Paradigm, Paraphrase, Paralogia, and Paralysis: All in the name of progress[1]

Bernard Kaplan

In *Philosophy in a new key* Susanne Langer (1942, p. 1) stresses the point that the questions posed by any interlocutor constrain and circumscribe the range of acceptable or palatable answers. Those who pose certain questions characteristically take for granted many assumptions that are open to dispute, or presuppose the truth of claims that may be false or problematic. Before I directly confront the substance of the paper by Vonèche, I would like to commend him for his refusal to be suborned by the question-begging queries and assertions posed to him by the Editors of this volume.

There is no doubt, among those who characterize themselves as 'developmental psychologists,' that there is as much diversity in paradigms – I do not have the aversion to that term that Vonèche expresses – as there is among those who refer to themselves as other kinds of psychologists, for example, clinical, social; it may be, however, that the contestation among developmental psychologists is not so much over 'theories' with respect to an agreed-upon object of their inquiries, as it is over a fundamental disagreement as to what a *developmental psychology* is all about.

Vonèche, correctly, espies behind that seemingly innocuous observation, especially when coupled with the immediately follow-

[1] Since first person singular pronouns are used throughout the paper on which I shall comment, I will metonymically refer in my commentary only to Vonèche, intending, in such reference, both authors.

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ing musings concerning the paths that ought to be considered in order to attain 'progress' in 'developmental theory building,' a covert condemnation of such diversity and a tacit belief that we all ought to aspire to some monolithic, canonical point of view and categorial system. Why, in the name of *progress*, one should aspire to monotony rather than polyphony or counterpoint is left both unexamined and unargued. Vonèche discerns behind this thesis a number of dubious assumptions regarding what is involved both in doing science and achieving progress in a congeries of activities putatively pretending to be scientific.

Again, the Editors seemed to be preoccupied with some kind of activity called 'theory building' in developmental psychology, uncritically applying the metaphor of 'building' or 'construction' to perspectives or points of view.[2] Vonèche rightly questions the value of such a metaphor, and requests an explication of the notion of 'theory-building,' whether in developmental psychology or anywhere else.

The Editors also wonder whether 'progress' is possible in such theory building, but in order to assess *progress* anywhere, Vonèche intimates, one must ask *progress toward what* (a telos), and must also delineate criteria for determining progress, stasis or regress *for the domain in question.*

Assuming that 'progress in theory building' is a desideratum, without specifying what they would take as constituting progress, the Editors ask Vonèche to pontificate on the appropriate steps toward the achievement of such 'progress.' Is it to be manifested in the form of: *integrating* various theories, either through eclectic picking and choosing – a mode of agglutination typically taken as antipodal and antagonistic to integration – or through some obscure Hegelian 'synthesis of opposites'? Or, alternatively, here invoking some antiquated positivist dogma, is it to be achieved by selecting the best among existing theories, through the formulation of 'better empirical tests' to separate the wheat from the chaff?

[2] For a relevant discussion of the etymology and transformations of 'theory,' see Toulmin (1982b, pp. 238-240). For the hazards involved in ontologizing metaphors, see Black (1962, pp. 241ff.), Lakoff and Johnson (1980). For a much richer conception of 'theory' than the Editors have in mind, see Culler (1987, pp. 29ff.) on 'theory' in current literary criticism.

Without going into too much detail in his initial response, Vonèche suggests a certain incoherence in the concept of integration entertained by the Editors, and points to the obscurity of the notion of 'better empirical tests,' questioning whether one can use such (dubious) tests to select 'the best theory' and jettison other points of view. He subsequently documents his objections to such theses.

Against the Editors' prejudicial presupposition that developmental psychology deals with individuals (bracketing the thesis that one does or must collapse individuals into 'structures of variables under transition'), Vonèche, at the outset, politely voices a demurral and deferral in the form of a question. In his place, I would surely have been far less conciliatory and civil. Clearly there are developmental approaches (points of view, theories, perspectives) to mentalities that do not focus on individuals or that take individual mentalities or modes of operation as derivative from larger social complexes.

Unless one rules out such theories *ex cathedra*, it is absurd to insist that 'developmental psychology' deals or must deal with individuals, either as they are apprehended in everyday life or as 'complex structures of variables under transition,' whatever that string of words is taken to mean or refer to.[3]

Once again diplomatically, Vonèche remarks that the questions addressed to him and the tasks set for him by the Editors are neither simple nor easy to deal with, either in the abstract or concrete. He, therefore, jettisons the questions as posed and formulates his own itinerary, on occasion criss-crossing the route set out by the Editors.

Having briefly commented on the questions by the Editors posed to Vonèche, and on his reactions to those questions, I turn now to

[3] This kind of reduction of individuals to 'complex structures of variables' seems to derive, as Hilary Putnam, following Edmund Husserl, observes, from the "new way of conceiving 'external objects'" introduced by Galileo: the "idea of the 'external world' as something whose true description ... consists of mathematical formulæ" (Putnam, 1987, p. 5). Putnam, in my view, provides excellent arguments against this kind of Galilean reductionism, especially with respect to those disciplines dealing with historically-culturally embedded human beings. See also Olafson (1979, Ch. 1), Mink (1987). The political implications of this rhetoric of scientism, stemming from an Enlightenment concerned with the progressive establishment of Freedom under Reason and rational-scientific inquiry, are discussed by Horkheimer and Adorno (1947/1972, pp. 82ff.), and Horkheimer (1947/1974). See also Arendt (1951/1958, pp. 42f.), Merchant (1980/1983, pp. 99-126, 164-252), Keller (1985, pp. 43-126), Grimshaw (1986, pp. 176-178, 184-185, 196-202).

the substance of his paper. There are three general issues that he decides to consider. I represent them in the form of questions as follows:

1) How does one identify *progress* in developmental psychology?
2) In what way, if any, can one analyze the process of scientific growth (progress, development?) with regard to the history of science?
3) Can a case study of the presumptively major figure in contemporary developmental psychology, namely, Jean Piaget, provide some understanding of the process of scientific growth (progress, development?) in developmental psychology?

Vonèche's treatment of the last issue concludes, *inter alia*, with some claims concerning what constitutes progress ('better science') in developmental psychology, if that predicate is assigned to theories of those who have achieved widespread recognition, are taken as successful in their scientific activity, and so on. Here, one might say, Vonèche tacitly offers one prescription as to how to attain fame and renown in the domain of developmental psychology, in particular, and, perhaps, science in general, derived from a close study of the practices of a presumably paradigmatic example of one (Piaget) who has, indeed, achieved such fame and renown, and who is taken widely to have instituted progress and, perhaps, even a scientific revolution in developmental psychology.

Without more ado, let us turn to Vonèche's treatment of the first question: How does one (or should one) identify progress in developmental psychology?

This is one of those questions which is likely to elicit responses testifying to the truth of Kant's observation that one can easily achieve simplicity by sacrificing all insight. Sophomorically taken, with all critical questions gagged, one may assume that everyone knows what is meant and referred to by developmental psychology (or its object, 'development'), and, of course, progress here would be simply a growth (increase) in knowledge about that object.

Considered at levels below the epidermis, however, one may recognize that this is indeed a labyrinthine question. Not only is the

idea of progress obscure, (see Bury, 1920; Tsanoff, 1971; Nisbet, 1980) but the notions of development and developmental psychology are, in Gallie's crisp formulation, "essentially contested concepts," that is, perpetually open to controversy, dissensus, and unresolvable disagreements. The labyrinth is further complicated for those (I, and perhaps Vonèche, among them) who reject out of hand any simplistic conflation of development with change over time, whether historical, ontogenetic or whatnot,[4] and who, tacitly or explicitly, presuppose a considerable overlap, verging on synonymy between development in any domain and progress in that domain. For us, the issue of identifying progress in developmental psychology is tantamount to identifying development in developmental psychology.

As I have already remarked, when one raises the issue of 'identifying progress' in 'developmental psychology' (or anywhere else), one must either presuppose or stipulate some criterion of *progress* with regard to the domain in question. To adapt a cogent contrast from Archibald MacLeish (1964, pp. 13ff.), progress is more like poetry than it is like lions. We may arrive at culturally or even transculturally accepted criteria as to lionhood, and may then send someone out to identify lions. On the other hand, we have not agreed upon notions as to what specifically is to be taken to constitute poetry (or progress), and may thus find that what is poetry (progress) for one coterie is noise, nonsense, degeneracy, stasis, or regression for another. To put this insight into technical terminology, befitting a discipline aspiring to the status of science, we may say that 'one man's meat is another man's poison.'

