

# Speaking to Theory and Speaking to the China Field\*

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*Bringing knowledge about China to the disciplines has reduced the outsized role that research on Europe and America has on many topics. But mainstreaming China studies also leads to certain tradeoffs. How should we manage these tradeoffs and produce research that is both true to China and contributes to the social sciences? In the last 40 years, China scholars have developed many strategies to navigate the territory between area studies and the social sciences. I myself have vacillated about how China studies and political science should interact and inform each other. How are scholars addressing this issue now, in an era of mixed methods, sophisticated quantitative research, experiments and “big data?”*

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Twice in the last 15 years, I wrote papers on the topic we are examining today: speaking to theory and speaking to the China field. But, oddly enough, the two essays pointed in the opposite direction. The first paper was an appeal to China scholars to search for low-hanging fruit in the disciplines and to mainstream China studies by locating opportunities to make theoretical and conceptual contributions to the social sciences (O'Brien, 2006). The gist of the argument was that many concepts and theories had been developed using Western cases and that it was easy to show that many concepts did not travel well and could be “busted” with empirical evidence from China. So, notions like institutionalization, representation and elections

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were all well and good, but were deeply-embedded in assumptions about pluralist democracies. By using China to show that these concepts were not as general as often thought, Sinologists could contribute to theory and demonstrate that experiences from Asia needed to be taken into account when creating social scientific knowledge.

This was an optimistic paper about bridging the divide between area studies and the disciplines. Although some of the suggestions underscored what should not be done rather than what should, the paper was also a plea to put scope conditions around generalizations and an effort to encourage theorists to broaden their empirical base. I wrote this essay as a spur to China scholars to take their disciplines more seriously and to look for ways to inform debates that were occurring outside the China field. I hoped to encourage area specialists to engage disciplinary colleagues who previously had never thought much about China and who did not imagine that something they assumed was true everywhere was obviously not true in China. The call was to be opportunistic: to go to the field with ideas about what you might find, but not to have an overly fixed research design. Then, conduct as many interviews as possible, read whatever Chinese empirical material could be located, and finally realize that some assumptions, derived from existing social science theory, were flat-out wrong.

In my own research, this has often turned out to be the case. Interviews showed that people's congress deputies did not think they had to choose between representing the state and representing their constituents but believed they could do both (O'Brien, 1994a).<sup>1</sup> Legislative leaders and staff were not seeking more autonomy from the Party, as theories of institutionalization would suggest, but wanted to cozy up to the powers-that-be so that congresses could obtain more resources and enlarge their jurisdiction (O'Brien, 1994b). Instituting grassroots elections was not a story about democratization but rather one of state-building and regaining control over villages after communes were disbanded (Kelliher, 1997; O'Brien & Li, 2000). Resistance was not a two-party game between protesters and the state (Scott, 1985), but a three-party game where the aggrieved exploited divisions within the state (O'Brien & Li, 2006). Repression sometimes relied on psychological coercion rather than overt force and was problematic to classify as "hard" or "soft" (O'Brien & Deng, 2017). After coming across these fairly obvious findings and recognizing that something that should not be true was, an opportunity for theory-building existed. The goal was then to bring theoretically-inconvenient facts back to one's colleagues who did not study far-off

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<sup>1</sup>For research on Chinese legislatures that finds the concept of "representation" helpful, see Manion (2015) and Truex (2016).

places and to say: “the China case needs to be considered, too, when we do grand or middle range theory.”

This approach to theoretically-informed area studies research is also conducive to structuring papers and finding “hooks” to hang a paper on. When I was working on people’s congresses, for example, the puzzles inspired by the wrong-headed questions that I set out with were: how can a legislator represent the interests of constituents and be an agent of the state at the same time? How can parliaments develop when they avoid confrontation and are subservient to strong executives? Why are leaders of higher-level people’s congresses not empire builders while leaders of lower congresses are willing to sacrifice their autonomy?

These were real puzzles in one sense, but not in another, in that interviewing had already demonstrated how to solve them. And then connecting the puzzles to theory was not difficult, even if by questioning common analytical vocabularies and developing my own concepts — such as remonstrating, embeddedness, rightful resistance and relational repression — it could be argued that my collaborators and I were risking making China less “legible” (Scott, 1998) than if we were to adopt existing concepts, such as parochial responsiveness, institutionalization, resistance and “soft repression.” For better or worse, I have always felt that this type of research and conceptual proliferation allows us to be both theoretically-informed and true to what is happening in China, and keeps researchers focused on striking examples of *x* rather than misleading accounts of *y*.

