

INTERVIEW

On Being the Editor of AJS

Andrew Abbott

aabbott@uchicago.edu

Abstract

Andrew Abbott, editor of the *American Journal of Sociology* from 2000 to 2016, explains in this interview the role of an editor, discusses criticisms against flagship journals, relativizes the influence of editors, praises colleagues participating in the peer review process and offers a look at the back-stage of academic publishing.

Keywords

American Journal of Sociology; editor; peer review; metrics; academic publishing

Note

Serendipities approached Andrew Abbott and asked him whether he would be willing to share his experiences as the long-term editor of one of the flagships of sociology, the *American Journal of Sociology*. Abbott agreed, but suggested to do the interview in written format. The following text are Abbott's answers and the questions in italics are from Christian Fleck.

Introduction

The questions asked range across a variety of areas, and so it is best to start with an overview of the aims and everyday processes of the journal during my tenure. It was written in 2016 and thus reflects my whole experience. It is also in the present tense, although I am no longer editor. That is because putting it into the past might make it seem that “everything has changed,” even though I would guess that things are much the same under my successor, Elisabeth Clemens. But the running of the journal is the editor's prerogative, and I cannot speak for her, only for myself.

After this opening section, I will answer whichever of your specific questions has not already been answered.

The AJS as of January 2016

1. THE AIM OF THE JOURNAL

The AJS has two aims. First, it aims to publish top quality work from across the subcommunities of the discipline: work that brings new theory, new method, or substantial new data, or that makes a decisive intervention in an ongoing debate. Second, it hopes and expects that that work will be writt



written in a way that will have meaning for a more general sociological reader. The journal, like the department that runs it, believes that there is a general reader; that sociology is a discipline, for all its diversity, and indeed that part of the discipline's discipline, so to speak, is its ability to embrace and to find interesting many ways of thinking about the social world. The job of the editor is to choreograph and administer a selection process that produces a journal as full as possible of papers that met those two standards - top quality in their subfield and substantial interest for a general sociological reader.

That the journal must cover the range of the discipline means that our standards are not simply an article-by-article matter, but that we must also use a decision process that will produce a journal that contains many different kinds of work and areas of work. This too is something we bear in mind continuously. I often say that the discipline is an archipelago of islands like the Philippines, with big islands like stratification, gender, and medical sociology, medium-sized ones like sociology of religion or sociology of science and little atolls like conversational analysis. The journal should have a couple of papers a year from each of the big islands, one a year from the mid-size ones, and a paper every now and then from the little atolls. The Editorial Board¹ has to bring about that representative sample even though people on the different islands submit at different rates, even though the various islands disagree wildly about the nature of good work, and, most important, even though some of the big islands have their own journals, often as prestigious or more prestigious (in the subfield) than we are: gender has *Signs* and *Gender and Society*, medical sociology has JHSB, organizations has ASQ, demography has *Demography*, and so on. Moreover, we have to make this discipline-wide representativeness happen through a process of individual decisions on papers one-at-a-time. We can't pile up papers for half a year and then sit with a spread sheet and decide which combination of a whole set of possible papers would work best. We simply have to constantly bear representativeness in mind, alongside top quality in subfield and substantial interest to the general reader.

2. THE AJS AS A SOCIAL STRUCTURE

The "we" of the journal involves three major constituencies: submitting authors, reviewers, and readers. Of these three, the readers – both the general readers and the experts – are the most important. If a decision comes down to a zero-sum situation and we must displease one or another of these constituencies, it is always the readers' interest that must predominate. The journal exists for the readers first and foremost.

The reader constraint is a relatively general one, however, and we have the referees to help us estimate it. So in practice we who run the journal are usually more focused on our obligations to the immediate constituencies - authors and referees.

To authors we have an obligation of fair and timely process. And secondarily we have the obligation to teach some of them some things they may have no other way to learn. Not everyone has colleagues near at hand to read his or her work, and so we must take seriously our de facto professional education function. Our referees often write excellent "teaching reviews," and that is an important, if unsung, professional activity. We get a surprising number of thank-you notes from rejected authors who have found the reviews very valuable. Indeed, our role in professional education and collegiality is one reason we hesitate to increase the number of desk rejects.

¹ Usually about five or six total: The editor, two or three other faculty members, and two senior PhD students in their fourth or fifth years of the Chicago sociology program. CF.



