds* of non- reductionist explanations we can get in particular cases. Wimsatt prefers to talk in terms of levels of organisation, Lewontin in terms of "quasi-independence", and Bechtel (p.163) in terms of Darden and Maull's notion of "interfield" theories. Presumably Churchland, the chief neurophilosophical materialist (cf. p.366) is in more reductionist spirits, contrary to Elster (p.137-143) who advocates the autonomy of functional and intentional explanations from causal explanation.<sup>4</sup>

---

<sup>4</sup> p.482-83, Callebaut mentioning a paper of mine (Engel 1992), where I defend something close to the supervenience option by distinguishing ontological physicalism from explanatory physicalism (reductionism) , and by accepting the

These difficulties about reduction illustrate the tensions inherent to the epistemological or explanatory stance of naturalism. Similar tensions affect the ontological stance. It says, in a nutshell, that all the facts that there are in the world are natural facts, and that there are no other entities than those that natural science posits. But unless one specifies what kind of natural facts (physical? quantum mechanical? neurophysiological? biological?) are supposed to exhaust this complete description, and what kind of entities are supposed to be excluded from the list (mental entities? moral entities? norms?), this slogan is hardly informative. It depends on what you put in “nature”. Of course, as Stroud (1996) remarks, it excludes at least explanations and facts having to do with a *supernatural* order, of the kind that classical philosophers like Descartes or Berkeley appealed to when

---

former but not the later, objects that “this solution hinges on [the] identification of naturalism with physicalist and materialist eliminativism and of naturalism with causal explanation, which ...is not warranted in general.” I do not see why. The fact that once the physical facts are fixed, all the other facts are does not entail that we can reduce all our biological, psychological, etc. explanations to physical and to causal ones. I agree, however, with Rosenberg and Wimsatt that supervenience only gives us a sort of general metaphysical picture which does not solve the difficult problems about explanation. But I agree that the supervenience option is subject to the characteristic tension between reductionism and anti-reductionism, as Kim (1993) has argued. Another, close to that of supervenience, which is not mentioned in the book is that of *emergence*. It was present in the XIXth century discussions of evolution, but it has obvious spiritualistic flavours which are not congenial for contemporary naturalism. It does not follow, however, that it would not be useful to



they tried to secure our knowledge of the external world by the warrant of a benevolent, omniscient, and omnipotent God. At least this is not something believed by the contemporary naturalists. But then everybody today is a naturalist in this sense. Even if we grant this, naturalism is subject to a characteristic tension, which is but a variant of the preceding one about explanation. On the one hand, if you allow *too much* to figure in the scope of what you call nature and of what is amenable to some form of naturalistic explanation, you extend the realm of natural facts at the price of triviality. For instance if you allow sociological or moral facts to be natural facts in this sense, there is not much left outside of nature for you to be a “naturalist” in the ice-cutting sense. On the other hand, if you allow *too little* to belong to nature, you exclude too much, at the price of making our ordinary practices and judgements implausible. For instance, if you say that there are really no colors in the world, you fail to explain how people can react, and make judgments about coloured objects; if you say that there are really no values, but only psychological emotions or expressive states of people in a value-free world, you fail to account for our evaluative attitudes and for the fact that we do distinguish between evaluative and descriptive statements<sup>5</sup>. The same is true, of course, of beliefs and other mental states, and for mathematical entities. As we saw above, the eliminative materialist is a pains explaining how and why she *believes* her own theory. As Stroud remarks, “one thing which seems not have been

---

compare the present debates with the previous ones in the light of this notion.

<sup>5</sup> These problems are familiar from contemporary philosophical work on meta-ethics. In general, discussions of ethics and proposal at evolutionary account of ethics in this book are quite blind to such issues. But this is another respect in which a little drop of conceptual analysis would be usefully added to the clouds of scientific information.

‘naturalized’ is naturalism itself” (*ibid.* p.43). This was the point that I tried to put forward above when commenting upon the commitments of the naturalists. If they insist that their epistemology is completely norm-free, and that it is useless to ask questions about scientific justification and rationality, they will end up with a theory which fails to account for the most ordinary judgments that we express about our ordinary and scientific beliefs. This, it will be said, is just what the reductionist predicament leads us to. A less reductionist attitude, one of “r-e-s-p-e-c-t”, as Callebaut says, quoting Aretha Franklin and Otis Redding, is called for. But then this non reductionist attitude must also avoid the pitfall of being too tolerant and of including too much. Stroud himself advocates a form of “open-minded” or “expansive” naturalism according to which “we must accept everything we find ourselves committed to in accounting for everything that we agree is so and want to explain.” (*ibid.*p54). But as he notes, this naturalistic thesis does not amount to anything very substantive or controversial. Between the Charybdis of reductionism and the Scylla of triviality, the naturalistic path thus appears to be much narrower than what most of the discussions of this book suggest. But in addition to being one of the best introductions available to these debates and to the rapid changes in the philosophy of evolution and in the philosophy of science of the recent years, it is one of the virtues of this book to make us think twice about these issues.

## References

- Dretske, F. (1996) *Naturalizing the mind*, MIT Press, Cambridge Mass.
- Engel, P. (1992) “Le rêve analytique et le réveil naturaliste”, *Le débat*, 72, 104-114
- (1997) “Normes logiques et évolution”, *Revue Internationale de philosophie*, 2, 201-219
- Goldman (1986) *Epistemology and Cognition*, Harvard, Harvard University Press
- Kim, J. (1993) *Supervenience and mind*, Cambridge, Cambridge University Press
- Kitcher, P. 1992, “The Naturalists Return”, *Philosophical Review*, CI, 52-114
- Laudan, L. 1987 “Progress or rationality? Prospects for normative naturalism”, *American Philosophical Quarterly*, 24; 19-31
- Lewontin, R. (1989) “The Evolution of Rationality”, in Osherson, D. & alii, *an Invitation to Cognitive Science*, vol. 3, MIT Press, Cambridge Mass.
- Quine, W.V.O. (1969) “Epistemology Naturalized”, in his *Ontological Relativity*, New York, Columbia University Press.
- Rosenberg, A. (1996) “A Field Guide to Recent Species of Naturalism”, *British Journal for the Philosophy of Science*, 47; 1-29
- Sober, E. (1981) “The Evolution of Rationality”, *Synthese*, 46; 95-120
- (1994) *From a Biological Point of View*, Cambridge, Cambridge University Press
- Stroud, B. (1996) “The Charm of Naturalism”, *Proceedings and Addresses of the American Philosophical Association* 70, 2, 43-55