

ways", than earlier eras. As for the relationship with social change, the innovators do not operate pro but rather contra the status quo, so that change is changing, but the culture in some of its manifestations - like organization - continues to churn out the relatively predictable and uninspired societal member. Whereas Parsons looked ridiculous in 1969, he's beginning to seem less so the more repressive the political situation becomes.

Secondly, Prof. Kerner is concerned that my "presentation is overly simplified" and "a vast array of phenomena is included in a relatively brief space", which "leads the reader to question how logically interrelated the material really is". This methodological-stylistic problem faced me from the beginning and was never satisfactorily resolved. When trying to say something "new" about social change - a subject which, as I note in the first paragraphs, can be practically equated with sociology - the emphasis must shift from tight, logical, puncture-proof arguments, to highly generalized formulations which support the thesis. The most trying section to compose was the historical chapter. The nature of historiography itself, the very bulk of data available, and the limits of time and talent on my part made a seemingly haphazard selection inevitable. Before doing the thesis, I studied some major works in the philosophy of inquiry, and came to the conclusion that my "argument" was not destined to be a debater's dream. Everything that went into the thesis seemed absolutely necessary to its author and

the many items and paths of thought omitted simply "seemed" less crucial. Obviously, I have no defense, if one is required, for the fact that it is a sprawling effort. It was a sprawling subject.

More personally disturbing, Prof. Kerner objects to my "categorical casting aside of numerous works with often glib comments as opposed to sound logical justifications". I frankly do not find in the thesis the source of this remark. I do lambast, for various reasons, Popper, Hirsch, Martindale, Boskoff (the latter three for negative reviews of LaPiere's book), but I also praise and exploit dozens of other authors throughout the thesis for their learning, sociologically and historically. Also "sound, logical justifications" are not always necessary or sufficient reasons for criticizing an author. Some works fall short of serious consideration long before the level of logic: they simply are not well conceived or executed.

The most important and serious of Prof. Kerner's notes is this: "there must be something going on out there that has been documented in a more empirical manner. Any evidence to that effect would only confirm your thesis". My approach to this issue has been two-fold: first, an examination of journals and books in the hope of finding good information about innovation as a process of cultural change, and second, a philosophical-methodological inquiry as to the likelihood of finding good supporting analyses. As for the first, the amount and quality of material is amazingly sparse

and poor. Most innovation studied has to do with modernization processes and other economic topics (see, e.g., Fritz Redlich, "Innovation in Business" and "The Role of Innovation in a Quasi-Static World", both reprinted in Steeped in Two Cultures (Harper Torchbooks, 1971) and Galbraith, Economics and the Public Purpose, pp. 146-54.), which though interesting, do not usually have much application to the thesis. Also, I felt in some sense justified in not seeking out every possible fragment of data since both LaPiere and Barnett surveyed the field with care, and my work depends on theirs. As for the logic of the situation, it is completely unsurprising that the processes of innovational change as I project them have not been studied, first because the study of individual behavior is extraordinarily difficult and always post facto in the case of the true innovator, and secondly, because this form of post-modern behavior is so new - in its current manifestation - that it has had insufficient time to enter the reluctantly accepting arms of academe.

Prof. Kerner was also bothered by my "weak" economic analysis, appropriately I believe. As a partial remedy I have studied Galbraith more carefully, and am now of the belief that the footnote on page 144 of the thesis is more accurate as economics than the relatively conservative arguments incorporated into the text. But for lack of time, an entire rewriting would be performed on the section. However, in keeping with my essentially Marxist orientation to the problem, I still feel that Galbraith's plan for

reform as expressed in his latest writing, is marvelous to contemplate and unlikely to be implemented. The relations of power and financial strength are such that the individual innovator may be able to feather his own nest (see Harry Browne on this issue, any of his books), but an essential alteration of economic arrangements will remain problematic. Remember that when a recent president sought to make Galbraith a ranking government economic advisor, Wall Street erupted in fury and made it plain that his "iconoclastic" view of economic reality was completely indigestible.

Connected with this, Prof. Kerner recognizes that "there are all kinds of international economic issues that could be raised", which I assiduously did not raise due to their horrible complexity and also since the thesis was designed to explain change in the most advanced sectors of the world only.

I hope these responses have clarified somewhat the muddier sections Prof. Kerner was good enough to elucidate for me.

