LETTER FROM THE CHAIR
Theory in the Trenches
Greta R. Krippner - University of Michigan

JUNIOR THEORIST SYMPOSIUM 2020
Call for Papers and Support

MECHANISM AS METAPHOR
Conversation on “The Multivalence of ‘Mechanism’ in Sociological Explanation”
Carly R. Knight - New York University
Isaac A. Reed - University of Virginia
Interviewed by Luis Flores - University of Michigan

SOC-FI: SOCIOLOGICAL FICTION

BOOK SYMPOSIUM
The Crisis of Expertise, by Gil Eyal
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CIVIL SPHERE CONFERENCE CALL FOR PAPERS

RECENT MEMBER PUBLICATIONS
As I suspect is the case for many members of the Section, my identity as a social theorist is inseparably tied to my identity as a teacher of undergraduate and especially graduate courses on sociological theory. Certainly, my engagement with and commitment to especially classical sociological theory has been deepened by my experience teaching the first-semester theory course to incoming graduate students at the University of Michigan almost continuously now for well over a decade. The task of brushing the dust off of canonical texts and making them sparkle anew for fresh recruits to Sociology – whether or not they see themselves as inclined toward theory – is an exhilarating challenge that defines the fall for me as much as shortened days and turning leaves.

Lately, this challenge has become a more daunting one. The canon is now under assault in a way that requires those of us who have oriented our teaching careers to transmitting these works to new generations of students to pause and take a step back. Graduate students have become increasingly skeptical – and in some cases, totally dismissive – that works written by “dead white men” (namely, Marx, Weber, and Durkheim) could have produced insights that are in any way relevant to the contemporary practice of sociology. My own syllabus is particularly vulnerable to these critiques since it begins not with classical sociological theory per se, but with the utilitarian/liberal tradition in political thought against which Marx and Durkheim in particular reacted in constituting a nascent sociological approach. (If you think that it is difficult to convince skeptical graduate students that there is any utility in reading Marx, try convincing them to read Locke!)

I had been aware for some years that there were rumblings around these issues, but it wasn’t until my students mounted a revolt against the canon last fall that I fully understood the depth of their discontent. I’ll admit that initially this seemed to me like a contest between the wisdom of age versus the radicalism of youth. Of course, I wasn’t totally blind to the limitations of the “dead white guys”: who could deny that there are critical exclusions in these texts, and that reading them appropriately requires addressing those exclusions? I was prepared to do this and soldier on in service of the
discipline—or at least I thought I was—but my students were reluctant to engage in this enterprise with me. I tried a lot of arguments to convince them, from inventoring concepts that these thinkers developed that still provide the basic conceptual framework of the discipline, to suggesting (in my most desperate moment) that to completely reject these thinkers was akin to a form of professional suicide.

Two-thirds of the way through the semester, the students had dug in, and I was losing faith that there was any value in my approach to teaching theory. I set aside a class period in which we could critically discuss the construction of the sociological canon, hoping to air the tensions, engage them constructively, and get back on track to finish the semester. But instead of dissipating the tensions, the session amplified them, and the classroom erupted in expressions of fury and despair that had no precedent in my teaching career. In the lingo of college teaching, it was a “hot moment”—or more accurately, a “hot two hours.” What affected me most was the pain students reported experiencing upon reading many of the texts I assigned in the course. I told them that the texts were painful because the world they reflected—constructed—was in fact full of pain, and that as sociologists we couldn’t avert our eyes from the ugly reality of a social world built on hierarchy and exclusion. But of course, I was well aware in saying this that, for me, reading Locke, Malthus, or Marx was an intellectual exercise; I did not feel personally assaulted by these texts and their silences. I realized I had to grapple with a gaping chasm between my experience and what my students were experiencing in encountering the classical canon.

What to do? As the semester concluded, I began to contemplate what a different approach to teaching theory would look like. Those ruminations continued over the following year, and into this fall when—with considerable trepidation—I rolled out a new version of the course. Here is how I explained the objectives of the course—and my approach to teaching it—on my syllabus:

*The main purpose of the course is to provide a survey of some of the key themes and recurrent questions that have motivated sociology as a discipline from its founding to the present. This immediately introduces the problem of the “canon”—a problem that has roiled the discipline in recent years when the canon’s many exclusions and blind spots have been foregrounded. Rather than leaving such problems to one side, we will integrate them as fully as possible into our discussions. The intention here is neither to encourage blind reverence for works that contain obvious flaws and limitations, nor to encourage the outright dismissal of whatever insights these works might offer, but to excavate these texts for a series of problems that are necessary to make sense of the contemporary field of sociology.*

“Inhabiting the space between ‘blind reverence’ and ‘outright dismissal’ is easier said than done...”

Inhabiting the space between “blind reverence” and “outright dismissal” is easier said than done, but I followed several guiding principles that have made this possible. First, rather than approaching these texts in terms of their context of production, I have attempted to situate theories by examining their conditions of possibility. It is perhaps a subtle shift, but I think a critical one. By “context of production,” I mean an analysis of the particular circumstances in which specific theorists wrote, including the conventional ideas available to them at the time they fashioned their ideas. This of course is the bread and butter of theoretical exegesis, and in certain respects it is essential to interpreting the meaning of a text. But there
is the temptation here to explain too much away, giving theorists a “pass” by suggesting that we ought not judge them by moral and social values “ahead” of their own time. Instead of engaging texts from this angle, I focus on the historical “conditions of possibility” that determine the production and selection of these texts as “canonical,” as well as the exclusion of other texts from this elevated status. Put differently, the emphasis here is less on celebrating or condemning particular theorists for what they did – or did not – write, and more on developing a sociology of knowledge that helps us to understand how the ideas that underpin the discipline gained their position of privilege.

A second orienting principle is to seek continuous dialogue between canonical texts and perspectives that are implicitly or explicitly excluded from those texts. In my reconstructed syllabus, I do this by systematically pairing readings: we read Locke’s *Second Treatise of Government* against Patricia Williams’s essay, “On Being the Object of Property”; we interrogate Adam Smith’s musings on the division of labor against Paula England’s critique of dichotomous thinking in economics; we study Marx’s historical materialism against Cedric Robinson’s early formulation of “racial capitalism”; the Marx of *Capital* is read against feminist writings on care work; Weber’s “Bureaucracy” gets placed side-by-side with Joan Acker’s and Victor Ray’s attempt to “gender” and “racialize” organizational theory, respectively; Gayle Rubin’s “Traffic in Women” is situated in a global context by virtue of an encounter with Kimberly Hoang’s *Dealing in Desire*, and so on. In each such pairing, the question is whether a particular exclusion is integral to the text, or merely incidental to the author’s argument and meaning. If we re-narrated the theory fully attending to social difference, would that theory operate in basically the same way or would its architecture and logic be fundamentally changed?

The third organizing principle of my revamped course is the concept of “repurposing”: we consider whether and to what extent theories that are built on problematic foundations (when considered from the vantage point of race, gender, sexuality, the non-Western world, and so on) can nevertheless be directed toward ends that are different than those imagined by their originators. There are many instances of such repurposeings in social theory. Perhaps my favorite such maneuver is Gayle Rubin’s appropriation of Claude Levi-Strauss and Sigmund Freud in order to build a feminist theory of the origins of gender subordination. Rubin observes:

“...the emphasis here is ... on developing a sociology of knowledge that helps us to understand how the ideas that underpin the discipline gained their position of privilege...”

“...The place to begin to unravel the system of relationships by which women become the prey of men is in the overlapping works of Claude Levi-Strauss and Sigmund Freud. The domestication of women is discussed, under other names, in both of their oeuvres. In reading through these works, one begins to have a sense of a systematic social apparatus which takes up females as raw materials and fashions domesticated women as products. Neither Freud nor Levi-Strauss sees his work in this light, and certainly neither turns a critical glance upon the processes he describes. Their analyses and descriptions must be read, therefore, in something like the way Marx read the classical political economists who preceded him. Freud and Levi-Strauss are in some sense analogous to Ricardo and Smith: they see neither the implications of what they are
saying, nor the implicit critique their work can generate when subjected to a feminist eye. Nevertheless, they provide conceptual tools with which one can build descriptions of the part of the social life that is the locus of the oppression of women, of sexual minorities, and of certain aspects of human personality within individuals.”[1]

Of course, repurposing theory is no simple exercise. As Rubin notes later in the same essay, “Levi-Strauss and Freud write within intellectual traditions produced by a culture in which women are oppressed. The danger in my enterprise is that the sexism in the traditions of which they are a part tends to be dragged in with each borrowing.”[2]

Herein lies the key conundrum posed by attempts to repurpose theory: can we lift concepts out of their original contexts and shape them to new ends, or are those concepts forever contaminated with the mark of their origins? There may not be one answer to this question, but I appreciate the formulation offered by one of my students this fall: “You eat the meat, and throw away the bone.”

