Journal of Economic Perspectives—Volume 8, Number 1—Winter 1994—Pages 165–179

How Are the Mighty Fallen: Rejected Classic Articles by Leading Economists

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Do elite economists suffer publication setbacks? Are the economists who produce the important articles content with the refereeing process? We asked over 140 leading economists, including all living winners of the Nobel Prize and John Bates Clark Medal, to describe instances in which journals rejected their papers. We hit a nerve. More than 60 percent responded, many with several blistering pages.[1] Paul Krugman expressed the tone of many letters: "Thanks for the opportunity to let off a bit of steam."[2]

A few economists indicate that no journal has rejected their work. Most of these authors publish mainly in books, and submit few papers to journals. John Kenneth Galbraith explains that his unblemished record "is not entirely the result of the excellence of my writing, much as I would like to believe it so. The deeper truth is that not for many years now have I submitted more than a very few papers to our, as they are called, learned journals. Consequently there has not been a great deal to reject." Robert Solow's experience is similar: "The fact is that I have never had a paper rejected by a journal. Probably this is because I *hate* writing articles."

In contrast, almost all leading economists who regularly submit to journals have suffered rejection, often frequently. In the big leagues, even the best hitters regularly strike out. For example, Paul Samuelson states: "Yes, journals have rejected papers of mine, some of them later regarded as 'classics.' I used

[1]A forthcoming book—*Rejected: Leading Economists Ponder the Publication Process*—presents all of the responses in full, with additional commentary by leading journal editors and further analysis and publication guidance (see Shepherd, forthcoming 1994).
[2]One response expressed incomplete enthusiasm: "I consider your project to be basically derisive, and not worth my attention."

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to say, with only moderate exaggeration, that the quality of papers of mine at first rejected is not less than the quality of papers accepted at once." Our survey demonstrates that many papers that have become classics were rejected initially by at least one journal—and often by more than one. A publisher rejected George Orwell's *Animal Farm* because "[i]t's impossible to sell animal stories in the U.S.A." (Bernard, 1990). Similarly, economics journals can overlook excellence.

This paper presents a selection of dispatches from the publication battlefront. We begin by discussing rejections that winners of the Nobel Prize and John Bates Clark Medal have endured, and some other notable cases. We then turn to the record of John Maynard Keynes' quirky refusals, when he was the *Economics Journal*'s editor, of several important articles and authors. Finally, we offer some thoughts about the implications of these findings.[3]

The Grim Reaper Knocks On All Doors: Nobel Laureates and Clark Medalists

Most winners of the Nobel Prize and John Bates Clark Medal have had papers rejected: only three of the 20 winners who responded in our survey did not admit at least one rejection. The spurned stars were diverse: conservatives, mathematical economists, non-mathematical progressives, Keynesians, monetarists, neo-classicists, young, old, authors of papers on a broad range of subjects.

James Tobin remembers vividly the rejection of a paper that he prepared as the inaugural Cowles Foundation Discussion Paper (CFDP), after the Foundation moved to Yale. "It was a great coup for Yale University and its Economics Department when in 1955 the Cowles Commission moved from Chicago to Yale. It was for me too, because I became the Director of this world-renowned research group. ... I had by fortunate chance a paper all ready to be CFDP 1. What could be better than to have the first paper distributed in Cowles's new life authored by the new director, recruited from the Yale faculty, not imported from Chicago." Tobin's paper for the first time extended probit (0, 1) regression analysis to applications with multiple regressors.

Tobin submitted the paper to the *Journal of the American Statistical Association*. The journal rejected it, twice. "The referees for the *Journal of the American Statistical Association* were not impressed, not even after I re-submitted with many of their specific complaints treated." The paper died until Tobin's 1975 volume of collected essays resurrected it.[4]

[3]Unless we indicate otherwise, all of the rejections that we report were unconditional, with no leave to revise and resubmit.

[4]Table 1 presents the eventual citations for many of the papers that we discuss.

Table 1
Some Rejected Papers, Listed by Place of Eventual Publication

Akerlof, George, "The Market for 'Lemons': Quality, Uncertainty and the Market Mechanism," *Quarterly Journal of Economics,* August 1970, *84*:3, 488–500.

Arthur, W. Brian, "Competing Technologies, Increasing Returns, and Lock-In by Historical Events," *Economic Journal,* March 1989, *99,* 116–31.

Becker, Gary S., "Competition and Democracy," *Journal of Law and Economics,* 1958, *1,* 105–09.

Becker, Gary S., "A Theory of the Allocation of Time," *Economic Journal,* September 1965, *75,* 493–517.

Bhagwati, Jagdish, "Immiserizing Growth: A Geometrical Note," *Review of Economic Studies,* June 1958, *25,* 201–05.

