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Theorizing with Archives: Contingency, Mistakes, and Plausible Alternatives

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Abstract
What are "good" kinds of archival evidence for theorizing? Surprisingly, the word archive and discussions of the archival process rarely appear in methods textbooks or discussions of methods in historical sociology. Yet, much recent historicized sociology relies upon documents left over by a small group of actors to make large-scale claims. To address this oversight, we leverage the evidentiary strengths of qualitative sociology and translate them for historical sociology. Our central argument is that three kinds of archival evidence are likely to produce generalizable claims: positive contingency, learning by mistakes, and plausible alternatives. Examples are illustrated with the cases of jail overcrowding in Los Angeles County, anti-redlining policy in the Federal Reserve, and immigration policy in the Dillingham commission.

Keywords Archive · Ethnography · Theory · Method · Historical sociology

While reviewing the ongoing intellectual shifts in historical sociology, Clemens (2007) uses the term "historicized sociology" to describe an emerging trend. The new scholarship, Clemens notes, tends to perceive history primarily as a continual process of ordering and

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reordering. Because what we perceive to be historical outcomes are usually just snapshots of this continuum, the more important exercise becomes documenting how outcomes consolidate. This shift in focus leads to "a distinctive style of finding form in the flow of events" as well as a new framework of "defining what constitutes an interesting problem" (Clemens 2007, 533).

While Clemens and others (see Ermakoff 2019) are right to note that process tracing and an emphasis on emergence have become hallmarks of historicized sociology, in her article and others, there is little discussion of a fundamental component of historicized research: the archive.[1] In fact, the word archive and discussions of the archival experience rarely appear in methods textbooks (e.g., Lange 2013) or discussions of methods in historical sociology (Adams et al. 2005; Clemens 2007; Mahoney 2004; Steinmetz 2005). Consequently, it is up to the researcher to learn how—usually through trial and error—to recognize good evidence for the kinds of claims that readers find compelling and relevant.

Knowing what kinds of archival evidence are amenable to sociological theorizing is a difficult question. First, there are problems with the documents. When a researcher goes into an archive seeking to understand how an outcome came to be, they often feel overwhelmed. There are too many documents, and those documents only contain half-truths about actors' intentions. Moreover, there seems to be an infinite number of networks among historical actors. Amidst the torrent of information and documents, researchers find themselves agonizing over how to properly develop a theoretical explanation out of reliable evidence. In the process, they go through many false starts and dead ends, before finally reaching a clear theoretical explanation. The confusion around archival evidence manifests itself on the other side of the research process as well: readers as well as reviewers of historical sociology have few guidelines for evaluating archival evidence (Mariampolski and Hughes 1978). How do we know the examples presented in a work of historical sociology are representative, credible, and persuasive enough to make theoretical claims?

Second, there is a distinct, but related problem of what kinds of external validity claims can be gleaned from case study research. Sometimes researchers lean too strongly in the direction of describing historical narratives while others veer too far in the direction of approximating other methods' notions of generalizability. Concerning the former, if we are simply describing a historical process, then what makes this research sociological and relevant to other contexts? Concerning the latter, how can research based on single case studies develop a theory that should matter elsewhere?

In order to address these questions, this article takes cues from a similar problem in ethnographic research and offers a framework for understanding what kinds of archival evidence are amenable to historicized theorizing. First, we translate a set of lessons learned in qualitative methods into a set of heuristics that can assist researchers in finding theoretically relevant archival evidence. Ethnographers have developed an understanding of the types of ethnographic scenes that make for useful generalizable causal claims (Katz 2002). Moreover, historical sociologists should, in a manner akin to an ethnographer being immersed in a new social setting, pay attention to the social worlds of historical actors they aim to study and follow them along in their journey as they move through and influence historical events (Vaughan 2004, 2016).

We leverage these evidentiary strengths of qualitative sociology and translate them for historical sociology. Our central argument is that three kinds of archival evidence are likely to provide leverage for theorizing. First, documents that represent moments of *positive*

[1] As we will discuss later, one exception is Ermakoff (2019), who offers broad advice.

contingency or moments when task-oriented groups successfully resolve mutual uncertainty. The forces that shape such resolutions clue us into the operative social forces that cause social change. Second, documents that capture the processes through which historical actors *learn by mistake*. The mistakes that historical actors make in decision-making can be more meaningful to sociologists than the eventual path taken; such mistakes highlight actors' priorities and contextualize their motivations. Finally, documents that provide a clue to *plausible alternatives*. The paths almost taken reveal much about the events necessary for causal chains.

Rather than subscribing to the mechanical understanding of causality, our approach is more amenable to what Pacewicz (2020) calls "constitutive" claims. These are external validity claims about the make-up of a phenomenon that straddle both causal and descriptive accounts. Instead of connecting existing variables to explain historical outcomes, this kind of externally relevant claim looks to identify the process through which those variables come to emerge, and how the emergence shapes the possible choices that historical actors considered in their critical junctures of action. It also shares an affinity with accounts of "formation stories" (Hirschman and Reed 2014) as well as "negative case" analyses (Emigh 1997). In total, our framework contributes to the aforementioned process-focused historicized approach by showing how one can make generalizable theoretical claims using primary documents and single cases.

We illustrate this argument through examples from our respective case studies: jail overcrowding in Los Angeles County, anti-redlining policy in the Federal Reserve, and immigration policy in the Dillingham commission. The case of jails in Los Angeles County illustrates the usefulness of positive contingency. By drawing on evidence taken from a within-case comparison of two resolutions to collective uncertainty around jail overcrowding, the researcher was able to move from a theory of mass imprisonment to a theory of crisis abatement. The case of the Federal Reserve illustrates the power of capturing how historical actors learn through their mistakes. Doing so can reveal the priorities of actors in multiple ways: it allows us to note what actors seek to save first when things go bad, and unpack knowledge production processes as actors work to construct lessons from negative events they experience. Lastly, the case of the Dillingham Commission illustrates what we can learn from plausible alternatives. By focusing on an idea that was seriously considered but ultimately discarded, we see more clearly how historical actors came to make critical decisions.

