

Trajectories

Newsletter of the ASA
Comparative and Historical Sociology Section
Vol 30 No 2-3 · Winter/Spring 2019



Chair's Introduction

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The *Trajectories* team has once again done an excellent job: the Winter/Spring 2019 issue is full of recent theoretical and empirical debates in the subfield of comparative and historical sociology. The issue contains a book panel, two conference reports, a book review, and a section on two recently published books. We kick off with a 2018 SSHA book session of potential interest to many of our members: *A Secular Age Beyond the West*, edited by Mirjam Künkler et al. The panel examines the implications of Charles Taylor's work on secularity for non-Western societies. The 2018 ASA panel on *Epistemology, Theory and*

Methods discusses numerous methodological questions in our field, which is followed by Richard Lachmann's review of *Kafka, Angry Poet* (2015) by Pascale Casanova. The 2018 SSHA session titled *Legitimacy, Populism, and Representative Politics*, as summarized by Barış Büyükokutan, Richard Lachmann, and Matty Lichtenstein, lays out the emergent political patterns across the globe. Finally, we conclude with two new books shedding light on the current state of democratic societies. Hope you enjoy the new issue of *Trajectories*. I look forward to seeing you all in NYC this August!

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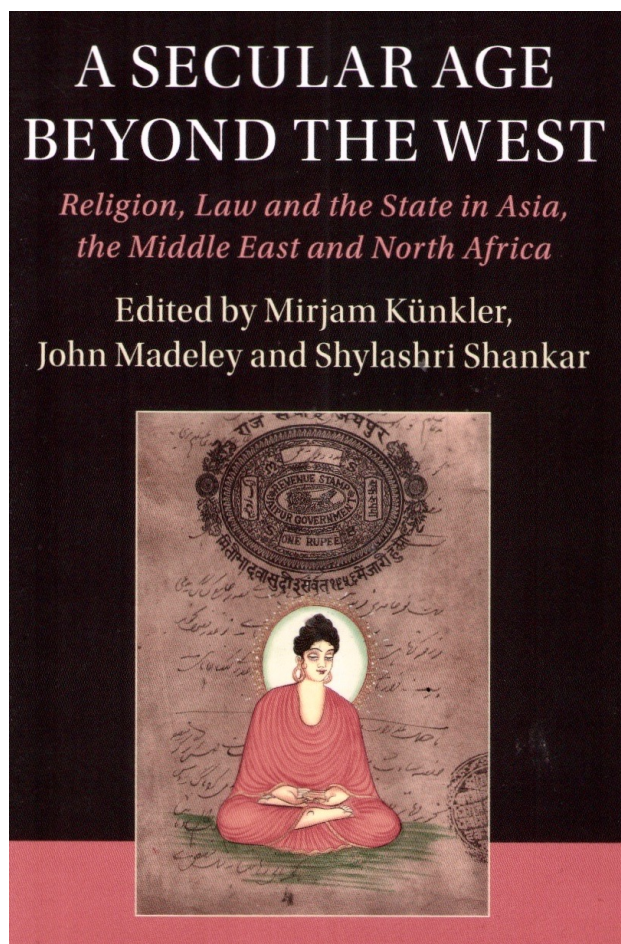
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A Secular Age Beyond the West: Religion, Law and the State in Asia, the Middle East and North Africa

by Mirjam Künkler, John Madeley, and Shylashri Shankar



This book panel took place at the Social Science History Association Annual Meeting on 10 November 2018 in Phoenix, Arizona. In what follows, panelists Ateş Altınordu, Florian Pohl, A.J. Hawks, Fatma Müge Göçek, and Jonathan Sheehan summarize their review of *A Secular Age Beyond the West*, followed by the response of the co-editors Mirjam Künkler, John Madeley, and Shylashri Shankar.

Review 1

Ateş Altınordu

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A Secular Age Beyond the West is a very rich book, both empirically and theoretically. The volume contains such diverse material in its many chapters, I cannot possibly aspire to discuss all of its contributions even in a cursory manner—instead, I'll focus my comments on its major theoretical arguments, not only those found in chapters explicitly devoted to theoretical issues (those by Gorski and the editors), but also those advanced by area specialists in their eleven case studies from Asia, the Middle East, and North Africa.

Many sociologists of religion writing in the 1990s and the following decade noted the dead end reached in debates between classical secularization theorists and proponents of the religious economy model (cf. Stark 1999, Chaves and Gorski 2001). Some led the way out of this impasse through comparative studies that seek to identify historical—and often political—factors that resulted in divergent “secular settlements” (Gorski 2003, Smith 2003, Casanova 2010). Some further called for an expansion of the scope of these studies beyond Europe and North America (Casanova 2006, Gorski and Altınordu 2008). Finally, a few sociologists and political scientists conducted comparative analyses investigating Western and non-Western secularities through common analytical frameworks (Kuru 2009, Akan 2017, Buckley 2017). These cross-religious and cross-regional comparisons reject the construction of distinctive analytical frameworks for the study of Western and non-

Western cases, a tendency which reproduces cultural assumptions about the radical incommensurability of religious phenomena in the West and the rest (Altınordu 2013).

While *A Secular Age beyond the West* does not explicitly juxtapose Western and non-Western cases—it investigates only the latter—it threads the same fine line of sensitivity to difference without intellectual compartmentalization. It does so by (1) applying the framework Charles Taylor developed on the basis of the historical experience of Western Christendom in *A Secular Age* (2007) to explore non-Western cases rooted in different religious traditions, and (2) exploring the limits and blind spots of Taylor's framework in light of these cases.

This is not a simple task, however, as Taylor's often allusive concepts developed for the phenomenological interpretation of Western secularity are rather ill-suited for the comparative-historical analysis of diverse secular settlements. Philip Gorski thus proposes a number of concepts and mechanisms drawn from other theoretical sources. The first is Luhmann's systems theory, which conceptualizes four forms of differentiation. Gorski uses these categories to develop fourfold ideal-types of the internal differentiation of religious systems on the one hand and the differentiation of religious and political systems on the other. The second theoretical source is Bourdieu's field theory which, Gorski suggests, foregrounds politics and power through a double focus on conflicts among religious elites and boundary struggles between religious and political fields. This move allows the explanation of secular settlements as outcomes of classification struggles. Finally, historical institutionalism prompts analytical attention to critical junctures where religion-state relations are fundamentally reconfigured, often in founding eras marked by the writing of constitutions and legal codes. The resulting settlements usually persist through longer periods in a path dependent way, with distributional coalitions and policy legacies

exerting constraining influence over the nature and scope of conflict over religion and secularism.

Given that the search for general theories of religious change have been largely abandoned by now (with some exceptions such as Norris and Inglehart 2011), theoretical advancement in the field is likely to come from the incorporation of "a more nuanced set of descriptive concepts and a richer stock of explanatory mechanisms" (p. 43) into the comparative-historical study of secularities, as exemplified by Gorski's chapter. The three sets of analytical tools discussed in this chapter promise to enable the development of more sophisticated explanations of diverse secular settlements across the globe.

Many classical secularization theorists argue that religious pluralism, a condition unleashed by the Protestant Reformation, is a major cause of secularization. In the most well-known version of the argument, Peter Berger in *The Sacred Canopy* (1969) claims that awareness of a plurality of belief systems challenges the plausibility structure of religion and thus contributes to a progressive decline of religiosity. Advocates of the religious economies model, on the other hand, assume more or less the opposite: a pluralism of competing religious firms uninhibited by state regulation, they argue, increases religious vitality. What makes Taylor's book most original and relevant for the study of secularities is his category of Secularity III, that is, conditions of belief marked by the availability of a plurality of religious/spiritual options, including unbelief, to large sections of society. This move shifts the standing of pluralism from an independent to a dependent variable. Rather than being a cause of secularization, Taylor argues, religious/spiritual pluralism *is* secularity itself.

Given the centrality of this element to Taylor's framework—including ease of conversion between alternative faiths and open unbelief as

a widely available option—its relevance to most of the non-Western world may at first seem marginal. As Künkler and Madeley emphasize in their concluding chapter, most non-Western settings feature “marker states” that differentiate between their citizens according to their religious identities, often privileging some and excluding others. They thus restrain the sort of religious/spiritual pluralism which, according to Taylor, marks the post-Christian societies of Europe and North America.

The volume as a whole is proof however, that Secularity III is not necessarily an irrelevant concept for non-Western societies. Scholars of secularity as institutional differentiation have long known that the binary question of whether a society is secular or not is not particularly productive. The point is rather to investigate particular institutional arrangements and ideologies that make up the secular settlement in question. (Is Germany secular? While answering this question in the affirmative makes sense in many ways, a simple ‘yes’ would gloss over the extensive cooperation between churches and the state in social provision or the offering of religious education in public schools in most *Länder*.) Similar with Secularity III: While a radical pluralism of religious/spiritual options is often hindered by marker states and militant groups in non-Western settings, it is still meaningful to ask: What kinds of religious/spiritual options are available, and what sorts of religious or philosophical difference are strongly suppressed? What forms of conversion are relatively common, and which boundaries are too costly to cross? What is the degree of interest in and the freedom allowed for open unbelief? How did these conditions of belief change over time, and why? And finally, what factors explain major differences in conditions of belief across different societies? Approaching Secularity III in this way allows the contributors to this volume to explore important aspects of conditions of belief in non-Western and non-Christian settings.

Finally, Jaffrelot in his chapter on Pakistan and Hashemi in his chapter on Iran identify “another route toward secularization,” (p.162) one that goes through the transformation of religion into a marker of national belonging. While Jaffrelot is right to observe that such coupling of religious and national identities is very common in the contemporary world, his proposal that we categorize this phenomenon as yet another form of secularity, perhaps as Secularity IV, runs into a number of problems. The first is that in many cases—as in communist Poland, for example—the coupling of religious and national identities results in heightened religiosity.

Jaffrelot’s position is based on the conviction that what seems as religiosity in these cases is actually something else: When religion comes to function as a boundary marker of the collectivity, it is emptied from its spiritual content and becomes a thin identity in the service of political mobilization. He builds this argument on Ashis Nandy’s distinction between religion-as-faith and religion-as-ideology. Yet in my view, this is a false dichotomy: Religion as pure faith is a romanticized—and probably secularist—construct, or at the very least a very rare historical occurrence. Religion is always already politicized, although the form and effects of its political dimension changes historically and across cases. Moreover, to the extent that it involves the de-differentiation of religious identity and political citizenship, religious nationalism may in fact more correctly be seen as a form of desecularization, as Saeed (2017) convincingly argues in her recent book. Thus, we should probably continue to discuss this phenomenon through the analytical framework of religious nationalism, rather than take it to be a new form of secularity.

A Secular Age Beyond the West makes major contributions to the comparative-historical study of secularities through its analytically focused and informative case studies, the comparative framework developed by the editors in the introductory and concluding chapters, and the descriptive concepts and

explanatory mechanisms proposed by Gorski that inform many chapters in the volume. The expertly written empirical chapters will be useful to those who need reliable overviews of the respective cases, while the theoretical contributions of Gorski and of Künkler and Madeley are likely to become common references in discussions in the field for a long time to come.

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Review 2

Florian Pohl

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With its comparative scope examining the history of religion-state relations in societies outside the North Atlantic World, *A Secular Age beyond the West* offers eleven compelling case studies. These cases draw attention to the context-specific processes that have led to the

formation of boundaries around notions of religion and state and how these affect religious practice and belief in each case. Given the still-influential view of the Muslim World's putative exceptionalism to the secularization narrative, the inclusion of six Muslim-majority societies among its case studies is especially significant. Representing different geographical regions, political systems and dominant Islamic currents, these cases demonstrate not only the variety of patterns of secularization in Muslim societies but also their dependence on specific social, cultural, and political conditions that call into question essentializing explanations of religion-state relations in terms of perennial characteristics of Islam. The varieties and dilemmas of secularization are articulated in Gorski's eponymous chapter that together with the editors' weighty introduction and conclusions frames the discussion of the eleven cases. Together they underscore the volume's contribution to the growing body of scholarship that has historicized our understanding of secularization by demonstrating empirically and comparatively that the term "secularization" does not refer to a circumscribed set of religion-state relations but captures a wide range of state policies and their regulatory effects on religion (e.g., Driessen 2010).

Each case study investigates the extent to which Taylor's three modes of secularity have developed in contexts where religious traditions other than Western Christianity are dominant. Despite the project's broader aim, a clear focus emerges on tracing the differentiation of political authority, law, economy, science, and education from religious norms or, in Taylor's terminology, Secularity I. Secularity II and III, the latter pivotal to Taylor's understanding of the secular characteristics of the North Atlantic world, are found to be mostly absent in the cases investigated. The reason why the volume spends more time than Taylor does examining Secularity I is that in all cases the modern state emerges as the central institution shaping and at times determining the possibilities of belief and unbelief. Thus, the general unavailability of

Secularity III in each case, the editors advance as an overall conclusion, has less to do with the inability of religious traditions other than Western Christianity to produce internal reforms that would make open religious non-belief acceptable and more with the shaping influence of state policies on the conditions of belief. In most of the cases investigated, the post-colonial state has not only remained deeply involved in religious affairs but has premised legal citizenship on religious belonging. It is this centrality of the state as "the one overwhelming factor in qualifying, shaping, molding conditions of belief" (345) that stands out to me among the project's key findings and on which I wish to comment in more detail.

Those of us studying questions of religion-state relations in Muslim communities will find in the volume's alternative history of Secularity I further empirical support to unsettle still-dominant categories in scholarship on "Islamism" or "Political Islam" where the analysis of Muslim activism in connection to the institutions of the modern state remains the primary focus. Oliver Roy (1994) is perhaps one of the most widely known scholars to center their understanding of political Islam on the goal to seize control over the institutions of the state. Too often, this perspective frames Muslim activism as trespassing on presumably secular politics and a religion-free public sphere. By demonstrating empirically how Secularity I has been characterized not simply by differentiation but by the modern state's appropriation of religion for the sake of nation-building, *A Secular Age Beyond the West* turns on its head the prevailing perspective of Islamic tradition's illicit intrusion into the neutral domain of the state. The case studies detail how the state's co-optation of Islam has been carried out, even in presumably secular nations such as Turkey, by nationalizing Islamic institutions from the legal system to the national education system. What these cases make visible then is not so much an incursion of Islamic tradition into the otherwise religion-free domain of politics but the increasingly intrusive powers of

the state in areas of social life that had previously been outside of its regulatory control. Consequently, Muslim social activism, whether activists actually seek to capture state power or not, cannot help but come into contact with the institutions of the state because, as Talal Asad has suggested, “[n]o movement that aspires to more than mere belief or inconsequential talk in public can remain indifferent to state power in a secular world” (1999, p. 191). By bringing into sharp relief the state's intrusive regulatory powers, *A Secular Age Beyond the West* encourages a more nuanced rethinking of the boundaries between the political and non-political upon which scholarly assessments of political Islam continue to rely.

