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Letter from the Editors

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FROM THE EDITORS: Comparative Politics and History

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To paraphrase (if not butcher) Kurt Vonnegut, political science has “become *re*-stuck in time”: in an extremely welcome trend over the past decade or so, political scientists have been (re)discovering history. This recognition demonstrates that even though our finished products – be they articles, books, or policy papers – are editorially organized and neatly typeset, the process of conducting the actual research that went into them is anything but tidy, as Gasparyan illustrates quite nicely here. It is often full of loose ends, intellectual rabbit holes, and *cul-de-sacs*, frequently caused by the conceptual messiness of historical context and the tangible challenges of data extraction.

Approaches that place history at the center of their inquiry – American political development and historical institutionalism, to name just two – have in the past been seen by some in the discipline as embarrassingly old-fashioned and out of touch with more cutting-edge modes of social inquiry. This is unfortunate. Even more problematic, argues Fenner, is our tendency to rely on “after-the-fact analysis,” which distorts our understanding of our data at its very source: “If the owl of Minerva flies at dusk,” she writes, “what use is it to know what people were thinking at noon?” To this rhetorical question in which many political scientists might seek com-

fort, her decidedly discomforting – *non*-rhetorical – answer is that if you wait until dusk, you’ve missed the real story altogether.

As a social *science*, we aspire to laboratory-like conditions, but as a *social* science, we will forever be challenged by the complications of human agency, the imprecision of comparing complex systems, and the inherent inelegance of historical context. Yet if we look the right way, history can provide us with conceptual leverage that other approaches miss, and can provide us with otherwise elusive answers to understanding political developments, state-society interactions, and behavioral outcomes. As Paglayan puts it: “historical knowledge can help in every step of the research process, from choosing a compelling research question...to developing new theories or refining existing ones...to testing the empirical validity of those theories.”

The pieces presented in this issue address several broad methodological and theoretical concerns, including:

- **Old solutions to emerging problems:** Cirone looks at the historical cases of lotteries as selection mechanisms for political leaders as a possible way out of the increasingly elite-driven democratic systems faced with increasingly intractable populist discontent.





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- **Backwards exploration of path dependent outcomes to identify root causes:** Luft puts the puzzle-driven approach when applied to historical research thusly, “the method is unavoidably a process of reverse-engineering, of back-casting, that moves from an outcome back to a point where an explanation begins to form.” Dincecco employs such an approach to examine development trajectories that, when seen over time, provide a mismatch with conventional wisdom. Wang upends a key assumption in the understanding of the (d)evolution of Chinese politics over time – with implications for the present day – by arguing that China’s meritocratic institutions actually *undermined* Chinese national political development.
- **Data generation:** Costalli and Ruggeri argue that conflict studies can be vastly expanded upon and improved by continuing to mine archives and other historically-grounded sources. Gaikwad goes further to advocate for the idea that certain types of historical data are necessary to compensate for wide swaths of contemporary data that are unavailable due to political sensitivities or other limitations of access, a theme echoed by Hundman’s analysis of military disobedience. It also allows us to suggest comparisons of contemporary democratic states and liberal institutions with their historical non-democratic and potentially illiberal antecedents, as Hollenbach argues.

As these scholars demonstrate, history allows us to reexamine older, musty concepts that might seem decidedly “unsexy,” but which might help us understand political phenomena as much as some of the more recent and fashionable conceptual tools in making sense of contemporary political developments. Indeed, they have been

captured by a new generation of young scholars and reenergized some older ones to dust off these concepts and reengage them in fascinating new ways, as Ricart-Huguet’s piece on the study of elites demonstrates. Moreover, these can be deployed in comparisons with governance structures, social movements, and the power of ideas across newly-discoverable spatial lines of comparison, exponentially enriching our discipline. And, if we are really ambitious, we might follow Hanson’s call to engage with our counterparts in the field of history.

History is also important in the policy context: if institutions are living history, what are the actual flies in the amber that differentiate one from the other? For those of us who study China, we handicap ourselves by discounting history, as our Chinese counterparts see – Vonnegut-like – the relevance of events that happened years, decades, even centuries ago as immediate and relevant as yesterday’s *New York Times* headlines. There are several versions of this almost-certainly apocryphal exchange between Mao Zedong and Andre Malraux (or, alternatively, Zhou Enlai and Henry Kissinger), but it nonetheless provides an important lesson of the weighting of history and how the accuracy and relevance of our work might suffer by assuming it out of existence: when asked about the significance of the French Revolution on world history, the Chinese leader replied with a straight face: “it is still too early to tell.”

Finally, and arguably most important, if we divorce ourselves from actual history, we allow for “fake history” to fill these gaps in our knowledge. Subotic’s chilling account of post-socialist historical revisionism should be a sobering clarion call for all of us studying social phenomena that perhaps the most effective way to counteract the negative effects to our knowledge base by

technological change and social media monopolization of discourse is to reengage in an old-fashioned immersion into the wealth of our historical past.