Before we begin, an important caveat. Many sociologists who have used archives will recognize what they do in what we describe. What we offer is not entirely novel but has remained mystified. Transparency in sources, rigorous case selection, and triangulation are all important principles. What is missing, however, in these principles is a specific guideline through which a researcher can access and obtain archival evidence. A more formal and explicit guideline will not only ease data analysis but also result in a more straightforward training of novice historical sociologists. Although historical sociology has been late to the conversation, the methodological and theoretical discussions on archives have been going on for decades (Agamben 2009; Foucault 1982; Garland 2014; Ginzburg 1989; Koselleck 2002; Stoler 2009), and the exchange between history and ethnography, in particular, (Dirks 2015; see also Biernacki 2012; Hunter 2013) has produced a novel understanding of the relationship between archival evidence and theory construction. By critically re-evaluating our practices as researchers, not only do we develop a reflexive insight into how our knowledge is produced, but we also gain leverage in understanding the very processes we aim to study. This is an attempt to empower those that take archival work seriously by showing that the messiness of the archive is not weaknesses to be hidden, but an epistemological asset for developing more robust theories about historical processes.

The Problem of Archives in Historicized Sociology

When sociologists use archival materials, they often face several different but related problems. First, what appear to be well contained objects of analysis can quickly proliferate into an unmanageable mess, and it is difficult for the researcher to know what is relevant to the study. When researching the problem of jail overcrowding, for instance, a researcher likely will not have the benefit of discovering a series of boxes neatly labeled "jail overcrowding." One must be able to discern what materials are relevant to the analysis of the social problem of jail overcrowding, how that problem came to be defined and constructed, and what kinds of solutions to jail overcrowding were considered and implemented. Tracing out connections between these issues and questions can be overwhelming without some signposts to make archival work more manageable.

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 paying attention. It is rare to find a smoking gun-style document that indisputably reveals the truth. Instead, each document may push the researcher forward in a small way, and collectively they can add up to something greater than their parts. How does a researcher weigh the importance of different archival documents? Or, more specifically, how does a researcher recognize a focal point through which to develop a theoretical argument?

Third, when faced with a large body of documents containing various acts, behaviors, value statements, etc., distinguishing what actors claim and what they actually believe can become difficult. That is, historical actors may openly claim that certain decisions are important, but still harbor concerns that other moments prior to those decisions were just as, if not more, consequential. Additionally, ethnographers point out that people can be duplicitous, especially when they are attempting to persuade an audience or to justify their actions (Jerolmack and Khan 2014). In reconstructing historical processes, researchers face a daunting challenge of separating what historical actors profess from what they actually do. In other words, how does a researcher "see through" particular historical documents and develop an understanding of the historical context against which to judge the validity of what appears on the archival material?

Yet, despite these problems, sociologists have used archival evidence taken from a small group of actors to construct arguments that have implications for large-scale phenomena. For instance, Prasad (2012) takes on the question of why there is more poverty in the United States compared to other countries with a similar level of prosperity. Prasad's wide-reaching (and successful) argument hinges, in large part, on the documents left by Huey Long, a depression-era politician who put agrarian populists on the national political map. "[S]tudying this particular actor and this particular moment[,]" Prasad writes, "gives us clues into why the politics of progressive taxation was so popular in mid-century America" (2012, 133). In his recent review of this scholarship, Ermakoff (2019) cites a number of important studies that rely on archival sources to produce externally relevant claims: administrative data (Gerteis 2007; Voss 1993), association membership (Riley 2005), legal records (Adams 2005, Fourcade 2011, Gould 2000), data provided by newsletters and gazetteers (Carruthers 1996; Su 2011), parliamentary debates (Dobbin 1994), published accounts and memoirs (Göçek 2015), administrative correspondence (Chibber 2003; Goldberg 2007), organizational records (Clemens 1997; Haydu 2008; Skocpol 1992), and diaries, letters, private notes (Freeland 2001).

In this review of historicized sociology (what he calls "genetic" approaches), Ermakoff (2019) offers two pieces of broad suggestions on how scholars should use archives to generate theory. First, Ermakoff (2019) suggests that because causal forces should be visible in actors'

behaviors and subjective states, scholars should trace dispositions or strategies of action over time. He elaborates only that such tracking should pay particular attention to moments of confrontation that interrupt long-term stability. Second, he offers the advice that because evidence from documents often misrepresent actors' standpoints, "analysts can seek to 'emulate historical ethnography'" by "triangulating different types of documents pertaining to the process they investigate" (Ermakoff 2019, 596).

While we are in fundamental agreement with such broad advice, we view this as only a starting point. We should be able to say something more about the kinds of evidence found in documents that can lead to better theorizing; that is, a heuristic for the specific analytical work that certain kinds of documentary evidence can provide. Qualitative sociologists have thought long and hard about such issues. What follows is an in-depth discussion of our translational intervention by connecting the ideas of Katz (2002), Vaughan (2004), among others, with approaches to claims-making central to historical sociology. We then provide three accessible examples, one for each kind of data we specify, that should assist in guiding the archival researcher. We conclude with an additional example and offer some further thoughts on the practical need for heuristics such as ours for training new historical sociologists.

From Qualitative Evidence to Historical Theorizing

Historical sociology has been primed to more fully use single cases to produce externally relevant theories through the use of "constitutive" claims (Pacewicz 2020) that highlight emergence and process. Formation stories (Hirschman and Reed 2014) and the negative case method (Emigh 1997) can be thought of as different ways for developing those kinds of claims. While this approach specifies the epistemological rules, our framework aims to develop heuristics. This framework can be especially useful by archival researchers who are faced with a question like: how should a researcher think and act when facing a trove of documents?

A good place for figuring out what constitutes the kinds of archival evidence that allow for externally relevant claims and theory building is ethnography. Although some may assume that ethnographers are strictly in the business of description, many have contemplated how descriptions of scenes can be turned into generalizable claims that matter in other contexts. We consider two critical pieces: Katz (2002) and Vaughan (2004). These two papers, which both appeared in the journal *Ethnography*, can help us piece together how archival evidence can make for convincing theories about historical cases.

Evidence in Qualitative Sociology

There is great affinity between the kinds of evidence that ethnographers capture and the kinds of evidence that historical sociologists may use when digging in the archives. Consider Becker's (1998) *Tricks of the Trade*, one of the foundational texts that ethnographers use to inculcate an appropriate disposition towards data collection. As if he was speaking of historical research, Becker (1998, 60) writes, "I wanted to know all the circumstances of an event, everything that was going on around it, everyone who was involved. I wanted to know the sequence of things, how one thing led to another, how this didn't happen until that happened." This is the "how" that ethnographers are taught to emphasize during data collection, to ask in field interviews "how" something happened, rather than "why" something happened.

