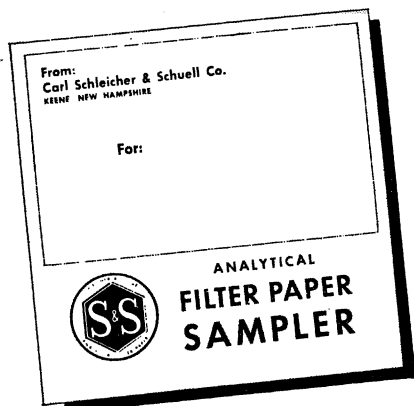


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these conditions, could be attributed directly to the similarity of the receptacle in which animals were placed initially to the test receptacle used for the feeding period. This would constitute a demonstration of the delayed feeding effect.

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Social Science Research

In a recent issue of *Science* [**136**, 509 (1962)] there appeared a joint review by Kenneth Boulding of Clarence Ayres's *Toward a Reasonable Society* and Marshall Dimock's *The New American Political Economy*. Boulding dismisses Dimock's book as a study in the pathology of rhetoric. He is somewhat kinder to Ayres, whom he classifies as the outstanding representative of the school of institutional economics. Then he goes on, "For all the enormous difference in quality in these two works . . . [each] represents, in a sense, a personal philosophy of society. . . . But [this type of intellectual activity] does not have that peculiar property of securely based information and carefully tested prediction which is the identifying mark of the scientific process." Then follows this patronizing comment: "Political Non-Science is not necessarily nonsense, though it seems to have a bias in that direction. It should not, however, be mistaken for that *political science based on quantitative data and testable theory, which is now in the making*" (italics mine).

May I remind Boulding of Aristotle's dictum that each field carries a method appropriate to the material it is examining. Paul Lazarsfeld is much more modest in his claims. He refers to the hostile outsiders who ask, "What has social research all added up to in the last fifty years? Is there any sociological finding that has not been anticipated by philosophers or novelists? The answer has to be qualified. True, it is unlikely that any surprising 'discoveries' will be made for quite some time to come."

I am certain that the work of Dimock and Ayres will survive Boulding's review. What is disturbing is the implication that the methods of the physical sciences are the sole key to unraveling the mysteries of the social disciplines. This is not an entirely new point of view. Herbert Spencer was able to gain a widespread audience for nonsense disguised as sociology by trading on the language of Darwin. Just as physics-thinking dominates our intellectual climate today so Darwinian thinking dominated the late-19th century cultural climate.

May I suggest that the social researchers who are engaged in quantitative research be somewhat less aggressive in proclaiming their virtues until they are able to come up with some insights not previously arrived at by other methods.

It is distressing that this quasi-scientific group, despite the poverty of the results thus far gleaned from their labors, demand control of all research in the social disciplines.

There is room for both traditions in our intellectual investigations. It is unfortunate that the physics-oriented group seem to have the inside track where funds from federal sources and from the private philanthropic foundations are concerned.

However, in the area of social research, despite all the financing and all the grants, we have yet to see men of the stature of Veblen or Commons or Mitchell emerge.

The whole field of social studies is in a serious state of flux. If social studies are to receive federal support, then the criterion of choice should be broader than the National Science Foundation's narrowly conceived ideas of what basic social science research is. The kind of intellectual authoritarian arrogance portrayed is doing well enough without additional federal funds to encourage it.

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Gomberg is reading things into my review which I did not say. I did not say, nor do I believe, that the methods of the physical sciences are the sole key to unraveling the mysteries of the social disciplines. I have always maintained that the methods of the humanities gave us true knowledge and important knowledge, and I have main-

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tained also that the social sciences by their very nature are bound to be a mixture of humanistic studies, derived from the knowledge which man has of himself through being a man, and "scientific" studies, derived from careful observation, sampling of the social universe, measurement, indexing, and testable theories. I do not think, for instance, that all of economics is contained in econometrics, but I think we owe a great deal to those who pioneered in the measurement of economic variables, and our capacity to control depressions, for instance, has been substantially increased by our ability to measure at least the gross variables of the economic system. Nor do I think that political science is encompassed in the nascent discipline of politico-metrics. Historically, however, the weight in political science has been wholly on the humanistic side of the study, and important contributions are still to be made on the other side. I believe this to be especially true in the conduct of international systems where

the absence of accurate and well-sampled quantitative information is a serious handicap.

I agree entirely with Gomberg that there is room for both traditions. Where, however, as in the case of political science, the introspective and philosophical tradition has been pursued almost to the exclusion of the other, it is surely legitimate to call attention to this imbalance.

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Feedback and Nonfeedback

This is a comment on a comment [Moment, *Science* **136**, 1056 (1962)] on a comment [Li, *ibid.* **136**, 1055 (1962)] on a comment [Moment, *ibid.* **136**, 262 (1962)]. Moment considers that the "nonfeedback model" suggested by Li to account for the high degrees of polymorphism in species such as the brittlestar *Ophiolis aculeata* "assumes a stable diversity already in existence which does not change, while the pro-

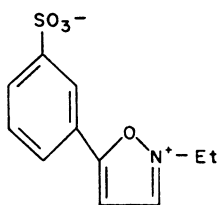
posed theory of protective variation assumes that any mutation producing a new pattern would enter the system with a selective advantage *which would decline as the frequency of the responsible gene increased*" (italics mine). On this basis, he visualizes the testability of the merits of the reflexive selection (feedback) model and the balancing selection (nonfeedback) model.

The two models may indeed yield testably different predictions, but in neither case is the selective value of a newly introduced allele unchanged as the allele increases in frequency.

In a randomly crossbreeding species, with heterozygote superiority at a locus (one form of "balancing selection"), a heterotic allele newly introduced, whether by mutation or immigration, will be present first in heterozygous condition and will remain so until its frequency, relative to population size, is high enough to create an appreciable probability that heterozygote-carriers will mate. From then on, its selective value will decrease from its initial maximum to the value determined by the equilibrium ratio of homozygotes to heterozygotes for the introduced allele, which value in turn will depend on the

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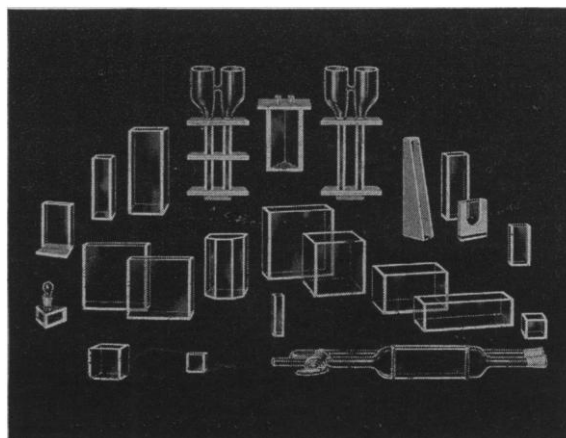
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