Black, Fisher, and Myron Scholes, "The Pricing of Options and Corporate Liabilities," *Journal of Political Economy,* May/June 1973, *81*:3, 637–54.

Buchanan, James M., "External and Internal Public Debt," *American Economic Review,* December 1957, *47*:6, 995–1000.

Chichilnisky, Graciela, "Basic Goods, Commodity Transfers and the New International Economic Order," *Journal of Development Economics,* December 1980, 7:4, 505–19.

Corden, W. Max, "The Structure of a Tariff System and the Effective Protective Rate," *Journal of Political Economy,* June 1966, *74*:3, 221–37.

Debreu, Gerard, "Numerical Representations of Technological Change," *Metroeconomica,* August 1954, *6*:2, 46–68.

Fisher, Franklin M., Zvi Griliches, and Carl Kaysen, "The Costs of Automobile Model Changes Since 1949," *Journal of Political Economy,* October 1962, *70*:5, 433–51.

Friedman, Milton, "Professor Pigou's Method for Measuring Elasticities of Demand from Budgetary Data," *Quarterly Journal of Economics,* November 1935, *50*:1, 151–63.

Harrod, Roy, "The Law of Decreasing Costs," *Economic Journal,* December 1931, *41,* 566–76.

Hotelling, Harold, "The Economics of Exhaustible Resources," *Journal of Political Economy,* April 1931, *39*:2, 137–75.

Jonung, Lars, "Ricardo on Machinery and the Present Unemployment: An Unpublished Manuscript by Knut Wicksell," *Econnomic Journal,* March 1981, *91*:361, 195–205.

Kalecki, Michal, "A Theorem on Technical Progress," *Review of Economic Studies,* May 1941, 7:1, 178–84.

Krugman, Paul R., "Increasing Returns, Monopolistic Competition, and International Trade," *Journal of International Economics,* November 1979, *9*:4, 469–79.

Krugman, Paul R., "Target Zones and Exchange Rate Dynamics," *Quarterly Journal of Economics,* August 1991, *106*:3, 669–82.

Lazear, Edward P., and Sherwin Rosen, "Rank-Order Tournaments as Optimal Labor Contracts," *Journal of Political Economy,* October 1981, *89*:5, 841–64.

Lucas, Robert E., "Expectations and the Neutrality of Money," *Journal of Economic Theory,* April 1972, *4*:2, 103–24.

May, Robert, and John Beddington, "Nonlinear Difference Equations: Stable Points, Stable Cycles, Chaos," mimeo, Princeton University, Department of Biology, 1975.

May, Robert, "Simple Mathematical Models with Very Complicated Dynamics," *Nature,* June 10, 1976, *261,* 459–67.

Modigliani, Franco, "Fluctuations in the Savings-Income Ratio: A Problem in Economic Forecasting," *Studies of Income and Wealth,* Vol. 11. York: National Bureau of Economic Research, 1949, 371–440.

Ohlin, Bertil, *Interregional and International Trade.* Cambridge: Harvard University Press, 1933, Chapters 1–3, Appendix I.

Scitovsky, Tibor, "A Reconsideration of the Theory of Tariffs," *Review of Economic Studies,* 1942, *9*:2, 89–110.

Sharpe, William, "Capital Asset Prices: A Theory of Equilibrium Under Conditions of Risk," *Journal of Finance,* 1964, *19*:3, 425–42.

Stolper, Wolfgang, and Samuelson, Paul A., Protection and Real Wages," *Review of Economic Studies,* November 1941, *9,* 58–73.

Tobin, James, "Multiple Probit Regression of Dichotomous Variables," *Collected Essays of James Tobin,* Vol. 2. Chicago: Markham, 1975, Chapter 43.

Tobin recalls that "[t]he rejection, anticlimactic as it happened to be, was disappointing. But all was not lost." Tobin soon developed a theory for handling $(0, x)$ variables with any number of regressors. "This analysis was baptized 'Tobit' by Arthur Goldberger. The name obviously echoed 'probit.' Maybe Goldberger was also evoking my chief claim to fame in those days, my one-paragraph appearance, thinly disguised as Tobit, in *The Caine Mutiny*, the popular novel by my 1942 naval reserve classmate Herman Wouk."