Ethnographers have grappled with the problem of moving from the "how" of data collection to the theoretical claims about "why." Katz (2002) offers an important attempt to formalize the kinds of evidence that make for convincing causal claims. Although he lists seven kinds of evidence, for our purposes we will focus on four that can help us think through archives. *Varied* data are useful in suggesting that a theorization or concept can help explain a multiplicity of actions and situations in a field site. This allows a researcher to compare situations within the case, "strengthening an explanation by modifying them through encounters with negative cases" (Katz 2002, 466). For example, Lara-Millán (2014) shows that it is criminal stigma, not racialization, that drives public ER admissions. He reveals that racialization works differently for different ethnic groups (sometimes to their benefit), but what links admissions decisions are patients' distance from criminal suspicion. *Vivid* examples are scenes in which actors innovate social forms in the face of overwhelming social forces. In essence, causal forces can be illustrated with examples of people developing new practices or standpoints to solve problems that line up with those causal forces. These are convincing examples because they put the reader "there" by showing "subjects sensitively alive to the demands of the situation" (Katz 2002, 74). Data can be *revealing* when scenes show sudden shifts in emotion or experience during anxious and uncertain situations. "When social interaction leads to abrupt turning points in people's behavior, it is a good bet that the scene crystalizes… social pressures that being generally diffused through the subject's lives, are usually less readily detected" (Katz 2002, 65).

Finally, *poignant* scenes are of particular importance. Katz gives the example of Duneier's (1999) claim that men who spend time on the street are less homeless, less suffering from a culture of poverty, and more proud owners of an ethic to work and provide services. The claim is supported by, in part, one scene in which an informant makes enough money selling used books to pay for housing but tells Duneier to hold onto his money so that he does not spend it all drinking. The man thanks Duneier for resisting his later attempt to use all the money for alcohol because he now has enough money to reinvest in his informal business that requires him to sleep on the street. Such scenes rely on forbearance or humility in the face of great social pressures that we would otherwise expect to alter human behavior. It is in such poignant scenes where people respond with emotionally intensified awareness that we can see how powerful social forces play a causal role in shaping their behavior, even if such forces remain in the background of people's lives.

Vaughan (2004) makes two related points that build of her study of the Challenger disaster and figure importantly in our engagement with qualitative methods. First is the necessity of reconstructing events from disparate sources into a chronological order. She had access to thousands of documents, interviews, and reports that each, in some small way, gave meaning to each event. But each source had placed events in different orders and in different magnitudes. This meant assembling her own timeline of events and attaching the meanings and information that disparate sources gave to those events. This allowed her to reconstruct the interactional space in which those historical actors were embedded so that she could truly understand why and how they made the decisions that led to the fatal outcome (see also Gross 2018). Actors have their own version of what they thought was going on at the time that it happened, yet it is only the historical sociologist that can reconstruct that situated chronology.

Second, this allowed Vaughan to be reflexive; juxtaposing her own a priori explanations, the explanations that actors gave to themselves, and what the reconstructed chronology explained. She had begun with her own explanation that resource scarcity had caused NASA engineers to tolerate cutting corners. This juxtaposed with the state's explanation that

engineers were deviant from their own safety checking rules. The chronology offered another theory: that it was conformity to a culture of risk tolerance and compartmentalizing information about that risk that produced the disaster. When Vaughan presented her finished theory to the engineers, they were aghast to learn how each department had different viewpoints of events and their organizational structure had encouraged them to send weak signals to one another about those viewpoints.

Theorizing in Historical Sociology

Historical sociologists have had a long debate about the utility of their research for generalizable knowledge. After all, historical sociology has usually been based on so-called small-N research, and some scholars outside of the field often cast skepticism on its potential for making a broader contribution (Lieberson 1991). Initially, Mill's method of similarity and difference was central to historical sociology's attempt to create generalizable knowledge. That is, by looking for common causal factors that explain different cases or differences in similar cases with divergent outcomes, historical sociology sought to develop theories that could be exported out of their immediate context (Skocpol and Somers 1980). Although this move justified the existence of historical sociology within the discipline of sociology, it often faced criticism from a different kind of skeptic, those who took the temporal component of history seriously. The emphasis on comparison led to a mechanistic understanding of history and effective "freezing" of what should be regarded as a dynamic, creative process. In other words, historical sociology sometimes presupposed the social world as made up of fixed entities that will always affect outcomes in the same way, no matter the context (Abbott 1988).

Recently, historical sociologists are increasingly moving away from the formal aspects of Mill's methods and becoming more open to an in-depth analysis of what could be construed as a single case (Clemens 2007; Ermakoff 2019; Goertz and Mahoney 2012). As Clemens (2007) made clear with her call for "historicized" sociology, this transition is highlighting *process* that leads to an outcome over the formal logic of comparison and variables.

Recently, formation stories (Hirschman and Reed 2014) and negative case analyses (Emigh 1997; Mahoney and Goertz 2004) have attracted attention from scholars interested in this transition. Formation stories are narrative accounts of new social types, be they new groups, organizational forms, concepts, and others. What is tricky about formation stories is that they contain both accounts of the nature of newly emergent phenomenon (its characteristics) and a causal narrative about how the new phenomenon emerged. Such accounts are related, as often—but not always—new phenomena acquire characteristics and qualities due to the way that they were formed. Examples of these would be an explanation of how think tanks became separate from lobby firms and universities (Medvetz 2012) or how autism became a common diagnosis (Eyal 2013).

