Letters

Protein Behavior

In a recent article [*Science* **128**, 815 (1958)], I. M. Klotz admirably points up the fact that electrostatic interactions involving proteins and small ions have generally had to be treated in an oversimplified way, and he calls attention to the many observations which would suggest that the properties of the water in the immediate vicinity of the protein molecule may be rather different from those of the water in the bulk solvent. His proposal regarding the effect of an ice structure would explain the greater difficulty of protonation of an $-N(CH_3)_2$ group but, it appears, would still not solve the problem of reduced reactivity of the —SH group with Ag^+, with which he initiated the discussion. In this case, of course, there would be no change in the charge to modify the structure of the ice lattice.

The proposal seems even more troublesome when one considers the —COOH groups in the protein. In this case a charge appears on the group when the hydrogen ion is lost. Consequently, the loss of the proton would be hindered, and the apparent *pK* should be increased as compared with the expected intrinsic *pK*. There seems to be little evidence for any considerable number of —COOH groups with unexpectedly high *pK*'s in proteins.

The lack of any evidence for electrostatic effects on the titration curve of the azomercurial would still be disturbing, however—unless one assumes that the reaction with hydrogen ion is leading to displacement of the azomercurial. If this were true, there would be no change in the charge, and one would not anticipate an electrostatic effect. Such a displacement of methylmercury would have been anticipated on the basis of the results of Hughes [*Cold Spring Harbor Symposia Quant. Biol.* **14**, 79 (1949)].

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Each of the three paragraphs of Mehl's letter raises essentially one question. The responses may be grouped, therefore, into the same arrangement.

1) "Maskedness" in the behavior of —SH groups, as Mehl would undoubtedly agree, is a problem of rates rather than equilibria. For example, Ag^+ is usually taken up even by masked —SH groups of proteins if we wait long enough. In terms of my model, the explanation (clearly implied if not explicitly stated in the article) is that Ag^+ would diffuse through "ice" much more slowly than through liquid water. While no actual data are available for diffusion of Ag^+ in ice, measurements with a similar monovalent ion, Li^+ [M. Eigen and L. DeMaeyer, *Proc. Roy. Soc. (London)* **247A**, 505 (1958)], certainly bear this point out.

2) The most careful theoretical analyses of the titration curves of a protein with, for example, 100 carboxyl groups have limited themselves to a single intrinsic dissociation constant. The spread of the titration curves has been accounted for by the assumption that there is a variable electrostatic factor, plus necessary additional assumptions in specific cases. Clearly, if deviations from ideality are attributed to these additional factors, and if we permit the possibility of only a single intrinsic constant, we cannot possibly find more than one intrinsic constant. As I have emphasized, however, the titration curves can also be accounted for by the assumption that there is a broad spectrum of *pK*'s for the carboxyl groups.

3) The explanation suggested by Mehl is not really tenable. We have not studied the titration curve by some general method which follows gross H^+ ion uptake but rather by a special spectrophotometric method which reveals specifically the uptake of H^+ by the $(CH_3)_2N$— group of the dye, not by the mercaptide group of the protein. It is the unusual *pK* and the unexpected shape of the curve for the optical titration of this particular $(CH_3)_2N$— group which must be interpreted; a displacement mechanism postulates changes in state of a different group at the opposite end of the molecule. Furthermore, one should also recall that if the dye had been displaced from the mercaptide linkage, no optical titration could have been obtained in the first place, for as we have mentioned previously [*Arch. Biochem. Biophys.* **63**, 77 (1956)], the dye is essentially insoluble in water alone.

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On Eschewing Teleology

A. J. Bernatowicz' stern admonitions [*Science* **128**, 1402 (1958)] to his biological colleagues and to all other scientists to eschew anthropomorphism and avoid even the appearance of teleological thinking (lest their students be not saved from corruption) confuses me. He wants mechanism, and thus also surely determinism, recognized as the language of science, and he wants the teacher of science not to depart from it. Does he mean *never*? Not with the student at the luncheon table? Not with his wife at the breakfast table? How rigorously must righteousness be applied? And must all scientists observe the canon? I think

Bernatowicz does believe just this, and I suspect that he also thinks that the scientific view of the world is the true view and that he who departs from proper deterministic language is dealing lightly with truth.

