would go to the opposite extreme from that performed by Ranken; the names become cues, whereas his are masks. The real conclusion to be drawn from Ranken's work is that names can be either a help or a hindrance in concept formation. Names categorize. If the categories implicit in the names have validity for the problem presented, the names will aid in the solution of the problem; if the categories are inappropriate, the names will be a hindrance; if the names are "neutral" (and they rarely are), they will not affect the solution of the problem.

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Baum's main point, that "names can be either a help or a hindrance," seems to be in complete accord with my principal conclusion, that "the effect of prior name learning depends on the nature of the problem" and that names "may facilitate performance in one problem but interfere with performance in another." The next step is to identify the variables which determine whether they help or hinder. The jigsaw and memory tasks were used because they exemplify certain factors believed to be relevant. The results from the jigsaw problem indicate that when the names do not explicitly encode the stimulus properties upon which problem solution depends, names hinder problem solving, other factors being equal.

Baum's suggestion, that giving each shape a different name made it less likely that subjects would notice similarities between top and bottom contours (the only kind of similarity that would be of direct relevance to the jigsaw problem), finds some support in the subjects' responses to the post-experimental question whether they noticed during training "that some of the shapes could be fitted together." Four of the eight subjects in the Unnamed-Jigsaw group reported noticing a total of 17 such pairings, a mean of 2.1 pairings out of a possible 16. Two subjects in the Named-Jigsaw group noticed a total of 6 pairings, a mean of 0.75. This difference does not seem to explain the superior jigsaw performance of the Unnamed group, however. In the first place, the difference between Named and Unnamed conditions in mean error scores (2.3) is greater than the difference in reported pairings (1.35). In the second place, the Named-Unnamed difference in error scores is found both among subjects who reported noticing one or more pairings (3.5 versus 2.8) and among those who reported noticing no pairings at all (5.3 versus 2.5). Noticing the pairings beforehand appears to facilitate jigsaw performance only in the Named condition, where it might be expected to play a more important role if in fact subjects in this condition have less figural information available at the time they solve the problems.

Baum points out that if the names do explicitly encode the stimulus properties which are relevant to the jigsaw problem, they might be expected to facilitate problem solving. I have investigated this variable, using numbers as the "first and last names," and using four-sided shapes in which the names encode only two of the sides (the "selected" contours). The names are learned in a classification task in which the other two sides (the "unselected" contours) are irrelevant. On jigsaw problems involving the selected contours, names facilitate problem solving, as Baum predicts, but only when subjects are explicitly instructed to use the names. In the absence of such instructions, even though the code has previously been explained, subjects with names make as many errors as subjects without names who have had no selection training, and make more errors than subjects who have had comparable selection training by a nonverbal procedure. It is definitely not the case that the subject "fits the names together as simply as he would fit the actual shapes together." Even with instructions to use the names, on problems involving selected, named contours, subjects make two and a half times as many errors (with time held constant) when the problems are given at the symbolic level as when they are given at the concrete level.

On problems involving unselected contours, more errors are made than with selected contours, and subjects with names make more errors than those without names (confirming the previously reported finding). As to the relative importance of facilitating and interfering effects of names in the real world, that will depend, as Baum points out, on the relation between the information encoded in the names and the information required for solving the problem. It will also depend on other factors, such as the extent to which problem solution involves short-term memory. With respect to the information factor, it might be noted that ordinary language coding is typically selective, and that the selected stimulus attributes and relations are necessarily those which are already known to the verbal community. In creative problem solving, which presumably involves responding to attributes and relations which have *not* previously been noticed, the effect of names on availability of information may be primarily negative.

Baum raises the question, which I discussed briefly in my report, whether subjects in the Unnamed conditions may not also have verbalized the shapes. In response to post-experimental questioning, subjects in these conditions reported some degree of verbalization for a mean of 4.4 shapes out of 8. In many of these instances, they reported that they thought of a word occasionally in the early training trials, but that it soon "dropped out." The difference in mean errors in favor of the jigsaw problem over the memory task was greater for subjects below the median, in amount of reported verbalization (1.8 versus 5.2), than for those above the median (3.5 versus 4.0), replicating within the Unnamed condition the interaction between naming and type of problem found in the Named-Unnamed comparison. It appears that the procedures used were effective in producing substantial differences in the extent to which the shapes were verbalized, and that this variable does in fact interact with the type of problem. A more decisive answer to the question of the role of spontaneous verbalization lies in the use of subjects in whom such verbalizations are minimal—young children, mental retardates, and subhuman primates.

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On Traditional and Modern Biology

Eugene Kaellis's philippic against "atomistic prejudice" in biology and his plea for the establishment of a Society of Holistic Biology [*Science* **140**, 1362 (28 June 1963)] reiterates a problem which, freed from its emotional content, remains serious enough to warrant discussion.

