Age and the Acceptance of Cliometrics

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Popular wisdom asserts, and life-cycle theories of human capital investment seem to imply, that older scientists are slower to accept new theories than are younger scientists. When this view is tested with evidence on the acceptance of cliometrics, however, the year-of-birth variable, although statistically significant, explains less than 10 percent of the variance in acceptance.

Paul Samuelson has claimed that an economist's response to the Keynesian revolution depended largely on the economist's age:

> The *General Theory* caught most economists under the age of thirty-five with the unexpected virulence of a disease first attacking and decimating an isolated tribe of south sea islanders. Economists beyond fifty turned out to be quite immune to the ailment. With time, most economists in-between began to run the fever, often without knowing or admitting their condition.[1]

Samuelson's view that age matters in acceptance is not unusual. Perhaps the most extreme and oft-quoted articulation of that view was given by Max Planck: ". . . a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."[2] Nearly all those who quote Planck's Principle accept it as true.[3] The usual

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[1] Paul Samuelson, "The General Theory," in Joseph Stiglitz, ed., *The Collected Papers of Paul A. Samuleson,* Vol. II (Cambridge, MA, 1966), pp. 1517–18.

[2] Max Planck, *Scientific Autobiography and Other Papers* (New York, 1949), pp. 33–34. In a less-cited passage, Planck gives a slightly different formulation to his principle: "An important scientific innovation rarely makes its way by gradually winning over and converting its opponents: it rarely happens that Saul becomes Paul. What does happen is that its opponents die out and that the growing generation is familiarized with the idea from the beginning: another instance of the fact that the future lies with youth" (*The Philosophy of Physics* [New York, 1936], p. 97).

[3] Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1962), p. 151; idem, "The Function of Measurement in Modern Physical Science," in Harry Woolf, ed., *Quantification* (Indianapolis, 1961), p. 348; Bernard Barber, "Resistance by Scientists to Scientific Discovery," rpt. in Bernard Barber and Walter Hirsch, eds., *The Sociology of Science* (New York, 1963), pp. 542–43; J. O. Wisdom, "The Nature of Normal Science," in Paul Arthur Schilpp, ed., *The Philosophy of Karl Popper*, Vol. 2 (LaSalle, IL, 1974), p. 829; Robert K. Merton and Harriet Zuckerman, "Age, Aging and Age Structure in Science," rpt. in Robert K. Merton, *The Sociology of Science* (New York, 1969), pp. 57–58; Daniel S. Greenberg, *The Politics of Pure Science* (New York, 1967), p. 45; and Gerald Holton, "The Duality and Growth of Physical Science," in *Thematic Origins of Scientific Thought* (Cambridge, MA, 1973), p. 394. The principle is also quoted and given a qualified endorsement in: Stephen G. Brush, *The Kind of Motion We Call Heat*, Book 1 in E. W. Montroll and J. L. Lebowitz, eds., *Studies in Statistical Mechanics*, Vol. 6 (New York, 1976), p. 640; Warren O. Hagstrom, *The Scientific Community* (New York, 1965), pp. 283 and 291; Sir Hermann Bondi, "What Is Progress in Science?" in Rom Harre, ed., *Problems of Scientific Revolution* (Oxford, 1975), p. 7; and G. N. Cantor, "The Edinburgh Phrenology Debate: 1803–1828," *Annals of Science*, 32 (1975), 196. The principle is mentioned non-

inference drawn from the principle is that science is an irrational enterprise, "rationality" here being understood as the adherence to objective standards in judging theories. Such, at any rate, was the inference drawn by the most noteworthy scholar to agree with Planck, Thomas Kuhn, in his controversial attack on the rationality of science.

Although the inference is *usually* drawn, I have shown elsewhere that even if older scientists do in fact accept new theories slowly, this phenomenon is compatible with all scientists evaluating theories on the basis of standards such as elegance, rigor, and explanatory scope.[4] My basic argument was that scientists can advance science either through devising new theories or through improving the current ones. With the accumulation of human capital in the current theory a scientist develops a comparative advantage in working to improve that theory. Thus older scientists would spend more time working to improve the current theory. If the speed of acceptance of a new theory depends on the extent of information about the quality of the new theory, and if extent of information depends on the amount of time spent devising new theories, then we would expect those who spend more time devising new theories (namely, the young) to accept the new theories sooner. Note that this would be true even if scientists of all ages were evaluating theories by the same objective criteria.