My second point also starts from the study's overall conclusion concerning the centrality of state policies in establishing faith and non-faith options, but here I want to consider briefly the limitations of the state-centric perspective for a discursive understanding of the concept of “religion” upon which the “the secular” depends. Although the editors from the outset acknowledge the need for greater genealogical work on the concept of religion in future studies of comparative secularity, the volume's state-centric perspective tends to make visible only one side of the power relations that shape the process by which specific formations come to be understood and categorized as “religion.” That the category of religion, like other discursive categories, is deeply political is neither a new nor a controversial claim. And as individual cases in this volume demonstrate, when applied to societies beyond the North Atlantic world it has served European colonial designs and, subsequently, the interests of national elites. Yet, as an interactive category (Hacking 2003), it is not limited to such coercive usage. That religion can be put to work in more ways than one is convincingly illustrated by Tisa Wenger (2005). In her study of the Pueblo Indian Dance Controversy of the 1920s, Wenger shows how “religion” can be used also as an effective means of resistance by

subordinated peoples. Similar perspectives on how the term religion is embraced and re-appropriated by those so described, including for subversive interests, will make welcome future additions to the insights generated by cases included in the current volume. In the chapter on Indonesia, for instance, Künkler's insightful discussion of the coercive effects the state's policy of *agamasasi* from the 1950s to the 1970s had on the development of Confucianism as one of the state-recognized religions leaves room for investigating how the shifts that resulted from employment of the category of *agama* or religion can be understood not only as coercive but also as a positive, constructive adaptation to contemporary conditions—adaptations that arguably began well before national independence in the early decades of the twentieth century. In such a longer historical trajectory, then, the 1965 recognition of Confucianism as one of six state-sanctioned expressions of religion might be understood not only as the result of a forced, state-induced process of reform but also an effective endorsement of the claims by Chinese citizens in defense of their national and cultural identity.

A final point I wish to submit here emerges from the volume's deliberately comparative thrust. Analyzing secularities beyond the west has solid implications for better understanding religion-state relations (Secularity I) within the modern West. Drawing once more on my greater familiarity with scholarship in the field of political Islam, I find few studies that analyze within a shared comparative framework the growing political demand by Muslims for fuller public inclusion of Islamic perspectives. John Bowen's studies of legal reforms in France and Indonesia are a noteworthy exception to the dearth of such global studies of political Islam (e.g., Bowen 2010). Yet, such political demand is evident both in polities where Muslims are a majority and in Muslim-minority countries. One of the dilemmas contributing to this scarceness is undoubtedly the challenge for any one scholar to familiarize themselves in sufficient

detail with the complexities of how the boundaries between religion and state have been drawn in each instance. The eleven case studies of *A Secular Age Beyond the West* offer rich resources in this regard. The challenge for social science and normative political theory—one that the editors must be commended for taking on in their analysis that frames the cases—is to understand not only the differences but also the similarities across such broad range of institutional and ideational structures. In doing so, we may come to question further the intrinsic sociological value of distinguishing between secularities in and beyond the west.

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Review 3

A.J. Hawks and Fatma Müge Göçek
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We want to open by conveying that we are highly impressed by this volume. The breadth of case studies is truly commendable and we are immensely grateful for the widely varied examples of Islamic societies in particular. Each chapter included is independently impressive and rich and we wish we could do a full review of each chapter separately.

Generally, our comments are organized into two categories. First, we will make a few observations within the context of the conceptual framework provided. And then secondly, we will make a more general post-colonial, ontologically oriented critique.

To that end, we want to quickly make a few brief comments within the existing framework. First, the editors and authors employ an excellent phrase throughout: the "differential burden of religion by the state". In simple terms, they explain, this relates to the different degree of "contact surface" a religion has with the state and confirm through these cases that religions are indeed "differentially burdened" by state policies. Accepting this, it seems like we must also talk about the differential burdening of religions by civil society (and culture at large), especially if we are trying to discuss Secularity III.¹ The pressures in society beyond the state that shape social life are deeply significant sociologically, and thus any full understanding of secularity should necessarily account for them.

Second, secularism and secularity are by nature defined over and against how we define religion, which the introduction (and Gorski's piece) acknowledge. As raised, we have a

¹ The concepts of Secularity I, II, and III are used as presented by Taylor in *A Secular Age* and adopted in the volume in question. Generally, Secularity I refers to the removal of religion from the Public Sphere and Secularity III refers to the "conditions" surrounding belief that make religious belief just one of several equally valid options.

definitional problem—one that the case studies explore in their particular contexts. In particular, can “religion” be a universal category? This raises the question: can we meaningfully compare these case studies of Secularity I, especially if the “thing” being divided out is not the same? The implicit answer of these authors seems to be yes (hence the volume), but this answer is not actually answered or defended so much as assumed.

And third, coming from a cultural perspective, we would like to hear more thoughts on the function of secularism/secularity as a concept in society (the “why”). Especially in the context of Secularity I. How is SI justified as a societal value? This was alluded to in the introduction chapter discussing cases that do not easily conform to “religion” definitions. But it seems this would be a worthwhile next step in the conversation. Consider one comparison for reference: In the US, SI is presented as protecting religion and conscience from tyranny. In Singapore, SI is presented as protecting national unity from religious disunity. What conclusions can we draw from this variety of cases, if any, about how secularism comes to serve particular functions? And are those functions substantially different or is it just semantics? We do not have a ready answer and would love to see more work along these lines.

With that, we want to pivot toward our more theoretically oriented response. Both Charles Taylor who has provided the conceptual framework for this book and the editors (Künkler, Madeley and Shankar) as well as the authors themselves do an excellent job of explaining the impact of *16th-19th century modernity* on *Western European* state and society and the ensuing tension between *religion and secularity* in the *public sphere*.

What we want to draw attention to are the constraints within which the authors all operate in terms of the concepts and institutions they employ: We would argue that these

formulations initially emerged at a particular space (Europe), at a particular time (16th-18th centuries), at a particular political, social and economic conjuncture that generated a fundamentally novel subspace (public sphere) and a way of life (modernity). At this very specific conjuncture, *religion* as it had been practiced in Western Europe intersected with all these elements to generate (Western European) *secularity*.

Hence the secularity analyzed here, we would argue, is a temporally and spatially bound end-product of a particular trajectory of social change.² Yet the scholars assume its universality and study it beyond the West through the three institutions of religion, law and the state, institutions they again assume to be universal and therefore socially constituted in a similar manner regardless of time and space.

The difference between what we propose and what the scholars practice is theoretical. The scholars assume that the similarity and difference in what happens ‘beyond the West’ is merely **epistemological**, that what is different or similar can be merely traced to the process of ‘knowledge’ formation. As such, one can use concepts like secularity, and institutions like religion, law and the state to simply study, through comparison, how they form elsewhere, outside of the West.

It is not surprising that they find the state—or rather the nation-state model imported from the West—ultimately determines the boundaries of secularity beyond the West. They do so because they have a particular conception of state they employ to interpret what they see on the ground: what they find is what they had conceptualized in the first place. What remains on the ground beyond that conceptualization is silenced. There are two problems here: first,

² If defined as such, the differentiation of secularity from secularism and secularization also does not make sense because scholars assume that all three are universal and therefore not bound by particular ‘real’ constraints when they indeed are.

scholars appear unaware that their standpoint is Western-centric, and second, that they need to be critically self-reflexive in order to move beyond the Western-bounded origins of their concepts and institutional analysis.

We would propose that the similarity and difference in what happens beyond the West is **ontological**, that what is different or similar needs to be traced not only to the formation of ‘knowledge’, but also the *social practice of everyday ‘experience.’* It is the combination of knowledge and experience, we would contend, that generate social reality and being. To a certain extent, this reflects our earlier comment about needing to consider civil society and culture more broadly.

Yet the only way to reach the component of experience is by going beyond the Western-centric standpoint, to focus on the standpoint of those living in societies ‘beyond the West.’ The indigenous ideological, political and regulatory systems have to be studied from the ground up within the local confines, temporally stretching back to periods that pre-date Western intervention and influence. Failure to do so would reify Western conceptualizations because one would ultimately end up seeing locally the mirror images of Western concepts, albeit in subverted and transmogrified form.

We would further argue that among all the conceptualizations in the edited volume, Phil Gorski’s comes closest to capturing what happens on the ground locally. After all, Bourdieu’s conceptions of the social field and habitus are open to capturing what is publicly observable on the ground. Of course, we are not suggesting that Gorski’s piece is literally the most local—the case studies are certainly more directly connected in that sense. Rather, we see Gorski’s proposed model as being most theoretically open to truly localized inputs that do not originate in Western conceptualizations. Yet, we would contend that Bourdieu too is constrained because of his inability to critically reflect on the fact that his concepts all draw

from Western knowledge and experience and, as such, privilege the public over the private, state over family, reason over belief, and observable behavior over non-observable human action such as emotions. As such, what is left out of the Bourdieusian framework comprise all those locally produced elements that do not have mirror images in the West. Unless one brings into the analysis the formulation of social reality from the standpoint of the locals, it would be difficult to get a sense of what really is happening beyond the West. All we would end up achieving would be to determine how much of what is happening locally is similar to and different from Western knowledge and practice.

Further, and finally, the very concepts being employed here are also concepts that were explicitly employed in some contexts to justify imperialism. Religious historian Tisa Wenger has pointed out that the idea of “religion” (followed closely by “religious freedom”) as understood in Western liberalism has been employed in an imperialist way. This naturally suggests that its corollary “secularism” is also tied up in imperial power. To that end, while we agree with Gorski that secularity is a field of contestation *a la* Bourdieu, we wonder to what extent this field’s boundaries were pre-set before the contest came to a particular nation-state. Thus, a different ontological problem: does a post-colonial consideration suggest that there are at a minimum two distinct contests over secularism, one amongst the colonizers and one amongst the colonized attempting to adopt and adapt?

We want to emphasize that this edited volume represents the finest work within the most theoretically sophisticated formulation in the West. It is also very praiseworthy that the scholars take this formulation beyond the West. Yet doing so reveals, we think, the limitations of the formulation, especially its temporal and spatial boundedness that the scholars do not adequately question. Interestingly, in his commentary on the volume, Charles Taylor

completely avoids this theoretical impasse by arguing that what he has formulated he has done so in order to solely explain what is happening in the West and why. He is not interested in the rest. Yet therein too, we would argue, lies a fundamental assumption: what has happened and what still happens in the West is not at all built on how the West colonized and exploited the world ‘beyond the West’ to grow into the hegemonic power it still wields today. History and local everyday practice thus emerge as the two elements that destabilize Western-centric formulations.

Yet we need to emphasize that the critical post-colonial critique offered here literally advocates an ontologically different take on all social processes. As such, it invites many more discussions and debates that go beyond what this edited volume has established. We congratulate the editors once again for an excellent job, one that has been established within Western-set limits.

Review 4

Jonathan Sheehan

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A Secular Age Beyond the West is a marvelous volume. The sheer variety of ways in which religion and the state—the study of whose “contact area,” as the introduction felicitously describes it, forms the bulk of the volume—is remarkable, it teaches us. The richness of its articles is testament to the vitality and integrity not just of these particular histories, but also the historiographies that engage with them. The expertise on offer here is humbling.

What, then, does an historian of Europe and a student of the history of Christianity—someone whose work is certainly *not* beyond the West—have to offer by way of useful perspective? Three things come to mind: a) as a critic from the European side of the volume's principal interlocutor, Charles Taylor and his *Secular Age*, I can offer some views on the model that many of the articles engage; b) as an early

modernist, I can offer some reflections on what a longer timescale might have to offer to articles that overwhelmingly focus on the history of the last half of the 20th century; c) as an intellectual and cultural historian, I can offer some challenges to the state-centered accounts that form the bulk of this volume.

A. From the beginning, I was curious how Taylor's model works when taken seriously by scholars of the non-West. I feel safe in saying, after reading the volume, that it doesn't work too well.

Collectively, the essays minimally point to the contingent nature of Taylor's framework, namely his gathering together of three different historical phenomena underneath a single umbrella called “secularity.” Secularity 1, 2, and 3—as they are termed in the book—name respectively the differentiation of law, politics, and economy from religion; the decline of religious belief and practice; and the shift in the possibilities of human experience in which religious belief becomes a matter of choice rather than a default reflex. Although Taylor seems to think that these are independent transformations, he not only gives them a shared conceptual vocabulary, but the history he tells also strongly suggests that, at least in the West, they are in fact a set of integrated processes. The essays in this volume show convincingly how unusual this integration must have been in the Euro-American context. The cases studied here, that is, show how easy it is to unbundle these various “secularities,” and to develop one or more in the absence of, or even in direct opposition to, the other. One can, the essays show, have more religious belief and more separation (Turkey and India, for example) or more separation and less choice (Indonesia), or less separation and more possibilities of choice (Morocco).

More stringently, it seems likely that the problem is more than just contingency. Indeed, it may well lie with the concept secularity in the first place. It could be construed as a

sociological descriptor, and as such serve reasonably well to account for trends in certain Western European societies. In much of the rest of the world, however, secularity is accompanied by aspirational political programs, or ideologies, usually called “secularism.” What is interesting in this regard, is how secularism does not play nearly as powerful an ideological role at the political level in the North Atlantic as it does elsewhere. France is of course the great exception, but by and large “secularism” is a minor thread in the constructive political projects of Euro-America. This is not to say that various aspects of what we would now call secularism—the transfer of education, say, from the hands of the clergy—are not crucial to the liberal, socialist, and nationalist political imaginaries of the modern west. But these aspects were usually subordinate to more comprehensive political projects, and “secularism” in itself largely the hobby horse of eccentrics. When we look beyond the West, as these essays make abundantly clear, it becomes terribly difficult to hold onto the diagnostic language of secularity—whether versions 1, 2, or 3—since the diagnoses have become themselves ostentatiously energized to do the work of managing states and their populations.

B. Most of the articles in the volume rely reasonably consistently on a before & after story, whose caesura is provided by the experience of twentieth-century colonialism. Part of my concern about this doubtless has to do with my parochial perspective—as an historian of early modern Europe, there is no correlative experience like colonialism that might be offered as a definitive rupture in cultural or political or religious history. This is not to say, however, that people have not tried to find something like this: in Germany, for example, the investiture crisis of the 12th century often serves as a kind of convenient caesura that can anchor something like a distinctively Western story. Two generations ago the Reformation served a similar purpose; and more recently it has been the wars of religion of the 16th and 17th centuries that

function as a threshold, especially for modern sociologists and anthropologists of religion more generally. On the whole, however, most of these stories are at best heuristics, but often injuries to the historical record.

Much of my own work has tried to unravel stories like this, to elaborate a differentiated history of modulation and transformation across key historical thresholds. In the past, I have criticized Taylor’s book for the bluntness of its historical argument, its lack of precision about when and where change happens. That said, I think Taylor has it right to insist that the processes that interest him need a wide temporal horizon to understand. This is something largely absent in the essays here. 1923 in Turkey; 1945 in Indonesia; 1947 in India and Pakistan; 1948 in Israel; 1949 in China: there were undoubtedly events of massive political and religious consequence, but assessing their nature would seem to demand substantial work on the past worlds that they overturned. Jonathan Wrytzen’s article on Morocco, in my view, offers one compelling model for how to think continuity and change across a substantial time horizon. But it would be very interesting to experiment with longer *durée* histories in order to get into better focus what sorts of things change and what stay the same in the organization of religious life in the non-West.

C. Finally, an observation from an intellectual and cultural historian. It is hardly a surprise, I suppose, given the topic and the disciplines on offer, that most of the articles (with some notable exceptions, e.g., John Madeley’s essay on Russia and Gudrun Kramer’s article on Egypt) view the issue from what one might call a governmental perspective. This resulted in a number of generalizations, however, about the societies in question that were difficult to evaluate for their validity. Is it the case that the presence of head scarfed women in Turkey’s malls represents a “thoroughly modern vision of entitlement to express private religious identities” (258)? Perhaps, although it is hard to

say unless we spend more time thinking from within these different communities, and not from the perspective of a disciplining state. How big is the “growing clerical demand for secularism” in Iran now (198)? Two clerics do not a movement make, as far as I can see. What is the experience of people who, in Indonesia, converted to the major sanctioned religions from indigenous traditions? Is this in the interest of acquiring state goods? Is this because they simply thought the major religions were, on the whole, a pretty good idea? I'm inclined to believe the former, as a trained secular academic, but it would be useful to hear some accounts of how these processes work on the level of individuals making choices about the world.