To reviewers/referees we have an obligation first to take their views seriously and, second, not to overburden them. (This second obligation to referees obviously conflicts with the professional education obligation to authors, as we know well.) Our attempt to be broadly representative means that we are often selecting different reviewers to handle different aspects of a paper – methods, general interest, theoretical solidity, etc. So we often must make decisions based on combinations of reviewers with different expertises, interests, and agendas. Doing this in a way that is fair to all the reviewers concerned is an important challenge in maintaining the AJS reviewer pool.

3. PROCESS

The AJS process is straightforward. An initial vetting is done by the student manuscript board. We have to check papers for the authors' current and PhD institutions (so we can avoid getting referees who know them and could be biased one way or the other). We have to establish a list of potential referees. Sadly, we have to check for duplication of prior work; we can no longer be sure that all authors will police themselves on this matter.

Once the referees are proposed and accepted or modified by the Managing Editor and myself, the requests for reviews go out. Today, most papers will require multiple new proposed reviewers as they go along. Prior to electronic solicitation, AJS got 3 reviewers for four requests 80% of the time. That figure is now down to 40% of the time, and for 25% of the papers we are eventually asking 7 or more people to get 3 reviews. And reviewers themselves are taking longer. The office spends a lot of time chasing late reviews.

When three reviews are in, the paper comes up for decision. AJS is one of the last major academic journals in existence that has a weekly board meeting – 9AM to noon nearly every Wednesday of the year. The faculty and student Associate Editors sit around the table. I chair the meeting, in which we go over the reviews and the paper. Often we get into elaborate discussions of methodological issues or interpretive questions. Often we use our sense of a reviewer's prior reviews to interpret ambiguities in the current review. If the paper is a revised one, we carefully go over the revision memo to make sure the prior referees' questions were addressed.

Sometimes there are so many questions that we postpone decision: perhaps we need another review – we'll come up with names on the spot. Or perhaps we need to lean on a delinquent referee who is covering a crucial angle. Or perhaps one of us will take the paper home and write yet another review. But if we are ready, I just start calling on people in a random order around the table, asking them to contribute their views. Consensus is usually quick. When you work together week after week, and in some cases year after year, you get a real sense of each other's views. I myself need to cast a deciding vote only rarely. The Board's decisions are indeed Board decisions.

4. MYTHS AND PROBLEMS

As this discussion makes clear, my supposed editorial power is a myth. In the first place, the main determinant of what appears in AJS is submission. If papers aren't submitted, they won't be published. For most areas that don't appear much in AJS, the reason is that their practitioners choose to take the best papers elsewhere. Since we in turn do not want to publish less than the best, the result is to make the field underrepresented in AJS. My taste has nothing to do with it.

In the second place, as I have just said, the Board makes the decisions and is surprisingly consensual. Any one of us can and does challenge that consensus from time and time, and sometimes persuades

the rest. But we generally read dissensus as requiring more information and cast about for new referees or adjudicators.

There could still be bias in such a system via preemptive decision (by authors) not to submit because I am editor, on the *belief* that I must exercise some biased authority, or that my previous discussion of the Board's strong consensus is a matter of misrepresentation or bad faith. But the evidence that I do not control the content is easily found in the journal's pages. One of my most cited papers is a critique of what I called "general linear reality," the ontological assumptions underneath standard sociological quantitative methods. Yet dozens of excellent papers in the GLR tradition have appeared in AJS during my editorship, as well they should. Myself, as a scholar, I have reservations about a considerable amount of what is published in the AJS. But it's not the *Abbott Journal of Sociology*, it's the *American Journal of Sociology*. What matters are 1. the views of the experts in the paper's subdiscipline, 2. the issues of bias that can arise in those views (examples of biased reviews are "not in my backyard" reviews, "I said this last year" reviews, "tempest in a teapot" reviews, "I love this because it does the kind of stuff I do" reviews, etc., etc.), and 3. the responses of the Board as it attempts to channel the general sociological reader. My own personal thoughts about the work are irrelevant.

So I myself do not try to read and judge all the papers from scratch. Our job is to pick good referees and then carefully to read the reviews and the paper in the light of the reviews, knowing what we know about the habits of the reviewer, the seeming quality of the paper, the styles of the subcommunity, and so on. As I noted, one of us may take a difficult paper home to read it carefully and write another review for the board. We may have to run some particular statistical worries by a colleague in statistics, or run a political sociology paper by a colleague in political science. The idea is that the paper has to be excellent in the terms of its own subcommunity *and* to have something interesting for the general reader. And our job is to make that happen.