Another important kind of single-case study are negative case analyses, which use the surprising non-occurrence of a historical outcome as an opportunity to test and expand existing theory (Emigh 1997). For example, when we see no capitalist development in a case when all the relevant indicators are present, that is an opportunity to look for other factors that that have not been covered by the theory of capitalist development. While many see negative case analysis as an extension of variable-based comparative analysis, Emigh (1997) clarifies otherwise. Because mechanisms are contextually dependent, anomalous cases "expand a theory's range of explanation" (Emigh 1997, 649) by altering what we think that theory consists of in the first place. That is, the anomalous case does not debunk the previously

existing theory, but instead uncovers conditions of possibility that likely applied to previously examined cases. Rather than view those conditions of possibility as instances of general law, the idea is to trace how such forces manifest in different contexts.[2]

Pacewicz (2020) has recently clarified the kind of external validity claims that such historicized sociology—both "formation stories" and "negative case" studies—are making and, we argue, the kinds of claims that archival documents are best suited. He calls these "constitutive" claims or external validity claims about the makeup and useful categorization of social phenomenon. In simple terms, these are claims that "x is a thing with the following properties" (Pacewicz 2020, 9). These claims are distinct from purely causal arguments as well as mere descriptions. It is in delineating the properties of a newly emergent phenomenon—for instance, arguing that "financialization" is best understood as corporations' growing dependence on profits derived via financial mechanisms (Krippner 2005)—that work to create implied causal arguments for other cases.

Strategically describing the make-up of an object or phenomenon allows single-case researchers to make claims that become externally relevant through comparison and analogy. Constitutive claims can be thought of as generalizable because they "improve others' intuitions about the phenomenon, including but not exclusively others' causal intuitions about appropriate casing, relevance criteria, and the range of plausible mechanistic accounts" (Pacewicz 2020, 9). That is, constitutive claims go beyond the specific case they are based on by explaining how things come to be established and how they work together to result in an outcome of interest. In this manner, constitutive claims provide a means of robustly theorizing about *emergence* and *process*, two of the most prized objects of analysis in "historicized" sociology. Although constitutive claims may not develop the *exact* theory that is directly applicable to cases beyond its origin, its elucidation of emergence and process in a particular case can trigger analogical thinking (Vaughan 2014), inspiring a variation of the explanation.

The connections we have outlined between qualitative sociology and historical sociology offer an approach that allows researchers to gain greater explanatory leverage and be more productive in the archive. As we have shown, Katz' (2002) formulation resonates with what historical sociologists call "critical junctures" that are often important in formation stories. These are moments when path-dependent institutional stability and reproduction become unsettled and actors and groups show the power to remake the world in a dramatic fashion (Capoccia and Kelemen 2007). Katz (2002) points out that social forces can be shown to shape social action when examples demonstrate how actors innovate in the face of great social pressure, how their standpoints shift during situations of great uncertainty, and through examples in which actors show emotional forbearance in the face of overwhelming odds. Such highly charged situations can be akin to unsettled historical moments in which decision-makers must innovate, shift standpoints, or persist. We should look for documents that illustrate such action.

Vaughan's (2004) approach resonates with historical sociology's discussion of negative case method. By reconstructing a chronology of events with documents, we can come to understand not just the decisions that led to an outcome of our interest, but also the possible universe of decisions that were contemplated but not made. By engaging with archival material

[2] A description in Burawoy's (1998: 19) closely related "extended case method" helps to clarify this point. The goal of this kind of theorizing is less "to seek out *common patterns among diverse cases*, so that context can be discounted…" and is instead found in "*tracing the source of small difference to external forces*… Here the purpose of the comparison is to causally connect the cases. Instead of reducing cases to instances of a general law, we make each case work in its connection to other cases."

chronologically, researchers are better able to pay attention to these possible divergences: 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? Careful analysis of these differences can provide the researcher with the leverage to exclude some potential alternative explanations of the outcome of interest. Katz (2002) also uses a similar logic to explain the usefulness of varied examples that illustrate how a social force unfolds differently across different contexts.

In total, the goal of usually single case historicized research is to make externally relevant "constitutive" claims (Pacewicz 2020) through the use of formation stories (Hirschman and Reed 2014) or negative case analysis (Emigh 1997). In what follows we transition one step further by explicitly discussing the ways we can theorize in this manner with archives. We aim for externally relevant theories from single cases, not by seeking to identify a combination of variables that leads to outcomes, but rather by mapping the realm of possibility as understood by historical actors of the time.

Positive Contingency, Learning by Mistake, and Plausible Alternatives

So far we have presented the need for understanding how archives can contribute to theorizing, the way that qualitative sociologists have theorized from evidence, and the way that historical sociologist aspire to make externally relevant claims. In order to better illuminate processes of decision-making from archival materials, we must be clearer about the work that we undertake.

The ideal starting point for understanding our approach is a distinct, but related confusion: the confusion that research subjects feel about their situations and the confusion researchers feel when they are studying them. The confusion of the researcher is not just a failed attempt at discovering the "right" kind of evidence, but a valuable experience through which we can gain insights into the historical actors and processes that we aim to study (see Dirks 2015). Both parties are engaged in the process of interpretation amid confusion and imperfect information. Both attempt to develop meaning from the insignificant, order from chaos. Our proposal acknowledges that interpretation is a part of existence, but that it is also a process in and of itself. When overlaid upon a social environment where groups of actors must engage with each other, this process is rendered even more explicit. Collective processes of learning, making mistakes, and making meaning are contingent and temporally contextual. Put another way, we advocate searching for moments in which actors are doing what we are doing: interpreting their social world through a pragmatic lens.

Positive Contingency

Documents that contain information about moments of mutual uncertainty are valuable for theorizing. These are moments where actors are collectively at a loss of knowing what to do next. Documents that represent how actors figured their way out of such uncertainty are better kinds of archival evidence. These are instances that Goffman (1982) called "where the action is." Moments when individuals risk something, have no idea what the outcome of the interaction will be, and come to innovate practices and cultural forms.

In his analysis of the French National Assembly on August 4th, 1789, Ermakoff (2015) gives us a clue to what these might look like in the archival record. For Ermakoff, there is a distinction between causally meaningful "positive contingency" and moments of pure chance:

under positive contingency, "individual agency" can have "big effects," and a small number of people can create conditions that affect a large number of people. Ermakoff also places emphasis on the emotion of the moment: in uncertain situations, actors tend to "search for behavioral cues from peers" and maintain a "wait-and-see attitude" while seeking to "align with a collective stance." These "expressions of ambivalence and surprise at one's own action provide post factum clues" to understanding positive contingency and their implications for a historical outcome (Ermakoff 2015, 67). The goal of the sociologist in the archive is to focus on the documents that capture positive contingency—an account of how a group made their way out of mutual uncertainty through interactions among them, creating conditions that had implications going far beyond the group's boundaries.