In this first essay on the China field, I came down in favor of more attention to theory, but not broad covering laws or sweeping generalizations. Rather, I argued for explaining what takes place in China more analytically by clustering findings under a host of often made-to-order concepts that brought Chinese experiences into social scientific ways of thinking more than had been done in the past. The focus was on theoretical anomalies and repackaging insights for disciplinary colleagues who had not recognized that much contention in China, for example, was neither “transgressive” nor “contained” (for these terms, see McAdam, Tarrow, & Tilly, 2001) but “boundary-spanning” (O’Brien, 2003). This approach to research entailed theoretical eclecticism and more reading of the social scientific literature, not less, as high-flown theory was tied to the most local of Chinese detail. This first essay was based on quite a hopeful view of the relationship between area studies and social science theorizing.

My second paper on the China field (O’Brien, 2011) was more pessimistic. Five years after publishing the first paper, I had observed some developments in China studies that made me uneasy. Students, including my own at Berkeley, were not only paying close attention to the disciplines, but also they had almost stopped talking

to each other and some felt that, for example, a person studying the auto industry in China and another researching sex work in China were not really in the same line of work. The China field, in my take, was becoming hollowed out, largely as a result of two trends: topical specialization (most scholars were working on small, separate islands with few bridges between them) and disciplinary specialization (people studying China in one discipline were finding it increasingly challenging to speak to those in other disciplines).

The argument sprang from a realization that my graduate syllabus on Chinese politics had become outdated. Since the early 1990s, I had arranged weekly readings around a series of debates: memorable battles over the reach of the state, neo-traditionalism, the role of *guanxi*, peasant power, civil society, corporatism and so on. But by the mid-2000s, I had the distinct feeling that the old debates had exhausted themselves and there were few new ones to replace them. Lively areas of contention remained on topics such as state capacity and nationalism, but most current debates were somewhat polemical, narrow-gauged or marked by agreement as much as disagreement.<sup>2</sup> Why had this happened? My thought was that China scholars were not meeting in area studies journals to hash things out as much as we had in the past, and instead were aiming to engage disciplinary colleagues who focused on social capital or protest or corruption or political trust or production chains in other countries. Our target readers, especially for young scholars seeking publication in top disciplinary journals, were changing so that we were putting the bulk of our efforts into delighting disciplinary colleagues rather than highlighting what our findings meant for understanding China. Why this had happened, I suggested, had something to do with career incentives and getting a good first job and rapid advancement.<sup>3</sup> But, it also was connected to China continuing to look more like other countries, so that political scientists, for example, wanted to share their findings on the political economy of development, the role of private entrepreneurs, the consequences of foreign direct investment, regulation and property rights with disciplinary colleagues. And this approach, I have to admit, has much to recommend to it, because it is entirely possible that it is time to put Chinese exceptionalism aside once and for all and to associate ourselves not with a single country but with problems that can be studied in that country.

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<sup>2</sup>For efforts to determine where the battle lines truly lie in the debate over rights versus rules consciousness, see [Li \(2010\)](#) and [Lorentzen & Scoggins \(2015\)](#).

<sup>3</sup>Truex (2017, p. 4) argues that “professional incentives” are a “barrier to knowledge accumulation. . . . As China scholars, we are forced to link our scholarship to general debates in the field to gain access to general interest journals, which have become the metric through which junior faculty and graduate students are evaluated.”

That is the disciplinary specialization side of the field's evolution. As for topical specialization, it is even more straightforward. Given the avalanche of information available on China, no one can begin to read everything that appears online or in the academic and popular press, and even trying to keep up with filters and postings on news groups is a challenge. If any depth is to be achieved in research, some division of labor is inevitable and desirable. It had also become possible, at least at the local level and prior to the rise of Xi Jinping, to go nearly everywhere and to examine many new topics. Maybe we are just following the example of scholars who study the United States, or the natural sciences, where subfields have proliferated at a rapid rate, and researchers have long ago learned to live with a high degree of topical specialization. Perhaps, we are simply witnessing the maturation of China studies in an era where it no longer makes sense (or is feasible) to be an expert on more than a few aspects of China. If this is the case, then specialization is the road to progress.

But, as I am sure is clear by now, I have some doubts about this line of thought. First, narrow topics cannot help but make it difficult for those who study other aspects of China to place new research or to figure out what it all adds up to. Topical specialization by its very nature produces high resolution pieces of a jigsaw puzzle, but little sense of how the pieces fit together and no box cover that illustrates what the puzzle depicts.

