To the Reader: This "interview" responds to questions posed to AA via the website http://www.socjobrumors.com/ , calling for a written response of some kind. The answers seemed useful, so I have posted the text here. AA

"AMA" Interview – Andrew Abbott

Andrew Abbott - I have perhaps taken the matter too seriously. But it seemed a useful exercise to think carefully about the questions behind the questions, so to speak. I have gathered the questions into four sets, since certain sets of questions revolve around similar areas of my work, and require common preliminary discussions before I get down to the questions themselves. These four areas are 1) Influence, 2.) AJS, 3) New structures in sociology , and 4) Questions I wish you had asked.

I – Influence (Q1, Q2, Q3)

Question 1. Your article "Transcending General Linear Reality" came out in 1988 as a critique, not of regression modeling per se, but of how the assumptions of these models crept into social theory. This paper continues to be taught in grad level theory courses. Has sociological theory and practice heeded your warnings since then? In what ways does the discipline continue to construe the world in general linear reality? What progress has been made?

I've pasted the abstract below as a refreshed/introduction for those unfamiliar:

This paper argues that the dominance of linear models has led many sociologists to construe the social world in terms of a "general linear reality." This reality assumes (1) that the social world consists of fixed entities with variable attributes, (2) that cause cannot flow from "small" to "large" attributes/events, (3) that causal attributes have only one causal pattern at once, (4) that the sequence of events does not influence their outcome, (5) that the "careers" of entities are largely independent, and (6) that causal attributes are generally independent of each other. The paper discusses examples of these assumptions in empirical work, consider standard and new methods addressing them, and briefly explores alternative models for reality that employ demographic, sequential, and network perspectives

Question 2. You wrote a great article about the trend in sociology vs. equilibrium in economics. In it, you urged people to take a "whole career" approach as an alternative, with more use of things like optimal matching. Is that still something you'd advocate? I think the idea is interesting, but investigating the things I want to investigate (large scale macro-historical change) it seems hard to get either quantitative or qualitative data on these sorts of careers and how they aggregate. You have that interesting article on sequencing and the adoption of the welfare state, but that stays entirely on the macro level, which is one solution to the dearth of individual career data. I can get something that resembles "Coleman's boat" (understanding how individual actions aggregate to macro events) on my subject, but I just can't imagine very many places where we'd be able to get whole career data, which seems to me leaves us stuck with trend and equilibrium for the most part. Are there other ways of thinking about social change that you're excited about?

Question 3. Regarding your book "Chaos of Disciplines (2001)" ... do you really believe that knowledge in the social sciences is non-cumulative? I'm pretty sure a lot of scholars have *taken issue with that specific claim and have tried to change your views on this particular issue. In sum, how has your position on the (non)cumulative character of the social sciences changed since 2001?*

Andrew Abbott - Q1 and Q2 seem to go together. Q1 is more or less "Has sociological theory and practice attended to the arguments of the article "Transcending General Linear Reality?" Q2 is in many ways a broader version of the effect question: nearly all the topics in Q2 concern substantive fields where I might have been expected to have an effect – occupations, careers, sequences. And one can't talk about impact without reflecting about cumulation, so Q3 goes here as well.

1. To assess my impact requires that I begin with my theory of how social science evolves through time, since that determines how I think about impact and cumulativity, which is the closing item in Q3.

My position on the non-cumulative character of social science remains strong, but is now more sophisticated than in the opening chapter of *Chaos of Disciplines*. I think there are three levels of "knowledge." There are "Rankean" facts that just pile up - things about which there is basically no question: the date of Jane Austen's birthday, the amount of money I had in Vanguard on 1 January 2016 before the markets opened, the number of shares transacted on NASDAQ on the first market day after that date, etc.

Above these Rankean facts are "generational paradigms:" local paradigms in the Kuhnian sense, with assumptions, preferred data, preferred methods, etc., and also with groups of adherents. Examples are status attainment theory, labeling theory, the strong programme in the sociology of science, linguistic turn, etc. These paradigms typically cumulate steadily for about twenty to thirty years, then fall apart or are deserted for (what are then thought to be) more interesting things. In part this happens through demographic succession: adherents get old and tired, while young people want excitement and novelty. In part it happens because the paradigms always start with a close match between empirical problems and analytic assumptions, and as these assumptions are moved to more - and more diverse - empirical areas (by the expansion and success of the paradigm), they work less well. Eventually the paradigm seems less compelling, and young people looking for excitement go elsewhere.

Above this second level is a third level of "categorical frameworks": things like methodological individualism, social emergentism, processualism, and dialectical conflict. These do not cumulate; they are general ways of thinking about the social world. Most of the time they talk past or ignore each other, simply providing alternative general philosophical frameworks for thinking about social life.

Sociology cumulates locally, in generational paradigms, but not broadly, at the level of the categorical frameworks. Categorical frameworks just tend to be restated, with new language. That is what I mean by saying that social science doesn't cumulate; there is no real improvement on the third level, at least in the sense that one or another of these frameworks gets disproved. By contrast, there is plenty of local cumulation, over limited intervals, in generational paradigms.

2. Given that view, how do we think about my "Transcending General Linear Reality" (TGLR) paper? It seems to have had *some* kind of impact. Using WoS unduplicated citations, TGLR has been cited about 200 times in 27 years, and *Time Matters*, of which it is the organizing chapter and which elaborates many of the same points, has been cited about 350 times in 14 years. So

something happened. But did that something help cumulation within some generational paradigm or affect the relations between categorical frameworks or change the relative attractiveness of one or another of those frameworks?

We can approach that question by thinking a bit about the citations themselves. Some of them came from people looking for polemical ammunition against positivist sociology. Some of them came from statistics teaching, where the paper is used as one of many injunctions to "be thoughtful about what you're doing." That is, much of the paper's citation was either polemical citation at the category level or hortatory ("get it right") citation at the generational-paradigm level. Neither of those uses involves any changing of minds among those already committed. In a way, though, that's because as far as statistics are concerned, the paper's argument was mainly against the causal interpretation of GLR. But most GLR sociology is really about policies, not causes, and as I noted in *Methods of Discovery*, regression is an excellent approach to deciding whether a policy works or not, as long as you recognize that it doesn't tell you about *how* it works. Policy evaluation is what Fisher developed ANOVA for.