The following example of the usefulness of such moments is taken from one of the author's research, a historical study of Los Angeles County jails between 1978 and 2015. The research began with a theory of "mass imprisonment," that changes in the size of the jail system were due to officials and the public's desire to warehouse as many people thought likely to commit crimes: young men of color. By honing in on moments of positive contingency, the researcher ended up instead with a theory of crisis abatement. That jail officials were less concerned with locking up poor people of color and more concerned with resolving resource and legitimacy crises that threatened their system—even if that mean releasing prisoners.

A comparison was made between two critical junctures in the jail system: an overcrowding crisis from 1992–1997 and 2004–2007. These two crisis periods were characterized by situations in which budgets prevented officials from expanding jail space and legal challenges that kept them from packing as many inmates as possible into existing space. There were jail riots, jail murders, protests, media exposés, and much consternation among politicians, bureaucrats, and prisoners' rights advocates. With a need to alleviate overcrowding and no money to do so, officials were caught between a rock and hard place. Public officials were at a loss on appropriate courses of action; that is, they faced mutual uncertainty.

At the end of both crises it seemed as if local officials had simply raised revenue and created jail space to diffuse the inmate population. Such a reading of the history would substantiate a theory of mass imprisonment; that jail officials simply found revenue and grew in response to their crises. However, if we unpack these two crisis periods, with an eye towards moments of positive contingency, the within-case comparison can generate a more useful theory about how crisis transforms to policy resolutions.

In the first period, the County was able to resolve the crisis by releasing 4000 inmates into the community and contracting out the subsequently empty bed space to the prison system of the State of California. To be clear, this meant that the local jail system released its own inmates and replaced them with inmates from another jurisdiction in order to earn a profit. The contract required much negotiation as County officials had to convince State officials to pay them a high rate for the bed space and the move had to be constructed in such a way that was legal.

The most important uncertainty was the difference between State inmates that were always held at the County level and these new transfers that would generate a profit. Counties always had a responsibility to temporarily hold a certain kind of State inmates, called parole revocations. When a parolee, who is an individual released from State incarceration under parole supervision, is thought to have violated the conditions of their parole, they are sent to their county jail to await hearings. Counties were compensated for this expense under what was called "daily assistance." The distinction between this traditional group of inmates and those the State was suddenly willing to pay for in 1997 was only relevant as a bureaucratic

fact. While this may seem mystifying, the confusion is well founded, as a sheriff official stated at the time: "…I don't have control of who's on the contract and who's not. I don't even know that information, frankly…"[3]

In the second period, the County had to appease legal concerns over jail overcrowding, once again. They reasoned that ending the contract with the State and kicking out State inmates in their jail would allow them to expand space for local inmates. However, as was the case in 1997, the 2006 moment was very uncertain: County officials feared that if the contract was canceled, the State might not take its prisoners back, as the State suffered from its own prison overcrowding. This fear was repeated *ad nausea*, as one top sheriff official said: "…We void the contract, I don't know that there's any guarantee that the State would come pick those prisoners up."[4] Through much negotiation and fact finding, County officials ultimately came to recognize their solution. They learned that while the State would indeed take its inmates back, as a result, State prison officials would release an equal amount of their own prisoners into the community from elsewhere in their system. Here is the moment this fact was discovered by County officials in 2006:

> Sheriff Lee Baca: As indicated months back, the CEO and I were somewhat worried that, if we sent them back, we eliminate the money resource that they'd compel us to keep the inmates. I'm pleased to announce to you today, *and I just heard this today*, that, in checking with the California department of corrections, they know that they're obligated to take those inmates out of our system. So this is a new piece of information that I learned a half hour ago before we began…
>
> Sup. Yaroslavsky: …it wasn't going to work that well because there were two kinds of prisoners, as I recall, State prisoners that we have, some that we are obligated to have, and some that we are taking, but that they have no place to take back. We could end up having those very prisoners without the money…
>
> Marc Klugman (Sheriff official): I'd like to respond a little bit. I talked to the State. Their position is that these 1,292 beds are part of their bed count per the contract. If they lose the contract, they lose the 1,292 beds. That means that the 1,292 parole violators in the system would be removed…
>
> Sup. Yaroslavsky: To where? To where?
>
> Marc Klugman (Sheriff official): To State prison. And then the following statement was made to me that *they would then parole back to Los Angeles County a larger number of State inmates who are convicted felons…*
>
> Sup. Yaroslavsky: They would be paroled on the street?
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> CEO. Janssen: They'll have their own early release.
>
> Sup. Yaroslavsky: So don't sweat the early release. This is an old George Carlin routine, don't sweat the thunderstorm.[5]

[3] 3/21/2006, Los Angeles County Board of Supervisors Meeting Transcript [here after date reported].
[4] 3/21/2006. County officials debated whether to terminate the contract in five meetings between 2/14/2006 and 8/1/2006; especially 2/21/06.
[5] 7/11/2006. Another similar exchanged occurred on 8/1/2006.

In this exchange, we can see the contingent quality of the 2006 moment: the uncertainty of whether the State would take these inmates back, the subsequent negotiation with State officials, and the realization that cancellation would cause inmates to be released into the community. An illusion is produced in which the County gets to claim that it is expanding its jail system by kicking out state inmates, not lose any money by doing so, but, in reality, other inmates are simply being released into the community. In one crisis situation, that of 1997, they were able release their own inmates and replace them with State inmates and generate revenue from the exchange. In 2006, conversely, they were able to kick out the State inmates (who were later released by the State) and replace them with their own inmates. County officials realized that these were really just the same group of individuals and that crisis abatement was more important than imprisonment.

The sociologists in the archive can look for moments like these in which unsettledness transforms into settledness. These are moments when the normal routines and ways of doing things become interrupted and special coordination is required to resolve the interruption. Without routines to fall back on, actors deliberate over what to do next. It is in these moments that we find situated meaning-making or those acts that best represent what the actors in our cases value most, such as how they think about their problems and resources, and what their true goals are. In contrast to documents that represent statements from officials during settled times or when nothing is at stake, documents that give us information about moments of positive contingency represent some of the richest kinds of archival evidence. This way of thinking about archives should be used when collecting documents, discerning which documents to present to readers as examples, and judging the quality of publications that use archival evidence.