Another myth is that Chicago people have an advantage at AJS. I have looked at the last 8 years' worth of data, both in terms of current location (as faculty or student), and in terms of location of PhD. In terms of current location of authors, the Chicago people are eighth in total submissions in this period and fifth in success percentage, among departments submitting twenty or more papers. In terms of authors' site of PhD or expected PhD, Chicago is fourth in terms of total submissions in this period and fourth in success percentage, among departments submitting at least thirty papers. (The data are not quite as good on PhD institution, since we lack PhD departments for about 20% of authors, so I compare a slightly smaller group. Note also that there is no feasible way to control for the different sizes of the PhD pools from various institutions, which must shape the submission rates to some extent.) On neither success indicator are the "Chicago and above" departments (five in the first case, four in the second) substantially above other departments. A few changed decisions on particular papers could reorder these lists a good deal. In sum, there is no convincing evidence of any Chicago effect, either by present affiliation or by site of PhD. Chicago looks the same as comparable departments, both in terms of submissions and success rate.

So that is the basic story about the AJS. Let me now consider the particular questions you have asked.

After Albion Small at the very beginning of AJS your term as editor has been the second longest. Since you did research on the history of AJS in the past, could you give us an idea what have been the major differences you experienced in your role as an editor compared with what you did know from researching the history of AJS?

The history tells us that the journal has had the same general structure since the great demographic transition of American sociology (and American academia more broadly) in the late 1970s. With the end of American academic expansion, American faculty hiring changed from a sellers' to a buyers' market. So vitas had to look better. The gradual result was a steady increase in publication per person per year. This was not generated by wonderful new techniques, which came at the same rate as before, or by intellectual revolutions, which also came with great regularity. It mostly derived from the market change.

It is true that a variety of forces in the 1960s and 1970s drew a particularly talented generation into the discipline, which might explain some of this increase in publication. But entry to the discipline (and academia in general) fell like a stone in the 1980s, once the news of the newly tight market spread. It is also true that technical changes (the personal computer) made writing articles much easier from the early 1980s onward, as did canned statistics (from mid 1970s), and similar developments. These technical changes reduced barriers to entry and contributed to the publication expansion. But I still think that the main factor was the switch to a buyers' market for faculty. As of the 1950s, the modal US PhD was publishing two or three papers in a lifetime.

We can also infer that the quality of the average submitted paper probably fell, since the pool of people writing articles had already been submitting their best work, and expansion inevitably meant publishing things they wouldn't have bothered to publish under the earlier regime. Only the "strong 1960s generation" factor militated against this decline in quality, while the market pressure and the lowered barriers to entry clearly favored it. All AJS editors since Charles Bidwell in the 1970s have therefore faced the same problems - lots of papers, of uneven quality, and a seeming dearth of excellence.

There are however some problems that have steadily worsened in the period from the 1970s to now (2018): declining disciplinary consensus, increasingly explicit political arguments in submissions, increasing specialization within subfields, competition from more and more journals (particularly, excellent specialty journals), differing international genres for article-writing, and, eventually, a loss of the professional ethics that had guaranteed that we didn't have to check for double publication, self-plagiarism, and the like. It's also true that the sheer size of the published record today means that journals are probably sometimes publishing things that had been done before - ten or twenty years ago - with no one (authors, referees, editors) being aware of that earlier publication.

Some of these worsening problems are things I did not expect. I knew about the overpublication and the quality decline. I knew about specialization, competing journals, and international differences. But I did not expect politicization to become so strong so quickly, and I certainly did not expect authors to try to publish in AJS articles that, in effect, changed only a few variables (and sometimes the theoretical framework) from articles they had already published in ASR or elsewhere.

I am sure that in the life of an editor there are episodes of excitement and success but also periods of frustration and anger. Could you let us know what have been your best and worst experiences as editor?

The best experience was getting notes from authors thanking us for the quality of the reviews and the editorial process. Of course, these were most touching when they came from authors who had been rejected.

