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PIAGET AMONG THE EVOLUTIONARY NATURALISTS, ANNO 1995

Werner Callebaut

ABSTRACT

Piaget's "monumental genetic approach" (Marc De Mey) to cognition is arguably both a naturalistic and an evolutionary research program in most (but not all) pertinent respects. Yet the impact of genetic epistemology on the evolutionary and naturalistic epistemologies and philosophies of science which are becoming dominant today is negligible. In the paper I attempt to clarify the scientific status and assess the reception and the relevance of genetic epistemology from the perspective of evolutionary naturalism. I also offer a partial explanation for the discrepancy between its undeniable relevance and limited actual impact on science studies.

Jean Piaget is universally recognized as "the most influential theorist in the history of developmental psychology and the study of human development" (*New Webster's Dictionary and Thesaurus of the English Language*, 1992). This paper deals with a different (though related) and somewhat controversial aspect of Piaget's scientific legacy: (i) the scientific status of genetic epistemology (GE) as Piaget's own peculiar brand of *evolutionary naturalism*, and (ii) the question of GE's relevance to and actual influence on contemporary evolutionary and (neo)naturalistic epistemologies and philosophies of science. I will assume here without discussion that the latter are becoming dominant on the philosophical scene today, at least in the United States.[1]

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[1] The title of Elliot Sober's recent volume, *From a Biological Point of View: Essays in Evolutionary Philosophy* (1994), which was intended to contrast with W. V. Quine's collection *From a Logical Point of View* (1953), epitomizes the shift from the logic-oriented and ultimately overformalistic approach to science that characterized the "received view" (Callebaut 1993: Ch. 2) to a philosophical approach that is closer to and

1. *From traditional epistemology to neonaturalistic philosophy of science*

For traditional epistemologists, the central tasks of epistemology were, in Susan Haack's words, the *explication of epistemic concepts* and the *ratification of criteria of justification*. They took it that these tasks are to be undertaken, "not by any kind of empirical investigation, but a priori" (Haack 1990: 199), on penalty of vicious circularity. This stance hinges on the plausibility of anti-psychologism, which allowed Frege to break with the older naturalistic tradition and Popper to confine his evolutionary epistemology to his World 3 of objective knowledge, excluding the knowing subject (Sober 1978; Notturno 1985; Kitcher 1992). Thus Hamlyn (1971) regards Piaget's claim that empirical facts are relevant to philosophical conclusions as a gross confusion.

Naturalists, on the contrary, suspect that the obvious lack of success of the "Euclidian" or "Cartesian" program of justifying (scientific) knowledge without appeal to any — even minimally — scientific premises, may ultimately be due to "the impossibility of the task" (Giere 1988: 11). They tend to play down the problem of vicious circularity (Callebaut 1993: 209-210, with further references). Concomitantly, they share a commitment to the continuity of epistemology and, most importantly, philosophy of science with science; but they differ among themselves over what form this continuity should take (Maffie 1990; Callebaut 1993).

As to evolutionary epistemology (EE), I will provisionally define it here as any epistemology "taking cognizance of and compatible with man's status as the product of biological and social evolution" (Campbell [1974] 1988: 393). This definition is not committed to acceptance of the central tenet of neo-Darwinism according to which "natural selection is the only known explanation for adaptation" in biology (Ridley 1993:

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continuous with the sciences and must, therefore, be informed by the history and sociology of science as well. As an *epistemological* naturalist, Quine, whose critique of the logical empiricists' strict analytic-synthetic distinction the Geneva school considered congenial to their own constructivist enterprise (Leo Apostel, personal communication), was of course himself instrumental in bringing about this change. How substantive this change is remains to be seen; for, as Sober remarks, "in spite of this change in the metaphilosophical landscape, much of philosophy proceeds as it did before" (p. 2), viz. by conceptual analysis.

323ff.), and thus leaves room for non-Darwinian and even Lamarckian evolutionary explanations (which many authors deem relevant to human *culture*). Defined this way, EE unambiguously includes Piagetian GE. The rather complicated issue of the exact nature of the relationship between GE and EE will be discussed in section 3.

Notice that (i) EEs may or may not be naturalized, (ii) not all naturalistic epistemologies are evolutionary, and (iii) naturalistic approaches may or may not be compatible with the traditional epistemological program (Haack [1990] refers to the first variety as "reformist naturalism" and to the second variety as "revolutionary naturalism"; cf. also Callebaut [1995a]). Let me illustrate this by some examples.

Ad (i): For instance, varieties of EE which rely on non-naturalized notions of rationality or intentionality, such as Karl Popper's EE or Nicholas Rescher's "rational selection theory", are non-naturalistic. If the hallmark of any naturalistic approach is the essential reliance on *causal-mechanistic explanation* (Callebaut 1993: section 4.1), it becomes imperative to ask whether the "mechanisms" GE appeals to in order to explain cognitive development (see section 3) are truly mechanistic or rather point to an "organismic model of man" (cf. Kesselring 1994: 299; Atkinson 1983: 16). An interpretation of GE along cybernetic lines — more specifically, in terms of a dynamic self-regulatory systems approach (Hahlweg and Hooker 1989; Hooker 1994a, 1994b; cf. already Cellérier, Papert and Voyat 1968) — seems most promising in this respect.

Ad (ii): Naturalistic epistemologists or philosophers of science who distance themselves from any EE worth the name (although not necessarily from Quine's dictum that there is "encouragement in Darwin") abound. Alvin Goldman (1986: 2, 149; 1992), who attempts to assimilate the cognitive sciences in a fairly traditional epistemological framework, and Philip Kitcher (1993: 300ff.) are prime recent examples. As to GE, if GE would not belong to EE, GE models — most notably Piaget and Garcia (1983) — "would simply be examples of a naturalistic epistemology according to which the study of the evaluation and history of science must itself be a science", as Apostel (1987: 311) puts it.

Ad (iii): Varieties of EE which accept the legitimacy of epistemological questions framed against the background of the "justified true belief" tradition but repudiate apriorism (in the sense of Haack), such as Konrad Lorenz's or Gerhard Vollmer's *bioepistemologies*, may be said to belong to reformist naturalism. And so do Donald Campbell's EE

(especially in its later elaborations, which emphasize normative and justificatory issues: see Callebaut 1993: 293-303, 469-470) or Goldman's (1986, 1992) individual and social "epistemics". Finally, philosophers such as James Bogen (1985), Andy Clark (1987) or Ronald Giere (1988) hold that (evolutionary) naturalism is inapt to counter Humean skepticism — traditional epistemology's main target — and point the way to a revolutionary naturalism that abandons the traditional epistemological program and replaces it by some scientific successor-subject.