As the semester draws to a close, I do not feel I have resolved all the issues involved in teaching theory in the transformed landscape of the present, but I do feel like I’ve learned some things – and will continue learning with the assistance of my students. Since I know from conversations with colleagues across the country that I am not alone in facing new challenges in the theory classroom, I’ve organized the program at next year’s ASA to engage in and reflect on these challenges. One invited panel on “Canonization” will explore the mechanisms by which some authors and texts are elevated to – and others excluded from – canonical status. An open panel on “Globalizing Social Theory” will interrogate the Western biases of sociological theory, and attempt to correct these biases, without reproducing a dichotomous mapping of the terrain of social theory (e.g., “the West” versus “the Rest,” or the “global South” versus the “global North”). A second open panel on “Heretical Theories” seeks to encourage precisely the kind of “repurposings” I’ve described here, inviting borrowings from unexpected (and sometimes controversial) sources to produce novel insights that can be applied to some of the most pressing problems our society currently faces. (A fourth invited panel on “Democracy in Distress” is not directly related to the themes I’ve explored here, but hopefully the motivation for that panel is clear in the current context.) I invite you to review the call for submissions (https://www.asanet.org/sites/default/files/section_calls.pdf) and look forward to your contributions to these various conversations!

With best wishes to all for a peaceful and prosperous New Year!

Greta

It is our honor to announce that Clayton Childress (University of Toronto), Shaneda Destine (University of Tennessee, Knoxville), and Hillary Angelo (University of California, Santa Cruz) will serve as discussants. The symposium will feature an "after-panel" titled "Theorizing for Troubled Times" (Panelists to be announced) and a keynote address from Kyle Green (SUNY Brockport) and Daniel Winchester (Purdue), winners of the 2019 Junior Theorist Award.

We invite all ABD graduate students, postdocs, and assistant professors who received their PhDs from 2018 onwards to submit up to a three-page précis (800-1000 words). The précis should include the key theoretical contribution of the paper and a general outline of the argument. Successful précis from last year’s symposium can be viewed here. Please note that the précis must be for a paper that is not under review or forthcoming at a journal.

Please remove all identifying information from your précis and submit it via this Google form. You can contact symposium organizers at juniortheorists@gmail.com with any questions. By mid-March we will extend up to 12 invitations to present at JTS 2020. Please plan to share a full paper by July 17, 2020. Presenters will be asked to attend the symposium in its entirety in order to hear fellow scholars’ work. Please plan accordingly.

Conference Organizers
Andy McCumber - UC Santa Barbara
Jaclyn Wypler - University of Wisconsin
MECHANISM AS METAPHOR
CONVERSATION
WITH CARLY KNIGHT
AND ISAAC REED

Interviewed by Luis Flores, Perspectives Co-Editor

In their article “Meaning and Modularity: The Multivalence of ‘Mechanism’ in Sociological Explanation” (2019, Sociological Theory), Knight and Reed disentangle and explore contradictions between “modular” and “meaningful” mechanistic models. The disjuncture, they argue, is grounded in “incompatible causal foundations and entails mechanistic models with distinct and conflicting evidentiary standards.” Below is a conversation with the authors on their provocative article.

Luis Flores (LF)
Invoking Clifford Geertz, your paper opens by suggesting that the concept of “mechanism” in sociology has surpassed a “first flush of celebrity” and is now ripe for an “operation of disentanglement” (234). Could you say a bit about how the idea for this paper emerged, and perhaps, what signs indicated for you that the “mechanism” idea in sociology had arrived at this stage?

Carly R. Knight (CRK)
We first had the idea for the paper a long time ago. My own background with mechanisms came from a causal inference perspective. But I think it was the publication of several theoretical pieces that were trying to pin down a sociological definition of mechanisms that got us thinking about whether these approaches were all compatible. Particularly, I remember reading the Handbook on
Analytical Sociology, Neil Gross’s 2009 ASR piece, and Matthew Norton’s Sociological Theory piece all for a class on explanation and feeling that there was a lot of heterogeneity that could be unpacked there. At the same time, it seemed that some researchers were using terms like “mechanism” and “process” interchangeably. When Isaac and I were talking about this one day, we got to wondering if there was anything more we could say about the difference between “mechanism” and “process” and what distinguished these various approaches.

Isaac A. Reed (IAR)

Yes, the paper developed over a long period of time, and I think that was a positive. Whatever clarity it has, was achieved, at least in part, via an extended period of refinement of the original idea. It was really an intense process of writing and rewriting, exchanging drafts over email, and so on—though often, also, of leaving the paper and coming back to it with fresh eyes until we really got it right. For my part, I felt after finishing Interpretation in Social Knowledge in 2010, that the discussion of mechanisms could have gone much further therein. I knew there were missing links between (1) discussions in the philosophy of science around mechanisms and discussions in sociology on the use of concepts in empirical analysis, and (2) the counterfactuals and causal inference perspective in sociology and cultural sociology. I also knew there were flaws in what was going on with mechanisms in sociology, but I wanted to get at them in a way that was truly pluralistic, and which tried to find missing links that were productive for future work in theory and research.

LF

You note the diffusion of the concept but stop short of sketching a genealogy of the drive for mechanistic explanation. We were curious to hear about the historical development of modular and meaningful mechanistic approaches—is there anything to our intuition that the modular form emerges first and meaningful approaches sought, in part, to adopt mechanistic explanation for different purposes? Do you have a sense of the disciplinary or intellectual problems/currents that drove the expansion of the mechanistic idea within sociology—perhaps, as you note, as a placebo for disciplinary ailments?

IAR

This is an interesting question. Certainly, doing such a genealogy would be interesting and worthwhile. I think it would be a bit more fragmented than you suggest, because one would need to include not only the trajectory of thinking about causal inference, but also a more diffuse but powerful intellectual formulation in sociology—including sociology that is quite “humanistic” or “cultural”—which is the sense that some aspects of the social world are quite regularized, but in a way that is not simple or easily grasped by just “looking on the surface” of social life (though I would note that surface/depth is also a metaphor).

What I mean by this is that there has always been a question: is the industrial (and now, algorithmic) social world mechanistic, in the same way as its factories (or AIs) are? On the other hand, the metaphor of mechanism has also been repeatedly used to describe supposedly pre-modern practices, such as ritual or gift exchange. The point is that I think that the metaphor of mechanism is perhaps quite old and widespread in work that calls itself sociology, though perhaps not always by that name, or not always in a codified way. I suspect that it arises when, for varying reasons, we find ourselves reaching for a study of regularity that is also a study of depth.
This is, perhaps, why the genealogical project would be a bit different than what we are doing in the paper, where we take up explicitly the project of “what we talk about when we talk about mechanisms,” now, in this (loosely defined) community of inquiry. Nonetheless, you are right that there is a link: the mechanisms concept, as it emerged into our current language via modular and non-modular accounts, appeared to merge the striving for causal explanation and the more interpretive/historical sensibility of a “mechanistic world” in one fell swoop. Our point is that there is a way to carefully articulate both ambitions, but it is not the way we have chosen so far. And so: there is more work to be done and we hope other folks will take up the larger project implicit in our paper. That is to place under the microscope of intense thought the specimens of sociological thought in our own time, which is to say, our concepts.

CRK

I agree with Isaac that a genealogical project would be fascinating but difficult—and a bit different from what we were up to. At a very basic level, it seems clear that mechanisms, at least as they have been most recently conceptualized, were meant to be a response to the idea that social science was about discovering generalizable “social laws” or “covering laws.” That’s at least how Hedstrom, Swedberg, and Elster, among others, framed the promise of mechanisms: we are not looking for laws, we are looking instead for empirical generalities. But my guess is that different conceptualizations of mechanisms will reveal very different genealogies. Apart from what Isaac mentioned with respect to mechanisms in causal inference versus cultural approaches, there’s a big difference between conceiving of mechanisms as forms of social explanation—the way Hedstrom and Swedberg do or the way Analytic Sociology does—and conceiving of mechanisms as the basis for causation—the way critical realists do. These approaches pull from very different theoretical traditions such that it is sometimes not really clear how much these approaches share, beyond similar terminology. Because of this, a genealogy of “mechanism” would be both incredibly useful but also likely very unwieldy.

IAR

Right! Note how both Swedberg, Hedstrom et. al. were offering “depth” in their response to the “surface” causality of covering laws and certain habits of statistical analysis; Bhaskar and the critical realists also offered “depth” in response to positivism. But when you dig down, is the use of the metaphor the same, in terms of the logic of theory and research that is implied? Perhaps not… this is the kind of thinking that led to our writing the paper together.

