

Conclusion:

These rules are tacitly known by the speaker, but they are neither learned (by induction, problem-solving or trial-and-error), nor determined by some general necessity. General intelligence and sensorimotor schemata cannot even begin to explain what is happening.

Chomsky's point failed to impress Piaget and the Piagetians. A lot of their counter-arguments turned on the possibility of explaining these facts "in some other way". One could not fail, I think, to be impressed, there and then, by the fact that no other way was actually proposed, but that it all turned around the sheer possibility that some other rule, at some other level, might explain all of the above. (Anthony Wilden even tried out Russellian logical types¹⁴ to no avail.) Wisely, and unflinchingly, Chomsky kept replying that this might well be the case, but that he did not expect it to be the case. (In fact, many years have gone by, and these alternative explanations are still sorely missing – for a precise account, firmly grounded in generative grammar, but altogether charitable to the Piagetian viewpoint, see Jackendoff, 1992.)

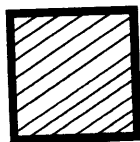
And finally came Fodor's *jeu de massacre*, one of the truly high points in the whole debate.

5.4. Fodor's demise of learning

His argument was not limited to language, but applicable to any theory of learning by means of conceptual enrichment. He went squarely against the very core of the Piagetian system. Nobody, in the other camp, really understood his argument at first (not even Putnam, in his critique, written after the debate),¹⁵ so let us try to simplify it drastically, still preserving its force:

*Fodor's argument against learning by enrichment*¹⁶

The typical situation of belief fixation or "learning":



is an exemplar



is not an exemplar

¹⁴LL, pp. 117–121.

¹⁵See his exchange with Fodor in Part II of LL.

¹⁶LL, pp. 143–149 and the ensuing discussion. The argument had been developed in greater detail by Fodor in his 1976 essay *The Language of Thought*, Hassocks: Harvester. (Reprinted in 1979 by Harvard University Press.)

Inductive hypothesis:

X is an exemplar if and only if X is Y

Target of the learning process (for example): come to the conclusion that

$Y = miv = \text{"square and gray"}$

(Alleged) stage 1: the subject has access, separately, to the concept "square", to the concept "gray", but not to the conjunct "square-and-gray".

(Alleged) stage 2: the subject constructs a tentative new concept Y, and tries it out on the experimental materials.

Y is not yet "*miv*"

Stage 3: the subject correctly comes to the conclusion that Y must be:

"*miv*" = "square-and-gray"

Fodor's argument: If this is the case, that is, if the language at stage 3 is really more powerful than the language at stages 1 and 2, then this transition cannot be the result of learning, it cannot come from induction.

Mini-proof:

At some point the subject must formulate the hypothesis that Y is true of (applies to, is satisfied by) all and only those things which are *mivs*, that is, which are square and gray. But this cannot happen unless the subject has the concept *miv*.

Unless Y is to all intents and purposes indistinguishable from *miv* (from "being square and gray"), we have no idea whatsoever of what Y could be.

Therefore: the language of stage 1 is not weaker (less powerful, more limited) than the language of stage 3. You always start with a language which is at least as powerful as any language which you can acquire.

Where do all these concepts come from?

Fodor's three hypotheses:

- (a) they are innate
- (b) God whispers them to you on Tuesdays

(c) you acquire them by falling on your head

Dismissing hypothesis (b), the only plausible conclusion is that they are innate (and/or arise for totally endogenous reasons, due – for instance – to a stepwise brain maturation. This latter is just a slightly less fancy version of hypothesis (c).

The audience was really impressed, Monod most of all. There was a distinct flavor of paradox in Fodor's position and he did not try to hide this fact. The whole argument sounds paradoxical, yet it is perfectly compelling. Fodor, to his own regret, cited, just as one concrete, indubitable, patented, example of a logical system which is "more powerful" than another, the case of propositional logic (a provably weaker system) versus first-order quantificational logic (a provably stronger system which contains the former as a sub-system). This created a lot of misunderstanding and endless discussions about the history of the discipline of logic. Fodor's thesis remained essentially unchallenged (in the book, it will be Hilary Putnam to accept the challenge, but that was after the debate).

In the aftermath of Fodor's onslaught on induction and learning, most of the ensuing debate revolved, on the one hand, around plain misunderstandings of his and Chomsky's position (leading to clarifications and reformulations) and on the other, around a rather idle insistence that their position appears vastly implausible and even paradoxical. This apparent implausibility was never denied by Fodor and Chomsky, but with the crucial proviso that the appearance of paradox persists only if we maintain the traditional assumptions of the domain. Their main point, however, was to subvert these very assumptions, not to maintain them.