Another good moment was saying thanks. For some years, I sent a hand-signed thank-you note to every single reviewer. That was a happy time, too, for it made me realize how wonderful was the

community that supported the journal. And saying that the Journal was a wonderful community gives me another happy opportunity to say thanks. Most of the work of making AJS into a community of editors, students, and referees was not done by me, but by the Managing Editor, Susan Allan. The AJS of the last thirty years is unthinkable without the talent, dedication, and personal warmth of this wonderful human being. She has been the continuity and the glue of AJS. The Journal's excellence over this period owes more to her than to anyone else.

The worst experience? Probably the one or two flame emails I got from people who resented rejections and who didn't wait long enough to cool off. But such emails were very few (less than five). That's because in fact the AJS process is scrupulously fair, and people know that. Out of the thousands of people whose work AJS rejected, there are only two people, to my knowledge, who simply talked speaking to me afterwards. So there weren't a lot of bad moments.

What are the two or three papers published during your tenure you liked most? And why?

This is an invidious question, and I wouldn't answer it even if I had favorites. But in fact, there is no answer. It wasn't my job to like papers or not like them. It was my job to make fair decisions in line with the Journal's aims and its rigorous professionalism. Therefore, Andrew Abbott the scholar and Andrew Abbott the editor had very little to do with each other. The editor was very concerned to keep the scholar out of the editing process completely, because the scholar has strong and quite particular positions about many things in the discipline. There had to be complete separation between the two personae. That meant paying much more attention to referees than to my own thinking, and it meant invoking my own analysis of a paper only when the referees left us in a real muddle. In practice, the only way to shut up my scholarly self was to curtail my own reading to a considerable degree, and I did that.

In the end, the result is that there are many fine papers in AJS, of many different kinds, and I'm happy to have been able to publish them.

In preparation for this interview, I talked to several people from different corners of the world, differently placed in the status hierarchy etc. Surprisingly enough all of them did not know how a journal like AJS functions nowadays. The one thing outsiders probably overestimate most is the degree of influences and direction an editor has. Am I right that it is wrong to suppose that you made decisions about acceptance or decline of papers at any time? But, why then acting as an editor when the chance to make a difference are low?

Yes, as the discussion above says, the editor actually has very little power. And, indeed, by today's professional ethics, it would be morally wrong for an editor to exercise any power that he or she did have. The days of journal editors' "making a difference" by choosing work to suit an agenda are over, particularly given the current generation's near-obsession with fairness. That kind of "making a difference" has moved to edited volumes and special issues.

Why then be a journal editor? Because someone in the department had to do it and most of my colleagues didn't want to. So I edited the journal even though it was a lot of work, a lot of emotional responsibility, and absorbed attention I could and perhaps should have given to scholarship. (I did manage to publish six books while editing the journal, so it can't have done *that* much damage to my productivity.) As I said, I edited the journal because it's a duty that has to be done by someone in my department, and I found it a mode of doing my departmental service that suited my inclinations and talents. It served both the department and me (and, I hope, the discipline) to have a long tenure, which in turn meant steady policies, predictable processes, and so on at AJS.

Could you describe in some detail the routines of AJS? Many papers come in, who is doing what with them? Is anyone of the editors reading all these pages or do you assign reviewers instead? After getting enough reviews back, you and your associates do read them and what's next?

I explained the AJS process above. With about 500 submitted new MS per year, and five members of the sitting editorial board, it is obvious that the board itself does not read all the papers. Given the diversity of the submissions, five people could not in any case cover the possible types of papers. This is what consulting editors are for, at AJS as elsewhere. The problem of such a journal is to maximize the fairness of the decision, given the general aims of the journal, as noted above. It is by no means necessarily the case that this could or would be done by focusing the editors' time on the reading of all papers. Rather, one aims to triage the papers into the obviously strong, the obviously impossible, and the not quickly decidable. One focuses all one's attention on the last category. Truly excellent papers would be a pleasure for us all to read, but in fact they get less attention than those on the edge of decision. Only in a very different submission environment could one focus on improving papers that are already excellent.

All the same, it should be noted that many authors of excellent papers are unwilling to believe that those papers can be improved in any way. Indeed, I often feel that way myself, when I first read suggestions in reviews of my own work. So it's not clear that extra attention to already-excellent papers would be a useful deployment of editorial effort for AJS.

