A Comment on Sida Liu’s “Powerless” Approach: A Virtue Out of Necessity?


Thank you for inviting me to comment on Sida Liu’s embrace of powerlessness in law and society research. The approach in his article is ambitious and innovative. It carves out potential research projects. It has the virtue of opening up avenues for research, including comparative research. It might be said, furthermore, to make a virtue out of necessity for research focused on East Asia, particularly China. It will be no surprise, however, that I also believe that the research orientation he offers pushes aside what is most interesting and important in comparative law and society research. I will proceed with a brief comment on his story of theory or its lack in law and society, next spend some time trying to understand Liu’s approach, and then I will suggest what is missing and perhaps why.

I will start with a comment on the characterization of law and society research in the article. Liu suggests that there has been some erosion of theory in law and society—necessitating his intervention. In contrast to Liu, I do not believe that that there ever was a move in law and society “from social science disciplines toward a more or less autonomous interdisciplinary field” (2). The point cannot be developed here, but the field of law and society in my opinion draws more on social science theory than ever. More importantly, but not discussed by Liu, is that the field remains strongly oriented toward law and the questions that arise within the legal field (Garth and Sterling 1998). What Liu sees as a kind of radicalization toward the “dominant power/inequality approach” (2) is therefore hardly radical. To be sure, the focus on issues of gender and race in the legal profession, or on social movements and law, is meant to be progressive, but the key point is that the approach also tends to align perfectly with the legal profession and its legitimate concerns about serving the underserved and providing access to legal careers (Dezalay and Garth 2014). The famous article by Marc Galanter on “Why the Haves Come Out Ahead,” referred to repeatedly by Liu, is from this perspective less a radical statement and more a call for procedural and professional reform to balance the scales of justice through public interest law and the like—law reform equalizing access. Much of the research taking seriously the rhetoric of the legal profession may indeed be

Bryant Garth is the Chancellor’s Professor of Law at the University of California at Irvine. He may be contacted at bgarth@law.uci.edu.

© 2015 American Bar Foundation
“atheoretical” in Liu’s terms but that is because it is framed by legal concerns – testing and reinforcing professional rhetoric.

I therefore do not share the diagnosis of what is wrong with law and society research. Given the gravitational pull of the legal profession and legal academy, we should expect much or even most of it to be atheoretical in the social science sense. And I think the number of individuals studying law and the legal profession using social science theories has increased substantially in recent years inside law schools and outside of them.

But the solution offered by Liu still merits attention as a new approach or new emphasis. The focus on powerlessness, if I understand it, highlights the forms of law. There are structural forms—the “administrative hierarchy of the judiciary, the hemispheres of the bar, or the ranking order of law schools” (8), for example. And there are “processual forms—the tournament of law firm growth, the recursivity of legal change, or the spatial mobility of law practitioners” (8). We can carve out this area of focus, he states, because the forms have a “certain degree of autonomy from its [the law’s] substance, but also exercises structural and processual constraints on the system’s power dynamics” (8). The focus is on the “persistent structural and processual forms of law that transcend the particularistic and critical orientations in contemporary law and society research” (8). Liu admires the systems theory of Luhmann because it focuses on these processes. Quite appropriately, he also wants to add actors to the impersonal systems theory. He finds social ecology helpful also. The “social space of law has its actors and locations… judges, prosecutors, law enforcement officers, legislators, legal scholars, …niches and jurisdictions,” which point to an agenda to study the “ways legal actors are located in various niches and jurisdictions” (12) in order “to understand the legal system’s formal shape” (13). The goal is to “capture the full dynamics of how legal actors and their locations are bundled together,” which “requires an inquiry into the social processes that produce the special outlook of the legal system as well as the temporality of legal change.” And the focus is on the “formal process of change” (14)—not the cause or the underlying dynamics.

What is meant is clarified by potential projects described in the article. Drawing on Galanter, Liu suggests seeing how a one-shot player develops into a repeat player. Note that it is not about power or clout in litigation, but about moving from one category to another. Drawing in part on the “naming, blaming, and claiming” literature, he also wants studies of processes of lawyer-client interaction—“the deconstruction of formal law and the reconstruction of the client”—“competitive cooperation” of litigation lawyers, and the “coordination” characteristic of the criminal justice system (18). Again, it is about how the formal categories—lawyer, client, judge—relate to each other systemically and “shape” the legal system. The research he proposes is not static. All these processes change over time by “events happening in everyday legal practice” (19) coming from the micro or macro level—such as political change. The key questions, however, involve the “shape of the legal system” (7). And there are strong possibilities for comparative research. Liu issues an “open invitation for sociolegal scholars across the
world to study the formal shape of law from their own cultural and historical contexts” (21).