Last autumn marked the 50th birthday of Wolfgang Stolper's and Paul Samuelson's "Protection and Real Wages." The article addressed the impact of tariffs on the distribution of income, and introduced general-equilibrium models as analytic tools. The authors submitted it to the *American Economic Review*, which rejected it bluntly. According to Samuelson, the referee "thought it would prejudice the noble cause of free trade; and, besides, it was primarily a theoretical curiosum." The *Review of Economic Studies* later published the paper.[5]

Econometrica rejected what Franco Modigliani describes as "one of the best known and widely cited of my early papers": his paper that introduced the Duesenberry-Modigliani consumption function. He explains: "In 1948–49 I had been working on a paper developing a theory of aggregate saving behavior which has since been known as the Duesenberry-Modigliani consumption function. ... I presented my paper at a 1949 Conference on Income and Wealth, and then submitted it for publication to *Econometrica*. The paper was returned with a letter from Trygve Haavelmo, who I believe was the Editor of *Econometrica*, rejecting my paper with no offer to revise and resubmit. As I recall, the only reason for rejecting the paper was that in his view these were no times for formulating ingenious new hypotheses, the important issue of the time being to pursue better estimation methods recognizing problems of simultaneity. By contrast, my paper used single equation methods." The paper later appeared in *Studies of Income and Wealth*.

In 1962, William Sharpe submitted his paper, "Capital Asset Prices: A Theory of Equilibrium Under Conditions of Risk," to the *Journal of Finance*. The paper, which was to have over 2,000 citations, introduced the capital asset pricing model. The *Journal* editor, on the advice of a referee, rejected the paper, although Sharpe remembers the rejection as "equivocal." The editor indicated that Sharpe's assumption that all investors made the same predictions was so "preposterous" that it made his conclusions "uninteresting" (Bernstein, 1992, pp. 194–95). Sharpe kept trying with the *Journal*, and succeeded only

[5]Important articles in the international economics field have been rejected with regularity. For example, Samuelson had difficulty publishing his paper that first exposed the transfer problem in trade theory. After Samuelson's results themselves became conventional, the *American Economic Review* rejected Graciela Chichilnisky's paper that both generalized and contradicted Samuelson's results. The *Journal of Development Economics* published Chichilnisky's paper instead, and later devoted a complete issue to the paper's ideas.

after new editors arrived. "[T]he editorship was in the process of being changed. Eventually other referees were brought in and the new editor agreed to publication, which took place in 1964."[6]

Gary Becker, who sits atop several citations rankings, feels that he too has suffered the slings and arrows of outrageous referee reports. "Like most economists, I have had a number of manuscripts rejected by journals and other publishers." He singles out one example. Early in his career, he submitted what became "Competition and Democracy" to the *Journal of Political Economy*. "The then editor Earl Hamilton agreed to publish it. He eventually withdrew the commitment because of negative comments by Frank Knight, who was one of the people who refereed the paper. I still have a copy of Knight's referee report, and I cannot say that I am any more impressed by it now than at that time." Becker "became discouraged by the report and put the article away" until he finally published it in the *Journal of Law and Economics* several years later. However, by that time, other articles had been published that employed the same approach.[7]

Franklin Fisher reports: "'The costs of automobile model changes since 1949' (written with Zvi Griliches and Carl Kaysen) is probably the best-known paper in which I ever had a hand." When the authors submitted the paper to the *AER*, "The paper received an enthusiastic referee's report but was nevertheless rejected by the *AER*." The editor indicated "that the automobile-model-change paper 'was not of sufficient independent interest to warrant publication in the *American Economic Review*.'" Fisher's story ends happily. "We easily published the study in the *Journal of Political Economy*, and it has gone on to be anthologized so many times that long ago I lost count" (Monz, 1992).

Although Paul Krugman has published several influential papers, journals reject most of his work. "This is in response to your letter of April 3 requesting stories about paper rejections—if any, you say!! As it happened, your letter arrived in the same day's mail as the *second* rejection of a paper that I thought (and still think) is one of my better ones. I don't know what other peoples' experience is, but I would estimate that 60% of my papers sent to refereed journals have been rejected on the first try." Despite his publication troubles, Krugman recently received the John Bates Clark Medal.

One of Krugman's examples: "I guess the biggest rejection I have had was of my first paper on monopolistic competition and trade. I sent 'Increasing

[6]The worst publishing experience that Theodore Schultz relates is a bad book review: "a review of one of my best books that appeared in the *Economic Journal*, UK. The reviewer, late Lord Ballard, wrote the most conceivable, devastating review. The consequences were that it condemned the book so severely that the readers of the review could not believe what they had read and promptly bought the book! The sales and translations could not have been better."

[7]Becker clashed with an editor regarding his article on the allocation of time. "It was originally submitted to the *Review of Economics and Statistics*. Although the editor, Otto Eckstein, agreed to publish it, he wanted me to cut it down by a huge amount. I became miffed at this suggestion, so I then submitted it to the *Economic Journal*."

Returns, Monopolistic Competition, and International Trade' to the *QJE* sometime in mid-1978. It took eight months to get a reply: a rejection based on a single referee report, which I now wish I had saved. The referee agreed that increasing returns and imperfect competition were very important in the international economy, but did not feel that our understanding of these issues would be helped by writing down formal models."

