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

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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

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“athoretical” 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


The “Powerless” Approach to the Sociology of Law and the Future of Law and Society

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LIU, SIDA. 2015. Law’s Social Forms: A Powerless Approach to the Sociology of Law. 

Sida Liu’s “Law’s Social Forms: A Powerless Approach to the Sociology of Law” is an exploratory essay that claims a theoretical turn in socio-legal studies. It is very positive that the author concentrates on the micro-macro link—i.e., the connection between specific legal practices, their social and institutional impact, and the general conclusions that can be drawn at the societal level—with several refreshing ideas that should spark further discussion. The essay is also highly intuitive, stemming from a particular reading of classical sociology to set a general analytical framework for the socio-legal field. He advocates the need for a canon in Law & Society, and a “powerless” perspective—a new quest for objectivity—as well.

I will concentrate on a few topics. What is he intending to say? Why is it important? And why, in my view, it misses the target? What follows are some comments to spell out these questions.

FORM AND SUBSTANCE: THE GENERAL THEORY OF STATE

The way Sida Liu praises the socio-legal tradition is worthwhile. Drawing from his readings of Max Weber and Georg Simmel, he distinguishes between the “form” and “substance” of legal systems, leaning on the former to reach the latter:

The powerless approach to the sociology of law that I seek to develop here focuses instead on the formal aspect of law, that is, the shape of the legal system and the social structures and processes that constitute its spatial and temporal dimensions. The powerless metaphor is not to suggest that this alternative approach pays no attention to power whatsoever, but to emphasize that power needs to be situated in these formal dimensions of law in order to understand its structural and processual limits. In other words, I am taking a radical stance on power in this article mainly for the sake of theoretical contrast, but it is important to recognize that these two approaches are not incompatible, but complementary for achieving a comprehensive and integrated sociological understanding of the legal system (2-3).

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However, it is slightly risky to trust this clear-cut distinction. To Max Weber, the young Hans Kelsen, and the late Georg Jellinek, what was at stake was not (or not only) legal systems and a theory of law, but a theory of state too. Law was conceived as a twofold static and dynamic system, and was supposed to get control over the state power of coercion [Macht], structuring “objective” law and guaranteeing “subjective rights” of individuals (and “subjective public rights” of the state). Jellinek wrote the first General Theory of State, containing in Section 3 an equally general theory of public law, in 1900. He tried to trade the tension between the “normative power of facts”, and the “factual power of norms.” Before him, Karl Friedrich von Gerber, Rudolf von Jhering, Paul Laband, Otto von Gierke, the legal scholars issued from the Bismarck era and the German unification in 1871, had transported the main civil law legal concepts [RechtsBegriffe] —the theory of contracts based on individual will and terms like subject, relation, norm, effect and dogmatic construction [Konstruktion]— into the public law. The “theory of subjective public rights” and the new emergent “legal personality” of the state (the state administration, to be precise, as holder of rights on its own name) came up as a result. Jhering defined the law “exactly as it is” in this way: the set of rules [norms] according to which coercion is exercised into a state. “State is the only source of law.” So, rules were represented as having a formal and a substantive (normative) side, and the latter was equated with the social, economic and political content of norms.

We might remember that the theory of Max Weber was born and shaped within this tradition, mainly between 1911 —the date of Jellinek’s death and first formulation of Kelsen’s Hauptprobleme— and 1919 — at the end of World War I and at the birth of the Weimar Republic. State theories tried to fix the situation of the so-called Dual-State of the Bismarck and Whilhelm I and II eras (where different rules operated for citizens in the market, and for state officers and institutions in the public sphere). I understand Liu’s interest for German and Austrian theory: he is also facing the “dual trajectory” of the construction of the legal system in China, that is, state institutionalization/symbiotic market; global legal institutions/competition and struggles at the local level. The “symbiotic exchange” between market relationships and a non-democratic state did not happen for the first time in China (17). There are historical analogues for the present situation. The fin-de-siècle European culture, notably in German speaking areas, adopted the Neo-Kantian language of “substance,” “form” and “limits” (or boundaries), to overcome the “formal” clash between facts and norms that the authoritative state represented. That is to say, to make freedom—the only natural right accepted as natural by Kant’s Practical Reason—happen. To me, this is the kernel, one of the more important issues that we can find in Liu’s work if we read it openly and attentively.