The fact that regression can't tell us much about causality is why there's been such a turn towards experiments, counterfactuals, and so on. But my article had no hand in producing that counterfactual turn, which is directly attributable to Donald Rubin and his followers. To the extent that there was a focused attack on inappropriate causal claims in social statistics, it was led by the late David Freedman of Berkeley. Freedman and I had a cordial relationship, as laborers in the "causal analysis is often dubious" vineyard. But Freedman was the one who supervised the plantings and made the wine; I just pushed a wheelbarrow and helped drink the produce. My main argument in TGLR, after all, was more about the effects of GLR on our conceptions of social ontology than about its effects on conceptions of causality.

To be sure, one of the alternate ways of thinking I urged in the TGLR article - networks - took off like a rocket in the years since that time. But again this owed nothing to me, but reflected mainly the advance in computing power that made estimation of large network models possible, when for many years they had been a boutique specialty of Harrison White and his many brilliant students.

In summary, in TGLR I wrote an introduction to how to think about the ways in which our uses of applied mathematics commit us to models of the world. I probably influenced some young people who were at the life-stage when one thinks about such issues. But for the most part, the article expressed reservations that had been stated long before (by Herbert Blumer in the 1930s, for example, in immediate response to Samuel Stouffer's applications of the new statistics) and that are still being reexpressed today. It was really a categorical exercise, a restatement of a long and standard view at the categorical level. The life cycle of such a piece is that for a while it gets a lot of decorative citation, but eventually some younger person will come along and say what is more or less the same thing but in new language. That person then will take over the role of standard decorative citation for this perennial argument, just as I did almost thirty years ago.

3. The various topics of Q2:

a. The trend vs equilibrium question refers to my paper on outcome, which argues that one always makes a particular philosophical value judgment when one decides how and when to measure "outcome." (It's the same kind of argument TGLR makes about ontology and causality.) This value judgment may then decisively affect how one goes on to think about things like inequality, injustice, etc. Thus, the outcome measurement decision involves the *practical* or *policy* side of generational paradigms, and so I think my calling attention to the very great variety in possible outcome measures could have a big effect. So far it has not had much effect at all. (The paper was rejected by both ASR and *Social Forces*. Nobody was even willing to think about the problem that our conventions about outcome are not value-free.

b. Whole careers. Yes, it makes great sense to compare whole careers rather than single moments in careers. And the life course literature is finally using optimal matching to do that. It only took about twenty-five years. (I first started using sequencing algorithms in 1982 and first published them in 1986.) In the early days I just applied the sequence approach to whatever showed up that seemed interesting. That's what you do when you're thirty-five, and that's where the welfare state paper came from: the "life course" of countries' involvements in welfare programs. But today, there is a lot of whole career data around: PSID and NLSY both have plenty of it, although the occupational coding can be problematic. Internationally, there are lots of datasets with long stretches of career data.

c. The welfare state article you mention offers an instructive lesson about how interesting projects can involve one in contradictory assumptions.

In that paper, I first did some optimal matching on sequences of the adoption of welfare programs, and nothing much showed up. There were no typical sequences. But then I remembered that the cases were not independent. Welfare policies could propagate at international meetings, or from WHO, WLO, etc. After all, my day job at the time was to write *System of Professions*, of which the main idea is that historical sequences are not independent. So I looked at diffusion of policies among the welfare states, by creating a matrix of inter-state proximities based on various properties and then comparing it with the matrix of welfare-state policy-sequence distances from optimal matching. (This comparison was done by what was later called QAP, but in those days you just programmed such things yourself. I got the idea from Phipps Arabie, Larry Hubert, and Ron Breiger. It took my PC a whole weekend to do the calculation.) But the diffusion approach didn't work either, so the paper finished with the idea that, essentially, country sequences were produced by random draws on "intensity functions" (like event density functions over time) that indicate how likely a given policy was at any particular point in time, as estimated by a moving average of the actual adoption dates. That final model - a kind of probabilistic (rather than institutional) world polity model - ended up seeming the best.

So whole careers is a fine approach, but it does run afoul of the problem of interdependence of cases and the possibilities that that interdependence changes over time. In the case of welfare states, N is small enough that this interdependence can matter a lot. When we are talking about careers in the labor force, case interdependence is less of a problem – there are a lot of cases and they are not likely to affect one another. In the labor force, the lack of independence issue lies rather in the marginals of the occupational distribution, which are driven by technology, the division of labor, offshoring, and other such things. Because stratification research has been mainly policy driven (with the main policy question being "are we 'really' egalitarian?") it wants to control those marginal effects, so it can ask about mobility that is not structural. Hence we use proportional adjustment and log-linear methods, which do that controlling.

But in everyday experience, those (controlled) marginals are what matter. You can't become a farmer if farming is disappearing as an occupation. Put another way, people may decide what to do on the basis of marginal reward (in the economic sense of "marginal," as in “marginal returns,”) but they live their lives on the basis of average reward. That’s why overeducation is a looming American problem. At the margin, it makes sense for an individual to get a college degree. But society-wide, the result is that the labor force has about twice the percentage of BA workers that are necessary to fill the BA jobs in the labor force, a situation that has not changed in twenty-five years.

More broadly still, theorizing individual careers *and* the labor force *at the same time* continues to be the great theoretical challenge for me. (I am rethinking the approach taken to that problem in my mobility paper of 2006 in the Morgan/Grusky/Fields volume.) I understand why other people want to focus on one side (individual careers viewed as independent experiences) or on the other ("macro" changes in the division of labor). But putting those things together arbitrarily simply means you're using up the total degrees of freedom twice - once in the pure career models, once in the DoL models. Hierarchical models do not resolve this, because the occupations and the individuals are in some sense on the same level: individuals move across occupations, not within them. That’s also why, despite my admiration for Jim Coleman, I don't believe in the Coleman bathtub. And it is probably also why I have never had much practical impact. My unwillingness to make simplifying assumptions and to go along with usual conventions of analysis guarantees that my work in this area of careers/occupations/divisions of labor remains too esoteric for practical effect.

d. Interestingly, even *System of Professions* with its 2,700 WoS citations has not had the impact one might imagine. Everybody cites the book, but very few pay any attention to its major argument, which is that it doesn’t ever make sense to analyze professions’ histories one-by-one. In fact, most articles about professions still write about their histories one by one. The book contains three chapter-length examples of how to study professions correctly. But how much influence have those examples had? Almost none. For an article on the subject of ignorance (it’s in *American Sociologist*) I once read all the articles containing citations to *System* in a single year (2008, I think.). Most of those citations were unnecessary, and most were ignoring the main point of the book. I’m highly cited, but less influential than you might think.

