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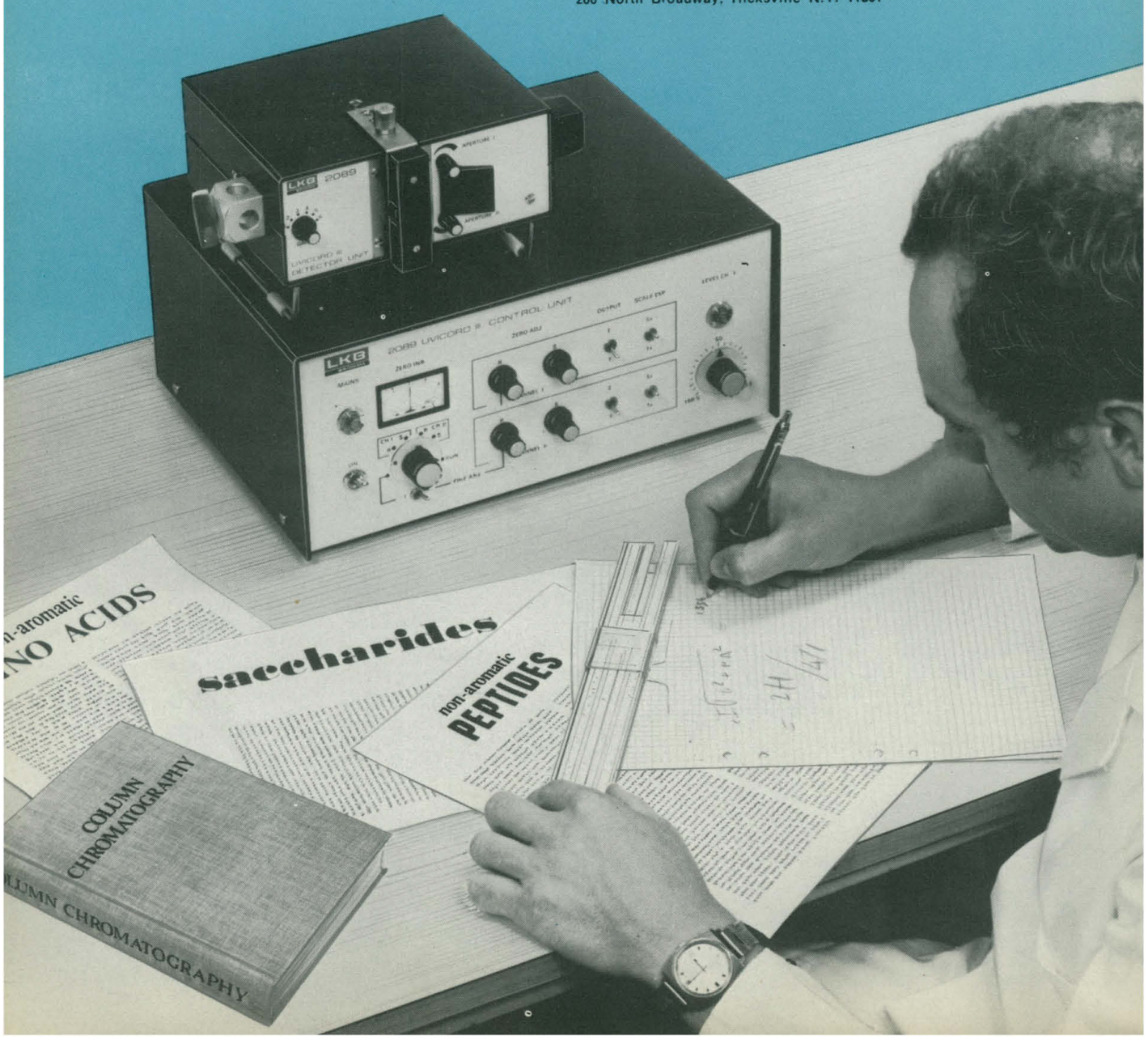