Learning by Mistake

The researcher's first attempt to answer the questions that motivated their work, through an organizational ethnography of a regulatory agency, was stymied by political drama. Unexpected Senate confirmation hearings suddenly cast a harsh spotlight on the acting head of the organization. The researcher's presence went from tolerable and sometimes useful curiosity to political liability, and they were gently asked to find some other place to capture everyone's word and deed. Undaunted and in need of an actual dissertation to finish, the researcher turned towards an archival approach under the hope that inanimate paper would be less likely to change its mind. The researcher headed to the archives of the Federal Reserve Bank of New York (FRBNY), which housed a large and systematically organized archive of the Reserve Bank. Given the centralized role that the FRBNY played in banking regulation within the Federal Reserve System, it could provide unique insight into the whole system's approach to regulatory matters. But when suddenly faced with a glut of documents and limited time, how could the researcher know where to look to address their initial site of research questions? What were these documents actually saying and how to bring them to life? Through trial and error, the researcher ultimately decided to focus on critical moments that were easily recognizable: moments when actors acknowledged their own mistakes.

Mistakes and iteration are often richer sources of social significance than epiphany: the emphasis on the moment of reckoning obscures the question of context in favor of individual cognitive processes—precisely the move that sociologists commonly try to avoid. How historical actors responded to mistakes provides information on why actors made them in the first place and why they sometimes repeat them. As actors attempt to find clarity from their

confusion and fix their mistakes, a sociologist immersed in the archive will be able to view this process as one of learning and knowledge production. It becomes possible to pose the question: why is *this* the takeaway from a particular experience and what does it mean? Why do historical actors recognize a problem in a particular way, and what forms of knowledge are they drawing upon in doing so? Through production, legitimation, and deployment of certain forms and sources of knowledge, historical actors learn about a problem—or construct their "definition of a situation," following Goffman's formula. This is not a unilateral process that happens instantly; it is more like an experiment, in which historical actors often try different things and learn from their mistakes. By focusing on these iterative processes of mistakes and learning, we come to understand not only the final "definition of a situation" but also other possible definitions that never made it as far. And on the basis of this initial definition, historical actors try out different solutions to their problems, testing reactions, gathering information from those tests, and weighing the costs and benefits to competing choices. In short, a proper observation of historical processes inevitably denotes learning by mistakes. What follows is an example of what can come from this focus on mistakes.

Starting in 1977, the Federal Reserve was charged with implementing the Community Reinvestment Act (CRA). This policy mandated that the Federal Reserve take a stronger role in enforcing anti-redlining laws. However, by the time the CRA underwent significant reform in 1989, a total of one bank had been sanctioned by the Fed in the first decade of its existence. The year of reform was also the year that the *Atlanta Journal Constitution*, under the direction of Bill Dedman, published the Pulitzer Prize winning series of articles exposing local banks' redlining practices: "The Color of Money." This produces a seemingly simple empirical question: why and how did the Federal Reserve fail to successfully implement the CRA? Without an in-depth examination of the primary documents within the Federal Reserve's archival record, the failure to adequately implement the CRA seems to be easily explained by regulatory capture: the Federal Reserve failed because it was unduly influenced by banks who found it unprofitable to lend to Black Americans. However, the archival record shows that the Fed had been implementing progressive regulator policies (including discrimination-focused policies like the Equal Credit Opportunity Act and the Home Mortgage Disclosure Act) throughout the 1970s with banks protesting the entire way. The Fed's distinct failure with the CRA cannot be explained by bank action alone.

The researcher's examination of archival records within the Fed itself, however, reveals an organization whose structure was incapable of directly addressing racism as a structural force. Its regulatory apparatus was most comfortable managing national economic systems in an ostensibly objective, impersonal, and hands-off manner. However, the CRA mandated that regulators directly adjudicate allegations of racism in communities across the United States. Consequently, the regulators inside the Fed saw the CRA as a threat to their organizational identity. The Fed refused to become enmeshed in "the sphere of political and social judgments."[6] To do so would be "beyond [the Federal Reserve's] legally mandated responsibilities."[7] The Federal Reserve saw its role as to "use traditional Federal Reserve resources such as economic research, strengthened examination procedures, and or ability [sic] to act as a neutral sponsor of meetings and conferences, rather than involve a fundamental change in the

[6] Bakstansky, Peter, and Ernest T. Patrikis. 1980. Meeting with Bronx and Brooklyn Community Representatives. Edited by Federal Reserve Bank of New York. New York: Federal Reserve Bank of New York.
[7] Ibid.

institutional workings of the Federal Reserve."[8] The Fed's initial plan to implement the CRA was to effectively minimize the policy's importance and relevance.

Unfortunately for the Federal Reserve, its initial strategy of minimization exposed the organization to the ire of politically connected activists. By August of 1980, The Federal Reserve Bank of New York (FRBNY) sent two vice presidents to attend meetings by activists in The Bronx. These meetings did not go well, as activists confronted these representatives and questioned the seriousness of the FRBNY's efforts to implement the CRA. These meetings also contained staff of at least one member of Congress. Additional meetings in Brooklyn prompted the president of the FRBNY, Anthony Solomon, to attend. With pressure mounting, Paul Volcker, the Chairman of the Federal Reserve Board of Governors, commissioned an internal review of the Fed's CRA efforts because he had heard from multiple sources that the Fed's efforts were worse than other agencies tasked with implementing the CRA. The review represented an acknowledgement of the Fed's failure to implement the CRA. It also resulted in a sixty-plus page document that contained over fifty suggestions for improvement.

This document and others related to the review, shine a critical light onto how the Fed processed this self-acknowledged failure. Additionally, these documents highlight the Fed's priorities and concerns, while also revealing how the Federal Reserve constructed a set of lessons from its failure and how those lessons structured its subsequent policy decisions. These documents reveal regulators' discomfort with "the subjective aspects of the CRA"[9]—mainly, bank examiners felt deeply uncomfortable making declarations of racial discrimination even with available Home Mortgage Disclosure Data. The Fed's regulators saw racism as fundamentally interpersonal rather than structural and thus outside of their purview as bank regulators. As a result, regulators were reluctant to enforce the CRA. They spoke of the need to potentially hire English majors from historically Black colleges rather than more economists so that they could better understand racial discrimination. In spite of possessing reams of quantitative data highlighting the significant imbalance in lending patterns, the organizational identity of the Fed's regulatory apparatus produced an orientation towards racial discrimination that rendered it as a regulatory irrelevance and produced the CRA's implementation failure.