Weber and Simmel could not participate into the harsh debates of the Republic of Weimar: the complex discourses on the political role of the social integration [Integration] that law and the state were supposed to produce. The form, the political form of the state was crucial, and pervaded all the social body. In the end, the polemic around the interpretation of Art. 48 of Weimar Constitution (redacted by Ugo Preuss), led to the Kelsen-Schmitt debate over the “defender of the Constitution,” that is, what democracy meant to the political system. Was it possible to have a rule of law
[Rechtsstaat] compatible with non-democratic political forms? Who should have the final say on constitutional matters?

Weimar represented the first experience of Germany in democracy, a transitional stage in which European lawyers (not only the Germans; see the French — e.g. Joseph Barthélemy) were still thinking that societies could be completely ruled by the law. European jurists belittled violence even after the World War I. State monopoly of violence was just taken for granted. Avoiding internal violence and the risk of war was not a real issue until the aftermath of 1945.²

Back to Weber and Simmel: they did not make the same use of neo-Kantian concepts and categories, nor of the German legal tradition. Weber embodied Jhering’s legal technique and Jellinek’s “empirical types” into his perspective. Simmel didn’t. This is why his conception of social form was so disruptive: his close attention to interactions, to tiny social grassroots reality, challenged the authority of the state. And moreover, Simmel was not interested in building a theory of state, but in setting a theory of sociality [Vergesellschaftung] where norms and regulations could not be linked solely to the authority and power of the state. But this “sociality” has a dark side as well, as nothing prevents political struggles from the conflictive cultural substrate. Nationalism mattered. Interestingly enough, Weber’s attitude in the beginning of World War I was much more tempered than Simmel’s commitment to German national values. Aligned with Paul Natorp and Max Scheler, he would write Werde was du bist [Become what you are] in 1915. War purifying fire would bring the German cultural essence back, as “the experience of life can hardly be based on either the mere materiality of things or the mere charm [Reiz] of their form.”³

LIU’S READING

My impression is that Liu’s account of the classics is more a matter of imagination, seeking for insight, than a literal or historical reading. He indistinctly refers to Simmel and Weber when talking about social forms. Yet, does he really need to go that far just to contend that legal processes are formally structured, situated, and evolving in space and time? The Neo-Hegelian twist of Heinrich Rickert, Ernst Cassirer, and Erwin Panofsky focused on the same point. It was called the empirical “transcendental” (applied to history or sociology). This was the turning point for the Frankfurt School as well.

Many scholars from quite different epistemic and political traditions, such as Peter Gay, Michael Stolleis, Duncan Kennedy, Stanley Paulson, André-Jean Arnaud, Giovanni Tarello, Riccardo Orestano, Jo Eric Khushal Murkens, have analyzed in detail the nuanced distinctions that were proposed in the state theory of that time.

² As it is well-known, Juan Linz, who left Francoist Spain in the fifties and settled as a political scientist at Yale, devoted all his life to address the problem of the multiple forms of totalitarian states and their effects. This is an issue that Liu will have to tackle in the future.
³ “Among those for whom, a hundred times over, life or death was the question of the hour, or even among those at home who have been fully exposed every day to the absolute fate of our time, the experience of life can hardly be based on either the mere materiality of things or the mere charm [Reiz] of their form. If just one inner success of the war is certain, it is that countless among us will now live more off what is essential” (Simmel 2007).
Social and legal thought, from Bismarck to *Vernunftrepublikaners*, are well described in intellectual history as a *philosophy of history*. Thus, the epistemological grounds for Liu’s “processual” sociology of law and the “powerless approach” he is advocating can be situated before the World War I and in the inter-war period. But, of course, he is using this background differently.