CRK

In pointing to heterogeneity within a seemingly unitary term, your paper is reminiscent of Gabriel Abend’s (2008) “The Meaning of Theory,” also in Sociological Theory. However, instead of arriving at a “semantic predicament,” you end by identifying a metaphorical one, laden with the politics of epistemic authority. Recognizing the “metaphorical” uses of mechanism, you argue, means abandoning a degree of “epistemic capital” (248). This loss, as you note, may be particular to proponents of meaningful mechanisms. If unveiling the metaphorical character of “mechanistic explanation” exposes asymmetries of epistemic capital, what might be the next challenges for sociological theorists hoping...
to support something like epistemic pluralism, or at the very least, a clearer division of labor in social inquiry?

CRK

It is really nice to have our paper compared with Abend’s "The Meaning of Theory" since I see our paper as engaged in a very similar style of project. And while we do not explicitly tackle the normative question Abend refers to as a “semantic predicament”—in our case, how ought sociologists use the term “mechanism”— I think we do end up in a similar place to Abend. By that, I mean our ultimate goal was to argue for a pluralistic approach and to acknowledge, much like Abend does with theory, that the multiple ways that mechanism models were being used were already generative. So even though we believe attempts to nail down a single definition of “mechanism” would be unlikely to resolve the inherent contradictions among different approaches, the actual practice of using a plurality of approaches is useful.

In describing mechanisms as metaphorical models, our goal was to point to the fact that (1) mechanistic explanation isn’t some grand unifying endeavor and (2) mechanistic explanation in and of itself does not solve the causality question. If this is the case, then, we hope abandoning mechanisms’ epistemic capital is a way to open the door for other similar kinds of meaningful models—like narrative, for instance. I also want to say that I don’t think our argument is particularly deflationary for meaningful mechanistic approaches. If anything, we had hoped to point to just how few of the relationships in our social world evince the kind of independence modular models require.

As for supporting epistemic pluralism, I am pretty optimistic on this front. I think social theorists should continue the projects they are already engaged in—that is, specifying how our explanations work. For example, in the realm of comparative/historical sociology, I was part of a group trying to tackle the question of what case-study comparison was doing if it wasn’t principally aiding in causal explanations. If, for example, the Mills method doesn’t successfully establish causation, then why do we compare? These are the kinds of questions social theorists are already working on and, at least from where I stand, epistemic pluralism seems alive and well.

IAR

I think sociological theorists who wish to pursue this sort of epistemic pluralism should examine concepts that are used with high frequency in sociology, and really try to arrive at careful and robust understandings of them. Note that I write “understandings”, not “definitions.” This is not so much a definitional issue as one of meaning in communities of inquiry, and that means pursuing something like the method of working that we did in this paper—combining philosophical reflection with analysis of sociological practice, and both of those with some argument about what we should mean by these concepts. I think one might call the project “theory as concept development,” and the method “working epistemics.” Or something like that.

LF

A central task in mechanistic explanation, you
highlight, is slicing through social complexity, “abstracting away a messy social reality into tractable and generalizable models” (238). The distinction between the two approaches revolves around the modular versus relationally interdependent character of these incisions. This core criteria of mechanistic explanation, however, seem to be shared by macro/grand theoretical traditions: approaches to field theory and Marxist class analysis, for example. We’re curious about where you see distinctions and similarities in this “carving of complexity” between mechanistic explanation and what is often called “grand theory”? The question seems prescient, since an appeal of mechanistic explanation has, to a degree, been its “meso-level” departure from the styles of abstraction in “grand” theories. It some ways, grand traditions prescribe the carving up social complexity into more or less standard pieces, but as you argue, approaches to “carving up” in mechanistic explanation are not fully deductive, but follow from ontological presuppositions.

CRK
This is a great question. Mechanistic models are certainly not alone in this endeavor at abstraction. Any form of social scientific explanation is necessarily engaged in abstraction since all models are idealized representations of complex phenomena. That’s true not just of mechanism models but also economic models, scientific formulas, maps, etc. We argued that what distinguished the meaningful from the modular mechanistic models was the relationship between these models’ components parts. That is, modular mechanisms models require a kind of independence among their components that just doesn’t hold for a lot of the social world that we might wish to model, for instance, the kinds of interactions or semiotic relationships that characterized meaningful mechanism models.

You raise a really interesting question about the difference between how grand theory versus middle-range theories go about modeling complexity. Generally, the distinction between grand theory and mechanisms is made in reference to levels- “macro” versus “meso” explanation, as you say. I think another fruitful way of characterizing that difference comes from the philosopher Philip Kitcher’s work on explanatory unification. He doesn’t discuss social scientific grand theory, exactly, but the general idea is that powerful, deep explanations are those whose abstracted models are able to unify seemingly distinct events and regularities with a minimal theoretical apparatus. Laura Franklin-Hall, who we cite in our paper, makes a similar point about how high-level explanation work by offering causal economy—high-level explanations give a lot of “bang for their buck.”

So grand theory isn’t only giving us an ontological architecture with which to describe the social world— it is also makes otherwise confusing social reality coherent by showing how disparate events are manifestations of similar underlying social forces. That is part of what makes them so attractive. Apart from that, any kind of grand theory is obviously extremely committed to the content of their unified causal explanation—whether its economic determinism for Marx or technologies of power for Foucault. So, both in terms of the form and content of their explanations, grand theories aspire to a kind of unification that I don’t think mechanistic approaches aim to, or really can, achieve.

IAR
Furthermore, there is no particular reason why so-called grand theory cannot admit one or another kind of mechanistic explanation. It depends upon the question it is used to answer, and the overall intellectual project—that’s the pragmatist nature of the human sciences for you. I would also point out that what is sometimes referred to as grand theory—though I’m not sure we should constantly use a pejorative term if we want to really have a discussion about this—tended to have a third component beyond the two mentioned by Carly:
an ambition to explain/interpret something that was in some way a historically specific, but extraordinarily widespread, phenomenon (“transition to capitalism in Europe”). Dan Hirschman has written about this a bit I believe. I consider Greta Krippner’s work in this light—it articulates concepts and makes unifications, but it is also explains one really important thing that affects millions of people (e.g. financialization, etc.)

There is, however, a further issue here. I think it is correct to criticize some aspects of the tradition of grand theory (whatever that is) as sometimes veering towards armchair philosophy (though as Andrew Abbott always pointed out, the so-called classical theorists loved data, historical evidence, etc.). But I would draw a distinction between the pejorative armchair and the necessity of reading relevant philosophical debates (philosophy of science, political philosophy, etc.) as part of one’s attempt to develop claims in the human sciences. One of the reasons I’m confident we really got to something important in this paper is that we forced ourselves to think with both the philosophers and the sociologists, while keep our own questions and concerns clearly in mind.

**LF**

To conclude, we’re curious to hear if/how your approach to sociological research has been informed by the arguments arrived at in your paper? How do you think the metaphorical and multivalent character of “mechanism” will impact your own orientation toward sociological research?

**IAR**

In my own work, causal pluralism (and its relationship to the interpretation of cultures) was necessary from the beginning, because my first research project required a lot of thinking about an event that was both real and a frequently-used metaphor in American culture (the Salem Witch Trials, used perhaps most famously, by Arthur Miller to describe McCarthyism). I was always concerned with what the witch trials revealed about certain cultural-political dynamics. (I finally got to the heart of the matter of Salem, I think, in my forthcoming book, though there remains one more paper to be written). But this work also required an analysis and use of concept-as-metaphor: in this case “performance” as a way to think about power. In my own work, when this way forward succeeds, it produces a very satisfying return for intellectual efforts made: aspects of empirical materials that one cannot get at quite as well with other metaphors become illuminated. And that’s all you can ask for in this vocation, is it not?

**CRK**

Well, I’m a lot more thoughtful about when I use the word “mechanism”! But I also think it has made me a lot more thoughtful about the kinds of explanations that I’m working on generating. A lot of my recent work, for instance, has been in computational sociology where I’m attempting to characterize long-run changes in culture, discourse, and rhetoric. These abstract characterizations are transposable and generalizable, but are not really casual and are not mechanisms. They are more like cultural genealogies, and it has been interesting to think about what these kinds of genealogies do: what kind of unique explanations we get from this kind of historical “carving” of the social world. But perhaps that’s another paper.
“Bloodchild” (1984) Octavia Butler

The story describes the interaction between humans and an alien species on a planet they are inhabiting together. In the story, a rough situation unfolds with a human giving birth to the aliens species. I use this story to highlight a complication central to postcolonial theory—there is no possibility of going back once the colonial relationship has been established. All groups are forever changed by the interaction. This leaves the question of how to move forward based on this shared history, one rife with inequality. Find Butler’s short story here.