NOTES

1. There has existed for nearly two decades within the discipline a publicized debate between the remnants of the old functionalists versus conflict theorists and the newer "radical caucus" adherents (not to mention the even more recent "radical functionalists"). Their basic disagreements stem from major methodological differences, resulting of course in equally major substantive battles. The old problems of objectivity, social causation, values and the researcher, etc. have come to the fore in books like Vidich and Stein's Sociology on Trial, Friedrichs' Sociology of Sociology, Reynolds and Reynolds' Sociology of Sociology and Myrdal's Objectivity in Social Research, among dozens of others. It seems at times that American sociologists, perhaps due to pressure from French, British and German colleagues, publish as many self-conscious, self-critical articles and books as original research or theory pieces. While there may be great utility in professional "soul-searching", my attention to the debate remains small, for another thesis or two on the subject itself would be entirely possible. However, within my scope there immediately arise the same problems with which these authors concern themselves, well-summarized for example in Norman Birnbaum's Toward a Critical Sociology. I have taken cognizance of our self-appraising colleagues and wish this to be

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understood: I write with the knowledge that my methodology is anything but airtight vis a vis questions of history and sociology, valuation and research, the efficacy of grand theory, that is, theory treating great spans of time and huge groupings of variables in "untangleable plethoras". But since much valuable research of this type has been done in the past, I write also with the conviction that protracted theorizing, if based on a careful reading of history with sociological insight, is not only advisable but indispensable to a discipline overloaded with unrelated print-outs.

At one point in my research, I compiled a bibliography and made notes toward a methodological statement (akin to those in all of Myrdal's works) the focus of which centered around historical/social causation and its "detection", value processes versus structural manifestations regarding the phenomenon of social change, the value-free motif in current research, and key epistemological problems growing out of the Kant-Marx-Mannheim-Habermas tradition and its many offshoots. But after toying with the problems of authoritative writing in this uncharted field, I carefully retreated, not with the admission that I was thereby forced to produce less sound research, but simply to get on with the actual project at hand. I began to feel like an airplane at the end of the runway, revving the engines for six hours preparing for a ten minute flight! Thus, all the fascinating, sometimes momentous, often trivial ramifications of the sociology of

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sociology will have to wait for proper attention. (See brief bibliography of works consulted at the end of this note.)

I have relied on several major theoretical texts in this study of social and cultural change, those of LaPiere, Barnett, Toffler and to a less noticeable degree, Brecht and Etzioni (see general bibliography). These books were selected from a large field due to their intensity, originality and usefulness to my method. Within social psychology I have used to some advantage Mills and Gerth's classic for like reasons. Of even more use however has been the synthetic, far-reaching thought of Marion Vanfossen whose probing into the future of post-modern culture is as sophisticated and original as any being done by social scientists at this time.

Partial bibliography of the sociology of sociology:

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2. Gouldner, op. cit., p. 29 ff.
 3. Blackburn, op. cit., passim.
 4. Op. cit., pp. ix - 48.
 5. Ibid., p. xii.
 6. Karl Popper, The Open Society and Its Enemies (Princeton: Princeton U. Press, 1962), 5th edition, revised, pp. 3 - 48.
 7. Richard P. Appelbaum, Theories of Social Change (Chicago: Markham Publishing Co., 1970).
 8. Pitirim A. Sorokin, (N.Y.: Dover Publications, 1963), originally entitled Social Philosophies of an Age of Crisis (1950).
 9. New York: McGraw-Hill, Inc., 1965.
 10. Homer G. Barnett, Innovation: The Basis of Cultural Change (N.Y.: McGraw-Hill Books Co., 1953).
 11. LaPiere, Social Change, p. vi. (Hereafter, SC.)
 12. Relying heavily in research of an extended nature upon a single major work would seem at best optimistic, at

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worst disastrous, depending of course upon the quality of the source and the use made of it. Therefore, it seems advisable to defend such a technique.

The study of social change has been intermittently popular with professionals for decades, and theories of change at both micro and macro levels abound. I have examined many of these theories, either directly or through comprehensive studies. After having discerned in most of them debilitating weaknesses which render them only somewhat useful, it was with great delight and relief that LaPiere's work came to my attention. Characterized in social-psychological terms, he seems to be an old wizened liberal whose desire for heightened human freedom pervades his work, yet who, through many years of study has found that standard change theories come up short for reasons he makes clear. However, in a book of 542 pages, it is not possible to offer lengthy refutations of other theoretical positions simply for the glorification of one's own thoughts. It is more important to delineate carefully and document as much as possible the theory being offered. This LaPiere has done, and this is what I shall seek to do in the following.