If I mention all of this is to show that I cannot follow Liu’s proposals without contextualizing (i) his readings of the classics and (ii) his challenging position before Law & Society approaches. I had to make an effort to understand what was meant by some sentences like the following one: “Whereas recursivity theory focuses on the recursive nature of diagnostic struggles, it overlooks the fact that the structural configuration of the legislative niche is also constituted by the settlement of jurisdictions in the process of diagnostic struggles” (16).

I realized that what Liu was actually discussing was how he could describe better the behavior, struggles and “turf battles” of lawyers in China transitional economy (which is a very good topic, by the way) in dialogue with Terrence Halliday, William Felstiner, Bryant Garth, Paul DiMaggio, and other scholars (2). Therefore he is using the same expressions, assuming that everybody will follow. But perhaps he could explain in a more understandable language that professional behavior has structural constraints, adapts, mirrors or mimics institutional forms, and it is subject to jurisdictional bonds.

As I already suggested, there is a silent discourse between the lines in Liu's discourse. Sooner or later, he will have to face and cope directly with the problems he is raising through his Weber’s and Simmel’s reading: (i) the dynamic structure of the contemporary totalitarian state and (ii) how this process is shaping civil society and culture (and fostering nationalism).

Let us move now to his notion of “powerless approach” for Law & Society studies.

**LIU’S CRITICISM**

According to Liu, the “powerless approach” allows us to put aside the moral, social and political values of the “power/inequality approach” and to concentrate on the “formal” structure and processes of the law (21). There is no need for a “critical” perspective that represents a “continuous constraint to explore alternative ways of doing law and society research.” Even more, “to some extent, becoming a law and society scholar implies a departure from the liberal conceptions of law and a conversion to a more or less critical perspective” (5, emphasis added).

Is this a personal experience? It looks like. Let’s go to the sixties and seventies to sense the times and compare it with Liu’s experience. I always liked the way that Dell Hymes explained the social sciences political turn in 1969, in the introduction of a collective book with an explicit title, *Reinventing Anthropology*:

> We are all in the situation of those in ‘traditional’ societies, whose ‘modernization’ we often consider. The homes in which we were born are frequently houses now torn down; the church in which we married is now a
parking lot; the streets in which we played, the bits of water in which we mused, are mutilated or gone; where we live now is not where we will die. If the need of much of the world is to transform itself economically in order to achieve some parity of power and well-being, we too, must often share in the need to forge new self-identities, to cope with new relations among cultural features, in order to salvage some kind of meaningfulness and symbolic self-preservation for a lifetime. We are all challenged and undermined by technological changes instituted by forces outside our control, forces which may take no note of our traditions or aspirations.

Reinventing Anthropology implied a generational turn. Stanley Diamond, Eric R. Wolf, William S. Willis, Gerald B. Berreman, and Laura Nader were among the authors of the essays, claiming for a more personal, social, and politically responsible anthropology. The book was crossed of non-scientific and non-objective. Especially, David Kaplan wrote an extended review that it is still worth reading about the relations of truth, middle-ground objectivity, moral values and political action. But Hymes’ answer to the reviews by Kaplan and Leland Donald was even more explicit, and equally interesting:

We need to come to terms, for reasons both scientific and democratic, with forms of knowledge that are inherently personal and situational. Knowledge accessible to participants in communities, in particular, is often not accessible to “objective” methods employed by some who govern, administer, and research them. In any case, when many of us object to “objectivity,” we are objecting, not to an ideal of adequate knowledge of reality, but to consequences of certain institutionalizations of such an ideal [Emphasis added, PC] Like others, I have seen institutionalized definitions of objectivity cripple inquiry, waste money, and destroy opportunities for communities and persons with whom one is personally, as well as ethnographically, concerned.