To be sure, however, my own experience persuades me that one's initial reaction ("nothing needs to be improved here") is always wrong. Referees' suggestions have inevitably proved helpful once one has fought down one's egotism and have paid attention to them. They aren't necessarily helpful in the sense that one does what referees want, but they are helpful in that they force one to see what can and should be improved, perhaps in a quite different way. I have a feeling that the authors of many already-excellent AJS submissions ended up behaving this way – not necessarily doing what referees recommended, but improving the paper in other and important ways, indirectly suggested by referee comments.

Some people argue that peer review produces too much mainstreaming of the publications? Do you remember any "heterodox" paper you thought worth to be printed which became declined according to the consensus of the reviewers? Or did it happen that you overruled them?

We published some funky things over the years. But at least in my time, we never published an article that *only* we (that is, only the sitting Editorial Board of five or six) thought was excellent. For us to publish an article, there *had* to be one outsider – one referee – who thought the paper was really excellent.

One therefore provided for funkiness by looking for reviewers who had broad tastes. The late Arthur Stinchcombe, for example, was a man who could find surprising virtues in unusual papers. Often, we would send an unusual paper to him or someone like him - somebody who had a feel for the unusual. Not all unusual things are great, after all; some of them are just wrong. So you need reviewers who can judge such papers. But on such papers, you also still need some mainstream people who will give you the more general expert reader's reaction.

But the bottom line is that we never moved ahead without having one strong outside reviewer who was completely convinced of a paper's importance.

During your term the academic publishing business experiences tremendous changes: digitalization on the one hand, metrics on the other. I guess AJS is still making enough money but most

probably only because of the library subscriptions. However, does it make sense to print journal issues any longer? The alternative to go online would make it possible to expand the size of any journal, so the question is: would an online only AJS publishing double number of articles losing in quality?

Whether or not to keep the journal in a print version is not my business. Subscriptions, in any case, do not have to be print-based.

Obviously, expanding the journal because it became purely an online document would be more or less costless. The reason against doing that expansion is simple. The journal is basically selling selectivity – its product is “excellent selection,” not “the papers in AJS.” Its mantra is “if you have time to read only six articles in the next two months, then here are six very good possibilities.” If we said, “Here are twelve possibilities” or “here are twenty-four possibilities,” then people would go to other journals, because they don’t have that much time to read. The whole point of journals and peer review is to save readers’ time by hierarchizing the things that could be read.

And indeed, the main problem of the digital environment is the lack of such quality signals, which have disappeared because while the costs of print and distribution used to justify the centrality of strict and intensive peer review, nothing real has replaced that justification. Indeed, now the online environment is full of bogus quality signals – citation level being the worst of them all. By contrast, AJS is about delivering a really careful quality signal based on a model peer review process. To move away from that would be crazy.

Let me come back to all those metrics, like impact factor, altmetrics, download statistics etc. In which way did you care about them during your tenure?

As my comment about citations suggests, I paid no attention to metrics at all. I have worked extensively with citation statistics for more than twenty years. I know what they are good for and what they are not good for. I have, indeed, written a careful analysis of all the articles in one year that cited my own most heavily cited book – an analysis which told me that many of the book’s citers have no idea what it actually says. Most important, I know that citations are not a linearly ordered system of quality. Most of “how much something is cited” is determined by its subfield, its ability to rephrase the obvious, its bandwagon status, and so on.

The only statistic I found interesting was the fact that the vast majority of hits on AJS articles in JSTOR result in a print. That is, the move to “digital” is not a move to “digital” at all. It’s merely increasing the use of paper and hiding that increase. On the good side, at least the fact of mass printing shows that people recognize that “reading online” is not real reading.

*A recent study on coercive citation practices (Wilhite Allen W. and Eric A. Fong, 2012. “Coercive Citation in Academic Publishing”, *Science*, 335: 542-543) did not mention AJS as coercer but many leading journals from different social sciences. What have been your experiences with regard to wishes from reviewer to quote their publications in papers they read for AJS?*

“Coercive citation” means two things. First (and worse) it can mean that a journal forces authors to cite articles which have appeared in that journal. The AJS does not do this. There is no pressure to cite AJS articles. It would be unethical to make such pressure. A discipline that allows such things is a discipline that will deservedly degenerate into rubbish. There are journals that are doing this kind of coercive citation, of course. But I will stand by my prediction: any field in which this becomes a widespread practice will die as a serious intellectual enterprise.