I surmise that, then as well as now, those who have concluded that the debate was won by Piaget did so solely on the grounds that the theses defended by Piaget sounded intuitively very plausible, while the theses presented on the other side sounded preposterous. It does not seem to occur to them that, in science, even preposterous hypotheses often turn out to be true (Piattelli-Palmarini, 1986, 1989).

5.5. *Sequels to the debate*

After the debate, Putnam, at least, took the trouble of explaining why these theses sound preposterous, and made an attempt to construct an attenuated version of what he thought was right in Chomsky's and Fodor's positions (even suggesting that they come closer than they believe to Piaget's).¹⁷ This line of

¹⁷LL, p. 300.

resistance against strong innatism and meaning atomism in the domain of lexical-conceptual semantics has been vastly expanded in subsequent philosophical works by Putnam, Fodor, Dennett, Millikan, Loar, Burge and others. I must leave it out, for reasons of space. I must also leave out the interesting sequels elicited by Kripke's essay on Wittgenstein (Kripke, 1982) revamping a special brand of skepticism about the notion of "following a rule", which have met interesting rejoinders by Chomsky (1986), Horwich (1984), and others.

The whole recent debate on connectionism has revamped several of the anti-specificity theses already present in Piaget (the most explicit link between the theses of connectionism and the Piaget–Chomsky debate has been made, in the domain of lexical-conceptual learning, by Ray Jackendoff (1992). Connectionist architectures are, in fact, a concrete embodiment of the idea of "order-from-noise". At the time of the debate Piaget could only summon in defense of the "order-from-noise" paradigm the physicochemical theories of Prigogine and his school, and some rather confused speculations by Heinz von Foerster (1960). Had he lived long enough to see present-day connectionism, I am persuaded that he would have endorsed it wholeheartedly. In fact, an implicit alliance between Piagetism and connectionism is amply consummated (Elman, 1989). This is not the place and time to re-examine the controversy on connectionism (Pinker & Mehler, 1988), but I wish to stress that many of the recent polemics do find their roots, *ante litteram*, in the Royaumont debate.

6. What happened ever since in linguistics and language acquisition

In the rest of this paper, I will briefly present a number of further developments which support the positions defended by Chomsky and Fodor. And I will do it precisely by showing that their theses, preposterous as they may have seemed, are presently the only plausible explanation for a variety of facts concerning language, language acquisition and cognitive development. The swing of the pendulum in their favor has not only continued, but has gained further momentum.

6.1. *The new turn in linguistic theory*

From October 1975 to the present day, the brand of linguistic theory called "generative grammar" has undergone an unprecedented growth. As a consequence of previous partial success (and, of course, past errors), Chomsky and others have developed the so-called "government-and-binding theory" (Chomsky, 1981, with important antecedents in work published in 1979 and 1980) (Chomsky, 1980), and more recently the "minimalist framework" (Chomsky,

1993). Referred to also as the “principles and parameters” approach (Giorgi & Longobardi, 1991; Haegeman, 1991; Lasnik & Uriagereka, 1988; Rizzi, 1990; van Riemsdijk & Williams, 1986), the turn of the 1980s has been a revolution within the revolution, bringing both genuinely novel features and extensions to the picture presented by Chomsky at Royaumont. The line of argument developed orally at Royaumont, and then sharpened in the written proceedings, has not only been preserved in the new theory, but even made more radical.