Submitted by: Jared Strohl, Ph.D. Candidate, University of Buffalo

Black Panther (2018)

Erika uses Black Panther to illustrate Weber’s authority types, and looking at what legitimizes each attempted leader’s power throughout the plot of Black Panther. Because of the nature of ideal-types, none of them actually “fit perfectly” into one type, but believe the ensuing class discussion is fruitful. Find her BP diagram here.

Submitted by: Erika Sanborne, Ph.D. Candidate, University of Minnesota

So-Fi Zine (2017- ) ed. Ashleigh Watson

Launched in 2017, this zine opens and creatively extends the promise and craft of sociological imagination. So Fi Zine publishes short stories, cartoons, photo essays, poetry, mini zines, sketches, and other creative works. The zine has published over 125 original creative contributions and featured invited pieces by Patricia Leavy, Howard Becker, Mark Carrigan, Les Back, Nirmal Puwar, Raewyn Connell and Michael Burawoy. Read So-Fi Zine here.

Submitted by: John-Paul Smiley, Independent Scholar
Gil Eyal’s new book is titled The Crisis of Expertise, and its core conceit is that such a crisis does exist: that distrust in experts and expert knowledge has become pervasive, and thus requires analysis. Eyal starts his project by defining expertise in a historically grounded way: not as simply some special body of knowledge or capacity for action held by individuals or groups, but as a concept emerging in conjunction with the technocratic state, and tied to the particular problem of its growing dependence on specialized knowledge, and its need to act in the face of competing claims to having the correct knowledge.

The Crisis of Expertise begins by mapping competing theories of expertise—such as artificial intelligence, the sociology of professions, or actor-network theory—along two axes: whether expertise is located inside or outside the individual, and whether it is fundamentally abstract or reflects practical, tacit knowledge. Eyal makes the case for a new approach to the study of expertise that is located in none of these quadrants.
but at their intersection. He also draws our attention to two key concepts in understanding it: trust (which he portrays as an unsteady position balanced somewhere between faith and knowledge, or default and vigilance) and risk (which he centers as fundamental to modern society, but also inherently productive of expertise problems).

It is also worth pointing out that Eyal sees the crisis of expertise as fundamentally a crisis of the regulatory state. It is the state that must navigate this difficult space in which multiple voices must be heard and taken into account, and decisive action must be taken, but never on the basis of fully conclusive scientific knowledge. Thus while Eyal does not provide prescriptive solutions to the crisis of expertise, the implication is that ending it will require developing better regulatory institutions.

Eyal then turns to the mapping of varying responses to the legitimation crisis onto another 2x2 table. In this one, one axis represents solutions relying on mechanical objectivity versus those focusing on expert judgement. The other distinguishes between technocratic and participatory decision-making. A fundamental point Eyal is interested in making is that all of these responses are prone to unintentionally further destabilize expertise, while, ironically, efforts to challenge science sometimes have the unintended consequence of reinforcing its authority.

Eyal does not try to prescribe solutions to this crisis, but closes the book by recovering a concept proposed by physicist Alvin Weinberg in the early 1970s, “trans-science” (Weinberg 1972). Trans-scientific problems, which are rife in the kinds of regulatory spaces Eyal is interested in, are not those which have moral or societal implications that complicate them. Rather, they are problems that are fundamentally amenable to scientific analysis, but that for practical reasons, science simply cannot answer. So we may suspect that Colony Collapse Disorder results from the cumulation of low-level stressors on bees, there is no practical way to determine exactly what combinations of stressors under what conditions might begin to produce it (Suryanarayanan and Kleinman 2016). Or we may know that there is a small but real chance of a catastrophic earthquake taking place in Northern California over the next five years, but we cannot specify precisely what that chance is, even though the question is still solidly in the scientific realm.

Expertise, Eyal concludes, lives in this realm of trans-science. It requires expert judgement, because these are not questions that can have precise answers. And because they require judgement, they can never fully escape the bounds of politics. The challenge they pose is one of redesigning state institutions with these questions, and this fundamental uncertainty, in mind.

Having now given some flavor of the book itself, I propose three questions provoked by it that I think are worthy of further discussion.

First, how does the crisis of expertise in the state differ from the crisis of faith in academic science, on the one hand, or in broader societal knowledge claims, on the other?

For Eyal, expertise is something that takes place at
the intersection of science and action, and is fundamentally a problem of the state. Yet the crisis of legitimacy of knowledge-producing institutions of course goes well beyond the state. In academia, we have the replication crisis in fields ranging from cancer research to experimental psychology. This involves experts, and attention by the public, but is not very directly tied to the state, or even, in the case of psychology, to applied knowledge.

On the other hand, we have the crisis of fake news in journalism, and more generally the fracturing of the media ecosystem so that different people no longer agree on basic empirical facts. Again, these developments only indirectly related to the state, and have little to do with specialized, abstract knowledge.

Yet both the replication crisis in science and the fake news crisis in media seem to share underlying characteristics with the crisis of expertise in the state, in which the means for distinguishing true claims from false have been delegitimated. In what ways is the crisis of expertise distinctive, and in what ways is it merely a special case of this larger breakdown in the legitimacy of knowledge-producing institutions?

Second, can we really bracket the legitimacy of regulatory science from the legitimacy of government institutions more generally?

In the middle of the book, Eyal spends some time trying to pick his way through a middle path between Habermas, who sees legitimacy as resting on a belief that with enough time for reasoned debate sufficient evidence could be produced to justify a decision, and Luhmann, who sees legitimacy as fundamentally deceptive, as it is never in fact possible to produce a fully reasoned defense of action. Eyal tries to thread this needle by suggesting that one critical piece of creating legitimate expertise involves ensuring the right “temporal frame” for decision-making—one that is slow enough to allow for input and deliberation and careful consideration of evidence, but faster than the temporal frame of pure science, since regulatory science needs to come at least to temporary conclusions so that action can be taken.

There’s little question that successful regulatory science involves careful balancing between the need for input from various nonscientific constituencies as well as (possibly competing) groups of scientific experts, in a timeframe that allows enough input while eventually coming to a close. But this framing of the problem only seems to me to make sense if we assume that challenges to regulatory expertise are, at some level, fundamentally about whether scientific claims are reasonable. But what if the issue is not one of reason, and the legitimacy of regulatory science, but of the legitimacy of government more generally?

Compare, for example, the fights over regulatory decisions at the EPA in the 1980s (e.g. Lash et al. 1984) to those that occur now. In both cases, scientists argue over interpretations of evidence, while interest groups lobby for interpretations that favor them, casting doubt on those that do not. But today, unlike the 1980s, such challengers can channel the energy of a political ecosystem in which a large fraction of the population has come to see the EPA, and government agencies more generally, as the villain. No appeal to either scientific reason or proceduralism is going to assuage those whose fundamental issue with the process is based on identity, not reason.
Finally, *cui bono? Where are power and interests in this story?*

It is striking that a book centrally concerned with the question of how expertise is established and legitimated pays little attention to questions of interests and power as a means of either supporting or challenging expertise. Of course Eyal mentions “merchants of doubt” (Oreskes and Conway 2010), agnotology (Proctor and Schiebinger 2008), and organized efforts to sow distrust in science. But it seems unrealistic to me to propose a starting point, at least, for thinking about structuring institutions that can create reliable regulatory science without thinking through all the reasons groups might not want them to.

And power and interests are not only a factor in challenges to expertise. They are also critical in upholding it. A working paper by political scientist Philip Rocco (2019) comes to mind, which demonstrates how interests and power help to uphold the Congressional Budget Office as a producer of neutral numbers. While past accounts of the CBO have emphasized its careful work to visibly cultivate neutrality and nonpartisanship, Rocco convincingly argues that at least as important is the fact that the powerful Budget Committees of Congress have come to rely on the CBO’s numbers as a buttress for their own influence. Because powerful actors benefit, the CBO survived the 1990s, a decade that saw the elimination of a peer office, the Office of Technology Assessment, and the CBO has persisted despite more recent challenges: its ability to be neutral rests on raw power.

*The Crisis of Expertise* is a very stimulating book to engage with for those of us interested in the uses and limits of expertise, and it is difficult to limit myself to three questions in response. I’ve already had one long conversation trying to think through its implications, and will doubtless have many more; we are all richer for having to it to react to, and reflect upon.