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SCIENCE

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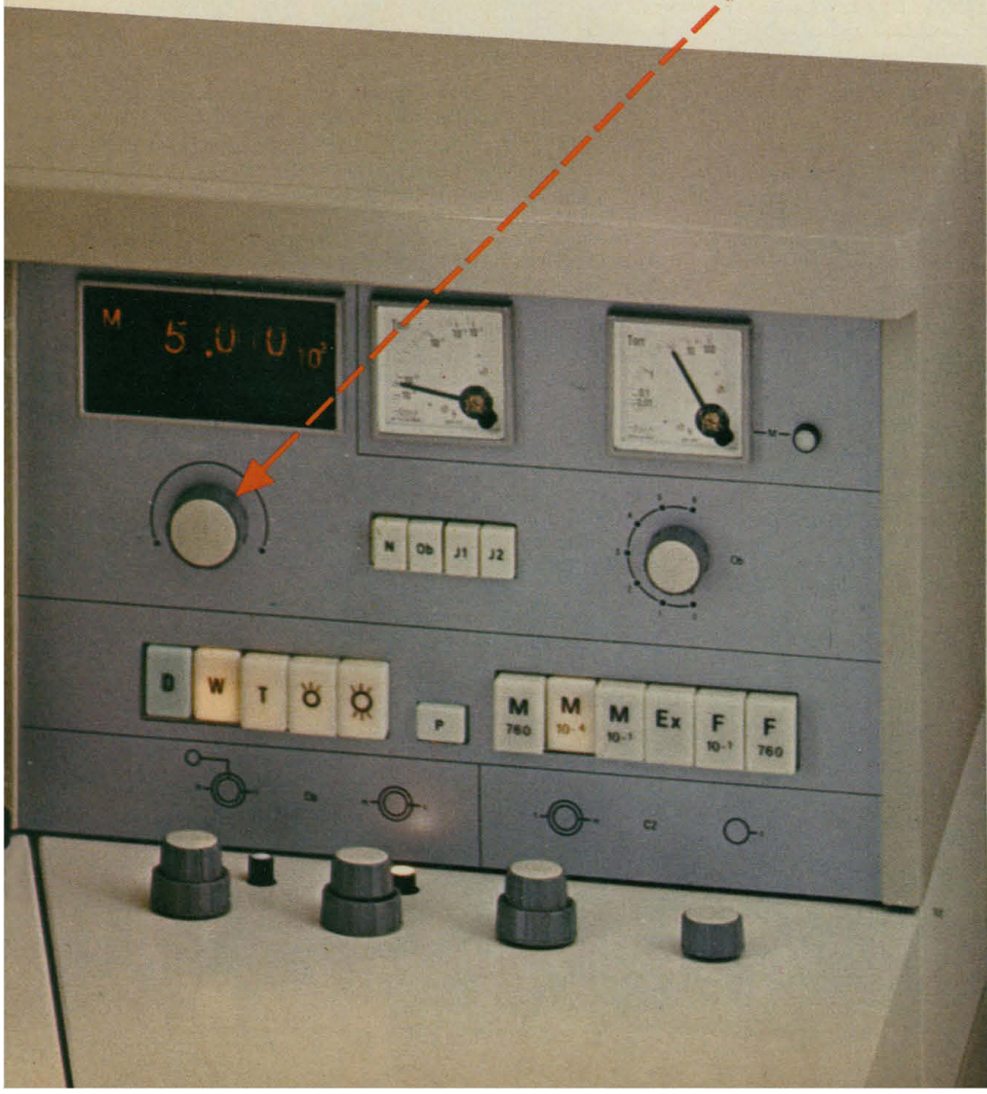

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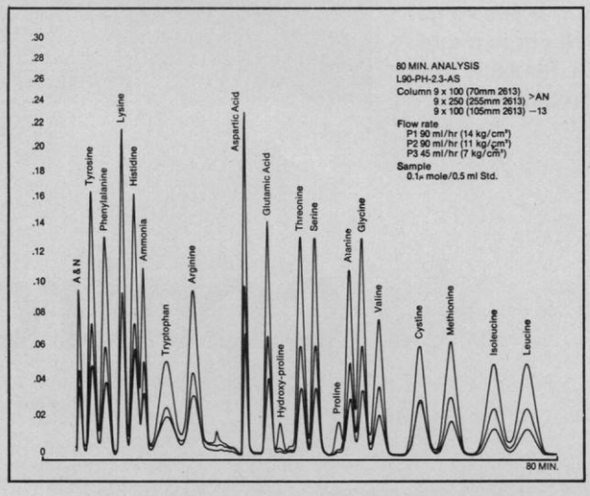


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45.3	.4687	.453,	.0160,	.1009,	PROLINE:
46.9	1.2884	.469,	.0265,	.1001,	ALANINE:
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51.9	1.3113	.519,	.0260,	.1007,	VALINE:
58.8	1.5937	.588,	.0150,	.0989,	CYSTINE:
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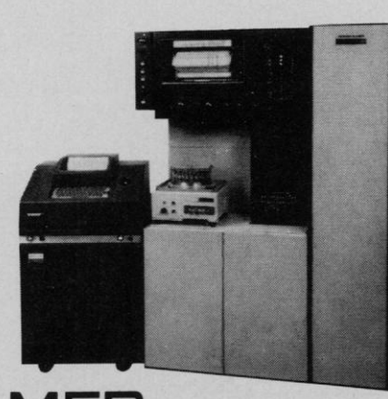

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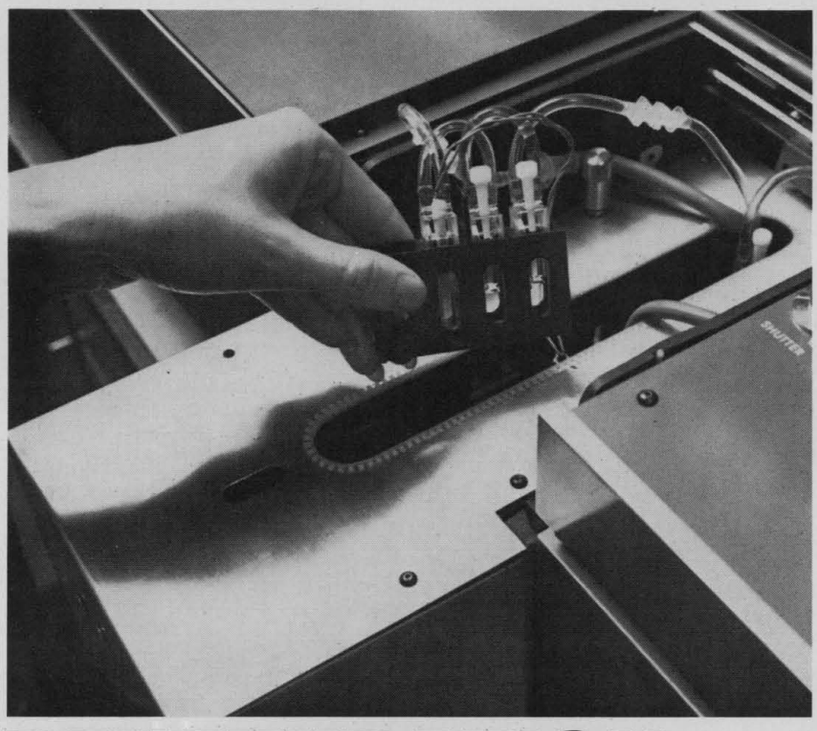