Just to cite a few examples, some of the oldest and most central notions of traditional grammars (subject, object, grammatical construction, phrase-structure rules, etc.) are now demoted to epiphenomena of much deeper and much more abstract notions of generative grammar. The importance of the lexicon has grown explosively, to the point that some linguists (certainly Chomsky himself, especially after 1992, in the “minimalist framework”) now claim that acquiring the lexicon is almost all a child has to do, in order to acquire a language. Everything else is generated by a strictly invariant, language-specific computational system, and by the several output conditions arising at the many interfaces of this computational system with other internal mental systems. Inevitably the inner structure of each lexical item (particularly the inner structure of verbs, adverbs and adjectivals) has become much richer and much more abstract than it ever was in the traditional grammars (Giorgi & Longobardi, 1991; Grimshaw, 1990; Hale & Keyser, 1993; Higginbotham, 1983; Jackendoff, 1990; Keyser & Roeper, 1992; Tenny, 1988). None of the highly abstract and tightly knit principles and parameters of universal grammar, nor any of the specific rules of the “core grammar” of a particular language, bear any resemblance whatsoever to derivations from non-linguistic principles (even less from sensorimotor schemata). The irrelevance of abstractions from motor schemata even in the development of sign languages is particularly striking (a point to which I will return). The exploration of possible phonemes and syllables by the congenitally deaf child, in the course of “babbling in the manual mode”, shows a marked linguistic specificity in its ontogenesis, not a continuity with generic manual gesturing (Petitto & Marentette, 1991). The case against any derivation of linguistic structures from non-linguistic gestures, and/or from the perception of generic movements, appears clear-cut even for the most “obvious” candidates, namely time relations (Hornstein, 1990), agent/patient relations (Grimshaw, 1990; Pinker, 1989), aspectual semantics (Tenny, 1988), the geometry of events (Pustejovsky, 1988), verbs of movement and change of possession (Jackendoff, 1983, 1990, 1992), reference (Higginbotham, 1985, 1988), and even co-reference in sign languages (Kegl, 1987).

The richness and depth of these recent developments as a whole is a proof that the program presented by Chomsky at Royaumont was sound and productive, while the very idea of a continuum between language and non-linguistic “precursors” was doomed. Many of the objections raised during that debate seem to

me to be automatically voided by the tumultuous progress made in generative grammar in the last 15 years or so. The extreme specificity of the language system, indeed, is a fact, not just a working hypothesis, even less a heuristically convenient postulation. Doubting that there are language-specific, innate computational capacities today is a bit like being still dubious about the very existence of molecules, in spite of the awesome progress of molecular biology.

There are, nonetheless, also particular data and particular developments that seem to me to refute the most fundamental tenets of Piagetian psychology (there never really was, nor will there ever be, a “Piagetian linguistics”). Limitations of space demand that I concentrate on these, and only these. I will do it briefly, and I will do it bluntly, because I think that these recent developments are blunt refutations of the most central “classical” assumptions then entertained by the Piagetian school.

6.2. *The knock-down case of pronouns in the congenitally deaf child*

First and foremost, I will sketch a perfect case against the dependence of language on motor schemes. It is based on a clear-cut, elegant and devastating piece of data from the acquisition of sign language by the congenitally deaf child. I give special privilege to this case, because it looks a lot like those decisive experiments in physics and in biology which refute, by a single stroke, long-entertained hypotheses. It is due to the psycholinguist Laura-Ann Petitto of McGill, probably the only researcher who has worked in depth both with chimps and with congenitally deaf children. (By the way, if language indeed were supervenient on motor schemata and on general intelligence, then chimps ought to have language, which is clearly not the case, as definitely shown, after the Royaumont debate, by Premack, Terrace, Bever, Seidenberg and by Petitto herself) (For reviews, see Premack, 1986; Roitblat, Bever, & Terrace, 1984).

The counter-case of pronouns in the congenitally deaf (Petitto, 1987)

If language were continuous with (prompted by, isomorphic with, supervenient on) motor schemata, then sign languages should show this causal dependency in a particularly clear and transparent way.

Within sign languages, the case of “constructing” personal pronouns (which superficially look a lot like pointing) out of generic pointing ought to be even more transparent.

It turns out that this is not the case. Data from the acquisition of pronouns in the congenitally deaf, in fact, show that:

- (1) Non-linguistic pointing is present and widely used long before pronouns are used.
- (2) Yet, pronouns appear suddenly (within a couple of weeks) and at exactly the same age in the hearing and in the congenitally deaf child.
- (3) In the few weeks preceding the appearance of linguistic pointing (i.e., pronouns) in the congenitally deaf child, generic pointing temporarily disappears.
- (4) Paradoxical as it may seem, the hearing child and the congenitally deaf child make the same mistakes in their initial use of pronouns ('me' to mean 'you', 'you' to mean 'me', etc.)

NB: This means "pointing" at oneself to refer to someone else, and vice versa. This kind of mistake is uniquely linguistic (as demonstrated by the exact parallelism with the hearing child) and never happens with generic pointing. If motor skills and general intelligence were involved at all, then these mistakes would be a disturbing symptom of deep-seated motor troubles and/or of an intolerably low level of "general intelligence" (of course, neither is the case).