In this issue of the Newsletter we also introduce an additional new feature: Q&A with the most recent Section Awards winners. Rafaela Dancygier (Luebbert Book Prize), Lisa Blaydes (Luebbert Book Prize Honorable Mention), Gabrielle

Kruks-Wisner (Luebbert Article Prize), Jonathan Homola, Miguel Pereira, William Simoneau, and Margit Tavits (Sage Paper Prize), Bryan D. Jones (Lijphart/Przeworski/Verba Dataset Award), and Nils B. Weidmann and Espen Geelmuyden Rød (Lijphart/Przeworski/Verba Dataset Award Honorable Mention) answer our questions about their research process and findings. ●

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THE IMPORTANCE AND PECULIARITIES OF ARCHIVAL WORK IN POLITICAL SCIENCE

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The scholarship on historical legacies has widely expanded in the last several years. This can be explained in part by some new directions in contemporary political science research. First, there is a growing number of studies about path-dependence and persistent effects of historical institutions on contemporary outcomes (Dell 2010; Iyer 2010; Michalopoulos and Papaioannou 2016; Guardado 2018). Second, there is always a demand for new original data in the discipline. This new data potentially can allow researchers to test hypotheses about institutional and economic development and state formation (Abramson and Carter 2016; Abramson 2017; Ali et al. 2018). Finally, historical, especially colonial-era, geographical data allows scholars to identify and estimate the causal effects of various institutions. (Lee and Schultz 2012; Bubb 2013; Lechler and McNamee 2018; Gasparyan 2019).

In this essay, first I explain the peculiarities and advantages of working with archival materials. Second, I elucidate biases and limitations of archival work in contemporary research. And finally, I use my own archival experience to show how my expectations about archival work mismatched the reality in the field. I use my research project about the impact of local self-government in-

stitutions (*zemstvos*) in 19th century Russia on party mobilization and voting behavior in 1917 as an underlying example to illustrate the necessity and complexity of archival work.

Features and Advantages of Archival Sources

Contemporary political science is significantly oriented towards using quantitative methods of analysis. A lot of new data comes from digital or Internet sources, which has allowed for the powerful and multifaceted application of advanced quantitative methods, including big data analysis. Accessing, extracting, and parsing this type of data requires a completely different set of skills than working in the archives and analyzing archival sources. Digital and machine-readable sources of information are able to be scraped and transformed into a convenient format for future analysis. Most of the archival materials, however, require summarizing and coding data by hand and often doing it on site. This brings new challenges for the scholar. First, it is necessary to be able to read, perceive and understand the information quickly. Second, when working with a large amount of archival sources, coding decisions should be done either well in advance or while parsing the information in the archives. This allows for easier extraction of data and

Data collection from archival sources requires completely different approaches compared with working with digital data.

immediate use of it without additional rendering of all the materials. Another issue is related to typos and errors. With digital data a scholar can always adjust the script, rerun the code or quickly go back to the original sources online; in archival work fixing mistakes will most likely require going back to the field. Hence, data collection from archival sources requires completely different approaches compared with working with digital data.

Archives serve as a source of a more detailed fine-grained data. The available off-the-shelf data is often provided at a highly aggregated level (national, regional or provincial). However, there is always a threat of ecological inference problems while working with more aggregated data (King, Tanner and Rosen 2004). Getting more detailed lower-level or even individual data can help to observe new concepts and results that can be estimated only at the micro-level, which might lead to novel contributions in the field.¹ In my project about *zemstvos* institutions, I work with election data for the 1917 All Russian Constituent Assembly that was published at the aggregated district level by Protasov (2014). However, working with the original files regarding these elections in the Russian archives allowed me to not only get data on election results, but also to access data on voter turnout, total population and the sex distribution at the precinct level. This revealed a broader picture of what the distribution of population that actually participated in these elections was, what the local attitudes towards these elections were, and potentially how important they were in each district.

Sometimes aggregated data has a significant amount of missing data. Fine-grained data can allow scholars to better understand the nature of this missing data. In my case, for instance, some districts completely lacked election results and others had missing values for several indicators (like vote share for some parties). I wanted to understand whether the lack of election results for the whole district happened due to some random events (unique archival materials got lost or accidentally destroyed) or this data was purposefully misreported? In the case of missing values, does it mean that they are not available (NAs) for certain idiosyncratic reasons, or are they just simply equal to nulls? By looking at the original election protocols, I was able to establish that missing data for the election turnout was driven by missing population data at the village level. The lack of any election results data for some districts can be explained by the incidents of spoiled ballots in a number of precincts. Moreover, since elections in different provinces were not held at the same time, going through the election's protocols allowed me to identify that in some provinces the election was never even held.