The paper eventually appeared in the *Journal of International Economics*, but only over two *JIE* referees' objections. Jagdish Bhagwati was the *JIE*'s editor at the time: "I published it myself *despite* two adverse referee reports by very distinguished experts on the theory of increasing returns! It did take some courage and also a strong sense of the importance of the paper for me to do so, since Krugman had been my student and normally I would lean over backwards not to publish my own students' work."

Our respondents indicate that most of their articles that endure initial rejection appear later in other journals. However, like Becker, Krugman notes that, even if another journal eventually prints a paper, the delay that initial rejection causes may permit others to beat the paper into the intellectual market. Krugman sent his "Target Zones and Exchange Rate Dynamics" to the *Journal of Political Economy*. "This time I got two favorable referee reports. The paper was nonetheless rejected... by [the referee] who thought that the paper was of 'insufficient general interest' for the *JPE*. The paper didn't come out (in the *QJE*) until August 1991. By that time the target zone literature, all of which made use of the techniques first introduced in my paper, had exploded, and consisted of at least a hundred published and unpublished pieces; in fact, I had to add a postscript to the *QJE* version referring to *subsequent* literature."

Journals have declined several of James Buchanan's papers. For example, he notes that "my first piece on public debt theory, 'External and Internal Public Debt,' which was finally published in *AER*, was curtly and rudely rejected by E. H. Chamberlin at *QJE*, saying simply 'We cannot accept the article.' There was no reason, no referee report, anything. That was the shortest rejection I ever got."

Gerard Debreu has dents in his publication record. "[A]round 1951 I submitted an article entitled 'Numerical Representations of Technological Change' to the *Journal of Political Economy* which rejected it. I believe that one of the reasons, maybe the main reason, given for that rejection was that the paper was too mathematical for the *J.P.E.*, and indeed it was." *Metroeconomica* later published the paper.

Harry Markowitz has "had my share of rejections." For example, he indicates that a paper on a new database "was rejected because it presented a 'sexist language.' In particular, it referred to 'workman-like... .'"

Finally, *Econometrica* refused a Kenneth Arrow paper on inventories—although, at the time, he was President of the Econometric Society, *Econometrica*'s parent organization. He remarks diplomatically that the incident demonstrated the Society's impartial integrity.

More Remarkable Rejections

George Akerlof's seminal contribution to the economics of information, "The Market for 'Lemons': Quality, Uncertainty and the Market Mechanism," considered whether markets would exist if product quality were unobservable. Before the *Quarterly Journal of Economics* finally accepted Akerlof's paper four years after he first sought to publish it, three journals called it a lemon. "I submitted it in June, 1967 to the *American Economic Review*. I got a reply from the editor which said that the article was interesting but the *American Economic Review* did not publish such trivial stuff."

The article next went to the *Journal of Political Economy*. Again it was rejected. Although the *AER* editor had refused the article because it was trivial, the *JPE* referee's report asserted the opposite: that the paper was too general to be true. "It seemed to give a universality to my paper that was never intended. It said amongst other things that eggs came in different qualities, but they were graded and then traded. Didn't 'The Market for 'Lemons'' predict that no markets would occur at all if there were quality differences? Thus, in the view of this referee my paper predicted too much. Perhaps he forgot that the paper predicted the nonexistence of many markets which do not, in fact, exist."

Akerlof kept trying. "I next sent the article to the *Review of Economic Studies*. I had been urged by one of its co-editors to do that. Instead it went to another editor whose view of 'The Market for 'Lemons'' was decidedly less favorable. It was rejected on the grounds again that it was 'trivial.' Finally I sent it to the *QJE* which accepted it with some degree of enthusiasm."

The rejections discouraged Akerlof. "I do think its early rocky reception did have an effect on my own work. It was not until 1973, when I spent 6 months on sabbatical in England, that I realized that quite a few people had read the paper, and even liked it. I believe I would have done follow-up work on 'The Market for 'Lemons'' sooner, if I had not been made to feel lucky just to have it published at all. (I must say I still feel very lucky that it was published.)"

Akerlof believes that journal editors refused the article both because they feared the introduction into economics of informational considerations and because they disliked the article's readable style. "The editors probably objected most to two things. They were afraid that if 'information' was brought into economics, it would lose all rigor, since in that case almost anything could be said—there being so many ways that information can affect an equilibrium. They also almost surely objected to the style of the article which did not reflect the usual solemnity of economic journals."

Robert May is a distinguished biologist who has produced important and influential work on chaos theory. Encouraged by several mathematical economists, May and John Beddington submitted an economically-oriented paper on endogenous instability in simple dynamic models to *Econometrica*. The journal's editor rejected the paper with a fill-the-blanks form letter:

> Dear Mr. May,
>
> Enclosed is/are the report(s) of a/two referee(s) on your paper. I regret it is not suitable for publication in *Econometrica*.
>
> Yours sincerely,
> [signature]

According to May, the lone, two-paragraph referee report indicated that the paper's findings "were well-known and not interesting. I wrote a cross reply to the editor, who said that his reviewer was expert and who was I anyway."

