ways maintained that logico-mathemental structures are not derived from language (an empiricist hypothesis) but from the general coordination of actions, with their permanent functional mechanisms of ordering (order of movements), embedding (of a subscheme into a total scheme, for example), establishing correspondences, and equivalences. All these factors intervene in the construction of numerical quantities, and they obviously suppose an innate neurological and organic functioning; as long as only such functioning is involved, without structural hereditary programmation. I accept the necessity of an innate point of departure.

This being so, a second problem immediately arises: What are the mechanisms which are necessary for this innate functioning to proceed toward the completed structure? In other words, is it necessary to suppose a progressive building up of structures which were not initially contained in the functional kernel? On this point Mehler and Bever's arguments seem equivocal; with regard to the psychological problem of number (we are not talking about linguistics), they continuously oscillate between a transformational functionalism and a preformational structuralism. They argue as if the innate kernel consisted nevertheless of some sort of preformed structure, which becomes veiled by bad "perceptual strategies" or by unsuccessful "performances," but which reappears when better circumstances permit.

We would like to ask two further questions. What are the conditions that cause the strategies employed sometimes to counteract, and sometimes to favor, this innate kernel? And once the obstacles have been overcome, is the final structure the same as the structure that existed at the age of 2½ to 3 years, or has it been transformed and enriched, and if so, why? These two questions are closely linked to one another and dominate the problem mentioned earlier: if the final structure is richer than the initial one, a construction must have taken place, and preformation is not the answer; moreover, the intermediate strategies must all have contributed to this construction and cannot be considered as good or bad in function of a (falsely) absolute model, since they constitute the necessary stages for the completion of this construction.

What is lacking in Mehler and Bever's argument is the concept of productive actions and of the operations which stem from these actions. The fundamental characteristic of operations is that they produce novelties (empiricists try to explain this fact by exogenous learning) by means of "reflexive abstraction" from actions at an earlier stage (and it is this endogenous process that presupposes a functional kernel with innate roots). Mehler and Bever only consider, on the one hand, what is innate, and on the other, perceptions and performances. Thus they cut the link between the subject and exterior reality, which leads them to consider the former as sometimes a winner, sometimes a loser. By constrast, the concept of operations explains, and is the only way to explain, how an initial functional kernel yields completed structures, that is, by a series of self-regulations and equilibrations in which even the errors play a functional success-promoting role.

To conclude, Mehler and Bever invoke an innate structure which supposedly accounts for early correct answers (we have interpreted these answers in a different way) and for the final successes but which does not explain why the structure is overpowered so easily during the intermediate stages, or why the final structure is richer than the initial one. I maintain that when these facts are explained, the concept of an "innate structure" becomes superfluous, that an innate functioning is sufficient. I maintain above all, that when the rather Manichaean notion of good and bad structures is replaced by an adequate theory of progressive equilibration starting from self-regulation, the idea of construction will prevail over that of preformation; for, as the great biologist Dobzhansky has said, though predetermination is impossible to disprove, it is on the contrary (and I would add, precisely for that reason) completely useless (*9*).

