Letters

Social Science: Identifying Major Advances

Several of the conclusions reached by Deutsch, Platt, and Senghaas in "Conditions favoring major advances in social science" (5 Feb., p. 450) are based on misinterpretations of the data. The assertions that recent advances in social science are accomplished outside of scholarly ("ivory tower") settings, require large-scale government funding, and are closely tied to practical applications are reflections of the authors' sampling bias.

1) The list of advances (Table 1) includes 19 cases consisting primarily of innovations in research methods (items 5, 15, 21, 22, 27, 33, 37, 38, 39, 41, 43, 44, 48, 49, 51, 52, 56, 60, and 62), which are not necessarily equivalent to actual advance in explanatory knowledge of social phenomena; for only four cases (items 33, 38, 39, and 52) can this be plausibly argued.

2) Political and economic-administrative techniques created in practical settings (items 3, 16, 17, 18, 32, 47, 59; and 39, 41, 50, 51, 54 which are partly created in practical settings) are lumped together with the basic work of scholars, although the former group is primarily responsible for most of the practical "applications" in this list of innovations.

3) Several fields of substantive research are included which attracted flurries of considerable interest from both within and outside the social sciences, but nevertheless cannot be said to have made major advances in explanatory theory; this would include item 26 (urban ecology) and item 36 (authoritarian personality studies), as well as various of the methods listed under point 1. There are also included several fields of research, such as learning theory (item 10, of which item 45 is a part) and economic development (item 50), which, despite their longevity, have not yet achieved minimal consensus on an explanatory paradigm. A continuous research tradition is not the same thing as a successful development of knowledge.

4) Several of the most successfully developed areas of social science theory and research are omitted: notably, the theory of power and goal-displacement in voluntary associations begun by Michels (1911) and developed since 1940 by Selznick, Lipset, and others; the dynamics of interpersonal behavior in formal organizations [theory deriving from Barnard (1938) and research tradition begun by Mayo *et al.*, during the 1930's, with major theoretically based studies in the 1940's by Selznick, Gouldner, and others]; the theory of social labeling in the study of deviance, developed by Lemert, Becker, Goffman *et al.* in the 1950's; and development of physiological psychology following on the brain-electrode experiments of Olds in 1953. The last, and related areas, are apparently excluded (p. 455) because they do not meet criteria of wider applicability, although one may not use such a standard in selecting a sample from which generalizations are to be made concerning trends in applicability of innovations.

It may be argued that despite a flurry of methodological innovations since World War II, there has been little real advance in social science knowledge; most of what there is has been associated with the names of Chomsky, Levi-Strauss, and the areas of study of organizations, deviance and social phenomenology, and physiological psychology. All of these are areas based heavily on individual researchers in traditional scholarly settings. Whatever the utility of the article as an appeal for support for the particular interests represented by Deutsch *et al.*, their argument has little merit either as sociology of science or as a general prescription for the advance of the social sciences.

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The list of "major advances" in the social sciences from 1900 to 1965 (Table 1) with which Deutsch, Platt, and Senghaas occupy themselves can be criticized with respect to clarity and completeness, if I may judge by the entries in economics.

Among the factual errors, there are the following:

Item 1. Pareto's law of income distribution appeared in volume 2 of his *Cours* (1897), and falls outside the period.

Item 12. The role of innovations in socioeconomic change possibly overlooks Marx.

Item 19. Social welfare functions in politics and economics omits such major, earlier figures as Edgeworth and Pareto (both before 1900).

Item 39. National income accounting ignores the pioneer work at the National Bureau of Economic Research in 1920–22, under Mitchell's leadership.

Item 54. Econometrics ignores *all* the pioneers: Moore, Lenoir, Frisch, Schultz, and others.

The analytical criteria for a major advance are surely unsatisfactorily loose. Major advances consist of (i) empirical relationships, not generally accepted (1); (ii) erroneous theories not generally accepted (Schumpeter on interest rates) (12); (iii) new theoretical models (30 and 35); (iv) comprehensive and systematic empirical measures of economic activity (39); (v) methods of maximization under simplified conditions (43); (vi) nine men's writings on a vast range of problems (50); (vii) an immense body of literature without any unified theoretical core or methodology (54); and (viii) work which has not yet had a major impact on economics (54). Classes i, ii, vi, vii, and viii do not seem appropriate to a list of major scientific contributions.

Conversely, their list omits numerous works of wide and persistent influence, such as Moore on statistical demand curves, Friedman on the consumption function and the demand for money, Hicks and Allen on utility, Samuelson on comparative statics and public goods, Means on rigid prices, Solow on growth models, and Haavelmo on simultaneous equation systems.

The history of science is plagued with impressionism. Let us hope that the authors face more seriously their own query: "Are there publicly verifiable criteria" of major advances?

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The letters by Stigler and Collins are serious and very welcome contributions to the debate we had hoped to set off with our article, and we are grateful for these and many other letters we received extending and correcting our Table 1.