In other words, while this book does a wonderful service by bringing the role of the state into focus—something largely missing in Taylor's book—you could use a bit more of his phenomenology or, even better, more history. Most of the articles make clear, for example, that “choice” (Secularity 3) is not really free and most of the places under consideration here. Yet it would be interesting to figure out how “choice” is experienced or negotiated at more local levels. From my Europeanist perspective, I've long mistrusted the clarity of the “age of faith --> age of choice” narrative, since it relies so much on clerical and governmental perspective to produce the former. One might concede that, on the political level, religion was not a matter of “choice” for most people before 1800—governments and churches were disinclined to offer choice, to say the least. And yet, from the perspective of consciousness and experience, religion may well have been importantly articulated around choices. Most people did not participate in religious life with nearly the stringency that the clergy might have hoped, suggesting that indeed, for many, choice *was* important. Religion was negotiated differently in different sociological contexts, moreover, and we would hardly imagine that peasant, an artisan, a city notary, a sailor, and so

forth would articulate their choices about, say, churchgoing in identical ways.

These are hardly revolutionary ideas. I raise them simply to point out that the interest in the powerful (states, churches, etc.) in managing religion, however defined, is a constant in human history. To say, with Partha Chatterjee, that “in all countries and in every historical period, secularization has been a coercive process in which the legal powers of the state, the disciplinary powers of family and school, and the persuasive powers of government and media have been used to produce the secular citizen who agrees to keep religion in the private domain,” is restate an historical truism (87). Namely that every government, every institution built to control human affairs in whatever form, has tried to exert itself on all relevant aspects of human life. This is as true for ancient as it is for modern theocracies; as true of ancient as it is for modern secular regimes. Without doubt, states seek to manage the affairs of their subjects as best they can. The question at the end of the day, though, is how these subjects have ideas, make choices, and create their own forms of life nevertheless.

Editors' Response

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We wish to thank the commentators for their thoughtful engagement with the volume, for taking the time to sift through the case studies so carefully, and for engaging with such nuance the comparative observations expressed in the introductory and concluding chapters.

We divide our response into four parts, responding to the comparative, perspectival, and definitional issues that have been highlighted by the commentators, and close

with reflections on a future research agenda for the comparative study of secularity.

Comparative Issues

Jonathan Sheehan is right to recognize in our engagement with Taylor a distancing from his hermeneutic focus. In particular, the volume's authors do not subscribe to the view that religious change can or ought to be studied predominantly through a history of ideas perspective but contend that it needs to be complemented (and sometimes indeed supplanted) by social history, which many see as highly shaped by state regulatory interventions. In contrast to Ateş Altınordu who argues in favor of the relevance of Secularity III for cases beyond the West, Sheehan concludes that Taylor's model "does not work too well" when "taken seriously by scholars of the non-West". To be precise, Sheehan would have to quarrel with Casanova more than Taylor, as Taylor chiefly took over the tripartite distinction of Secularity I, II, III from Casanova and also broadly adopted from him the definitions of Secularity I and II. Most contributors to our volume conclude that the paths leading to Secularity I differed markedly from those studied by Taylor (due in large part to the historical ruptures of colonialism and imperialism experienced in our case studies), but they also confirm the heuristic value of distinguishing, as Casanova and Taylor do, between the processes of emancipation of various spheres from the influence of religious norms and authority on the one hand, and declining levels of individual religiosity on the other. In fact, while debates about secularization continue to rage in the social sciences, most scholars today recognize the tremendous service Casanova did the debate by deconstructing the secularization thesis into its three dominant separate components. It is remarkable that Sheehan's study of the history of Christianity would prompt him to wish to integrate (or 're-bundle') these separate processes once again into one.

We would wish to push back against Sheehan's suggestion that colonialism, decolonization and modernizing revolutions did not create ruptures, which systematically redefined religious practices, notions of faith, boundaries between the religious and non-religious, and centers of religious authority. To give some examples: with the religious policy of the independent state, Indonesian citizens overnight were summarily compelled to embrace monotheistic notions of religion where a great variety of faiths and cosmological thought-worlds had previously existed. In Turkey, as Geoffrey Lewis has so eloquently shown, the language reforms of Atatürk meant that millions trained in Arabic script became virtually illiterate in their national language, and their religious experience decidedly nationalized. In South Asia, the 1947 partition created entire new states, drawn around perceived religious demographics. Boundaries were not only redrawn but created where none had previously existed, separating different religious life-worlds and fundamentally transforming notions of religion, religious law and religious authority. Whether colonialism, decolonization and modernizing revolutions did or did not create the diagnosed ruptures in our case studies is an empirical question. Sheehan does not substantiate his points empirically, nor does he engage with the evidence presented in our case studies. Instead his perspective appears to underscore how difficult it is to imagine, let alone grasp the impact of these systemic ruptures and transformations unless studied from up close, or actually experienced.

“colonialism, decolonization and modernizing revolutions ... systematically redefined religious practices, notions of faith, boundaries between the religious and non-religious...”

Perspectival Issues

Albert Hawks and Fatma Müge Göçek critique our focus on the vantage point of the state, favouring instead more bottom-up perspectives on how the realities of religion and secularity are interpreted and lived out by individuals and groups.

The volume's authors chose to focus on the entity which appeared to shape notions of the religious and the secular most forcefully. Historical experience of the heavy hand of a colonial state, and the continuation of the colonial framework in the laws and institutions of the post-colonial state meant that analyzing the governmental perspective was judged to be key to understanding the aetiology of these notions. We share Hawks and Göçek's sense that the "differential burdening of religion" could also be applied with benefit to the study of civil society and culture. As we see it, state-level objects – constitutions, law, political ideologies, bureaucratic regulations – cannot be divorced from the particular social experiences from which they emanate, and case studies have highlighted these. For example, the social imaginary of Hinduism as an ancient order emanated from select Brahmin scholars' experience of Hinduism, and it is this strand that found its entry into the Constituent Assembly debates and court judgments. Through the latter it was then reproduced and given a much broader public platform. In Indonesia, the post-independence Ministry of Religious Affairs was overwhelmingly staffed by leaders of a particular Islamic organization, the Nahdlatul Ulama, who largely modeled the ministry's approach to the major religions in the country on these particular leaders' view of Islam as a monotheistic religion with a core set of structural features that were then imposed also on Hinduism, Buddhism and Christianity. In other words, one could re-read particular case studies as studies of how social actors used the regulatory instrumentarium of the state to exert differential burdening of religion.

On a point applying to the Muslim-majority case studies in particular, Florian Pohl writes that "[b]y demonstrating empirically how Secularity I has been characterized not simply by differentiation but by the modern state's appropriation of religion for the sake of nation-building, *A Secular Age Beyond the West* turns on its head the prevailing perspective of Islamic tradition's illicit intrusion into the neutral domain of the state." We agree with this interpretation. Much of the literature on religion and the secular still explicitly or implicitly holds on to the idea that Muslim societies qua religion or culture prioritize Islam over everything else in public life. Often this goes hand in hand with the assumption that Islam is intrinsically unsuited to internal reforms that would recognize the values of pluralism and diversity. The six Muslim case studies in effect show that diversity is an old story here. More often than not religious identities only assumed a bureaucratic character with imperial and colonial policies that essentialized and then reified, while creating barriers for switching. In the post-colonial era efforts at internal reform face an uphill battle in an environment where their proponents are too often silenced and forced into exile. That, per contra, religion can also be used as an effective means of resistance, is exemplified by the alternative ways in which Islam is practiced in Iran, Pakistan or Indonesia, defying state categories, and building worlds of meaning against or in defiance of regime-sanctioned notions of Islam.

Finally, Hawks and Göçek would have liked to hear more about the function of secularism/secularity as a concept in society (the "why"), especially in the context of Secularity I. They write "In the US, Secularity I is presented as protecting religion and conscience from tyranny. In Singapore, Secularity I is presented as protecting national unity from religious disunity. What conclusions can we draw from this variety of cases, if any, about how secularism comes to serve particular functions?" In fact, much research in comparative legal and socio-legal studies has

been dedicated to exactly this issue: why is religious freedom interpreted by some high courts as freedom from religion, by others as freedom of religion, and by yet others as a type of equidistance to the public sphere of all recognized religions? There is a considerable body of work on “regulating religion”, which produced rich case studies on Europe and North America already in the 1980s and which in the past fifteen years has also seen the vibrant expansion of its research agenda to Latin America, Asia and Africa. Some of our volume’s authors have contributed to this research agenda in their other works; for this volume, the focus was simply a different one.

Definitional Issues

We share Altınordu’s observation regarding the concept of Secularity IV, i.e. that it is based on a false dichotomy between pure faith and politicised religion. In fact, we express as much in the concluding chapter. We also agree that religious nationalism, to the extent that it involves de-differentiation of religious identity and political citizenship, ought to be seen as a form of desecularization. Altınordu’s suggestion runs counter to, for example, trends in the anthropology of religion that regard any form of state regulation of religion as manifestations of secularization. We share Altınordu’s sense that such perspectives are more obfuscating than clarifying.

On a separate definitional point, Altınordu agrees with us that the binary question of whether a society is secular or not is not particularly productive, the point being rather to investigate particular institutional arrangements and ideologies that constitute the particular secular-religious settlements. But have we, by adopting Taylor’s usage and terminology with its definitions of religion and the secular, restricted the possibility of taking account of “non-Western” ways of thinking about these concepts? Hawks and Göçek ask: can we meaningfully compare our case studies of secularity, especially if the “thing” which it denotes is not the same? In particular, can

“religion” be a universal category? Our response to this question is “yes”, and for it to make sense in comparative research is contingent on its contextualization by thick description. We believe it is fair to say that our contributors have aimed to trace how public notions and private conceptions of religion changed in the 20th century as a result of public policies, societal pressures, and cultural practices. These public policies, societal pressures, and cultural practices could operate at the local, regional, national, or even supra-national level, as when international treaties introduced new vocabularies of religion which necessitated the repositioning of ritual practices or ethical traditions not captured by these notions (to either fit into those legal notions or to re-model as “culture” or “ethics” instead of religion, for example). Thanks to the fact that all contributors work in the primary languages of their case studies and either are members of or have lived in the societies they write about, they have been able to trace these changes of notions of religion also in the local languages, explaining how the injection of new bureaucratic or legal concepts necessitated the re-shaping of what religion was or is understood to be. The debate regarding whether “religion” can be treated as a universal category is of course a long-standing one, which has somewhat exhausted itself, or so it seems to us, among students of religion. Suffice to say that we broadly concur with Martin Riesebrodt, who in his “The Promise of Salvation: A Theory of Religion” pointed out that there is something to the fact that religious practitioners from different traditions recognize each other’s activities as similar or corresponding to each other, suggesting that the critique of religion as an imposed concept might be overdrawn. It also seems to us that if one took the position that “religion” is not a universal category, one would need to adopt a similar stand regarding the concepts of culture, law, or the economy, for example, withdrawing all of these phenomena from comparative inquiry, and thus eliminating, in the final analysis, any grounds for social science inquiry. To contextualize

what religion, economy, or law mean in different social environments (shaped by both social practices and language) is the life and blood of area specialists – it goes to the very core of what it means to be a historian or social scientist of a particular place, working in the local languages, whether this place is Sulawesi or New Jersey. These are the standards, anyway, we hold ourselves to. Hawks and Göçek write: “What we want to draw attention to are the constraints within which the authors all operate in terms of the concepts and institutions [sic] they employ: We would argue that these formulations initially emerged at a particular space (Europe), at a particular time (16th-18th centuries), at a particular political, social and economic conjuncture that generated a fundamentally novel subspace (public sphere) and a way of life (modernity). At this very specific conjuncture, *religion* as it had been practiced in Western Europe intersected with all these elements to generate (Western European) *secularity*.” But this conclusion, as we see it, defies the readings of all eleven case studies in the book. Whether one looks at China, Japan, Israel, Egypt, or Indonesia, the case studies are all about describing how local notions of religion did *not* map onto concepts of religion found in international treaties or governmental regulations, and how social actors then re-shaped these notions, whether in accordance with or defiance of official policy or societal pressures. What is Western-centric, in our view, is not to trace how diffusion and entanglements forced the reconceptualization of religion in particular contexts, as our authors do, but to assume, as Hawks and Göçek appear to, that every time one reads the English word “religion” in one of the case studies, it stands in for a Western-European Christian notion of religion.

Towards a comparative research agenda

Which way forward then in the comparative study of secularity? We would like to make three suggestions. Like Sheehan, we see a necessity for even longer *longue durée* perspectives. This will require the closer

collaboration with historians, and indeed the incorporation into the research agenda of the work of historians who share an interest in similar social science-driven questions. Recent scholarship in global history shows hopeful signs in this direction. Clemens Six, for example, has written an admirable study of how notions and practices of religion and the secular evolved from the 1930s onwards in the interplay between local and wider regional developments in the geographies of South and Southeast Asia.³ He impressively showcases how experiences with religious education or the introduction of women’s suffrage in one jurisdiction significantly influenced how people thought about religion and citizenship in other jurisdictions, and how these experiences influenced their sense of what it means to be “modern”. Perspectives highlighting longer-term trends in the diffusion of ideas will also invariably turn long-held views on their head: In his intellectual history of the European Enlightenment, Heiner Roetz, a scholar of Chinese History and Philosophy, has shown how the writings of key European Enlightenment thinkers were significantly shaped by notions of morality in classic Confucian writings, which were introduced to Europe in the early 17th century through Jesuit translations.⁴ Notions of the public good, today intimately associated with classic writings of political liberalism, to no small extent are indebted, Roetz’ work suggests, to Confucian notions of what is moral and just. Rather than giving additional weight to too facile assumptions about the comparative superiority of the European Enlightenment, which is then invoked to explain European scientific and economic development and the desirability of the global spread of its alleged ideals, Roetz

³ Clemens Six, *Secularism, Decolonisation, and the Cold War in South and Southeast Asia*, Routledge, 2017.

⁴ Heiner Roetz, “The Influence of Foreign Knowledge on Eighteenth Century European Secularism” in *Religion and Secularity - Transformations and Transfers of Religious Discourses in Europe and Asia*, edited by Marion Eggert and Lucian Hölscher, Brill 2013, 9-33.

highlights the travel of ideas in the opposite direction.

Second, and relatedly, future work on secularity will do well in transcending the confines of the nation-state. We did not conceive of our volume as an explicit study of entangled histories. Nevertheless, it is apparently read that way and for this we are deeply grateful.⁵ Many of our contributors work in a manner that points to entanglements, as they read and work with primary sources in several languages that go beyond the national unit of study. A great lacuna remains in understanding how state practices of regulating religion and societal responses to such efforts travel(ed) across national borders. State administrations often adopt practices from one another, and so do social movements, including religious movements. This has been highlighted recently in the literature on counter-terrorism policies, but is obviously a practice with much deeper historical and often fascinating intellectual roots. In this vein, a recent volume on the 1955 Bandung Conference stirringly documents how notions of statehood, managing diversity, and indeed of international law itself were shaped and exchanged at this conference and made their way into regional bodies of cooperation, such as the later ASEAN and regional legal treaties.⁶ Collectively, the cited works shift the gaze away from Western geographies and actors to highlight the local agency of Asian actors in shaping their futures, and indeed not only theirs: the Bandung conference had a remarkable impact on decolonization and concepts that were later legally enshrined, such

as self-determination, which scholars are only now beginning to appreciate.