Disciplinary specialization also has several worrisome consequences, including discouraging interdisciplinary, China-centered discussion. Most of the debates I previously taught in my graduate Chinese politics seminar drew in Sinologists across the social sciences. You could hardly tell whether an author was a political scientist, sociologist or economist, and often enough some of us would switch teams and act like we were paid-up members of another discipline. These fluid boundaries and exchanges become less likely when a high level of immersion in political science is expected from sociologists or economists or anthropologists or historians, who are busy enough with their own discipline and China's role in it. Disciplinary specialization threatens to cut off experts on China from each other and create new islands of research, fenced off by disciplinary jargon and parochial concerns, which can inhibit interdisciplinary work and hamper efforts to develop a holistic understanding of Chinese politics, economics, culture and society. In other words, what looks like integration from a disciplinary perspective may be fragmentation from the vantage point of China studies.

Disciplinary specialization leads us to discuss less as a group and to have less common knowledge. Leaning toward a discipline also alters our topics and debates. The disciplinary disputes that excite many young Sinologists may be pivotal in political science or sociology or economics, but that does not make them so for China.

The old debates that structured my syllabus had China scholars on both sides and exploring and adjudicating them touched on issues that virtually all Sinologists thought were important. In the newer disciplinary-focused research, the other side is often unknown to many China scholars and the received wisdom in the discipline is sometimes clearly not the case in China. This creates a potential for theoretical and conceptual payoffs that are indecipherable or not overly revealing to Sinologists: findings that are slightly disappointing, or obvious, or of abiding interest primarily to those who know little about China.

It is of course possible that the gains from disciplinary and topical specialization outweigh the costs. All of China is too much to keep up with and bringing knowledge about China to the disciplines can play a useful part in reducing the paradigmatic role that European and American experiences still enjoy on too many questions. Nor must any of us make a one-time, either-or choice between allegiance to China studies or to our discipline, and we can all think of scholars who, across time and in different works, address distinct audiences and identify more with area studies or disciplinary colleagues through a career.

But even if there can be a mutually-reinforcing relationship between the disciplines and area studies, there are questions about what Emerson (2008, p. 305) calls the “terms of enlistment.” In my view, it would be a mistake to follow the example of economics, where country-based studies are uncommon and area specialists are marginalized. Nor do I think it is wise to revive the old search for a science of comparative politics that has little use for country names. Instead, there are still at least three good reasons to keep producing country specialists on China. First, social science theories come and go but China will persist. It is a sad truth that much social science knowledge disappears with little trace because it is ignored or falls out of fashion and is supplanted by the next big thing. Second, mainstreaming the study of China may at times be wrong-headed or premature if it springs from a belief that China is more familiar than it is. Using theories and concepts from the social sciences to interpret protest or voting behavior or elections or bureaucracy may place findings from China in a Procrustean bed that slices off what matters most. Third, interest in China is high and shows no sign of abating. A large and growing demand for informed, broad-gauged analysis exists, both inside and outside the classroom. If scholars do not address this desire for knowledge about China, and instead limit themselves to ever more-focused studies and intramural disciplinary debates, others will step into the breach.

Can these two assessments of the China field, written seven years apart, be brought together or are they incompatible? For a time, I thought I might write a book

with the first paper as chapter one and the second as the conclusion and then see if I could work my way from one to the other without anyone noticing they were inconsistent. I never tackled this project and will instead leave these appraisals sitting next to each other as divergent takes on how area studies and the disciplines interact and inform each other: two perspectives on what China studies can bring to social science theorizing, packaged with a warning about the dangers of doing just that.

What about the future and the next generation of China scholarship? What trends have emerged in the 2010s, as a new crop of Ph.D. students and younger academics, some now past the tenure bar, are striving ever more diligently to contribute to their disciplines? What can we say about speaking to theory and speaking to the China field in the era of mixed methods, sophisticated quantitative research, and the experimental and “big data” revolutions that so many young China scholars are gravitating toward, at least in political science?

From the perspective of a consumer rather than a producer of this work, there are a few trends that stand out. If I were to write a full-fledged third paper on the China field, I would emphasize that this is an era of measurement and of rather narrowly-conceived empirical studies that aim to say something about very grand topics, such as comparative authoritarianism.

Beginning with measurement and simply-stated findings: great care is put into hammering down a relationship and the takeaway from many papers published in leading disciplinary journals can, without great harm, be reduced to a single sentence. The influential King, Pan, and Roberts (2013) study in *The American Political Science Review*, for example, is almost always cited the same way: calls for collective action lead to censorship but oppositional speech alone does not. Like the field of American politics decades before, one senses that referees’ suggestions nowadays often focus on “what about this test?” or “your measure is not quite right.” This produces long discussions in top-tier journals that are devoted to methodological objections that could weaken a finding or make it disappear. This fastidiousness about evidence and its validity has many benefits, including more attention to causal inference than traditional observational work ever had. And readers often are left to struggle to find grounds to question what has been found, not least because there is just one issue on the table and doing so requires dismantling a formidable methodological apparatus and building a whole new one.<sup>4</sup>

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<sup>4</sup>See Han (2015, 2018) for a critique of elements of King et al. (2013, 2017). It is noteworthy that Han takes on the exemplary and impressive edifice King et al. have built using a different methodology: field work and interviews in China.