Perhaps a better way of putting it is that the level of influence one imagines when one is young is probably not actually ever experienced by anybody. Even the things that appear most influential have only a small fraction of the influence we might imagine. That, too, I think is partly a result of professional demography, which I discuss more in answers to later questions. In the rapidly expanding academia of pre 1975, huge influence was possible because a few senior people were surrounded by many young people ready to be influenced. In the flat age structure of today’s academia, most readers already have their minds made up about most things, because the average colleague-reader is much older.

e. What are other ways of thinking about social change that I am excited about? I’m excited about the same old processual approach that I’ve been espousing since the 1990s. But I’m still working out the details.

II - AJS (Q 4, Q5, Q6)

Question 4. As an editor to one of the top journals in sociology, what are some of the things you are looking out for in manuscripts and what are some of the common mistakes people should avoid when submitting to AJS?

Question 5. There is an ongoing critique, most recently by Peter Bearman in the last AMA, about the state of sociology journals. Part of the problem is the over-emphasis on literature review and excessive citations and multiple R&Rs, much to the detriment of speedy publication process. Hence we see sociological journals with a radically different structure such as Sociological Science and Socius. Do you see AJS slowly transitioning away from the old model to the new one? If not, what are some of the justifications for the old model that AJS appears to be adhering to? Alternatively, what is lost if sociology journals adopt the new model?

Question 6. There has been some criticism directed towards AJS recently from this board (SJMR) and from some prominent sociologists that has centered on the journal's long wait times for responses and on its alleged lack of transparency and accountability. Some of this criticism may have been exacerbated by the fact that there is no rotation in editorship at AJS. Are you aware of these critiques? How have you tried to mitigate some of your own potential personal biases as editor? Are there checks in balances in place in AJS? If so, what are they? Do you have plans to change the current model and/or relinquish or share editorship of the journal anytime soon? Why or why not?

Andrew Abbott - I can't answer questions about AJS in a vacuum, since I'm well aware that a statement I make in any public setting will be treated as if it were an official policy. So I begin with a general statement about the journal, then move to the detailed questions.

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 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. My job as editor is to choreograph and administer a selection process that produces a journal as full as possible of papers that met those two standards of 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 likely 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. We have to bring about that representation 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 discipline-wide representativeness happen through a process of individual decisions on papers one-at-a-time. We can't sit with a spread sheet and say "we need a paper of type X from area Y using method Q to fill in this box." We simply have to constantly bear representation 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 other constituencies - authors and referees. To authors we have an obligation of fair and timely process. And secondarily, to authors, we have the obligation to teach at least 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.

To reviewers we have an obligation 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 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). 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 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 was 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 should make 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 not to submit because I am editor, on the *belief* that I must exercise some grand authority, or that the 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. Earlier in this interview, I was asked about TGLR – a paper questioning one whole line of sociological work. 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 (for example, there 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 attempting 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 our own 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 the 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 up" departments (five in the first case, four in the second) substantially above other departments. A few changed decisions 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.

5. Specific Questions

a. Articles and Mistakes:

What should articles look like? The short answer is simple: we are looking for articles that fit the standards described above. The way to be published in AJS is to write an article that introduces new theory, new method, substantial new data, or that makes a decisive intervention in an ongoing debate, AND that is interesting not just to its subdisciplinary community, but also to a more general sociological reader. The way to get published in AJS – or in ASR, or SF, or *Demography* or ASQ etc. – is to write a good article.

Similarly, there is a short answer about "mistakes." Asking whether there are common "mistakes" in AJS submissions seems to imply that all articles are potentially publishable in AJS, absent some particularly damaging "mistakes." But that is not true. There are thousands of fine articles published in sociology each year that do not do the things just listed (present new theory, new data, new method or make a decisive intervention). For example, there are articles that are excellent but standard contributions to mature literatures, articles that are careful reviews of literature but without theoretical ambitions, articles that are adding new individual cases to a familiar case literature - in short, articles that are more appropriate in the many fine specialty journals. The most common "mistake," if I have to put it that way, is to send AJS a paper that - whether good or bad - belongs elsewhere.

While it seems obvious to say so, the best guide to what an AJS paper should look like is what AJS papers DO look like - the papers in the published journal. And the most common "mistake" in AJS submissions, put simply, is to send us a paper that doesn't look like the papers we do publish.

b. Complaints

I get plenty of feedback on AJS without needing to go to the web to look for it. The vast majority of that feedback is positive and concerns the quality of the reviewing. We get dozens of thank-you notes annually, mostly from people who are rejected. In fifteen years, I've gotten perhaps five flame emails, and there have been only two people whom I once knew personally who refuse to speak to me since their rejections. Given that on my watch AJS has turned down thousands of authors - including hundreds of my friends, students, colleagues, and acquaintances - that's an indicator that we have a careful process, that we obey it rigidly, and that our colleagues respect our commitment to doing the job as best we can.

The main complaints about AJS concern waiting time. The waiting time has become hard to handle.

i. Some of this reflects software problems - most journal software does not really protect author identity. The Press software is no different in that respect, and so we have to do a lot of preprocessing in the office to guarantee that every page that goes to a reviewer has a confidentiality statement and that the file has been stripped of all those hidden identifiers that your software puts into your articles without your knowing it. To be sure, all this work may actually be a waste of time. We estimate that over 90% of papers submitted can be identified from the internet from their text alone, largely via authors' own web pages. Most reviewers don't make that identification, by the way, but it is still possible. So perhaps we should be less rigid about confidentiality, because it is very time-consuming. But we ourselves haven't changed our attitude about confidentiality yet, and the vast majority of our reviewers have not changed either.

ii. Historically, the main cause of long times to review has been the electronic solicitation of reviews. Acceptance of reviewing tasks plummeted when the electronic systems for soliciting reviews came in. As I noted earlier, in the last years we were operating in the mail, on 80% of the papers we got three reviewers with no more than four requests. That's because it is hard to say no when the paper just shows up in the mail. But online, one or two clicks and you've refused. And, moreover, there are far more requests for reviewing, from journals both near and far, because asking is now costless, which it was not in the mail era. So refusals have risen sharply, as I noted earlier. Every time a reviewer refuses, it adds time to the paper's total, because we have to check the further list of suggested reviewers to make sure they haven't been asked to do something in the meantime, etc. And often potential reviewers hold onto a paper for weeks before saying "You know, I'm really not the right person." All that time goes onto the meter. Any paper that goes beyond four months with us is a paper where several reviewers have changed their minds about reviewing. We have gone as high as sixteen requests to get three reviews.