May gave up on economists. "At this point, back then, I simply decided that economists were not worth bothering with (life being very busy), and that generally the ends I wish to serve outside biology would be adequately handled by the review I was then writing for *Nature*. This was the 1976 *Nature* review (which remains, I believe, the most cited paper in the field of 'chaos,' which currently is going on for 2,000 citations)."

Robert Lucas' 1972 paper, "Expectations and the Neutrality of Money," introduced rational expectations concepts into monetary theory and macroeconomics. However, the *American Economic Review* rejected the paper. The editor, writing in 1970 before the explosion in economics journals' mathematical complexity, objected to the paper's technical style. "If it has a clear result, it is hidden by the exposition." The referee concurred: "I find the paper exceedingly formal and I am not sure I fully understand the economics of the theorems Lucas presents. ... I have been following fairly closely the format of the articles published in the *AER*, and in comparison, Lucas' exposition is pitched at what I think is a distressingly arid level. The exposition is much more formal, for example, than either that of the original Samuelson paper or that of the Cass-Yaari paper—both of which took pains to get at the economic content of their theorems." Lucas eventually published the paper in the *Journal of Economic Theory*.

The *Quarterly Journal of Economics* missed its chance for "Immiserizing Growth," Jagdish Bhagwati's first professional paper. He notes, "The *QJE* turndown of the paper, influential as it became soon after publication, was perhaps due to the luck of the draw which all of us face as authors, sometimes driving us to distraction when the referees appear to be tendentious and capricious."

It appears that some authors' relationships with journal editors may have permitted the authors to avoid the risk of rejection: editors at several journals apparently permit certain authors sometimes to bypass the journals' normal refereeing processes. Richard Posner explains: "I am afraid I have no interesting anecdotes for you. I have had papers turned down, all right, but very few *economics* papers. Most of my economics papers have been published by journals edited by close friends (such as Ronald Coase and Bill Landes, when they edited the *Journal of Law and Economics*, or George Stigler and Sam Peltzman when they edited the *JPE*, or the *Bell Journal* when it was edited by Paul MacAvoy), and in many of these cases there weren't even formal submissions."

Similarly, Ronald Coase notes, "I have never found any difficulty in getting my articles published. I have either published in house journals (e.g. *Economica*) or the article was written as a result of a request (e.g. for a conference) and publication was assured."

Others suggest that an editor who exempts the papers of intellectual allies from the regular selection process may tend unfairly to reject work that disagrees with the editor's views. One economist submitted a paper on entry barriers to a Chicago journal. The rejection was "a 13-page essay citing every Chicago deity as to why there could be no such thing as entry barriers; the referee's essay made no reference to the paper at hand."

Only after years of rejections by four journals did Brian Arthur's "Competing Technologies, Increasing Returns, and Lock-In by Historical Events" appear in the *Economic Journal*. Arthur had employed a simple writing style. "I was at pains to keep the ideas in the forefront and not buried under a lot of theorems and pseudo-mathematical verbiage. I greatly admired Akerlof's Lemons paper as a piece of exposition and decided to write the paper in a similar, accessible, informative style. Given the current economics editorial process, this proved to be disastrous."

The paper began a six-year odyssey. "First it was dismissed at *AER* in desultory fashion. Then I submitted it to *QJE*, and it was turned down there. Then because Clower had left *AER* I resubmitted it to *AER*. It underwent one refereeing go-round, followed by two appeals. Finally, two years after this second submission, *AER* turned it down again. ... I then submitted it to *EJ*; and it got turned down. I appealed; and finally, in 1989, *EJ* published it. ... The problem was consistently that the ideas were 'already known' somehow, not formulated in a sophisticated format, as an i-o game problem, or the discussion was too 'chatty' and therefore naive. I put the paper through eight rewrites in this process; each time it became stiffer, more formal, less informative, and possibly as a result more publishable."

Like other authors, Arthur suggests that delay from the rejections threatened his ideas' currency. "Because papers based on mine had started to appear in the literature," referees told him that "the idea... is already recognized in the literature."[8]

Two journals rejected the paper by Fischer Black and Myron Scholes that contained their widely-used option-pricing formula. They first sent what would become "The Pricing of Options and Corporate Liabilities" to the *Journal of Political Economy*. The editor rejected it, without even sending it to referees. Too

[8]Waldrop (1992) provides a more complete story of Arthur's difficulties.

much finance, too little economics. They then tried the *Review of Economics and Statistics*. Again they received a rejection without even a referee report. The *JPE* published the piece only after Chicagoans Eugene Fama and Merton Miller spoke with the *JPE*'s Chicago editor. Because of the delay, the *Journal of Finance* printed Black's and Scholes' empirical tests of their formula before the *JPE* printed the formula itself (Bernstein, 1992, pp. 220–221 supplies this anecdote).

