

Remarks to a graduate student audience on publication and building a c.v.

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Do you want to have an awesome looking cv? You do?! Well it's easy! On the first page, write in italics, *publications*, and then under that, write your name, and then some titles of your papers, and after each, write "American Journal of Sociology" or "American Sociological Review" and some years and page numbers! Of course, that's sort of like trying to cure a fever by sticking the thermometer in the refrigerator, but that's usually how graduate students are thinking—putting the cart before the horse. But if all you care about is the signifier, and not the signified, be my guest.

But if you think that a c.v. is basically a simple record of your actual publications, then you don't want to build a c.v.—that will happen automatically as you publish. So now the question is, so how do I get all these publications? Well, again, you're putting the cart before the horse.

Publication is a **by-product** of doing something important.

So, if you have a discovery that is so important other people are probably going to be interested in learning about it, then by all means, make it public! That's what publication means. Do you have something that is really of no interest to anyone but you? Then maybe keep it private.

Now actually, there are actually two routes to publication, the random and the non-random. You can take something that shouldn't be public, paint it up real purdy, and throw it over to a journal. They'll basically pick three people at random, who will study it very carefully using the standards of their subdisciplines, which means that their responses are basically equivalent to a

coin toss, and maybe they'll accept it. More likely, one will tell you it's too sweet and to add salt, another that it's not sweet enough and to add sugar, and a third will ask you to please make a pluffety pie. And you'll drive yourself crazy, going in circles, and doing various magic rituals to try to get them to accept it.

So I'm going to focus on the easier route, which is publishing something that *should* be published. The first question is, what should be published? I notice that a lot of students here seem to think that the very great thing that they're doing that should be published is a "theory." They're often surprised, offended and hurt when I suggest that this isn't where they should be putting their energy, and take it personally. But that's the very problem with these theories—they *are* personal. Theories are like poop. There's no shame in making it, lots of us do. But no one really wants anybody else's.

What we **really** like is when someone else is helping us with our **own** stuff. When there's something we care about, and someone has an answer. Or even, when someone **shows** (**shows**, not **says**) that the way we were trying to do things is wrong.

Grad students often only get this in a kind of distorted, mystical, cargo-cult way—as if it were only about being able to wrap up your thing somehow, put stickers on it, and give a salespitch. Unfortunately, there are sociologists dumb enough to be fooled by that. But you're not guaranteed to get three of them as reviewers. Remember being a kid, and you're walking home and you remember, oh sh-t! It's mother's day! I was gonna get a Pepsi, but I guess I could use the buck to buy something for her. Then again, this ole weed don't look so bad. What if I rip the

Pepsi can in half when I done, maybe crayon over it so it looks like I meant it to be that, kinda fluff up the flower part of the weed?

Your mother has to love you, even if you give her a lame-ass present on mother's day.

Reviewers don't.

Ok, so how do you get started on a project that should end in publication? Here are some ideas. Read the journals. See what people are arguing about, and what they find of interest. Why not work on that? Graduate students are afraid of overlapping with anyone else, and they think they're better off when they stake their claim far from civilization. There's a reason no one's there. It's fallow ground. Go with the group is a good first-approximation rule of thumb.

Definitely, that's not something you hear from a lot of us at Chicago. But presumably, the reason you're here is that you want answers to the question, how do I stay in this game. Now sociology isn't really a game or a race. And even if it is, or is to some extent, you are free to ignore everything having to do with the competitive professional stuff, this and get the full support of the faculty. However, you do need to understand the likely implications of your decisions, and not complain later that they were made when you were not of sound mind and body. To the extent that there are a limited number of viable careers out there waiting for you, yes, there is something repellently race-like about progressing in this field.

But this is a race in which you get to choose where to start. And what direction to face. If you position yourself towards the front—that is, you know where the field is, where it is going, what

questions are considered important, and which not—then you make it extremely likely that you can stay in that race. If you decide to start out in the woods, facing opposite everyone else, then run as fast as you might, the odds are definitely against you.

Don't get me wrong. I'm one of the people who started in the woods, I like the woods, and I like people who like the woods. If you want to hang out in the woods with me, that's great, pull up a toadstool. But if you don't, don't think you can have it both ways.