The message was crystal clear: power and the conditions in which ethnography has to be developed needs to be explicitly addressed and stated theoretically, as the interactions between the ethnographer and the context she/he intends to describe are complex and non-linear, and consequences cannot be ignored. To visualize it through an example borrowed from Laura Nader: the massacre of half-million people by government forces in 1964 and 1965 was included only as a footnote in Clifford Geertz’s essay on the cockfight in Indonesia.4

I don’t think things have changed that much since the seventies. On the contrary, perhaps they have worsened. There are lots of reasons to be critical with the use of science and the quest for objectivity in social sciences, law and economy if they do not contribute to solve problems. There are reasons as well to be cautious about strong political beliefs intertwining with empirical research. I agree with Hymes: institutionalization remains a problem, and a big one. But this leads to other

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4 Nader is referring to the Indonesia killings of 1965-1966, where the so-called "New Order" of general Suharto started after the purge of Communists and the killing of estimated 500,000 people by the army, civilian groups and local militias. Political analysts called at that time the Indonesian regime a “bureaucratic polity.” See Karl D. Jackson, Lucian W. Pye (Eds.) Political Power and Communications in Indonesia, Berkeley: University of California Press, 1978. But the facts have been never spelled out (including the possible imprisonment of one million people).
difficult questions. What kind of knowledge has to be applied and is more suited to the main objectives described by Hymes?

Official programs to implement the rule of law and foster economic growth sometimes are more than dubious. Peter Evans, Lant Pritchett, Michael Woolcock, Elinor Ostrom, and many other scholars focusing on developing countries have questioned the Weberian paradigm. The problem is generalization, level of abstraction, and standardization of the intended solutions. They have repeated it many times: these programs often amount to sending “experts” who operate institutions in Denmark to design institutions in Djibouti. At best, “this would be like sending a cab driver to design a car.” However, in spite of their criticism, Law and Society scholars have been driving such cabs as well. Many of them have been directly counseling all sorts of regimes, as researchers working in the main American universities do as a natural move. I think that this being on the activist edge, barracking for one or the other side without taking into account the deep differences of “really distant cultures” is what makes Liu feel uncomfortable, and I can understand that too.

But this is, again, a delicate question. What is knowledge? And what does it mean applying “socio-legal knowledge?” Putting it crudely, what is the difference between a political activist and a hired gun? On the one hand, moral impulse might be at the grassroots of the research, but findings and results lean more on methodology than on the researcher’s will. On the other hand, commitment with a line of thought usually means a line of research studies, not a particular one but many related studies, and this is a constraint also because you get used to watching the world from this particular point of view, and this can be a trap. The more you get involved and committed, the less you are able to question what you are doing.

Back to Liu. The way a researcher gets involved with his time, his informants, his field, and his own research is quite personal. No one except himself can answer this sort of questions. I can imagine the obstacles to carry out a qualitative project under surveillance. But begging the question is not a good strategy. One cannot have one’s cake and eat it. Again, sooner or later, the issue of power and how to deal with it will emerge and, as many other ethnographers before him, he will give an answer on his own behalf. Both Hymes’ and Kaplan’s reflections can help.

THE PRAGMATIC SIDE OF SOCIO-LEGAL KNOWLEDGE

Let’s talk a bit about the production of a theoretical canon. You never know what your actions will bring about, but at least you can control the intellectual arrogance that your position entails to bridge the distance between your initial intuitions and your final results. Between the two ends of this rope, there are multiple ways and possibilities to exercise one’s strength and abilities. Let’s call them affordances, because this is exactly what knowledge does. It empowers, it affords you

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for acting, without any need to be personally in touch with the people who are inspiring you. In this sense, there is certainly a bulk of Law & Society knowledge—that is, knowledge produced by people defining themselves under that label—that is usable and shareable.