Well, the language of science is descriptive and not purposive. I am wholly in accord with Bernatowicz on that point. But, if we are talking science, I go farther where he stops short, for I note—remaining descriptive because I am being scientific—that we have here a case of one organism attacking others because these others behave differently from the one. Scientifically, all we have is a case description of an idiosyncratic organism, Bernatowicz, which does not, on the showing of his paper, represent the species but a departure from it. Is this organism quieted by this description of his behavior, or do his words suddenly seem to carry an anthropomorphic meaning? Do they say that his purpose was to convince others of his own rightness, thus ruling him out of the class of scientific organisms because he speaks in terms of purpose? If Bernatowicz wants rigor and the scientific attitude all the time, let him, a biologist, include his own conduct under the scientific rules. Was he or was he not being purposive? I think he was.

For myself, I take the use of the scientific language more lightly, less rigorously, and I am glad that most of my colleagues do too. J. J. Thomson said that a scientific activity is a policy, and he might have made his comment broader to say that all scientific activity is a policy to be accepted when it works and changed when it does not. I do not pretend to be a scientist in all my speech, nor do I want students to be. I want them to have values and purposes, to know when to escape the scientific strait jacket and also when to get a new strait jacket. There should be nothing so inflexible about science or scientific conduct or scientific language. What we want for scientists is wisdom, good judgment, the ability to speak and write English delicately and significantly, enough wisdom to break any rule when a good purpose will be supported thereby.

Urbanity never hurt a scientist yet. Using English with skill, relying on the connotations to work as they should, is less secure than the rigor of mathematics but an art capable of much greater refinement. I find in none of Bernatowicz' examples the sort of thing that should be changed if it is given in a proper scientific context. A strict adherence to his rule would eliminate entirely from scientific speech the phrase *in order that,* and that seems to me to be a preposterous suggestion. The Darwinian theory was a way of changing purposive events into descriptive, and for me (and I should hope for most students) the context of most of these disapproved sentences would be the nonpurposive, descriptive concept of natural selection.

In any event—say I, anthropomorphically purposive, and purposively anthropomorphic—let us not warp our young scientists by rigid rules. Always remembering that we do not wish for all to be alike, let us cultivate urbanity, wisdom, and flexibility. Let us help them to learn to speak and write well, clearly, freely, attractively, and differently from one another. Let us hope they can learn to think of science as a happy way of life, not as a harsh taskmaster with a code of morals dangling at his belt.

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In the interests of clear thinking one should certainly be wary of any statements that are unjustifiably teleological, as A. J. Bernatowicz has pointed out. Judging from his conclusions, however, I suspect that he would prefer to leave out the word *unjustifiably* in my first sentence, as would many other scientists, although not all. [See L. K. Frank, G. E. Hutchinson, W. K. Livingston, W. S. McCulloch, N. Weiner, *Ann. N.Y. Acad. Sci.* **50**, 187 (1948)].

Let me make plain at the beginning that I would agree that in many circumstances teleological language is inappropriate, implying the existence of agencies beyond our ability to observe directly. The examples quoted by Bernatowicz, I would judge, all demonstrate misapplication of teleological concepts. However, not all of them are inherently unsuited to such treatment.

What do we mean when we say that a statement is "teleological"? Do we not mean that it implies some directing or governing factor which by its nature "causes" events to follow certain observed courses? Certainly in this sense the idea of "force" or "gravitation" or "field" is, or can easily be seen as, teleological, invoking the presence of an agency which we can observe only by inference. But if we are careful to note that, for example, a "field" is a map of what we might expect to observe, then we can still use the concept of "field" at one level of explanation, without implying the existence of any incompletely defined agency.

There is a more important area of teleological thinking, however, in which the above "way out" is of no use. This is the area of biology, physiology, psychology, and other sciences of life. Let me emphasize that when one approaches these disciplines in an attempt to imitate the (in principle) rigorous methods of physics, then any use of teleological thinking is bound to go astray. But if one approaches living entities from the sys-

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tems standpoint, particularly employing even the simpler concepts of feed-back theory, he finds himself tending toward teleological expressions, because they are far more accurate and conservative of words than "mere" statement of relationship among "fundamental" variables. Feed-back systems are best discussed with transitive verbs.

In any case, where a closed chain of cause-effect relationships is known to exist, feed-back exists, by definition, and in living systems this feed-back is often both significant and of the correct sign to qualify as negative feed-back. All negative feed-back systems can properly be thought of as control systems, and all control systems operate to maintain their input signals at some "reference level." The reference level might be set where it is as a result of physical properties of the control-system components, or it might be determined by a signal entering the control system from outside it. This reference level is in all respects a *goal* for the system. The system will (within its limits of complexity) produce whatever outputs are required by the momentary environment to bring its input to this reference level.

In other words, one can say with clear meaning that the *purpose* of the system's behavior is to make its inputs approach some goal state. Many modern psychologists have unfortunately rejected this language for discussing behavior because it is teleological or "anthropomorphic," but one is here properly accused of speaking anthropomorphically only when he tries to describe the goal of a control system in terms appropriate to himself rather than to the control system. To make this clear, let us look at one of Bernatowicz' examples.