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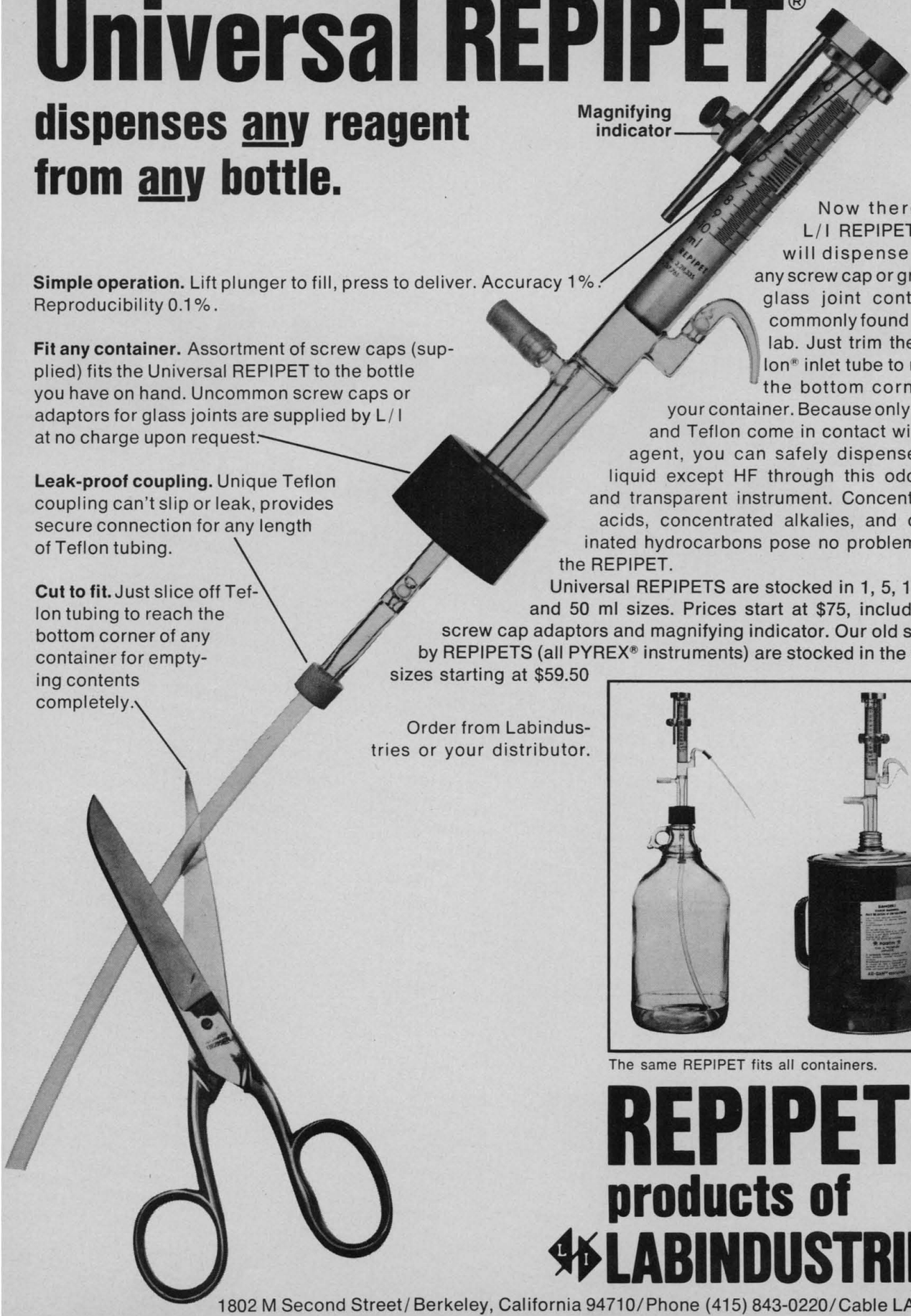