However, we are not fully aware, as researchers, of how and what our findings and ideas will be used for. There is a difference between referring to concepts and theories and working out them. Let’s put the example of the litigation pyramid by William Felstiner, Richard Abel, and Austin Sarat (1980-81). Liu is quoting it when explaining his “eventful temporality perspective” (20). He discusses the pyramid as a conceptual construct, as a whole, to show how his own position differs. As it happens in this kind of texts, the original ideas are quoted and paraphrased for the sake of the discussion. Therefore, for Liu, it “is essentially a stage model, in which the transformation of disputes occurs step by step following a fixed sequence and leading to a dispute pyramid” (20).

But there are multiple ways of approaching to the same topic, depending on the kind of problem you want to solve. Some years ago I had to coordinate a long study on the implementation of ADR in Catalonia (Spain) (Casanovas, Magre, and Lauroba 2010). In 2000, Catalonia was a territory with 6 million inhabitants. In 2010, population had increased by 7.5 million. It is easy to see the impact of such immigration rates on services and facilities, from hospitals and health care units to schooling and courts. To cope with such a sudden increase, people reacted in several ways. Some negative: 700,000 inhabitants migrated internally in those years. Some positive: Alternative processes to settle disputes were set in many private and public facilities to avoid violence and manage conflicts. In schools, where this cultural melting pot was immediately visible and was causing thousands of frictions, kids learned negotiation and mediation methods that were put in place by the students themselves, as a part of their regular curricula.

We found that in 2008 141,602 mediations were carried out and 154,384 people were mediated. In other words, more than 2% of the Catalan population was involved in a mediation process, and from 10 to 15% had performed some kind of action “near mediation” (supporting activities, such as informing or translating). Some evidence was needed about the viability and sustainability of institutionalizing ADR and mediation in the country. Thus, we looked at the heavy court caseloads and at litigation floods to show how the judicial deadlock was produced. In 2010, the number of cases turned to judicial bodies in Catalonia was 20 per 100 inhabitants. In other words, one out of five inhabitants had a lawsuit in court. This could be counterbalanced implementing mediation processes connected to the courts, as it was the case.

We took the litigation pyramid as a template. We couldn’t reproduce it because we lacked fundamental data at the base, so we transferred the original idea into a new mold. Montserrat Guillén and Mercedes Ayuso sorted different scenarios out of it to calculate and project litigation rates (with and without mediation) for the coming years in Spanish courts. They foresaw that the extension of ADR to only 20%
of court cases could save 4 million € a year.\(^6\) Thus, we did not quote or refer to the litigation pyramid, but we acted upon it as a blueprint for a different kind of new methodological usage.

I think most of Law and Society knowledge is produced through this active, contextual, problem-solving way. I could mention other examples. Liu states that a “persistent skepticism of ‘law on the books’ is often associated with a strong emphasis on unequal justice and the power dynamics of ‘bargaining in the shadow of the law’” (1). Well, bargaining in the shadow of the law is not a theory, but a useful descriptive model and as such has been assumed, by John Zeleznikow and Emilia Bellucci, among many others, to set a contextual framework for Online Dispute Resolution tools (ODR). I am sure that Zeleznikow and Bellucci knew almost nothing about its underlying philosophy, because this is not what they required to develop their work. The same occurs with Liu's interpretation of Stewart Macaulay's work. Or with my own: I did not need to assume that “law is not free” to develop a relational approach to law, partially based on the notion of Macaulay's non-contractual relations, and on Ian McNeil's notion of relational contract.

LAW & SOCIETY

Is this pointing at a “theoretical canon” (2)? I cannot see the need for such a thing. How could Law & Society produce it? For what it seems, Law & Society is a huge academic association comprising an amazing set of topics, interests, fields and methodologies. It is a kind of academic market as well, spotting out people interested in all kinds of empirical approaches to law, and helping them in a wide range of objectives, from presenting scientific findings to looking for jobs.

In the sixties and seventies, this empirical trend alone justified the presence of such an association. I am not repeating here the L & S story at the Law Faculties in the mid seventies. Still in 1995, Richard L. Abel could write: “When asked I study I usually respond gnomically: everything about law except the rules.” It was not really an assertion (and so, it could not be true), but rather a challenging motto that became a flagship for socio-legal scholars.