2. *Genetic epistemology as a major contribution to the project of an integral epistemology*

A common, stereotyped assessment of GE drives a wedge between the Geneva School's theoretical and experimental investigations of psychogenesis — which, for Piaget, extends biological ontogenesis, viz. embryogenesis, albeit nonreductionistically[2] — and Piaget's "idiosyncratic" conception of biological evolution, viz. his "third way" in phylogeny "beyond Darwinism and Lamarckism". Proponents of this divided picture of Piaget's "monumental genetic approach" (De Mey 1982: 226) incite psychologists, philosophers and other students of cognition[3]
- to pursue GE, which they typically present as a useful ontogenetic complement to the phylogenetic focus of EE (e.g., Vollmer 1986: 38, 181, 185; Kesselring 1994: 300-309), but
- to abandon Piaget's "third way" in biology without regrets, as "a view that few thinkers acquainted with evolutionary theory are likely to take seriously" (Munévar 1989).

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[2] Hooker (1994b: 217-218 and passim) gives a reconstruction of "the fundamental hypothesis of GE" by specifying the nature of (i) the mutual relations between phylogenesis and ontogenesis and (ii) the mutual relations between "cognogenesis" (i.e. the evolution of public knowledge) and psychogenesis in terms of homomorphisms which specify "commonality of regulatory processes". In Hooker (1994a) he tries to go beyond this "first order" account of the mapping processes involved by taking into account the complex feedback and feedforward relations between populations and their individuals via the environment.

[3] Giere (1992) gives a good sense of the complicated nature of cognitive science as an interdisciplinary endeavor.

As far as I can see, the case against Piaget's biology — which, not incidentally, is usually badly understood by its critics — is less convincing than is often suggested. If a plausible interpretation of its core idea of *phenocopy* can be given (see sections 3 and 6), or, alternatively, if it can be shown that "the general regulatory framework and program of GE" stands, *irrespective* of the ultimate fate of the phenocopy theory, as at least one author has argued convincingly (Hooker 1994a, 1994b), then *this* rationale for the divided picture of GE disappears. But even independently of the outcome of the debate on Piaget's biological views I want to reject this "schizophrenic" stance. My reasons are partly immanent to the Piagetian enterprise as I perceive it and partly a response to what I regard as the curse of *postmodernism*, which I use here as a catchword for much of the irrationality and irrationalism that characterize our current *Zeitgeist*. I briefly discuss both points in turn.

It may be useful to recall that Piaget was a naturalist not only in the philosophical sense of an advocate and practitioner of "the doctrine that scientific laws are adequate to account for all phenomena" but also in the biological sense of "a student of natural history" (*Webster's Ninth New Collegiate Dictionary*, 1989). His admirers like to point out that he was a field biologist in his early youth (before the first World War), before turning to psychology, and then again at the end of his career (during the 1970s), when he investigated snails and plants in his native Switzerland to buttress his views on the phenocopy phenomenon. The *New Webster's Dictionary* specifies that the other naturalism is "a mode of thought (religious, moral or philosophical) glorifying nature and excluding supernatural and spiritual elements". Those acquainted with Piaget's 1918 novel, *Recherche* (English summary in Piaget 1977) might claim that this restrictive notion of naturalism is inapt to fit the young Piaget's romantic *Weltanschauung*, impregnated as it was with Bergsonian vitalism and liberal Protestantism (Vidal 1987). To this I would reply that *Recherche* may also be read as testimony to the young Piaget's struggle to rid himself of the lazy manners of thinking which characterized much of the philosophy (and psychology, which remained very philosophical) of his

day in favor of an austere scientific, i.e. a naturalistic approach.[4] Paraphrasing a remark by Dennett, one might say that it must have taken guts to strike the naturalistic pose in the intellectual climate in which Piaget grew up, and that actually *doing* naturalized philosophy would prove even more difficult — as it still does today!

I can be fairly brief on the postmodernism issue here as I have commented on this conundrum elsewhere (Callebaut 1995a, 1995b). It seems to me that the failure of "the scientific attitude to life" to capture an increasing segment of the post-1968 generations' imagination[5] is at least partly due to the continued separation of "nature" and "nurture" in our world view, which postmodernism and social constructionism exacerbate even further by cultivating a picture of human cognition as radically contingent and subjective ("autonomous"). Another reason for the current demise of the rationality of science is surely the "thin", lifeless conception of rationality we largely owe to the positivists and empiricists. What is called for, it would seem, is an *integral epistemology* as envisaged by such authors as Hooker (1987: Ch. 7: "Understanding and Control: An Essay on the Structuralist Dynamics of Human Cognition"; cf. also his 1994a), Campbell (1988: Pt. 5) or Shimony (1992: Ch. 1). Ideally, such an epistemology should not "sever affect, cognition, and society", but, on the contrary, "[strengthen] their interconnections to the point where love of truth and love of humanity are seen as one", as Gruber (1982: 263-264) puts it solemnly.

It is no coincidence that this exhortion stems from a Piagetian, for it has been argued that GE is a prime candidate research program for such an integral approach to cognition and action. Recall that for Piaget, thinking (including its apex, *logical* thinking) was *interiorized action*, as Apostel (e.g., 1976-1977, 1980, and especially 1986) has reminded us

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[4] Cf. also his sour remark, in *Insights and Illusions of Philosophy* (Piaget 1965/1971), on the general public's tendency ("capital crime") to side with the whimsical philosophers against the methodical scientists. It may therefore come as a surprise that Atkinson (1983: 3) would hedge GE against criticism of its empirical robustness by stressing the "coherence and philosophical adequacy" of Piaget's theory, which she takes to be "logically prior to its testing for empirical reliability". Still, this move is actually in the vein of the older Piaget's own response to methodological criticism coming from empiricist quarters. Such is the irony of history!