**References**


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**The Political Field in the ‘Middle Lane’**

Stephanie Mudge  
*University of California, Davis*

This engaging and thought-provoking book begins with the primary dilemma of the contemporary crisis of expertise: experts have never been more ubiquitous and necessary, and expertise has never been more politicized. Seeking to move beyond unhelpful antinomies (facts versus fakeness; expertise versus ignorance), Eyal narrows the focus from “science” in general (which he notes, following Latour, has no singular existence) to regulatory science specifically. Regulatory science, by contrast with the action-oriented fast lane of legal and policy sciences and the slow lane of the ever-
revisable hard sciences, resides in the contentious “middle lane,” doing the work of bridging “open-forward scientific facts with closed, actionable legal and policy facts.” A site of the production of “cutoffs, thresholds, guidelines, surrogate end points, acceptable risk levels, consensus documents, expert assessments, simulations, stress tests,” the middle lane is especially contentious and crisis-prone because of its location at the “interface” of science and policy. In the middle lane there can be no “long-termism” defense against accusations of bias or arbitrariness; “one cannot wait for the long-term,” where social mechanisms of judgment, interpretation, and interpersonal trust “stand exposed in the glaring light of a decision taken in the here and now” (8). This, Eyal argues, is where the real contention lies.

It is easy to forget nowadays that today’s crisis of expertise was long in the making. As Eyal reminds us, the 1960s saw both the rise and the politicization of the category of the expert. As it turns out, there is no final way of knowing the difference between experts and non-experts, or evaluating the legitimacy of expert claims. Expertise is not the same thing as skill; it can be both specialized and practical; it is both tacit and linked to evaluations of abilities to explicitly formulate it; it is not always clear what forms of expertise are most relevant or important.

In a particularly deft move Eyal lays out how varying conceptions of expertise can be thought of in fielded terms—that is, one can map out scholarly contention over the nature of expertise as a field of struggles over who should be understood as an expert. The space of contention, in his analysis, is organized by two axes: the problem of extension (technocrats or citizens?) and the problem of trust (rules or judgment?). In the latter case Eyal reminds us that trust, like gift exchange, has a “two-fold truth” (quoting Bourdieu): it has to be grounded in both personal faith and in “organized, collective self-deception”—that is, a willingness to sustain the relationship in the absence of guarantees.

What polluted the trust relationship that faith in expertise requires? This is the million-dollar question. The answer Eyal settles on, drawing from Ulrich Beck, is that the rise of risk, the impossibility of any final expert or expertise of risk, the inevitable failure of efforts to assess and manage risk (risk can’t be eliminated!), and the use of quantified risk to camouflage otherwise ethical and political concerns, undermined “the reputation and credibility of scientific institutions, experts, expert systems, and government agencies” (62). The crisis, then, is not “knowledge and ignorance,” but rather “authority, legitimacy, credibility, and reputation” (84).

Here the book takes a surprisingly optimistic turn: as the crisis deepens, the world of expertise builds a capacity to defend and, perhaps more importantly, to limit itself, becoming able to identify the questions it cannot answer—what Eyal calls “trans-science as a vocation.”

This is a really thought-provoking assessment. It is surprisingly optimistic, which is refreshing. But, then again … perhaps because I’m not in a very optimistic frame of mind just now, or maybe because we’re never really able to read things without putting them in the context of one’s own work, it raises questions about where politics and political institutions are situated in the making and (hopefully) resolution of the crisis of expertise.

Shouldn’t our default assumption be that the politicization of “scientific” and “fact-based” truth-claiming in the regulatory sciences is rooted at least partly, or maybe primarily, in politics? And if so then doesn’t this suggest that, if we are to grasp the crisis of expertise historically, then (as I’ve argued elsewhere) we have to link the sociology of expertise to the sociology of states and parties? What I’m especially interested in here is not so much the place of the legitimacy of government or democracy in the story of the crisis of expertise, but rather the place of the integrity of political representation and functioning of
the political field. Indeed, one could argue that the “middle lane” that Eyal describes is the political field.

To think this through, let’s reconsider the matter of trust and the axis of contention over rules or judgment in the case of economics in the 1960s. Was economics’ politicization generated by the problem of risk? Well, maybe, but the stage for this was set in the so-called Keynesian period in which, as I show in my recent book, an economics that was very much centered on judgment (as opposed to rule-based decision-making) was also de facto allied with American New Deal Democrats and, in Western Europe, with socialist, laborite, and social-democratic parties. The initial effect, on the political right and the (linked) non-Keynesian corners of economics professions, was emphasis on governing according to rules given by markets; the longer-term effect was that, in time, orthodoxies within economics shifted to rules—a way of thinking that does not dovetail too well with the necessities of political strategy and coalition-building.

And so, by the 1990s, in the political field we find the rise of strategic political expertise or, in other words, specialized expertise in the art of winning elections. The flipside of contention over trust, it would seem, was a tectonic shift in representative politics away from representation and toward the science of strategy.

The problem, in this case, is that the dynamics and effects of the crisis of expertise cannot be understood without an analysis of the relationship between expertise and partisan politics. In a way this isn’t too surprising. It was, after all, Mannheim who identified the rise of parties with the production of “ideology.” Mannheim saw the particularization of knowledge claims, and recognition thereof, as having origins in an emergent association between knowledge-claiming and partisan organization. Bourdieu updated this story for the contemporary period: for him the problem was not so much the rise of parties and ideology but rather a re-centering of institutions that are supposed to be communicative and representative on winning at all costs. The key thing here is the reduction, in other words, of representative politics to theater and the alienation that is its natural correlate. In such a world one wonders how expertise of any form can be established or legitimated, since the space of “interface” between science and law or policy—which, in democratic orders, is populated by political parties—operates on a logic in which all truth claims are subordinated to strategic office-seeking.

The upshot is that there is a kind of “pollution” or “contamination” story that one could tell that is not internal to fields of expertise or contention over it. It originates in the political field, and from there it encroaches on both expert professions and the technocratic state. What emerges, then, is a world in which partisanship and political strategy dominate, and are perceived as being dominant over, truth, government, and representational claims-making; a world in which everything—the weather, everyday language, styles of dress—takes on a partisan hue. One wonders, in this world, whether any kind of non-partisan ‘expert’ is historically possible.

Last thing—and this is going to be one of those questions that I really should elaborate, but instead I’ll just put it out there and leave it at that. It seems to me there is a political economy to the story Eyal tells, as well: expertise can be contaminated by partisan power-seeking, or it can be contaminated by the dominance of profit-seeking. This comes up in the book here and there. Is there not also a story of the rise of financialized capitalism in the crisis of expertise? Of states that increasingly, in the terms of Marion Fourcade and Kieran Healy, “see like a market”?

These are my central questions for Gil—which, of course, I would be unable to formulate had he not written a really excellent book!
Recently, the *New York Times* reported on a new draft of a proposal issued by the Environmental Protection Agency, called “Strengthening Transparency in Regulatory Science.” If adopted, the measure “would require that scientists disclose all of their raw data, including confidential medical records, before the agency could consider an academic study’s conclusions.” In other words, given that confidential medical records cannot be disclosed, the intended purpose of the measure is to inhibit the use of data drawing on health records to inform policymaking to protect clean air and water. Yet this effort to weaken the evidentiary base of regulatory policy was promoted by the current leadership of the EPA in the language of scientific virtue. “Good science is science that can be replicated and independently validated,” Andrew Wheeler, the head of the agency, told Congress: “That is why we’re moving forward to ensure that the science supporting agency decisions is transparent and available for evaluation by the public and stakeholders.”[1]

Perhaps few will be shocked by such duplicitous and cynical rhetoric from the current director of the EPA. But readers of Gil Eyal’s wonderful new book will immediately notice how astutely Wheeler has appropriated the crisis of expertise. When and why should we trust the expert findings on which the EPA relies to generate its regulations? On the one hand, by invoking replication and an emphasis on procedures of validation, Wheeler appeals to our faith in “mechanical objectivity” as a warrant of the credibility of expert claims: Of course we want studies to be reproducible! On the other hand, by gesturing at “the public and stakeholders,” Wheeler tips his hat in the direction of the democratic revolt against expert technocracy, and acknowledges the widespread dissatisfaction with simply leaving important judgments up to the experts: How can stakeholders have their say, unless all data are made available? Close readers of *The Crisis of Expertise* might well conclude that every position in the agonistic field that characterizes the contemporary debate over expertise has nowadays become grist for the mill of cynical obfuscation. “Crisis of expertise” indeed.

But if the episode demonstrates the indisputable timeliness of Eyal’s intervention and the handy analytical tools that Eyal offers us, it also suggests some of the weird and wild complexity of the current debates surrounding expertise. What is expertise, anyway? Who gets to be called an expert? When should experts be trusted? How are experts best held accountable? How are expert findings best incorporated into political processes? And, yes, how might we respond when the very defenses of expertise are appropriated and manipulated? I’m tempted to say that the most controversial claim in Eyal’s book is the one implied by the first word of the title: “The.” Really? Just one crisis? Perhaps there’s too much at stake here to be satisfactorily encompassed by a singular definite article.