Second, coercive citation can mean that reviewers suggest that they themselves be cited. Yes, reviewers do this. It is quite obvious to anyone who reads many reviews. (I read about 17,900 reviews as editor of AJS). At one point near the end of my tenure, I did an analysis of many years of AJS reviews, and found that IF a review contained the string “(19” or “(20”, then the chances were about 1 in 3 that the review also contained the reviewer’s last name. There are a lot of slippages in such a measure, and one could see them as making it either higher or lower than the “true” figure. But this measure means that it is a reasonable guess that there’s a good bit of “suggesting” of such citations going on. It’s not overwhelming, but it’s there.

I think much of this suggesting follows from the excess of publication. Here’s the argument. Most good ideas are quite common – hundreds of people have them. (Merton was underestimating when he talked about “multiples.”) In a system where everybody has to publish all the time, more people are going to publish these common ideas, instead of saying (as we did, when wondering whether to publish as graduate students in the 1970s) “Oh, everybody probably knows that.” Today, young scholars can’t afford to think that.

So I think many referees do genuinely see, in articles they referee, things they have themselves published. It just has to do with the sheer excess of publication, which means that many people in effect publish the same things – commonplace, everyday ideas that are almost truisms. I don’t think there’s a lot of maliciousness or pushy self-interest here. It’s just produced by the death of reading, which means that scholars aren’t aware of the hundreds of *other* people who have had “their” idea and, indeed, have published it elsewhere.

The solution to this is of course to stop useless publication. I have no idea how to do that.

Could it be that authors submitting to AJS did align their list of references with unneeded quotes from AJS?

Yes, I suppose people could pad their reference lists with AJS articles. But referees are not going to be misled by seeing a lot of AJS citations. They still read articles critically. If the Board itself were doing all the reading and deciding, there would be more danger from this. But it doesn’t do so, so there is less problem than one might expect.

By a rough calculation during your term as editors AJS published 600+ research articles. Following your own advice in Digital Paper we sorted them by the frequency of their being quoted by others and the distribution picture is somewhat surprising. Excluding the most recent volumes the range of times cited goes from 0 (5 times) up to 1103 for R.S. Burt’s Structural holes and good ideas from 2004. It seems to me that this paper is anything than an original contribution but just a follow up or a personal view back. On the other end there is a paper by two renowned scholars Rueschemeyer and Mahoney on A neo-utilitarian theory of class? from 2000 which got cited by others only two times. So, do citation say anything in sociology? If so, about what?

Yes, citations mean a whole bunch of things. The problem is that in any particular case, you don’t know which of those things a citation level means.

For example, sometimes citations are high because an article simply gives common sense a clever name. Mark Granovetter’s celebrated “embeddedness” article is an example. At the time it was published, many of us couldn’t understand why it was published at all. Its content was totally obvious. Any first-year sociology student knew that the economy was embedded in social processes. But the article became a useful “index” to that common idea, and it gets its thousands of citations because of that. I myself have a similar kind of trademark on the word “jurisdiction,” it seems. Indeed, you will

find that the most heavily cited articles in sociology are generally articles that are indexed by single words or phrases and are themselves often largely summaries of literatures or works beyond them: embeddedness, toolkit, weak ties, reciprocity, relational sociology, etc.

Or again, sometimes an article is heavily cited because it was the first use of a particular method – Tuma, Hannan, and Groenveld (AJS 1977) on event history methods, for example. Or sometimes an article genuinely does have a kind of new concept – Sampson and Raudenbush’s “collective efficacy” article is an example, although there too we see the importance of a good phrase. (All too many articles come to AJS with a phrase in quotes in the abstract; young authors know all about this need to coin a clever phrase.) Or again, sometimes a citation is just used to identify the citing author as a member of this or that school or subgroup. Or an article can be heavily cited simply because it is in a subfield that consists of lots of short articles with many citations. I once did an analysis of all references in sociology in a given year and it turned out that ten per cent of them had appeared in the *Journal of Marriage and the Family*! No wonder family demographers have high citation counts – it happens because of the shape of article they write.

So citations can mean lots and lots of things. In any particular case, however, we don’t know which one they do mean. As a result, citations as general measures and as inter-article comparisons are largely meaningless. It would helpful if people would all tell their deans this obvious fact.

AJS is one of the flagship journals in sociology worldwide but it is still the American journal of sociology? You explained your policy by calling US sociology an archipelago with some large island and a lot of small ones and AJS should mirror this. What’s about the international stance of AJS? Do you think people from abroad do have the same chance to get published or why are there differences?