In the heteroglossia and heteroaxiologica that characterize the human condition, one would have to know whose *voice* utters 'progress' or 'development' and what prejudices and values inhabit the bodies/minds of those who speak. This caveat, of course, applies to ourselves as well as others. The issue here, as I have often maintained (Kaplan, 1966, 1967, 1981, 1983a,b, 1985), is directly apposite to issues and controversies about *development* and *developmental change*, since as I noted above, I identify *development* in any domain with progress towards a (generally stipulated) telos. What I

[4] It seems to me that this conflation operates in much of what is euphemistically called developmental psychology, and vitiates the field, making it a barren wasteland, pimpled by 'empirical studies'of various phenomena that occur as a function of time, age, and so forth, and by a motley of inquiries into antecedent or concomitant conditions 'associated with' culturally defined classes of historical (i.e., ontogenetic) events.

would argue, among other things, is that a discipline that pretends to devote itself to a value-neutral 'objective-empirical' determination of development, either *simpliciter* or in particular, is as much deluded and engaged in a fatuous enterprise as a discipline that seeks empirically to assess progress in general or particular.

Now, Vonèche acknowledges this point, at least up to a point. He remarks: "the very notion of progress is contaminated by the ideological perspective one advocates" (p. 154).[5] This being so, one would expect Vonèche quickly to set forth his own 'ideological perspective,' so that his readers may have a clearer notion of the real *Wizard of Oz* behind a screen of omniscience, who utters oracular assertions and judgments about others. But this he does not do. Instead, like a Democritean atom, he swerves to dissect, in a helter-skelter way, what others have said, or are taken by Vonèche to imply, concerning the criteria of 'progress': in the history of (physical) science; in an advocacy of one or another *Weltanschauung*, world-hypothesis or root-metaphor; in the cultural and discursive practices of the members of a younger generation, who have been disillusioned by 'grand theory,' and enticed by the professional rewards deriving from the myopic cultivation of their own gardens; by unreflective advocates of 'reception-theory' or 'reader-response' with regard to the evaluation of texts; and so on.

In exhibiting the presumed views of these diffuse and often overlapping groups, Vonèche does not refrain from invoking his own inexplicit notions of 'true progress,' in order to smite the arrogant or intellectually indolent buttocks and back. Nor, like some of those omniscient narrators of Victorian fiction, is he averse to throwing his own moral and cultural-historical observations on to the compost pile. Let me try now to trace Vonèche's meandering itinerary, which belies the traditional axiom that the shortest distance between two points is a straight line. *Pari passu*, I will comment on aspects of that intrepid voyage.

For some reason, Vonèche begins his attempt to identify progress in developmental psychology by examining and criticizing the views

[5] It is not clear whether Vonèche takes ideological interests to be manifested only with regard to the interpretation of progress, or whether he takes ideology to infuse all interpretation; in the latter instance, of course, he would entrap himself in the supposedly Teutonic-inspired "holistic, organismic, totalizing" perspective he elsewhere deprecates; see Lichtheim (1967), Carlsnæs (1981), Frisby (1983), McClellan (1986). More about this below.

of two current historians-philosophers of science, views that putatively have some bearing, or that some might insist *ought* to have some bearing on the determination of the 'scientific status' of developmental psychology and on judgments as to whether or not there has been progress in the supposed *science* of developmental psychology. He, thus, in a Derridean deferral, displaces his first question for his second.

It should be obvious that such a consideration demands clarity with respect to what constitutes *science* as well as to what constitutes *progress in science*. Moreover, it demands that one does not preempt or sidestep the issues of whether, and by whose authority, progress in developmental psychology (or, let us say, historiography) is to be determined by criteria some have established for the natural science disciplines, and have taken to have a universal hegemony. Such questions are typically scorned or ignored by those who believe that there is some divinely established canonical procedure for demarcating science from other human activities and socio-cultural discursive practices.

Apparently assuming that the notion of science (or the demarcation of science from other human activities) is clear and uncontested,[6] and that which holds for physics must hold for developmental psychology, Vonèche begins his curtailed and caricatured quasi-historical survey of two different conceptions of progress in science maintained by current philosophers/historians of science, by discussing critically certain aspects of T. S. Kuhn's *Structure of scientific revolutions*; more exactly, by expatiating on some unidentified interpretations of Kuhn's (1962) monograph. His other protagonist (and foil) in the battle among the giants is Karl Popper.

Since, in this gigantomachia among historians of (physical) science, Popper enjoys seniority and temporal priority over Kuhn, and, indeed is one of the seeming targets of Kuhn's revisionism, I will reverse the order of Vonèche's exposition, and consider his treatment of Popper's view before I examine his representation of Kuhn's (1962) theses. One should keep in mind that what Popper or Kuhn have to say about the history of physical science or the nature

[6] For an appreciation of the complexities involved in attempts to legitimatize what ought and ought not to be taken to constitute science/knowledge, and for the politics (or ideology, if you will) implicated in distinguishing an empyrean realm of science from other practices that can at best enjoy the status of a *gnoseologica inferiora*, see Black, 1954, pp. 3-23; Foucault, 1972, 1980; Feyerabend, 1975; Lloyd, 1984; Nussbaum, 1986.

of 'progress' in that discipline may be irrelevant to our judgments as to 'progress' in developmental psychology.

I deal with the theses of Popper and Kuhn with respect to the history of, and progress in, (physical) science only because Vonèche, perhaps ambivalently, thought it important to deal with them. He presumably did so on the basis of the widespread belief (which Vonèche may not share) that if one can descry the marks of progress or growth in knowledge in the realm of physical inquiry, one will have gained some insight into how we ought to construe progress and its instrumentalities in the adolescent domain of a psychology (developmental and otherwise), aspiring to become adult and be 'scientific.' Thus, psychology, a mere Switzerland among the great powers, by taking physics and its history as its role model, may eventually achieve its ambition, and obtain great-power status, in the elite club of truly scientific ('hard') disciplines.

As is well known, Popper, against the logical-positivist emphasis on verification, verifiability, confirmability, and so forth, as the distinguishing marks of scientific and, hence, intelligible, meaningful discourse, focused on a criterion of falsifiability to demarcate not sense from non-sense, but science from metaphysics. He believed that science/knowledge in physics and, presumptively, in all other domains (see Popper, 1985, pp. 171, ff.)[7] progressed only by virtue of conjectures and refutations, by the articulation and advancement of hypotheses and by the subjection of such hypotheses to increasingly stringent attempts at falsification.

As a realist, (Popper, 1985), assumes that there is a determinate universe, ultimately transparent to human knowledge, that one can know through the use of proper procedures. Accepting a version of the Heraclitean dictum that 'Nature loves to hide,' Popper believes that we can uncover many of nature's secrets (physical, social, psychological, whatever) through guessing, either on a large or small scale. But, like love, guessing is not enough. We must be ever willing and eager to expose our guesses to maximum fire power – to unbridled, unstinting attempts to falsify those guesses in every

[7] In a 1960 essay, Popper asserts: "Although I shall confine discussion to the growth of knowledge in science, my remarks are applicable, without much change, ... to the growth of pre-scientific knowledge also ... to the general way in which men, and even animals, acquire new factual knowledge about the world. ... For the growth of ordinary human knowledge writ large" (Popper, 1985, p. 170).

conceivable way. Only thus has 'progress' been achieved. Only thus will scientific progress be achieved.

In his intellectual autobiography, Popper (1976) discusses his 'resolution' of the problem of scientific progress, linking this resolution to the clarification of the nature of scientific method. He remarks:

> Progress *consisted* in moving towards theories which tell us more and more – theories of ever greater content. But the more a theory says the more it excludes or forbids, and the greater are the opportunities for falsifying it. So a theory with greater content is one which can be more severely tested. This consideration led to a theory in which scientific progress turned out not to consist in the accumulation of observations but in the overthrow of less good theories and their replacement by better ones, in particular by theories of greater content. Thus there was competition between theories – a kind of Darwinian struggle for survival (Popper, 1974/1976, p. 79, my emphasis).