Nowadays, things are different. The converse sentence cannot be true either—everything about rules except the law. Scholars working on social structure, human and artificial agency, institutions and social systems cannot get rid of rules, norms, protocols, standards and the like so easily. They have to work legal theory, analytical languages, and empirical approaches to law at the same time. This is my case. I work on regulatory matters in artificial and hybrid contexts. When asked what I study I usually respond: Law & Society. Everybody understands, as it is a useful label in technological workplaces.

I think that Sida Liu perhaps is taking “Law & Society” (a movement, an academic association) and “Sociology of Law” (a field of research, a discipline) as synonymous, and this can lead to misunderstandings of his work. Moreover, his work

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focuses on legal professionals, and more specifically on lawyers and lawyering only. There is a huge distance, then, between the scope and the kind of data he is currently managing and what it would be needed to discuss such an issue in a more general level.

Liu writes at the end of his paper: “Arguably this is a radical departure from the conventional wisdoms in the field, yet my ultimate goal is not to construct another abstract sociological theory of law, but to establish the theoretical scaffolding for understanding empirically the legal system’s formal shape and how it changes over time” (21).

Setting an epistemological scaffold (an ephemeral one, just to get data sorted) is not building up a social theory. Yet, I am afraid that despite his claims this is what he is getting in the end. Liu sets a sociological approach explaining and interpreting at the same time lawyers’ behavior by means (i) of the particular philosophy of history that describes the emergence of "formal" structures (in the phenomenological or comprehensive sense: legal systems, normative systems, political structures) and (ii) stemming from behavioral struggles, interactions, and micro-situations. But, what links? How has the micro-macro link has been solved? How does the process of connecting and inferring sociological knowledge from raw social data work?

I do believe that obtaining situated and detailed contextualized knowledge is essential. But I also believe that methodology and the language chosen to analyze and convey findings is what allows the researcher to become interdisciplinary and discuss the results with other related disciplines, because he can show and compare the consistency of his findings.

After the seminal Dartmouth Seminar in 1956, the cognitive revolution and the linguistic turn in the seventies and eighties, the Internet in the nineties, the Semantic Web on the eve of the new millennium, Web 2.0 ten years ago, and the recent Web of Data and Big Data Science, it is difficult not to pay attention to new ways of integrating ethnography, statistical analysis, technology and socio-legal approaches. Formal methods seem well-suited to analyze and model everyday practice, interaction and planning of legal players and stakeholders (not only lawyers and judges, but rulers, managers, officers, administrators, even artificial agents performing legal roles and functions). But, to my knowledge, Liu never uses the bulk of tools that are available to him to perform his analysis. He jumps, then, from the gathering of qualitative data to general theories without modeling, and this hinders his ability to disclose and discuss openly the internal thread and consistency of his reasoning. In online ethnography, for instance, at least Twitter, sentiment, and network analyses, plus Natural Language Processing techniques, could help him to discover (or to test) patterns that are usually not visible at first sight.

I am aware that what I am saying is more a suggestion than a criticism. Liu's findings about the behavior and practices of Chinese lawyers are real findings, and assessments should be focused on them. He cannot be blamed for not doing what he was not intending to do. The problem arises at the epistemic level, when he generalizes his assumptions to a general methodology and to a canon for all legal

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7 The 1956 Summer Dartmouth Conference was the starting point for the new discipline of Artificial Intelligence.
processes. To be fair, I openly avow my “law and technology” bias. And to be clear, I believe that Liu leverages at his best his socio-legal background, but perhaps he could explore other conceptual possibilities as well. He is raising some important but difficult topics that would benefit from a closer attention to analytical concepts that are common in other related fields—Cognitive Science, Social Ecology, Behavioral and Ecological Economics, Computer Science and Artificial Intelligence. These concepts include language, information, data, metadata, modeling, processing, memory, and knowledge.