The problem with internationalizing AJS is that AJS has for the last thirty or so years favored a certain kind or genre of article. This has a rough rhetorical form: 1. big theoretical puzzle, 2. broad review of literature(s) leading to a restructuring or focusing of the theoretical puzzle(s), 3. new data, 4. lengthy analysis (covering lots of details), and 5. attempt to draw general conclusions. Probably two-thirds of AJS articles are like this. It’s an American genre – indeed, even in the US, it’s just an AJS genre. AJS articles also tend to be large and (to some eyes) pretentious.

The problem is that other countries often favor different genres. The Dutch publish lots of very short, scientific articles, with very focused and limiting theoretical assumptions of a kind that disappeared thirty years ago even from the quantitative short-article form in the US. By contrast, the French often write articles with multiple methods, aiming to come to a “best understanding” of a given empirical situation, rather than an advance of a (supposed) theoretical trajectory.

I could go on. The fact is that there are (very loose) national styles of article, and “the AJS article” is a style that seems mainly American. During my tenure we published more articles from abroad, but they tended to be cast into the AJS genre.

I don’t think there is much to be done about this. There ought to be multiple genres of articles, and it makes more sense for journals to select for a particular genre than for a particular subject matter or methodology or theory or nation. So I feel that the internationalism question is actually hostage to the genre question.

Besides having been the editor of one of the leading journals in US sociology over the last couple of years you expressed more than once harsh criticisms against your fellow sociologists in the US.

Was your role as editor influenced by these opinions and in which way did you try to change the direction of US sociology via AJS?

Yes, Andrew Abbott the scholar and disciplinary figure has made a number of comments about other sociologists and sociological work over the last many years. But that has had nothing to do with what has appeared in AJS. I made no attempt to change US sociology via AJS, but aimed rather to make AJS reflect what was happening in the discipline in terms of areas and methods. The evidence of my success is that AJS is full of things I inveighed against as an individual scholar, but that are common, indeed valued, in the discipline: these are things like (to give them my personal names for them...) unnecessarily complicated methodology, illegitimate ontological assumptions, and internally contradictory theories. And we even published some things that I found repetitive, boring, one-sidedly political, and so on. My job as editor was to provide the discipline with a collection of excellent work in the styles that it – the discipline – chooses to favor. And I did so. My personal preferences as a scholar had next to nothing to do with the journal. Had they done so, it would have looked very different indeed.

Recently German sociologist Wolfgang Streeck commented approvingly on your editorial success when he claimed: “In American sociology we have seen interesting developments taking place in recent years. Many articles in the American Journal of Sociology now have an historical background and understand that the United States of America is not as a matter of course the universal model of modern society. Moreover, the economy, and indeed the political economy, is given an increasing role. Fewer and fewer sociologists today seem to be willing to abide by the peace treaty that Talcott Parsons negotiated with the Harvard economics department, defining the turfs of the two disciplines in such a way that they didn’t get into conflict with each other – in effect depriving sociology of some of its most important and most foundational themes.” (Interview in Sociologica 2016 (3):14, Doi: 10.2383/85816) Do you agree?

Yes, the journal has a more international flavor than it had forty years ago, and probably even more than fifteen years ago. And that is partly a matter of my policies, and of my predecessors’ policies. My own personal contribution to this trend was the Barbara Celarent series (of review articles of old social thought from around the world), which preoccupied me for the last six years that I edited the journal and which tried to open a space for social thought from outside Europe and North America. (Its success is evident in the 120,000 hits a year on the Barbara Celarent papers section of my website.)

But I think most of the new internationalism arises from factors much larger than the journal. First, there is much more international contact among sociologists, simply because of the vast increase in international travel consequent on US airline deregulation, the Schengen system, and a host of other “globalization” factors. As a result, European sociologists come to ASA, PAA, SSHA, and other US sociological venues, and vice versa. Second, EU funding has underwritten a large amount of comparative work, both within Europe and between Europe and the US, and this has produced a new emphasis on comparison that has reduced US isolationism. (This has been particularly noticeable in some of the ISA subgroups, like RC 28.) Third, “American sociology” is now very often not sociology done by “Americans,” in the sense of native-born citizens of the United States. American sociology departments are full of East Asians, Europeans, and others who are not native-born citizens. About a quarter of my own department is first generation migrants. If you were to include second and third generation migrants, that figure would be half the department.