Whether such an argument succeeds in making Planck's Principle compatible with the rationality of science need not be settled here. In any case, the first question to be answered is: was Planck right? Until several years ago, the truth of his principle was merely assumed, but recently a few studies have begun to test it.

Perhaps the simplest of tests has been to see whether Planck's Principle is true for Planck himself. Stephen Brush, the respected historian of physics, while generally accepting Planck's Principle, adds the following ironic comment: "One of the curiosities of the story of the resurrection of atomism is that both Ostwald and Planck provide counter-examples to the Planck rule, since both of them did eventually 'see the light' and accept the statistical-molecular interpretation of thermodynamics, reversing their earlier positions."[5] Warren Hagstrom also mentions Planck as a counter-example and adds Mach as a further counter-example.[6] Independently making the same point, John Blackmore has cited Planck, Helmholtz, Ostwald, and Heisenberg as counter-examples.[7]

Finding important scientists who are counter-examples to Planck's Principle is one way to test the truth of the principle. But such counter-examples can never serve as conclusive refutation of the principle, since they leave open the possibility that the principle still holds for scientists most of the time. A better test would be to select scientists in a less ad hoc manner. The only paper so far published that meets this requirement is that written by David L. Hull, Peter D. Tessner, and myself entitled "Planck's Principle."[8] We performed

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commitally in Roger D. Rosenkrantz, *Inference, Method and Decision* (Dordrecht, Holland, 1977); and Wolfgang Stegmuller, *The Structure and Dynamics of Theories* (New York, 1976), p. 148. The principle is referred to disapprovingly in: Paul Feyerabend, "Consolations for the Specialist," in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (London, 1970), p. 203; and Israel Scheffler, *Science and Subjectivity* (Indianapolis, 1967), pp. 18–19. Most recently, the principle has received an oblique but approving mention in: A. W. Coats, "The Historical Context of the 'New' Economic History," *The Journal of European Economic History*, 9 (Spring 1980), 190–92.

[4] Arthur Mansfield Diamond, "Science as a Rational Enterprise" (Ph.D. diss., Univ. of Chicago, 1978).

[5] Brush, *The Kind of Motion*, p. 94.

[6] Hagstrom, *Scientific Community*, p. 291.

[7] John T. Blackmore, "Is Planck's 'Principle' True?" *British Journal for the Philosophy of Science*, 29 (1978), 347–49.

[8] Our paper appeared in *Science*, 202 (Nov. 17, 1978), 717–23. In an unpublished, econometrically sophisticated examination of the acceptance of the wage-push theory of inflation, David Levy of the National Planning Association reports results that disconfirm Planck's Principle ("What Was Classical Monetary Theory?" [mimeo. draft, Feb. 19, 1980], pp. 30–32). I attempted another test by using

two sorts of tests to see whether age influenced the acceptance by British scientists of evolution in the ten years following the 1859 publication of *The Origin of Species.* In the first test we found that for those who accepted evolution within ten years, the speed of acceptance was not related to age. In the second test we ran a logit regression with acceptance as the dependent variable and year of birth as the independent variable. The coefficient on year of birth was estimated to be 0.046 with a t-statistic of 2.159 and the coefficient of determination for the regression was .06.[9] Thus, while it was statistically significant in the direction predicted by Planck's Principle, age explained less than 10 percent of the variation in acceptance.

Before results for the evolution case can be accepted with much confidence, their robustness must be proved by data on age and acceptance for other cases. The acceptance of cliometrics is an appealing case for several reasons. First, it is far distant from evolution in time and subject (and importance?), and so might be expected to reveal any differences over time or subject matter in the significance of age. Another non-negligible advantage of this case is that a consensus seems to exist on what a cliometrician is. This contrasts, for instance, with the otherwise attractive Keynesian case where controversy still rages.[10] A final favorable factor is that biographical data for all ages of acceptors and rejectors is readily available in the Economic History Association's directory of members.[11] Contrast this with the evolution case, where young rejectors were probably underrepresented because their lesser eventual fame decreased the likelihood of learning their year of birth.

