

INTRODUCTION

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When the Vienna Circle, in their pamphlet *Wissenschaftliche Weltauffassung*, solemnly professed that “this must be certain: there is no such thing as philosophy as a basic or universal science alongside for above *the various fields of the one empirical science*” (1929/1973, p. 19; italics mine), they intended to leave no room for special “philosophic” assertions. Yet to their well-known tenet that “there is no way to genuine knowledge other than the way of experience; there is no realm of ideas that stands over or above experience”, they immediately added the qualification: “Nevertheless, the work of ‘philosophic’ or ‘foundational’ investigations remains important in accord with the scientific world-conception” (ibid.). Logical or rational *reconstructions* (in Carnap’s later terminology) of theories etc. they took to be the philosopher of science’s agenda rather than a properly *empirical* study of science as “a set of visible phenomena” (Barnes & Edge, 1982, p. 3) as envisaged by present-day sociologists of science. For only reconstructions, so it was thought in the heyday of Logical-Empiricism, would allow the philosopher to overcome the inhibiting prejudices regarding concepts, theories and methods that tend to come with day-to-day scientific practice and thinking.¹

With the benefit of hindsight we can now perceive clearly the ambiguities of the Logical-Empiricists’ programme. Their major project, the (re)construction of a Unified Science, we can safely say at present, did not deliver the goods it was intended for, despite all the efforts that were devoted to it. The many disagreements over the reduction of Mendelian genetics to molecular biology, the most thoroughly investigated case of an alleged theory reduction to date (see, e.g., Wimsatt, 1979, Brandon & Burian, 1984, and Sober, 1984, for an overview), illustrate this failure more poignantly than anything else.² While it would be historically grossly incorrect to claim that

Carnap's physicalism, microreductionism (Oppenheim & Putnam, 1958) and related doctrines have *engendered* an intellectual climate which left room only for a philosophy of (certain parts of) physics — and saddled many a scientist or philosopher working in or on a different area with “physics envy” —, they have certainly been used — and abused — to *consecrate* this pre-existing quasi-monopoly, which was broken in the 1970s only.

The famous zoologist Ernst Mayr once commented on the effects of this situation in these terms :

I have some five or six volumes on my book shelves which include the misleading words “philosophy of science” in their title. In actual fact each of these volumes is a philosophy of physics, many physicist-philosophers naively assuming that what applies to physics will apply to any branch of science. (Mayr, 1969, p. 197.)

Anyone acquainted with biology and biologists is familiar with complaints to the extent that few, if any, thinkers of analytic persuasion have escaped this generality trap. But until recently, matters were even worse. As David Hull has keenly observed (and this takes us back to the reconstruction versus observation issue mentioned earlier), only on rare occasions did “a little science” creep into philosophy of science, which amounted to little more than a branch of armchair epistemology : “Most of the work in philosophy of science has nothing to do with science of any kind” (Hull, 1979, p. 421) — a candid remark which, six years later, alas, remains valid to a large extent. Again the reductionism debate, where some of the advocates of the classical model of reduction have felt free to reconstruct historical episodes *ad libitum*, provides a good example. More generally, the main trouble with the “received view” seems to be that the amount of complexity one has to introduce to make small improvements has by now far outgrown the benefits (in terms of improved understanding etc.) one can reasonably expect.

Thus fundamental reworkings of the very foundations of the “received view” were — and to a large extent still are — in order. This is not the place to assess in any detail the impact (being) made by the various “waves” of empirical information to have washed over philosophy of science in recent times (to use Tom Nickles' happy phrase) : the *historical wave* (Thomas Kuhn and others), the *socio-logical wave* (constructivism, the Edinburgh and Bath schools, etc.),

and, most recently, a *third wave* characterized by the entirely new phenomenon of philosophers (such as Ronald Giere and David Hull) engaging themselves in empirical studies of scientific work as it is “really” practiced *in situ* (Nickles, 1984 and personal communication). I only note here that one result has been that the boundaries between science and its philosophy — or rather, between the *various sciences* and their *various philosophies* — become more and more blurred. In order to do the philosophy of a field well, one must understand *at least* the basics of this field well (and preferably more). On the other hand, good science will very often be quite “philosophical” in essential ways (though many scientists continue to dismiss this).

Along with cognitive psychology, biology — and especially evolutionary biology — is probably the area where the new symbiosis of science and philosophy is most advanced. Not entirely incidentally, much research undertaken in both areas is of a *strategic* nature.³

Whereas some of the papers gathered here are of a more traditional kind, most of them bear witness to this new development. In “Why the panda provides no comfort to the creationist”, Richard Burian challenges a view commonly held among philosophers of science, according to which “scientific creationism” is unfalsifiable (and therefore unscientific). Burian argues that quite to the contrary, the central claims of creationism can be shown to be *falsified*.

David Depew’s paper is an incisive commentary on Wiley & Brooks’ recent proposal to link biological and physical evolution by way of nonequilibrium thermodynamics (which goes counter to Prigogine’s view that thermodynamics does not offer a causal and explanatory principle rival to natural selection). It is a nice example of the “anti-Newtonian”, anti-reductionist (in the traditional sense) style of thinking which characterizes many “post-post-positivists”. Depew also explores the implications of the “methodological pluralism” he advocates for our views on the linkage between (theories about) biology and human culture.

