The wish list of an editor - some reflections on editing the Scandinavian Journal of Economics.

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The Scandinavian Journal of Economics (SJE) was started in 1899. It has gone through a couple of changes of name and switched from publishing in Swedish to English, but there is an unbroken tradition, very nicely summarized in Persson (1998). About two years ago the author of that piece, Mats Persson, called me and wanted to discuss something that was too important to deal with over the phone. There was a sense of cloak and dagger, but after we had trouble finding a time to meet, we did things over the phone nevertheless. The question that Mats popped was if I would be willing to serve as one of three editors at the SJE. My wife frequently complains that, apart from a monthly contribution to Médecins sans Frontières, I am an individualist who does little to contribute to society. I don't agree and here then was a tangible way to demonstrate my pro-sociality. I also figured that I would learn something in the process, several of the people that I admire the most in the profession have served as editors. Correlation is of course not causation, but I've always liked a good correlation, so I was happy to accept Mats' offer of much work for low pay.

In Persson (1998) the question "what is the role of journals?" is discussed. I also want to build my reflections on editing from that basis. Given that we all have homepages and post manuscripts on the web it is clear that journals' role of *disseminating* knowledge have come to take the back seat. I actually think that it is not unimportant – just as I stumble upon interesting books in the bookstore, I can get caught by an article that I peruse in the bus on my way to work. The more important role for journals nowadays is as a *quality controlled repository*. By having gone through the editorial process a paper is presumed to be in the clear that it provides new knowledge with a scientific basis. Calculations are correct, empirical methods are applied as best we know how and arguments have a solid foundation. Not only is this quality controlled version available, it is also available in one final version making future references easy. We can refer to Quah (1993) or Fehr and Schmidt (2004) and not need to go on archeological expeditions for different versions. Finally, and this was not mentioned in Persson (1998), publications serve as a *measure of an individual's scholarly output*. How many papers in different types of journals does person A have – this matters for tenure, for promotions, for research grants and for intergroup prestige. Gossip of the style, did you hear about her "Econometrica that went straight in", are eerily reminiscent of how a front flip 720 is viewed in the fun park of any ski resort.

What motivates me for the job as editor is providing the quality controlled repository. However, the measure part of the job casts a sometimes unattractive shadow in cases when it seems like maximizing the number of articles published is the main goal of authors. There are many ways to slice the steady inflow of papers that I see as an editor. Some papers are truly interesting to a broad audience (in my interpretation) and well executed. Little problem there – I can easily ask people at the top of the game in the field to act as referees and the whole process is rather straightforward. At the other end of the spectrum, there are many ways for a paper to fail. They may be poorly written, they may use data that are abjectly inappropriate for the question posed, or they may consider a truly contrived theoretical twist. We receive thankfully few such papers, but in any case they present little problem for me as an editor. I do a summary reject. I need to read and motivate my decision, but the work involved is straightforward. The tough part of the job comes from some of the papers in between. The paper is competently executed but the contribution is hard to judge. Consider for instance an empirical paper that uses appropriate data but is one of a list of papers that examine similar issues. Some papers I find rather boring, but my editorial arrogance has not yet reached the level where I can give that as argument for rejecting a paper. And some other papers I find truly interesting – but is it really new and is the contribution really as important as the authors claim that it is?

How can we make the handling of these intermediate cases as smooth and accurate as possible? Let me organize my discussion around a set of suggestions for how referees, editors and authors should behave. I am under no illusion that practices will change, but writing wish lists can be a worthy activity in its own. My son, who can write "a dog" 250 times for his birthday wish list can, I'm sure, testify to as much. So let's kick it off and start with my wishes for referees.

Please give me a quick short review that gives an honest evaluation of the contribution rather than saying no, or work too long with providing a large number of marginal improvements. Finding referees that I trust is often rather time consuming for papers of what one might call intermediate quality. I want people that are likely to catch any omissions or errors. The level among the referees varies hugely, but as a general rule I am impressed with the quality of the services provided in this respect. As a rule I receive careful and precise evaluations at the technical level of a paper. Where I often receive less aid is in judging the contribution.

Let me create a hypothetical example. In long distance running it is common to have “rabbits”, or pace-setters. Someone whose role it is to set the pace early on and then drop out of the race after having burnt himself out part-way through the race. Consider a literature that examines the effect of rabbits on finish times and the identity of the winner in long-distance races. In terms of policy this may matter – New York marathon banned the use of rabbits in 2007, whereas other marathon races allow them. Not a huge issue, admittedly, but maybe it says something fundamental about the way we respond to incentives and what makes us competitive? Anyway, suppose that I sit and look through this paper that examines the causal effect of pace-keepers on finishing times in marathon races. The paper is well written and the authors argue that they have a particularly clean identification strategy and that the findings have important consequences.