- (5) Another piece of conclusive evidence (Petitto & Marentette, 1991) is the autonomy and the specificity of babbling in the ontogeny of sign languages. There is no hope any more of deriving from motor schemata even the form of syllables in the manual mode, let alone the meaning of words and the structure of the sentence. And, since there is such a clear discontinuity even between generic gesturing and the basic elements of sign language, one can safely dismiss the alleged continuity between gestural schemata and the structure of the sentence in spoken languages.

In my opinion at least, this is precisely the way a scientific hypothesis is definitively refuted: you make it as clear, as specific and as predictive as possible, then look for an ideal experiment, one in which the phenomenon stands out unambiguously, in its purest form, then see whether the experiment confirms or refutes the hypothesis. The rest is idle discussion on vague metaphors.

6.3. *On the inexistence of horizontal stages*

There are numerous other recent (and some not so recent) data which militate powerfully against the very existence of Piaget's horizontal stages.

Let us be reminded that a horizontal stage à la Piaget is one in which a concept or a "logical operation" is either present or absent *in toto*; its presence or absence

will show up no matter which problem you present to the child, from mathematics to gardening, from geometry to economics from language to moral judgement. Well, there are overwhelming reasons to believe that these horizontal capacities simply do not exist. Every direct or indirect proof in favor of the modularity of mind (Fodor, 1983; Garfield, 1987), of the content-drivenness of problem-solving, is *ipso facto* a disproof of the existence of horizontal stages. Let's see some of the most salient ones:

Evidence for the non-existence of Piaget's "horizontal" stages

Domain specificity (modularity) of problem-solving:

- (a) At a given age, the child who applies conservation to one kind of problem (involving volume and weight) does not apply it to other kinds of problems (involving speed, temperature, concentration, etc.).
 - (b) The conservation of identity is qualitatively different for different conceptual kinds (animals, artifacts, nominal kinds, etc.) and the conceptual transition to conservation of identity or kind takes place at different ages for each of the different kinds.
- (For reviews, see: Carey, 1985; Keil, 1979, 1986; Markman, 1989)

Typical Piagetian "illogicalities" can be elicited also in the adult:

- (a) The power of what Amos Tversky and Daniel Kahneman have called typicality, anchoring and ease of representation (Kahneman, Slovic, & Tversky, 1982) has nothing to do with "having" versus "not having" a concept. It has to do with the domain to which our intuitions of what is "typical" apply. Cognitive strategies, and the underlying "heuristics and biases" often do not generalize from one domain to the next, not even in the adult.
- (b) There would be no end to the succession of "stages", well up to, and beyond, the level of Nobel laureates (Piattelli-Palmarini, 1991, 1993).
- (c) Some of our intuitions about typicality are the opposite of what we should derive from actual experience (the signal case is offered by intuitions about probability).

In many cases, there is no abstractive assimilation at all of objective external structures by the mind, and even less so a mandatory, logically determined one.

Many of the typical experiments à la Tversky and Kahneman replicate exactly the qualitative results obtained by Piaget and his collaborators on children. The

simple secret is to change the domain. The typical experiment à la Piaget was to elicit from, say, a 5-year-old the judgement that there are more girls than children in a photograph, more roses than flowers in a bouquet, more cars than vehicles in a drawing, etc. The explanation was given by Piaget as: Lack of possession of the concepts of set/subset/super-set. But the majority of highly educated adults judge that there are more seven-letter words of the form

----ing

(where each “-” stands for a letter whatsoever) than there are of the form

----i--

They judge, just like the 5-year-old child, that the subset has *more* members than the super-set. These same educated adults will judge that there are more words of English which begin with an “r” than there are which have “r” in the third-to-the-last position (the reality is that there are vastly more words of the second kind than of the first). Do they lack the concept of subset? No! The correct explanation has to do with our intuitions of what is most typical of a kind, and what we can easily represent mentally. The easier it is for us to mentally generate typical members of the set, the larger the set appears to us. The same applies to the child: it is easier to mentally generate typical instances of a simple set (boys, roses, flowers, etc.) than to generate instances of a disjunctive set (children = boys or girls; flowers = roses or carnations or ...; vehicles = cars or trucks or ...). What counts is familiarity with, ease of representation of, and typicality of, the standard exemplar of a specific set, in a specific domain. There is no “horizontal” lack of a certain concept everywhere. These “heuristics and biases” are never horizontal, but always vertical, domain-specific, in a word, modular.

6.4. *Further counters from linguistics and from biology*

Let me now come back to language proper, and to evolutionary biology. I will conclude with a drastic simplification of recent progress in these domains which bears direct, and rather final, negative consequences for the core Piagetian hypotheses, as presented at Royaumont.