One expression of the new methodological pluralism is the so-called *semantic view of theories*, developed by a number of authors from the late 1950s on. On the semantic view (if I am allowed to grossly simplify it for brevity’s sake here), “theory” and “reality” are not to be connected at the level of “laws of nature”

(held to be universal); they can only be put into contact with each other through the use of *models*, i.e. more or less ideal system descriptions which in turn are more or less realistically interpreted according to how well they match particular facts. The next three papers deal with the semantic or "non-statement" view of theories. Both Richardson and Thompson contrast it with the classical, deductive-nomological form of explanation due to Hempel and Oppenheim, and show how the latter breaks down when one tries to apply it to even the simplest cases from evolutionary biology. Robert Richardson points to the unrealistic character of some of the assumptions made in common population-genetic models (as used in, e.g., sociobiology), and offers an illuminating discussion of adequacy conditions to be imposed on theories and models. Paul Thompson applies the semantic view to Michael Ruse's sketch (cast in traditional terms) of the axiomatic structure of population genetics as well as to Mary Williams' widely acclaimed axiomatization of the theory of natural selection. He argues that both proposals are inadequate, and that any adequate account will have to incorporate the *interaction* of the theories of heredity and of natural selection, which was typically neglected in the past. Finally, Elisabeth Lloyd convincingly shows how the semantic approach can be used to delineate "differences concerning the scope of application" from "differences in the description or specification" of ideal systems, and illustrates this with a number of cases, again taken from population genetics (the Hardy-Weinberg equilibrium and various selectionist/adaptationist debates).

A perennial issue in the philosophy of biology (if one may use the expression) concerns the ontological status of species. As has been shown by Michael Ghiselin and David Hull in particular, it took a very long time before biologists began to realize that in order to succeed, the Darwinian revolution required a fundamental rethinking of the metaphysical underpinnings of evolution. Hull is certainly the most eloquent spokesman for the radical position that species have to be viewed as individuals (in the logical sense), not classes — a view akin to (though not entirely identical with) what Mayr has termed "population thinking". Kristin Guyot argues that although the hypothesis that species are individuals is rich in biological import, its justifications have been superficial. She raises a number of objections to it which support the claim that the hypothesis is false, yet utterly useful. Again we get a flavor of the level of sophistication reached in this area of philosophical thinking.

Maturana & Varela's work on *autopoiesis* has been widely acclaimed as a fresh start in our thinking on the relationship between living systems and processes of cognition. Srdjan Lelas' paper critically assesses some of the claims made by these authors from a non-foundationalist, naturalistic epistemological perspective. He advocates an interactionist epistemology which would allow us to get rid of the "subject-object polarization" and the "mirror imagery" he takes to be responsible for the notable lack of progress in our understanding of human and animal cognition.

To conclude, Andy Clark, a philosopher influenced by Michael Dummett's brand of anti-realism, explores the implications of the adoption of the naturalistic stance (cf. Quine) for epistemology as traditionally understood. He is in search of a position which would allow the evolutionary epistemologist to give up altogether the notion, taken to be untenable, of the *world-in-itself* (a step few advocates of evolutionary epistemology are willing to take at present), whilst resisting Rorty's alternative anthropocentric description of reality as whatever *human beings* can agree at a given time exist. It should be noted that a growing number of philosophers of biology feel that only an evolutionary framework applied to (scientific) cognition itself can do justice to their epistemological intuitions. As a result, philosophy of biology and evolutionary epistemology are more intimately related than an outsider might expect (see, e.g., Depew & Weber, 1984).

A number of "hot" topics in the philosophy of biology (e.g., the nature of functional organization, or the units of evolution controversy) have not been discussed or have only barely been touched in this collection of papers. Nonetheless I think we can say that the reader is offered a fair sample of the type(s) of philosophical (and scientific) research currently undertaken under the label "philosophy of biology", a field which (as so many other fields of philosophical investigation) is rapidly being professionalized. It is up to her or him to decide whether the benefits of this trend outweigh the costs (my bet) or *vice versa*. History holds many ruses in its bag of tricks. The return, "after a metaphysical interlude, to a unified picture of this world which had, in a sense, been at the basis of magical beliefs, free from theology, in the earliest times" (*Wissenschaftliche Weltauffassung*, 1929/1973, p. 19), an ideal first advocated, then unwillingly betrayed by the Logical-Empiricists, might well be approached better *this* time by *this* roundabout — provided in really succeeds in overcoming the gap between Science

and Philosophy which, after all, is only a few centuries old.

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NOTES

¹ For a thoughtful discussion of the history of the notion of "rational reconstruction" and its usefulness in current, "historicized," approaches, see Mittelstrass (1980). It is rather ironic that this kind of "return to the given" should be advocated by ultra-relativists of the Edinburgh school !

² I would not like to be misunderstood here. I am not claiming that the classical account of reduction is false (nor that it is true), but that the important issues at stake cannot possibly be cast in terms of such a simplistic dichotomy. In this sense, the anti-positivist Jürgen Habermas was right when he declared in an interview, a couple of years ago : "Rien n'est aujourd'hui moins acceptable que, par exemple, la distinction normative accordée à une science unifiée, le concept de l'*unified science*" (*Le Monde*, 5—6.8.1984).

³ "Strategic research : Basic research carried out with the expectation that it will produce a broad base of knowledge likely to form the background to the solution of recognised current or future practical problems" (Irvine & Martin, 1984, p. 4).

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