[5] Gross and Levitt (1994) document and criticize this tendency well.

many times. Needless to say, the radically anti-reductionistic tendency within postmodernism and its cult of "différences" and "différance" is diametrically opposed to the integration envisaged here.[6] This tension is felt acutely in the study of cultural learning, where some researchers (mainly cultural psychologists) feel that "it is never appropriate to compare the behavior of people from different cultures, who live in worlds that are fundamentally different from ours" (Tomasello et al. 1993: 546). This "irreductionism" is matched by the conviction of certain cognitive ethologists that it is inappropriate to compare different species that live in even more distant worlds. On this view, each culture and each species occupies its own *Umwelt*, which is "incommensurable" to all other *Umwelten*. All that is left for scientists to do here is to describe these different worlds ethnographically or ethologically. If it became generally accepted, this attitude would kill science, at least as we know it. But the argument that is invoked to justify this attitude is bogus. Firstly, as Tomasello et al. rightly observe, "the presuppositions of the investigator are just as unavoidable in naturalistic description as they are in comparisons, or even in experiments for that matter" (ibid.; "naturalistic" is used here in the sense of "natural history"). More interestingly, they point out that the *reductio ad absurdum* of this argument is in its application to human development: "May we not make comparisons between different levels of child functioning because at each age or stage the child is in a different world — or even individuals in their own individual worlds?"[7]

3. *GE as evolutionary naturalism*

The unseasonableness and inappropriateness of the divided picture of GE becomes even more clear if one realizes how it is exactly that Piaget and his school elaborated the overall project announced in *Recherche*. For convenience, I will stick to the common habit of distinguishing between

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[6] Cf. also Michel Serres' (1993) recantation of his own earlier "irreductionism", to borrow a term from his student Bruno Latour.

[7] The possibility of such a radical incomparability has actually been envisaged, e.g., by authors working in the German school of radical constructivism; cf. also Parfit's (1984) discussion of what makes for the unity of a person.

the "psychological" and the "biological" aspects of GE, which I take to be justified to the extent that psychology cannot, according to the master from Geneva, be reduced exhaustively to biology. Piaget's own view of reductive explanation (see most notably "The Multiplicity of Forms of Psychological Explanation" [1963], in Piaget 1977: 746-766) may be summarized in terms of his concept of the "circle of the sciences" (see, e.g., Gillièron 1987).

Qua psychology, GE is concerned with "the development of thought from concrete to abstract, simple to complex and subjective to objective" as a "continuous series of transformations from simple hereditary reflexes to the operations of formal thought" (Atkinson 1983: 41). Human experience is initially subjective; but increasingly abstract and general structural concepts, gradually filiated from the structures discernible in the child's actions during the sensori-motor period, provide the form of experience which ultimately confers objectivity on human understanding. (Atkinson 1983, Engels 1989: ch. 6, and Hooker 1994 are good introductions to GE from the evolutionary-naturalistic perspective that concerns us here.)

Apart from equilibration, which may be viewed as a generic biological phenomenon (see below), the mechanisms Piagetians invoke to explain cognitive development are (i) "*abstraction réfléchissante*", i.e. a dynamized improvement on the static notion of equilibration, (ii) *decentration*, and (iii) *becoming conscious* (prise de conscience). These three core notions, Piagetians never tire of insisting, "are specific to human cognition and lift it out of the diversity of biological organizational processes" (Kesselring 1994: 299); yet the notion of "abstraction réfléchissante" was clearly inspired by the embryologist Waddington's *homeorhesis*.

Qua biology, GE comprises three or maybe four theoretical core notions:

(1) The theory of the *phenocopy* process, i.e. the genetic copying of a phenotypic adaptation ("Baldwin effect"), which seemed to Piaget ([1975] 1977: 812) "analogous, on the organic plane, to the replacement of the exogenous by the endogenous ... on the cognitive plane" (see section 6).

(2) The theory of phenocopy is part of a wider endeavor to extend the notion of selection so as to include *internal selection*, which Piaget relates to Baldwin's "organic selection" and to Waddington's "genetic assimilation". As a parenthesis, I should mention here, to correct the

common perception according to which Campbell's EE is "strictly neo-Darwinian", that Campbellian EE not only considers a hierarchy of levels on which Darwinian mechanisms operate (which allows to highlight the phenomenon of *vicarious* selection), but also places great emphasis on internal selection factors and mechanisms. Both these features are departures from neo-Darwinian orthodoxy and bridge much of the gap between Campbellian EE and the sort of evolutionary systems biology envisaged by, say, Hooker, which itself extends GE.

(3) Still more generally, Piaget places great emphasis on the *internal autonomy* of organisms. This feature was taken up and even more radicalized by the "autopoiesis" school of Maturana and Varela and is being extended to the sociocultural realm by radical constructivists à la Siegfried Schmidt (1987, 1992, 1994).

(4) Closely related to this is (the younger) Piaget's conception of an *equilibration* factor in development (which, as I said, he later relaxed), in addition to the factors maturation, experience with the physical environment, and experience with the social environment. Equilibration is neither genetically inherited nor acquired from the environment (cf. Wuketits 1988: 130; Hooker 1994a: 227-237), but rather "an active process tending toward the growth of intelligence, more and more complex, flexible, and inclusive structures" (Gruber and Vonèche in Piaget 1977: 783). This is entirely in line with the well-known conception that "behavior is the motor of evolution" (Piaget 1976) which, far from being unique to Piaget, was the general wisdom among nineteenth-century Darwinians (Richards 1987), before the "hardening of the Modern Synthesis".

4. *GE, bioepistemology ("Viennese" EE), and Kant*

The peculiarity and originality of GE come to the fore when GE is contrasted with "bioepistemology", which is the term I will use here to refer to the "Viennese" variety of EE of Lorenz's Altenberg Circle, continued today by the zoologist Riedl, the philosopher-biologist Wuketits (Riedl and Wuketits 1987) and more idiosyncratically by the German physicist-philosopher Gerhard Vollmer (1985). As a theory of "cognogenesis", i.e. the evolution of the structures and processes of perception and cognition (Hahlweg and Hooker 1989: 26), Lorenz's bioepistemology claims to be

able to cope with the Kantian predicament — transcendental idealism. It hopes to do so by explaining our "inbuilt spectacles", the a priori forms and categories of individual human experience, in a naturalistic (scientific) and nonfoundational (fallibilist) way, by regarding them as the result of *phylogenetic information gain* (Hahlweg and Hooker 1989: 25-30).[8] This research strategy not only takes phylogeny as its starting point; it also depends heavily on comparisons between the cognitive abilities and performances of human and nonhuman animals. Piagetians, on the contrary, justify their focus on the cognitive development of the individual human person by invoking a methodological *faute de mieux* argument, which may be summarized as: (i) Unfortunately we don't know much about the cognitive aspect of biogenesis (e.g., what was the psychology of Neanderthal man?); (2) ergo, "like the biologists, we will have to address ontogeny" (cf. Piaget as quoted in Vollmer 1985: 181; cf. Kesselring 1984: 298-299).[9]

Bioepistemologists have sensitized us to the problem of the anthropomorphism which seems to be "inevitably present in the very structure of human thought" (Hahlweg and Hooker 1989: 30) — and whose (limited) *heuristic* utility, I want to insist, continues to be defended in our day (see section 6). A scientific realist such as Vollmer (1985: 322 and passim) likes to refer to this "remedial function" (Hahlweg and Hooker) as "die *wahre* kopernikanische Wende" (the *true* Copernican turn), which he contrasts with the constructivist Kant's "anthropocentrism".