Now, one will observe that the tense marker here (consist*ed*) suggests that Popper arrived at this historical insight *inductively* and not through some version of hypothetical-deductive procedure, despite his animus against inductivism (Popper, 1974/1976). One will also observe that Popper here seems to assume some kind of 'law of history' at least with regard to knowledge-acquisition, despite his persistent rejection of the possibility of any historical laws (Popper, 1974/1976, 1977). Again, Popper seems to advocate, in the name of progress, some essential method, requisite to achieving 'progress in the growth of knowledge' anywhere, despite his recurrent tirades against 'essentialism.' Finally, it may be noted that Popper exploits his own inductively derived neo-Darwinian 'law' to reconstruct a dubious and highly selective history of (physical) science, not far removed from the kinds of histories reconstructed by those 'philosophers of history' (e.g., Hegel, Marx), whom Popper denigrates at every turn (Popper, 1974/1976, 1977, 1985).

Co-opted and subverted by his own trope (White, 1973; de Man, 1983) Popper, thus, abandons his own solemn strictures concerning the role of falsifiability and increasingly stringent tests in the 'growth of knowledge,' even in the pre-scientific, dubious discipline of historiography, and guided by his fixed star of what progress in science *really* or *essentially* means (a neo-Darwinian struggle in

which the fittest survive), has given us a *parti pris* version of what happened in the history of science.[8]

I have devoted so much space to surveying those aspects of Popper's position that are *relevant* to the issue of scientific progress mainly because Vonèche does not seem to me to have done so; moreover, *pace* Vonèche, it is this view of the putative dynamics underlying 'progressive' scientific change that Kuhn contests in his now classic monograph.

I turn next to both Vonèche's representation of what is of central relevance in Kuhn's monograph and – a radically different thing – Kuhn's actual emphases. Partly due, perhaps, to his anachronistic handling of the relations between Popper and Kuhn, Vonèche, in discussing Kuhn's *opus*, seems to lose sight of the issue he set for himself, namely, how, if at all, one can identify progress in developmental psychology. Given that issue, one might have expected Vonèche to extract and highlight what, if anything, Kuhn has to say about 'progress' in (physical) science, on the arguable assumption that this narrative might have some relevance to the thorny issue of identifying progress in developmental psychology.

But Vonèche does nothing of the sort. Rather, in a markedly desultory way, he begins with Kuhn's off-hand and gratuitous distinction between an *immature* science – riven with, and divided by, a plurality of competing paradigms – and a *mature* ('real,' 'hard') science – marked at any one time by the hegemony of a single dominant paradigm. He then leaps off in several different directions, very few having much to do with Kuhn's book or its significance, none leading even to the outskirts of Rome.

Thus, *inter alia*, Vonèche alludes to, and bemoans, the 'immaturity' of psychology in these terms; observes how misleading it is to be preoccupied with the supposed preparadigmatic status of psychology; dismisses, against all of the evidence, the centrality of *paradigms* in Kuhn's representation of the history of science; speculates on the appeal of the concept of paradigm to *hoi polloi*; and seeks to deflate the 'undue associations' some (?) have drawn

[8] It would be unfair to Popper to treat him merely as an evolutionist; his functionally *a priori, idee fixe*, presumption of what constitutes progress in science/knowledge governs the selection of what he takes to be relevant to his historical reconstruction of science.

between the concept of paradigm and Popper's emphasis on falsification as essential to true science.

I believe that Vonèche is off the mark on almost every one of these digressive issues (see Lakatos & Musgrave, 1969; Gutting, 1980; Nozick, 1981, p. 626), but even if he were on target, they would have little or nothing to do with the significance of *The structure of scientific revolutions* to the main topic of the identification of progress either in science in general or in a developmental psychology that aspires to attain the status of a 'real' science.

There is neither space nor place here for a point-by-point refutation of Vonèche's contentions on these eccentric and marginal issues. I will, therefore, leave them in limbo, and turn to the ostensible pertinence of Kuhn's monograph for the central issue of progress in science. This pertinence is very well sketched in a concise way by Ernest Gellner in his 1982 article, *The paradox in paradigms*.

> One of the reasons for the importance of Kuhn's theory was that it provided what Popper recognized as the most interesting challenge to his Kamikaze theory of science and scientists. Kuhn's central point is that the overwhelming majority of scientists do not observe this Popper-commended samurai code of cognitive daring: they do not propose risky theories so that their immolation on the altar of science should advance learning by eliminating one further possibility. (Gellner, 1982, p. 451.)

Gellner continues:

> On the contrary, they uncritically accept the current dominant vision, the *paradigm*, and only work on points of detail within it. You can question some theories all the time, and all theories at some time, but you cannot question all theories all the time. Radical revolutions in science, as in politics, occur but rarely, when a system collapses and thereby obliges those who inhabited it to erect something fundamentally new. What is very important for understanding Kuhn is that he said not merely that this is *how things were*, but also that this is how they *should* be. Had he merely noted the prevalence of 'normal' (paradigm-respecting) science, the Popperians could have replied, and in fact they said this anyway – well so much the worse for those timid conformists. They may in fact behave in this way, but the sooner they stop the better for the advancement of knowledge. But Kuhn plausibly argued that this sheepish conformism was not just a weakness, but a necessary precondition of the very existence of science. He deliberately goes out of his way to shock the Popperians and all wishy-washy, well meaning liberals, by statements such as that, "to turn Sir Karl's view on its head, it

> is precisely the abandonment of critical discourse that marks the transition to science (T. S. Kuhn's *The essential tension*, 1977). (Gellner, 1982, p. 451.)

Gellner goes on:

> Just as Hobbes claimed that obedience to a Sovereign was the precondition of a Commonwealth, so Kuhn claimed that obedience to a paradigm – a cognitive sovereign – was the precondition of a scientific community and its cumulative work. Fundamental alternatives of vision are seldom if ever conclusively settled; if each worker on the great scientific enterprise feels ever free and inclined to re-open those issues, nothing much gets done, even if it fails to be done at the highest and deepest intellectual level. Kuhn had noticed this happening amongst social scientists when he rubbed shoulders with them at a think-tank (and as a matter of fact, one of his worst mistakes is the tacit assumption that pre-scientific humanity at large, and modern social scientists, resemble each other). (Gellner, 1982, p. 452.)

From this succinct synopsis by Gellner (and from comparable reviews of Kuhn's monograph by such knowledgeable specialists in the history and philosophy of science as Shapere, 1964; Musgrave, 1971; Toulmin, 1970) one can discern the central issues in the controversy between Popperians and Kuhnians, obscured or marginalized in Vonèche's treatment. One may even gain an inkling as to the relevance (or irrelevance) of this dispute over what happened in the history of (physical) science to the axiological and prescriptive question of what ought to be taken to constitute progress in 'science,' generally, and in developmental psychology.

We have already seen what constitutes 'progress' for Popper – the replacement of theories of lesser *scope* and *precision* by theories (conjectures) of greater scope and precision, (I here adopt Pepper's, 1942, inescapably vague criteria for assessing world-hypotheses). Whether or not such replacements happened in the nitty-gritty of history is really irrelevant to Popper. That is what 'progress' *means* to him, and if actual history (assuming, questionably, that it is possible to achieve a representation of such a history) does not unroll in this manner, so much the worse for actual history. One can always write a piece of historical fiction, in which events have happened and will happen according to plan. The moral of this fabulous history for those developmental psychologists, who aspire to the august status of natural scientists?

> Get your act together. Come up with comprehensive, unambiguous hypotheses, and work your utmost, hoping that others will do their best, to burn up or 'waste' these guesses in restoked and recurrent fires by ordeal. Test! Test! Only constant exposure to the white-hot flames of falsification purify, and warrant entry of a conjecture into the vestibule of Truth.

What *Good News* cometh from the oracle on the basis of our consideration of Kuhn's narrative? Here amphiboly prevails. In part, this ambiguity stems from the fact that Kuhn, in principle, claimed to represent what actually happened in the actual history of physical science, *wie es eigentlich gewesen war*, and was therefore precluded, in principle, from drawing any morals from his tale or arriving at any prescriptions for the future, even though, as Gellner remarks, neither he nor others could resist the temptation to derive universal lessons from a singular history.

Moreover, given that the epithet, *progress*, is less a description than an assessment of a series of events, Kuhn, *qua* ostensibly neutral historian, describing things as they were without an axe to grind, was prohibited from talking about progress, albeit not barred from talking about change.