That said, the book is hugely helpful in corralling the topic and organizing a wide array of theoretical resources and empirical studies of expertise. What Eyal means by “the crisis” is clear enough: our society is pulled in two directions at once by our profound suspicion of an enterprise that we do not even pretend to be able to do without. Eyal does a marvelous job
of decomposing this crisis, taking up the various proposed solutions in turn, and locating them all in social space in relation to one another. This analysis alone would justify the book, but Eyal gives us more, including suggestions about the historical rise of the crisis, and careful consideration of the entwined fates of concepts related to expertise, such as trust and risk. He does all this in lively prose that not only is powered by great examples but also is much more fun to read than the idea of “crisis” would seem to authorize. In other words, this is a splendid book that, in 149 pages, accomplishes a great deal.

I would like to say a word about where the book does not really tread, before turning to the vexed question of “what is to be done” that Eyal takes up in its final pages. I think that because of his close focus on the politics of regulatory science, Eyal misses the chance to paint the crisis (or crises) of expertise as broadly cultural phenomena. Regulatory politics concerns weighty political matters of immediate practical importance, from the safety of the medications we consume to the cleanliness of the air we breathe. But it does not exhaust the ways in which crises of expertise infuse everyday life.

In a book that I’m completing, which examines the rise and spread of new ideas about something called sexual health, I take up at one point the question of who serves, nowadays, as the experts on how we should manage our sexuality. Is it a doctor or a researcher? A dating and relationship coach? A BDSM practitioner/educator? The young woman whose “sex ed” videos on YouTube, posted from her dorm room, received more than 100 million views? Or the porn star who produced a series of educational DVDs, with demonstrations by live performers, that reflect, according to her website, her “passion for education”? One interesting article posted this year on “Bustle,” written by a “certified sex educator and writer,” is entitled “How To Tell If A Sexual Health Resource Is Legit.”[2] The author’s suggestions are not unhelpful, but in the end it’s an impossible question to answer. Yet people care enormously about the kinds of issues these experts take up—perhaps more so, for better or worse, than they care about the debates of regulatory rule-making. My point is that the crises of expertise even more thoroughly saturate the fabric of our culture than the “statist” bias of this book would suggest.

But let me take the book on its own terms and say a few words about how Eyal wraps things up. The conclusion to the book is short, and Eyal begins it with a clear disclaimer: “I did not write this book to offer a solution to the crisis of expertise. I do not have one” (p. 142). Fair enough: Personally, I have no problem with a book that is diagnostic rather than prescriptive. Eyal’s emphasis, in these final pages, is appropriately on the virtues of critical scholarship in driving home the inconvenient truths that make the expertise crisis so very difficult to resolve. Indeed, Eyal uses these pages in large part to summarize why every known position in the expertise wars founders on the shoals of one inconveniently placed rock formation or another. Those who put their faith in a Weberian separation of facts and values confront the problem that, in the domain of expertise, the two are necessarily blurred. Yet those who reject the Weberian...
distinction and who seek instead a more democratic and inclusive mode of participatory science should acknowledge that such forums—precisely because they throw open the closed doors of science—lack mechanisms to end debate and are "poor machineries for producing legitimacy" (p. 148). Indeed, they are easily exploited by self-interested "merchants of doubt."[3]

Well, frankly, the potential for exploitation is everywhere in this story, as suggested by my opening anecdote concerning the top administrator of the EPA. So, how do we then respond? I agree with Eyal that the answer cannot be to suggest that the orchestration of expertise should "be modeled on the open agora" (p. 148). What should it be like, then? Eyal briefly allows us to glimpse his cards (and perhaps lays them right on the table) when he proposes on the final page that the field of decisionmaking should be "composed of the relations of trust and mutual support between the repeat players, though every effort should be made to expand their ranks." Moreover, "at the core of this network there should be...a dedicated and autonomous state agency," staffed by "a revalued, rededicated, professionalized and emboldened civil service" (p. 149).

This is an interesting choice of gauntlet to throw down, and it has a resonance at the present moment that Eyal could not fully have foreseen when he wrote those words. Certainly some of those who have portrayed the recent drama of the impeachment hearings as the "revenge of the nerds" have proposed that the expertise and dedication of State Department officials is all that stands in the way of the barbarian onslaught—that is, the ignorant, crass, and dangerous opportunism of Trump, Giuliani, et al. And yet, I think one might rightly applaud the virtues of having on the ground in Ukraine experts who actually know a thing or two about Ukraine, while nonetheless observing that insular expert judgment in places like the State Department has sometimes, over the years, led to disastrous decisions that have caused much injustice and suffering around the world. I do not mean to associate Eyal with a defense of such decisions. Yet, beyond saying that he would expand the ranks of the "repeat players," he gives us few clues to imagine how "a revalued, rededicated, professionalized and emboldened civil service" could be structured, or circumscribed, so as to rescue us from that fate.

If we have to choose our devils, I’ll stick with a more participatory politics of expertise, myself—though I agree with Eyal that the trick is to imagine the kinds of institutional supports that might keep it honest. I continue to find lessons, however ambiguous, in the work of the AIDS treatment activists that I studied some decades ago.[4] AIDS activists were deeply invested in the scientific process at the same time as they mounted their critiques of specific practices and practitioners. For obvious reasons committed to the goal of advancing knowledge about AIDS and producing effective treatments, treatment activists could not afford the luxury of endorsing scientific arguments just because they found those arguments politically useful. To the contrary, practical necessities generally (though of course not always) worked to ensure that activist interventions with regard to clinical trials would be based on scrupulous self-education, careful assessments, and sincere concerns with validity, reliability, and efficacy—not on an opportunistic endorsement of whatever seemed consistent with predetermined political stances. To be sure, this example suggests that only in particular circumstances will "lay expertise" help bring about better science. Yet, perhaps it’s possible to extract, from this example and others, some ideas about institutional mechanisms that could promote virtuous examples of participatory
forms of expert decisionmaking. I’ll continue to place my hopes there. If we have to choose our devils, I’ll stick with a more participatory politics of expertise, myself—though I agree with Eyal that the trick is to imagine the kinds of institutional supports that might keep it honest. I continue to find lessons, however ambiguous, in the work of the AIDS treatment activists that I studied some decades ago. AIDS activists were deeply invested in the scientific process at the same time as they mounted their critiques of specific practices and practitioners. For obvious reasons committed to the goal of advancing knowledge about AIDS and producing effective treatments, treatment activists could not afford the luxury of endorsing scientific arguments just because they found those arguments politically useful. To the contrary, practical necessities generally (though of course not always) worked to ensure that activist interventions with regard to clinical trials would be based on scrupulous self-education, careful assessments, and sincere concerns with validity, reliability, and efficacy—not on an opportunistic endorsement of whatever seemed consistent with predetermined political stances. To be sure, this example suggests that only in particular circumstances will “lay expertise” help bring about better science. Yet, perhaps it’s possible to extract, from this example and others, some ideas about institutional mechanisms that could promote virtuous examples of participatory forms of expert decisionmaking. I’ll continue to place my hopes there.

interdisciplinary discussion has emerged on the power of expertise to shape social life. The program this year includes studies of development projects, consultancies, algorithmic governance, economic expertise, legal expertise, environmental expertise, among many others. Across disciplines there are today analyses of algorithms, evaluations, economic paradigms, audits, financial instruments, blueprints, censuses, and policy instruments, much of it predicated on what Timothy Mitchell described in 2002 as the “rule of experts.”

But, what if experts don’t quite rule? And what if expertise itself is in a kind of crisis of legitimacy, despite the fact that everyday life depends, so much, on expertise? In this technified and risk-managed world, what are we to make of the so-called Merchants of Doubt, not to mention Vaxxers or Fake News. This is our predicament, and this is what the book helps us grapple with as social scientists, not so much giving us one answer or a way out, but a set of orientations of how to approach the problem and the stakes of doing so in one way or another. The book, in addition to sorting debates around expertise, offers us a framework to think about its crisis. As Gil writes, efforts to improve expertise (“attempts to organize, pluralize, mechanize, or outsource expertise”), wind up in a “self-reinforcing vortex of mutual pollution and mutual undermining.”

And as compelling as the descriptions of these dynamics are, I could not help but wonder if these are not features of something broader. There is a fundamental STS insight that there is a relationship between scientific re-presentation and democratic re-presentation as profoundly political acts, which present themselves as a-political in different ways. What might thinking of them together look like? And while the book, particularly in the earlier parts, connects these dynamics to a crisis of legitimacy of the state, the foregrounding of expertise eventually blurs the background of democracy.