References and Notes

1. J. Mehler and T. G. Bever, *Science* **158**, 141 (1967).
2. For example, N. Chomsky, *Aspects of the Theory of Syntax* (M.I.T. Press, Cambridge, Mass., 1965).
3. M. Laurendeau and A. Pinard, *Les premières notions spatiales chez l'enfant* (Délachaux et Niestlé, Neuchâtel, in press).
4. In our previous experiments with A. Szeminska (see ref. *5*), not all our subjects without conservation between 4 and 5 years of age considered the longer row as containing more elements; some (and these were not the most advanced) considered that there were more elements in the crowded row. Mehler and Bever's results now suggest that such answers show a residue of an earlier stage at which the criterion of quantification is either crowding or sometimes crowding and sometimes length.
5. J. Piaget and A. Szeminska, *La genèse du nombre chez l'enfant* (Délachaux et Niestlé, Neuchâtel, 1941); translation, *The Child's Conception of Number* (Routledge and Kegan Paul, London, 1952).
6. Five subjects from 2 years, 3 months to 2 years, 7 months; ten subjects from 2 years, 8 months to 2 years, 11 months; nine subjects from 3 years 0 month to 3 years, 4 months; and five from 3 years, 5 months to 3 years, 10 months; as well as three subjects from 4 years 0 month, to 4 years, 1 month, whom we have excluded, though reactions were analogous to those of the younger subjects.
7. It may be worth pointing out that misuse of the term "conservation" also occurs in the work of other authors. Bower, in his admirable experiments on newborn babies, seems to have proved (but his results will have to be confirmed by others) that at the end of the first week of life the baby already recognizes an object which has been hidden behind a screen and which is then shown to him again; or at least, that the baby distinguishes this object from another one. However, this only proves that recognition is a very early phenomenon, which I have never denied, it does not tell us whether for the baby the hidden object continues to exist when it is placed behind another object, or whether the object has momentarily ceased to exist, like an image that can disappear and reappear and then be recognized as having been seen already. The hypothesis of conservation would suppose a substantification of reality and an organization of space; recognition by itself does not indicate whether this organization is present or not, or even whether such organization is possible or not at this age. The hypothesis that the newborn baby's universe consists of tableaux which disappear and reappear is much simpler and just as compatible with very early recognition. With regard to the early "tunnel-effect" this phenomenon has to be dissociated from a simple oculocephalogyral reflex, a problem for separate discussion elsewhere.
8. J. Piaget and B. Inhelder, *L'Image mentale chez l'enfant* (Presses Universitaires de France, Paris, 1966); and J. S. Bruner, *Amer. Psychol.* **19**, 1 (1964); J. S. Bruner *et al.*, *Studies in Cognitive Growth* (Wiley, New York, 1966).
9. Th. Dobzhansky, in *Hundert Jahre Evolutionsforschung*, G. Heberer and F. Schwanitz, Eds. (Fisher, Stuttgart, 1960), p. 32.

Reply by J. Mehler and T. G. Bever

Our research has been focused on the cognitive capacities of the 2-year-old child. We reported that the 2-year-old performs better than the 4-year-old in judging the relative quantity of rows of clay balls (*1*). (The stimuli are represented in Fig. 1*b* of Piaget's discussion.) Since Piaget developed the theoretical problem as well as the general techniques used, there are many points of agreement between our initial paper and his critique, and some points that remain to be clarified (compare *2* and *3*).

With respect to the experimental issues, Piaget suggests that the young child responds to the relative density or "crowding" in the shorter row, not to its relative numerosity. We have recently used numerically equal rows

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with one row shorter and denser than the other and with length relations similar to those shown in Fig. 1*b*. We found that there was no tendency for children at any age to choose the denser row as having "more" (*4*). Therefore, the young child appears to make correct quantitative decisions without reference to the relative density of the rows.

Piaget mentions several difficulties he found in replicating our results (*5*). (i) The child tends to choose systematically the row closest to him. However, this effect could not have contributed to our results because in our experiment row orientation was systematically varied. (One-fourth of our subjects, in each age group, saw rows perpendicular to their frontal plane with the shorter line on their right, and one-fourth with the shorter line on the left. One-fourth of the subjects saw the lines parallel to their frontal plan with the shorter line closer to them while the remaining fourth saw the shorter line farther from them.)

(ii) "Preliminary experiments show that many very young children do not understand the term *more*." Our most recent experiments indicate that the young child responds as though he understands the word *more*: he correctly chooses the row with more balls when it is shorter, longer, or the same length as a reference row; conversely he does not consistently choose the longer or denser row when it has the same number of items as a reference row (*4*). Although such correct performance does not necessarily prove correct comprehension, it is a strong sign of it.

This linguistic objection led Piaget to use an experimental procedure that is different from ours; for the relational word *more* [and *same* in our subsequent work (*4*)] Piaget used the absolute terms, *a lot, a little,* and *not a lot.* Although the use of such absolute terms possibly stabilizes the child's behavior (as Piaget indicates), it is likely that, even for a child, a relational problem is very different from an absolute one. Thus, the fact that most of the children tested by Piaget indicate that one of two rows has "a lot" or "a little" cannot be compared directly with our finding that he knows (and says) which row has "more." Furthermore, to compare Piaget's results with ours, only the first response given by his subjects can be used. Throughout, Piaget interviewed his subjects on the same sort of problem several times, and all the responses were considered equivalent. However, we have found that repeated questioning from an adult may lead a young child to believe there was something wrong with his response, and thus he may change it (*4*).