“A great lacuna remains in understanding how state practices of regulating religion and societal responses to such efforts travel(ed) across national borders.”

Third, the comparative study of secularity in various environments would much profit from closer analyses in conceptual history. This would mean that participants in these debates would need to immerse themselves much more deeply in the languages used in the environments studied, and a drawing into the field of the sociology of religion of area studies experts. The graduate training of future comparative sociologists (and social scientists more broadly) has required less and less foreign language training, when more would be needed to make original contributions to extant literatures. At the same time, current area studies are often skeptical of comparative, social scientific approaches for fear these might “dilute” the authenticity of their case study. But in order to understand our communalities and differences in the global village we jointly inhabit, there is no way around foreign language training combined with the audacity to try and think through each other’s categories. This is the case for the sociology of religion in particular and the social sciences more broadly. Anything less runs the risk of remaining within compartmentalized thought-worlds without a real possibility to understand one another, and as a consequence to solve collective action problems – which, as we see it, is still a major *raison d’être* for the social sciences.

To close, we sincerely thank the five critics for having prompted us to think through these issues more deeply and to sharpen our perspective on how the comparative study of religion and ‘its others’ might proceed.

⁵ Marian Burchardt, review of “A Secular Age Beyond the West. Religion, Law, and the State in Asia, the Middle East, and North Africa” ed. by Mirjam Künkler, John Madeley, and Shylashri Shankar, Cambridge University Press, 2018. In *Oxford Journal of Law and Religion*, Volume 7, Issue 3, October 2018, pages 580–582, <https://doi.org/10.1093/ojlr/rwy040>.

⁶ Luis Eslava, Michael Fakhri, Vasuki Nesiah (eds.): *Bandung, Global History, and International Law: Critical Pasts and Pending Futures*, Cambridge University Press, 2018.

Epistemology, Theory, and Methods in Comparative-Historical Sociology

ASA 2018 Panel



This panel took place on 11 August 2018 at the Annual Meeting of the American Sociological Association in Philadelphia. Panelists Isaac Ariail Reed, Paul Lichterman, Anna Skarpelis, Sunmin Kim, Brian Sargent, Armando Lara-Millán, Laura García-Montoya, and James Mahoney provide a summary of their work, and Emily Erikson discusses the panel for *Trajectories*.

A Pragmatist Approach to Comparison and Causality in Historical Sociology

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Let's say you are advising, or writing, a three-case study dissertation. You want the dissertation to be welcome scholarly news. That usually means it makes a causal argument with

comparisons. You also are alive to the criticisms of approaches that define comparison cases as collections of factors or variables.

What do you do? Our paper proposes a novel answer that draws on pragmatist epistemology and social philosophy, and ethnographic research procedures. This short summary focuses on our approach to defining cases, and theorizing and justifying case comparisons, and then ends with the briefest mention of normative criteria in selecting cases. The paper illustrates our approach with Reed's research on the Salem witch trials and the Whiskey Rebellion in the US.

Historians and ethnographers potentially share an orientation to chains of meaningful action that emerge in relation to problems. Actors conceive, often change, their ends in view depending on their encounters with problems. Chains of action unfold across time and space, crisscrossing between collective and individual

actors. Custom, habit, creativity and reflexivity all play a part in the unfolding action. Of the perhaps innumerable chains, which do we choose to interpret? And which do we compare in order to arrive at an interpretive explanation?

We propose that historical sociologists embrace this scenario of action, and we offer a pragmatist approach to the problem of which actions to interpret and compare. It is indebted to John Dewey and his notion of “conduct”—meaning social action that occurs somewhere on the arc between reflexive understanding and habit. Making the historical sociologist an investigator of conduct opens up challenges but also possibilities that we would lose with a current, deservedly prominent approach.

We call that approach “causal combinatorics.” A category of approaches really, it draws on set theory to formalize comparative method in the interest of causal analysis. This line of research includes Qualitative Comparative Analysis, and important work by our fellow panelists. Work in this category uses cases to represent different combinations of elements or factors that do or don’t work, in experimental time, to produce outcomes of interest: fascism or no fascism, for example. The focus is on combinations of qualities or factors, not average effects of linear correlations taken as causes. This approach bids us produce medium-N research design, not small-N. And that’s not a bad way to write a dissertation, or an award-winning mature work!

The causal combinatorics approach has been one of many different approaches to the fine line between history and sociology. Another conversation that has taken place concerning the relationship between history and sociology has been focused on events and temporality. Causal combinatorics itself considered temporality through concepts such as path-dependence and critical events. Others have plied a more interpretive approach, asking how events are cognized as events by the actors involved, while events are happening. For example, Abbott advocates for a concept of

duration—referring to an experience of time that is holistic—using the metaphor of a musical phrase to capture this. In contrast to Abbott, but also outside the causal combinatorics frame, Sewell has advocated “eventful time” in contrast to “experimental time.” In his view, precisely because events transform the basic rules of the game of social life, events also render *causality temporally heterogeneous*.

For us, the turn to events and temporality has two implications. First, these works invite a larger reorientation toward action, interaction and interpretation as a process. And second, they reintroduce the problem of comparisons in the service of causal analysis—even if Abbott’s perspective seems to shift away from that enterprise. That means we are back to looking for *relevant* counterfactual cases, since we don’t determine the relevant counterfactuals from a table of factor-combinations (searching the historical record for a given combination). Of course, we could just foreswear comparisons. But do we really tell colleagues and students not to do comparison?

We propose a different justification for comparisons, a pragmatist historical sociology of conduct that imbibes the spirit of ‘taking action on its own terms’ while maintaining the comparative causal enterprise. Let’s come back, then, to a typical three-case study dissertation. In the causal combinatorics mode, each case is a different, independent collection of factors. The investigator supposes these different collections have led to different outcomes of the same class of event—revolution or no revolution, for example.

Our approach defines and relates cases differently. Each case is an end in itself (a problem of interpretive explanation for the investigator). Each is also a means to solving the problem of explanation in the other cases. The investigator defines cases in terms of *different sequences of meaningful action* -- rather than in terms of similar/different

outcomes. The investigator conceives the different action sequences as different iterations of a shared conceptual category, not different collections of variables. These mutually informing cases constitute a triangle, a “mini-world” of interpretation and explanation. Our approach involves four aspects: theoretical casing; comparison by way of “theoretical translation”; critical-reflexive judgments by a community of inquiry; and (occasionally) the use of normative counterfactuals.

Casing. For ethnographers, including historical ethnographers (for example, Glaeser, 2011), the challenge is to case the chains of meaningful action in a way that honors and juggles the actors’ meanings and the investigator’s meanings (Lichterman and Reed 2015). We don’t want to uncritically adopt actors’ categories as our own terms of analysis. Yet actors’ meanings of course are integral to meaningful chains of action. We have to ask, as ethnographers do: “What do we have a case of?” In order to *case* our historical observations on the French Revolution, for example, typically we would draw on theories of revolution and say we have a case of a particular kind of revolution. But a theory of revolution usually is a theory of a class of *outcomes*; the revolution happens or doesn’t happen, or ends in dictatorship or democracy.

“the challenge is to case the chains of meaningful action in a way that honors and juggles the actors’ meanings and the investigator’s meanings.”

Our method, in contrast, starts with a conceptual casing that admits of meaningful action. We want to have *cases of unfolding conduct*, and then follow conduct toward eventual outcomes, rather than *starting* with

outcomes—as in a theory of revolution—and looking back, combing the empirical record with preselected factors taken from theories of revolution. That method usually truncates meaningful action. So we have to tack back and forth between a sociological concept of action and the acts and actors’ meanings we have observed. This is the core hermeneutic process leading to interpretive explanation in historical sociology.

Comparison through theoretical translation. Conceptualization and comparison work together with casing. In our approach, causal claims are contrastive. We ask a “why question”: Why did some series or “duration” of linked actions unfold, instead of a series that could have transpired but didn’t? The comparison cases in our mini-world of investigation are contrasts, just as they are in ethnographic studies that follow analytic induction. We “*translate*” cases into theoretical terms that admit of situated action—whether the “situation” is of the sort Goffman studied, or rather Geertz. Reed’s (2016) comparison of the Whiskey Rebellion and the Salem Witch Trials illustrates these first two aspects of our method.

While a rebellion is not a witch-hunt, Reed compared the episodes as cases of a conceptual category: the coherence of an ideological response to “unsettled times.” Reed found in Salem ample evidence of an uncertain situation, in which a group of ministers could use the power of coherent rhetoric to make their interpretation of the situation convincing. The why-question emerged: Why did the Whiskey Rebellion not seem to go this way? The Whiskey rebels had access to a coherent, millenarian worldview, but their speeches and other communications conveyed incoherence, humor, nervousness—not the grinding seriousness of Cotton Mather. Having translated the casing concept across both cases, Reed took the 1692 Salem case as the counterfactual comparison for the 1794 Whiskey Rebellion, and vice versa. Each was cased with the same conceptual category, assisting the other’s ability

to address the question of how historical actors render action into a coherent story in times of crisis.

Following this lead, Reed discerned in the Whiskey Rebellion a series of varyingly coherent interpretations, rather than a preponderance of explicit, coherent worldviews as were articulated in Salem. The Whiskey Rebellion thematized grievances and social conflict, without resolving them in an overarching, dominant interpretation. Understood this way, the Whiskey Rebellion case shed more light on Salem. Again, cases illuminate each other. While the Whiskey Rebellion was a site of varied interpretations—a *thematization of extant conflict*—the Salem Witch Trials, in contrast, were a site of “fetishization” of scapegoats (accused women), opening a gap between the conflicts and the terms in which actors interpreted them. So Reed theorized two formats of rhetorical struggle—thematization and fetishization in unsettled times. But how did Reed know which comparisons to select?

The community of inquiry. Put compactly, both C.S. Peirce and John Dewey argued that adequate knowledge claims emerge only in critical-reflexive dialogue with a community of inquiry that assesses the empirical worth of claims *and* the claims’ ability to generate further inquiry. Contemporary Deweyan and feminist epistemologists would point out those communities—disciplinary, sub-disciplinary or trans-disciplinary—rest to some degree on conventional relevance criteria for “a good question” or “a good comparison.” These criteria keep the community focused on shared concerns—but may also induce some tunnel vision. Sometimes, the researcher has reason to try reorienting the community’s dominant approaches to its shared research concerns; we call that meta-communicative dialogue. In this case, the community’s concern is how to conceptualize crisis. Is it a historical conjuncture of organizational and institutional dynamics, or a state of feeling that elicits in

different actors various kinds of action and uses of culture? Reed’s discussion of thematization and fetishization was an attempt to re-orient some of the community’s dialogue about crisis times. Finally, and very provisionally:

Enter the normative? Communities of inquiry sometimes engage practical or “normative,” questions, as Dewey knew. A classic example is “why no socialism in the US?” Our paper considers the use of comparisons and theoretical translations that derive *not only* from other chains of historical action, but from normative aspirations about how action *could* have unfolded.

Consider Du Bois’ *Black Reconstruction in America* (1935). Du Bois compares the trajectories of the nineteenth century Black and White worker, and in turn makes comparisons with European, especially British working-class politics. He also compared with a normative counterfactual case of “what might have happened.” That was the possibility that proponents of universal suffrage and advocates of subordinated labor could have united in a democratic front to challenge racialized capitalism. DuBois analyzed incipient efforts to unite white and black workers, actual instances of black suffrage before the end of Reconstruction, as well as the post-civil war political ambitions of abolition advocates.

Our paper discusses how contrasts drawn from normative questions can be subject to the same critical-reflexive scrutiny as other contrasts. A community that is ready to critically examine cases inspired by normative causal questions, rather than assume those questions aren’t operating, is one that is *more* ready to identify normative blinders in case and concept selection. Under these conditions, comparisons in the service of normative questions may be generative for future inquiry.

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Life on File: Archival Epistemology as Theory

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History is produced from what the archive offers – Marisa J. Fuentes⁷

If history is indeed produced from what the archive offers, sociologists need to better understand the epistemological implications surrounding the collection, analysis and interpretation of archival documents—the process of “production of knowledge about social life” (Reed 2010: 20). Records and archives are part of a life cycle that ranges from record creation to record keeping and record display and end in their deployment in scholars’ analyses, and yet historical sociology has remained comparatively silent on both the precise nature of these archival objects, as well as on the implications of their genesis, preservation and archivization for our scholarly practice. I do not suggest, as Paul Rabinow did about ethnographic writing thirty years ago, that

historical sociology is in crisis; rather, my intervention targets comparative historical sociological (CHS) research methodologies and practices, and suggests ways in which scholars can be more epistemologically conscious when reconstructing the past (Rabinow 1986). Just like ethnographers reflect on site choice and demographers submit their data and analyses to robustness checks, historical comparative sociologists ought to interrogate their records. My aim here is to connect documentary production to archivization and scholarly interpretation, and to ask how taking this relationship seriously affects comparative historical sociological work as well as sociological theorizing.

“If history is indeed produced from what the archive offers, sociologists need to better understand the epistemological implications surrounding... archival documents”

“Archivization produces as much as it records the event” (Manoff 2004: 12), and hence unpacking the material bases of documents is critical, particularly in comparative research. Archival bodies become a tool for better understanding the relationship between history, technology, the archive and its interpretation. This holds relevance beyond our subfield: It speaks to organizational sociology by treating the creation of documentary reality as an organizational and thus traceable and legible process; to cultural sociology by unboxing how the meanings we can recover are contingent on specific power structures, organizational processes and the agency of professional archivists; to historical comparative sociology by providing a framework to think about comparison of distinct datasets; and to mainstream sociology in advancing how we can think about what it means to conduct robust

⁷ P. 146, in Fuentes, Marisa J. 2016. *Dispossessed lives : enslaved women, violence, and the archive*: University of Pennsylvania Press, Inc.

research, as archival bodies can help us dispel the illusion of false necessity.

Archival Bodies as Epistemology, or: Subjectivity in the Archive

An epistemology of the archive has to consider the archive as a field in itself, a space for the production of historical knowledge, rather than seeing archival work simply as a form of fieldwork that extracts evidence or works on the basis of found objects. Framing the question of *archival bodies* around epistemological concerns allows addressing questions of positionality, subjectivity and a pluralism of meaning structures, thus productively melding the literatures of standpoint theory within sociology, memory studies within history, and postcolonialism within anthropology. In sociology, standpoint theory has dealt with the impact of positionality most urgently. Patricia Hill Collins best described distinct epistemologies as localized, partial and situated forms of knowledge, and makes sense of how some epistemologies have historically trumped others: “Far from being the apolitical study of truth, epistemology points to the ways in which power relations shape who is believed and why” (Hill Collins 2015: 252). In history, concerns about power and the archive are localized in the field of memory studies. Scrutinizing *lieux de mémoire* means untangling practices of generating history, means understanding the ‘structuring of forgetfulness’ (Nora 1997: 4; Rouso 1994: 4). Both perspectives overlap with anthropological and postcolonial engagements with the archive that ask us to see state actors as “cultural agents of ‘fact’ production” and that caution scholars to engage an ethnographic, rather than purely extractive, ways with the archive (Stoler 2002; Stoler 2010).