If we follow Rawls for a second, and we think of democracy (the rule of the people) in terms of the exercise of public reason, then we can think of expertise in a democracy as the exercise of specialized reason in service of public reason. The crisis of legitimacy of expertise has to be, I think, understood, in terms of a broader crisis of democracy. If we trusted our democracies and then we had a crisis of expertise, it might be one thing, but democracies have been in a crisis of legitimacy for some time.

The arguments about this crisis are well-known.[1] Three decades into neoliberalism, liberal democracy and political parties working within its framework have reached a limit in terms of their ability to represent large swaths of the world’s majority. Take Europe or North America in the last three decades and the conditions of the majority of the population of the continent: increased inequality, insecurity, lessened social mobility, and existential threats like climate change. In response, increasingly rigid social democratic and labor parties have tilted right in an attempt to capture an electoral “center” only to have their social base taken from them in many countries, where right-wing movements have been better able to give expression, however distortedly, to discontentment and existential fears. In response to the Right’s organizing and full-throated political talk...
of the “people,” (however narrow) these parties have responded with arid and pro-market policies, in an odd way becoming defenders of an establishment that has not worked for so many.

To recast this in terms of expertise: some of the dynamics Gil describes have neat parallels here. The logic of inclusion and pluralizing, which were seen as ways to democratize democracy, are now used against it (when chauvinistic politicians and movements decry the excesses of multiculturalism in Europe, say), or used perversely (when right-wing movements use the language of multiculturalism and inclusion to demand white identity politics and white representation). One of the undertones of The Crisis of Expertise, that tools and logics that belonged to progressives have now switched sides, has clear resonance when we think of democracy. And I think one of the virtues of participatory democracy and participatory science the book underplays is the potential for rescuing common projects.

It’s hard to know the boundaries of the crisis of expertise. For sure, the book offers, in addition to an extremely useful guide to debates about expertise, a compelling account of the dynamics of its undoing. But it invites questions, for me anyway, about the institutions around expertise, particularly democracy and the mechanisms linking popular will to popular rule. For me the solutions to the crisis necessarily have to do with these. These, of course, are empirical and theoretical questions that we need to work on. Thank you for getting us started.

[1] I rehearse these in my book, We The Sovereign (2018). Sorry for the shameless plug!

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Reply to my Critics
Gil Eyal
Columbia University

There is a certain emotional ambivalence that a panel of this sort holds for me, the author of the book. What’s the point of “author-meets-critics” panels? Why do we hold them? There is, obviously, a substantive interest. The topic is important, we’d like to debate it and we will. Undeniably, there is also a promotional (and self-promotional) dimension. And of course, it is also a ritual, designed to “honor” the author. Why, then, the ambivalence?

Well, what kind of ritual is it? This is a “rite of passage” for the book. It now leaves my own small circle, where it was protected, and becomes a public personage. The author-meets-critics panel is a ritual by which the message is imparted to me, politely but firmly, that I no longer have sole jurisdiction; that the book is no longer a child, a minor whose parents (namely me) have the right to say the last word about them. It is a young adult, self-reliant and entitled to live an independent life, to socialize with whoever they want, to say whatever they want, perhaps even to contradict me.

Hence, this is a bitter-sweet moment, a moment of farewell tinged with anxiety – what will happen to the book out there, in the big, wide world? Will it disappear? Will it associate with the wrong sort? Will it come back,
like the prodigal son, and ask for money?

I take comfort, however, in knowing that I entrust the book in good hands. This rite of passage has the added meaning of appointing godparents and guardians, who hereby promise to check on the youngster and make sure it doesn’t stray beyond the bounds of reason and good taste. Panelists – I’d like to thank you in advance for keeping an eye and sometimes lending a hand to guide the book and prevent it from making easily foreseen mistakes.

I would like to thank Steve Epstein – for whom this is the second time around, as he already played this role for my previous book, *The Autism Matrix*. Certain passages in the book are better read as picking up a conversation, a friendly debate with Steve, from where we left off last time. For Beth Popp Berman, as well, this is an ongoing conversation, began when she was a PhD student at Berkeley and I was junior faculty. Already then, she was the one to ask the hard questions and never pull her punches. I’d like to thank Stephanie Mudge and Gianpaolo Baiocchi as well for their incisive yet generous comments.

Given the short time, I cannot deal with all the issues raised by the panelists. I will limit my comments to two issues, that seem to be raised by all or most panelists. Very briefly, they all seem to be asking, albeit in different ways: *Is that it? Is that all there is to it?* And then they also ask: *ok, then what shall we do about it?*

To begin with the first question, Steve Epstein asks: Is there one crisis of expertise or many? By focusing on regulatory science, am I not missing the big picture of the broader cultural phenomena that constitute these multiple crises of expertise? Beth Popp Berman asks: isn’t the crisis of expertise merely a special case of a much more general breakdown in the legitimacy of knowledge-producing practices, among which she includes also academic science and the media? Stephanie Mudge asks, can we really say that the locus of the ‘crisis of expertise’ is internal to expertise and its forms? Don’t we have to locate the crisis in the broader context of politics, partisan institutions, states, and their relations to experts and expertise? Can we explain a crisis of truth-claiming without an analysis of politics and democratic representation? This question of Stephanie intersects with another question of Beth’s: *qui bono?* What about power and interests? Isn’t it all about partisanship? After all, even the “neutrality” of regulatory agencies or Congressional oversight bodies is itself only possible on the basis of power relations.

These are not exactly the same questions, but they do circle around the same apparent flaw in the book, which is the narrow focus on the interface between science and the state. I should say in my defense that I did not start out intending to write a book so focused on regulatory science. The problem imposed itself on me, so to speak, with every news cycle.

I began with an examination of the history of the word “expertise” itself, a historical pragmatics of how and when the word was used. I tried to show that we only began to use it in contexts when it was no longer clear who the experts were; in contexts when claims were made on behalf of entities radically different than the prototypical expert of the time (such as government agencies, computers or laypersons); in contexts where it was no longer clear how to decide between competing claims to expertise; in short, we began speaking in terms of “expertise” not as a function of the growth of expert society, but of its crisis, when there was “increasing instability and doubt regarding the established professions, regulatory science and other authorities.”

The interface between science and the state was only one such context. In the book, I may perhaps focus on it overmuch, but I mention also the context of scientific expert testimony, just as often in private litigation, as well as the rise of expert systems, algorithms, lay expertise, etc.
So, my answer is yes, the book’s focus on regulatory science, especially in the last few chapters, is too narrow and should be broadened; but also no, I don’t think there is a more general breakdown of the legitimacy of knowledge-producing practices. I see no evidence, for example, that basic science, i.e. the sort of science that pursues knowledge without any link to legal, regulatory or policy decisions, is in crisis. I do not think the reproducibility project represents such a crisis, unless it is linked to research about the efficacy of medications or the advisability of certain nutritional guidelines, or what have you. The surveys about trust in science, whatever their problems, certainly do not bear out this diagnosis.

I am similarly skeptical of the claims that we are now in a “post-truth” era. This would imply that there was a time, back in the past, when we lived “in truth” and we all agreed on “basic empirical facts.” There was never such a time, especially not in this country built by millenarian sects. “Fake news” too are not new. What’s new is that this Russian export product is now marketed here as well, faster than before, and in greater bulk. And yes, there are more customers for this merchandise because, as you say, the media ecosystem is fractured, but this fracturing has begun much earlier and is related to the intensification of jurisdictional struggles. Ten years ago, before anyone mentioned “post-truth,” media scholars were writing about the pluralization of forms of journalistic expertise, and the competition that a new breed of bloggers and news sites posed to traditional journalists.

Regarding the role of political partisanship, raised by both Stephanie and Beth, I think it is part of the story. I think, in fact, this is a story that the book does tell, and in a way that helps clarify the predicament. It is partly why I was dragged into writing something that was narrowly focused on regulatory science, because this is what has become so partisan. We should distinguish two different meanings of partisanship. First, as the organizing principle of the political sphere. Stephanie goes back to Mannheim and Bourdieu. I would add Carl Schmitt. The distinction between friend and enemy is the basic organizing principle of the political. Partisanship was always there. The current meaning of partisanship in the US identifies a specific version of this, when the lines of enmity are increasingly aligned rather than cut through parties and constituencies, as they might have done in the past (Northern and Southern Democrats did not see eye to eye). So “polarization” is a better term. Nonetheless, why should regulatory policy of all things become partisan and polarizing? Yes, there is a political economy to it. That’s part of my story. Each regulatory decision picks winners and losers. It has redistributive effects. So it is disputed. Science is mobilized to defend it, and then science itself, but specifically regulatory science, becomes infected as well. This is a recurrent crisis. The 1994 Republicans were no less dismissive of regulatory science than today’s Republicans. And if you look for similar vehement attacks on regulatory science a little bit further back, you are more likely to find them on the left. As I say in the chapter on “risk,” it seems as if the tables have turned. The longhaired environmentalist critics of yesteryear are now the defenders of regulatory science, and the former conservative defenders of the status-quo now attack it. In reality, I think, it is the same dilemma playing itself over and over again, albeit with different protagonists.