Piagetians, on the other hand, fear that by tackling the problem of knowledge from the phylogenetic side and concentrating on the similarities rather than the differences between human and animal cognition, bioepistemology is unable to reach the level of "the specific performances of human cognition" (Kesselring 1994: 302). Cf. also Lelas (1989: 154): "Perhaps ... the fault lies simply in an impatient jump from the cognitive

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[8] The difficulties any naturalistic, i.e. causalist reinterpretation of the "transcendental ego" must run into are analyzed very perceptively by Engels (1989: ch. 9) and Shimony (1993: 21-61).

[9] Presented in this form, the argument presupposes that evolution (inheritance) and development (growth) are inextricably and causally linked, which Weismann's concept of the germ plasm and the central dogma of molecular biology seem to exclude (see, e.g., Bowler 1992: 190). This obviously betrays Piaget's penchant for Lamarckist explanations (cf. section 6).

apparatus accounted for by bio-evolution [viz., Vollmer's "experiential knowledge" as well as Brunswik's reason-like or "ratiomorphous" sub-human forms of "knowledge", or rather information], to reason". Thus, according to Hahlweg and Hooker (1989: 30), bioepistemology "offers no insight into the growth or progress of scientific knowledge, a fortiori no insight into the development of methodology itself, which we hold to be a crucial element of an adequate EE". Bioepistemology may explain why common sense is successful in the "mesocosm" (Vollmer), but "in its static universality it is silent on the relation of the ontogenesis of knowledge to its phylogenesis, or indeed on the basic character of knowledge itself in relation to the interaction of organisms with their environment" (Hahlweg and Hooker 1989: 30).

GE, on the contrary, takes as its point of departure the *constructive activity* that marks ontogenesis. This allows Piagetians to attack the Kantian problem in a fresh way, which Apostel (1980: 125ff.) has likened to Hegel's attempted synthesis of the Kantian thesis (timeless invariants as the core of universal intelligence) and the Romantic philosophers' antithesis (historical development of forms of feeling and imagination), viz.: "historical development as the self-development of the timeless invariants".[10] Recall that in Kant's critical philosophy, the a priori concepts of the pure understanding cannot provide the *content* of experience, which is supplied by perception. The reliability bioepistemologists still confer to perception perpetuates this dualism of form and content. GE is superior in this respect, for Piagetian structures "do supply the content at the higher levels of development when it becomes possible to perform operations on operations" (Atkinson 1983: 41).

5. *GE as a scientific theory of cognition*

Like any old scientific theory, any *naturalized* theory of (scientific) knowledge acquisition, however abstract, ought to *testable* at some level, if only indirectly (Callebaut 1993). GE, as the *theory* by which Piaget attempted to "explain knowledge, and in particular scientific knowledge,

[10] A challenge to this position is posed by Sober (1978: 181), who maintains that "the search for constancies is not characteristic of a *new* developmental stage; it seems to be characteristic of *all* developmental states".

on the basis of its history, its sociogenesis, and especially the psychological origins of the notions and operations upon which it is based" (Piaget 1970, quoted in Hahlweg and Hooker 1989: 30), was the achievement that motivated all his (if not his school's) painstaking experimental psychological investigations. GE, then, would seem to offer such a testable theory, and certainly a theory more robust than, say, extant Campbellian EE, which (as Apostel 1987: 312 insists) Campbell himself grants "has the nature of a challenge: he calls on others to discover or refute the existence of his levels [of selection] and he is fully aware that the EE he is presenting now is purely speculative".

Now it is certainly also correct to observe
(1) that Piaget's GE "has not met with the acclaim that initially greeted his empirical studies of children", and
(2) that "there has been increasing criticism of Piaget's empirical work, questioning not only his way of achieving his results but also his highly interpretative presentation of them" (Atkinson 1983: 4).
Atkinson was primarily addressing an audience of students of child development, who, like psychologists more generally, have a proclivity not to take methodological matters lightly. One might be tempted to try to explain (1) in terms of (2); but such an empiricist, "internalist" explanation of the little impact of GE on (Anglo-American) psychology (*and* philosophy) would at best be partial and certainly naive, as should become plain in section 8.[11] Rather than buttressing a defense of GE along such lines, I want to point to the methodological moral from post-positivist philosophy of science, which shows convincingly that the straitjacket of behaviorism, definitional operationism and what-have-you which older empiricists imposed on psychology was unduly restrictive and never delivered the goods that were expected and promised (see, e.g., Campbell 1988). Justification, we now realize, is never a linear affair (see Nickles

[11] Traditionally, the first issue, concerning the reception or acceptance of GE, would have been viewed as a "purely sociological" one, in contradistinction to the second, "genuinely methodological" issue of validation. However, in terms of the evolutionary and naturalistic epistemology I embrace myself, any full-fledged explanation and assessment of "success in the knowledge game" requires that one pay attention not only to the individual and social construction of knowledge claims, but also to their individual and social selection (which includes their validation) and retention. From this perspective, both acceptance and validation are cognitive *and* social affairs.

and Wimsatt in Callebaut 1993: 154-157, 207-214). In none of my readings on GE have I come across any peculiarities that would single it out as more problematic than any other grand theoretical schemes.

A potentially more serious challenge to GE — as far as I am concerned — comes from the principled advocates of *causal-mechanistic explanation* in the biological and social sciences (see, for instance, the positions of William Bechtel, Wesley Salmon and William Wimsatt as pictured in Callebaut 1993: Ch. 4). Now, as Atkinson (1983: 9ff.) stresses, Piaget characteristically uses two distinct kinds of explanation of the general facts he discovers (which he does not himself distinguish as alternatives). One explanation is in terms of *assimilation* and *accomodation*, the other in terms of a probabilistic, structural model:

> The structural model is offered to show that the facts to be explained can be formally deduced from one another. The biological model provides the substrate, the underlying reality of the logical model.... [Piaget] seems to think that he is offering one explanation of the facts and that the structural and biological are different facets of one explanation and add up to a composite view of the development of intelligence. When he can show that the formal relations between laws follow the actual temporal relations between events then he achieves his ideal explanation. (p. 10)

Since he sees logic as the essence of intellectual functioning, Piaget sets out to explain the *emergence* of logic biologically.