In effect, Kuhn may be taken to have shown that changes in the course of history in the physical sciences did not conform to Popper's Kamikaze formula, but consisted rather in peaks of *paradigmatic* hegemony accompanied and followed by plateaus of 'normal science.' On reflection and historical reconstruction, exemplary, world-historical figures institute a (rare) revolution, and then there are long stretches in which devotees of the exemplary revolutionary figure seek to fill up all the spaces in the brave new world constituted by that figure.

This paradigm-conforming 'normal science' persists, with only some tinkering and temporary repairs, until a new avatar of the archetypal revolutionary figure appears – a sufficiently 'strong' poet in Harold Bloom's literary history (1973, 1976) – to dispossess the former vicar of God from his/her privileged seat at the right hand, and recruits new disciples, including a few converts from the old regime, to spread the new message across the land.

Now, whatever reservations he may have with respect to other criteria for *science*, Kuhn seems almost to define a 'science' by the mystical presence of a widely accepted *paradigm*, whose implications and applications are then worked out by busy bees who make

up the fraternity (and now, perhaps, sorority) of paradigm-abiding 'normal scientists.'

It is *within* the paradigm that the foundational categories are set, the proper methods established and canonized, the relevant issues delineated, and the rules for proper interpretation authorized. Those who seek sanctuary within the fold (normal scientists) do not raise basic questions about any of these issues, on pain of being deemed 'heretical' or 'unscientific.' Theirs is not to reason why, theirs is but to do or die. Although a close look from an historical distance at the topography of change might show that the towering peaks on the landscape have some subterranean connection with earlier emerging peaks, it is only in the spread and range of the plateau emanating from a particular peak that progress in a discipline can be discerned. Revolutionary paradigmsetters may propose, but only rule-abiding normal scientists dispose. And they, alone, working within the house that Jack built, achieve progress *in that house*. If someone comes along to erect a new house, some pieces from the old one may be expropriated, but progress ceases in Jack's domicile and begins, if at all, in Jill's or John's, Larry's, Moe's or Curley's. Where too many houses are being erected at the same time on a limited plot of land, and where too many clusters of underlaborers (to use Locke's phrase) despoil the atmosphere with their cacophonous cries, there can only be confusion and chaos. Too many paradigms at one time spoil the land for all.

Now, assuming the versimilitude of this version of what really happened in the history of 'science,' nothing about progress (or regress, or stasis) follows, unless one imposes, from some psuedo-Archimedean point outside the reconstructed process, some standards or criteria as to what is to be taken to be progressive.

An instance from the history of psychology may be taken to illustrate this point. In the not too distant past, Clark Hull, aspiring to the status of Newton in the behavioral sciences, attempted to set up a new model for psychological inquiry, one that would clear the land of all competing approaches. Comprised of a stew of metaphysical, epistemological and axiological assumptions, this Hullian paradigm spread through much of academic psychology in the United States, and even invaded some of the red-light districts of clinical psychology.

A relatively neutral historian, and even one who regarded Hull's paradigm as a disaster and regression, would have no difficulty in demonstrating this historical occurrence. For the Hull worshippers, this dissemination of stimulus-response-reinforcement, was doubtless a manifestation of *progress*. But for those who believed that the Hullian enterprise was misguided from the start, the dissemination of his paradigm was more like the uncontrolled spread of a virus to which Vonèche refers in one of his metaphors for progress, a model that would allow a cancer to be progressive because it grows and spreads.

There is no doubt that other theories (perspectives, points of view) dotted the landscape during the Hullian hegemony, but they were, in the main, marginalized. They might have considered themselves as the only living things on the landscape, but for the Hullians and their coterie of hangers-on, they were more like trees polluting the atmosphere, to borrow an immortal analysis by Ronald Reagan.

In any case, there was the kind of dominant paradigm in psychology, to which Kuhn calls attention in the history of physics. And this paradigm in psychology was not replaced, *pace* Popper, by a new, more comprehensive theory, but by a flood of different, and typically unconnected theories of varying scope and precision. These diverse approaches, which still mar the landscape, can be said to have been and to be incommensurate as well as *incommunicado* with each other.

Let us grant – a large waiver – that Kuhn is correct (within current paradigms for historical knowledge) about what happened in the history of physical science. And, let us grant further that his narrative may be said to pertain in its morphology – a new morphology of the folk tale? – to some portion of the history of psychology. What follows from that for the determination of progress in science (allowing for the moment that psychology belongs to that august domain)? Can one conclude from an induction based on one, or perhaps, two narratives that, in general, this is the way all disciplines unfold? Can one licitly conclude, even if the questionable extrapolation is allowed, that this is the way in which all disciplines, *if they are to be taken as sciences*, must unfold? Can one illicitly (here, at least) leap from the is to the ought, and suggest that this is what all disciplines *ought* to do in their processes of transition through time,

if they want to be accorded the status of science and if they want to achieve progress in their science? One has merely to pose those questions to realize how absurd it is for anyone to take Kuhn's work as having either nomothetic or normative implications for science as a whole or for developmental psychology.

We can conclude from all of this that a history of a discipline, whether physics or psychology, tells us nothing about the criteria for progress we should institute for assessing progress in developmental psychology or anywhere else. Strangely enough, for quite different and, for me, unfathomable, reasons, Vonèche, who decided to bring Popper and Kuhn into the act, seems to come to the same conclusion.

Having dropped them, he indulges in an interlude in which he quickly considers and dismisses different conceptions of progress that appear to be entertained by various groups of anonymous others. For some of the inane unnamed, progress 'seems to boil down to trendy changes more than anything else. ...' From this point of view, any change, in any domain and in any direction, is taken *ipso facto* to constitute progress. Rooted in the *Myth of Progress* (which must be distinguished from the mere idea of progress (see Bury, 1932; Tsanoff, 1971; Nisbet, 1980), this view overlaps considerably with the one that crudely equates progress with novelty. Here, change itself is mercifully not enough, since some changes may be revivals of long forgotten doctrines from the despised past. If one assumes, however, that history does not repeat itself, change and novelty fuse.

Finally, some, acknowledging that neither change nor recency are enough, may identify progress in any domain with relative public approval or public enthusiasm for certain changes or novelties, the pertinent publics ranging from all those living, through the live ones in a specified locale to those comprising or taken to constitute the community of 'experts' in the field in question.

Here, a progressive step in a field or in the world at large is equated with greater public approbation of some point of view or perspective, whether ascertained via a random sample in a Gallup poll or assessed by the frequency of positive citations and lengthy references in the papers and books of presumed 'cognoscenti' in the field.

Now, although we may find such identifications revolting or deplorable, as Vonèche does, one has no grounds for ruling out such conflations, unless one *stipulates* some (other) standard of progress

(advance, development) distinct from mere change, widely acceptable change or recency.

Indeed, one must assume that such a stipulated standard or cluster of standards is tacitly operative when Vonèche remarks that the conflation of progress with change "is incomplete," and goes on to maintain that: "progress is different from change because *some changes are not progressive but regressive and sometimes even revolting*" (p. 153). Vonèche, however, as noted before, does not *explicate* his own conception of progress in developmental psychology (or anywhere else), either for *la longue duree* or for the current situation. At least initially, he side-steps *that* central requirement: namely, that he (and we) must have some notion of what *he* means by progress before we (or he) can appreciate his own *obiter dicta* concerning 'advances' in theory-building in developmental psychology or his assessments of what others have had to say with respect to the criteria or indices of 'progress in science.'

Apparently quickly sated with his 'bashing of the masses,' Vonèche manages, through some devious processes of association that a psychoanalyst might construe as primary process thinking, to arrive at two conflicting metaphors for 'progress,' one of which he arbitrarily decides to baptize as *mechanistic*, the other as *organismic*. This creative act provides him with the opportunity to bash others, this time with names and faces, namely, the Germans, Jan Christian Smuts, and, mildly, Clark University (metonymically alluding to Heinz Werner and me, who are taken to represent the organismic-developmental perspective in the U.S.). The ostensible rationale for this excursus is that some gravitating around Clark University argue "that the organismic approach is fuller and richer than the mechanical one" (p. 154). This is apparently taken to imply that, for them, progress in science, in psychology and in developmental psychology is more likely to be attained if one casts off a theory exemplifying a mechanistic 'world-hypothesis' and adopts one incarnating an organismic or holistic (one might add here, structuralist) 'world hypothesis.'[9]

[9] I believe that I was among the first among psychologists (beginning in 1953) to discuss the implications of Pepper's (1942) formulations of these two Weltanschauungen. I am pleased to learn that aspects of my unpublished lectures (see Kaplan, 1960) were disseminated beyond the walls of Clark by some of my former students (e.g., Mehrabian, Langer, Overton).