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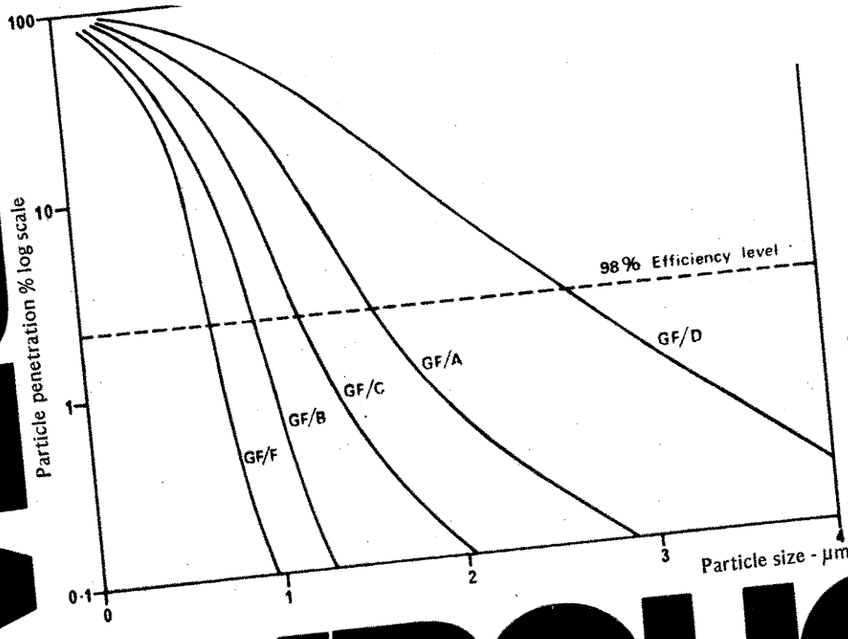

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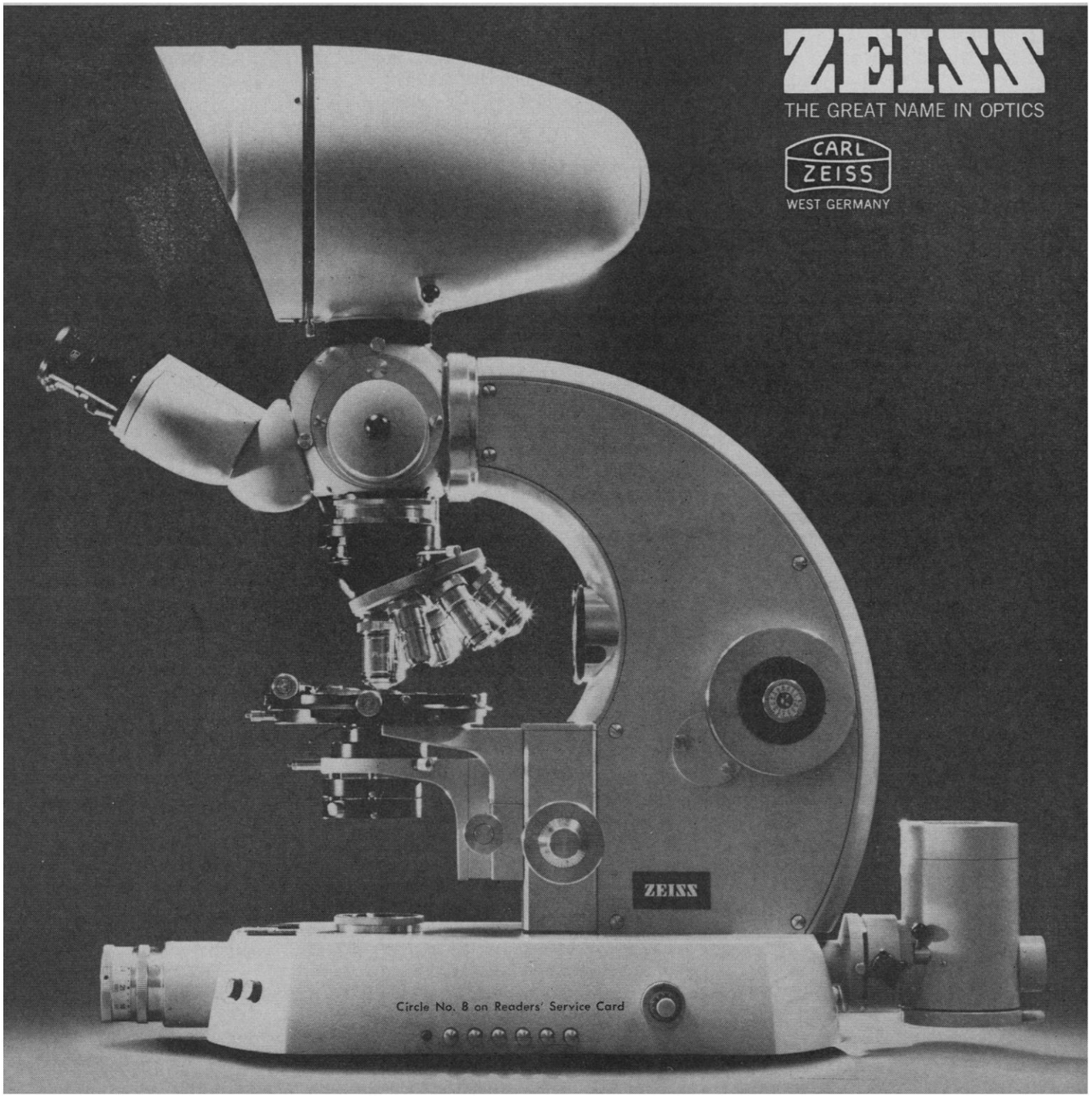

the system. Powers does not provide adequate, empirically based definitions of the key concepts, such as "reference signal," and in this sense his model is nonbehavioristic. Nevertheless, as far as one can determine, the model is mechanistic, in that the components of the feedback loop are analyzed as a unidirectional, linear causal chain. The very fact that the components can be analyzed in this way indicates that there is no dialectic interpenetration, or reciprocal interaction, because in such interactions the components are inseparable from the whole or structure that comprises them (*1*).