Out of academic curiosity I studied reviews of Social Change in the major journals. Don Martindale (AJS, 71: 203-4, Sept. '65), Walter Hirsch (Social Forces, xliv, #1: 136-7, Aug. '65) and Alvin Boskoff (ASR, XXX: 639-40, Aug. '65) all made slightly differing but equally imperceptive, inade-

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quate and imprecise attacks on the book. It is amazing and depressing that particularly Martindale and Hirsch, two important men in the discipline, should so unskillfully handle LaPiere's achievement. After reading very carefully their reviews, it would be easy to construct a point by point contradictory statement, but aside from psychological vindication of LaPiere such an exercise would serve no purpose and would consume valuable hours. Their attacks, finally, for this reader are unworthy of the book.

By way of sample illustration, both Martindale and Hirsch extract one line from LaPiere's Preface ("The theory upon which the following is based, and which for reasons of personal preference is kept more implicit than explicit, constitutes a sharp break with the traditional theories that...") - although both reviewers succeed in misquoting him or out-of-contexting the line beyond recognition - neither writer understood the point of the sentence. LaPiere wanted simply to acknowledge his use of Barnett's theory, but was not going to footnote every instance. Both reviewers seized upon the sentence, claiming for it meanings which were clearly not intended. It seems neither gave their reading the same dedication and care LaPiere gave his writing, which upon reflection is not surprising.

Also of interest is LaPiere's treatment of both Martindale and Hirsch in an extended footnote to page 34 of Social Change in which he writes: "Don Martindale's Social

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Life and Cultural Change (1962) is only an elaboration of certain aspects of Max Weber's theory of the role of religion in the determination of social stability and change...". Regarding Hirsch, "Of the hundreds of books that were published over the past decade by American sociologists about sociological matters, only nine are specifically on social change. Of these, two (...Explorations in Social Change, 1964, eds. Walter Hirsch and George Zollschan) are collections of discrete essays and articles". Martindale wrote the introduction to Hirsch's book so we realize that the negative reviews were another example of sociology-fraternity black-balling.

Hirsch's reader, Explorations..., in spite of its 800 pages is predominantly pompous and protracted mediocrity. The few articles which rise above the trivial or bombastic concern such old standards as Sorokin's work. Sorokin's piece in the book is by far the most readable and sociologically sensitive. That the antiseptic Purdue "thinkers" would eschew LaPiere's vision of reality does not come as a shock.

Finally, Zollschan wrote an article on Freud's "reality principle", and the book in toto is supposedly constructed around that marvelously chic term, "the dialectic". Naturally, LaPiere regards Freud as the world's worst sociologist, and he frowns on elevating a simple cognitive tool to the position of "school"-ishness.

Boskoff's review is only slightly less inadequate in

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this realm of proto-scholarship. He attacks LaPiere's polemics as "out of place" i.e. in poor taste, etc., without discussing the possible validity of the author's arguments. The book for him is a "disquieting mixture" of elements, "not text nor reference nor tract". By God, sounds like an innovation!

13. SC, p. 1.

14. SC, p. 2.

15. See Frank E. Manuel, The Prophets of Paris (Cambridge: Harvard U. Press, 1962), pp. 53 - 102.

16. See J.B. Bury, The Idea of Progress (N.Y.: Dover Publications, Inc., 1955).

17. Sylvia Thrupp, ed., Change in Medieval Society (N.Y.: Appleton-Century-Crofts, 1964). Also, for a sensitive and informed statement about the middle ages, regarding the growth of freedom, see Herbert J. Muller, Freedom in the Western World (N.Y.: Harper and Row Publishers, 1963), pp. 47-51.

18. SC, p. 4.

19. Harry W. Laidler, The History of Socialism (N.Y.: Thomas Y. Crowell, 1968), pp. 184-222.

20. Arnold Green's useful distinction between "segmented" and "inclusive" social movements is used here. See his Sociology (N.Y.: McGraw-Hill Book Co., 1952), Chpt. 26.

21. SC, pp. 14-15.

22. SC, p. 29.

23. SC, pp. 35n-36n.

24. SC, p. 37.

25. SC, p. 38n.

26. SC, pp. 38-9.

27. SC, pp. 40-58.

28. SC, pp. 59-64.

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29. SC, p. 41.