Since Vonèche, in one of his asides, referred to above, links Clark to 'organicism,' and hence, for him, totalitarianism, irrationalism, racism, and mysticism, I will, below, on a point of personal privilege, devote more time to examining Vonèche's diatribe against 'organicism,' and his fanciful culture-history than is warranted by the topic under consideration.

Before I take on Vonèche, let me quote him, so that I will not be accused of torching a straw man:

> It is usually argued, at least in certain circles, for instance those gravitating around *Clark University* as far as the States are concerned that the organismic approach is fuller and richer than the mechanical one. Without trying to build a case in favor of either one of them, I think that a word of caution is in order here. Rarely is it noticed that the mechanical approach is associated (blessed word) with the English-speaking world, whereas the organismic approach is frequently adopted in the German-speaking one. In Europe, Germans have always considered themselves different from Westerners (France and the United Kingdom, essentially); they were suspicious about the Westerner's ideas on democracy, rationalism or individualism. Germans have always been convinced that their soul is deeper than other people's and that they have privileged access to the so-called organismic totality. This obsession of a great nation, midway between East and West, at the crossroads of opposite political regimes, social systems and values, ... can be traced back in German ideology from Holderin to Heidegger. This *Weltanschauung* has led the Germans to Hitler. One should be aware of this before making the case for organicism. After all, 'holism' was eloquently defended by the South-African General Smuts. (Original ms.)

Vonèche continues:

> There is an obvious (sic!) relationship between Gestalt and German ideology, so much so that some of the Gestaltists sided up with the Nazis on philosophical grounds. Ironically, some other Gestaltists had to flee from Germany because of racial laws that were silly extensions of the organismic approach. Nevertheless, the fact remains that a romantic, organismic approach to development privileges some strong mystical, authoritarian, anti-rational, mass-psychology trends in human nature under the guise of a mere biological observation. (Original ms.)

Where is one to begin in trying to counter this flurry of accusations and insinuations, worthy of a Joe McCarthy? First, perhaps, I should get Vonèche's gratuitous swipe at Clark University

(Werner and myself) out of the way. Although I am hesitant in drawing any sharp distinction between nature and culture, I hope I will be excused for suggesting that it is 'natural' for one who adopts a certain perspective or theory to take that viewpoint as superior – "fuller and richer" – than other contrasting viewpoints.

Even if one accepts Cusanus' dictum that all points on the circumference are equidistant from the infinitely removed center, one is not likely to consider a perspective other than, and antagonistic to, one's own as better – "fuller and richer" – than one's own; if that unsettling thought entered one's mind, one would, unless unconsciously otherwise compelled, surely change one's viewpoint.

I would even venture to guess that Vonèche, gifted as he may be with a magnanimous spirit of pure tolerance, takes whatever viewpoint he holds on any issue as being 'fuller and richer' than viewpoints in opposition to his own. As Booth (1985, p. 469) observes, even those who are avowed and adamant pluralists or relativists with respect to perspectives, take this pluralist 'meta-perspective' to be one that is superior to any "monolithic one-sided" outlook.

I might note that neither Werner, to my knowledge, or I have ever talked of organicism being 'fuller and richer' than mechanism. We obviously took it to be a better 'paradigm' – world-hypotheses being paradigms claiming unqualified scope – than 'mechanism,' especially for a developmental approach to phenomena, simply because 'mechanism,' in one of its multiple meanings, denies real change or development, and seeks, in the words of the obviously Teutonic, A. N. Whitehead – really Weisskopf – to construe "nature ... as a dull affair, soundless, scentless, colourless; merely the hurrying of material, endlessly, meaninglessly" (Whitehead, 1925, p. 56). A similar point was made, again by a closet Teuton, C. D. Broad (1925, pp. 43-94), in his acute discussion of "Mechanism and its Alternatives." And, once more, from another angle, by still another covert Germanophile, R. G. Collingwood (1945/1956, pp. 93ff). More recently, it has been articulated by one Charles Taylor (1985), who if not of German blood and soil, must at least have been contaminated by Teutonic thought and ideology through his immersion in the writings of Herder, Hegel, and so forth, (Taylor, 1975, 1979), in the process of writing *Hegel* and *Hegel and modern society*.

In sum, I would say that it was partly because, in one of its dominant meanings and uses, the world hypothesis of mechanism had no place for the reality of life, development, rationality, creativity, novelty or individuality – to which Vonèche links it – that Werner, I believe, and I, I know, rejected it in favor of an organismic-developmental point of view.[10]

Let us turn to Vonèche's next point. Wearing self-imposed blinders, suffering, metaphorically, from scotopic vision, and viewing history as if reflected in a Coney island distorting mirror, Vonèche manages to come up with an observation 'rarely noticed' by others, namely, that: "the mechanical approach is *associated* with the English speaking world, whereas the organismic approach is *frequently* adopted in the German-speaking one" (Original ms.). Having doubtless arrived at these statistically significant correlations through some neutral, objective, mechanical procedure, Vonèche 'associates' this dubious finding with the presumed fact that Germans (as a unified *Volk*, perhaps) have not only considered themselves different from others (a unique phenomenon in the history of nations), but were: "also suspicious about the Westerners" (i.e., Anglo-French) "ideas on democracy, rationalism or individualism" (Original ms.).

Perhaps concerned that the reader may be too obtuse to grasp his insinuation, Vonèche makes his point explicit:

> Germans have always been convinced that their soul is deeper than that of other people's and that they have privileged access to the so-called organismic totality. This obsession ... can be traced back in German ideology from Holderin to Heidegger. *This Weltanschauung had led the Germans to Hitler* (Original ms.).

Having proffered this dish, Vonèche adds some dessert to the menu: we are presented, as a kind of afterthought, with the startling

[10] I should note that neither Werner (1948), Werner with me (1956, 1963, nor I alone (Kaplan, 1967, 1981, 1983) adopted an unqualified 'organicism' that is found, for example, in Goldstein (1939, 1960). That position, derived in part from Herder, and often taken over as an instrument against colonialist hegemony by anthropological relativists (see Diamond, 1974), was inclined to take each nation, species, or even individual as a more or less self-contained, sovereign totality, and hence, in principle, if not in practice, to reject a comparative-organismic-developmental approach, which ranked practices, performances, or groups in a developmental order.

information that "after all, 'holism' (presumably a twin of organicism) was eloquently defended by the South-African General Smuts." That Smuts was not German, was educated in England and not Germany, that he was influenced by Whitman, Clerk-Maxwell and Henri Bergson (closet Teutons?) in writing his book (Smuts, 1926/1961), and not by Holderin, Herder, Hegel or Heidegger, and that his book extols the values of individuality, creativity, and freedom to which Vonèche is addicted, is irrelevant.[11] What Vonèche really wants to suggest, by innuendo rather than evidence, is that, beyond any mere contingent "association," if any, there is an intrinsic, internal connection between Organicism-Holism and Nazi-ism-Fascism-Apartheid. It is tempting to deflate this remarkable nonsense by pointing out that Vonèche, to arrive at these "internal connections" must presuppose one kind of "organismic" approach in which all phenomena within a society are taken, necessarily, as expressions of a common *Volksgeist*.

By his own kind of 'reasoning,' that fact would presumably, via the attribution of guilt by association of ideas in which he indulges, make Vonèche a Nazi-Fascist-Racist. Of course, it is possible to look at these putative correlations *mechanistically* (i.e., in the atomistic connotations of that term) and conclude that even if the facts were correct, there was only contingency, and no inner connection, among them. But such a deconstructive turn, borrowed in part from Derrida and de Man, might itself be taken by Vonèche as Teutonically inspired, given the obvious 'influence' of the Germans, Nietzsche and Heidegger, on deconstruction.

Therefore, putting Vonèche's own exhibition of 'blindness and insight' on a back burner, we may briefly examine his claims about the world, namely, the presumed association, on one hand, between mechanism and the putative Anglo-American-French tradition of democracy, rationalism, and individualism; and, on the other hand, between organicism and the supposed Germanic tradition of authoritarianism, anti-rationalism, mysticism and mass psychology.