There are some strategies for these new problems. First, we could desk reject more papers, but that has its own problems. As I noted earlier, many colleagues - particularly those without easy recourse to good professional advice - need and value the advice that reviewing gives, even when they get rejected. Second, we could move to "review lite," as some journals have done. But our reviewing is really the heart of what we do, both as a service to authors who get comments that – as they often tell us – greatly improve their papers, and as a service to referees who often tell us they enjoy participating in what feels like a communal disciplinary activity. Third, we could be more stringent about the departments that push second or third year students through the exercise of submitting a paper to a top journal, with little expectation of publication, but getting good reviews from thoughtful people. This is free-riding, using up the AJS referee pool for graduate teaching that should be done in the submitting department. So we could get more stringent with student papers - we do get a lot of student papers from particular departments. Fourth, we are always trying to streamline administration in the office and eliminate any delays in decision.

Mainly by working hard on various aspects of administration and finding new reviewers we cut the AJS median review time by nearly a month this past year. It's still higher than I want, and the tail remains worrisome. But the journal is doing much better.

iii. We are getting more appeals of decisions than before. This too is simply because of the ease of electronic appealing – people just send me a personal email, most of the time.

We have confidence in our reviewers and confidence in our process. So appeals almost never result in changes of decisions. Otherwise, of course, we would get dozens of appeals a week and would be making all of our decisions two and three times over. What matters is that we have a rigorous process and follow it carefully. If our process makes mistakes, then an author has every right to prove us wrong by publishing elsewhere to great acclaim. When that happens, we will happily own up to our mistake.

Although I do not know how authors feel about appeals, it seems to me – both from reading appeals and from reacting to my own reviews from other journals – that even with senior scholars appeals originate not in actual problems but in the structural consequences of double-blind reviewing. It is inevitable that an author feels somewhat trashed by the reviews, even when those reviews are in fact careful and constructive. Double-blind reviewing strips out paralinguistic cues that would gentle the comments if they were delivered face to face. Many referees try to compensate for this by beginning their reviews with careful summaries of a paper and compliments for its strong points, which I think is a very good thing. But even that sometimes doesn't work, and an author is hurt, and then angry. Sometimes, an author thinks he or she has identified a particular hand that is stabbing him or her in the back and announces that identity in an appeal. Authors are almost *never* correct about these identifications, in our experience, and they are also almost never correct about the ill will of reviewers. It is an overwhelming fact, as I have learned from reading thousands of reviews, that the vast majority of our colleagues are truly excellent and concerned reviewers. In fact, 99.9% of the time, our colleagues write solid, constructive reviews. It is simply the structure of and de-contextualization of double-blind reviewing that creates the ill feeling.

c. Abbott as editor? I endure as editor for a number of reasons. Some of them are positive: I have very broad tastes. I've published a lot of different kinds of work. I have the connections to call in top quality reviewers suddenly in case of really tough papers. I've edited the journal for a long time and therefore know how to read reviews (I have read about 20,000 of them.)

Others reasons that I continue as editor involve disincentives. Editing is time-consuming. You have to say a lot of negative things to good friends, students, and colleagues. You have (increasingly) to deal with appeals and other unhappinesses. There's no salary bump, no extra research fund, no perks other than a course off. And intellectually, one cannot make the journal "personal" in any sense; the department would (quite rightly) remove an editor who tried to steer AJS towards his or her own interests and hence change the journal from the "general purpose" mission I stated earlier.

When I interviewed Peter Blau about his editorship (in the course of writing *Department and Discipline*, he said, "I was given the AJS editorship as a kind of booby prize with tenure." In fact, the journal editorship has long been regarded as something of a chore. It happens to be the right chore for me given my situation, and my colleagues seem to think I do an OK job. So that's where it sits.

d. The new model (Question 5 – What are my views about old and new models of sociology journals, and in particular *Sociological Science* and *Socius*.) It's quite possible to have different kinds of journals. The natural sciences have PNAS, for example, which is basically a non-refereed sponsorship journal (articles must have sponsors who are members of the NAS) that is used for the purpose of establishing priority in highly cumulative fields where priority is believed to be establishable and important. Economics has the working papers series of the NBER for much the same purpose. Some people think sociology is like those disciplines, some don't.

The Board of the AJS and, more broadly, the Department that founded and that maintains the journal, both believe that there is room for a journal like AJS as currently managed, with the aims and processes that the journal currently has. If other people want to run journals of other kinds, that is their business. Most major disciplines have many different kinds of journals, and sociology is unlikely to be different.

III – New structures in sociology and my relations to them, particularly younger colleagues, students, etc. (Questions 7, 8, 9)

Question 7. How have the kinds of students who pursue a sociology PhD changed over the years? How has graduate advising changed? How has the sociology job market changed? What impact, if any, do you think that these changes will have on sociology as a professional discipline?

Question 8. In the acknowledgements for "The Explanation of Social Action", John Levi Martin says "Andrew Abbott's work blazed the trail followed here—Τοὺς πρώτους ἡμῶν ἐξέλυσε".

Here's a comment from a weird online forum dedicated to Greek translations by a certain jlmartin5: "Hi! I need to translate "He set the first of us free" into ancient Greek. This is to be an acknowledgement in a book of someone who I think will get a big kick out of being talked about so nicely, but...it will drive him crazy trying to remember which philosopher said this, when it's actually from The Matrix." The question is: do you currently suffer from some sort of prank-induced craziness? The real question, however, is: have you been influenced by younger sociologists in recent years? if so, are these influences going to show up in your next big theory book?