In a nutshell (which we have plenty of out here in the woods), the thing that hurts Chicago students the most is that their recipe for a project is “let me take something that no one but me finds very interesting, and tell you something very subtle and complex about it, which will require your utmost attention for some time.” That's like having a marketing plan for Domino's pizza that says, let's make a Limburger cheese pizza and charge \$45 a pie. So first thing is, why don't you do a little market research? Read the journals. See what people in different fields actually think is potentially important.

Do you think that this makes you a conformist as opposed to an individual? If so, you are wrong. As Hegel argued in the beginning of the *Philosophy of Right*, if we try to achieve our freedom merely by cutting ourselves off from materiality and reflecting on our selves, all we come up with is the arbitrariness of *particularity*. To be an *individual* means joining your particularity with universality, which means in this case, figuring out what you contribute to the communal imperative of cumulative social science. Or whatever.

And do you think this is a recipe for mediocrity? Quite the contrary. Guess what—sociology is hard. Easy to make mistakes. If you insist on starting your own little pile out in the woods, chances are whatever you do isn't going to be that great. When you figure out what other people are interested in, and try to contribute to that, you have the opportunity to do something *better* than others have done. Yes, we have problems figuring out what are good article and bad ones, but if you're doing something like a previous ASR article but much better, well then, that's a strong indication of quality.

OK, suppose you read the journals and you don't find anything of interest to you. Well, actually I think it's pretty reasonable to say, maybe I don't want to be a sociologist, as opposed to trying to make a living selling people stuff you're pretty sure they don't want. Just because I could do it doesn't mean you can. So let's say you basically know your interests, but don't know how to pursue them. Why not go to a faculty member and say, if you had the time, what would *you* do? Bingo. Chances are, that's a plausible project.

Now it might be that you do have a sense of what you want to work on, and it's just going to take some time, so you are afraid that by the time it all comes together, you'll be leaving with no publications. So what to do? It's always a good idea to have multiple projects, two from column A, one from column B. That way if the first thing tanks, you have a back up plan already live. How should you get these going? Collaborate.

Chicagoans do not do enough collaboration, and many even believe that it is antithetical to deep intellectual engagement to collaborate. Tell that to Marx and Engels, Durkheim and Mauss, Park

and Burgess, Adorno and Horkheimer, Shils and Parsons, Boltanski and Thenevot... and certainly, these days, we're really seeing more and more young scholars doing all sorts of collaborations: those with faculty when they were graduate students, with other students older and younger, and so on. Further, they are insufficiently penalized, which means that if you and a friend each put the others name on your paper, so that you have two joint authored instead of one sole authored, you come out ahead. Of course, I'm not saying you should do this! It's a thought experiment that demonstrates that it just doesn't *hurt* to collaborate.

Collaboration is a way to have backburner projects come to fruition, but also to learn how to publish. For this, as in many other things, there are three options: born lucky, trial and error, and mentorship. In fact, although everyone at Chicago will completely reject that sort of exploitative co-authorship where an advisor puts his or her name on all his or her students' papers, often the best thing you can do with a good, but flawed, paper, is to ask your advisor to collaborate. This is especially true with younger faculty, who tend to have the best sense of where the field is right now. A junior faculty member can come in as the second author and turn a rough paper into a strong contribution. They should of course give you some advice just as your teacher, but there's a limit to what they can do without it being unethical, if their names aren't on it.

We really want you to be independent scholars. But that doesn't mean that *before* you are well into your training, you are actually qualified to choose an important research topic. Maybe you are, maybe you aren't. But if you do drop your pet peeve when you realize it isn't an important sociological question, that doesn't mean that you lack independence. It means you are learning.

So let's assume that you actually have begun a project that handles a question or issue that people other than you care about. What do you need to do besides your work? Exactly what most Chicago students avoid doing, which is staying in touch with others working on the same topic. I don't know how many times I see projects crashing and burning because a student has not spent a few weeks trying to find out what experts in the area know about an issue. And even when they do this, they often see this as something having to do with a "literature review." Once done, they drop it. They never, say, google the key words of their project again to see if someone has, in the past few weeks, posted a paper solving their problem better.