At the same time, young Chinese politics scholars are remarkably well versed in the latest methodological innovations, be the regression discontinuity, matching, machine learning or the field and survey experiment wave that is sweeping the discipline. China scholars are no longer outsiders looking in, which every department needs one of, or laggards racing to keep up with breakthroughs in more teched-up precincts of political science. Many of my China students, and those in most graduate programs, can teach methods with the best of them. And more importantly, better data are being analyzed better, often with ingenious mixed method designs,<sup>5</sup> and the most sought-after young China scholars compete well in “best athlete” faculty searches where China is unmentioned in the job description or is one specialty listed among a number of others.

Along with unprecedented attention to coding, operationalization and causality, the study of Chinese politics is tacking decisively toward comparative politics. But this is not happening in the way I envisaged 15 years ago, when I saw problematizing problematic concepts to be a particularly tasty, low-hanging fruit to grab. Studies of authoritarian polities throughout the world now regularly include China, and many papers in disciplinary outlets and area studies journals speak to the growing literature on authoritarian responsiveness, resilience and institutions, information problems, and signaling in ways I could not have imagined a decade ago.

But for all the benefits of mainstreaming, scientific rigor, and greater interest in other countries and comparative politics, I worry that something is also being lost. Single sentence findings, firmly nailed down using multiple approaches and large datasets, often stand alone as isolated islands, a substantial distance away from other single sentence findings, and no amount of hand waving and skipping up the “ladder of abstraction” (Sartori, 1970) can turn a tiny story about one feature of Chinese politics into a telling, theory-enhancing contribution to the understanding of authoritarianism in China or, even more ambitiously, authoritarianism worldwide. Side-by-side comparison with other countries, or glancing efforts to place China in comparative perspective in a paper’s conclusion or introduction, can work against accumulation of knowledge about China, and leave the field of Chinese politics even more “hollowed out” (O’Brien, 2011). A strong focus on methodology only heightens this self-isolating tendency as findings are often de-contextualized, and citations to other China scholars sometimes assume a drive-by character: offhand decoration that referees may call for, but authors and readers struggle to take seriously, or grapple with deeply.

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<sup>5</sup>For research that uses an imaginative combination of interviewing, surveys, experiments and satellite data, see Mattingly (2016).

Broad, in-depth knowledge of China threatens to become a frivolous luxury when the real game is placing findings in relationship to what is known about other countries or speaking about a form of authoritarianism. Concentrating on comparison and measurement, in other words, may be crowding out context and efforts to develop a rich, rounded picture of Chinese politics.<sup>6</sup> Fieldwork and getting close to the subjects of our study, their world, and their understanding of it, may be turning into a time-consuming, expensive indulgence that, unfortunately, has also become more difficult to do in today's China. Feasibility concerns are hastening the turn toward high-level comparison and abstraction, as is the new audience for much research: other scholars of representation, responsiveness, information or control, not Sinologists working on other aspects of China. And members of this disciplinary audience, by their nature, cannot concern themselves too much with the most local of Chinese detail, and instead prefer a fairly stylized account of what is transpiring on the ground. Perhaps I am mistaken, and as I suggested seven years ago this is just a necessary maturing of China field and all to the good, but I do wonder if something important is being lost, too.

All that said, we are undoubtedly in a golden age of studying China from a social scientist's perspective. The infusion of scholars from China into foreign universities and better-trained Sinologists worldwide brings more talent to the table than we have ever had before. At the same time, China-based students of contemporary China are already making a distinctive contribution and are poised to make important breakthroughs in coming years (Guo, 2012, 2018). Strategies for speaking both to the China field and theory are surely being dreamed up that will artfully finesse the tradeoffs and tensions I have highlighted. A vibrant field of study relies on "revolutionary successors" to pioneer new approaches and creative uses of data unforeseen just a few years before. We now have many scholars eager to ensure that China receives its due in the academy, and I for one am excited to see where the next generation takes the study of Chinese politics, economics, society and culture.

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<sup>6</sup>Mertha (2017) recommends pursuing "the lost (or diminishing) art of being China generalists. This has several advantages, but the most germane of these is that doing so forces us to look past our chosen substantive and methodological approaches in painting the larger picture."

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