Question 9. Dear Professor Abbott,

*Do you like rap music? I wrote you a rap inspired by my favorite rapper, Lil B (hope you like that wonton soup!): I'm back in the habit, I'm back with a couple habits, charms and I'm silly wabbit, fiendish like fanatics, feeling groggy while I grab it, Ritalin charge me like I'm plastic, SJMR got too much traffic, but I'm a pro like Andy Abbot, his articles all classic, AJS is sort of magic, with it R1 automatic, a job electromagnet, without it feeling tragic, man the market makes me half-sick, so homeslice's latest tactic, the world is mine I'm Jurassic, finna do something drastic, I'm like go go inspector gadget, only top two when I draft it, cause SC give much static, their decisions all erratic, their decisions are all half-bitch, "please read the submitted packet", "nah 'top 2' or you can get s**t, your career's in the casket", that s**t seems like a racket, an ASR would be fantastic, move me up a few tax bracket, an AJS and I'm ecstatic, ecstasy and I'm romantic, but these ho's won't let me have it, no these ho's won't let me have it.*

Which leads me to my second question: in most subfields (ethnography and heavily computational work might be exceptions), an ASR or an AJS is all but required for the "top jobs" for junior faculty. By all accounts, it hasn't always been like this. What is your take on this development? In departmental hiring of junior faculty at Chicago, do you tend to *support this norm or (knowing how the sausage is made) push back against it? In my own department, I've seen students put a lot of effort in developing an "ASR", rather than developing a broader range of articles for a broader range of publications. For many student, it becomes a heavy gamble where they invest most of their time and energy into a single make or break article in the time leading up to going on the market. I assume you've seen and, as an adviser, encouraged this sort of strategy. This is, of course, an easily understandable sociological process (ASR and AJS essentially become credentialing services for "top candidates") but it seems to me create incentives for a very specific kind of work. What's your take on ASRs, AJSs, job market candidates, and the state of the field?*

Andrew Abbott - 1. With these questions about the nature of the field and my relation to younger scholars, it is important – as in section I – to say some general things about the nature of academia, since they shape my thinking about new structures and younger colleagues.

From 1890 to 1970 US academia expanded more or less exponentially, with breaks for the First World War and the Depression / Second World War. As elementary demography tells us, this meant that in those years academia was very young, with lots of grad students, a medium number of exciting middle aged folks, and a relatively small cadre of senior leaders. In such a demography, novelty was easy, because whole topics were unexamined by the smaller earlier (senior) generations. Theory stood in reasonable relation to empirical work because, in effect, the numerous young people applied the theories of the fewer older people. There were not dozens of competing theoretical systems and terminologies, but just a handful of grand schools of thought, usually associated with the elder statesmen of dominant departments. And as late as the early 1960s, sociology, like most disciplines, derived about 50% of its PhDs from 15 departments.

After 1975, expansion stopped and academia moved to a pure replacement demography, in which one scholar must leave for one scholar to enter. The age distribution became flat. By comparison with earlier periods, we have fewer young scholars and more old ones. But we have not revised our concept of the scholarly life course. As a result, we have too many people producing grand theories (because there are relatively more older scholars) and too few reading them (because there are relatively fewer grad students and assistant professors). At the same time, the immense size of academia has burned up dissertation and empirical topics very rapidly; only our continuous reinvention of empirical methods (and our calm ignoring of huge amounts of “dated” work and “irrelevant data”) allows us to avoid the quite evident fact that we have investigated most topics dozens of times before.

To be sure, the demography of the very top of this system has changed less. There are still ten to fifteen dominating departments, as there were in 1975 and, in fact, in 1955. They are not as dominant in PhD production, but they remain dominant in other ways. There are PhDs produced elsewhere in the system, but they are not really competitive for the top departments, which hire mostly from themselves. Neither the size nor, for the most part, the identity of these institutions has changed. Considering size, for example, Chicago's on-sociology-budget faculty in 1955 numbered 12 (4 Prof, 2 AssoProf, 6 AsstProf, but there were 5 associated professors of sociology with voting memberships, for 17 total). In 1975 there were 22 (15 Prof, 2 AssoProf, 5 AsstProf, but no associates with formal professorial titles). Today, the faculty number 23 (12 Prof, 6 AssoProf, 5 AsstProf. Thus, aside from some slight deformations induced by faculty staying employed a little longer (but they're starting later) and by the demographic signatures left by past rapid expansion, there is the same demography today in the elite departments that there was four decades ago, in terms of places and dynamics. Non-expansion in the elite has meant that the “age problem” – an age structure incompatible with earlier conceptions of the typical scholarly life course – has an even longer history in the elite departments than elsewhere.

The competition was therefore just as tight in the elites forty years ago as it is now, but the materials of competition were not AJS or ASR articles - there were no more than two or three of each per year by graduate students (even dissertators) in the 1970s. Competition in the 1970s happened through dissertation chapters that were read (!) by hiring departments, by serious recommendation letters, and by a kind of insider competition between grand patrons peddling their latest young hotshots.

We're sociologists, so I don't need to tell you what has happened. Every one of these appraisal systems was watered down because of the intense pressure created by the new buyer's market that was caused by the end of exponential expansion. The first crack in the article system came in the late 1970s when grad student assistants on quantitative projects, who used to be in the acknowledgement notes of articles, started showing up as authors. There was no change in division of labor, just in labeling. Second, the recommendation letters turned from real appraisals of strengths and weaknesses to symphonies of superlatives that ought to make their authors ashamed of themselves. I don't know when the letters began to be watered down, but they are today completely worthless. As for phone calls (today, emails), the discipline is too large for such things to matter. There is a loose kind of elite, but it does not have the cosy closeness that still existed as late as 1975. The dominant folks in sociology in 1975 had gotten PhDs in the 1950s, when the entire discipline numbered about four thousand, and the elite nucleus comprised people who had gone to about five major departments. Now the discipline is fourteen thousand, and has been for a long time. And the elite nucleus comes from a broader range of places. Moreover, the multi-paradigm character of the field has undercut hiring departments' ability effectively to read dissertations that ranged across widely different areas.