> However, the nature of logic raises questions of truth and validity. Although Piaget has little to say about truth, he offers his structural model as a counterpart to his biological model. The first is an explanation of the emergence of logical thought, the second is offered as an explanation, or at least explication, of its validity. (ibid.)

At this point, traditional epistemologists like Hamlyn require a *logical* or at least a *formal* justification, whereas principled naturalists deem Piaget's formal justification inadequate: communication breaks down. It is obvious that Piaget saw clearly that the deductive-nomological account of explanation dear to Hempel gives us, at best, half of the story. That subsumption by itself does not explain is an insight he shares with scientific realists such as Bunge or Apostel. He at least *intended* to offer genuine causal-mechanistic explanations. However, it would be highly unfair, I think, to hold against the Piagetians that they did not ultimately

succeed in disentangling the "reasons versus causes" muddle (cf. Bunge 1979, especially Ch. 9; Dretske 1988), which comprises the issue of the reducibility of functional explanation to causal explanation (cf. the current revival of Aristotle's doctrine of the four causes in evolutionary systems theory). Not incidentally, this problem cluster also infests Popper's "EE": According to Popper's "Principle of Transference", what is true logically in his abstract "World 3" (where cognitive contents belong) is true causally in his material "World 1" and in his psychological "World 2" (biological entities are to be parsed among Worlds 1 and 2 here). As Hooker (1994b: 239) rightly remarks, "this labels the problem rather than solves it". Actually it would be fair to say that *nobody* has come up with a convincing solution of the "reasons versus causes" *problematique* to date, and there are even some good reasons to doubt whether the limited and imperfect cognitive agents we are *ever* will, barring a revolution in our thinking about what constitutes ideal or optimal behavior (cf. Callebaut 1996 on "satisficing" as a truly naturalistic alternative to optimization thinking).

6. *Misgivings: phenocopy and recapitulation*

To round off my (all too superficial) treatment of the biological roots of GE, I would like to go back to the issue of phenocopy and say a few words about that other red herring, Piaget's alleged recapitulationist view of the relation between "cognogenesis" (viz., the history of science) and individual psychogenesis, which was elaborated in most detail in Piaget and Garcia (1983).

Hooker (1994b) has analyzed Piaget's theoretical and experimental work on *phenocopy* in detail. He labels "the conservative enrichment of neo-Darwinian theory with the 'theory of the phenotype'" *pheno-neo-Darwinism* ('pheno' for 'phenotype')" (p. 230) and contrasts it with (several varieties of) *quasi-Lamarckism*. Because of the technicalities involved it will be impossible to adequately summarize Hooker's findings here, and I have to refer the interested reader to his original paper (Hooker 1994b: 227-237). But for our purposes it will be sufficient to quote his conclusions, which I will fully endorse here. With respect to Piaget's position, Hooker thinks, "(i) there is unresolvable ambiguity on the matter in most of Piaget's writings, (ii) in at least some places he is

committed to [quasi-Lamarckism], but ... (iii) pheno-neo-Darwinism suffices to capture almost all that he says and certainly the core of his regulatory systems approach" (Hooker 1994b: 235). Hooker's own present view is "that the ultimate status of phenocopy is not important in itself, that from a regulatory systems perspective it is probably mixed (some insight, sone confusion, some error), and that it can safely be left to await a day when more is known (both empirically and theoretically) about the regulatory processes involved." (p. 237).

With respect to Piaget's view of *recapitulation* — recall that his entire enterprise seems to presupposes that evolution and development are inextricably linked — a similar "optimistic" conclusion may be reached if one compares the Piagetian corpus with the recent thinking of biologists and philosophers and historians of biology on this score. As Hooker (1994b: 220) notes, Piaget is not very consistent on the issue; he claims to reject recapitulation but adds immediately that a relationship of a general kind between ontogeny and phylogeny "remains valid in the main, provided allowance is made for the influence of changes in speed and short circuits". If this statement can be taken as Piaget's "final truth" on recapitulation, it will be found to be consistent with the opinion of a leading expert on the matter, Stephen Jay Gould, who in his classic study *Ontogeny and Phylogeny* (1977) definitively rejected the strong recapitulation thesis (Haeckel's biogenetic law) but also made a strong case for a weaker, more generalized version of recapitulation. The latter is cast in terms of *heterochrony*, which is viewed (today) as "a general term for evolutionary changes in the timing of development" (Gould 1992: 158). Stuart Kauffman's innovative analyses on lineage-specific and developmental constraints and Wimsatt's "developmental lock" model of *generative entrenchment*, a model of the relevance of development for evolution which (among other things) sheds new light on the "innate-acquired" distinction, are two additional recent examples of relevant work in this domain (for an informal introduction, see Callebaut 1993: Ch. 9, with references). More generally, it is obvious that in biology, developmental issues are coming to the fore again, after having been eclipsed by genetics for too long. The neo-Darwinian premise that, thanks to Weismannism (acquired characters are not inherited), "the processes by which organisms develop can be treated as if they were in a black box" (Maynard Smith 1995: 28) is still ritually invoked. But meanwhile, development and other issues that the "hardening of the Modern Synthesis"

succeeded to expel temporarily are coming back with a vengeance. It remains to be seen whether it will prove possible to integrate them into the neo-Darwinian framework (Depew and Weber 1995).[12]

Let us take stock. Our brief review of phenocopy and recapitulation suggests that save for certain ambiguities, confusions and contradictions inherent in Piaget's writings (which would seem inevitable, given the considerable time span and sheer volume of his intellectual production), the "problem" with Piaget's biology exists as much in the eye of the neo-Darwinian beholder (a biologist or one of his "naturalized" emulators) as it is a real problem for GE or a possible successor theory.

Further ammo for some weak and general version of recapitulation could, I guess, be derived from the insight of cognitive scientists and others that analogical and metaphorical reasoning have always played an important role in creative scientific reasoning and continue to do so even in the most advanced sciences. One could refer here to Gerald Holton's observation (inspired by Ernst Topitsch and Talcott Parsons) that until fairly recent times ordinary folk *and* scientists have "conceived of what is remote, unknown, or difficult to understand in terms of what is near, well-known or self-evident in everyday terms. Social and artistic processes and productions have often served as explanations by analogy for the universe as a system — in short, by projecting outward into the universe