If I deem that this is too narrow for the SJE I do a summary reject even if the paper is well written. But maybe this is really a fundamental contribution to the link between incentives and competition in sports? The authors certainly give that impression and I am not well versed enough to say no straight off the bat. Ideally, I want to find referees with some publications in top journals that are in the field somewhat broader defined, say in sport economics. But they politely decline. Some introspection suggests that they do so after a brief look at the paper - even if pressed for time I will say yes to referee a paper that I find really interesting after having glanced at it. I take it as a negative signal if I have trouble finding referees to accept. A three line evaluation would be very helpful for me – is it just a lack of time or is it that one feels the contribution is too narrow? In the end I find referees with a list of publications in what sometimes feels as a very narrow subfield (economics of marathon running). In my experience such referees often deliver a careful check of correctness and suggest improvements. When I receive reports back in these cases they often suggest a revise/resubmit. I then give the paper a more careful reading and may check some central reference. If I’m not convinced I turn to associate editors to get a second opinion. This is part of what we have associate editors for, but they are a limited set of people that to some extent face the same problem that I do, that they are a bit too far removed to easily judge the contribution. So, quick evaluations from people within the field broadly defined would be lovely.

Keep to deadlines – and if you can't communicate with us. I haven't done the statistics on this one, but it certainly feels like most referees wait until after the deadline with sending in reports. Some wait quite a bit and we do not know if it is because they are running a week or late or if because they have forgotten. Automatic reminders are sent out and this would be a great time to let us know why you haven't sent in the report – "been swamped, sorry, you'll have it by May 5", would be helpful. Unfortunately, response rates to automatic reminders are close to zero. More comforting is that most people are very quick in responding to personal communication, frequently with a note "I'll get it done by next week." In practice this means more like three weeks after perhaps another nudge. I tremendously appreciate the good service that we get from almost all referees. I also have full respect for that we are all bogged down with work and sometimes you just need to shut out the world and focus on research. But by ignoring deadlines we generate more, rather than less, email clutter. Rather set a longer time horizon and keep it than be overoptimistic. Then there's the case of setting a long deadline and subsequently ignoring it, not a favorite…Lest I give the impression that the process drags on forever at the SJE, let me note that most papers get a first decision within three months, but it takes considerable work to keep it that way.

A little aside: Sometimes one hears the advice that "don't send in your referee report to quickly, you'll just get to do too many reports". It may be true that people who never respond or who break deadlines in a truly notable way are less likely to be asked again. But if you actually send in reports we may turn to you again at some point in the future regardless of how quick you were in answering. In the system we use to handle papers we see the history of referees. We wait at least a year before asking again and are very conscious to avoid overusing the services provided by referees.

Apart from the above two points – keep doing what you do. The previous remarks should not overshadow that we typically get careful reports within reasonable time which serve as a quality check and improve the papers. We are grateful for that. Before turning to my wish list for authors let me also make a wish for the work of other editors.

Cut the innovative a bit of slack. Different subfields tend to develop a pattern for what is the standard way of proceeding and failing to follow that procedure is given a no-no by the elders (the referees in this case). After micro foundations were introduced in macro it should all be micro founded – good in principle but forcing us to work with complex models when sometimes striking simplifications can bring us more. In the structural models that we use in empirical industrial organization some short cuts and omissions are perfectly fine, whereas others are the equivalent of a certain recommendation of reject. We should do all the loops and tricks to solve some problems that we know how to tackle, whereas other problems are perfectly fine to sweep under the rug. For really innovative work some of these check lists can add a lot of complexity. It is part of the referees' work to point these possible omissions out. Part of my work as editor is to guide authors on which concerns of the referees that I see as crucial and which concerns where a bit of explaining and discussion will do. Ideally referees also comment on the relative importance of their different concerns in the letter to the editor but if they don't, it really is the job of the editor to take this into account.

Similar concerns arise in also in micro-econometric work. If I'm producing the umpteenth paper on the effect of hares on finishing times in marathon races, the quality of the data and identification really need to be outstanding to make a contribution in an area where we already know much. If referees coming from this mind frame are confronted with new data and a new question they may be unduly critical. Again, it is the role of editors to provide a check on this. However expectation among authors regarding the likely demands placed on various types of projects is an important facet of what questions we dare pose. Some of the papers that we receive in microeconometrics that examine "new" questions are very weak in the handling of the data and the description and analysis of identification. Maybe skilled researchers, in a rational response to the editorial process, are excessively playing it safe?

Let me finally turn to my wish list regarding authors. One set of wishes concern the crafting of papers. Be careful to *place your paper in the literature and take the time to really polish your paper*. These are obvious demands that we place on authors and many fulfill this the first time around. Surprisingly many authors are too sloppy in this respect however and I believe that, when as a junior faculty you see some of the "big names" publishing rather homely ideas in good journals, an important explanation is their craftsmanship in expressing their ideas, rather than just fame begetting fame.