The result of all of this has been to place an ever bigger premium on publication and to force graduate students to publish at levels of professional development far earlier than those at which *our* professors thought *we* were ready to publish. I get dozens of papers a year at AJS that are in no way different from the papers I and my peers wrote in graduate school. We threw them away, because they were mere exercises. But today’s students send them to AJS or some place similar. Published papers have become the only means effectively to mark progress, faute de mieux. There is nothing the discipline can do about that problem short of getting much more serious about reading the dissertation work of candidates (but that has the problem of internal consensus on rankings) and of following the late Roger Gould's advice that "we should say we will read only seventy-five pages for tenure." That is, we could tell every job candidate we want thirty truly excellent pages, not a whole pile of random stuff.

As question 9 suggests, from the student point of view, to avoid catastrophic loss it is probably a better strategy to publish a bunch of articles outside AJS or ASR, rather than to risk all on a paper in the elite journal market, with its much greater likelihood of failure, because one is there competing with the best work of one’s older and much more experienced colleagues.

Most important, however, it is a mistake to think that a job in a “top department” is the only avenue to a great (or even a fulfilling) career. Many people have great and rewarding careers outside the summit departments. There are dozens and dozens of departments of sociology in the US that have ten really talented and interesting colleagues in them. And many people who end up at the truly elite departments began elsewhere. And many who begin in those truly elite departments finish their careers – often quite happily – elsewhere. We are in sociology because we love it as a form of learning, not because we want this or that form of status. Yes, you can argue that "that's easy for AA to say." But I have had smart colleagues at both places I have taught, and I am well aware that had luck been different I could have spent my whole career somewhere other than Chicago and that that would not have been the end of my life. Perhaps I might have focused on my own work more had I gone somewhere else, or stayed at Rutgers. Who knows? Perhaps I would be *better* known or done *better* work had I followed some other trajectory.

Do good work – that is the only thing that is its own reward.

2. Specific questions

Question 7 – Changes in types of students, grad advising, job market. Larger consequences for discipline

a. type of students . The first change was towards women, who now dominate most levels and parts of the discipline, as indeed they do most academic disciplines outside STEM. I haven't studied this change well enough to have a sense of the causes and consequences, but I can say that I wouldn't believe most secondary analyses of those causes without primary level study of them myself. Most secondary analyses of this change are hopelessly political.

The second change - to me today more noticeable - is toward international students, who were almost invisible in my own graduate years and constitute nearly the majority of the graduate students with whom I work today. Again, I would need to study the exact numbers and patterns of this change in detail to speak of it in detail.

A third change that I suspect but cannot verify for sure is that sociology is drawing fewer students with strong mathematical backgrounds than it did forty years ago. In part, this reflects the move to canned statistics, which happened while I was in graduate school, and which meant that *all* of the mathematical foundations of our empirical analyses have become more obscure to practitioners. But that said, I do have a sense that the really serious math-heads in the social science are more and more sucked into economics. I'm not sure we're finding the Leo Goodmans, the Dudley Duncans, the Jim Colemans, and the Harrison Whites. I could be wrong, though.

A fourth change, for which the evidence at Chicago, at least, is very clear, is that graduate students are older, that fewer of them have come straight from college, and that, as those changes would predict, many more of them are married and now some of them have children (almost none had children in the 1970s when I was in grad school.).

All four of these changes (gender, national origin, math, age) were already under way by the time I arrived at Chicago as faculty in 1991. The main structural change since then is the emergence of postdocs, which 25 years ago existed only in a few quantitative areas, and then only rarely. Now 1. postdocs are considerably more common, 2. some of them come with money that moves with the student rather than the supervisor (that is, the student finds the supervisor and applies from a central pot), and 3. there is a reinvention of "teaching post-docs" on the qualitative side that (sort of) recreates the old cannon-fodder assistant professorships that used to be characteristic of places like Harvard. The postdoc phenomenon has come alongside a Mellon-induced pressure to shorten the PhD, not quite to the overgrown MA that is characteristic in Europe, but still, to a length that seems to me (and many others) too short to produce a real scholar.

Of these changes, I think age and national origin are the most important. Advising a 27 year old person who may have substantial work experience and extensive cross-cultural knowledge, and who may be a second-language speaker of English, is very different from advising an American 23-year-old less than two years away from college, without much experience of the social world beyond educational institutions.

But the general issues of graduate school are still the same. You have to help a student find a way to deploy the character and talents he or she has, helping your student create a self that has a feasible identity as a working social scientist. Every student is different. Some need much supervision, others less. Some need calming, some need prodding. Some need self-confidence, some need self-restraint. And their lives are constantly changing as they experiment with scholarship and theories and methodologies (not to mention partners and roommates and jobs and advisors). That stuff never changes. You never fail to be surprised, to miss things, to play catchup, to try to help students out. So I don't know, really, how all these changes are going to affect the discipline, but I do know that getting a job at Chicago was, for me, like winning the lottery because of all the adventure - sometimes crazy, sometimes painful - of teaching graduate students. Hopefully my students will figure out how to do it in their time – indeed I'm confident they will.

Question 8 . Do I suffer from prank-induced craziness? Well, my Greek is rusty enough that I had to look up the first aorist verb in that phrase. But I neglected to worry about which philosopher it was from, and thereby failed to be driven crazy. And I've never seen The Matrix, so I couldn't have known about that.

The "real question" here was "have you been influenced by younger sociologists in recent years?" Well, in an indirect way, yes. I have spent every Wednesday for the last fifteen years reading, reflecting on, and talking about articles they want to publish in AJS – most of the people who send pieces to AJS are a lot younger than I am. So the reading I do for AJS inevitably teaches me things about what young people are thinking, how they are addressing questions, what questions they think important, and so on. Beyond AJS, I have many students, in widely varying areas, and they are always pushing me to read this or that. And the process of searching for new possibilities for hiring leads to plenty of reading as well. Like any senior academic, I'm inevitably influenced by all of that input. And I do occasionally read material because it's immediately relevant to something I'm working on, although given that my empirical work over the last five years has mostly been on the sociology of libraries, the social sciences, and the humanities, there's not a lot for me to read in sociology itself.

What kind of an effect does all this have? I don't know. It probably does not influence my core thinking much. In fact, I haven't read any other theorist systematically since the early 1980s. Once you begin to develop your own way of thinking about things, other theorists' work seemed to require a lot of translation time (into your own system) for relatively little reward. It is more efficient to think things up on your own.