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[12] I am less confident in this respect than Apostel (1987: 312), who concludes his comparison of EE and GE with the conciliatory words that "the problem of the evolutionary explanation of knowledge generation belongs to a future time, when it will be possible to compare many detailed histories of scientific theories and of everyday skills with *an enriched neo-Darwinism*" (my italics). Piaget (1952, quoted in Atkinson 1983: 15) holds that "a certain continuity" exists "between intelligence and the purely biological processes of morphogenesis and adaptation to the environment". On David Hull's view, gradually changing intellectual lineages (like their biological counterparts) tend to change indefinitely through time. Similarity being intransitive, it makes no sense to try to define a "common hard core" or "essence" that would remain invariant across generations (Hull in Callebaut 1993: 278-285, 307-312). (I take it that Piaget would agree with the gradualism of this view, but would, as a Continental thinker, insist on some sort of invariance of form. Cf. Atkinson [1983: 14]: "If each stage is a transformation of the previous one then although the later stages might appear to be totally different from the earliest stages nevertheless there is a continuum, a chain of changes.") If Hull's view applies to the evolving entity (i.e., a lineage) of Darwinism itself, it seems to me quite conceivable that, say, the evolutionary systems theory that is being articulated right now (see, e.g., Salthe 1985) might end up being "(neo)Darwinistic" in name only.

conceptual images from the domain of social and productive action" (Holton 1972: 102). GE's "faute de mieux" argument to justify the psychogenetic approach may be reconsidered in this light. The reader who finds this strange is invited to ponder Daniel Dennett's provocative plea for using the "intentional stance" in the study of (animal) cognitive ethology: Dennett argues that even *after* a "killjoy", i.e. a "stupid" causal-mechanistic explanation, has been discovered to replace a previous intentional explanation, ascribing intentionality and rationality may continue to be heuristically useful (see Callebaut 1996). We have seen that bioepistemologists claim to "de-anthropomorphize" Kant. One could say that they emphasize the *ideal* — objective knowledge — which they share with GE, whereas my "methodological rationale" for the (cautious) reliance on recapitulation stresses a heuristic *means* to this end (it is, if you wish, a second-order application of the decentration theme, now applied to the *scientists'* movement from psychogenesis to cognogenesis).

Elsewhere I have defined EE as "the attempt to explain animal and human cognition, including science, in a Darwinian fashion" (Callebaut 1993: 286). I agree with Apostel (1987: 311-312) that neither extant (Campbellian) EE nor extant GE are to be considered as classical *neo*-Darwinism. But in the light of certain recent developments in the philosophy and history of biology (Richards 1992a, 1992b; Depew and Weber 1995), I still want to defend the claim that the "enriched", integral epistemology both EE and GE aim at will be Darwinian in the *historical* sense. It is well known that Darwin himself as well as other leading nineteenth-century evolutionists — most notably James Mark Baldwin — did pioneering work on EE (Richards 1987; Campbell 1988). It is interesting (and also more than a bit ironic) to note in this context that at least one contemporary historian of science is willing to challenge the standard view (represented by Bowler, Gould, Mayr and others) which maximizes the distance between Darwin as the "patron saint" of biology and the "evil" recapitulationists, in particular Ernst Haeckel, in whose progressivism, political Prussianism, and racism Gould sees a harbinger of Nazism (Richards 1992a: 177; cf. Callebaut 1993: 434-436). Richards prepares the reader for his heresy with the following words:

> "Historians have understood ... two usages of 'evolution' to be quite separate in meaning, like the 'bark' of the dog and the 'bark' of the tree; and to link them would have been comparable to supposing that a tree might bite you. However, as I slipped into the research, I

> became ever more wary of trees. I now believe these terms are historically joined through a process not unlike evolution itself: the older embryological idea gradually became transformed over two centuries into the more familiar one referring to species change, while yet retaining vestiges of the past." (Richards 1992a: xiii)

Richards (1992a: 172,174) maintains that "like Haeckel, Darwin clearly viewed the development of the embryo as comparable to flipping through a series of daguerreotypes, which would produce a dynamic picture of the phylogenetic history of the species". He goes on to argue that "Gould and Mayr have a scientific interest in reading Darwin as they have" (p. 177), i.e. in an ideological, Whiggish way.[13] I want to quote him in extenso, for the controversy which is hereby opened seems to me highly relevant to the future articulation of EE and GE *qua theory of science* (and to a lesser extent also to the philosophy of social science), as will become obvious in a minute:

> "To have [Darwin's] blessing on scientific positions one wishes to maintain in the late twentieth century can only advance their cause. Both of these historian-scientists regard freely flowing variational possibilities as the juice of evolution; and suspect constraints (like recapitulation) that act to inhibit the flow can ... only produce stagnation. But more fundamentally they reject utterly any notions of guidance in evolution by teleological factors, more than a whiff of which the history of recapitulation exudes. Recapitulation theory has always been joined to ideals of progress; and for Gould and Mayr progressivist evolutionary processes can only be the result of fixed goals to be achieved — teleology in another guise. All of these unhappy scientific changelings could be more easily buried if Darwin himself were to chant the obsequies." (Richards 1992a: 176-177)

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[13] To those who would be inclined to think that the anti-essentialist argument I used in the previous note undermines my position here I would reply that the difference between looking forward and looking back is crucial: The history of science can be used (*pace* postmodernist antifoundationalism: cf. Richards 1992a: 174-176) to debunk ideological (ab)uses of the history of science — in this case, the masking of the continuity of the Continental tradition (to which both bioepistemology and GE belong) emphasizing constraints with the evolutionary thinking of Darwin and his contemporaries by a militant neo-Darwinism projecting its own difference on Darwin as the common past. Darwin belongs to *all* of us!

7. *From teleology to evolution to constraints*

At this juncture I want to bring another historian of science into the picture, who acknowledges Piaget's influence on his thinking. It is well known that at the heart of Thomas Kuhn's epochal theory of scientific change is a stage model, in which periods of "normal science", "crisis", and "revolution" alternate (Kuhn 1970, 1977; De Mey 1982). Kuhn's "normal science" has been likened to Piaget's "assimilation", and both may be viewed as forms of internal selection; whereas periods of "revolutionary science", during which science is more vulnerable to external influences, may be profitably compared to "accomodation" (cf. Campbell in Callebaut 1993: 296). Toward the end of *The Structure of Scientific Revolutions*, in a section called "Progress through Revolutions", Kuhn compares the historian of science's (i.e. his own) predicament to the situation Darwin faced during the 1840s and 1850s, and singles out Darwin's causal-historical approach — as opposed to the teleological approach that characterized both the idealist tradition of the *Naturphilosophen* and their followers and the materialism of the Lamarckians — as his most profound innovation. Having warned his readers that "the analogy that relates the evolution of organisms [sic] to the evolution of scientific ideas can easily be pushed too far", Kuhn goes on to compare the resolution of scientific revolutions as "the selection by conflict within the scientific community of the fittest way to practice future science" with the famous examples of the evolution of "such marvelously adapted organs as the eye and hand of man" — both processes may have occurred "without benefit of a goal" (Kuhn 1970: 172-173). He does not seem to be aware that by deploying his stage model as a model of *evolution* he falls victim to the confusion between evolutionary (or "variational") explanation and developmental explanation.