Piaget notes that only the more numerous row was actually manipulated in our original experiment, and he argues that this might account for the results. (iii) Either the young child understands "more" as an additive term and simply chooses the row that he observes being supplemented or (iv) the young child chooses the row that he observes the experimenter manipulate. In a recent experiment, however, we have controlled both possibilities by prior preparation of all rows on cardboard strips. Under these conditions, in which rows are manipulated equally, neither row receiving additional pellets, young children perform extremely well (93 percent choose the correct row as having more), and older children perform markedly less well (*4*).

We agree with Piaget that the term "conservation" should not have been used in our first paper. We used a kind of "*over*conservation" technique because, like Piaget, we observed that quantitative concepts (particularly the concept of equality) are difficult for the young child to verbalize. The "overconservation" technique allowed the child to exhibit judgements of relative quantities in nonverbal responses (pointing to one row, or taking it). Furthermore, we felt that the high level of performance that the 2-year-old achieved on this task (and on later tasks) indicated a fundamental *capacity* to appreciate quantitative relations of the sort that could be studied with the true conservation paradigm. [We have discussed elsewhere the relation between our first experiments and the study of the capacity for conservation and also some true conservation studies with young children (*4*).]

With respect to theoretical issues we take our results to mean that the 2-year-old already has certain basic cognitive capacities; these capacities are subject to the child's general limitations on such expressive functions as attention and memory. To overcome these limits, the child forms (intuitive) perceptual generalizations that extend his capacities beyond the behavioral limits. Since these perceptual generalizations fail in critical cases, the child is ultimately impelled by his experience to integrate them into a system that includes both the basic logical capacities and the perceptual generalizations. This interpretation does not assume a notion of competition between "good" and "bad" strategies, as Piaget suggests. In particular, the perceptual generalizations that the child develops at the end of the third year are not "bad"; on the contrary, they are appropriate consequences of the interaction among the child's basic capacities, his subsidiary skills and experience. Therefore, our interpretation does not "cut the link between the subject and exterior reality." (However, we are not claiming that there is any obvious external reason for the formation of the perceptual generalizations at a particular stage of development, nor is there any automatic empiricist way of accounting for those attributes of stimuli that become the basis for generalizations and those that do not. That is, while the perceptual generalizations represent adaptations to experience, there is nothing in the external world sufficient to explain the particular experiences that the child recognizes, nor why he adapts to them. The organization and motive for these developments must be found in the child himself.)

Our interpretation seems close to Piaget's interpretation that "(young) children expect conservation, but . . . they have to construct new means of quantification. . . . The inadequacy of the means of quantification explains nonconservation . . . very young children *pass through* a stage of nonconservation as they reorganize relations which they cannot yet grasp in full." (Emphasis here is ours.)

Every theory of cognition must include a description of the relation between innate and learned components. Piaget postulates as a basic innate mechanism, the "reflective abstraction process" that extracts the regularities, rules, operations, and coordinations of the child's actions and transfers these to the cognitive domain. Of course, the "reflective abstraction process" entails powerful nativist assumptions, because it must have internal structure, already defined domains of application, and built-in capacity to segment actions and their coordinations. Thus, we must examine further concepts such as "reflective abstraction process" and "actions" before they will have explanatory value. On the one hand, we consider that Piaget's formulation is inadequate, not because of its nativism but rather because of the unjustified emphasis placed upon action as the *only* datum upon which the innate equipment can operate. On the other hand, we cannot conceive of actions without internal guiding principles, which are themselves, crucial components for the genesis of cognitive capacities.