Of course, such a position is only for someone like me, who has a desire to develop a coherent logic, whereby all of my work makes sense as part of a single common project. Most sociologists don't need to worry about that, because personal consistency is not the only meaningful criterion for a scholarly life; moving across fields and paradigms provides interconnections that the discipline needs, for example. But for people like me, other people's theoretical work gets in the way of the consistency we seek. I do however read immense amounts of non-theoretical material – history, literature, empirical work from sociology, anthropology, political science. I read that to find examples, to think through empirical cases I need to theorize, to test my argments, and so on.

The way I see it, doing social theory is a bit like being a composer. There are composers who borrowed ideas, motifs, and even larger bits from others, and who in that borrowing transformed the borrowings in astounding directions. G. F. Handel is usually considered to be the great example of this procedure. I'm not like that. When I was younger I learned theorizing by working my way through some of the theory classics in excruciating detail, reworking the logic, testing the inferences, etc. I was like J. S. Bach copying and reworking Vivaldi concerti as a personal discipline. But now I just do my own stuff – write my own music in my own way. Perhaps my insistence on personal consistency is a bad idea – I could end up like Paul Hindemith, producer of a monumental and consistent body of work that is so recognizable that after five or six measures it can be easily identified. Many people find it boring, perhaps because of its consistency and idiosyncrasy. But it worked for Hindemith, and that would be good enough for me.

I'm of course far below the level of any of the people just mentioned, but they provide useful examples for me. It works (for me) to think of my sometimes embarrassing disattention to other theorists as analogous to a composer's writing for him or herself.

Question 9 – I don't know anything about rap. It came after my generation passed the age when you always make it a point to listen to new forms of young people's music. (I think that age is probably some time in the mid thirties. For me, it was about the time when I no longer taught intro sociology to 500 students at a pop and could give up on au courant class examples.) So I'm flattered by there being a rap here, but don't know what to make of it. I'm a little worried about the mention of a "casket," which I am much closer to occupying today than I was back when I taught intro to 500 Rutgers undergraduates.

But the heart of Question 9 is about a lot of strategic details vis a vis the sociological career. It seems to me that if we are mainly interested in the pursuit of status, most of us would be far better off investing our time and talents in some other field of endeavor. Academics in general has always been a fairly competitive world and the expansion that concealed that competition – outside the elites at least – has come to an end. So if status is your concern, you should do something else, where status is easier to get. If we are in this academic racket for anything, it is for love of the work. Anybody who can get a sociology PhD could go into the private sector and parlay his or her analytic skills into five times the money - and for a shorter working day. The reason for doing sociology is because you have to, because you don't make sense to yourself any other way. If you don't think that, you shouldn't be in the sociology trade.

IV – the questions I wished you had asked

Question 10. What's the answer to a question that you wish we had asked you, but didn't?

Andrew Abbott - The question you didn't ask is the most important one: What do I think are the great issues confronting the discipline today, intellectual and institutional? The very fact that this question was not asked is worrisome to me. To be sure, I think I'm correct in believing that this blog is mainly a forum for people in first career stages, and it is not surprising that the questions are focused on careerist issues, either for you as readers (are the journals fair, are they really helping the discipline, are there new types of publishing?) or for you as people thinking about individual careers (is AA played out as an editor?, has AA's work had an effect?, what does AA think of the job market), etc. But really, the only interview question directly aimed at the large-scale future was the question about cumulation.

But since you - the young people in the discipline - are soon going to be the custodians of this little chunk of academia called sociology, you need to have much greater ambition than that. You need to be asking: Will sociology survive at all? Will universities in their current guise survive at all? How can sociology deal actively with the transitions that come with the digitization of everything? What are the intellectual questions worth spending a lifetime addressing? You need to be asking my generation whether we feel we were a success or a failure - as a generation - and why and how that happened. So here are some big questions you might think about asking not just me, but lots of people.

1. Will intellectual life survive in universities?

As I noted, the end of academic expansion in the 1970s led to a buyer's market for academic talent, a competition-forced watering-down of all academic currencies (publication, authorship, etc.), and, in consequence, a proliferation of unimportant publication. This proliferation has more or less destroyed the academic communication system, which has become hostage to the function of evaluation. That simple historical reality provides the basic mechanism behind all your questions about academic journalism.

The average professor of whatever, prior to 1970, published one or two things in a lifetime. That's not because he was a dope or lazy or untrained. It's because he published only when and if he felt he really wanted to publish. Today, professors must write all the time to satisfy personal personnel evaluations and, now, even worse, in order to support their universities' attempts to crawl up in the meaningless rankings. Because of the ensuing publication epidemic, nobody is actually reading much of anything. They're scanning, pillaging, lifting – the internet favors very superficial engagement with material, looking for particular bits useful in one's own current project. To anyone who has worked in academia for at least twenty years, it is obvious that this kind of behavior (both the overpublishing and the new forms of "reading") are driven by a new, neoliberal management style, and that that style is more or less destroying the old forms of intellectual life in the university. It may well already have done so in the sciences, many of which seem to me to be rapidly degenerating into mere engineering. Sociology may be lucky because we are so unimportant.

In my view, the entire system has to change or real intellectualism will die in the academy. The future is not about whether *Sociological Science* sets a model AJS should follow. The future is about whether *any* journal is going to contain anything other than mechanical production, for purely personnel-related reasons, by scholars who would really rather be thinking and reflecting, and who would prefer to write only if they feel genuinely called to write. The twentieth century knowledge project was a wild success, even at publication rates (by individual) that were far, far lower than today. There is no evidence that the new system will do as well.

2. Will our kind of data survive?

As most of you know, the British election polls all got the recent election wrong. I was at Nuffield College during the election - the headquarters of some of the top British political pundits - and the main speculation was that the miscall happened because most of the current election polls (they told me this is true in the US as well) are now based on opt-in online surveys.

What do you think are the chances that opt-in online surveys are representative? Right – the chance is zero. And in fact, there's every chance that the political parties hired people to answer early and often in ways that would suit those parties' strategies.

We have the same issue at AJS every week. Are hired panels of data respondents legitimate databases for top-quality sociology? Does scraping an enormous amount of clearly non-representative data from the internet produce something that has real meaning, beyond what it tells us about that population of unknown extent? Are samples of IP addresses the same as samples of people?