Some of the points raised by Stigler

have coincided with our own views from the outset. This would have been clearer if space limitations had not prevented our including in the article a column, "Based on whose work," which is part of our longer study (*Mental Health Research Institute Communication No. 273,* May 1970). This column lists Pareto among the forerunners of work on the social welfare function (19), and Moore and Frisch among the pioneers of econometrics (54). Pareto is also listed among the forerunners of general systems analysis (40).

The contribution of Marx to the role of innovations in socioeconomic change (12) seemed to us rather remote and general as compared with the more direct forerunners of Schumpeter's work on innovations (not interest rates), such as Böhm-Bawerk, Walras, Edgeworth, and Clark, all of whom we listed as pioneers. The role of Mitchell and the National Bureau of Economic Research in the 1920's as pioneers of national income accounting (39) did not seem to us as important as it does to Stigler.

The question of dating contributions which developed over a number of years is notoriously difficult. We listed Pareto together with Gini under the theory and measurement of income inequalities (1), despite Pareto's publication of 1897, because we felt that much of the decisive development toward making the theory operational in terms of measurements and quantitative comparisons across countries and groups occurred after 1900. We similarly included the next six contributions on our list, all of which started before 1900, but which seemed to us to have reached full stature only thereafter, such as Weber's work on bureaucracy (2), and Freud's development of psychoanalysis and depth psychology (4).

To have excluded these seven cross-1900 contributions would have made the post-1930 period look much more productive and still more quantity-oriented than the 1900–1930 period. It would also have increased the relative weight of a few major U.S. research communities, such as Chicago and Cambridge, and the weight of the cases in our study which were team-oriented, capital-intensive, and connected with research institutions. Thus it would have strengthened still further some of our findings. We chose to do the opposite: to include these borderline cases and to report those findings that would survive their inclusion.

Accepting Stigler's views on all these matters or listing Samuelson's contribution as "comparative statics," rather than under our broadly interpreted heading of "econometrics" (54), would not have changed our major findings.

Collins' comments are much more distant from the nature of our study. In essence, he proposes an entirely different one. His notion of "real" science is centered on "explanatory theory," while our operational definition of a major advance called for the discovery of a demonstrable new fact or relationship, or of a repeatable new method or operation, and, in any case, for a major impact on social science. In the work of Maxwell, Hertz, and Marconi, it seems that Collins would have looked for their "explanatory theory," which was the "ether" theory, long since discarded. We would look at Maxwell's equations, Hertz's waves, and Marconi's wireless telegraphy, all of which remained lasting and cumulative contributions even though the accompanying explanatory theories have changed.

Collins, however, also objects to the finding of our study. He perceives them as denigrating the importance of the work of individuals in a "scholarly" and "ivory-tower" setting, and as extolling large team projects with "government funding." But the words "scholarly" and "government funding" are not ours.

We do report that much work came, and presumably will continue to come, from relatively few locations of concentrated intellectual effort such as London, the two Cambridges, Chicago, New York, and Washington. But most of this work was scholarly, as we understand the term, and though much of it required a good deal of capital, there were a wide variety of sources of support, public and private. We did not find that most of the advances required large bureaucratically organized research teams, nor did they need monolithic control by a single organization. What seems to have worked best were small teams in large places—a spatial concentration of stimulation and support, a plurality of persons, organizations, and initiatives, and some communication with the world of practical needs.

Here again, our main findings would stand, even if we had included the cases which Collins recommends. The work of Barnard and Maye, together with the studies that developed from it, was not a lone wolf affair, nor was it unconnected with the needs of practice. Likewise, the work of Michels and Lipset on power in organizations, of Becker and Goffman on the social labeling of deviants, and of Chomsky (which we listed) on structural linguistics would have fitted in well with our findings of location at a relatively few centers, some connection with practical needs, and rising requirements of manpower and capital in recent decades.

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Registry of Women Scientists

I am a biologist engaged in the very disheartening task of seeking a teaching position for the fall 1971 academic year. Typical of the replies to my letters of application is the letter I received from the University of Colorado stating that "just under 300 applications" had been received for the position I sought. It is obvious that schools seeking faculty for 1971 will have an excellent market to select from —so much so that it would seem possible to hire exactly the person a school wanted. Accordingly it seems appropriate for schools hiring faculty to concern themselves with any imbalance (race, sex ratio) that might exist in their departments.

It is interesting to note that, at the recent meetings of the Federation of American Societies for Experimental Biology held in Chicago, a group of women registered at the meeting organized themselves into the Association of Women in Science. The purpose of this new organization is "to promote equal opportunities for women to enter the professions and to achieve their career goals." An immediate aim of the new organization is to prepare a North American registry of women in science. This registry will be available to professionally trained women seeking employment and to employers seeking to fill academic or industrial positions. Thus, the registry will be of help to Magasanik (19 Feb., p. 631) in his desire to add qualified women to his staff. Inquiries regarding membership in the Association of Women in Science can be addressed to Dr. Gertrude Schloer, Mt. Sinai School of Medicine, City University of New York, New York 10029.

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