We owe to Piaget our bias toward a functional innateness rather than toward a preformational one. (Indeed, we have never intended to argue exclusively for innate *structure* as opposed to innate *process* which can abstract structure from experience.) Nevertheless cognitive processes (like chemical reactions) are difficult to observe directly—rather we must examine the structure of initial, intermediate, and final functioning and then describe intervening processes to account for the development of the child's performance. Piaget has devoted a great deal of attention to the operations which the child develops by age 7 to 8 and also to the absence of those operations during the preceding years. We are now attempting to expand these investigations by examining the initial strategies and heuristics that the 2-year-old child has as he *starts* this phase of his cognitive development. Whether a particular component of cognition is to be viewed as "innate" or as the result of early learning will be a question for further theoretical and empirical investigations.

References and Notes

1. J. Mehler and T. G. Bever, *Science* **158**, 141 (1967).
2. ———, *Int. J. Psychol.*, in press.
3. T. Bever, J. Mehler, V. Valian, in *Structure and Psychology of Language*, T. Bever and W. Weksel, Eds. (Holt, Rinehart and Winston, New York, in press), vol. 2.
4. T. G. Bever, J. Mehler, J. Epstein, *Science* **162**, 921 (1968).
5. The numbers (i), (ii), (iii), and (iv) follow the preceding article by Piaget.

6. Supported by Foundation Funds for Research in Psychiatry, grant G67-380, and by ARPA contract SD-187.

NEWS AND COMMENT

Philip Handler: National Academy Nominates a Worldly "High Priest"

I must remind you that a religion with naught but a priesthood, no matter how enthusiastic, devoted or dedicated, but without a laity cannot long survive.—Philip Handler, in a speech delivered earlier this year.

Philip Handler has many admirers in the scientific community and few critics. Some of those who criticize him do so because they think he is "too much of the world, and not enough of the priesthood."

Handler would be the first to agree that scientists must get out in the world to inform the "laity" and battle for the needs of science, rather than be content merely to work within the "priesthood." His skill in advocacy was one of the attributes which led the official committee of the National Academy of Sciences to nominate Handler for the Academy presidency to replace Frederick Seitz, newly designated president of Rockefeller University. (Although this nomination carries Handler a long way toward election, any 50 of the Academy's 800 members can nominate another presidential candidate. Ballots will be sent to members on 15 December, and the results will be announced 15 January. The new president will take office on 1 July.)

Handler, now 51, has taught biochemistry at Duke University for almost 30 years and, according to men in his field, has done research of note. However, as Handler said in an interview with *Science* in discussing his nomination for the Academy presidency, "I hope I've been a competent biochemist, but that's not why I've been chosen for this job."

This gregarious biochemist, who was elected to the National Academy only 4 years ago, has made his name as an organizer, administrator, adviser, and spokesman for science, especially in Washington. He began moving up in the organizational world of science when he became secretary of the American Society of Biological Chemists in 1953; he was later elected president. He has held various other positions in scientific organizations, including membership on the governing Council of the National Academy—to which he was elected earlier this year.

But it is in advising the federal government that Handler has left his largest mark, espeically in the past half dozen years. During this period, Handler estimates, he has spent from a quarter to a third of his time in Washington. From 1964 to 1967 he served as a member of the President's Science Advisory Committee (PSAC), where he is said to have urged greater attention to civilian science and technology and to the problem of adequate federal funding for the universities. PSAC's chairman, Donald F. Hornig, comments, "PSAC is made up of strong people, prima donnas in fact, and Handler held his own. He was one of the strong contributors."

Handler's most intensive governmental involvement has been with the National Science Foundation (NSF) and with its policy-making body, the National Science Board. He was appointed a member of that group in 1962; the Board elected him chairman in 1966 and reelected him chairman this year. In this role, he has helped develop the NSF reorganization bill which was enacted into law this year. Handler is said to be a talented, if sometimes loquacious, chairman by his fellow Board members.

Some scientists familiar with the NSF raise the question of whether Handler will resign from the National Science Board if elected to the presidency of the National Academy. These scientists argue that it would be more difficult for the Academy to offer independent advice to the government if its president served in a major federal post, and they also point out that the Academy receives federal funds, including some from the NSF. In an interview, Handler said he did not feel it would be necessary for him to resign from the Board if he is elected to the Academy presidency. As for his remaining as chairman, he would leave that decision to his fellow Board members. If he should become president of the Academy, Handler said, he would never let himself be reelected chairman of the Na-