Now, as I have already indicated, the slogans, *mechanism* and *organicism* each comprises multiple and often antagonistic connota-

[11] It is noteworthy that Bergson's *creative evolution* seems to have had the same kind of effect on Smuts as Vonèche claims it had on Piaget. One might also note the potent influence of Rousseau on Holderin (de Man, 1984, pp. 19-46).

tions (see Kaplan, 1967). Moreover, each has undergone depletions, additions and rearrangements in different places and periods. Although some have sought, for analytic purposes, to fix their meanings, typically in different ways, (e.g., Whitehead, 1925; Broad, 1925; Pepper, 1942, Dijksterhuis, 1950; Stark, 1963), they have often suffered the fate of "words that lose their meaning" (see J. B. White, 1984), and have, often enough, become exemplars of epithets in the service of invective. In this sense, they have been no different from other words of power abused for political purposes, for example, rationality, (see Wilson, 1971; Lloyd, 1984) democracy (see LaFaber, 1988).

With regard to such political-ideological uses of the terms, *mechanism* and *organicism*, it would be absurd to deny that, in certain periods of history, namely, in the Enlightenment (see Cassirer, 1932/1955; Gay, 1966) battle against the combined forces of the ancien regime – church, tradition, obscurantism, and arbitrary authority, *mechanism*, – metonymically representing Newtonian, scientific modes of thought, the elimination of supernatural intervention, the capacity for human beings to rearrange a diseased world and progressively secure a utopia of individual autonomy, personal liberty, and so forth, through Observation and Reason – was understandably, if irrelevantly, invoked.[12]

On the other hand, it would be not only mistaken, but fatuous, to claim, even for this period – Enlightenment, *siecle des lumieres, Aufklarung* – that mechanism and its cortege of slogans were inherent in, or the sole property of, a distinctive cluster of national groups – intrinsic aspects of their *Geist*. After all, it was a German, Immanuel Kant, who penned one of the outstanding manifestos for this period, "*Beantwortung der Frage: Was ist Aufklarung?,*" and who is often characterized as the Philosopher of the Revolution (Mead, 1936, pp. 25-49).

Conversely, it was in the heartland of 18th century English and French discourse that powerful voices (e.g., Edmund Burke, J. J. Rousseau) resounded, attacking some of the most cherished rationalist and cosmopolitan views of the 'Enlightenment,' and providing

[12] The metaphorical extension of Newtonian-like ideas through all realms of social thought and practice, in the service of undoing the ancient cosmos, and establishing a brave new world is brilliantly treated by Halevy (1960). The ironic Heraclitean enantiodromia of the Enlightenment vision is examined by Horkheimer (1947/1974), Horkheimer and Adorno (1947/1972); Arendt (1956/1968), and Talmon (1970).

viruses for the mystical, irrational Romantic counter-revolution in many lands. Holderin, one of Vonèche's Teutonic villains, was, after all, deeply immersed in, and admittedly indebted to, the thought of that ambivalent Franco-phone Genevan, Jean-Jacques Rousseau (see Gay, 1966; de Man, 1984).

On *l'autre cote de la rue, organicism* was scarcely of German provenance, but can be found advocated in the writings of John of Salisbury, of English origin, in the 12th century, not to go further back. To be sure, organicism, in its emphases on the importance of history, society and culture, was taken up by Germans in the 18th and 19th centuries, again as a political-ideological instrument, to combat the cosmopolitanism, deracination, and hubris they took to mark the policies of exponents of the 'Enlightenment.' But such 'organicism' as an instrument of reaction, was not and has not been peculiar to Germans, either in the 18th, 19th or 20th centuries. It was, as noted, strongly expressed by some 18th century English writers, for example, Burke, and also some early and later French 'nationalists' (e.g., de Maistre, Taine, Drumont, Sorel, Le Bon, Barres, Maurras, many of them as rabid, if not as 'effective,' anti-Semites as was Adolph (see McClelland, 1970).[13]

Finally, as pointed out, *organicism*, admittedly an instrument of 'reaction' in certain contexts, has been exploited, by anthropologists and others, through its often noted affinities to historicism and cultural relativism, as a potent slogan in the process of advocating 'liberation,' 'emancipation,' and 'self-determination,' against the baleful hegemony of bourgeois, capitalist, colonialist, and/or patriarchal rulers, who have sought to transform local practices, norms and values into cosmically sanctioned 'laws' or 'scientifically established' empirical generalizations. One might, *horribile dictu*, even locate Vonèche, in some of his discourse, among such "rebellious subjects and enemies to peace."

What is the upshot of all this? Certainly not that the issue of "recourse to root-metaphors" (world-hypotheses) is useless or irrelevant to the assessment of 'progress' in developmental psychology, although recourse to anything is irrelevant, unless one states

[13] The conditions and motives for manifestations of an 'organismic' orientation, (here, nationalistic, xenophobic, patriotic, irrationalist, etc.) in the most diverse societies, is alluded to in McClelland, and powerfully illuminated by Stern (1965). Their treatments, unlike Vonèche's, have little to do with the invocation of a national *Geist*.

what one means by progress, and one specifies criteria for determining whether the telos has been reached or approximated. Perhaps the real moral comes from Wittgenstein, one who had a foot in both of the Vonèche-erected divided and distinguished worlds: "Whereof one does not know, one should not speak."

Having dismissed the relevance of 'root metaphors' for his project, Vonèche next considers what those chained inside the Cave – members of 'the younger generation' of developmental psychologists – do and think in their unreflective pursuit of 'progress.' Here one finds, according to Vonèche, disenchanted novice masons, specializing in limited domains of puzzle-solving, trying to add a brick here and there to the amorphous edifice of 'knowledge,' seeking to emulate their image of 'hard scientists,' ignoring history, and working mainly to obtain recognition from their peers and rewards from their employers.

Vonèche is especially dismissive of those in the cortege of the Emperor with New Clothes, who take, as the sole or main criterion of progress in their discipline or in other disciplines the awarding of prizes and rewards to individuals or sets of practices. Such awards and rewards, Vonèche implies, are often not due to merit, but are rather generated by transitory fads and fashions, friendship patterns and gossip among the self-styled elite, citation patterns in the literature, log-rolling, and bandwagon effects.

The real innovators, the forerunners of the future, Vonèche intimates – in large measure because they reject the dogmas and doctrines of the mainstream, appear on the scene when the times are out of joint, or articulate ways of thinking and speaking that are alien or unintelligible to the members of the Court – for example, a Vico, are ignored, ridiculed or dismissed by their generation. They are no more welcome or rewarded than is the young child who disturbs the equanimity of the Court by pointing out that the Emperor is not wearing any clothes.

Finally, also suffering Vonèche's opprobrium, are those so simple minded as to judge something as progressive or an advance because it secures public approval or manages to penetrate the thought and discourse of the naive, ordinary, folk throughout a society, for example, psychoanalysis. No *Vox populi, vox Dei* for him.

Once again, without letting on what makes up his criteria of progress, Vonèche assures us that these views "contain more ele-

ments of social approval than *true* criteria of progress" (p. 156) and therefore, should certainly be relegated to the heap.

There is little point here in reviewing Vonèche's rehearsal of the five, six or seven traditional criteria advanced by those who *stipulate* that 'scientization' of any discipline should be the telos in terms of which progress *ought* to be assessed. I strongly agree with Vonèche that these criteria: "are inspired by a specific epistemological approach of [sic!] science, namely empiricism. Seen from another epistemological point of view, every one of these criteria would be questionable" (p. 157).

Nor will I comment on Vonèche's lucubrations on 'policy-research,' and the preoccupation of many psychologists with 'objects' constituted through life in the *Lebenswelt* (e.g., 'divorce') rather than with the alembicated, rigorously constituted abstract objects of real science. All of these reflections are clearly prolegomena to the main points that Vonèche wishes to assert:

> First ... the object of developmental psychology [unlike the object of the physics or astronomy] changes continually. Second, psychologists influence their object of study: self-fulfilling prophecy, Pygmalion effect, and so on. ... Third, developmental psychologists deal with down-to-earth and day-to-day problems such as toilettraining, breast-feeding and the rest. Fourth, they deal with ideology ... (p. 159).

All of these points, as Vonèche hints, might be taken to suggest that developmental psychology (and even much of psychology, in general) is not, cannot be, and ought not pretend to be, a natural science, but should, instead recognize its much closer affinities to history and narratology (including biography) than to physics, astronomy, or any other objectifying, nomothetically-oriented discipline.