If you do that, you might find that you have an article worth submitting. And you do submit it, and find it gets rejected. Then what do you do? A friend of mine would have a second package made up, postage and all, addressed to another journal. Before he even read the reviews, he'd drop it in the mail. This is a very prolific writer. He got rejected a lot, but it didn't slow him down.

Rejections are not necessarily informative. Many reviewers are not actually qualified to review the papers they are given, and editors generally just don't care. Most reviewers don't review; each will make a suggestion that you do your thing somewhat differently, and you can go in circles, even if there was nothing wrong with your first trial. You can get pissed about that, but you can also learn to have a lot of sympathy with the editors who rely on such crap. Sure they're scraping the bottom of the barrel, but a lot of work goes into that scrape. Anyway, don't get mad, don't get depressed, just get going. Find another place. When three reviewers all tell you

the *same* thing, then I think you should start taking it seriously. Otherwise, don't let a rejection stop you. Even if you recognize that the problem was just that a reviewer made a mistake, and want to revise so that no one will make that mistake again, think twice. You can't anticipate every crazy interpretation someone will make. Every bad reviewer is bad in his or her own way.

And now let's say you have an R&R. Then what do you do? The mistake I see a lot of students making is thinking that if they combine unjustified stubbornness ("I have full confidence in my results") with craven obsequiousness ("Reviewer A makes a brilliant point here") they'll cancel out. What folks are actually looking for is almost always not a rhetorical response but an empirical one. Do the other analyses if you can. If you're doing something based on fieldwork, and you can actually return to the field, or ring up a few informants, why not? It's often a drop in the bucket compared to the time and effort and money you've spent so far. If you can't get new data, you need to just admit it, explain the existing limitations, and what we *can* learn from your work.

What if you get conflicting responses from reviewers? Sometimes the editor will give you a clear direction to go, but very often it's a simple form letter. In that case, think about it, figure out which reviewer you think has the more compelling case, and then write to the editor. Lay out the contradiction, point out that you can't satisfy both, why you side with one, what your plan for revision is, and see if the editor agrees. Almost every editor will give a plausibly substantive response here, something like "this makes sense" or "no, the other reviewer has a stronger claim." Write a revision memo explaining in detail how you responded to each and every point made by each reviewer. Summarize the most important points in a letter to the editor. Make it

all easy to read. You can ask other people, like your advisor, for comments before you send it off. But ignore what most of your fellow students say—they tend to focus on minutiae and on negatives (which is why they turn into bad reviewers until they get their sea legs).

Now maybe this next part is obvious, but maybe it isn't. The way to get a lot done is to work a lot, and the way to work a lot is to love what you do. If you're gritting your teeth to do your sociology, and you don't actually enjoy your research, you won't get a lot done. Everyone has hobbies and stuff, that's fine. But if you aren't excited to do your work, at least most of the time, you can bet you won't have too much of it to show for your time here.

Okay, last thing I want to leave you with. I'm going to repeat, because this is the biggest point I have to make. It takes a very long time for things to go through the publication pipeline.

Sometimes you get lucky, but there's no way to plan on it. Do you want to have publications when you are finishing up and going on the job market? I can't tell you how to do this. But I can tell you how *not* to do it. It is the combination of these:

- 1) First, make sure that you collect your own data, so that one or two years have gone by before you even have a chance of writing something.
- 2) Second, don't collect your data with any preconception as to issues or question that are of interest to the sorts of people for whom your data might plausibly be of interest.
- 3) Third, spend your time spinning an elaborate theoretical structure of your own devising, instead of listening to what other people in your field are thinking and talking about.

So note that it's the combination of these that is deadly. You can jump into data collection and have a decent paper pretty soon if you are pretty sure that your data are relevant to things other people are interested in. Still, you might want to have your first project involve collected data, so that you can have something in the pipeline while you are collecting your own.

What if you want to collect your own data, develop your own theory, and all that stuff? Is there any way to make it work? There is, and it is this: *take a very long time*. If you want to spend 12 years here, you really can do this. And I think 12 years is a fine amount of time, and I'm not the only person here who does, though there are fewer and fewer of us all the time. But if you can't do the time, don't do the crime.