Powers concludes that "Behavior itself is seen in terms of this model to be self-determined in a specific and highly significant sense that calls into serious doubt the ultimate feasibility of operant conditioning of human beings." Were it not for the ambiguity of the meaning of "ultimate feasibility," one could reject the statement on empirical grounds. The research literature is replete with studies demonstrating operant conditioning in human subjects by human experimenters, in some cases without the subjects' being aware even that they were subjects (*2*). One can also, in any case, question the statement on theoretical grounds, because "self-determined" has, as Powers says, a specific meaning in the model, and this meaning has no implication of free will. In the model, "organisms are self-determined in terms of inner control of what they sense, at every level of organization except the highest level." Inner control refers to feedback ("error signal") regarding the discrepancy between the "reference signal," or goal, and the "sensor signal," or input. As the model is mechanistic, the error signal will inexorably produce specific "effector functions," or responses. That is, the responses are completely determined by the error signal (and, perhaps, by the state of the system), and the error signal is completely determined by the reference signal and sensor signal. As the sensor signal is determined by the environment, any variability in self-determination must come from variability in reference signals. Their source is not specified in the model (except at the highest level, at which they are assumed to be biogenetically determined). The model does not demand a reference signal that prohibits operant conditioning; this reference signal was introduced ex hypothesi and is not entailed by the model.

In summary, the model (i) is not explanatory, (ii) is not the only mechanistic model that provides a derivation of purpose, and (iii) does not intrinsically preclude human operant conditioning.

HAYNE W. REESE
Department of Psychology, West Virginia University, Morgantown 26506

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1. H. W. Reese and W. F. Overton, in *Life-Span Developmental Psychology: Research and Theory*, L. R. Goulet and P. B. Baltes, Eds. (Academic Press, New York, 1970); pp. 115–145; W. F. Overton and H. W. Reese, in *Life-Span Developmental Psychology: Methodological Issues*, J. R. Nesselroade and H. W. Reese, Eds. (Academic Press, New York, 1973), pp. 65–86; W. F. Overton, *Hum. Develop.*, in press; L. von Bertalanffy, *General System Theory* (Braziller, New York, 1968).
2. H. M. Rosenfeld and D. M. Baer, *Psychol. Rev.* **76**, 425 (1969).

The comments by Baum and Reese on my control-system approach to understanding behavior are the most balanced I have received from behaviorists. I thank them for trying to find a place for my work within behaviorism, an attempt that reflects generosity, but not understanding, of what I said (or tried to say). The conceptual basis of control-system theory is so alien to behavioristic thought that there can be no such easy reconciliation. The best we can hope for is a constructive confrontation.

Baum says that a scientific psychology must deal with the observable, which to him means behavior. Behavior, however, is not something self-evident that anyone can see just by looking. What is the behavior of a man walking? Is he really tensing his leg muscles, moving his legs, walking, going to buy a paper, on his way to work, making a living for his family, or maintaining his self-respect? The point of view of the observer defines the behavior he sees. The *actual* behavior of the nervous system consists only of sending neural signals to muscles and glands; that is the last event that truly reflects the system's output. From that point outward, the results of that output become more and more mixed with properties of any events in the external physical environment, so that even such elementary behavior as a "movement" no longer is a unique indicator of a particular activity in the nervous system. Thus, while Baum's pronouncement seems reasonable on the surface, it ignores one of the deepest conceptual dilemmas of behaviorism.

The control-system model shows that behavior at any level, as well as its relationship to "stimulus events," makes sense as soon as one recognizes the

concept of the controlled quantity. To find the proper definition of the controlled quantity, the observer must recognize that his own point of view determines the behavior he will observe, and he must find an objective way to discover the *right* point of view —namely, that of the behaving system. The observer must try to find out which of the infinity of potential controlled quantities is the one that the behaving system is actually sensing and controlling. Only when the controlled quantity has been correctly identified can the observer see that the system's outputs are always such as to counter the effects which environmental disturbances would otherwise have on the controlled quantity. In my article I presented an experimental paradigm, new to psychology, for testing hypotheses concerning the controlled quantity and its reference level.

In the section on controlled quantities in my article, there appears an approximation, $g(d) \approx -h(o)$, which says that the cause-effect relationships that can be observed between stimulus events and consequences of nervous system outputs—responses—are expressible wholly in terms of the physics of the local environment, containing almost no information about the behaving system at all. I see no way in which behaviorism can survive a full understanding of the derivation and significance of this harmless expression. If control-system theory does indeed describe correctly the relationship between organisms and their environments, behaviorism has been in the grip of a powerful illusion since its conceptual bases were laid.

It is therefore not possible that behaviorism already contains an adequate treatment of feedback phenomena; if it did, a behaviorist would have discovered this illusion already. Many behaviorists have observed feedback phenomena, but they have tried to deal with them by translating the terminology of control-system theory in such a way that well-accepted behavioristic principles would remain undisturbed. That is why "purpose" has lost its original meaning of inner purpose or intentionality, and has been redefined as *consequences*. That redefinition was necessitated by the fact that early behaviorists knew of no physical system that could contain inner purposes—their telephone-switchboard model had no place for them, and control-system theory then lay far in the foreseeable future.