"There has to be some sort of mechanism for raising sap [in trees], and energy is lavishly expended in the process." The word *lavishly* aside, this is clearly a hypothesis that a control system exists. But the "goal" of the system has been expressed in the observer's, not the system's, terms: "raising sap" is one event that a human observer might notice, to be sure, but the plant can hardly be suspected of being directly sensitive to the height of the sap column. Rather, we might guess that the presence or absence of sap in critical portions of the tree affects biochemical processes, and some of the products of these processes may well be controlled variables for which specific "reference levels" are determined by the plant's physical properties.

The control system which maintains this kind of variable at a particular level does so by controlling the transfer of energy from storage into work or chemical synthesis; thus, it is quite appropriate to use the transitive verb *expend*.

Teleology and anthropomorphism were

properly rejected when scientists realized that they were projecting their own goals into the systems they were studying. Indeed, lacking the basic concept of feedback system, they could only study scientifically those systems in which "straight-through" space-time relationships were predominant. But when at last scientific thought began to develop in studies of living systems, the lesson had been learned too well, and what proved a crucial advance in the development of physics and chemistry may now turn out to have been in some ways a backward step in the life sciences. The rejection of teleology ultimately enabled students of behavior to develop reliable means for accumulating data, but it may have been that very step which so far has prevented the organization of those mountains of data into comprehensive theories, not only in psychology but in biology, biochemistry, medicine, sociology, and so forth.

If I have succeeded in communicating anything, I hope it is this: teleological concepts can be misused, but the ideas of purpose, goal, or directivity that they denote are both appropriate and proper when one speaks of control systems; I believe at the moment, along with many others, that feed-back control is a pervasive and fundamental feature of living systems. Perhaps it is time that at least the students of living creatures reconsider their goal of rejecting teleological concepts *in toto*. Perhaps in this way we might at last arrive at an "anthropomorphic" theory of human behavior.

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A. J. Bernatowicz says (page 1404), "To the beginner, the idea of natural law presupposes a lawgiver. . . ." Why only to a beginner? Beginner in what? I would say that to any clear thinker the idea of natural law presupposes a lawgiver.

And I fully intend the implication that anyone who succeeds in not drawing this conclusion is not a clear thinker.

ANTHONY STANDEN

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Bernatowicz' rogues' gallery of teleologists may be augmented by one far more important than any he quotes: "What can be more curious than that the hand of man, formed for grasping, that of a mole for digging, the leg of a horse, the paddle of the porpoise, and the wing of the bat, should all be constructed on the same pattern . . ." (*1*).

Not all of Bernatowicz' teleologists, animists, and anthropomorphists are villains. Some are ignorant and some merely careless. The author of my quo-

tation cannot be judged ignorant, even now that we have a century's advantage of him. He polished with a lapidary's care and can hardly have let this and other similar passages stand through six editions from simple oversight. As for being a villain, he is the very archhero of antiteleology. Can it be that Bernatowicz has overlooked one of "the usual excuses," or that he has in certain instances mistaken something else for teleology?

Among the things most characteristic of organisms—most distinctive of living as opposed to inorganic systems—is a sort of *directedness*. Their structures and activities have an *adaptedness*, an evident and vital *usefulness* to the organism. With sterile rigor one may say that "the hand of man grasps," that "sap rises in trees," or that "sea turtles go ashore and lay their eggs." How coincidental it is that such activities just happen to serve the necessities of the organism and of its species! How downright providential! How, in short, inescapably teleological! The "rigorous" statement lands us again squarely in the pre-Darwinian dilemma.

Darwin's answer and ours is to accept the common-sense view that "the hand of man [is] formed for grasping" (*1*), that "there has to be some sort of mechanism for raising sap" (*2*), and that "turtles go ashore to lay their eggs" (*2*) to the end (*teleos*) that the individual and the species may survive. But this end is (usually) unconscious and impersonal. Naive teleology is controverted not by ignoring the obvious existence of such ends but by providing a naturalistic, materialistic explanation of the adaptive characteristics serving them. Surely that is clear enough in the large, omitted context (for example, in *2*) of some of Bernatowicz' brief excerpts. It has recently been quite fully and, I think, lucidly discussed by one of the authors condemned by Bernatowicz (*3*). Because *teleology* has indeed become a dirty word, Pittendrigh has suggested that this undeniable end-directedness of evolution and of organisms be called "teleonomy."