We are living another scientific and technological revolution. After reading Liu’s final statement, I am not sure that the core of Law & Society scholars are coming to terms with it, to use Hymes’ expression. This is a new challenge. Socio-legal knowledge on rights, conflict management, transitional justice, judicial systems and other commonly studied issues is much needed to face the new “social ecosystems” in corporate, organizational, and administrative settings. Especially for debugging what Ed Feigenbaum (1984) called the “knowledge acquisition bottleneck.”

Doing that, the notion of what we understand by “law” and “practicing law” necessarily changes. It is not “techno lawyering” that is at stake, but crowd, social and collective intelligence, the Semantic Web stack, the architecture of the Internet of Things, and how all of this is producing deep changes in social and professional practices and behaviors. People are becoming ubiquitous and de-located in space and time, global and personal. And communities and groups are experiencing the same type of phenomena. Law & Society researchers could explore and describe them thoroughly without losing their roots and their own bulk of already acquired knowledge. I could not detect any reference to these issues in Liu’s elaborated proposal.

There are two natural moves to take in the next future: (i) analyzing and modeling socio-legal knowledge from this point of view as well, and (ii) teaming up to tackle empirical studies using different strategies and expertise (not leaving people alone in the field, gathering painstakingly small amounts of data). Hymes’ “forms of knowledge that are inherently personal and situational,” “knowledge accessible to participants in communities,” are processable today in a more objective way. I am afraid that the legacy of Herbert Simon, Allen Newell, Marvin Minsky and John McCarthy, the computer scientists who coined the term “Artificial Intelligence” at Dartmouth, is not mainstream in Law and Society agenda, or not yet. I’d like to be wrong, because this is what is making the powerless approach to socio-legal research effectively powerless.

Raising, identifying and describing problems are even more important than setting general theories. Therefore, I encourage Liu to follow the integrative way in which he is conducting his research on Chinese lawyers and institutions. All relevant data should be taken into account. There is no need, on the contrary, to institutionalize the discourse itself, turning it into a Report to an Academy and edging a kind of Anti Anti-American pragmatic paradox with dichotomies that replicate themselves.
Thinking of a new open society to design in common better institutions, as Liu is already doing too, seems a better goal for Law and Society scholars. I am convinced that this is where our future lies.

REFERENCES


Response to Garth and Casanovas


First of all, I would like to thank the Law & Social Inquiry editors for inviting two distinguished sociolegal theorists to comment on my article “Law’s Social Forms.” Both comments are very insightful and provocative, and I appreciate the opportunity to offer a short response to some of the issues that Professors Garth and Casanovas have discussed. I believe that such serious theoretical exchanges are what contemporary law and society scholarship desperately needs to move forward in the twenty-first century. My original intention of writing this article was precisely to initiate such a conversation among law and society scholars.

Casanovas’s excellent overview of the German and Austrian historical lineage to Weber and Simmel indicates the neo-Kantian origin of the form/substance distinction that I adopt in the article. He is correct that my objective is not to trace the intellectual origin of the formal approach to sociolegal studies, but to use Weber and Simmel, two founding figures in sociology, as the starting point of a new spatial and processual theory of law that I have been developing in recent years. I engage more with the American law and society tradition than with European social theories in the article because the former has been more dominant in contemporary sociology of law, especially in empirical studies. It is also my hope to make the powerless approach that I outline in the article applicable to empirical research on various aspects of the legal system in future work.

As a long-time follower of Pierre Bourdieu, Garth’s comment on my dismissal of power in the article is much expected. The Bourdieusian approach is a great example of the theoretical combination of social space and power, which I have always regarded as one of the most innovative social theories in the late twentieth century. Dezalay and Garth’s applications of Bourdieu, however, emphasize more on power and capital than on the social structure of the juridical field. To some extent, their orientation also reflects the dominance of the power/inequality approach that I discuss in the article. If I were to conduct a

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Bourdiesuan analysis of the field of law in Asia or Latin America, I would put more emphasis on mapping the spatial configuration of this field, that is, how different legal actors are located into various positions and how their power struggles shape the social structure of the field. In other words, my primary theoretical interest lies in the structures and processes that constitute the formal shape of the legal system, regardless of their relations to power and inequality. The “powerless” metaphor is not to put power and political economy aside as Garth suggests, but to highlight the theoretical distinction between the forms and substances of law, as well as the necessity of treating social forms as independent units of analysis in law and society research.