In control-system terms, a purpose

is not a consequence of behavior, but a model inside the organism for what it wants the perceptual consequences of its outputs (modified by environmental disturbances or not) to be. When I bowl, my inner purpose is to perceive all the pins falling on the first ball. What I perceive is generally something different. I am still doing my best to alter my outputs in such a way as to reduce the error between what I generally perceive and what I intend to perceive. Another observer can discover that intended perception by manipulating my environment until he finds the state where I cease to alter my outputs in opposition to the changes he causes. There is nothing metaphysical or conjectural about this process. But it does not make any sense in behavioristic terms, because it is designed around rigorous laws of feedback, not around the imprecise usages of the term feedback that are found in behaviorism.

There seems to be a general impression that feedback is analyzable (in Reese's terms) by following a "unidirectional, linear causal chain" around and around a closed circle (I trust that Reese noticed that the circle *is* closed). That approach to feedback, often expressed as taking into account the effects of a response on subsequent stimuli, is the natural one, but, as every beginning control-system engineer soon discovers, it leads to totally incorrect predictions of the behavior of the system being modeled. The qualitative chain-of-events approach leaves out the crucial factor of system dynamics; when that is properly taken into account, through use of a physical analysis of the system and its environment and application of differential equations or transform methods, a very different and surprising picture emerges. If the control system one wants to model is free of spontaneous, self-sustained oscillations (as normal behavioral systems are), time lags in the system can safely be ignored, and the behavior of the whole system can be seen quite correctly as occurring *simultaneously* with disturbances. The output changes *along with* the disturbance (a normal, slowly varying disturbance), and the input variable being monitored continually tracks the inner reference signal, if a variable inner reference signal exists. There are no loopholes in this analysis; if organisms are in the negative feedback relationship with their environments, this is how they behave. To arrive at a different conclusion, one would have to show that the bases of

control-system theory are wrong, and he would have a lot of engineers who use it every day to convince.

Thus, the attempts by behaviorists to bring feedback phenomena into the scope of their conceptual scheme have involved only a superficial adoption of certain terms and loose qualitative observations, the true beauty and power of control-system concepts having been left behind. The distortions of feedback theory that occurred in the adoption of the terminology were precisely those which would prevent change in the basic conceptual scheme of behaviorism (this should not surprise control-theory fans, since all organisms manipulate their own perceptions to keep them in the desired state).

There is no "reference signal that prohibits operant conditioning," as Reese puts it while guessing wrong about what I meant. Operant conditioning is a fact; in my model, it is a portion of a control process whereby organisms modify their own inner structure of control systems as a means of keeping certain critical variables (W. R. Ashby's term, as I noted), at genetically established reference levels. I was talking about the *feasibility* of people deliberately trying to control the behavior of other people through deliberate application of operant conditioning.

In order to control another person, one must establish contingencies or schedules of reinforcement. Whatever one chooses to use as a reward, he must make sure (i) that the subject needs or wants the reward and (ii) that the *only* way the subject can obtain the reward is by doing what the experimenter wants to perceive him doing. The experimenter, of course, is trying to control his own perceptions relative to his own inner purposes, using the subject as his means.

The establishment of contingencies, therefore, requires that the experimenter already be the sole source of something the subject wants, and establishing that situation is where operant conditioning will fail as a way of controlling behavior—as it has failed throughout recorded history. An experimenter trying to control people rather than laboratory animals cannot conceal the fact that he has what the subject wants, and is withholding it until the subject does what the experimenter demands. If one person can establish a contingency, another person can see that he has done so, and can decide to "unestablish" it. If the act that the experimenter wants to see performed in any way inconveniences the subject, the subject will be forced by his own nature to find a way to circumvent the contingency. He can operate properly only on the basis of his own inner purposes, not on the basis of the experimenter's. Only a god capable of seeing a person's entire structure of inner goals could establish contingencies for that person without creating conflicts that would lead to a direct and violent confrontation. Even then, the god would be constrained to controlling the person in ways that created no uncorrectable errors in that person's control hierarchy.

Operant conditioning is only a modern term for what people have been trying to do to each other since civilization started. Everyone knows that people seek rewards and will change their behavior, within limits and as necessary, to get those rewards. But rewarding always implies withholding, and withholding what people need is a sure way to create violent and bloody conflict. An excellent case can be made for the statement that the present state of the world is the direct result of people trying to set up contingencies of reward for each other. It is time we realized that this principle of social interaction is the cause of, not the solution to, our most serious human problems.

Finally, I want to acknowledge the justice of some of the criticisms of my work. I know that I have overgeneralized in speaking of "behaviorists" when I really should have said, "some behaviorists." My aim is to find ways to effect a transition from what I believe is an outmoded view of the nature of human nature—and animal nature—to what seems a vastly more productive and humane point of view. My attitude toward what I see as the basic errors of behaviorism is not one of irritation or superiority. My model is only a feeble step in the right general direction. It is instead that there is an enormously difficult task ahead—but, considering what I see as the possible results of success, worth all the effort. I hope that Baum and Reese and other behaviorists will come to see it this way after careful consideration. I know their task is harder than mine, and it would be even harder if this clash of ideas were set up so that someone had to win, and someone had to lose.