“I am similarly skeptical of the claims that we are now in a ‘post-truth’ era. This would imply that there was a time, back in the past, when we lived ‘in truth’ and we all agreed on ‘basic empirical facts’”
But yes, the book’s focus should be broadened, as Steve says. I would argue that the underlying framework is broader than the substantive topic or the definite article in the book’s title would make it seem. While I speak of “the crisis” in the singular, I also speak about “engines of crisis” in the plural. I mention several. I begin indeed with “the ever-closer entanglement and blurring of boundaries represented by regulatory and policy science,” but then I speak also of other engines, namely “the intensifying of jurisdictional struggles, exacerbating the uncertainty as to who are the relevant experts for the problems at hand,” as well as “the constant dynamic of “overflowing” by which technical or organizational solutions to existing problems generate a new set of unforeseen problems and create new publics composed of stakeholders and lay experts.” While I push back against the all-too-easy story that it is all “because internet” (to borrow Gretchen McCulloch’s clever title), I also include the rise of the internet in the analysis (albeit all too briefly) as “the great multiplier” and “great accelerator”.

The underlying framework, therefore, is of multiple currents that flow into and amplify one another. I think that the cultural phenomena of which Steve is speaking fit within this framework. I would add a fourth engine, which was always there for me in the background, but for which I could not find analytical use in the context of this book, unfortunately limited by its scope and substantive focus. This fourth engine would be the resistances to pastoral power. Foucault speaks of the 1960s as a time of great upheaval, akin to the Protestant Reformation, in the sense that one saw multiple struggles, multiple movements, all challenging the authority of pasters of various kinds – doctors, psychiatrists, psychoanalysts, sexologists, criminologists, judges and wardens, social workers, the list can be extended. Just as the Catholic priest dispenses grace and salvation to each individual member of the flock, on condition that they accept the priest’s authority as an examiner of souls, so pastoral power demands voluntary obedience in the name of taking care of the flock, each and all.

What the various resistances to pastoral power had in common is that they rejected this power that promised to take care of them, on condition that they make themselves legible to it. Instead, they valorized practices of the “care of self”. One finds this articulated forcefully in one of the Ur-texts of the time, Theodore Roszak’s *The Making of a Counter-Culture*, especially in the chapter about “Technocracy’s Children”. Technocracy rules by raising the standard of living, making life more comfortable, taking care of you. But this is a trap. The younger generation rejects the authority of technocratic experts in the name of “care of self.” As Fred Turner argued, a straight line leads from the counter-culture to the cyber-culture. The internet did not invent lay experts or self-advocates or other self-appointed pastors. The ideals of self-care, self-nomination, speaking for oneself, being the expert on oneself or on one’s children; the perception of pastoral authority as polluting because of the dependency and co-dependency it creates; these long predated the internet. But it definitely multiplied them a thousandth-fold, pluralized their provenance, and even more importantly, speeded up immeasurably the ability to solicit pastoral advice. This is another engine of crisis, intertwined with all the others. Expertise, legitimacy, trust, are simply a good way to talk about what these different processes have in common, and how they intertwine and reinforce one another.

The contemporary struggles over vaccination illustrate what I mean by the intertwining of these engines of crisis. The problematization of vaccination, the doubts about it – which do not necessarily rise to the level of being “anti-vaxxers,” just as often it is “vaccine hesitancy” – arise from all the four engines I discussed, and are certainly amplified by their intertwining. First of all, vaccination is mandated in various ways, especially as a condition for attending schools. This mandate relies on the regulatory science of the
Advisory Committee on Immunization Practices (ACIP) of the CDC, which makes recommendations about what immunizations to include, how many, in what schedule, etc. Vaccination, ultimately, is egalitarian redistributive politics, so we should not be surprised that the regulatory science involved also becomes infected and politicized. We all pay “premiums,” so to speak, to create pooled resources (“herd immunity”), and we all assume a certain risk of injury thereby. Hence the national childhood vaccine injury act which sets up a special legal process to determine no-fault liability and compensation. These can appear as forms of “state capture,” rigged in favor of the manufacturers. The language of risk attempts to depoliticize the whole process, but ultimately it is all about trust. “Overflowing” is also relevant. Not just the matter of possible injury, but the idea of “body burden” created by loading too many immunizations together, as well as the question of preservatives, etc. Since nobody knew what are the effects of ethyl mercury, the response of the advisory committee was clumsy and protracted. You might find this surprising, but there are jurisdictional struggles here as well. Not only is vaccine hesitancy supported by alternative medicine practitioners of various kinds, naturopaths, homeopaths, etc., but if you examine the membership of anti-vaccination organizations, as Catherine Tan did, you will find there quite a few family doctors and nurse practitioners. They are protecting their jurisdiction over advice to the patient as against the mandates devised by public health experts. Finally, the ideals of self-care, of being the expert on one’s children; the perception of pastoral authority as polluting because of the dependency it creates; these animate the resistance to vaccination. As Jennifer Reich shows, it is suffused with the ethos of individual responsibility for one’s health and one’s children health, and shot through with suspicion of compulsory vaccination as blind dependency on the technocracy. That said, we would not be where we are today if only one of these engines was operative. It is their intertwining that amplifies one another and translates the anxieties and fears that have always attended vaccination, since Edward Jenner, into the sort of crisis of legitimacy we face today.

Finally, I’d like to respond briefly to the question raised by Steve and Gianpaolo regarding what to do about the current crisis, and regarding my - as Steve puts it delicately – “interesting choice of gauntlet to throw down.” Steve says that he would rather “stick with the participatory politics of expertise,” though he recognizes that “the trick is to imagine the kind of institutional supports that might keep it honest.” He makes a convincing case that lay participation in regulatory science is not just ethically desirable, nor even just for the purpose of legitimacy; it can make for better science. I agree. The book makes a somewhat different point, but to the same effect. It says that from the point of view of legitimacy, inclusion and participation are a one-way road. “Once inclusion has been institutionalized, it represents an irreversible barrier and it becomes impossible to revert to full-blown exclusion strategies.”

But I’d like to double down on my endorsement of Justice Breyer’s call for a new administrative agency overseeing the regulation of risk. Just as participation makes for better regulatory science, I’d like to suggest that a dedicated civil service agency would make for better activism and better lay expertise. It would provide them with a solid target, not a slippery opponent that morphs before you can land a punch. The two need one another. ACT-UP activists spilling fake blood on the steps of the FDA are like prophets, carrying their message to the temple of regulatory science. Religion is renewed and rationalized, we learned from Weber, neither by prophets nor by priests alone, but from their interplay.
CALL FOR PAPERS
INTERNATIONAL MINI-CONFERENCE ON CIVIL SPHERE THEORY
THURSDAY, JULY 2, 2020
Brno, Czech Republic

ABSTRACT DEADLINE
February 1, 2020

Jeffrey Alexander and ISA Research Committee on Sociological Theory (RC16) mid-term conference organizer Csaba Szaló and co-President Brad West, along with other international scholars interested in global issues and the civil sphere, are organizing a mini-conference on civil sphere theory (CST). Theorists and empirical sociologists can discuss, criticize, and inspire one another, interacting around a common set of intellectual symbols. The aim is to further develop, and revise, CST, continuing the discussions among nearly 100 sociologists from around the world that have produced “civil sphere” volumes on Latin America, East Asia, the Nordic countries, and radicalism, with volumes on India, Canada, and cultural trauma in process.

The one-day conference will take place on Thursday, July 2, 2020, in Brno, Czech Republic, ahead of RC 16’s mid-term meetings (July 2-4, 2020), which will kick off that same evening with a joint reception.

Though this mini-conference is being organized around an ISA meeting, all intellectuals, theorists, sociologists and scholars interested in matters related to civil sphere theory are invited to participate; ISA membership or affiliation is not required, unless you wish to participate in the RC 16 mid-terms after (highly encouraged!). Brno local organizers will organize very reasonable housing and meals; covering such expenses, however, will be your own responsibility. (There may be a possibility of defraying some travel costs for young and emerging scholars who will be presenting papers.)

If you are interested in presenting a paper, please send a paper title and abstract (200 words) to Jeffrey Alexander (Jeffrey.alexander@yale.edu) and Anne Marie Champagne (anne.champagne@yale.edu) by February 1, 2020. If you would like to attend without presenting a paper, please send us a letter indicating your interest. Note: This is an indication of interest only. We will be asking for a firm commitment by April 1, 2020.
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