"I have, of course, had articles turned down," says Oliver Williamson. Brookings rejected his book *Markets and Hierarchies: Analysis and Antitrust Implications*. "The referees were of orthodox persuasions and did not see much merit in the exercise. The approach, the author, or both were believed to be so beyond redemption that no revision was invited. ... The Free Press later published it, in 1975. The 1990 citations exceed those to *The General Theory* and to the *Wealth of Nations*, though not those to Marx (*Capital*)."

James March takes his lumps in good humor. "I have certainly had articles rejected, even on occasion for good reasons. ... I recall on one occasion a referee filing a two paragraph commentary on a paper I co-authored suggesting (in the first paragraph) that the key theorem involved was trivially obvious and (in the second) that it was wrong. I thought on the whole that he ought to choose."[9]

The Visible Hand of John Maynard Keynes

Through much of the first half of this century, John Maynard Keynes edited the *Economic Journal*, the period's premier economics publication. Our respondents provided a striking number of comments about Keynes as editor. Kenneth Boulding submitted his first article to Keynes, "and received a delightful conditional acceptance with some very valuable suggestions for improving the article, which I followed." Keynes then published Boulding's revised paper. However, other encounters with Keynes—who was often advised by his student Frank Ramsey—produced less delight.[10]

In 1923, Bertil Ohlin submitted to the *Economic Journal* a paper that introduced the factor proportions theorem in international economics. The theorem eventually earned Ohlin a Nobel Prize. Keynes returned the

[9]Gordon Tullock refuses to permit rejection to discourage him. He is preparing a book made up solely of his papers that journals have refused. We hope that he finds a publisher.

[10]Jan Tinbergen describes Keynes' confidence. "In 1946 I had the privilege to meet personally John Maynard Keynes. I informed him that I had estimated the price elasticity of the demand for export goods of a number of countries and found figures around −2, the figure he had used intuitively in his famous 'The Economic Consequences of the Peace' (1920). I thought he would be happy that his intuition had been 'proved to be correct'; typically an econometrician's attitude. His reaction was different: 'how pleasant for you to have found the correct figure.' For him his intuition was the truth, rather than results of econometrics. He may have been right! This may have been a lesson for me."

manuscript with a blunt rejection note: "This amounts to nothing and should be refused, J.M.K." Ohlin explained, "Probably by mistake [the note] was included in the package, when I got my manuscript back. I still have the note, and regard it as a valuable document. The paper Keynes rejected was never published" (Patinkin and Lieth, 1977, pp. 161–2).

Similarly, Keynes rejected Harold Hotelling's "The Economics of Exhaustible Resources." The paper stated what is now known as Hotelling's Law: that the price of an exhaustible resource rises with the interest rate. Hotelling proved the result using the calculus of variations. However, the *Economic Journal* had earlier published Frank Ramsey's "The Mathematics of Saving," which also used calculus of variations.

Although the two papers addressed different topics, Keynes rejected Hotelling's piece on the basis that the calculus of variations technique was overly complex, and, in any event, the *Economic Journal* had already published Ramsey's article that used the same technique. Kenneth Arrow recalls: "When I spoke to Hotelling along these lines, he gravely informed me that he had originally submitted the paper to the *Economic Journal*. Although it had, he said, some motto which implied that it was open to economic analysis of all viewpoints, the paper was rejected as being too difficult for its readers. It was then published by the *Journal of Political Economy*, which was certainly not noted as an organ for mathematical economics."

Keynes rejected Roy Harrod's article that first sketched the marginal revenue curve. Although the *Economic Journal* finally published the article years later, Harrod felt that the delay in publication cost him credit for the new concept. Harrod writes in his biography of Keynes (1951, p. 159n) that he was "injured by Keynes' zeal": "During 1928 I submitted a short article, setting out what I called the 'increment of aggregate demand curve.' Keynes showed this to F. P. Ramsey who raised objections. Being in poor health at the time, and heavily burdened with college duties, I was discouraged and put the article away in a drawer for eighteen months. I then took the matter up with Ramsey, who was an old friend, and he recanted. The article was re-submitted and appeared in June 1930. . . . [I]f Keynes had not listened so readily to Ramsey's criticisms and the article had appeared in 1928, any claim to have 'invented' this well-known tool in economics would be without challenge."

Paul Samuelson remembers, "Roy Harrod went to his grave bitter because Maynard Keynes, absolute monarch at the *Economic Journal*, turned down his early breakthroughs in the economics of imperfect competition. Thus, Harrod was robbed of credit for the 'marginal revenue' nomenclature. All this was on the advice of Frank Ramsey, genius in logic and mathematics. To genius every new idea is indeed 'obvious' and besides all that was already in 1838 Cournot. Hard cheese for Harrod, or for any of us, if the trace of our new brainchild can be found in 1750 Hume or 1826 von Thunen."