Subsequently, as we shall see, Vonèche moves ambivalently in this direction. At this point, however, partly accepting the criteria of scientization he earlier elaborated and then criticized, he acerbically remarks: "The assessment of progress in developmental psychology is not easy because of the *methodological sloppiness* inherent to the very subject-matter" (p. 159). With that observation. Vonèche turns to try his hand at the historiography and meta-history (see White, 1973) underlying diverse histories of science. Although there is much of incidental value in Vonèche's observations, and even

more warranting discussion and dispute, I shall limit myself here to quoting his, not unexpected, pessimistic conclusions:

> The difficulty of such approaches are evident. They seem to be due to the nomothetic nature of the explanations put forth. It seems paradoxical to me to look for law and order in history (the domain in which progress ostensibly has to be assessed). Order is logical by definition. Lawfulness is an attempt at finding regularities in nature. Hence, both notions presuppose a deeper principle of deductibility. But history is not deductive, by definition. Thus, there is some internal contradiction in trying to find laws in history. Moreover, ... such an attempt leads to strong bias and to more difficulties than true solutions (p. 161).

In sum, insofar as history is unpredictable and imprevisible, (one might say the same of 'the development of individuals or societies,' under the usual interpretation of development), it is absurd to search for *general laws* in history (or development), although one is not precluded from using *general concepts* in the exploration and understanding of individual cases (see here, Louch, 1966).

It is under cover of these important but arguable theses that Vonèche is led to his project: "... to look for another approach that does not favor certain factors over others and contain too many internal contradictions" (p. 162). This approach is the above-mentioned move to idiography, an approach, one might note, that may as justifiably be taken to have its provenance (Dilthey, Windelband, Rickert, Weber, Cassirer, the *Methodenstreit* between the advocates of *Naturwissenschaft* and *Geisteswissenschaft*) in German 'ideology' as does the *organicism* Vonèche takes to be exclusively Teutonic; an approach that is still taken by many in the Anglo-American world to be a reflection of the pernicious obscurantism, irrationalism, mysticism, and regressive romanticism insinuated into Western thought by the malevolent Germans, and undercutting the progressive march toward knowledge, insured by nomothetic-deductive scientization.

Rebuffing the editorial question that prompted his title, Vonèche veers, for quite good reasons, to a radically different problematic: to attempt, via a single biography or case-study, and perhaps eventually through a collection of such case-studies, to ascertain the factors which determine the acceptance, rejection, neglect, suspicion, and surprise success of (any and all?) developmental theories. A cynic might discern here a nomothetic passion beneath the placid idiographic mask, but I will not dwell on that issue at this time.

Before I comment on Vonèche's case study or biography of his mentor and colleague, Jean Piaget, one should note *what* he is proposing and the *implications* of his proposal for developmental psychology. As far as I can see, using a gambit he attributes to Piaget, he sidesteps the issue as posed – jettisoning the *axiological* question of progress in developmental psychology – and gives himself the *empirical* task of describing "a resistible rise in the domain of developmental psychology" (p. 165), that is, a, so to speak, historical 'explanation' of how and why Piaget, through a variety of changing historico-cultural contexts, managed to attain eminence, renown and hegemony in his area of inquiry.

One may ask whether, in proposing this idiographic procedure, Vonèche is, in a not too indirect way, suggesting that the procedures of much of contemporary developmental psychology, outside of clinical settings, is like trying to play soccer with baseball bats or tennis with footballs? Is he hinting that if we wish really to understand human beings in their ontogenesis, we must focus on what William Blake referred to as "minute particulars" in an individual life history, and not, via an assortment of statistical techniques and experimental designs, take as our telos the achievement of some more or less vacuous empirical generalizations, based on the examination of "structures of variables under transition"?

Is he subtly proposing that, if one is really interested in the development (whether in the honorific sense or merely as change over time) of human beings, one must use procedures akin to those of the historians or story-teller, and forego aspirations to transform (developmental) psychology into a nomothetic 'natural science'?

Barring any response to these crucial questions and ignoring all of the massive literature pertaining to historical knowledge and the epistemic worth of autobiography and biography (e.g., Barzun, 1975; Danto, 1985; Dray, 1966; Foucault, 1980; Gay, 1985; Gunn, 1982; Krieger, 1974; McKeon, 1986; Mink, 1987; Novarr, 1986; Olafson, 1979; Olney, 1980; Petrie, 1981; Ricoeur, 1965; Segre, 1976; Toulmin, 1982b; Weintraub, 1975; White, 1978, 1980, 1982, 1985), Vonèche devotes about sixty percent of his paper to his interpretation of Piaget's career, and of Piaget's conscious or unconscious strategies, successful or failed, for achieving front-runner status, and maintaining a position at least one lap ahead of the next nearest horse in the Developmental Psychology Derby. Despite the

dominant position of a narrative of Piaget's life in Vonèche's presentation, I will ignore his mixed bag of obituary, archival information and questionable speculation, in part for reasons akin to those advanced by Robert Skidelsky (1987).

After reviewing a number of early 'models' of biography – the *whitewash biography* of the Victorian period, in which the sins of the great were hidden from the vulgar; the Georgian *debunking biography*, in which the protagonist is subtly denigrated (or damned with faint praise) by the narrator; the *reductionist biography* of the Marxist-Freudian variety, in which biographers were led not to report what happened in the history of the individual, but to *explain* it, to show that the individual's *res gestæ* were illusions, the real action taking place either in the unconscious or, if one were a Marxist, in the class-struggle – Skidelsky discusses the thesis, mainly advanced by those of a psychoanalytic persuasion, that there are intrinsic connections between personality, shaped by whatever circumstances, and achievement or reputation.

As Skidelsky notes, it is this attempt to account for achievement in terms of personality that has, in fact, become the central justification of biography. But Skidelsky, rightly I believe, will buy none of that. He asks: "In what way does knowledge of Einstein's life illuminate the theory of relativity, or knowledge of Picasso's love-affairs help us to appreciate his paintings?" Or, one might add, a superficial knowledge of Piaget's life illuminate either his accomplishments or his reputation? For Skidelsky and for me any justification of biography along these lines is bound to fail. I will return to this issue later on.

If Vonèche's 'biography of Piaget' is irrelevant, even for answering the question as to why Piaget did what he did and attained the eminence he has, is there anything that can be salvaged from his case-history? Perhaps the 'model' which Vonèche gradually uncovers, and which he summarizes schematically in the *Conclusion* to his paper.

On the basis, apparently, of his analysis of Piaget's career, Vonèche intimates – in general or in only one or a few cases? – that if one is to achieve success in the market place, it is not enough to work hard and long; one must realize that reputation for eminence, either among the cognoscenti in the field or among a wider community, is, like *goodness or the good life* (see Nussbaum, 1986):

"overdetermined" and subject to tyche and the imprevisible and uncontrollable vicissitudes of fate. Ripeness or readiness is all!

One must also both recognize and ignore the fact that, like death or taxes, there will always be different and incompatible-incommensurable world hypotheses in the domain of one's endeavor, others who operate with different epistemological, methodological and interpretive assumptions. One can only hope, like automobile manufacturers striving for a larger share of the market, that one's products are more appreciated by the learned consumers than the products of others.

Third, if one is to go beyond a worshipful appreciation by myopic specialists, one ought at least to try to show the scope of one's categories, their applicability to the most diverse realms of human interest. One need not, in this our world, concern one's self with one's reputation or identifiability by the readers of *Time Magazine*, *Le Monde*, *Pravda*, or the *Manchester Guardian*; but one should work in such a way as to receive mention if not use by those in other fields.

But, fourth, intimations of breadth or scope are not enough. One must also, in one's work manifest precision in specific inquiries, at least with respect to the criteria tacitly employed by those one wishes to impress. One may even lose the race in the end if the concerned onlookers discern a certain sloppiness of movement.

Fifth, one should realize that 'slow and steady wins the race.' "The introduction of novelty in science is better achieved by a strategy of little steps" (p. 185). One ought to try out one's ideas in a small group, then publish in a 'socially acceptable form in professional journals,' and, only later, publish a revised work in book form. To try to bypass these 'stages,' and leap from the post position into the lead may result in a 'broken leg' (presumably the reason why Piaget's philosophical essays were not 'well-received').

Sixth, especially if one is concerned with reputation for *la longue duree*, one should have patience and hope: one should recognize that history reveals 'swings of the pendulum' – what L. S. Feuer (1975) calls "the laws of wings" and "law of alternation" – one may expect, even though one is not around to witness it, that one's work will be marginalized in changed circumstances; and one may hope that one's ideas will be revived in a later future, at least partly with one's name attached to it.