Archival Bodies as Theory

The relationship between theory and comparative historical sociology has been a fraught one, with the ‘death of theory’ proclaimed at regular intervals, just to be refuted in due course (Quadagno and Knapp

1992; Sewell 2005). In *Event Catalogs as Theories*, Charles Tilly asserted that “all social research rests (...) [on] two theories: a theory explaining the phenomenon under study, another theory explaining the generation of evidence concerning the phenomenon” (Tilly 2002: 248). The recovery of traces, our observation of them and the reconstruction of the original phenomena hence become a question of theory. Analogously, I argue that epistemological considerations – epistemology defined here as issues pertaining to “the creation and dissemination of knowledge in particular areas of inquiry” (Steup 2017) – around archives fall as much under the purview of sociological theory as they do under the purview of research methods.

Tilly set out three questions scholars should ask about the generation of evidence in order to explain phenomena: 1) How does the phenomenon of interest leave traces? 2) How can a researcher elicit or observe these traces? And 3) how can we reconstruct whatever is of interest of the phenomenon (cause/effect)? Here, I deploy the term *archival bodies* to set out a partially analogous argument about epistemology in archival research and illustrate what is at stake with examples from my own historical archival fieldwork into racialization processes of the mid-20th century German National Socialist and the Japanese authoritarian regimes.⁸ *Archival bodies* thus denote processes of knowledge construction and organization around the recovery, reconstruction and depiction of events, processes and persons on three levels:

1. The creation of documentary evidence from social life by persons and organizations, i.e. *the turning of a person into a record*, an archival

⁸ Two additional archival bodies – the materiality of the records themselves, and the body of the researcher in the archive – and their attendant problematics, like how hapticity impacts research and what happens when one moves from analog to digital archives, are not treated in this essay.

body in the singular [establishing what is a trace, or identifying appropriate data];

2. The construction of sets of files into a *body of records*, an archival body made up of a plethora of files, by archiving professionals working in organizations dedicated to the preservation and archiving of records who have their own archival projects in mind [data collection, and archivization];

3. The *recovery* of persons from an institutionally preserved archival body by the researcher [transcending the ‘mere record’ through subjectivity reconstruction and interpretation].

Archival bodies are corporeal in the double sense: First, they denote how researchers draw together a corpus, or bodies of material, for analysis; second, they are birth-giving in that they govern how a person can emerge in and through archival documentation. Both are decisively important for the reconstruction of events, but more importantly, for that of subjectivities: How are lives partially pre-narrated through modalities of the record? How do the files themselves act? Questions of personhood and agency are crucial to most sociological analyses even at the macro-historical level, and yet we spend little time reflecting on how the material we draw on contributes to shaping the characters that materialize in our narratives. Archival bodies allow for a more cautious reassembly of historical lives while taking seriously both the individual subjectivities of those captured in the records, as well as the organizational and historical prerogatives shaping the fixing of life on file. The paper this excerpt is based on—which I will submit as part of an invited issue on the sociology of archives curated by Andrew Deener and Claudio Benzecry for *Qualitative Sociology* later this term—illustrates the argument empirically with examples from my own dissertation fieldwork on racial classification in the multi-ethnic annexationist empires that were Germany and Japan, collected over three years in roughly a dozen archives on three continents.

Archival Bodies in History

Bodies are acted upon when traces are left on paper; this is not merely a figurative observation. In the National Socialist regime’s use of archives, being found in the archive could be advantageous if you wanted to prove Aryan status, and not being found could be detrimental; on the other hand, being recorded in religious registers could spell deportation and death (Adler 1974; Majer 2003). Perversely then, the giving of life—by creating a record that was preserved in the archives—could also lead to the taking of life, for those whose existence had been tallied on paper for deportation and murder.

The file is “the most despised of all ethnographic objects” and yet may be the object closest to recording social action of the historical past: Files are “closest to the presence of speech” in “the imagined chain of replacements for the spoken language, *supplements*” (Latour 1986: 26; Vismann 2008: 8). Dorothy E. Smith refers to the phenomenon of treating the text as *internally determined structure of meaning* as document time: An instance in which the text becomes fixed as a social accomplishment (Smith 1974). This fixing of people and processes through “routine textually-mediated practices of people engaged in their daily activities” of course leaves us with a curtailed record of interaction (Cahill 1998: 143; Kameo and Whalen 2015: 210); but in their proximity to quotidian action, files also unwittingly record additional information, and the physical scars they bear of handling and use—marginalia, stamps, burn marks on the pages—allow insights into action a transcribed digital record no longer contains.

And yet, the relationship of truth between files and the social world they purport to record is a tense one. On the one hand, *quod non est in actis, non est in mundo*: this tenet of Roman Law, that *what isn’t in the records, isn’t in the world* condenses one of the fundamental epistemological and cultural sociological challenges facing historical comparativists –

that of dealing with questions of state power in determining what is kept in the archive, of whose stories are told (Taeger 2002). If Nazi killings and Japanese colonial atrocities are less frequently recorded than observations of genealogical presence, this is a sign of power, and not of an absence of the phenomenon in question. Foucault described the archive as a “‘system of discursivity’ that establish[ed] the possibility of what can be said” (Manoff 2004: 18). Even a strong interest in population control does not necessarily lead to the population appearing in the archive in detail: In *Dispossessed Lives*, Fuentes retells how the only ‘voice’ she could find of the enslaved women of 18th century Bridgetown, Barbados, was reference to their screams in court records and accounts by abolitionists. Screams, she writes, became “the historical genre of the enslaved in the colonial archive” (Fuentes 2016: 143). While historians and sociologists can read the archive against the bias grain, as Fuentes suggests, the files themselves in their accessibility and usage are already imbued with meaning. Archives are not “an objective representation of the past, but rather [as] a selection of objects that have been preserved for a variety of reasons (...). These objects cannot provide direct and unmediated access to the past” (Manoff 2004: 14). They move through the world and in their interpretation by different actors, they become agents of collective memory, taking on vastly different meanings and use values depending on context. Rousso famously remarked that historical memory was structured forgetfulness (Rousso 1994). If forgetfulness is structured, so is remembrance: archival records are often selectively used, and contested files are read as representing the truth.

Even where records are plentiful, sociology as a field may refuse engagement – the expansion of sociology as a profession under National Socialism is an open secret in the history of German sociology that few acknowledge, and that others have made their life’s work at the cost of professional marginalization and their work being labeled ‘un-sociological’ (Christ

and Suderland 2014; Klingemann 1996). In many ways, this mirrors American sociology’s failure to engage with the history of slavery more generally, and the American history of slavery in particular (Patterson 2018). Calling for a mindful form of subjectivity recovery of those treated poorly by history, but how differently should we write the history of perpetrators? And more generally, beyond asking ‘how should we *read* a source,’ what does a critical engagement with archival records look like?

Returning to Tilly’s search for reconstructing traces of real-life phenomena, we have to take account of the different types of *archival bodies*—first recorded in files and records, then selectively archived and ultimately interpreted by us—by adequately stripping them down to their historical and archival-organizational scaffolding. Quite apart from normative obligations to do right by the dead, the question of person recovery is also fundamentally ontological in nature. Craig Calhoun termed the danger inherent in seeing the archive as a record of ‘how it really was,’ rather than treating it as a potentially unreliable informant, the *illusion of false necessity* (Calhoun 2003). When confronted with her Stasi files, East German author Christa Wolf wrote that the “perverse mountain of files has turned into a kind of negative grail, to which one makes a pilgrimage in order to experience truth, judgment or absolution.” (Gitlin April 4, 1993). Forms of inquiry that take the archive as a fixed historical record, in which documents are taken in lieu of the ‘lost object’ and are used for “positivistic authentication and pseudo-scientific legitimization”, are problematic (Freshwater 2003: 730).

Archival Bodies in Sociological Practice

How does a person emerge through archival documentation? How do files act? What is preserved, why, and how accessible are the files? These questions surrounding archival bodies ought to be of particular concern to comparativists. After all, how can we engage in

meaningful comparison when the organizations and contexts the person of interest is embedded in are so fundamentally different? Power and agency are inevitably bound up with one another in the archive: whether a person at all appears in the archive offers some cues about their status. Eastern European ethnic Germans appear in the German Federal Archives because the National Socialist (NS) regime had a strong interest in identifying and resettling this diaspora for purposes of ethnic cleansing; genealogical records that were mostly kept in church and city archives became a tool for implementing these racial policies. Being found in the archive could conversely also protect from this new form of violence, ranging from harassment to murder—if one could prove non-Jewishness through the Aryan Confirmation. In colonial Japan on the other hand, although colonial subjects were registered in ‘ethnic registries,’ the information contained within were much less multi-dimensional than the German archives, ostensibly because what was of interest to the Japanese state was a blanket ethnic designation. In practice, this left me with paper trails of up to one hundred pages per person for the German case, and little more than basic demographic data, tabulated in endless military booklets, on Japanese colonial subjects. That ethnic Germans emerge as much more three-dimensional characters through the files than ethnic Koreans do in Japanese archives has nothing to do with any characteristic pertaining to the persons we are seeking to reconstruct themselves, and everything to do with organizational practice, file creation and archival preferences and practices. The resulting differential availability in quantity of files does not mean that the Japanese were unconcerned about ‘racial fit,’ but that they in many cases drew blanket conclusions for entire populations.

If Pascal in *Meditations* espoused a materialist vision of archives, Walter Benjamin turned to a culturalist interpretation of remembering, “in which mental habits across time rather than physical things in the present are what bring the

past into view, and in which specific heirs are necessary for the work of memorialization to succeed” (Fritzsche 2006: 185). It is the task of historical sociologists to recognize the contingency of files, without fearing that this responsible epistemological practice be misconstrued as casting a shadow over the robustness of our research. Recovering the person from the archive demands ‘transcending the mere record’ and justifies the sociological part of historical sociology in that we can productively draw on theory and comparison to construct our arguments (Calhoun 2003). Archival bodies one and two—how organizations put bodies onto file by pinning real life onto documentary reality, and the ways in which archives construct, preserve and make accessible documentation—both shape archival body three, or what type of person *can emerge* from the archive. If we think of authoritarian or colonial state records of subjected populations as partisan fragments, it becomes the task of the researcher to engage in acts of exposition, recovery and rehabilitation. It is our task to engage alternative interpretive devices and dislodge the historical genres already present in the archival record.

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Where is the Archive in Historical Sociology? The Case for Ethnographic Dispositions

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In this paper, we offer guidelines on how historical sociologists should engage with archives. Rather than focusing on the epistemological value of archival evidence—e.g. how we can generalize from a limited number of historical cases—we highlight the pragmatic aspects of *doing* historical sociology in archives: how do historical sociologists approach the material within archives? What sorts of materials should they look for while attempting to generate a theory? We argue that historical sociologists can benefit from thinking like an ethnographer. By specifying ethnographic dispositions, we lay out what it means to think like an ethnographer in archives.

Thinking Like an Ethnographer

When a historical sociologist goes into an archive, she may feel overwhelmed, for multiple reasons. First, it is difficult for the researcher to know which documents are

relevant to the study. As soon as she gets into a relevant archive and opens up a storage box, she will discover that there is no folder labeled with her specific topic of interest. The challenge of identification can be overwhelming, especially for those who embark on their first archival project. Second, many individual documents simultaneously feel as if they contain one small part of the key to the puzzle and are also so trivial that they might not be worth attention. Each document may push the researcher in a small way, but collectively they can add up to something greater than their parts. Even after passing the hurdle of identification, a historical sociologist faces the challenge of weighting: what is important and what is not? Third, when faced with a large body of documents containing various ideas, behaviors, and value statements, separating what actors openly profess from what they truly believe is difficult. Actors can be duplicitous while attempting to persuade an audience in the course of making a decision. As Paul Willis (1981) demonstrated in his classic ethnography of working class youth in Britain, “penetrating” the minds of historical actors is always a difficult feat, even after correctly identifying and weighting all the relevant materials. Identifying the shortest path out of this maze becomes a key goal for the researcher. Researchers often move through these false starts and dead ends before finally reaching an effective and parsimonious theoretical explanation of complex historical phenomena.

These detours, we argue, are not just failed attempts at discovering the “right” kind of evidence, but valuable opportunities through which we can gain insights into the historical actors and processes that we aim to study. Just like an archival researcher, historical actors often experience confusion when faced with a challenge of making crucial decisions. There are too many things for them to consider, and they know too little about the possible implications of their actions. What we perceive as a historical outcome often emerges from such processes of confusion and mistakes, as a

consolidation of nervous, haphazard decisions by historical actors.

Just like an ethnographer attempting to immerse themselves into a new social setting, a historical sociologist in an archive should adopt the attitude of a (purported) naive observer, pay attention to the social contexts of historical actors, and follow them along in their journey as they move through historical events. Rather than projecting an abstract explanation from a top-down perspective, a historical sociologist should first attempt to immerse herself in the situation of the historical actors, pursuing the position of “fly on the wall” eavesdropping on people in the archives.

Although sociology has been late to the conversation, the methodological and theoretical discussions on archives have been going on for decades (Foucault 1982; Ginzburg 1989; Koselleck 2002; Stoler 2010; Agamben 2009), and the exchange between history and ethnography (Stoler 2008; see also Biernacki 2012; Hunter 2013) has produced a novel understanding of the relationship between archival evidence and theory construction. Drawing inspirations from these precedents, we discuss how a historical sociologist can actually think like an ethnographer in an archive. In our manuscript, we also discuss how this approach led us to rethink our respective cases in our book projects—i.e. jail overcrowding in the L.A. county in the 1990s, the federal reserve and housing desegregation in the 1980s, and the immigration restriction in the 1920s—by forcing us to consider new evidence and theory. In this feature, however, we focus on explaining the analytical leverages and pragmatic guidelines emanating from ethnographic dispositions.

Ethnographic Dispositions

Naiveté

It can be all too easy to approach the archives with a sense of knowing what comes next, due to our temporal relation to those represented in the archival materials. Simply put, by virtue of

living in the present, we already know who won the key historical struggles and who survived to tell the tale. Because our interest lies not just in discovering historical facts but in constructing theories out of historical events, we tend to start with a set of research questions based on an observed outcome. The traditional approaches have often relied on comparison of carefully selected cases, designed to isolate key variables of interest. Before going into an archive, we are told, we should be able to clearly articulate our research question and hypothesis, in order to effectively obtain the answer we want. This is one way of approaching the archive, but not the only way.

“a historical sociologist should first attempt to immerse herself in the situation of the historical actors, pursuing the position of ‘fly on the wall’ eavesdropping on people in the archives.”

We call for those venturing into the archives to approach the materials with a disposition similar to ethnographers. When an ethnographer embarks on fieldwork, she often assumes the position of a naive, untrained observer. Indeed it is often the case that an ethnographer knows little about the setting, at least not as well as the people whom she is trying to study. This naiveté is not just being inexperienced and ignorant, but a particular epistemological stance an ethnographer takes to maximize analytical leverage in the given setting. An archival researcher should attempt to approach the archive with as few presuppositions as possible, and remain open about what the story could be, what the outcome might mean, or what actors intend when they talk. Just like an ethnographer seeing the quotidian in new ways, an archival researcher should assume the position of a novice observer, leveraging her naiveté to

uncover the hidden structure under the mundane.

Through naiveté we can develop a better appreciation for contingency in historical processes. As Ermakoff (2015) argued, “positive contingency”—defined as a moment when individual agency gain maximum leverage in a causal process—remains underappreciated when we look at a historical process from a deterministic perspective. By maintaining a naive stance, however, we are freed from our preconceived notions and better equipped to capture the key moments and decisions.