It is through the researcher's attention to subjects' ascribed failure and the Fed's attempts to learn from that failure that allowed the researcher to identify the richest and most relevant documents for an explanation that could be generalized beyond this particular case. A focus on learning by mistakes helped the researcher identify the unique empirical richness that this particular collection of documents contained amongst the thousands of other documents. The mistakes reveal moments of poignant reflection and expressions of vivid concerns that show us how racialized values become sanitized into objective notions of what constitutes good governance. The Fed's racialized organizational identity (for more on racialized organizations see Ray 2019) reveals how the state's capacity to fight white supremacy is hindered by and through the proxies of bureaucracy and interpreted through ideas about good, rational governance.

Plausible Alternatives

The focus on moments of confusion and instability also forces us to reckon with plausible alternatives, or what Ann Stoler calls "what *might* be" as opposed to "what was" (2009: 21). In

[8] Ibid.

[9] Multiple authors. 1983. CRA Implementation Evaluation Report. New York: Archives of Federal Reserve Bank of New York.

order to tell the full story of how something came to be, we must take seriously the potential alternatives. Scholars attuned to the formal criteria of causal inference warn us against looking only at positive outcomes and ignoring the absence of such outcomes. A true causal explanation should be able to explain why certain things happened as well as why certain things did not happen. The attention to negative cases is essential in this regard (Emigh 1997; Mahoney and Goertz 2004). Our approach, however, differs slightly from a formal negative case method, which is oriented toward finding a case that does not feature a given outcome of interest. Following our emphasis on how historical actors made sense of their situation, we argue that a negative case also lies in the historical actors' understanding of what could have been possible. In other words, rather than formally identifying a separate case that historical actors were unaware of, we should pay more attention to what historical actors perceived as real possibilities—e.g., second best options, failed first tries, and things actors would like to try but did not believe to be possible. We can then ask questions such as: Why did they not choose these second-best options? Why did the first option—the historical outcome as we know it—prevail over other equally plausible solutions?

The case of the Dillingham Commission and its impact on the early twentieth century American immigration policy illustrated the necessity to engage with plausible alternatives. In the early twentieth century, nativists were mobilizing for federal-level immigration restriction, targeting southern and eastern European immigrants, such as Italians and Jews from Poland and Hungary. As stated in the literature on immigration history (King 2002; Zeidel 2004; Zolberg 2006: 232–238; Tichenor 2002: 42–43; Perlmann 2018; Benton-Cohen 2018), the Dillingham Commission was a nativist-led data collection project focusing on immigrants: empirical data was supposed to portray southern and eastern European immigrants as "undesirable races" lacking capacity to assimilate into American society. Most of the historical literature on the subject argues that the Commission achieved what its nativist leaders envisioned, thereby strengthening the rising immigration restriction movement in the early twentieth century.

When embarking on the project, the researcher was not totally sure of what he would like to find. He *felt* that the Commission constituted a critical juncture in the history of immigration policy-making, but was not clear about how to develop a compelling theory out of it. He started by visiting all the archives mentioned in the books about the Commission. He went through the documents that other scholars have examined, and in the process, found the archival collections that were not utilized extensively by previous scholarship. These were typical messy documents: low-level employees of the Commission were disputing the mandate of their bosses; the Commission leaders were confused about the findings; and intellectuals outside the Commission were using its data to challenge its conclusions. Slowly the researcher began to realize that the Commission encompasses the moments of confusion and contingency, the moments neglected in the post-hoc account of the Commission. By tracing these documents, he was able to develop a different explanation.

The empirical data showed that these immigrants were in fact not "undesirable" at all: they were more similar to northern and western European immigrants that came before them, and the nativist attempts to single them out resulted in providing a foundation—in the form of empirical data—for pro-immigrant intellectuals to advocate on their behalf. Southern and eastern European immigrants were indeed different, yet their difference was something that would easily diminish over time, not an immutable, essential one as the nativists would like to have portrayed.

The result of the Dillingham Commission's inquiry was, contrary to the aims of its nativist leaders, not a clearly defined, bright boundary between Americans and southern and eastern European immigrants, but a larger category of whiteness, that included not only the Anglo-Saxon, Protestants but also Italians and Jews from Poland and Hungary. The national quotas act in the 1920s

institutionalized this finding by allocating different quotas for different European nations while setting up a bright boundary against non-white immigrants, such as those from Asia. In short, the ending of the Dillingham Commission story is not a complete victory for nativists, but one with a lot of loose ends; in fact, the nativists' attack against southern and eastern Europeans resulted in a vast amount of government-mandated data, which effectively became an intellectual platform that provided further disputes on the nature and scope of whiteness in coming decades.

From the presentist perspective, the nativist victory ending makes much sense: after all, Europeans were mostly classed as white throughout the American history (Fox and Guglielmo 2012), and the whiteness of Jews and Italians seem like an inevitable historical outcome. It is not easy to think of other ways in which the boundary of whiteness could have been drawn to include non-Europeans who are deemed not white today.

The people of the Dillingham Commission, however, did not think so—at least some of them. Idiosyncratic as it may sound, some intellectuals at the time argued that Japanese immigrants were more deserving of whiteness, and therefore citizenship status, than southern and eastern Europeans, based on their characteristics. Yamato Ichihashi, a research assistant who, on behalf of the Commission, collected data on the Japanese immigrants living in California, advanced such an argument: the Japanese were educated, family-oriented, hard-working farmers who spoke English and strived to embody the American ideal of freedom and self-determination through their pursuit of independent land ownership (Ichihashi 1932; see also Chang 1997). They were more like other whites—northern and western Europeans and native-born Americans—than southern and eastern European immigrants, Ichihashi reasoned, and therefore should be included in the national community of citizenship.

In short, the idea of whiteness had a plausible alternative: rather than focusing on the geographic boundary of Europe, it could have amplified the moral politics associated with it, emphasizing "desirability" more than nationality or ancestry. However limited, this idea had some advocates and supporters among the people of the Dillingham Commission. Although eventually Europeanness prevailed as the criteria for whiteness, we gain a valuable understanding of different dimensions of the category by indulging in the claims of Japanese whiteness. What did it mean to be white? How did the whiteness tie to the institution of citizenship? Without reviewing the plausible alternative of whiteness that include Japanese immigrants, these questions cannot be answered properly.