The most important potential disadvantage of the cliometrics case is that economic history is seldom thought of as a science. Yet this disadvantage is more apparent than real since a strong case can be made that the aim of economic history is the same as that of science: to provide a theoretical explanation of phenomena. McCloskey so argues when he claims that what distinguishes the cliometrician from the non-cliometrician is not the use of theoretical terms, but rather the competent use of those terms in the service of coherent, testable explanations. The non-cliometrician "is master of Ersatz Economics," while the cliometrician is master of genuine economics.[12]

The sample I used to test Planck's Principle for the cliometrics case consisted of the first 117 North Americans listed in the Economic History Association's membership directory for whom year of birth and year of Ph.D. information was available.[13] Those on the list were considered to have accepted cliometrics if their names appeared on Donald

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marginal utility theory, drawing my sample from a list of acceptors of marginal utility compiled by George Stigler, "The Adoption of the Marginal Utility Theory" (in *The Marginal Revolution in Economics,* ed. R. D. Collison Black, et al. [Durham, NC, 1973], p. 312). The test was of questionable value, however, because of difficulty in pinpointing the year of acceptance, small sample size, and an inappropriate age range (from 4 to 34) for acceptors at the time of the invention of marginal utility (1871). See Diamond, "Science as a Rational Enterprise," pp. 124–29.

[9] The pseudo coefficient of determination is the logit equivalent of the ordinary-least-squares coefficient of determination (R-square). The method for calculating the pseudo R^2 used here is described in Leo A. Goodman, "A Modified Multiple Regression Approach to the Analysis of Dichotomous Variables," *American Sociological Review,* 37 (Feb. 1972), 42–44; and Allan L. McCutcheon, "The Centrality of Corporate and Competitive Class Identification" (M.A. thesis, Univ. of Chicago, 1977), pp. 36–38.

[10] See e.g., Axel Leijonhufvud, *On Keynesian Economics and the Economics of Keynes* (London, 1968), and Joan Robinson, "What Has Become of the Keynesian Revolution?" in Milo Keynes, ed., *Essays on John Maynard Keynes* (London, 1975), pp. 123–31.

[11] Rondo Cameron and Louis Galambos, eds., *Handbook of the Economic History Association and Directory of Members, 1977,* special issue of this JOURNAL, 37 (Summer 1977).

[12] Donald N. McCloskey, "The Achievements of the Cliometric School," this JOURNAL, 38 (March 1978), 15.

[13] There was no special reason for stopping with the 117th person. A list of the sample, including birth year, Ph.D. year, and classification as cliometrician or non-cliometrician, is available upon request from the author.

TABLE 1
PROBIT REGRESSIONS TESTING IMPACT OF YEAR OF BIRTH AND YEAR OF PH.D. ON ACCEPTANCE OF CLIOMETRICS

	Regression Number	
	1	*2*
Year of Birth[a]	.024	—
	(2.155)	
Year of Ph.D.[a]	—	.021
		(1.803)
Constant[a]	−45.886	−42.093
	(−2.175)	(−1.821)
Number in Sample	117	117
Pseudo R^2	.07	.05

[a] t-statistics are reported in parentheses.

McCloskey's mailing list for a 1978 Cliometrics Conference held at the University of Chicago. It had been McCloskey's intention to send notice of the conference to every academic in the country whom he considered to be a cliometrician. Everyone (including McCloskey) is apt to agree that the mailing list was not a foolproof filter for separating the cliometricians from the non-cliometricians. But what is important for our purposes is that the errors of identification were few and that they were not systematically related to age.

The results of the cliometrics test of Planck's Principle are reported in Table 1. In the first regression the acceptance and rejection dummy variable is regressed on year of birth. In the second it is regressed on year of receipt of Ph.D. This latter regression would be more appropriate if the reason that the old are hypothesized to resist new theories is a supposed reluctance to retool technical skills. The coefficients on year of birth and year of Ph.D. are both positive, but only the former is significant for a two-tailed test. The results thus suggest that age does indeed influence acceptance; however, as with the evolution samples, the pseudo R-squares are very low, indicating that age plays a relatively minor role in explaining why some reject and others accept cliometrics.

Common wisdom and the sophisticated economic theory of life-cycle human capital investment predict that the old will be slow to change. Both wisdom and theory are weakly confirmed by the evidence that the year-of-birth variable is of the expected sign and is statistically significant. But such a confirmation does not justify the extreme view expressed in Planck's Principle. Age matters, but it does not matter much.