Observing Actions in Context

Ethnographers are typically advised to prioritize their own eyes over their assumptions about people and setting—in other words, they are encouraged to forget about who the actors are and instead collect data on how they act in their respective social settings. We tend to assume that a certain type of people (e.g. colonial administrators) act in a certain way (e.g. denigrate indigenous culture), but often times this assumption is dependent on the situation (e.g. identifying more with indigenous rulers to boast their standing in homeland; see Steinmetz 2007). As Howard Becker (1998: 45) puts it, “people do whatever they have to or whatever seems good to them at the time, and that since situations change, there is no reason to expect that they act in consistent ways.”

This theoretical perspective has implications in archives as well. When historical sociologists read documents, the focus should not only be on “what” happened, but also on “how” something happened—events and actions should be understood in their context, in their relationship to things around them. When ethnographer urge people to tell “how” something happened, it is an invitation for stories with more informative detail, accounts that include not only reasoning for whatever was done, but also the actions of others that contributed to the outcome. When going into a field site, Becker (1998: 60)

“wanted to know all the circumstances of an event, everything that was going on around it, everyone who was involved,” Because he “wanted to know the sequence of things, how one thing led to another, how this didn’t happen until that happened.”

With this stance, we can understand how historical actors learn from their mistakes and change through a particular historical process. Just as researchers learn from dead ends and false starts in archives, historical actors also learn through blunders, and their dispositions and actions evolve as they engage in historical processes. By focusing on “how,” we develop a more dynamic appreciation of how actors and situations come together to produce historical outcomes.

Incrementalism

Ethnographers typically stay within a field site for an extended period of time, following along as events unfold. In the process, they pay attention to the specific manner in which events unfold rather than starting with the outcome and working backwards. Because events of interest are by definition rare, ethnographers often try to spend as much time in the field site as possible, immersed in the mundane everyday routine of the setting, waiting for that spark of eventfulness. Being there and seeing it first hand, as they say, is an ideal of ethnographic immersion, although it is easier said than done. After all, things happen fast and one cannot be present in the field site every day, every hour.

The ideal of immersion has a better chance in archives. Luckily for an archival researcher, the time in the archives progresses rather slowly. Different, but limited accounts of historical events are frozen in the format of documents, waiting to be discovered. It is as if there is not just one but a multitude of flies on the wall, reporting to historical sociologists their own accounts of how things unfolded. By going through these multitude of accounts, a historical sociologist obtains not just a birds-eye view but

a Rashomon-like perspective into the historical process.

In pursuing incrementalism, we can gain comparative leverage to exclude alternative explanations. It is tempting to divide archival evidences into two camps, ones that mattered in producing outcomes and ones that led to dead-ends. In reading incrementally and sequentially, a historical sociologist is better able to pay attention to these diverging paths: what propelled historical actors to go down a particular path that was not successful? What does that tell us about the path that was eventually successful? By doing this we can see which alternative routes were actually feasible to the actors in their particular contexts and rule out explanations that made no sense to actors embedded in those contexts.

Deep Dive into Archives

With ethnographic dispositions, the troubles we encounter in the archives as researchers become an asset rather than burden. That is, much like ethnographers, we try to walk the path along with historical actors, tracing the process through which they learned, as we are learning from archival documents ourselves. We do understand that time and funding are limited for our historical sociologists, and that a historical sociologist cannot stay forever immersed in the historical setting. In order to make a new contribution to knowledge, however, one cannot avoid the exercise of getting lost and finding her way back again. We hope the ethnographic dispositions presented here help future researchers finding their way in archives.

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The Logic of Critical Event Analysis

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Historical and case study analysts have long focused attention on critical events—sometimes referred to as watersheds, turning points, or critical junctures—when explaining outcomes of interest. These analysts suggest that, during relatively well-defined periods, cases

experience occurrences that are highly consequential for their subsequent development. Examples of critical events from comparative-historical work include the choice of coalitional partner by liberal political parties in interwar Europe (Luebbert 1991); episodes of labor incorporation into the state in Latin America (Collier and Collier 1991); the fate of tribal groups during independence struggles in North Africa (Charrad 2001); the formation of inter-ethnic associations in Indian cities (Varshney 2002); and the choices by white moderates about racial exclusion in the United States, Brazil, and South Africa (Marx 1998).

In this paper, we seek to contribute to this literature by offering a framework for the identification and analysis of critical events in case study research. The paper is motivated in part by the lack of a theory of causality in the literature on critical junctures and path dependence. As a result, when researchers assert that event X was a critical juncture for outcome Y, it is hard to know exactly what they mean by the assertion. This framework offers a general theory of causality for the analysis of individual cases.

We define a critical event as a contingent event that is causally important for an outcome of interest in a particular case. The definition features three components: event, contingency, and causal importance. An event is a temporally bounded state of affairs. To serve as causes in explanations, events must be well-defined. We define a contingent event as an event that was not expected to occur, given well-specified expectations. Causal events are also a subset of all events, X as a causal event for Y if: (1) X and Y are events that occur in the actual world; (2) X occurs before Y in time; and (3) there is a logical relationship between X and Y that can be specified using necessary and/or sufficient conditions. Causally important events are a subset of causal events. We define and measure the causal importance of an event for an outcome using two dimensions: logical importance (i.e., the extent to which the event is necessary or sufficient for an outcome) and

logical relevance (i.e., the extent to which the event is necessary and sufficient for an outcome). A causally important event has a high level of logical importance and a non-trivial level of logical relevance.

In this framework, critical events are at the intersection of contingent events and causally important events. We show why both causal importance and contingency are essential ingredients of critical events by considering non-critical events in which only one of these features is present. We emphasize that critical event analysis is not always an appropriate mode of explanation. Non-critical events often or usually best explain outcomes of interest. In seeking to clarify critical event analysis, therefore, we do not wish to suggest that most explanations in the social sciences should take this form. We view the question of whether critical event analysis is an appropriate framework as an empirical matter: it depends on whether a critical event best explains the outcome of interest.

Token Causality

Our framework is part of a larger and ongoing scholarly effort to develop a theory of token (or actual) causality. With token causality, one explores whether a specific state of affairs is a cause of a specific outcome in a particular case. Although case study and historical researchers have made much methodological progress in recent years, they still lack a fully adequate framework for the study of token causes.

Our approach to critical event analysis is broadly consistent with regularity theories of causation. Regularity theories hold that causation between X and Y is marked by spatiotemporal contiguity, temporal succession, and constant conjunction (Mackie 1974; Wright 1985; Psillos 2002; see also Hume 1975; Mill 1911). We introduce two refinements to extend a regularity theory to the analysis of individual cases. First, the cases under analysis are almost entirely possible cases rather than actual cases. The use of possible world semantics and

counterfactual analysis allow us to gain the leverage needed to examine regularities when only one actual world case is under analysis. Second, our framework derives inferential power by analyzing the chain of events that links an initial event X to a final outcome Y. In analyzing this causal chain, however, we retain a regularity approach: each of the links in the chain is a separate relationship among events that is analyzed as a causal regularity.

“With token causality, one explores whether a specific state of affairs is a cause of a specific outcome in a particular case.”

The distribution of possible world cases across an event (X) and its logical complement or negation ($\neg X$) allows the researcher to make a statement about the probability of the event even though the researcher analyzes only one actual world case. If the researcher asserts that X is or was more likely than $\neg X$, she does so because a higher proportion of possible world cases are or were located in set X than in set $\neg X$. If she asserts that the probability of event X is 80%, she does so because 80% of all possible world cases are located in the set for event X (the remaining 20% are in $\neg X$). We use this approach in defining events, contingency, and causally importance—i.e., the constituents of a critical event.

Defining Critical Events

Events

Events are well-bounded episodes marked by the unfolding of coherent modes of activity. They serve as temporal and substantive anchors for organizing the analysis of a case study. A well-defined event is temporally bounded; that is, it has a crisp beginning and end. The temporal boundedness of events facilitates their

use in causal analysis. In addition, a well-defined event calls attention to its specific components—what we call its sufficiency properties and its necessity properties.

First, events play a productive and generative role in actively bringing about an outcome (cf. Lewis 2000; McDermott 2002). We call this aspect of an event its sufficiency properties. Events produce outcomes by generating a distinctive process that culminates in the outcome of interest. Second, events play a permissive and enabling role in allowing for the occurrence of an outcome. We call this aspect of an event its necessity properties. Events enable and permit outcomes by influencing the context or circumstances in which the outcome occurs. To identify necessity properties, therefore, the analyst can consider those aspects of the event whose counterfactual negation would (or would likely) cause the negation of the outcome of interest.

In sum, when formulating hypotheses about the sufficiency properties of an event, the challenge is identifying the productive causal chain that links the event to the outcome. When formulating hypotheses about the necessity properties of an event, the challenge is identifying the aspects of content and circumstance that the event shifts to allow for the outcome of interest.

Contingency

The concept of contingency has been used in the social sciences in different ways. However, we argue that despite differences, formulations have in common the following idea: an event is contingent when it is not expected. The source of these expectations may vary, ranging from likelihood functions to theoretical traditions, to known causal factors, to common sense intuitions. In each case, however, the contingent event is not well anticipated by the relevant model, theory, function, or belief system. In this sense, events are contingent vis-à-vis our expectations; our expectations make events contingent. It is worth noticing that this understanding of contingency is consistent with

the philosophical definition: a contingent event is neither necessary nor impossible. Contingency calls attention to the non-necessity of the occurrence of the event; a contingent event happens in some possible worlds but not in others. In turn, we propose an empirical measure of contingency that examines the distribution of possible world cases across the event and its negation. For a token event X , contingency is measured as the percentage of possible world cases in the set $\sim X$.

Causal importance

The idea that causal importance should be built into the definition of a critical event is not particularly controversial. However, case study researchers currently lack a widely shared framework for conceptualizing and measuring causal importance. We propose heuristics involving counterfactual analysis to help estimate this distribution. The heuristics require the analyst to consider possible world cases that are and are not consistent with necessity and sufficiency. Of special importance is the closest possible inconsistent case, which is the case closest to the actual world in which $X = 0$ and $Y = 1$ for the evaluation of necessity properties or $X = 1$ and $Y = 0$ for the evaluation of sufficiency properties.

With necessity properties, the analyst constructs possible worlds by considering different versions of X while holding all else constant in context and circumstance. Some versions of X require more extensive rewrites than others and thus are more distant from the actual world. Under this approach, X is more necessary for Y to the degree that the closest possible inconsistent case requires a large miracle for the occurrence of X . With sufficiency properties, by contrast, the analyst constructs possible worlds by holding X constant (identical to the actual world) and introducing via miracle changes to contingent aspects of context and circumstance. Some of these changes are more significant than others, requiring larger rewrites for their instantiation. In the approach, X is more sufficient for Y to the degree that the closest possible inconsistent case requires large

changes to contingent aspects of context and circumstance.

The relative importance of any given token cause is a function of its necessity properties and its sufficiency properties. Any token cause will become more important as it comes closer to being both necessary and sufficient for the outcome of interest. For our purposes, a core issue concerns the threshold of necessity and sufficiency required for causal importance and thus a critical event. In addressing this issue, we give identical weight to necessity and sufficiency, privileging neither. We require that a critical event meet minimal thresholds on the dimensions of logical importance and logical relevance. To measure logical importance, we consider the extent to which a condition is necessary or sufficient for an outcome. To qualify as a critical event, we propose that a condition must be substantially higher than 50% on either the necessity dimension or the sufficiency dimension. For logical relevance, we measure the extent to which a condition is necessary and sufficient for an outcome. To qualify as a critical event, we propose that a condition must be substantially higher than 0% for both the necessity and the sufficiency dimensions. The first threshold of logical importance has the effect of ensuring that all critical events are conditions that approximate necessary, sufficient, or necessary and sufficient conditions. The second threshold of logical relevance has the effect of ensuring that no trivial conditions are included as critical events.

Discussion

Defined by both causal importance and contingency, a critical event allows for a parsimonious explanation of a puzzling outcome. The appeal of this mode of explanation is partly linked to its causal austerity. But its attraction is also related to the idea that an event that was not predetermined decisively shapes the future of a case. With critical event analysis, the counterfactual question of what would have happened - what could have happened - is spotlighted and given

center stage. Critical event explanation accords with self-understandings of one's own personal development: we often explain our own trajectory using critical event analysis. Given these appealing features, historical and case study researchers will surely continue to search for critical events in their explanations of puzzling outcomes.

Yet our goal has not been to suggest that all or even most case study explanations should or will be able to identify critical events. In fact, in other modes of explanation, non-critical events are key to understand puzzling outcomes. The category of non-critical event can be partitioned into three kinds of events: events that are contingent but not causally important; events that are causally important but not contingent; and events that are neither contingent nor causally important.

First, some events are contingent but not causally important. This is the case if their occurrence of is theoretically surprising but does not play a major role in enabling or generating an outcome. These events may appear as pieces of noise that shape an outcome in small, unpredictable, and inconsistent ways. In general, they are assumed to be too random and inconsequential to merit sustained attention. Another important set of events, are those that are important causes that are not contingent. These events differ from critical events in that their occurrence is anticipated by and encapsulated within theoretical orientations. When explaining contingent outcomes, these non-contingent, important causes are not featured in the explanation because they are logically impossible: an individually important cause of a contingent outcome must itself be contingent.

Finally, some events are neither causally important nor contingent. These events may help set the general context against which important causal processes unfold. They may also contain within them important events; or they may be overarching events within which

important sub- events are located. Usually, however, events that are neither causally important nor contingent refer to trivial and expected occurrences that are ignored or taken for granted in case study explanation. A major exception exists in which unimportant, non-contingent events do play the leading explanatory role: gradualist explanations of contingent outcomes. With a gradualist explanation, many small, unexceptional causes push in a consistent direction to enable and produce a puzzling outcome (cf. Thelen 1999; 2003; 2004; Streeck and Thelen 2005; Mahoney and Thelen 2010).

In one sense, critical event analysis and gradualist analysis are rival frameworks that compete with one another. For any puzzlingly outcome in a particular case, one can pose the question of whether critical event analysis or gradualist analysis better explains the outcome. Likewise, if a scholar asserts that a critical event explains in substantial measure an outcome, one can always explore the rival hypothesis that a gradual process actually explains the outcome (or vice versa). In another sense, however, the two modes complement one another rather than compete. On this view, some contingent outcomes are best explained by critical events, whereas others are best explained by gradualist processes of change. The two frameworks may both be useful for case study research, though not simultaneously useful for a specific outcome in a particular case. This complementarity view suggests an important research agenda focused on identifying when and why a given framework is more useful for explaining a given outcome. It is an open empirical question regarding the frequency of punctuated versus gradual change in the social and political world. And it is an open question regarding the kinds of puzzlingly outcomes that are best explained by gradual processes of change versus critical events.

Going forward, the framework of token causation developed in this paper could offer a common vocabulary and a common orientation

for synthesizing work on critical events and work on gradual change. The point of such a synthesis would not be to dissolve critical event analysis and gradualist explanation into a compromise framework in which all change follows an intermediate pace. Rather, the goal must be to arrive at a satisfactory solution for understanding when and why one kind of change prevails rather than the other. By beginning to clarify the critical event side of this possible synthesis, we hope that this paper encourages new methodological work by scholars interested in the other gradualist side.

“the goal must be to arrive at a satisfactory solution for understanding when and why one kind of change prevails rather than the other.”

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Discussion

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We have four outstanding papers here, two tackling archival issues and two tackling causal issues. Anna Skarpelis and Armando Lara-Millan, Brian Sargent, and Sunmin Kim are all advocating for a more thoughtful approach to archival research. Interestingly, Skarpelis suggests stepping outside of the archive to map its structure whereas Lara Millan, Sargent, and Kim suggest immersing and indeed losing oneself in the archives in a sort of directionless way to pull out some of its hidden structures. Similarly, Laura García-Montoya & Jim Mahoney are in tension with Isaac Reed & Paul Lichterman, where one team calls for focus on events and the other on dissolving events into linked chains of meaning and conduct. Both, however, converge in situating their conversation around counterfactual causal reasoning and in focusing on the drivers of action rather than the outcomes, which I think is interesting as well as potentially problematic.