The entry in force of the physical sciences and of physical scientists into biology in these decades is a historical fact. Owing to the particular perspectives of the physical sciences, a different character is superimposed upon traditional biology which is apt to change this field from a predominantly observational and descriptive discipline into an analytical and causal science.

The concept of mechanistic and thus deterministic biology in the Western World arose perhaps with the posthumous work of Descartes, *De homine*, and a dispute between mechanistic and vitalistic biology still continues. The vitalistic school argues that the totality of life is a quality *sua generis* which cannot be described or reassembled as a summation of its component processes because it is imbued with a formative or directive force (*vis vitalis*, entelechy) which is metaphysical in nature and thus evades scientific comprehension. Traditional biology, whether conscious of it or not, has frequently assumed this vitalistic position of intellectual resignation.

Mechanistic biology (and currently foremost, molecular biology) takes the position that the phenomenon of life represents an unresolved form of chemistry and physics and, therefore, barring subatomic indeterminacy, is rigidly determined at the molecular genetic level. The overwhelming complexity, for example, of ecological situations is considered the result of a large number of uncontrolled variables, but not a valid argument against the principle of determinism.

Thus the disputation between "traditional biology" and "atomistic prejudice," to use Kaellis's terminology, can be regarded as the contemporary form of the dispute between vitalistic and mechanistic biology.

That molecular biology and its related disciplines enjoy rapid growth and a measure of success is, in part, due to the fact that modern biology can draw upon a body of knowledge, experimental methodology, and qualified manpower which are all derived from the high state of development of the physical sciences. In a deeper sense, however, the ascendency of molecular biology is the result of applying an intellectual approach to which traditional biology is not nearly so amenable. What is meant is that the field uses the scientific method of formulation of hypotheses, critical experimentation to directly reject or verify such hypotheses, generalization on the next higher level, and repetition of the process at that level. Traditional biology has not developed great generalized theories striving for the ultimate recognition of the laws of nature with the notable exception of the theory of evolution. Certainly, the various taxonomical systems do not belong in this category of generalizations.

While it may be traumatic to feel that one's proprietory relationship to a traditional field of learning is being invaded, such experiences are not without precedents in the history of science or in the biographies of scientists. The advents of heliocentric astronomy, the oxygen theory of oxidation, the germ theory of disease, the theory of evolution, or the theory of relativity, just to name a few, have not been uncontroversial. There may even have been pleas to establish the Society for Geocentric Astronomy or the Phlogiston Club in order to oppose heliocentric or oxygenistic "prejudices." Science, however, does not advance in this manner but is eminently self-correcting of bias irrespective of where it resides.

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Ethical Code for Scientists?

In recent months a bibliography (*1*) has been assembled entitled "Some ethical problems of science and technology." It covers the period from January 1955 to July 1963 and includes about 300 references in English in a compilation characterized as "not exhaustive."

This bibliography supplies ample evidence of the interest of scientists, engineers, and the public in the ethical aspects of the relationships of scientists and engineers to society and to one another. There emerges, however, one item of substantial difference between the approaches of the engineers and the scientists to their ethical problems, which deserves attention. Engineers have shown a definite interest in organized action (*2*) to improve ethical practice—for example, by emphasis on ethical considerations in the training of engineers, and by the adoption of formal codes of ethics by the various professional societies of engineering. Except among the psychologists, who have adopted a set of "Ethical Standards for Psychologists" (*3*), there is no evidence of similar action by scientists, who seem to have confined their efforts in the area of ethics to discussion.

It is true that it has been proposed at least twice (*4*) that scientists as a group should adopt a code of ethical practice. In their proposal, Pigman and Carmichael, in 1950, discussed the scope of such a code in some detail, but thus far there has been no indication that these, or any similar proposals, are being acted upon.

In taking formal action in the area of ethics, engineers are in accord with traditions long established in other professions (*5*) and with a strong trend in many other occupational groups toward ethical self-regulation. It is tempting, therefore, to speculate on the reasons for the divergence of scientists from what has become substantially the norm of social conduct.

One relevant factor, clearly, is the traditional remoteness of scientists from the temptations of the market place and from stresses generated by competition for professional advantage, for power, and for influence. But even in 1950 Pigman and Carmichael were observing that this remoteness was a thing of the past. Today, such a worldly problem as conflict of interest (*6*) is far from a trivial concern for many scientists, and one can readily argue that, in his role as government adviser, government contractor, government official dispensing large sums of public money, grant recipient, entrepreneur, consultant, supervisor, or employee, the scientist is at least as much enmeshed in ethical problems as the engineer.

Perhaps scientists have merely been somewhat slow to adapt to the great changes which have taken place so rapidly in the scientific professions in recent years. Perhaps the reasons for the difference in approach lie deeper. In any case, the record of need and of precedent suggest that a course of positive action in the area of ethics is something which merits the thoughtful attention of the scientist and the scientist-educator, and of their professional organizations.

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