Lewontin, Sober and others have carefully analyzed these two distinct accounts of explanation, which are both perfectly legitimate when applied to their respective proper domains; see Callebaut (1993: 144-147, 334). It is, for instance, populations or species that evolve, not individuals; the logic of variational explanation, which views evolutionary change as the result of the replacement of individuals within a population, does not also require that the individuals who compose the population change themselves. Adding developmental or other constraints to this picture will, of course, complicate matters considerably. It helps to explain phenomena

as diverse as, say, (i) the increasing conservatism of evolution at earlier stages in development, (ii) the occurrence and persistence of "frozen accidents" and "vestigial traits" in evolution, or (iii) the circumstance that changes in the structure of developmental programs which have the effect of moving the expression of traits earlier in development can increase the maximum size and complexity of developmental programs that can be maintained by selection through reductions in the cost of maintaining parts of the structure. William Wimsatt and some of his students have discussed several dozens of such evolutionary consequences of formal models of development in published and forthcoming work, and are extending their analyses to cognitive development and evolution (references in Callebaut 1993).[14]

The confusion Kuhn was prone to was tantamount in biology before the triumph of Weismannism and still occurs frequently in the social sciences today, where much of the presumed animosity against "evolutionary thinking" is really motivated by fear of a revival of the teleologism and progressivism that were part and parcel of the earlier excesses of — *horresco referens* — "social Darwinism" and Spencerism.

The situation in social science is actually even more complicated. Authors who *are* aware of the logical distinction between variational and developmental explanation often dismiss the latter as inadequate — prescientific or downright false (cf. Hull in Callebaut 1993: 56-57). The inadvertent result of such methodological cautiousness is *the social-scientific equivalent of the "black-boxing" of development by neo-Darwinian biologists*. Philippe Van Parijs's distinction between an inappropriate evolution*ist* perspective and an appropriate evolution*ary* perspective illustrates this phenomenon well. For Van Parijs (1981: 51), an evolutionist perspective "essentially consists in looking at history (whether biological or social) as development, as progress, as a succession of stages of increasing complexity or perfection. Its explanatory claims are often restricted to spelling out a *logic* of development: the succession of stages which it presents only reflects the fact that one stage is a precondition for the next one". Karl Marx's theory of economic formations (at least in its more "vulgar" versions), Walt Rostow's "stages of economic

[14] For instance, Wimsatt himself has discussed the superiority of Kuhnian "normal science" as compared to pre-paradigmatic science in terms of the last principle listed here.

growth" and, of course, Piaget's GE (and Kohlberg's and Habermas's elaborations of genetic psychology with respect to moral and societal development) are the more obvious targets here. Dynamic extensions of evolutionist models are also given short shrift, because they remain teleological: "the succession of stages [such explanations present] then corresponds to a path on which species or societies are 'dragged forward' towards some 'End of History' by reference to which the various stages need to be explained". Exit Lamarck once again! Van Parijs then goes on define and defend the proper evolutionary perspective, which focuses on mechanisms of selection between actual (as opposed to potential) alternatives.

There is something paradoxal about this distinction and the methodological moral that goes with it. Van Parijs's criticism of "evolutionist" thinking is clearly inspired by his naturalistic concern that explanations that do not specify detailed mechanisms (Jon Elster's "nuts and bolts") are suspect. However, an important methodological lesson from the investigation of developmental and evolutionary constraints during the last decade or so is that contrary to what Van Parijs suggests, it is typically the abstract "genetic bookkeeping" approach of evolutionary biologists relying on optimization strategies that allows the quick and dirty reasoning we have come to associate with (popular) sociobiology; whereas a research strategy which has an eye on the problems constraints pose to evolution will readily involve one in the painstaking search for causal factors and mechanisms (cf. Callebaut 1996).[15]

8. *Obstacles to the acceptance of GE within the wider field of science studies*

Assessments of scientific accomplishments or research programs can take many forms. At one end of the spectrum are more or less apologetic interpretations that bring out the best and emphasize a more or less grand theory's presumed growth potential rather than its actual deficiencies and limitations. Apostel's (1983, 1986) papers on the unacknowledged or

[15] A more favorable reading of this episode would regard Van Parijs's evolutionary perspective as an improvement on his evolutionist perspective, and the current call for causal-mechanistic explanations as a further improvement on evolutionary explanations.

unappreciated dimensions of Piaget-the-logician and Piaget-the-sociologist, respectively, are examples of such apologetics that immediately come to mind. Such exercises in "Whig futurology", I take it, can have a very useful heuristic function in science, although my colleagues in history or sociology of science would probably beg to disagree. At the other extreme, the reception of ideas is studied scientifically, using the resources of multiple and sophisticated sociological and scientometric techniques, preferably computer-assisted. Quite understandably, EE, with its emphasis on the selective recognition and more or less "blind retention" (Campbell) of ideas in addition to their variation, should welcome the latter type of enterprise, of which Delpos's (1994) study of the reception of Lorenzian EE is a recent prime example. Here the outcome might well be that "there is little justice in the history of science": that deserving predecessors went unrecognized, etc. (cf. Hull in Callebaut 1993: 308). In this paper I have tried to treat GE's potential as an evolutionary and naturalistic theory of (scientific) cognition fairly; which locates me, I hope, somewhere near the middle of the spectrum I just alluded to. To further balance my picture of GE, I should add a few remarks on the near-absence of GE from most major contemporary debates in the emerging interdiscipline of science studies.

I already mentioned Kuhn's intellectual debt to Piaget, but also the widespread confusion with respect to the status of Kuhn's stage theory, which he largely caused himself (in his more recent work, he seems to be gradually abandoning stages in favor of a less discontinuous cognitive view). I also compared Piaget to that other great pioneer of naturalistic epistemology, Quine (who was at one time Kuhn's mentor), whose principled behaviorism and austere empiricism may now seem out of phase with some of the more promising recent developments. I am thinking in particular of the cognitive sciences, of which GE rightfully claims to be an important predecessor. On the other hand, outside the domains of EE (e.g., Vollmer 1985), cognitive science (e.g., Giere 1992) and, of course, science teaching (e.g., Rowell 1989), actual acknowledgements of Piaget's influence as an epistemologist or psychologist are very rare indeed. Thus in science and technology studies, although naturalistic and social-constructionist approaches abound in this field, neither the "yellow bible", *Science, Technology and Society* (1977) nor its new "upgrade", the *Handbook of Science and Technology Studies*, contain a single reference to Piaget or GE in general, although the former volume has a

chapter on psychology of science and both books discuss stage models and fairly extensively deal with Kuhn's account of scientific change. Even more surprisingly, Piaget plays no role in Alvin Goldman's "epistemics", despite Goldman's avowed interest in learning stages (Goldman in Tomasello 1993). The list of such "absences" could be augmented ad libitum.

