Different Drummers: Offbeat, Oft-Rejected

A drummer who sounds a new beat either marches the band in a new direction or gets fired. Several economists' forceful dissent both led the profession to new orthodoxies and earned Nobel Prizes. However, the profession often scorns brilliant unorthodox ideas--or, still worse, neglects them.

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Responsible for thirteen books and over fifty articles, David Colander has presented a strong dissenting voice on economic theory issues. In addition, his insight has illuminated the economic profession's sociology.

Congratulations on a well-conceived project. I'd be delighted to help in any way that I can.

I suspect I have a much higher rate of rejection than the average economist, let alone the notables you're including in the study. In fact, most of my best theoretical work is still being rejected. It was because so much of my theoretical work was being rejected that I started looking at the sociology of the profession. (It was either that or a shrink.).

In my consideration of the sociology of the profession I've spent a lot of time thinking about the publication process (See my paper on the evolution of macroeconomics in the enclosed book) and am currently working on a paper entitled "The Gatekeepers." So if you want some quick feedback on drafts of your paper, fax it to me and I will happily respond. In this letter I simply provide some random thoughts on publishing. Give me a call if you want anything elaborated.

For newly minted Ph.D.'s (untenured) the issue is publication. For established economists, the issue is influence. I can now get just about anything I write published--with 170 journals, publication isn't hard--but getting it published where more than just people who agree with me will read it is another matter. Thus, one's rejection rate varies directly with how much one reaches out to talk to economists with whom one disagrees. Since I disagree with many, and am continually reaching out, I am rejected rather often, even though I know I could publish the papers elsewhere.

In today's profession, publication is essentially a tombstone, establishing a claim on an idea. The real ferment of ideas and thought is in working papers and discussions. Thus, I haven't even tried to publish my latest papers on the use of aggregate demand in economics; and on New Keynesian economics; instead I've sent them around to people whose opinions I value, and have tried to convince them, in letters and in arguments, that my ideas in the paper are important. This is the level at which the real debate about real issues takes place. You might look up Aaron Director's role at Chicago; he published little, but behind the scenes played a major role in getting Chicago's ideas accepted.

I have found that the best way to picture the publication process is as a conversation (or bull session) in which a group of economists is engaged in a debate. Each conversation has rules of discussion which limit the expression of ideas. Each takes certain assumptions as given. How much flexibility is allowed depends on the editor. If you know the conversation, follow the limits, and are good, getting published is not hard. If you do not meet any one of these three requirements, getting published is almost impossible.

Initially my difficulty in getting published derived from my lack of understanding of the rules and conventions of the various journals. I have since learned them and still get rejected, but the reason is because of my desire to communicate with mainstream economists and to change the nature of their conversation. Doing so necessarily involves violating acceptable form, questioning unquestionable assumptions, and having people conclude you're either stupid, crazy, or lacking in training and ability. People have concluded all the above about me.

I've been rejected much more in my mind than I am on paper; there is a rational expectations process which stops me, and I suspect others, from sending in papers to journals until I think there's even a slight chance of acceptance. For example, the new Keynesian literature has finally caught up with the approach to macro that I followed in the late 1970s and early 1980s. My papers on the topic were firmly rejected early on, so that I felt that the best way to get my ideas out was to publish them in a textbook. My macro textbook published in 1986, was one of the first to spell out a New Keynesian model and use the term. Now I see a potential opening for my New Keynesian work and I am currently circulating the enclosed papers, and will submit them once I feel the time is right.

Many rejected authors get mad at either reviewers or the system, attributing bias and malice to them. I think that is wrong. Clearly there is some malice and bias in the reviewing process, but it isn't out of line with other disciplines. Most of the problem is simply built into the structure of the profession. My rejections reflect the different focus of my research compared to the profession's rather than malice. Most reviewers reject my papers because they honestly think I have nothing to say and should not be allowed space in the journal.

I suspect that Nobel Prize winners and future Nobel Prize winners will be rejected more than other economists, because they are generally more willing to question the underlying debate, rather than add a footnote.

But enough random thoughts. Let me recount the specific instance which set me on my current path.

My Ph.D. was initially three essays on optimal taxation, a hot issue at that time. In 1974 Columbia had two stars who worked in this area--Ned Phelps and Bill Vickrey. I had completed two essays and was working on a third. The essays were acceptable, but quite honestly, pedestrian. They didn't excite me but they met the dissertation criteria; they looked impressive, and they dealt with a hot topic.

One day while playing mind games in economics (something I often do) I thought up a plan to create property rights in prices and allow trading in those rights as an inflation cure. I drafted a paper, sent it to Vickrey, who responded with a letter telling me it was brilliant. This did wonders for my ego. My optimal taxation work had never been described as anything close to brilliant. I asked Vickrey whether, if I dropped my current thesis and expanded the idea, I could get a Ph.D. out of it, and whether I could get it published. He responded yes to both, so I switched.

He was right about the thesis, but wrong about publication. The paper was rejected at all the top journals I submitted it to, and after two years of rejections it was clear that this paper was going nowhere. Then I happened upon a BPEA paper by Abba Lerner which had a similar idea. Since I had sent Lerner a copy of my earlier paper and he didn't cite it, I was a bit upset at the unfairness of it all. At the AEA meetings that next year both he and I were presenting papers; his session was attended by hundreds and in the question and answer period, I got up and told him his paper was wrong for three reasons, which I listed. I then sat down, having had my say. My session was scheduled for the last day of the meetings and two people were in the audience, one of which was Abba. Following the session, he came up to me and said he was thinking about my points and he thought I was right and that we should get together and do a book. I asked "When?" he said "Now," so we stayed in Chicago and outlined the book, MAP, that I did with him. So eventually, Vickrey was right, but all my papers about MAP are still relegated to books, non-mainstream, and less than the highest-ranked journals.

Luckily for me, my future was not dependent on getting those ideas published in top journals. To insure that it didn't I chose to teach at an undergraduate school. Here I can deal with issues I consider important in the manner that I find most appropriate.

Abba had no regard, nor sense of the profession's conventions, and he took me around telling people I was very bright and about the best young economist around. During that time I broke the barrier and was accepted as a legitimate economist, albeit, a bit strange. Since then getting published has never been difficult, but getting published where the ideas will be seriously considered remains difficult, if not impossible. It was at that point that I started to analyze the sociology, incentives and nature of the economics profession.

I've published a few papers on the idea that Vickrey and Lerner both thought was a great contribution to economic thinking, but the idea is still not understood by the profession because it can't be translated into a formal model. Why? Because it deals with the interrelationship between the dynamics and statics in a general disequilibrium aggregate monetary economy. Developing a formal model of it would require a mathematical model which is far beyond where the state of current mathematical modelling is. (I did get one paper on it in the recent papers and proceedings issue of the *AER*; I enclose a copy). When ideas outstrip the currently used techniques, there is no mainstream outlet for them.

I've thought about starting a new journal entitled *Economic Ideas* which would be devoted to such issues, but rejected it; there are too many journals out there. In the meantime I continue to rack up top-ranked journal rejections as badges of honor.

Sincerely,
David C. Colander