One example from his own research on China is given (Liu, Liang, and Michelson 2014). The article on the spatial mobility of Chinese lawyers shows how mobility of lawyers began because of national policies and income inequality between regions in China, that those who moved typically went to the bottom of the professional hierarchy, but that there were enough success stories to sustain a steady migration nevertheless. Comparative study could take up the topic of what drives mobility—a process—of lawyers in other places. Comparative study can also examine the other topics that Liu sketches—boundaries between lawyer and client, lawyer-lawyer collaboration and cooperation, etc.

Liu of course recognizes that power is important, but he wants an agenda that puts power aside. The crucial move, quoted earlier, is the assumption of that the legal system has a “certain degree of autonomy from its substance, but also exercises structural and processual constraints on the system’s power dynamics” (8). The projects take for granted the autonomy of law and the legal system and seek to work within that assumption. This embrace of autonomy explains Liu’s critique of the approach Yves Dezalay and I take to Pierre Bourdieu in our most recent two books (Dezalay and Garth 2002; 2010). Liu chides us in particular for not paying enough attention in Asian Legal Revivals to the “habitus” of lawyers. He notes our seeming “tendency of overemphasizing human agency and in minimizing the field’s structural constraints on individual actors.” Therefore, he contends, the “stories of the legal elite in various countries are told without being connected to the structural constraints of the juridical field” (11). He states that he still awaits what he calls a “special analysis of the legal system” drawing on Bourdieu. It is telling that Liu wants to talk of the legal system instead of the legal field.

Any analysis of the legal field must take into account fields of power and the importance of various forms of capital that shape the agency of lawyers. Those we describe in our book are not depicted as free human agents acting without structure (or habitus built out of that structure), as Liu states, but they operate within structures of power that give some place for law but also can trump law much of the time. I do not want to defend our approach here, but it is not surprising that a powerless approach wants to take for granted that the legal is the legal and worth studying because, in part, there is a certain autonomy from substance and power. For Bourdieu, however, the concept of the “legal field” is a tool for analysis and not a thing to be studied in isolation. It is a tool to examine power, the rules of the game for governance of the state and the economy, the role of law as a component of those rules, the role of competing forms of expertise and legitimacy (party, religion, economics, for example), and the relationship of the production of governing expertise also to social reproduction. The more general relationship of social capital and legal capital—crucial to the relative autonomy of law—and the processes that serve to produce autonomy (or not) are not within the scope of powerless research. The approach he favors, in other words, starts and stays within the “legal system.”
Liu notes that the “power/inequality” approach “does not necessarily provide the most effective analytical tools for studying legal systems in other social contexts.” Instead of “gender and race,” for example, he sees a need to focus more on “development, human rights, institutional transplants, judicial reforms, religious and ethnic conflicts, and so on” (6). But the powerless approach again misses where the power dynamic comes from that helps produce the topics. It treats these topics as if they exist apart from global hierarchies and hegemonies. They did not appear spontaneously on national agendas. Suppose, for example, that jury trials became important in a particular country, just as oral trials have become important in Chile and Argentina. They could be studied as different processes, involving different kinds of legal collaboration, for example; or one might be more interested in how the idea was exported and imported, who benefitted, and how it was shaped by local hierarchies (see, e.g., Langer 2004). Liu would presumably want to study, for example, how Chilean juries find the facts versus U.S. juries rather than the political economy dimensions of importing and exporting jury trials.

From my perspective, in other words, the interesting comparative and global topics do not isolate the legal system as a given, but instead seek to explain the position of law in the governance of the state and the economy, how that is changing, how it relates to international competition for power and hegemony, and how it relates to national structures of power, including competing forms of expertise and legitimacy. A potential criticism of this project that I favor, I should note, is that the problematique of the place of law is a Western and especially U.S. research topic. That is a legitimate challenge. But one response is that empires and their legacies—and globalization U.S.-style—have made this topic—and legal legitimacy—resonate to a greater or lesser degree globally, and that it provides a point of entry to examine competition and conflict—resistance to hegemony—as well.

In short, Liu’s powerlessness puts political economy aside. Thus, in the study of the mobility of Chinese lawyers, the approach I outline might ask more about whether provincial lawyers who move and become marginalized in the cities are more likely to try to build their careers under the banner of human rights, or by linking up with other marginalized groups, or linking up with transnational entities to build their local positions—and with what consequences.

But it may be that in China and perhaps elsewhere in East Asia there is not much happening in the political economy of law. The competing forms of legitimacy—family, party, economic growth—dominate and will perhaps continue to do so. A research agenda that privileges scrutiny of the relative autonomy of law, the embedding of social capital into legal capital, and the position of law and lawyers in the governance of the state and the economy, may not appear fruitful and furthermore may be controversial (Cheng Li 2013). It may not even be interesting to the Chinese legal profession with many more immediate issues of concern. And legal elites may not want to rock the boat. It is to the credit of Liu that he defines an agenda that offers much potential for further research in places such as China. I am convinced that we can learn much from that
research, and it will often speak to the issues that more interest me. But as Bourdieu among others teaches, making a virtue out of necessity—celebrating powerlessness—puts important questions for sociological research off limits.

REFERENCES