In a world where all forms of online response data can and probably are being manipulated by economic and political interests, and in which a combination of computer algorithms and cheap call centers employing people who desperately need employment is completely exhausting the public's willingness to answer surveys, we are being confronted by an enormous wash of "data" that my generation's teachers - and even more, those of the Lazarsfeld generation - would have regarded as utter garbage. But it is likely soon to be the only affordable data. Representative sample surveys are heroically expensive, because phone survey response rates have been destroyed by telemarketers.

Are we going to get "representative" data again? Should we be deciding that the new kinds of data are representative, but of something different? What about administrative data that is now "openly available as part of transparency?" Is that data to be trusted, or is it the same thing as we see when firms that maintain two types of accounting – the "transparency" stuff is all lies? All these and many related questions are also problems your generation will have to deal with.

3. What is the ongoing relation between data and politics?

Data is now a political question. For example, consider all that data stored in ICPSR. With the kinds of search and matching algorithms we have today, you can probably combine those datasets with other data (both inside ICPSR and beyond) and, in a substantial number of the cases, you can thereby individually identify many of the "anonymous" respondents of the original surveys, given their demographic data. (To take a comparable example, for about 10% of individuals in the enumerated part of the US Census, there exist some four demographic variables that will identify them uniquely.) I imagine the commercial and governmental sectors can already easily perform these identifications. So "anonymous" political opinions are very likely to be identifiable. That's virtually certain for data gathered from now on, and probably true of a surprising amount of data gathered in the past.

Or to take another example, conservative attornies general in the states are already using matching algorithms to push people off electoral rolls throughout the states (because someone of a similar name registered in another state). Etc. So there are people out there who are quite willing to use all the power of computation to further quite particular political ends. And this will include both using data (as in my first example) and changing data. Don't get me started on data stability – the centralization of data (e.g., in one or two "authoritative" electronic copies rather than in thousands of distributed, printed [and hence not easily changeable] copies) makes changing data (in politically desirable directions) child's play. In my view, this kind of problem is a far greater political issue than the kinds of political matters that preoccupy the ASA. ASA's political preoccupations are often noble and important issues, but while the ASA and many similar organizations have spent millions of hours debating and arguing these issues, the conservatives have taken over much of the government of the country - quietly, efficiently, legally, and using a lot of high end computational social science. They have the data, the "science," and the algorithms to implement something quite authoritarian, but to do so in a completely invisible way, by modifying data, making things and even people disappear from data of various kinds, etc. That is very important and should concern sociology profoundly. Data stability and use are crucial political issues in an era of complete data centralization, such as we now have.

4. Computation –

All this means, in my view, that a firm grounding in computer science and algorithmic thinking is as necessary in quantitative sociology going forward as is a grounding in statistics. And I mean a real command of the material, not just a push-button acquaintance. Social statistics was launched for the most part by serious mathematicians - Leo Goodman the applied mathematician, Harrison White the physicist, Jim Coleman the engineer, etc. Serious computational work in sociology is going to need to come from people with foundational training in that subject.

But at the same time, computation is going to produce a huge amount of bad work, as we are already seeing in the digital humanities. So another thing your generation is going to have to do is create standards for what is good and what is bad computational social science. That is not easy - creating standards for good statistics took a good thirty years, it seems to me (from 1930 to 1960.) And it's also the case that computer-based qualitative work may have similar issues. For example, I'm not persuaded that the indexing programs for field notes add anything to careful, multiple reading and reflection. And of course wordles and other forms of down-market MDS and cluster analysis lead to mostly artefactual interpretations. So your generation has to figure that out as well. Maybe there are places where "science" is not the direction sociology should be taking.

5. How should the globalization of sociology happen?

I have written about this elsewhere; this is one of the main points of my Barbara Celarent series. The big lesson there is that much of the rest of the world has quite different traditions of social thought than does the West. While there are many people elsewhere in the world who have taken up Western normative analyses to do local critical and political work, there are even more who do not accept the normative ontologies of the West at all, but who believe devoutly in the rightness of hierarchy, or social domination, or any of a dozen other things not acceptable to Western social scientists of whatever political stripe. The globalization of sociology can move in one of two directions: towards pushing Western ideals on the world or towards trying to learn from the social thought of non-western societies. Your generation will decide which road is taken.

And that issue directly raises the issue of our own ideals in the West: What does it mean to "reach our highest level of personal self-realization" and is that, actually, a worthwhile goal for society, or is it compatible with a just society in any case?

Liberal societies have lost faith in many things, and seem to have difficulty projecting values that are beyond "I want to have a nice and successful life." There is an inability to see that religion might actually be about something that matters, although perhaps the threat of drastic climate change will wake up the liberal societies to their normative and value weakness, or perhaps that wake up will be accomplished by the recognition that there are billions of people in the world who imagine they live for something other than optimal individual experience.

Moreover, the immense productivity of the economic system of the world has actually – once the current absurd maldistribution of its products is rectified – created a world in which, as Keynes noted 90 years ago, the economic problem of making a living is essentially solved. Under such conditions, what will people live for? Wealthy liberal societies are confronting that problem already and reaping a predictable harvest of decadence as a result. Why should we live? Your generation will face this question more acutely than any for a long time – and “to have a good career as a sociologist” is probably not the right answer.

So those are just some things to think about. In the shorter run, of course, things will happen. There will be opportunities won and lost, ups and downs. Some of you will end up in elite departments, some in less elite departments, some in four-year colleges, some in libraries, some in the private sector, the nonprofit sector and so on. If Joe Hermanowicz's book on scientists is any guide, most of you will find rich and personally satisfying lives wherever you end up. Fulfilment stems more from character and other aspects of your personal situation than it does from professional success.

Of course, it seems easy for me to say that, because in professional terms I have been lucky beyond my dreams. And of course it *is* wonderful to have my position, with its rewards, freedoms, and largely pleasurable “responsibilities.” But the way achievement systems work is that achieving your dreams just means that you get more dreams. So I do not feel I have yet achieved the success I want. I myself have all the same questions you have asked me, just posed at a different level. But for me there is no obvious being to whom to pose them, and there is not much time left in any case.

One goes on because it’s not about achievement. It’s about tomorrow’s interesting idea, or Thursday’s insight, or Friday’s worried student. We do what we do because preserving real intellectual life is what matters. And you are the future of that preservation.