In sum, I think internationalization is certainly becoming stronger, but in the main, the Journal's internationalization is just an indicator of larger phenomena. That said, both I and, I am sure, my successor, believe strongly that internationalization is a good thing.

As for the relation with economics, I differ from Streeck's view. Yes, it is true that economic themes have returned to sociology. But this is not really about the ending of any Parsons treaty with the economists. It's really about the peculiar history of economics in the last fifty years.

Economics as of the mid-twentieth century was still a general social science. Figures like Schumpeter and Knight, and, in the following generation, figures like Arrow, Samuelson, and Gerschenkron, were general intellectuals, thinking broadly about all social issues, and viewing economics proper as a way of understanding that part of the social world that was effectively measured by money. But Friedman and Becker – the new Chicago School, as opposed to the Veblen/Knight Chicago School – defined economics not substantively, but purely in terms of method and scientization. Gary Becker didn't really write a treatise about the family as everybody else conceived it, he wrote a treatise about an abstraction that was the family without anything in it that could not be "seen" by economic theory and methods.

Although this move was defined at the time as imperialism (e.g., by Hirschleifer), in practice this "methodization" had the effect of defining economics as a fixed set of techniques, and of defining economists as engineers rather than as intellectuals. It actually left the entire realm of political economy open to sociologists, political scientists, and historians. Becker and company simply didn't care about that area, and their methods had become such that they could – quite literally – no longer see any facts that controverted their view of the world. The result, sadly, has been the intellectual suicide of a noble and brilliant discipline.

But perhaps a better metaphor is that economics has turned into sort of reverse black hole, unable to receive any genuine message from outside itself. Intelligent economists are reduced to reinventing the social psychology of the 1960s and calling it "behavioral economics." Economists win Nobel prizes for "discovering" that people are sometimes irrational, apparently having forgotten that Friedman's classic 1953 paper argued not that everybody was rational, but that absolute rationality, although a nutty assumption, would prove profoundly interesting to make, at least for a while.

As for the sociologists who write about political economy, I myself think they are mired in Marxist analyses that aren't relevant in a world where economies and nations and classes crosscut each other in bewildering new patterns. I agree that great explanatory problems lie ahead. I just don't think that the nineteenth century armamentarium is going to help us much.

If you would do it again, what would you do differently?

At this point, I sometimes regret that I spent so much time editing the journal when I should have been writing my theory book. But on the other hand, editing the journal changed me a good deal. It made me more intellectually tolerant, it forced me to think extensively about the rest of the world (through Barbara), it gave me extensive experience with colleagues. So I wouldn't be the me I am today without having done it. And it is probable that the theory book I would have written - had I had more time to devote to it a decade ago - would not have been as good as the one I can write now, because I am older, which means that I have learned some useful intellectual lessons and that I write under more pressure of time and hence will write a more concise book. Of course, it's also true that I might have wasted the time I spent editing the journal doing other, useless things, so maybe there's nothing to regret at all.



A somewhat bigger regret is that I was unable to publish in the AJS for sixteen years. I write articles that are precisely the kind of article that AJS aims for, and - it should be noted – the kind of article that ASR is generally unwilling to publish (as I know quite well from experience). It undoubtedly hurt my reputation that for over a third of my career I was unable to publish in the sociological journal best suited to my type of work. I have published at least five articles and chapters elsewhere that would have been ideally suited to AJS (although, to be sure, who knows if my colleagues would have published them!), and a lot more people would have seen them in AJS.

But when it's all said and done, I would not have done anything differently. I had a wonderful time editing the journal. I talked to smart colleagues week after week about everything under the sociological sun. I had the friendship and support of a wonderful managing editor, Susan Allan. I got to read the work of hundreds of authors and of thousands of reviewers, nearly all of them thoughtful and admirable. I got to help authors - both those accepted and those rejected - to improve their work. I got to spend time with the dozens of graduate students who worked with the journal. If there was a "privilege" to editing the journal, it was all this, not any mythical ability to "shape the discipline." It was a privilege to have a three hour meeting, every week, dedicated to talking and reflecting with colleagues, both present and present-in-writing, about the nature of the social world and how we know it. It was a magnificent gift, and I am deeply grateful for it.

Interviewer: Christian Fleck.