Keynes drew first blood with Milton Friedman. "The first professional paper that I published was entitled 'Professor Pigou's Method for Measuring

Elasticities of Demand from Budgetary Data.' It was initially submitted to the *Economic Journal*. It consisted of a criticism of some work by the famous British economist A. C. Pigou. I received a reply from John Maynard Keynes, who was then editor of the *Economic Journal*, saying that he had shown it to Professor Pigou and Professor Pigou did not believe the criticism was correct, and therefore he was not inclined to publish it."

Friedman then sent the piece on to the *QJE*. "After having it refereed—I believe by Wassily Leontief since the paper was highly mathematical—Professor Taussig accepted it and the paper was published in the November 1935 issue of the *Quarterly Journal of Economics*."

Keynes also refused to publish what became one of Tibor Scitovsky's best-known papers. "One of my earliest, most quoted and reprinted papers, 'A Reconsideration of the Theory of Tariffs,' was turned down by Keynes as unsuitable for publication in the *Economic Journal* and was published soon thereafter in the 1942 Feb. issue of the *Review of Economic Studies*. Curiously enough, Keynes' closest friend and collaborator, R. F. (later Lord) Kahn, was the first person to quote from and draw attention to the main points of that paper in a short note in the first 1947–48 issue of the same *Review*."[11]

Lessons and Implications

The refereeing process displays a Dr. Jekyll and Mr. Hyde personality. Many respondents praised the positive side: that the refereeing process guides the best work to the best journals, matches unusual papers with appropriate publications, induces improvements in the papers themselves—and preserves the reputations of famous economists by keeping their bad work unpublished. For example, Edward Lazear and Sherwin Rosen indicate that their three-year ordeal with the *Journal of Political Economy* over "Rank-Order Tournaments as Optimal Labor Contracts" was worthwhile: Lazear thanks the referee "for the pain and suffering that he put a young professor through. It was time well spent."[12] Jean Tirole notes, "One of my best papers was rejected once, but it was entirely my fault." Takashi Negishi's experience was similar. "As far as my

[11]Similarly, in 1924, Keynes rejected an article by Knut Wicksell that later was published as "Ricardo on Machinery and the Present Unemployment: An Unpublished Manuscript by Knut Wicksell," *Economic Journal*, see Jonung (1981). Keynes also refused Michal Kalecki's "A Theorem on Technical Progress." It later appeared in the *Review of Economic Studies* (1941).

[12]Similarly, Max Corden suggests that one journal's rejection of what became an important paper proved fortunate; it permitted another journal's editor to improve the paper greatly. "The article of mine which has had the biggest influence, as judged by citations, on the subsequent literature and on empirical work, is 'The Structure of a Tariff System and the Effective Protective Rate,' [which appeared in the *Journal of Political Economy* in 1966]. The first version was rejected by the *EJ*. The criticisms were technical and dealt with rather minor points, and the referee clearly did not perceive the significance of the main idea. But I then revised it and sent it to Harry Johnson for advice. He suggested I submit it to the *JPE* (of which he was the editor). He then made numerous constructive suggestions for improvements, all of which I accepted. The original *EJ* referees had done me a service in leading me to publish a far better paper."

own papers are concerned, in most cases I thought editors and referees were right for rejected papers, so that I did not try other journals." Amartya Sen agrees: "I was on the whole lucky with submissions but those that were rejected were deservedly chucked!"

However, our project also revealed much dissatisfaction with the process. Many respondents deplored bored, careless editors and referees. Roy Radner, who has "had quite a few papers rejected by journals," notes: "My casual impression is that much of the refereeing—at least for economics journals—is careless, irresponsible, and narrow-minded. This can be frustrating for both authors and editors." William Baumol concurs: "We have all had rejections that infuriated us because the reviewers always seem not to have read our work with the care and understanding that it merits." Similarly, Graciela Chichilnisky notes, "The more innovative and interesting the paper, the more likely it is to be rejected, in my experience. Editors seldom read papers, and referees don't read them carefully either." Richard Freeman describes the "relief one normally gets from a rejection: the certain knowledge that the editor and referees are blind baseball umpires, members of The Three Stooges, or incompetents in even more drastic ways."

Many respondents indicated special difficulty in obtaining fair journal evaluations of unorthodox papers. The evolving attitudes of journals toward mathematical complexity present the issue starkly. Until the 1970s, editors regularly rejected articles because they contained technical mathematics.[13] The dominant editorial orthodoxy emphasized intuition, and viewed sophisticated mathematics as arid and irrelevant. Early papers by Tinbergen, Friedman, Hotelling, Debreu, and Lucas were all rejected for excess mathematics.[14]

In the 1970s, the technical tide rolled in. Leading journals filled with theorems and equations. Articles that contained only clear ideas in clear prose began to be rejected because they contained insufficient mathematics. Examples include the Akerlof and Arthur articles.