This leads to a basic question that Casanovas asks, namely, where does my idea of law’s social forms come from? In addition to Weber and Simmel, the most significant influence on my thinking originates from the Chicago School of sociology. Since my Ph.D. years at the University of Chicago, I have been fascinated by the fact that most Chicago School sociologists (from Robert E. Park to Ervin Goffman to Andrew Abbott) downplay the importance of power and domination in their writings. This theoretical orientation reflects the enduring influence of Simmel, but it is also related to the ecological view of society that Park and Burgess developed in the 1920s, which resembles a plant ecology in which various plant species coexist and develop through their mutual interaction. Ecology is a powerless approach because its focus is on how actors produce social structures through processes of interaction, not how some powerful actors dominate others and produce social inequality as in Bourdieu or the Marxian tradition. Despite the Chicago School’s influence in various areas of sociology, there have been few attempts to use its spatial and processual approach in the sociology of law. The same thing can be said of Simmel’s formal sociology. My article, therefore, is an effort to fill in this theoretical gap by proposing a preliminary research agenda. The form/substance distinction may seem neo-Kantian to Casanovas and other European scholars, but its “blood” is deeply sociological and, to some extent, not only Chinese but also American.

Casanovas argues that most law and society scholarship is produced in an “active, contextual, problem-solving way” and thus there is no need for the so-called “theoretical canon.” Garth also points out that the progressive orientation of law and society scholarship may reflect the legal profession’s “legitimation concerns about serving the underserved and providing access to legal careers.” While I totally agree that all classics were produced in the particular time, locations and cultural contexts of their writing, I do see the value of a theoretical canon. As a sociologist, I have always been puzzled by the fact that some areas such as organizational sociology or political sociology have strong and clear lineages from classical social theories, whereas theoretical foundations of the sociology of law are ambiguous, messy, and increasingly thin. Even the shadows of Marx, Durkheim, Weber, and Simmel are rarely found in contemporary sociolegal studies. Perhaps the scope of “law and society” is broader than the “sociology of law” for researchers with other disciplinary backgrounds, but at
least for the latter concept, some serious theoretical work needs to be done and my article is an attempt to reconnect contemporary law and society scholarship with classical social theories.

Finally, let me respond to Casanovas’s question about the role of my personal experience in this article. While my interest in social forms dates back to my childhood obsession with Euclidean geometry, it is the recent experience of teaching the sociology of law at the University of Wisconsin that made me increasingly puzzled by and dissatisfied with the overwhelming emphasis on power and inequality in American law and society scholarship. Working with legendary law and society scholars such as Marc Galanter, Stewart Macaulay, David Trubek, and Howard Erlanger also raised my curiosity of the historical and social contexts of this scholarship. The law and society movement has passed its fiftieth anniversary and I fear that it is experiencing a “midlife crisis” after the retirement of its founding figures. It is the task of younger generations of sociolegal researchers, including myself, to keep this movement alive and well in the United States and globally. I think the best way to do this is to push the boundaries of the “sociolegal imagination” with a radical departure from its core theoretical legacy, or what I refer to as the power/inequality approach. As Casanovas points out, the powerless approach that I have outlined in this article is indeed a jump from empirical research to general theory without a full-fledged toolkit of concepts and models. It will probably take me another 10-20 years to work out all the details, but I sincerely believe that a spatial and processual perspective would enrich the theoretical canon of law and society and generate a variety of interesting empirical projects beyond the scope of existing scholarship.