Several factors, I take it, help explain this situation, in addition to the obvious culture gap (the language gap should not be the problem in the case of Piaget since much of his work has been translated in major languages). I mention what I take to be the most important factors, in no particular order.

(1) *The conservatism of the "underlaborer of science" attitude.* — Philosophy of science means different things to different people. Its task may be seen as the *formulation of world views* that are in some sense based on scientific theories,[16] or, alternatively, as the sort of *exposition of the presuppositions and predispositions of scientists* one typically finds in the works of great scientists who pause to take stock or who look back on their career after being Nobelized. The conception that became dominant after World War II takes philosophy of science to be a particular kind of *meta-investigation of science*. Hand in hand with the recent professionalization and naturalization of their enterprise, philosophers have increasingly become specialists in a narrow field (a "philosophy of" something). As a consequence, many philosophers of science have come to regard themselves as "underlaborers" for science — a stance which a cynic might regard as a rationalization of these philosophers having lost any sense of the global picture. They identify themselves with John Locke's dictum that "it is ambitious enough to be employed as an underlabourer in clearing the ground a little, and removing some of the rubbish that lies in the way to knowledge". And when, exceptionally, they *try* to take into account the global picture, they do it entirely in the name of *scientific* rationality (e.g. Kitcher 1993). It goes without saying that the underlaborer attitude is conservative in its taste for what is to be regarded as good science and abhors dissidence and "speculation". Such narrow-mindedness is causing the odd "general" philosopher or humanist to

[16] This conception underlies, for instance, the foundation Worldviews in Flanders, whose intellectual leader is Leo Apostel.

propose that philosophers become more self-conscious or even arrogant once again, which may not be the best solution either (see Callebaut 1995a). Actually, the tendency to be "holier than the pope" is not confined to philosophers of science: it seems to occur wherever power relations are asymmetrical enough.[17] Confronted with a grand and quite controversial philosophico-scientific project such as GE, "underlaborers" are likely to turn away from it. At best they will consider it a useful resource, to be mined in function of their specific and quite narrow intellectual and professional purposes.

(2) *The continuity of humans with animals.* — With respect to the human sociocultural realm, naturalists emphasize methodological continuity (monism) as opposed to the methodological dualism of, say, *Natur-* versus *Geisteswissenschaften.* However, methodological monism should not be mistaken for a reductionism that aims at reducing the conceptual apparatus and ontology of more special scientific fields to those of a more general field, such as the "eliminative materialism" of the Churchlands (see Wimsatt in Callebaut 1993). Nevertheless, many scientists and naturalistic and/or evolutionary ("underlaborer") philosophers of science have a tendency to endorse eliminativist reductionism. In the context of the debate between EE (in particular, bioepistemology) and GE, proponents of GE typically emphasize the unique character of human learning and culture (e.g., B. Schneuwly in Tomasello et al. 1993), while proponents of EE stress animal-to-human continuity. These complementary biases may help explain the limited impact of GE on the current debate concerning learning in animals, even when extended to humans. See, for instance, the Group Report on "Biology of Learning in Humans" prepared for the 1983 Dahlem Workshop on the Biology of Learning.

(3) *The drifting apart of logic and philosophy of science.* — In the last twenty years or so, logic (which, for the positivists, was the *nec plus ultra* of philosophical methods) and philosophy of science have drifted apart. "Logic has edged closer to mathematics and computer science. And philosophers have become more skeptical that logic provides general techniques for solving philosophical problems", as Sober (1994: 2) puts

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[17] For instance, Bernard Feltz (personal communication) has suggested to me that many biologists who are Catholics seem to profess hardcore neo-Darwinism to shield off potential criticism of the influence of their religious views on their science.

it. A summary inspection of scientific citations confirms that GE participates in this general movement.

(4) *From the logic to the sociology of science.* — A standard criticism of Piaget's work is that GE has missed the full significance of the connections between concept acquisition and "enculturation" brought to the fore by the late Wittgenstein and others (Toulmin 1972: 37). Alas, Piaget's insistence that enculturation requires that societal influences first be absorbed by the child's assimilation schemata, which happen to be GE's primary focus (Ros 1991: 25), apparently fails to convince most "transcendental lingualists" and social or radical constructionists. Boyd and Richerson (1985: 45) point to what seems to me to be a more serious objection to GE: "While social learning theorists acknowledge the importance of development, they suggest that Piagetian developmental stages may result from the fact that social learning at one stage may often depend on what is acquired via social learning at earlier ages, rather than from organic development".

(5) *Nietzche lives.* — Finally, even if we don't like to hear it, our postmodernist *Zeitgeist* definitely favors the intuitive, unsystematic type of philosopher whose fuzzy "message" suggests profound thinking and, most importantly, appeals to the media — definitely not Piaget's cup of tea. Small wonder, then, that we have Michel Foucault say, in response to the suggestion that his structuralism was really Piaget's invention, "Je ne le crois guère, il n'est pas capable, le pauvre. Il n'a jamais rien inventé" (quoted in Droit 1994: X). No comment.

9. *Conclusion*

When, to round off, I try to bring together the several threads running through this essayistic paper, I am struck by the complementarity of what I perceive as the major weaknesses of EE and GE, respectively: The blunt realism of EE, which in some of its bioepistemological variants borders on naive realism, has been easy prey to more sophisticated critics such as Engels (1989) or Kesselring (1994). So far, so good! On the other hand, it is difficult to deny any longer that anti-realism — the *easy* pose for both constructivists (Gillièron 1987) and empiricists (Quine 1953) to strike — is now being taken to extremes any right-minded person ought to object to (Gross and Levitt 1994). This seems to me to

add a moral and even a political dimension to Giere's (1988) argument to the effect that the position we really ought to articulate in the light of recent developments in science studies is a *constructive realism.* In Apostel's (1987: 313) words, "even if we need the power of our action to construct the structure of our models, this does not prevent these models to be partially homomorphic to real systems outside the knower, only dimly exhibited by the perceptual field". Let EE and GE come together to give us the best of both their worlds!

Limburgs Universitair Centrum
Rijksuniversiteit Limburg

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