A rejection usually does not kill a paper; among our examples, a rejected paper usually finds life at another journal, even if the paper is unorthodox. Richard Nelson explains "that while, if one is writing something that is not quite orthodox one must expect some rejections, if one keeps on searching out

[13]Indeed, even the major journals were unable to print mathematical notation. When Franco Modigliani and Merton Miller submitted their 1958 paper that set forth the Modigliani-Miller theorem to the *American Economic Review*, the editor refused to permit the paper to include $\bar{X}$; the *American Economic Review*'s type fonts contained no mathematical symbols. The authors put up a fuss and finally obtained their $\bar{X}$.

[14]Back in 1948, Charles Roos (1948, pp. 127–28) reported a case that suggested the difficulties of combining mathematics, statistics, and economics. A young economist sought to extend static economic theory into a testable dynamic structure. His paper used technical mathematics and statistics. A leading American economics journal refused to publish the paper unless he removed the mathematics and statistics. A mathematics journal would publish it only without the statistics and economic theory. A statistics journal demanded that he eliminate the mathematics and the economics.

other journals, one finally will get published in a good place. Indeed, I think that is significantly more true today than it was, say, fifteen years ago. A whole collection of new journals has opened up since that time signalling welcomes to somewhat unorthodox approaches."[15]

However, even if a paper eventually is published, delay from earlier rejections can permit competing papers to be published first, or can reduce the paper's impact. For example, the *American Economic Review*, *Review of Economics and Statistics*, and *Economic Journal* all rejected one of F. M. Scherer's papers. The journal that eventually published the piece had, at the time, only 55 United States subscribers.

Responses from several journal editors seek to hearten authors by noting that an article's rejection may constitute neither a personal rebuke nor disparagement of the article's ideas. However, the following rejection letter from a Chinese economics journal inflicts the same damage as a blunt, two-sentence refusal: "We have read your manuscript with boundless delight. If we were to publish your paper, it would be impossible for us to publish any work of lower standard. And as it is unthinkable that in the next thousand years we shall see its equal, we are, to our regret, compelled to return your divine composition, and to beg you a thousand times to overlook our short sight and timidity" (Bernard, 1990, p. 44).

The risk of rejection that even leading economists confront causes not only anxiety and anger, but also Job-like reflection. Every economist at some point ponders: "Why me?" Paul Krugman's conclusion: "The self-serving answer is that my stuff is so incredibly innovative that people don't get the point. More likely, I somehow rub referees and editors the wrong way, maybe by claiming more originality than I really have. Whatever the cause, I still open return letters from journals with fear and trembling, and more often than not get bad news. I am having a terrible time with my current work on economic geography: referees tell me that it's obvious, it's wrong, and anyway they said it years ago."

Whether rejection is gentle or rough, baseless or correct, it arouses passion. Richard Freeman remarks, "Everyone has a 'good' paper rejected at one time because of a vicious unfair stupid referee, and everyone has a 'bad' paper rejected at one time because it deserves to be buried. Neither are quite as devastating as a teenager being rejected in some passionate one-sided romance, but still you can't forget them."[16]

[15]Several respondents find it easier to publish in some fields than in others. For example, Vernon Smith has had relative difficulty in publishing his experimental economics papers. "This is the way we (experimentalists) live! A far cry from the days when I did only theory. Then I could publish my toilet paper."

[16]In addition to the stories that we have told in detail, respondents complained of discrimination on the basis of sex and politics, and of promising young researchers' being discouraged by publication frustration, to the point of leaving economics. However, Blank (1991) suggests that sex discrimination does not exist at one major economics journal.

Are the tales of publishing woe merely frictions of a healthy reviewing process? Or are they major injustices in a fundamentally rotten system? Thomas Schelling bravely acknowledges what most other referees know: even the most fair and conscientious referees and editors err. "I do remember recommending to the Harvard University Press that it not publish a manuscript that, when they published against my advice, did go on to become important. I don't dare let anybody know what manuscript it was." Nonetheless, the outpouring of irritation and anger at the publication process that our project provoked—by the famous economists whom the process has benefitted most—creates concern about whether the process functions adequately.

■ *We thank William Shepherd, Joseph Stiglitz, Timothy Taylor, and Gavin Wright for their thoughtful comments and advice, and Kenneth J. Arrow for inspiring the project. Most of all, we are grateful to the leading economists who contributed anecdotes and analysis, and we regret that this article's shortness forces us to save much of their wit and insight for the forthcoming book, Rejected: Leading Economists Ponder the Publication Process (Shepherd, forthcoming 1994).*

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