

After the surge: doing quantitative research in the field

There has been a lot of talk about surges lately. It seems to be everyone's favorite metaphor. Well, there is a surge happening in comparative politics right now, especially the study of order, conflict and violence. It is the surge of enthusiasm for hard methods—statistics, natural experiments, and field experiments.

It is also surge of people, mostly young academics, into the field for surveys and random trials, drawn by the allure of science, and the apparent success of those who partook in the first offensive.

Like some other surges we can think of, however, its impacts may be overestimated, and the hype may exaggerate reality. For the unsuspecting graduate student, the surge could actually turn out to be a quagmire.

I'm going to talk about life after the surge, which is your life and, in principle, your dissertation.

I was asked to speak to our first year PhD students this week about the direction of research in comparative politics (for the uninitiated: essentially the study of politics outside the US). I'm (almost) utterly unqualified to do so, so I decided to discuss how quantitative methods are being used in the field, and how they should be aiming for tomorrow's research frontier, not yesterday's.

I'd like to think the comments apply to quant methods in the field more generally (and I'm quite certain readers will let me know if not). I owe post-squash game conversations with [David Atkin](#) for inspiration. So blame adrenaline and exhaustion (or David) for the greatest lunacies.

The rest of my talking notes are after the fold.

Now is the moment to pause and reevaluate how to move ahead with micro-level empirical methods in politics—what I'm going to call micrometrics. Why? Mainly because, so far, we have surprisingly little to show from the surveys and experiments in comparative politics. I can't think of many recent papers that radically changed my beliefs or rigorously proved a novel theory. There are a good many new surveys and experiments coming out over the next couple of years that are more promising. But the tried and true traditional methods of comparative politics, in my mind, continue to be more persuasive.

It doesn't have to be that way. Quantitative methods have the potential to change our priors and challenge theories. And they probably will.

Micrometrics is a new technology in comparative politics, and like any new technology, it is evolving fast. The papers produced in the last few years—the very ones you will study in class—are

already obsolete. It would be a mistake to look at these micrometric papers and assume that this is the technological frontier, that this is what is good enough for your dissertation. That is no longer the case.

What got published in a top journal five years ago might not get published today, and I can almost guarantee you it will not get well published when you are about to graduate. You need to figure out strategy after the surge.

Why do I sound confident about this fact? Well, we have the unique benefit of seeing hindsight right now. Several close cousins—American politics, political economy, and development economics—have been playing with micrometrics in the field for at least a decade more than comparative politics. We can look at the evolution in those fields for an idea what the future holds.

Here's what I see.

The first assault. Here a few innovative minds pull off the first experiment in the field, or a radically different survey. Maybe they collect data in hard places, suggest a new kind of data, or pull together a neat panel (which is still rare). The results of these papers aren't all that important, and the methods might be a little flawed. But the methods are so novel, and open up so many new opportunities and ideas, that it doesn't matter. The leaders of the first assault are acclaimed leaders of the entire field. They quickly (and deservedly) get tenure.

The surge. The first assault generates a lot of attention and excitement. Other scholars see possibilities where they never thought to look before. There is a wave of imitation and innovation, and big questions start to get tackled. A lot of the surge comes from the students of the first assault scholars (who are multiplying by the year). The journals are hungry for these papers, and every project is by definition innovative.

The projects, however, are a little slapdash. You might convincingly identify an experimental effect, but it's a muddy treatment of many different things. The theory that's being tested isn't all that clear. Very few papers are giving new and convincing answers to old questions, but most are getting close.

The first sign the surge is over: people start talking about special issues of a field journal, and the (now tenured) leaders of the first assault form a working group with thrice-yearly meetings.

This is roughly where we are now in comparative politics.

The counterattack. Everybody who didn't jump into the surge takes a long, hopeful look at the papers that emerge and don't really find all that many findings that they can use. More questions

are raised than answers. And lots of methodological and theoretical meddles that didn't seem so apparent a few years ago are now emblazed in neon.

Graduate students who do just as good a job as their advisors find it harder to get jobs in the top schools. It's harder and harder to get a panel survey or experiment in the top journals. People start asking hard questions: What theory are you testing? Aren't there four other mechanisms you haven't tested? Does this instrumental variable meet the exclusion restriction?

I would place development economics and American politics as in this stage and, at the same, overlapping into the next one.

The (ultimately rewarding) slog. Some of the scholars from the first assault and the surge rethink their strategy. They think hard about research design, mechanisms, and theory. They design tighter, better planned, better executed surveys and experiments.

This means that field surveys and experiments are probably going to get more and more controlled. They are probably going to move closer to looking like a lab outside the lab. They are going to be built of teams with different expertise, and need lots of staff. There will be more experimental trial and error, where scholars run multiple experiments aimed at a single theory until they get it right.

Papers that get published in the top journals at this stage pack what seems like four papers into one. They have theory, clever tests, and innovative measurement. They probably answer a narrower question well.

In the slog, it becomes slightly harder for graduate students to compete with junior faculty, since the juniors have more time and resources for trial and error, staffs, or multiple projects. Faculty also have the benefit of experience.

But graduate students have lots of advantages here too. There is probably more funding around, and more advisors to consult, than before the surge. Students can also spend more time in the field than their supervisors and challenge their beliefs and results. Dissertations can have a big impact, and draw notice, by doing important work better.

The unsurprising message: graduate students have to keep pushing the frontier.

Here are a few ideas for life after the surge.

Do not leap to the big field experiment or data collection. This is the most common mistake.

From a big question where some evidence and theory exists, think about small pieces that can be tested.

Look for opportunities to answer hard questions: natural experiments, possible instruments, exogenous variation, tight experiments. Be very selective. Think about, investigate, and discard ideas on a regular basis.

Push ahead two or three or four good ideas. One will start to look great. Push every other project to the side and focus all your energy on the most promising.

Experiments and panel surveys often take several years. Any question you can answer well in a cross-section is much faster

Write out your theory and models in advance. Present them in seminars, before you start the experiment or survey.

Write out your regressions. Write out your identification assumptions. Think of problems. Think of ways to test the problems in the survey or experiment.

Worry about research design, not measurement. Don't go create a new variable. Data collection is the last step in a survey or field experiment.

If you come up with a decent paper or finding, and you're still in your fourth year, think about the mini-survey or experiment that could test your result and add a section to your paper to make it outstanding.

Finally, if you're bored at the idea of life after the surge, look for the next first assault.

Try to identify a completely novel question that lots of people will be interested in.

Where there are big questions with almost no data, you can get away with just about anything.

Where there are many assumptions but little theory or data, look for counterintuitive results and try to build a new theory. If you can overturn conventional beliefs, you are in.

Whatever way you go, remember that you need to be on tomorrow's frontier, not yesterday's. If this sounds anxiety-producing, it is. Angst and anxiety are the fertile soil from which dissertations grow.

If you think that sounds miserable, wait until you start thinking about your tenure packet.

Actually, it's only miserable in the worst moments. Most of the time it's exciting and rewarding. You get out of bed every day and push your brain to its limits. Those limits expand a little bit every day. People will eventually pay you to do this, even though you would secretly do it for free.

Ultimately, you should be doing what you love. If you don't love it, chances are you won't be any good at it. So keep that a first priority. But pushing yourself to the frontier is often rewarding for its own sake, and pays off in your academic career. Try to keep that in mind during the most anxious, vexing moments. I do.