Let me begin with the archivists. Skarpelis has a very interesting and eminently readable paper. She refers to the subfield of “computational historical sociology”, which I’m going to say is possibly overstating things but I am happy to see this term in circulation and hope it gains traction. She makes many excellent points, but there are three areas where I would raise questions.

There is a potential conflation here between the archives and the state, which makes for a particularly dark view of archives. Anna’s cases are two fascist states, so I can see how she arrived here. But many archives are not state products. Company archives, merchant archives, market records, letters, museum collections, the list goes on and on. In many of these the structural is incidental to what the people were trying to accomplish, accidental and much less insidious than the power of records of activity in a fascist state. But also very important. Second, Skarpelis writes of the inevitability of the archival process turning a person into record. Again, I will say this is not the case in many non-state archives, which record transactions. Indeed, there is an argument to be made that most archives record interactions and transactions, not people. But again, this may be about different types of archives. And third, there is Borgesian tension in the argument, which calls for a careful reconstruction of the history of archives in order to fully understand their documents. Essentially you need an archive for the archive. To me this suggests not that this is an impossible task, but that the best way to conduct historical research is to combine archives. I wonder if Skarpelis would agree that this is consistent with her argument and an effective approach.

A different tack is suggested in Lara-Millan, Sargent, and Kim’s thoughtful paper about the ethnographic imagination in archival research. I like the idea of losing oneself in the archives, and agree alongside luminaries like Charles Peirce and Guy Debord, that this is an intuition building strategy. But there is a danger here. Is

a detailed description of events always good? Do we always want to see from the eyes of one person. What we might lose is the systematic and comparative perspective. We are scholars, and I think the end goal is not only to understand why people made the choices they did. It is also to understand how certain outcomes were produced. We want to know more than the people. We want to see for example how opportunities constrain action, how interdependencies in decision-making produce unexpected outcomes, and how long-term structural processes can explode into visibility in sudden events.

“I think the end goal is not only to understand why people made the choices they did. It is also to understand how certain outcomes were produced.”

Garcia-Montoya & Mahoney share with Lara-Millan, Sargent, and Kim. a focus on contingency, so let me then ask both what if these contingent events are just moments of visibility for temporally unfolding structural conditions, in the way that revolution can be tied to demographic change? I also worry that the emphasis on truly contingent events may give you the least predictive power of all approaches. If an event is truly unusual, it is not likely to occur again, so then our theories aren't very powerful.

But accepting that there is a benefit to identifying these significant events. I think there still maybe issues that are difficult to work out. What parts of an event are important? How you do case an event? Does it matter that there is an election or is it more important that the election was between Bush and Gore? If it is about estimating the causal effect of Bush and Gore running against each other with Ralph Nader as

a 3rd party contender, then what is the value of that for understanding future elections. I am also very unsure as to how I can estimate the probability of events that don't happen. And finally, I wonder about the choice of elections as an example. Elections are a highly structured moment of contingency. They are designed that way. How useful is this approach for less structured and contingent moments?

Isaac Reed and Paul Lichterman are also concerned with causal analysis and make a compelling case for investigating “actively constructed cases to compare different chains of meaningful action that theoretical categories make comparable.” The idea here is to focus on similar patterns of conduct and their outcomes rather than picking chains of action that are tied to some particular outcome. My first question, is how different is this from that refrain we all got drilled into us in graduate school: don't select on the independent variable. Does study of collective action accomplish this approach to situated chains of meaning and conduct? It seems to satisfy some of the conditions.

The paper raises very serious and under-thought issues: Can settings be variables? Can structures be agents? Are forms different from contents? And importantly, are we doing damage to our explanations by swapping out these fundamentally different types of objects and inserting them into the same causal models. It is terrific to progress being made on these issues. But I wonder if meta communication is going to provide an answer. In the end metacommunication in the sciences failed Du Bois, but sustained Dewey and James. How can we make it better?

In conclusion, while I think this each of these papers makes important contributions, the common thread running throughout may be an animosity to outcomes and an emphasis on contingency. I find this concerning. Now more than ever, don't we want research that helps us solve particular problems?

Franz Kafka, Sociologist of Domination

Richard Lachmann



Kafka, Angry Poet ([2011] 2015), Pascale Casanova's final book (she died in September 2018), offers an innovative and insightful reading of Kafka's literary work and of his place in early twentieth century Czech, German, and Jewish intellectual debates. However, Casanova does more, and what she does deserves attention from sociologists concerned with understanding the dynamics of domination in all its forms.

Casanova frames her book with a dissection of the ways in which the literary and political space that Kafka occupied was marginalized and dominated. Kafka's solution, which he developed in his novels, short stories, and other writings, obviously allowed him to transcend those restrictions to become one of the most innovative and influential writers of the twentieth century. Casanova challenges the existing interpretations of Kafka: internalist ones that ignore historical and social context, biographical and psychological readings that focus on Kafka's individual experiences and emotions, metaphysical and religious readings, "ones that foreground his Jewish identity," and "post-modern readings (for which the meaning of Kafka's writings is undecidable" (p. 4). While Casanova also sees herself as rejecting

"historical and sociopolitical readings," (p. 4) what she actually does is to develop a more sophisticated historical analysis that is at once broader and more precise than past efforts to place Kafka within time and space. In so doing, she employs her theory of the dynamics of cultural inequality to find within Kafka an analysis of domination that transcended the circumstances of his era and social position.

Casanova begins by locating Kafka in the particular literary and political space he occupied during his years as a writer in the first quarter of the twentieth century. As a Jew living in Prague, during a period when it still was part of the Austro-Hungarian Empire, Kafka was marginal to both the blossoming Czech nationalist movement and to the German literary world, which became increasingly nationalistic and intolerant of Jews. In any case, all the German writers in Prague lived in a backwater relative to Vienna, the capital of the Austro-Hungarian Empire, which itself was a backwater compared to Berlin, the dominant center of German culture and on its way (before it was permanently marginalized in the Nazi era) to rivaling Paris and London as a world cultural center.

The Jewish and Czech writers in Prague faced, albeit on different terrains, what Casanova calls the dilemma of the small nation. They came from a place without an extensive recognized body of literature. As Casanova showed in her earlier and widely influential book, *The World Republic of Letters* ([1999] 2007), first written as a dissertation under Pierre Bourdieu's supervision, writers from cultural peripheries most often seek to contribute to the building of a national literature. They followed the prescription developed in the eighteenth century

by Johann Gottfried von Herder who argued that new nations-- which were to be defined by the “necessary bonds between nation, language and people” (p. 59)-- could build literary greatness, even in the absence of an ancient tradition (which favored Greek and Latin and then French, English, and German) by drawing on “folk tales, songs, legends, proverbs, and epics—the supposed literary emanations and objectivizations of the peculiar spirit of that people,” expressed of course in a distinct national language (p 49). Herder’s prescription, which Casanova showed was highly influential in shaping writers’ ambitions and careers, and even more in determining the reception of their work, was most easily adopted by writers who were part of a national majority in formation. Such writers produced works that were stylistically conservative and often directly engaged with domestic politics. Such literature, though, has little influence beyond its borders because it “had no autonomous existence...but [was] the emanation of a people” (p. 60). (I present a longer overview of *The World Republic of Letters* in Lachmann 2010, pp. 92-95.)

Writers who were minorities within nations faced different limits and choices. German speaking Bohemian Jews, like Kafka, were marginalized in multiple ways. First they were seen as part of the oppressive German-speaking Austrian elite even as Christian German nationalist writers excluded them. Second, they didn’t have their own national language, a fate that also befell Jews everywhere in Western Europe. Third, as Jews, they were subject to repeated outbreaks of anti-Semitism, which escalated as Czech nationalism gained strength. Most searing for Kafka’s generation was the Hilsner Affair. Leopold Hilsner, a Bohemian Jew was accused of blood libel following the discovery in 1899 of the murdered body of a young girl. The accusation provoked attacks on Jews and Hilsner was convicted and sentenced to death (the sentence was later commuted and at the end of World War I he was finally pardoned). Fourth, educated Jews in Prague and

in other European cities were estranged from the more religious Jews of rural Eastern Europe.

Kafka and his generation of highly educated Jews who grew up in secular homes debated whether they had a future in Prague and if so how to make careers as writers. For many, the answer was to leave: to go to Vienna or Berlin, where they could be part of a society that was wholly German. Few educated Jews in this era joined the mass of poor Eastern European Jews who migrated to America (mainly to New York).

Many Jews who remained in Central and Eastern Europe were attracted to Zionism or Bundism. These two movements had dramatically different ideas of how Jews could achieve emancipation and escape domination. Of course, in the end Zionism won out, creating its desired state in Palestine, while Bundism has disappeared. However, in the early twentieth century, Bundism seemed just as viable. Jews were gaining legal rights in much of Europe and socialist parties were winning elections, first in Germany and increasingly elsewhere in Western Europe.

Zionism was a way for Jews to reject assimilation into majority European cultures by creating a new nation, with its own Hebrew language, in Palestine. For Zionists, literature could no longer be apolitical and autonomous (i.e., just expressing an author’s personal sensibilities and aesthetic choices); it had to engage with the project of Jewish nation building. Bundists, by contrast, sought to build a transnational Marxist party that would protect Jews’ civil and workers rights. Jews in the Bundist vision would gain emancipation in the diaspora through a class-based political party and unity through a common language, Yiddish, which already was spoken by most Jews in Eastern Europe. Unlike the Zionists, “Bundists were militantly anticlerical and opposed the perpetuation of religious practices” (p. 128).

In the early twentieth century Zionists and Bundists alike looked to the non-assimilated Jews of Eastern Europe as “an ideal typical form of the ‘people’, offering Western Jews a living storehouse and repertoire of Jewish cultural traditions” (p. 49). Martin Buber, the dominant Zionist intellectual, published books of Hassidic stories that like all ‘rediscoveries’ of tradition were in part made up.

When Casanova turns to Kafka himself, she presents someone who was engaged in the political struggles of his time and place. Kafka’s day job was as an official in the government agency that provided accident insurance for workers. Contrary to portrayals in many popular biographies, Kafka did not see this as a stultifying bureaucratic position. Rather, this occupation flowed from his commitment to working class politics, and he played a central role in creating a social benefit capitalist employers were required to fund.

Casanova shows how Kafka broke out of the narrow choices open to writers from “small countries” or to minorities, which Kafka was twice as a German speaker in a majority Czech city (Prague) whose majority was agitating to form a national homeland separate from the Austro-Hungarian Empire, and as a Jew. Kafka was inspired by Yiddish theater troupes that visited Prague, even though he didn’t know the language. However, the Yiddish plays and poems that most impressed and influenced him were secular and socialist, not religious, and often had been written and first produced in America. Kafka thus reversed the intellectual move developed by Buber and other Zionists, of discovering a rural, Eastern European “world which seemed to them frozen and immobilized in time and which they saw as a reservoir of rites, traditions and ancestral customs they had now forgotten as a result of the assimilation imposed by their fathers” (p. 142).

In reality, the Eastern Jews who didn’t leave for America were becoming urbanized, secular, and politicized. “Eastern Jewish intellectuals were

working...to secularize Jewish culture, to invent forms of the novel, poetry and drama that would enable them to accede to literary modernity...to ‘modernize’ their culture without denying their Jewishness” (p. 142). Kafka instead looked to the secular and political Bundists for a Judaism that could challenge domination, whether directed at Jews in particular or at a broader array of people: workers, the colonized, and oppressed minorities throughout the world. Similarly, he looked to the characteristics he saw in “little literatures” like Yiddish—their concern with ‘the people’ rather than literary history or an author’s individual prestige, their “liveliness,” and political engagement—as Kafka “prepared his own literary project...to adopt a dominated stance within a dominant literature” (p. 159).

Casanova’s mapping of Kafka’s place, and that of his fellow German-speaking Jews, in the global field of literature and in Jewish politics produces two valuable results. First, it leads Casanova to produce a new reading of Kafka’s literary works. Second, it allows her to discover Kafka’s depiction of domination, which can offer something to sociologists and others concerned with that dynamic.

Kafka’s key literary technique, his use of unreliable narrators speaking to readers in a “free indirect style” (a technique developed by Flaubert in *Madame Bovary*), although one in which the narrator often describes himself as I, allowed him to construct ambiguous narratives. In that way Kafka was, in Casanova’s analysis, able to develop a “new literary form, built onto existing [Eastern European Jewish] tradition [which] required the takeover and use of ‘popular’ genres...In this way [Kafka] can be said to have chosen to leave himself wholly out of the picture, so as to speak not of and for himself...but for ‘everyone’” (p. 225).

Casanova draws out the implications of Kafka’s method in her analysis of his story, “An Old Manuscript.” The story is set in ancient China

and told from the (unreliable) point of view of a tradesman who, as the author of this supposedly found manuscript, recounts the arrival of barbarians in an unnamed capital. The narrator's fantastical and racist description of the barbarians echoes and satirizes anti-Semitic tropes as well as white Americans' depictions of US blacks. (Kafka also drew clear parallels between Jewish and African Americans in his first novel, which he titled *The Man Who Disappeared*—later published as *Amerika*.) The tradesman's final comments are critical of the emperor and his court, in which he offered an implied but apt description of the Austrian emperor's passivity or acquiescence in anti-Semitic and other forms of mob violence and systematic discrimination in his empire. J. M. Coetzee used the same techniques to make similar political points in his 1980 novel *Waiting for the Barbarians*.

Kafka's story, "Jackals and Arabs" again is told from the point of view of an outsider, a traveler who recounts the jackals' efforts to enlist him in killing Arabs whom the jackals see as alien and unclean. Most superficially, this is a story of "two social groups at odds over a ritual law" (p. 259). It also is a satire of German anthropologists' renditions of Arab folktales that were published in Germany in the early twentieth century. Indeed, the settings of many of Kafka's stories track the journeys of German colonists and anthropologists. However, Casanova rightly asserts that this story like Kafka's other writings "does not really concern either the Jews in their relations with Germans...or indeed any identifiable nation whatsoever. 'Jackals and Arabs' can be said to describe, more broadly, the constitution of the spontaneous representations of all dominated marginal peoples whose codes of behavior are not spontaneously in accord with the dominant ones. Seen from this angle, Kafka is not producing an enchanted version of the life of dominated peoples but trying rather to provide an 'ethnological' version that can explain...the spontaneously racist vision of the traveller" (p. 259).

Casanova sees Kafka's fiction as allegory, with "hidden but intended meaning" (p. 301), not, as Walter Benjamin and Theodor Adorno thought, an indecipherable parable. *The Trial* is on one level "an almost theatrical staging of the...situation of anti-Semitic denunciation and suspicion from the standpoint of the victim" (p. 306). His short story, "In the Penal Colony," focuses on the machine, "on judicial organization and the avenues by which collective belief is perpetuated," while Kafka "concerns himself in *The Trial* with both the social workings of this machine and, most importantly, the effect of this law on the accused themselves" (p. 306).