30. Harry E. Barnes, A History of Historical Writing (N.Y.: Dover Publications, Inc., 1962), pp. 245-49.

31. Eugene D. Genovese, The Political Economy of Slavery (N.Y.: Random House, 1967).

32. Some of the most useful books in this area are:
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Fischer, David. Historians' Fallacies. N.Y.: Harper and Row,
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Press, 1957.

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and Row, 1969.

Zinn, Howard. The Politics of History. Boston: Beacon Press,
1971.

There are bits and pieces of insightful material throughout the well-known "Philosophy of Science" literature, notably Karl Popper's The Logic of Scientific Discovery.

33. SC, p. 50.

34. SC, p. 57.

35. e.g. Freedom, Power and Democratic Planning and Man and Society in an Age of Reconstruction.

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36. Robert A. Dahl and Charles E. Lindblom, Politics, Economics and Welfare, Torchbook (N.Y.: Harper and Row, 1963).

37. John Friedmann, Retracking America: A Theory of Transactive Planning, Anchor Books (N.Y.: Doubleday, 1973).

38. SC, p. 69.

39. SC, pp. 70-102.

40. SC, p. 99.

41. Edward P. Cheyney, The Dawn of a New Era 1250-1453, Vol. 1 of The Rise of Modern Europe Series, ed. by William I. Langer (to date 18 volumes; New York: Harper and Row, 1936-present), pp. 37-63.

42. Barnett, op. cit., pp. 3, 9.

43. Ibid., p. 97.

44. SC, p. 103.

45. SC, p. 107.

46. SC, p. 108.

47. SC, p. 109.

48. SC, p. 110.

49. SC, p. 111. See John Jewkes et al, The Sources of Inventions (N.Y.: St. Martin's Press, 1958) for this portion vs. S.C. Gilfillan, The Sociology of Invention (Chicago: Follet Pub. Co., 1935) which pushes the "social forces" idea.

50. SC, p. 112.

51. SC, pp. 114-15.

52. SC, p. 116.

53. SC, p. 117.

54. SC, p. 118.

55. SC, p. 120.

56. SC, pp. 123-27.

57. SC, p. 138.

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58. SC, pp. 139-73.
59. SC, pp. 174-211.
60. Christopher Hill, Reformation to Industrial Revolution (Baltimore: Penguin Books, 1969), p. 15, cited in Harrington, Socialism (N.Y.: Bantam Books, 1973), p. 117.
61. Freedom in the Western World (N.Y.: Harper and Row, 1963), p. 30.
62. Friedrich Heer, The Medieval World (N.Y.: New American Library, 1962), p. 30; see also Sylvia Thrupp, op. cit.
63. e.g. Friedrich Heer, Norman Cantor, G.R.Coulton, Henri Pirenne, Marc Bloch, etc. See general bibliography.
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66. Muller, op. cit., p. 36.
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68. Ibid., p. 49.
69. Bryce in Muller, op. cit., p. 49.
70. Charles H. Haskins, The 12th Century Renaissance (N.Y.: World Pub. Co., 1957).
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72. op. cit., passim.
73. Ibid., p. v.
74. Journal of British Studies, X:# 2, May, 1971.
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76. George Sabine, The History of Political Thought (N.Y.: Henry Holt and Co., 1937), p. 220.
77. Muller, op. cit., p. 56.
78. Cheyney, op. cit., pp. 37-63.
79. Ibid., pp. 64-109.

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80. Ibid., pp. 110-47.
81. Frederick Engels, Peasant Wars in Germany (N.Y.: International Publishers, 1926).
82. Myron P. Gilmore, The World of Humanism 1453-1517 (N.Y.: Harper and Row, 1952), p. 46.
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84. Ibid., pp. 47-8.
85. Ibid., pp. 49-56.
86. Ibid., pp. 52-3.
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89. Ibid., pp. 460-61.
90. Ibid., p. 749.
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93. Ibid., p. 3.
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97. R.H. Tawney, Agrarian Problem of the 16th Century (London: Longman, Green and Co., 1912).
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100. Muller, op. cit., p. 173.
101. Erich Fromm, Escape from Freedom (N.Y.: Discus Books, 1965), p. xii.

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102. Muller, op. cit., p. 255.

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107. William L. Langer, Political and Social Upheaval 1832-1852 (N.Y.: Harper and Row, 1969), pp. 54-106.

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