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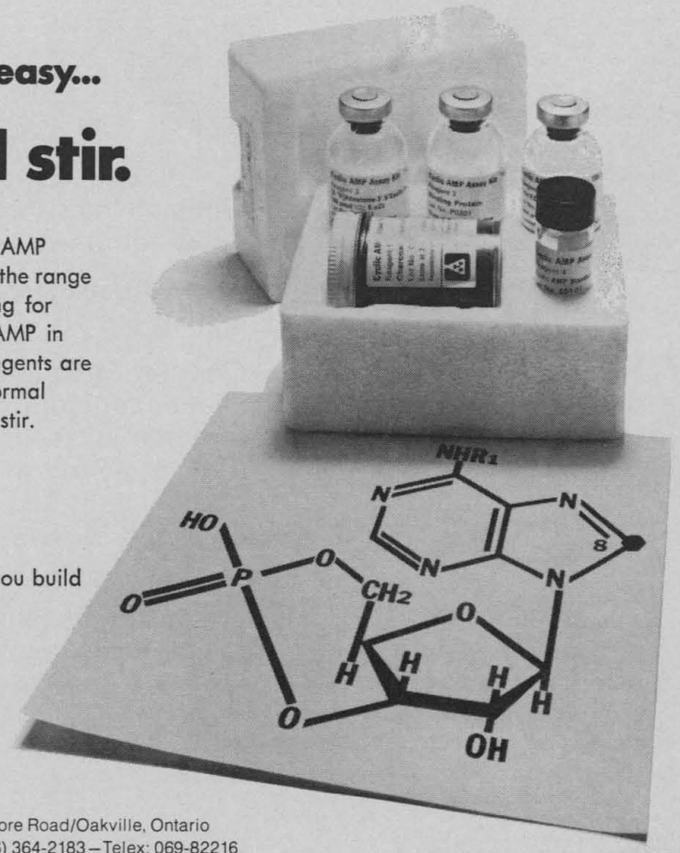

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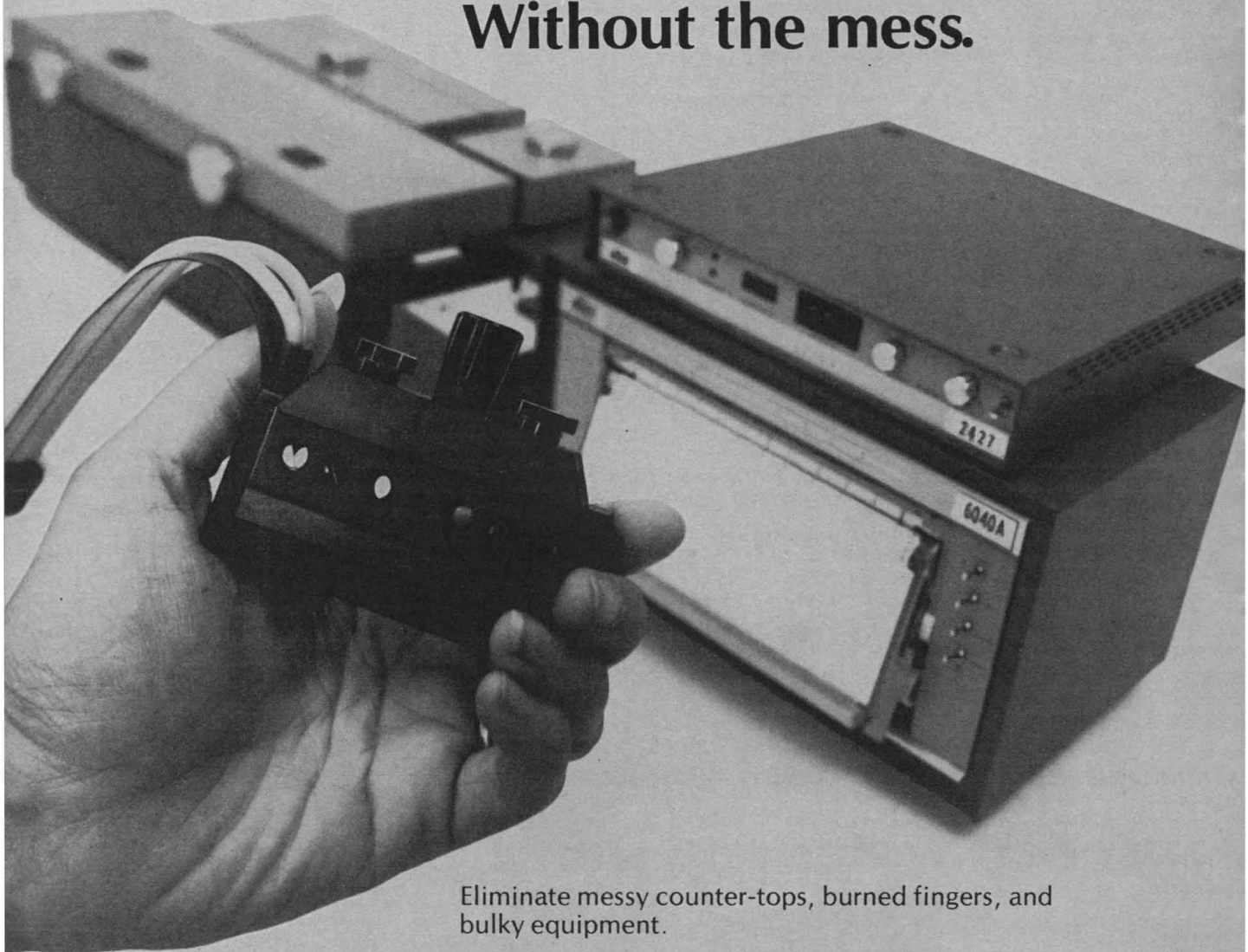

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21 September 1973, Volume 181, Number 4105 **SCIENCE**

Public Views of Scientists

A Louis Harris poll taken in late 1972 allows one to make a quantitative analysis of how the public feels about scientists. The result is rather different from what has often been bemoaned.