The court that arrests and eventually executes Josef K. is unofficial yet "known to all and accepted by everyone [even as it] operates on rumour and semi-official materials, and *in camera*" (p. 307). Court decisions are never published, and K. only learns of the court's judgment when he is taken from his home to be executed. The legal process is "aimed first at producing, then maintaining all possible forms of social humiliation" (p. 308). The initial arrest divided K. and all the other defendants from the rest of society, subjecting them to endless humiliation that eventually makes it impossible for them to pursue their careers or interact normally with others in society who are not defendants.

Drawing on Bourdieu's distinction between shame and guilt, Casanova argues that K. is shamed by the court and by ordinary people's reactions to his condition. Contrary to various psychoanalytic interpretations of the novel, at no point in the novel does K. express guilt over a crime that is never specified and for which he repeatedly asserts his innocence. The last line of the novel makes this explicit: "it seemed as if his shame would live on after him" (p. 319). Throughout the novel, K.'s encounters with the court take place before audiences (his neighbors, the large crowds hanging out around the court and attending hearings [scenes that are vividly shown in Orson Welles' film version of

The Trial)). Spectators and their contemptuous reactions to K. and his situation produce shame. They deploy, to use the contrast Bourdieu developed and Casanova cites, symbolic rather than physical violence. Casanova notes that women are shamed throughout the novel and makes a convincing case for a proto-feminist (or at least highly observant) Kafka.

While Casanova makes use of Bourdieu's concepts in her analysis of Kafka, she makes a strong case that Kafka goes beyond Bourdieu by showing resistance to domination and shaming in much of his fiction. Kafka, through innovative literary techniques, is able to model resistance at the same time as he depicts domination and its reproduction. Casanova argues that she was able to find this element in Kafka only after she located him in the particular debates of Jewish intellectuals in early twentieth century Prague. However, while Kafka was engaged in his moment on a particular terrain, he drew parallels within and across works among different sorts of domination. He implicitly compared the conversion of African youths to Christianity with Jewish assimilationism. "In the Penal Colony," set in a nameless French colony, "makes the connection...between invisible situations of political oppression—such as those affected the Jews of Eastern Europe—and colonial situations" (p. 337). Like K., the condemned man in the penal colony never knows he has been condemned until his gruesome execution.

What can we as comparative-historical sociologists learn from Casanova and Kafka? From Casanova, we get a methodological model of how to excavate historical subjects' temporal and social location so that we can understand how they comprehend and react to their condition of domination (or, if rulers, to the specific sources of their power). Casanova shows how Kafka's anger became invisible when the specific world in which he wrote and upon which he commented disappeared. Only by recovering the political disputes of his time

and place can we make sense of the literary 'weapons of the weak' that Kafka developed.

Casanova's message is directed not just at analysts like us but also at the oppressed. She argues that understanding one's situation is the basis of resistance, and understanding needs to be both specific to one's situation and capable of finding parallels to other dominated peoples as Kafka succeeded in doing. Kafka offers tools that differ from the ones that Bourdieu or most of us employ. If "the greatest obstacle to freedom lies in submission to authority, in the symbolic potency of power, in the most dominated themselves internalizing a belief in the necessity of obedience to authority which consequently has no need to impose itself by force" (pp. 324-25), then the dominated and we as analysts can undermine that authority using the tools that Kafka employs in his fiction: using unreliable narrators to expose cant and the cruelty of power, and to show the differing yet parallel ways in which domination transforms the oppressors and the oppressed. Kafka's place on the periphery of the literary world gave him an opening to innovate that elevated him to the apex of twentieth century literature. Those tools can be used by the oppressed in other circumstances. Casanova rightly acknowledges, "In no way did [Kafka] harbor the illusion that he would win out" (p. 366). Neither should we as academics think that we will win out. However, radical honesty and intellectual innovation are their own victories, ones not available to oppressors.

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Legitimacy, Populism, and Representative Politics

Social Science History 2018 Panel



One of the presidential panels of Social Science History Association's 2018 Annual Meeting in Phoenix, Arizona addressed the related issues of legitimacy, populism, and representational politics. Chaired by Richard Lachmann (University at Albany), the panel brought together interesting work by four emerging scholars, two assistant professors and two graduate students. Below is a summary by Barış Büyükokutan (Koç University & *Trajectories* co-editor), Richard Lachmann, and Matty Lichtenstein (UC Berkeley).

In "The Knowledge Trap: Turkey's Buddha Cult and the Eclipse of Populist Power," Barış Büyükokutan took up the question of how to deal with populism in polities where it has already attained power. Specifically, Büyükokutan asked how populist parties are typically, but not always, able to stoke mass resentment against cultural elites and harness that resentment to their permanent campaigns for reelection. Focusing on three recent battles in Turkey's culture war, he found that the

populist Justice and Development Party (JDP) failed to demonize secularized Turkish middle classes' interest in eastern spirituality as an affront to Islam and Turkishness in spite of a suitable conjuncture in the fall of 2017. Büyükokutan explained this momentary "eclipse of populist hegemony" with reference to the actions of the group that the JDP sought to demonize. In online debates concerning arcane cultural items such as eastern spirituality, university-educated Turks are, he showed, usually very quick to label JDP supporters as ignorant hacks. While that move serves to reproduce antipopulists' cultural capital, it also reinforces the sense of victimization across the aisle and unwittingly pushes nonelite voters to JDP. These did not happen in the debate on eastern spirituality since, Büyükokutan showed, middle class Turks did not take the bait. Unconnected to any real experts on the matter due to the peripheral status of Turkey in East Asian migration networks, they felt little confidence in asserting the truth of the matter. Simple nonresponse thus saved the day.

In “Institutionalizing Personhood: Child Welfare Agencies and the Regulation of Perinatal Care,” Matty Lichtenstein asked why a public health issue, namely drug and alcohol dependency in pregnant and postpartum women and their children, has increasingly fallen under the jurisdiction of child protection agencies in the United States. Lichtenstein presented this question as a way to open up the larger problem of the role of the state in family regulation, tracing how state intervention in American families has changed over time. More concretely, Lichtenstein asked how state-regulated professional fields manage to expand their authority over contested social realms.

Drawing on archival, legal, and survey data, Lichtenstein showed how a confluence of U.S. social policy and legal changes in the 1960s and 1970s spurred an increase in child welfare funding and regulations, shifts that gradually expanded child welfare agencies and established them as a regulatory authority over U.S. families by the 1980s. During the following decade, efforts to criminalize substance-dependent pregnant and postpartum women almost always failed. Combining research on national trends with case studies of federal and state legal development, Lichtenstein argued that once child welfare was institutionalized as a legitimate protective authority, legislatures saw these agencies as a useful protective framing for expanding regulation of pregnant and postpartum women. Through a multi-level explanation of these sociopolitical changes, she showed how the institutionalization of child welfare contributed to the development of perinatal protective policies in the United States.

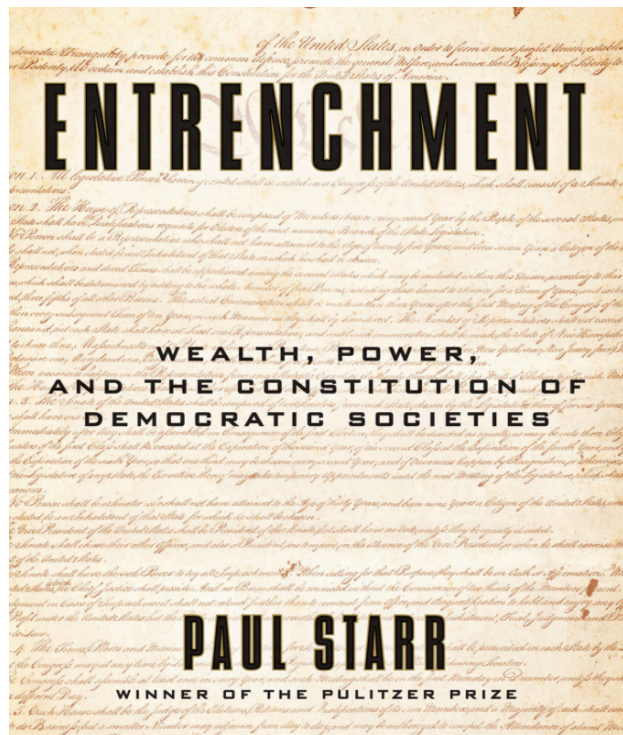
In “A Crisis of Legitimacy: Relationships of Influence in the Negotiated End of Apartheid” Eric Schoon and Robert Vandenberg (Ohio State University) examined why National Party, which ruled South Africa in the Apartheid Era, gave up power even as it retained an overwhelming military edge and enjoyed continued legitimacy among white voters.

Schoon and Vandenberg showed that the party depended on its ability to maintain diplomatic standing in the international community, bolster economic growth, and maintain an ideologically committed base. It thus required support from key allies within each of these arenas to accomplish these tasks. In the face of a global anti-apartheid movement, Schoon and Vandenberg showed, these allies faced increasing pressure from their own constituencies to support the anti-apartheid movement. White South Africans thus faced growing global isolation, prompting efforts from within the NP to end the system of minority rule.

The 1976 Soweto uprising is the key turning point in Schoon and Vandenberg’s analysis, though not for its direct results. Armed resistance to apartheid was ineffectual, but ANC, the leading anti-apartheid organization, launched decisive diplomatic and public relations efforts. These efforts were generously helped by the white government’s brutality and lack of understanding of how its escalation of the conflict would appear abroad. Most crucially, the U.S. public was revolted by massacres, police killings, and unrestrained racism, and inspired by the ANC’s general non-violence and nobility. This culminated in Congress’s passage of the Comprehensive Anti-Apartheid Act in 1986, which blocked Reagan’s veto forced limited sanctions. This led the white business elite and academics to enter into private discussions with the ANC, eventually forcing the government to join in and reach a settlement.

Taken together, these three papers underscore the contingent nature of cultural power—they point toward the numerous and changing hurdles that elites must clear in order to convert ideas to sanctions and thus remain in charge. While elites usually pull this feat off, they may not always do so, making any settlement tentative.

Recently Published Books



Entrenchment: Wealth, Power, and the Constitution of Democratic Societies

Paul Starr

Yale University Press, 2019

Much of our politics today, Paul Starr writes, is a struggle over entrenchment—efforts to bring about change in ways that opponents will find difficult to undo. That is why the stakes of contemporary politics are so high. In this wide-ranging book, Starr examines how changes at the foundations of society become hard to reverse—yet sometimes are overturned. Overcoming aristocratic power was the formative problem for eighteenth-century revolutions. Overcoming slavery was the central problem for early American democracy. Controlling the power of concentrated wealth has been an ongoing struggle in the world's capitalist democracies. The battles continue today in the troubled democracies of our time,

with the rise of both oligarchy and populist nationalism and the danger that illiberal forces will entrench themselves in power. *Entrenchment* raises fundamental questions about the origins of our institutions and urgent questions about the future.

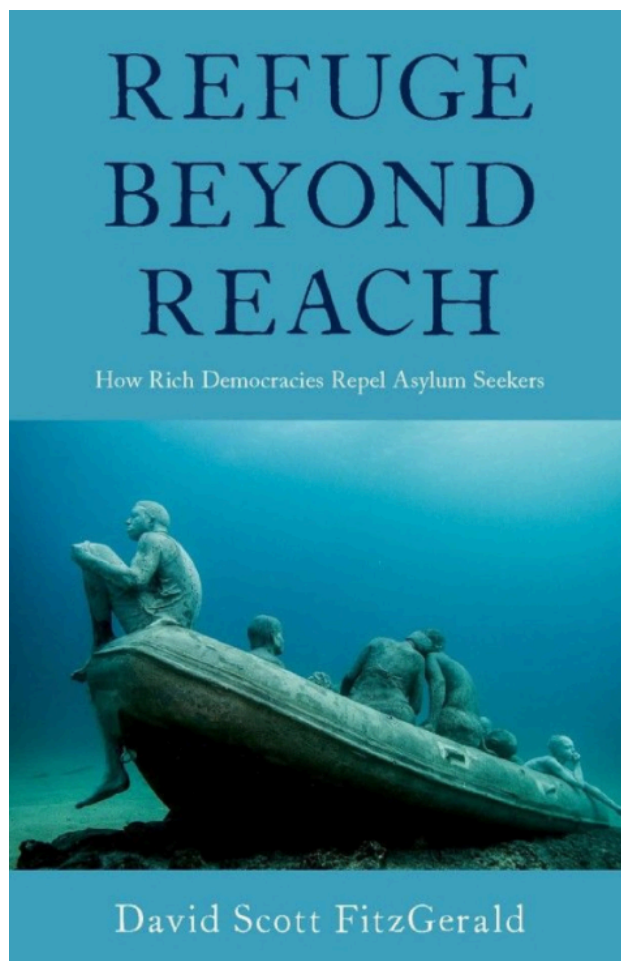
Reviews

“With the depth of historic and political insight he is celebrated for, Paul Starr gives us a new and crucial lens through which to view what is happening in America—the entrenchment of great wealth through political power strong enough to lock in that wealth, and, hence, perpetuate its power. Starr’s great achievement is to view the struggle of our era not in narrow partisan terms, but through the framework of increasingly concentrated power, and to reveal the multiple ways it is changing the very definition of American society. A tour de force.”—Robert B. Reich, author of *The Common Good* and *Saving Capitalism*

“An original framework which casts new light on America’s political and constitutional predicaments in the age of Trump.”—Bruce Ackerman, author of *Revolutionary Constitutions: Charismatic Leadership and the Rule of Law*

About the Author

Paul Starr is professor of sociology and public affairs at Princeton University, cofounder and founding co-editor of *The American Prospect* magazine and winner of the Pulitzer Prize for General Nonfiction and the Bancroft Prize in American History. He has published seven previous books including *The Social Transformation of American Medicine*, *The Creation of the Media*, *Freedom’s Power*, and *Remedy and Reaction*.



Refuge Beyond Reach: How Rich Democracies Repel Asylum Seekers

David Scott FitzGerald

Oxford University Press, 2019

In *Refuge beyond Reach*, David Scott FitzGerald traces how rich democracies have deliberately and systematically shut down most legal paths to safety. Drawing on official government documents, information obtained via WikiLeaks, and interviews with asylum seekers, he finds that for ninety-nine percent of refugees, the only way to find safety in one of the prosperous democracies of the Global North is to reach its territory and then ask for asylum. FitzGerald shows how the US, Canada, Europe, and Australia comply with the letter of the law while violating the spirit of those laws through

a range of deterrence methods - first designed to keep out Jews fleeing the Nazis - that have now evolved into a pervasive global system of "remote control." While some of the most draconian remote control practices continue in secret, FitzGerald identifies some pressure points and finds that a diffuse humanitarian obligation to help those in need is more difficult for governments to evade than the law alone.

Reviews

"A powerful and disturbing account"—T. Alexander Aleinikoff, Professor and Director of the Zolberg Institute on Migration and Mobility, The New School, former UN Deputy High Commissioner for Refugees

"an important book for our time and age"—Jan Egeland, Secretary General, Norwegian Refugee Council, former UN Undersecretary General for Humanitarian Affairs

"An eye-opener and useful reference for scholars and practitioners alike"—Charmain Mohamed, Head of Refugee and Migrant Rights, Amnesty International

About the Author

David Scott FitzGerald is Theodore E. Gildred Chair in U.S.-Mexican Relations, Professor of Sociology, and Co-Director of the Center for Comparative Immigration Studies at the University of California, San Diego. His research analyzes policies regulating migration and asylum in countries of origin, transit, and destination. FitzGerald's books include *Culling the Masses: The Democratic Origins of Racist Immigration Policy in the Americas*, which won the American Sociological Association's Distinguished Scholarly Book Award, and *A Nation of Emigrants: How Mexico Manages its Migration*.