Recent developments in linguistic theory

(I chose those which further exclude any continuity with sensorimotor schemata, and make “language learning” an empty metaphor.)

- The essentials of syntactic structures are derived (“projected”) directly from the lexicon (and, of course, there are no motor “equivalents” to the lexicon).
- Rules are replaced by principles and parameters. Most (maybe all) parameters have only two possible positions. “Learning” a given language means acquiring the lexicon and (in the most recent “minimalist framework” (Chomsky, 1993), one should rather say “thereby”) setting the correct values for all the parameters.

(Language acquisition is not an induction, but a selection: Lightfoot, 1989; Piattelli-Palmarini, 1989.)

- There are in every natural language (sign languages included) silent elements, phonetically inexpressed particles called “empty categories”, and these cannot be “learned”, because they are not part of the sensory, explicit, input to the learner.

(Language acquisition cannot be based on imitation, generalization and assimilation.)

- Linguistic principles are highly specific, they bear *no* resemblance to general “laws of thought”, and have no explanation in terms of communicative efficacy.

(Self-regulation, adaptation and pragmatic expediency explain nothing at all in this domain.)

(The best, yet still unconvincing, adaptationist reconstruction is to be found in Pinker & Bloom, 1990, see also the peer commentaries to that paper.)

- The form of linguistic principles is very specific, mostly stating what cannot be done to highly abstract and uniquely linguistic elements, categories and constructs (based on notions such as c-command, X-bar, PRO, projection of a lexical head, trace of a nounphrase, specifier of an inflectional phrase, etc.).

The typical principle of universal grammar sounds a bit like the following:

“do whatever you please, but never do such-and-such to so-and-so.”

(There is no hope, not even the dimmest one, of translating these entities, these principles and these constraints into generic notions that apply to language as a “particular case”. Nothing in motor control even remotely resembles these kinds of notions.)

(For a clear, global presentation of this theory, see Haegeman, 1991. For the recent minimalist framework, see Chomsky, 1993; for the parametric approach to language acquisition, see Lightfoot, 1989; Manzini & Wexler, 1987; Piattelli-Palmarini, 1989; Roeper & Williams, 1987; Wexler, 1982; Wexler & Manzini, 1987; and the vast literature cited in these works. For the existence of “empty categories” in sign languages, see Kegl, 1986, 1987.)

Last, but not least, modern biology and evolutionary theory offer further and more radical reasons to refute Piaget's basic tenets about life and evolution:

Knock-down arguments from modern evolutionary theory

- No inheritable feedback is even remotely possible from individual experience to the genes.
- The metaphor of “problem-solving” as a driving force in evolution (in particular in speciation; Schull, 1990) is wrong: each species creates its own specific problems (Lewontin, 1982, 1983, 1990b, Piattelli-Palmarini, 1989, 1990b, 1990c).
- Novelty and complexification do not logically, nor even factually, imply an “enrichment”, since they often arise as a consequence of impoverishment and specialization. (The possibility of evolutionary complexification by means of impoverishment was first demonstrated in bacteria by Boris Ephrussi; see Jacob, 1977, 1987.)
- Life is “basically” what it is: the old grand theories (auto-equilibration, minimization of disturbance, increasing autonomization, increasing adaptation, increasing order from noise, etc.) have never explained anything. It proved impossible to deduce biological structures and functions from first principles.
- Even the most transparent (one would have said) instances of “adaptations” of organs-cum-behaviors to environmental conditions are sometimes fallacious. (Ten different elaborate kinds of mouthpiece organs for cutting, crunching, searing and syphoning have evolved in insects one hundred million years before there were any flowers on earth (Labandeira & Sepkoski, 1993). Until very recently (July 1993) these organs had universally and “obviously” been judged to offer examples of exquisite and fine-tuned selective adaptations to the environment and to the mode-of-life of their bearers.)
- Biological evolution is not (at least not always) gradualistic (Eldredge & Gould, 1972; Gould, 1984; Gould & Eldredge, 1977, 1993; Gould & Lewontin, 1984; Gould & Vrba, 1982) and does not (at least not always) proceed through a stepwise combinatorial enrichment out of pre-existing more “primitive” structures. The brain, for one, did not evolve by piling up new structures “on top” of older units (Changeux, Heidmann, & Patte, 1984; Edelman, 1987).
- Selection out of a vast innate repertoire is the only mechanism of growth, acquisition and complexification which we can scientifically understand (Piattelli-Palmarini, 1986, 1989, 1990a). (The theory of, and data on, language acquisition in the “principles-and-parameters” framework confirm the success of selective theories in the domain of linguistics – as rightly foreshadowed by Chomsky, Fodor, and Mehler at Royaumont.)