With Bernatowicz, "we stand against the evil" of man- or god-oriented teleology, of animistic personification, and of illegitimate anthropomorphism. We stand equally against the "evil" of vitalism. (By the way, is such emotional language "rigorous"?) But when teleonomy, *sensu* Pittendrigh, is mistaken for teleology, "rigor" becomes evasion. It is stark reductionism that denies to life its most essential characteristics. It is a blind alley leading only to a biology without *bios*.

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References

1. C. Darwin, *On the Origin of Species* (Murray, London, 1859).
2. G. G. Simpson, C. S. Pittendrigh, L. H. Tiffany, *Life: An Introduction to Biology* (Harcourt, Brace, New York, 1957).
3. C. S. Pittendrigh, in A. Roe and G. G. Simpson, Eds., *Behavior and Evolution* (Yale Univ. Press, New Haven, Conn., 1958), chap. 18.

From a microcosmic listening post the following ditty was picked up and recorded. There is uncertainty as to the author.

The Man and the Electron

Said A. J. B. to the electron,
"It's getting damned hot under here!"
"Jump my boy," said the electron,
"Your pants are beginning to sear."

Said A. J. B. to the electron,
"I didn't mean to run into the man!"
"Take a hint from me," said the electron,
"Keep out of crowds whenever you can."

Said A. J. B. to the electron,
"Oh boy, look at the fem over there!"
"The attraction is mutual," said the electron,
"Run meet her, then please disappear!"

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E. G. Boring asks whether I want the teacher of science *never* to depart from the language of science. Now, *never* is an absolute, and it is difficult indeed to subscribe to an absolute. I would prefer to say that, so long as he purports to communicate the scientists' way of looking at the universe, the teacher of science would do well to avoid expressions inconsonant with that aim. Personally, I find it difficult to change as I move from classroom to luncheon table, and it seems dangerous to assume that the reverse change, between lunch and class, would be easier. It does not follow that avoiding teleology betrays a belief "that the scientific view of the world is the true view" (whatever "true view" may mean). Teachers of other disciplines present *their* views of the world and the "truth"; each contributes to a liberal education, and I would not dilute the contribution of the science teacher by conceding that his approach need not be thoroughly disciplined. This does not imply the superiority of one intellectual discipline over another.

We are agreed that language should be used with wisdom, good judgment, delicacy, and urbanity, but I do not see that precision and rigor are incompatible with these requirements. Unlike Boring, I am not confident that "scientific context" will develop proper attitudes irrespective of the choice of words. The modes of thought instilled in the student are, I maintain, due to the words. Schrödinger [*What Is Life? and Other Scientific Essays* (Doubleday, Garden City, N.Y.,

1956), p. 146] puts its very well: "In the inseparable union of speech and thought the primacy, rather paradoxically, rests with speech. When we hear the same words again and again pronounced with authority, we are apt to forget that they were originally meant as an abbreviation; we are induced to believe that they describe a reality."

Between Powers and myself there seems to be no disagreement—we will not teach teleological thinking in those areas where we insist that teleology is to be avoided. If teleological thinking becomes fruitful in certain areas, we hope that teaching in those areas will become appropriately oriented. I suspect, however, that such teleology as he describes will need to be presented at a considerably more sophisticated level than the examples I cited.

As for natural laws and the presupposition of a lawgiver, I can do no more than expose my thinking. Briefly, and therefore with oversimplification, I consider a natural law as a generalization of our observations. As such, each presupposes a "law-stater," hence Hooke's law, Boyle's law, and so on. The idea of a lawgiver has always seemed to me to derive from a mistaken analogy with juridical law. If, *from the viewpoint of science,* it is meaningful to presuppose a lawgiver of natural laws, I shall be grateful to hear the argument.

There seems to be no essential disagreement between my article and Simpson's letter. Simpson feels that "teleonomy" is a valid orientation to biology and would defend such expressions; I offered no argument against any philosophy, be it teleology or teleonomy, that one may *deliberately* include in his teaching. But even if one's convictions in favor of teleonomy lead to language such as I quoted, which way will the student bend—toward vitalism, naive teleology, and animism or toward teleonomy? Simpson thinks that the context from which some of my excerpts came would serve to controvert naive teleology and to inculcate teleonomy. Perhaps, but the naive, even primitive, attitudes I discover in students do not leave me optimistic.

Said Stout to the interphase nucleus,
"I'm tired, I've been going all day."
And the nucleus wearily answered,
"Me too; I'm for hitting the hay!"

Perry Stout's discovery of poetry (?) showing anthropomorphic wording at its ludicrous extreme is a device not unlike the idea I received from Julius Roth of the University of Chicago. Roth suggests cartoons, with quotations from texts as the captions. The possibilities are endless—at least until they reach an editor!

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