Federation in Moderation

A Department of Science and Technology that would develop support for neglected areas of research but that would not disturb areas of research now being successfully pursued has been urged publicly on several occasions by Lloyd V. Berkner, president of Associated Universities, Inc. The scientific community, in general, has opposed a cabinet-level administration for science, in part because of the fear that the attempt to straighten out organization lines might seriously disrupt current scientific activities. By seeking ways of gaining leadership in scientific areas where other nations now surpass us, rather than stressing organizational elegance, Berkner attempts to meet this objection.

One aim of the proposed department—and it seems to us that in the long run this aim would prove to be the most important one—is to bring into focus government responsibility for developing certain new scientific areas of importance to the nation. The new areas would be those that employ expensive equipment, that require a coordinated effort by many people, and that do not fit readily into existing agencies. The 10-year program in oceanography recently proposed by a committee of the National Academy of Sciences—National Research Council might be developed by a division of the new department. Administrative arrangements for the new activities would be made, at least in part, through national laboratories—that is, laboratories supported but not operated by government agencies. Brookhaven National Laboratory, which is supported by the Atomic Energy Commission but operated by Associated Universities, Inc., might be taken as a model.

A second aim of the proposed move is to bring under central direction existing agencies that because of present organizational difficulties are not discharging their responsibilities with desirable vigor. Lack of full development of a field of research might occur because a bureau has no special relation to the task of the department in which it is located and thus finds it difficult to attract supporting funds. Development of a field of research might also be hampered because related scientific activities are divided among several departments. According to Berkner, the existing agencies that might with profit be brought into the proposed department include the Coast and Geodetic Survey, the Hydrographic Office, the Geological Survey, the Weather Bureau, and the National Bureau of Standards.

Although some present scientific agencies in the Government would be transferred to the proposed department, other agencies would not be disturbed. A research and development program that is closely related to the task of the department in which it is located would not be transferred. The Department of Defense, for example, would keep its Office of Defense Research and Engineering. And independent agencies that are of good size and of specialized function would not be transferred since they might deflect attention from the new activities that the proposed department is designed to develop. The National Aeronautics and Space Administration, for example, would retain its independence.

Although there may still be room for disagreement about the inclusion in the proposed department of this or that scientific activity, or even about creating a new department, many observers probably will laud the effort to devise a plan that by and large attempts to supplement rather than supplant present administrative arrangements. And observers who have opposed previous plans for a Department of Science and Technology probably will agree that new proposals should be considered on their own merits.—J.T.
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<table>
<thead>
<tr>
<th>Syringe Capacity</th>
<th>No. 1</th>
<th>No. 2</th>
<th>No. 3</th>
<th>No. 4</th>
<th>No. 5</th>
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</thead>
<tbody>
<tr>
<td>5</td>
<td>.0046</td>
<td>.023</td>
<td>.116</td>
<td>.58</td>
<td>2.9</td>
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<tr>
<td>10</td>
<td>.0059</td>
<td>.030</td>
<td>.148</td>
<td>.78</td>
<td>3.7</td>
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<tr>
<td>20</td>
<td>.0118</td>
<td>.059</td>
<td>.29</td>
<td>1.48</td>
<td>7.4</td>
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<tr>
<td>30</td>
<td>.0157</td>
<td>.078</td>
<td>.39</td>
<td>1.96</td>
<td>9.8</td>
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<td>50</td>
<td>.024</td>
<td>.12</td>
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20 July. Structure and cellular aspects of membranes: George E. Palade; Heinz Holter; Manfred L. Karnovsky.

21 July. Molecular architecture of the membranes: F. O. Schmitt; J. B. Finean; W. Stoeckenius; J. Lawrence Oncley; J. D. Robertson.


23 July. Physics and chemistry of membranes: George Scatchard; Torsten Teorell; R. Schlögl; Theodore Shedlovsky.

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Bo THORELL, Cell Studies with Microspectrography—(pp. 95-119)
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E. PETER GERDUSCHEK and ALFRED HOLZER, Application of Light Scattering to Biological Systems: Deoxyribonucleic Acid and the Muscle Protein—(pp. 411-551)
PAUL HOWARD-FLANDERS, Physical and Chemical Mechanisms in the Injury of Cells by Ionizing Radiations—(pp. 553-603)

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Forthcoming Events

April
2–4. Marine Biology, 20th annual, Corvallis, Ore. (J. E. McCaulley, Dept. of Zoology, Oregon State College, Corvallis.)
6–7. Chemical and Petroleum Instrumentation, 2nd natl. symp., St. Louis, Mo. (H. S. Kindler, Director of Technical and Educational Services, ISA, 515 Sixth Ave., Pittsburgh 22, Pa.)
6–9. American Acad. of General Practice, San Francisco, Calif. (M. F. Cahal, Volker Blvd. at Brookside, Kansas City 16, Mo.)
8. Evolution of Cell Populations, conf. (by invitation), Atlantic City, N.J. (D. C. Hetherington, Dept. of Anatomy, Duke Univ. School of Medicine, Durham, N.C.)
8–9. Tissue Culture Assoc., 10th annual, Atlantic City, N.J. (D. C. Hetherington, Dept. of Anatomy, Duke Univ. School of Medicine, Durham, N.C.)
10–16. Mental Health, 2nd Caribbean conf., St. Thomas, Virgin Islands. (Mrs. E. L. M. Shulterbrandt, Bureau of Mental Health, St. Thomas, V.1.)
13–17. American Soc. for Artificial Internal Organs, Atlantic City, N.J. (C. K. Kirby, ASAIO, 110 Maloney Bldg., University Hospital, 5600 Spruce St., Philadelphia 4, Pa.)
12–15. Neurosurgery, 8th Latin American congr., Santiago, Chile. (A. Asenjo G., Casilla 70-D, Santiago, Chile.)
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Talvitie, N. A. and Hyslop, Frances