7. Conclusion

I may seem to have been saying rather harsh things about Piaget and his school. This was not my intention. I think there are overwhelming reasons to conclude that his approach was fundamentally wrong, but this is no judgement on his personal merits. It took a great mind to draw such a vast and coherent picture, one that is still attractive to many developmental psychologists the world over, one that appeared as deep, novel and important to many researchers in a variety of fields, from the philosophy of science to anthropology, from ethics to sociology, from mathematics to feminist studies. He certainly introduced, or rather reintroduced, into psychology a much-welcome rationalistic and anti-empiricist stance, combined with an unerring flair for experimentation. I am told by the best present-day experimentalists in cognitive development that, even if his interpretations of the data are often wrong, the reproducibility of his original data is always next to perfect. In hindsight, and judging from a different theoretical frame, we see that often he did not perform the next inevitable check, or the decisive counter-experiment, but he never erred in what he actually did, or in telling what he actually found. Much of present-day experimentation on the child's cognitive development stems, directly or indirectly, from his classic experiments and those of his collaborators.

Piaget was truly a "universal" thinker, with an insatiable curiosity for facts and theories well beyond his profession. He had an encyclopaedic mind, and was, alas, one of the last global intellectuals. Most of all, he brought to perfection, and elaborated down to the most minute details, a theory which was intuitively *very* appealing. This, as I have endeavored to show, was his strong point, and also his great weakness. The very basic intuitions, to which Piaget brought order and depth, and between which he established unprecedented systematic interconnections, have turned out to be wrong, misleading, or empty. They were, indeed, *prima facie* very plausible – no one would want to deny that – but often in science the implausible must triumph over the plausible, if the truth lies on the side of the implausible.

This is what Piaget refused to accept, to the point that, in spite of his towering intelligence, he could not understand the message brought to him by Chomsky and Fodor at Royaumont. For the ideological reasons so well explained by Chomsky at the Royaumont debate and elsewhere, in the domain of psychology and linguistics (at odds with physics, chemistry and molecular biology) hypotheses that appear, at first blush, preposterous are often simply assumed to be wrong, without even listening to reason, proof or experiment. With these notes, I hope to contribute just a little to the demise of this strange and irrational attitude in cognitive science. Also in linguistics, in psychology and in cognitive science the *prima facie* implausible can turn out to be true, or close enough to the truth. In fact, my main point here, as in previous articles (Piattelli-Palmarini, 1986, 1989, 1990a), is that it already has.

I wholeheartedly agree with what Chomsky said at the very end of the debate. Little of what we hypothesize today will survive in the long run. Twenty or fifty years from now we will probably have gained much deeper and much better insights into these matters, and not much of present-day theorizing will still be valid. But what is important is that we may look back and ascertain that those hypotheses, those explanations, were at least on the right track, that they were of the right kind. As I endeavored to show, at least this much is already happening now, with respect to the debate.

In this sense and in this sense only, I have allowed myself the liberty of speaking of “winners” and “losers”. The race is mostly still ahead of us, and all I have offered here are arguments in favour of a certain choice for the kind of competition still to come.

A final, very personal touch: I have fond memories of my conversations with Jean Piaget. I was always impressed by his *bonhomie*, his wit, his eager search for better understanding, his serene attitude towards life. He has run a long, difficult race, and has left a highly talented multitude behind him. No one could have led that race with greater aplomb, and no one ever will. It is no paradox, I believe, to admire him for his great achievements, but also feel sorry for the path he insisted on choosing. It was a bit painful, at least for some of us at Royaumont, to see him lose an important confrontation, one which he had eagerly sought, without fully realizing what was happening to him, and to his most cherished ideas, and why. His search for a compromise was unsuccessful, simply because the compromise was neither possible nor desirable.

I heard Gregory Bateson, after the meeting, define Piaget as a “lay saint”. He was implying, I believe, that Chomsky and Fodor had fulfilled the ungracious role of executioners. But it would be a paradox to admire Piaget as much as Bateson did, and still wish he had been lulled by the false conclusion of a possible compromise. Not even the saints appreciate such forms of inordinate devotion.

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