Finally, in the hope of reputation and eminence, one should avoid being a 'joiner' in the inevitable disputes among the combatants in the 'field'; like the French intellectuals and 'strong poets' (Bloom, 1973, 1976) one should find one's distinctive path or seem to find one's singular way, among the mine fields, even at the cost of misprisioning or conveniently forgetting one's predecessors or the sources of one's ideas: a convenient amnesia for those who aspire to originality.

Now, one may grant – again a large waiver – that Vonèche's 'model,' his categories of interpretation, are applicable to Piaget. Is that it? A unique application? Or does he intend them to have a general import? In the latter case, he might want to express his hypothesis to account for or 'explain' attainment in developmental psychology or elsewhere, in that kind of pseudo-scientific formula so admired by social scientists, concerned with 'structures of variables under transition': $S = f(a, b, c, d, e, f, g)$, where S is success or achieved eminence in a field, and the lower case letters represent the 'variables' mentioned by Vonèche. Bright and ambitious 'normal science' apprentices might then seek to operationalize the 'variables,' including the optative ones. In this way, one might subtly suggest that idiographic studies can open the way to 'law-like' generalizations, and allow for the kind of 'covering law' model advocated by devotees of a unified science.

My own hunch is that Vonèche is remarkably ambivalent about this issue. On one hand, he seems to suggest that one can generalize from (his representation of) the vicissitudes of Piaget's career and reputation; on the other, he denies order or law in history, and is therefore, obliged, at best, to take Piaget's life[14] as a singular instance, informing us not at all as to how success or eminence, *in general*, has been or will be achieved in the past or future. For

[14] It should not be overlooked that this representation of Piaget's life and his manner of responding to various contingencies and conditions, is mediated through Vonèche. Although presented as if from an Archimedean standpoint, here, as elsewhere, the attitudes and especially the ambivalences of the narrator toward the subject cannot be ignored. Consider the different 'true stories' that Speakes, Reagan, Meese, Nancy and even Ronny might tell in writing up a case-study or history of Ronald Reagan's rise to eminence (see Kermode, 1983; Elliott, 1982). Proximity, intimacy, and co-nationality may be far from the best bases for choosing a subject.

purposes of either general description or advice to aspirants for prizes and honorary degrees, his 'case-study' is otiose.[15]

If one can scarcely use the vicissitudes and contingencies of Piaget's life as an archetypal-mythical model of 'what makes Sammy run' or 'how to achieve success and eminence' – and Vonèche surely knows this – then what purpose does it serve to represent, even schematically, the course of that life? Here we may get some partial insight from Skidelsky (1987).

It is Skidelsky's (1987) thesis that the only way in which biography as an undertaking can recover its main function is to go back to its roots. "These roots lie in ancestor-worship." He remarks further:

> Ancestors were – in many parts of the world still are – sources of identity, of wisdom, of encouragement. Tales of famous ancestors were ways of establishing family claims to positions; of bringing relevant testimony to bear on problems of living; of generating pride in a community's achievements (Skidelsky, 1987, p. 1250).

For Skidelsky, a biographer's most important contract is with the 'family' of the protagonist, and concerns the uses to which the famous man or woman can be put by descendants. (One may perhaps see in Vonèche's 'Georgian' treatment of Piaget, an ambivalence toward the heritage: a desire to participate in Piaget's glory and yet to suggest that Le Patron was less than an unqualified hero governed by an unwavering vision.)

A very important function of biography (manifest in Vonèche's treatment of Piaget) is to bring testimony from the past to bear on the present. The past, recent or otherwise, is ransacked in the service of modernity.

> The role of biography here is not to water dead flowers, but to give the assurance that that which has already flowered to some degree may achieve a more perfect flowering in the future. Every modern movement or tendency has its ancestral roll call, which connects what has happened with what might possibly hap-

[15] Although Vonèche alludes to the overwhelming role of any number of concurrent or subsequent contingencies in determining the reputation or eminence of an individual in a field, he scarcely mentions the role of reader response of contemporary and subsequent generations, the possibilities of transforming someone's work into an "industry," and the machinations of disprized media in canonizing someone and elevating him/her to sainthood (see, in this respect, Kermode, 1985).

> pen....The confidence we can have that certain arrangements for living can 'work' for us depends crucially on whether or not they have been tried out before (Skidelsky, 1987, p. 1250).

Skidelsky concludes:

> Biography should not try to explain or illuminate achievement for us, which it cannot do. Rather the fame of the subject, because of which the biography is written, gives 'permission' for a way of life. This links modern biography to what has always been its main purpose: to hold up lives as examples. The life itself is an achievement; not the explanation of it. What distinguishes modern biography from more traditional ancestor worship is the much greater range of ancestral lives on offer. Whereas in the past the exemplary principle worked in favour of tradition, today it works in favor of pluralism. This reflects great changes in social and historical circumstances; in our attitude to work and play, to ourselves, and to our relationships with others and the world. Biography relates grand movements of consciousness to patterns of individual lives. It tells us stories, which cause us to dream, and from our dreams to make plans (Skidelsky, 1987, p. 1250).

I do not mean to suggest that Skidelsky's analysis is exhaustive or conclusive. It does, however, provide a rationale for Vonèche's devotion of so much of his manuscript to the life and hard times of Piaget. It is, to be sure, not an *Imitation of Christ*, but certainly to some a 'hero for our times,' warts and all, that may serve as an incentive for Genevans and members of Piaget Societies throughout the world.[16]

Such a 'biography' of an exemplary figure in the Western alliance (France, England, America) is perhaps especially imperative at this time, when one can spy on the horizon, already adumbrated by hagiographies and already securing 'converts,' a new Buddha, slouching toward Bethlehem, one whose doctrinal genealogy can easily be traced back to the malevolent, organismic, holistic, totalizing Teutons. I refer here to Vygotsky (see Wertsch, 1985), either alone or linked with Bakhtin (1981; Clark & Holquist, 1984). Although discussions of Vygotsky occasionally allude to Heinz Werner's influence, it seems to me that the significance of Werner's

[16] I cite here, without further comment, Freud's remarks, "Whoever undertakes to write a biography binds himself to lying, to concealment, to hypocrisy, to flummery, and even to hiding his own lack of understanding." Ironically, this statement appears in Jones' biography of Freud.

innovations in method are underplayed. I may also note, without stressing issues of priority, the fundamental similarity in the basic categories of Werner and Bakhtin: namely, the emphasis on the universality of forces of differentiation and integration in all of existence (see Kaplan, 1967). Despite Vonèche's obituary for an 'organismic-developmental' approach, Werner's way of construing development may, in a 'swing of the pendulum,' be resurrected (see Kaplan, 1983). To be sure, that resurrection, in spirit if not in name, will require ideal readers.

If we put to one side the possible hortatory value of the bulk of Vonèche's paper for the kin of the Godfather, what else can we take from it, we who do not worship, even ambivalently, at the shrine of Le Patron?

It should be clear that, in my view, Vonèche neither addresses the issue posed to him by the Editors nor presents an alternative model for 'developmental psychology' or 'theory building' in developmental psychology. Was it, then, all for nought?

Not at all! What Vonèche has done, wittingly or not, in a professional context in which certain metaphysical, epistemological, methodological and hermeneutic pre-suppositions as well as aims and purposes are taken for granted, is explicitly to put many of those assumptions in question, and, obliquely, to suggest, in a global way to be sure, certain new lines of inquiry for a future generation of developmental psychologists – lines that will, perhaps, link them more closely with the human sciences. Although I do not believe that he has gone far enough (see Kaplan, 1983a, b), this voice, from one of the now revered centers of developmentalism, may serve to arouse the somnolent field of developmental psychology from its dogmatic slumbers.

A final, personal, remark: I do not see developmental psychology going anywhere until developmentalists realize that they do not occupy a limited field of psychology, but constitute a group dedicated to a way of looking at all aspects of psychology; until they recognize that psychology is not an encapsulated discipline, but is inextricably linked with politics, economics, anthropology, and all of the humanities; until they disavow their methodolatry of the natural sciences and reject the shibboleths of those who insist that we must go beyond the 'manifest image' of human beings to reconstruct a 'scientific

image'; until they ask about theories advanced and work done, Cicero's classic question, "Cui bono?"

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