

Comparative Politics and Popular Contention in China

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I redid the syllabus for my graduate course on Collective Action in China last summer. After nearly 20 years, I ditched the social movement focus, which had led me to group close-to-the-ground readings on protest in China under familiar concepts such as opportunities, framing, mobilizing structures, repertoires of contention, tactical innovation, grievances and outcomes. Although the old structure had become ever-easier to use, as more and more China scholars placed their findings in relation to the contentious politics literature, I felt the class was becoming a little stale, as energy seeped out of the social movement field, and China scholars were drawn to other theoretical moorings. In the course of dropping scholars like Tilly, Tarrow and McAdam from the syllabus and not feeling a need to replace them with other non-China types, I also had a realization: younger Sinologists often bring their own theories with them, whether it's Jessica Weiss on signaling, Peter Lorentzen and Martin Dimitrov on information, Lily Tsai on noncompliance, Mary Gallagher on legal consciousness, Lynette Ong on third-party repression, Rachel Stern and Jon Hassid on uncertainty and control parables, or Diana Fu on protest that's collectively organized but individually performed. This trend should make us more legible to comparativists who are interested in these issues around the world, though it may make it a little harder for us to appreciate each other, unless we're versed in (and fascinated by) the questions these theoretical traditions generate. It's also somewhat odd that my new syllabus probably has fewer names on it that are familiar to non-China scholars than it had two decades ago, when there was a much greater danger that China studies was off in a corner by itself and we were exotic zoo animals who were interesting to look at but not really

relevant to mainstream concerns. As I taught the class this fall, and focused on emerging topics like protest leadership, organization, soft and hard repression, and the policy consequences of contention, I was also struck how many readings were even closer-to-the-ground accounts of this or that, and though they could be placed next to each other in a way that told us a lot about, say, protest diffusion in China, there wasn't all that much effort to address the broader significance of the findings elsewhere. This made me wonder if, in some respects, we're possibly becoming even less legible to people who study contention in other places, if we're not orientalizing ourselves, as we dig up and relate wonderful stories about single incidents, which are full of telling detail and ring true, but don't add up well and may not draw the attention they deserve from comparativists. If people like me, who have been thinking about contention in China for decades struggle to pull back and conclude what we know about a topic like protest leadership, what chance does a comparativist who wants to dip into the literature quickly have of figuring it out? Maybe this is partly because most of the readings on my syllabus were drawn from area studies journals, but I don't think that selection bias or my personal preferences are the whole story. The best writing on protest in China, with not enough exceptions (and I'm not including books here) is still in *China Quarterly* and similar journals, and I do wonder after my deep dive this semester, what non-China scholars make of it even with a good faith effort to extract take-home points and theoretical insights that will delight those who know little about China, but much about protest.

So, with this background in mind, how do I respond to the questions that Wang Yuhua sent us a few months ago? Where were we? Ten or twenty years ago we didn't know nearly as

much about protest in contemporary China, but there was a bigger consensus on how to organize what we did know around the study of social movements and contentious politics.

Where are we now? We've learned much more about protest and repression in China (though the difficulty of doing field work these days threatens to bring that to a grinding halt), and some people are bringing theories from hither and yon to bear, which may enliven the field and produce new insights. But at the same time some of the deepest and most interesting research is more China-centric than ever.

Where should we go? I do sense that the social movement turn, with its normal science, orderly and tidy approach to knowledge accumulation has run its course, and it's not clear what will replace it. The idea of contentious politics about 15 years ago opened up the field of play a little, but attention to mechanisms, for instance, never really took off. I've always thought the smartest people on popular contention worked in the resistance school, and had ethnographic instincts, so maybe the slight uptick of interest in fields like political anthropology will draw us away from the structural sociology that informs so much work while complementing the political economy that's on the ascendance in the wider discipline. Maybe we're entering a hundred flowers blooming era, where all to the good, some China scholars are doing protest event analysis, others are thinking about organization in creative ways that don't remind us in the slightest of the civil rights, women's or anti-nuclear movement in democracies, and still others are trying to figure out if the Center really has too little information about discontent and is willing to risk protest to get it, while others are wondering if technological changes and innovations in protest control means the Center has plenty of information and is instead troubled by how to make sense of it.

Finally, Yuhua's last question: How can this topic advance the general discussion in Comparative Politics? That's the hardest question of all, especially for me, since I've been writing mainly in area studies journals for the last five years or so. One thought, which is hardly new and is already much in evidence, is to use protest to speak to larger questions related to authoritarian resilience, institutions, or responsiveness. We see a good bit of this already, and while sometimes it feels a tad like decoration or depends on merrily jumping up the ladder of abstraction several steps at a time, this type of research could well produce findings interesting (and digestible) for comparativists. Another possibility is what I mentioned at the top: bring theory from unexpected quarters and bounce China evidence on protest and repression off it to show that some assumption about how politics works in democracies or other authoritarian countries isn't quite right when you look at China. If we can do this, comparativists will pay attention to what we discover and perhaps we can not only slot China into existing frameworks, but also uncover unexplored assumptions and over-generalizations that have crept into theories that have ignored what we can learn from China.