Of course, not everything is equally plausible. One could in principle certainly imagine the world in which the nativist leaders of the Dillingham Commission gave up their belief on the Anglo-Saxon hegemony of the United States and fully surrendered to an open border immigration policy disregarding the concept of race altogether. However, as theorists of negative case method have demonstrated through the concept of "possibility principle," negative cases are only useful when they are possible (Mahoney and Goertz 2004). A purely theoretical combination of factors bears no leverage in terms of developing a robust theoretical explanation. In the same manner, the alternatives that were unthinkable from the perspective of historical actors are not very meaningful in our analysis, because, just like we are, historical actors are bound by their time and space, and thus limited in terms of what they can think as plausible. Simply put, it is impossible for the early twentieth century nativists to become non-racists; yet, they could have become a different kind of racists, one that privileges "desirable" Japanese over "undesirable" Italians. Although this did not happen, this historical speculation around a plausible alternative teaches us much about what actually transpired.

The notion of plausible alternatives teaches us to be careful in evaluating causal claims in historical sociology, especially when those claims involve absence of an outcome. We cannot

just assume that all of the possible cases presented themselves to historical actors; on the contrary, these actors thought and acted within the limited realm of possibilities and were only able to make choices based on what they perceived to be possible. In other words, alternatives should always be *plausible* not to us but to our historical actors—otherwise we risk projecting our own knowledge and perspective onto the past.

Conclusion

By presenting three case studies, we have described what kinds of evidence we looked for in our projects and how it helped us in building theories. Our approach is applicable beyond our respective case studies, to most research that seeks to pay attention to historical processes. We see two distinctive contributions for our framework: First, it can render existing causal and constitutive claims more comprehensive, thus making them more powerful. Second, it provides a template for expanding historical sociology to include a broader set of subject matters that lend themselves to robust single case studies. While the former might certainly appeal to established scholars, the latter should be of interest to graduate students who find their research topics not "fitting in" too well with the existing classics of historical sociology.

Let us provide examples of the former through a revisit of an important work of historical sociology—Prasad's (2012) analysis of progressive income tax in the United States. In answering the question of why there is so much more poverty in the US than other comparable countries, Prasad highlights how the mid-twentieth-century farmers (or agrarian populists, as they were then known) leveraged their political power in the Midwest swing states to build a taxation system based on income tax that privileged the middle-class over the working poor. Huey Long, a depression-era politician who put agrarian populists on the national political map, figures prominently in Prasad's account of the farmers' push towards income tax. As a governor facing overproduction of cotton in Louisiana, Long first proposed a collective ban on additional planting of cotton. After that proposal failed, he considered a barter system, and he also suggested the remonetization of silver as a means to increase money supply with little success. By 1933, Long arrived at income taxes as a politically successful solution that would provide the citizenry the means to buy farmer's products.

We see much congruence between Prasad's explanation and our framework. Huey Long's campaign pressures mark what we call positive contingency; his ideas evolved through the mistakes that he made on the campaign trail; and he eventually arrived at progressive income tax only after carefully considering and discarding other plausible alternatives. Although the comparison with European nation-states powerfully demonstrates that the poverty rate in US is an outlier and that the unstable tax revenue stream is to blame for the compromised welfare state, it is Huey Long's story that makes Prasad's book a work of *historicized* sociology (Clemens 2007), not just historical sociology: she details the process through which progressive income tax emerged as a viable policy option, and documents how the proposal shaped the compromised development of American welfare state. In this light, our framework—*positive contingency*, *learning by mistakes*, and *plausible alternatives*—makes clear what historicized sociology can do: highlighting and explaining key causal processes that shape the present.

As for the latter claim on broadening the subject matter of historical sociology, our framework aims to provide a lexicon for justifying and promoting historical research on what is perceived to be a single case. When a graduate student reveals that they intend to write a dissertation on a single historical case, they can face skeptical reactions from advisors and colleagues. How would they develop a generalizable theory from a limited number of cases

(Lieberson 1991)? If the student was lucky enough to have taken a seminar in historical sociology, they would murmur something along the lines of works by Moore Barrington (1966), Theda Skocpol (1992), and Tilley Charles (1990), about how macro-comparison of nation-states can provide sufficient variations in both the outcome of interest and numerous independent variables to warrant for a valid causal inference. This may or may not buy them some time and (tentative) approval to proceed. Even if such a seminar was offered, it is likely to never address the experience of the archival encounter directly. To make things even more complicated, they may want to study a topic that falls outside of the canon of historical sociology mentioned above—e.g., school desegregation campaigns in the 1960s Chicago; eugenics and the origin of Planned Parenthood; the emergence of a unified writing system in early modern China; and the list goes on. Faced with skeptical reactions and a dearth of training, they may lack a clear language to justify their topic of choice, not to mention effective preparation for what they will try do while collecting and analyzing their data. In our experiences, these aspiring historical sociologists often choose to present themselves as not necessarily comparative-historical sociologists but as sociologists of a particular contemporary social problem who happen to study past events. This can undercut the intellectual development of individual students as well as the development, diversity, and richness of the field in general.

The framework presented in this article aims to intervene at this point. As Howard Becker's *Tricks of the Trade* guided many aspiring qualitative researchers to navigate conversations with skeptical audiences, we provide vocabulary for aspiring historical sociologists. By asking a different set of questions and looking for archival evidence that matches those questions, our approach makes it easy for scholars to think through *process* and *emergence* in history, which Clemens (2007) noted as the most important topics of interest for newly emerging scholarship in "historicized sociology." *Positive contingency* traces the point of emergence by distinguishing critical moments of instability from mere randomness; *learning by mistakes* highlights iterative cognition of historical actors as they engage in historical actions that lead to important outcomes; and lastly, *plausible alternatives* invites us to ponder seriously about the options available to historical actors and how they approached the fateful choice that switched the course of the history. We do understand that for students of history and qualitative sociology, these notions are hardly new, if rarely formally outlined as we have here. Researchers should immerse themselves in the historical settings, putting themselves in historical actors' positions in order to truly understand how they felt and what they thought. Then we will have more accurate and comprehensive accounts of how things come to be and what that says about similar phenomena in other times and places.

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