While the proportion of the public expressing "great confidence" in the people "running science" has fallen from 56 percent in 1966 to 37 percent in 1972, this decline does not support the notion that *the* public is disenchanted with science. Moreover, the trend might already be reversing itself. Five percent more people in 1972 than in 1971 expressed great confidence in the men and women of science. (Comparable data on 1973 are not yet available.)

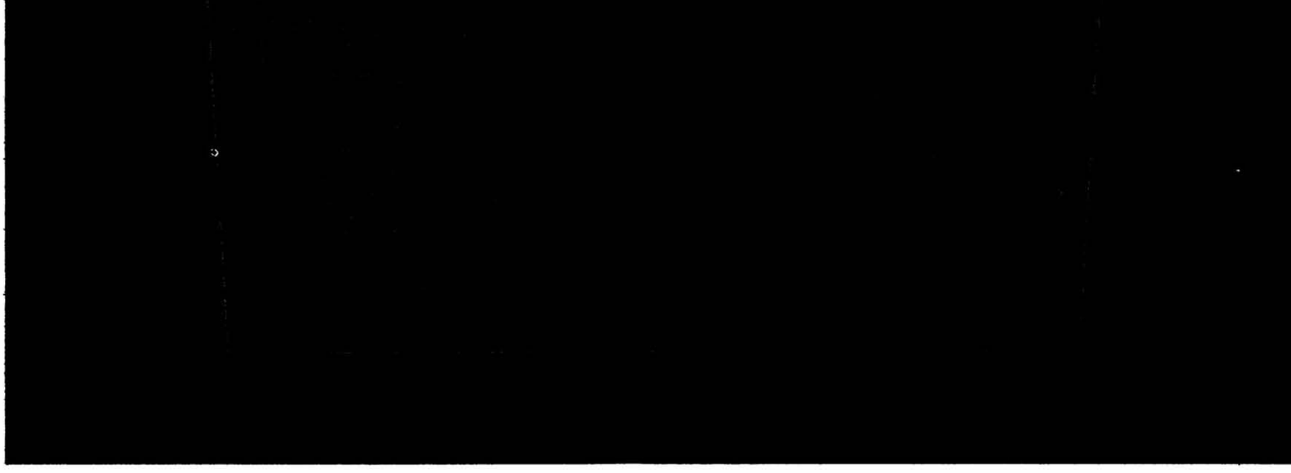
This falling away from science is part of a general lessening of faith in American institutions and authorities rather than a major antiscience ground swell. Questions were asked about 16 institutional areas, ranging from religion to the military, from the press to major U.S. companies. Appreciation for all of them, without exception, has fallen since 1966 to below the 50 percent mark.

Science fares better than most institutions. It ranks third in the confidence list, surpassed only by medicine and finance. It ranks higher than, among other things, the U.S. Supreme Court, the Congress, and the federal executive branch. The relative position of science has improved. It ranked fifth in 1966 and since then has surpassed the military and education in the public's trust. While in 1966 it was 16 percentage points away from the highest rating, in 1972 the distance was only 11 points.

Equally interesting are conclusions we have drawn from details of the poll. Young people are *not* the main source of lack of confidence, their elders are. Highest ratings were given to scientists by the age group 18 to 29 (41 percent "great confidence"), the lowest by those aged 50 and over (33 percent). The age group in between was rather close to the younger one (40 percent). The college educated are *not* Luddites or members of the counterculture or of antiscience brigades. They express significantly more confidence in scientists than do high school graduates (49 percent and 33 percent, respectively), who in turn appreciate scientists much more than do those with still less education (20 percent).

People in the Deep South, in the rural parts of the country, and whose income is lower than $10,000 a year stand out as least confident in the scientific community, as compared to those economically better off and those in the more developed parts of the country. That more "liberal" Americans might add to this main source of discontent is suggested by the fact that those who intended to vote for McGovern were less favorable to scientists than those who intended to vote for Nixon, by a margin of 33 to 41 percent.

Poll data, especially when we must draw on one poll alone, do not provide a precise and reliable reading of the mind of the public. But the data do provide a useful antidote to quick overgeneralizations and grand simplifications as to the scope, source, and direction of antiscience sentiments. For the friends of science, there seems little ground for hysterical alarm. While obviously the work and values of science must be carried to many Americans, a job to which the AAAS has been devoting increasingly more effort, there is certainly no reason to despair of public support or to believe that a greater recognition of the merits of science cannot be regained.—AMITAI ETZIONI, *Professor of Sociology, Columbia University, and Director, Center for Policy Research, Inc., 475 Riverside Drive, New York 10027, and* CLYDE Z. NUNN, *Senior Research Associate, Center for Policy Research, Inc.*



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