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Salaman, C., Amler, R., Hyman, R. M., Chabien, R., and Europe, D. I.
The Journal of Urology, 80(3):377-381, Mar., 1958

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Campbell, J. R., Bassett, C. A. L., Hudy, J., and Naback, C. R.

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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

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(Continued from page 610)

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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.

WILLIAM T. POWERS
Veterans Administration Hospital, Chicago, Illinois

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 into 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 STANDELN
Interscience Publishers, New York

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-
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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!"

Perry R. Stout  
Kearney Foundation of Soil Science,  
University of California, Berkeley

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., 1944)]

References
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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 led to banes 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 warmly answered, “Me too; I’m fighting the hay!”

Perry Stout’s discovery of poetry (?) showing anthropomorphics 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!

ALBERT J. BENATOWICZ
Department of Botany,
University of Hawaii, Honolulu
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Letters

Vertebrate Metamorphosis

Biological teaching in its manifold aspects emphasizes cyclical development in nature. In his article George Wald gives interesting details, supported by chemical evidence, of two opposed metamorphoses in fishes, one bringing a fish to maturity, the second one returning it to its natual environment [Science 128, 1481 (1958)]. He admits that in land vertebrates physiological changes of the second type do not take place but contends that the entry of a single representative cell, the spermatozoon, into the womb is an analogous event, leading to the completion of the life cycle of such animals in water.

This appears to be open to question. What I find far more objectionable, however, is his use of a Biblical quotation, which is cut short, obviously to lend support to his thesis. To Nicodemus' question as to how a man could be reborn, "can he enter the second time into his mother's womb, and be born? Jesus answered, Verily, verily, I say unto thee, Except a man be born of water ..." (this is where Wald leaves off). However, the sentence continues: "... and of the Spirit, he cannot enter into the kingdom of God." Nobody who knows anything at all about Christian teaching would believe that Jesus is talking about physical rebirth. He is solely concerned with spiritual conversion.

To prevent misrepresentation of this kind in the future, I believe it would be wise for the editors of Science to check carefully on the accuracy of references from fields other than their own.

PAUL H. KOPPER

Biology Department,
Washburn University, Topeka, Kansas

Quantitative Gram Reaction

The semiquantitative evaluation of the Gram reaction reported by T. Mittwer (1) is based on applying small amounts of stained and iodinated suspensions of bacterial cells as spots to filter paper, as in paper chromatographic techniques. The resulting streaks of crystal violet are compared for length, as it is assumed that variation in length depends upon the degree of Gram staining behavior, the most Gram-positive species showing the longest streak.

In principle Mittwer's method appears to be a suitable one. However, his staining of bacteria with an overdiluted crystal violet solution (0.1 percent) reduces the relevance and reliability of his evaluation of the degree of Gram-positive behavior. This is especially so since, as Barbaro and Kennedy have conclusively demonstrated, an increase in dye concentration is accompanied by a differential dye uptake between Gram-positive and Gram-negative bacteria (2). These authors used 10-percent solutions of crystal violet in accordance with the recommended range (1 to 10 percent) of the Gram staining procedure (2). My model experiments also illustrate the fact that uptake of crystal violet by proteins can be substantially increased by raising the concentration of the dye. For example, in 10 minutes 1 mg of casein and 1 mg of iodinated casein take up 1.7 and 2.6 μg, respectively, of crystal violet from a Tris-buffered (pH 7.2) 10-4 M dye solution at 20°C; a sixfold increase in dye concentration raises the previous values to 4.2 and 7.2 μg, respectively. The data were obtained with the aid of a Perkin-Elmer Spectracord spectrophotometer at H2O = 595 mμ. If, however, we deal with bacteria rather than proteins, a suboptimal range of 0.1-percent in dye concentration must be considered. In and below that critical range the differential in crystal violet uptake between a Gram-positive and a Gram-negative organism ceases to exist (2).

Finally, it should be pointed out that a semimicro method for measuring the degree of Gram-positive staining behavior of bacteria and other biological material has been published (4). We used a concentration of 1-percent crystal violet and a chromatographic technique, separating the total amount of dye taken up by bacteria into compact spots; then the spots were eluted, and their dye content was quantitatively determined with a spectrophotometer.

ROLAND FISCHER
Ohio State University Health Center, Columbus Psychiatric Institute and Hospital, Columbus

References

Roland Fischer's comment appears to be based upon my use of an "overdiluted" solution of crystal violet. It is true that an increase in dye concentration can be correlated with an increase in "dye uptake" by a constant amount of bacterial cell material, with the time of contact constant (1). However, no reduction in the reliability of my method should result from the use of 0.1-percent dye solution, since numerous tests had established the validity of this concentration in routine qualitative Gram differentiation. Such tests can be confirmed by anyone in a few minutes. In fact, a rather