Further wishes of mine border on somewhat presumptuous advice about how one should conduct research. This little branch of philosophy of science (and of life) usually surfaces in the wee hours after a number of beers in a conference setting, but given the invitation to share thoughts on editing I might as well take the opportunity to vent some ideas. I claim no originality here, but some questions need to be ever repeated.

Maximizing the number of papers should not be the goal. In my ideal world a paper is just a summary of something that you are really dying to share with the world. It is not primarily a publication. Some people are amazingly productive and generate that kind of ideas all the time. Others may generate a lot of publications by making slight variations on previous papers and sending them to different journals. I can see that the incentives early on in a career are to publish papers and do so quickly, but after tenure I wish that many of us could slow down. The opportunity cost of writing another variation on a common theme is that it may crowd out more fundamental work.

A highlight of the year in Stockholm is seeing the Nobel laureates early December. In 2005 Thomas Schelling was one of the laureates and I got to listen to an after-dinner speech of his. One of his points was the following – get tenure, then use that freedom to really do what you want (research, not surfing). Not all of us have the profundity of a Coase or a Schelling within us, but I think our long run impact would often be much higher with fewer rather than more papers. Andrew Wiles' withdrawal to prove Fermat's last theorem is perhaps an extreme example, but I think we would be better economists if we to a greater extent really dug down to create that new data set, that new theory or paid our dues to implement a market design. I've heard at least one very smart guy in his fifties regretting that he did not devote more time to fundamental issues.

One common advice is that there should be one idea per paper. If you have three ideas that come out of the same model, make each of them into a paper. One argument is purely career wise – it gives you more publications. Another motivation can be that this makes it more accessible for other researchers. If I am looking for information about how pace setters affect the results in marathon races maybe it is useful to find that upfront rather than in a paper that examines that a paper on the effect of pace setters in a number of different competition formats. I agree to that. But the way papers are written in economics all too often bury the contribution deep in the paper. There are often many similar papers circulating with connections between them that are far from crystal clear. Rather than stressing that one simply makes a little (but interesting) twist to a previous model, many authors try to portray each work as more different from previous papers than it in reality is. I've had a handful of cases where the papers border on plagiarizing the author's own work. A reference is given for instance to Cat (2004) noting that they study a similar problem and the paper then proceeds to present a model where the reader is given every impression that it is new. But when you check Cat (2004) you find that the same model is there. This is of course bad form and a sharply worded rejection will follow.

Of course, if you have a model and think that one interesting implication is worth working out, I do not suggest that you should never do so. My wish is simply that papers which are spinoffs on other papers are very clear on this. I have nothing against "We use the model of Pheidippides (2010) to study the effect of payout structure on finishing times in marathons" and then move directly into action. Or, "using the data and econometric model from Radcliffe (2003), this paper simulates the effect of extreme heat on marathon dropout rates". I am much more positively inclined to such a paper than to one that uses very similar text as another paper does and then strives hard to differentiate itself. Papers in some of the natural sciences are much shorter and with much of the background buried in appendixes. This could be a useful model for economics as well, but it is hard for only one journal to change practices.

Look at problems that are close to your heart or experience. Being the editor of a journal that is based in Europe this suggestion largely amounts to kicking in an open door. Even so, let me comment on the perception that I often hear that you need to work with US data or US policy issues to publish well. It may be true that if you send papers to for instance the *American Economic Review* based on data from Sweden, you need to base your contribution on that you have exceptional data or a truly compelling identification strategy. So, if working with data from smaller countries, or on policy issues of particular relevance to such countries, you need perhaps to be even more careful in motivating your work and placing it in the literature. But good work should do this in any case and your ability to contribute is so much greater if you have a deep knowledge of the institutional setting for the issues that you study. The likelihood that you produce something really interesting is then much greater, and we may learn that similar issues are faced by many others. The Nobel foundation publishes autobiographies of the laureates on their website[1] and browsing through these it is striking how often laureates say that their groundbreaking ideas have been shaped by personal experiences. A good example is George Akerlof's experiences in India that bolstered his interest in why and when markets fail. It is impossible to read works like Keynes' (1920) "The Economic Consequences of the Peace" or Hirschman's (1945) "National power and the structure of foreign trade" without sensing the passion they felt for

[1] nobelprize.org

their subjects. That makes them interesting to read also close to a hundred years after World War I ended or seventy years after Nazi foreign policy attempted to dominate Europe. Of course, it would be unrealistic to expect contributions at this level from most of us most of the time, but also in our run-of-the-mill work I wish we would stay close to our passions. Most of us would earn much higher wages in other lines of work, why stay in academia if not from following our passions? If nothing else it makes my life as editor more stimulating.

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