While working with any secondary sources, it is important to be aware of the ideological spectrum of opinions about certain events. This is valid for historiography and historical narratives too. That is why it is necessary to supplement such literature with original archival documents that simply describe the sequence of events. Getting the ideological spectrum of different opinions from the historiography side and complementing it with original documents that describe the events' timeline allows the author to

1. Some of the more recent studies about the effects of self-government reform in Russia in the second half of 19th century (Nafziger, 2011, 2012; Dower et al., 2018) use fine-grained data from the original statistical handbooks and 1897 census documents.



see history more objectively and update their own original opinion. In my case, for instance, there exist historical narratives (Veselovskij 1909; Starr 2015) that describe features of self-government reform (*zemstvo* reform) and details about preparation for its implementation. However, archival documents provide official statements and state documents, which can lead to a better perception of the causes and consequences of this event. Features of the All Russian Constituent Assembly election of 1917 were also previously described by historians (Radkey 1989; Protasov 2014) and social scientists (Ivanchenko, Kynev and Lyubarev 2005; Castañeda Dower and Markevich 2017). These secondary sources allowed me to gain basic information about the election. I learned that it was the first Russian election that was held under universal suffrage, and it was also separately organized in each province. Each province had their own list of parties or political groups that ran for seats in a newly formed representative body. However, for me it was also important to understand how easy it was for a newly formed party to register and actually run in the election. These secondary sources did not have such detailed information. The Russian archives, on the other hand, contain correspondence between election committee members, lists of party candidates, election announcements and copies of the special elections committee meetings' original protocols with details about parties' registration and explanations of the registration rejections. Going through these documents helped me to better understand the place of smaller parties and political groups in this election. It provided me a less biased view on the election and helped explain how it was perceived at the regional level and what it meant for bigger parties. Seeing multiple ethnic and regional specific groups in some provinces allowed me to assume

that in certain places of the country there were no strict barriers for these parties and political groups to participate, and they could have used these elections as a chance to get representation at the national level.

Limitations and Biases

Original archival materials help to see the underlying conditions and understand the context of a particular problem. However, there is a number of limitations of archival work that researchers should be aware of.

Selection bias is one of the problems related to archival work (Lee 2017). Archives contain only a sample of the documents that were produced by different individuals and institutions. The rest of the documents could have been destroyed, lost, or stolen, and it is hard for a scholar to judge how significant the missing information is. While working in the State Archive of the Russian Federation I went over almost all the files (approximately 400) that contained information about the All Russian Constituent Assembly election. I collected all the protocols and reports with the elections' results, and still lacked data for certain districts and villages. I knew the exact districts for which information was missing, but the reasons for this missing data were unclear.

Usually, archives belong to a larger network of the state or regional archives, and there can be multiple archives that contain information on a specific research topic. From my experience of working in the Russian archives, it was important to know the proper time period, question, and the level of analysis, since there are separate archives for different historical periods (for example, before and after 1917), issue-specific archives (for example, the State Economic Archive) and regional and local archives.



Using archival sources obviously requires knowledge of the local language, which is already not a trivial task, and this is one of the reasons why some scholars in the social sciences still prefer to rely on secondary translated sources of information. Working with old archival materials can be even more challenging due to the physical conditions of the documents: bad quality of print, bright photocopies of the documents, torn pages, handwritten information or use of an older version of the language. These factors increase the time needed for accessing, reading and extracting data from such sources.

Going to the archives with a vague undefined topic makes it almost impossible to search for relevant information. Archival work requires key preparation. It is crucial to have a narrow question and an idea of what type of data and materials are needed. Can these materials be gained through open-access or easily accessible sources? Only when these sources lack the necessary data, it makes sense to go to the archives. If one does decide to work in archives, I have found that it is useful to explore archival materials through electronic catalogs and select folders and cases that can be related to the chosen topic in advance. This significantly speeds the process in the field.

Expectations vs. Reality

As a graduate student with mostly quantitative training, I was always more concentrated on data analysis rather than data collection. However, when I started working on my historical project, I realized that it is important to get fine-grained data to have a better measure of my outcomes. Additionally, I wanted to have more detailed information about All Russian Constituent Assembly election of 1917 in general

and all the minor parties and groups that were participating in it. Hence, I decided to explore the available archival materials that were related to this election.

I knew that archives contain a massive amount of unorganized information; however, I have never been exposed to specific training in conducting archival work. Being usually more focused on methodological and computational aspects of research, I had to rethink my data collection approach to adjust for the peculiarities of archival work. Talking to colleagues in the history department helped me to narrow the sources and directed me to the proper archives. Narrowing down the topic of interest and the exact data needs allowed me to be more efficient during the preparation stage and while working in the archives. In attempts to be more time efficient, I extensively planned my archival work trip. I learned archival schedules, explored the online catalogs of the archival materials, created personal online accounts and started ordering necessary materials on the date when I was supposed to arrive to the archive. That significantly helped with time management. However, the reality did not fully match my expectations, and I still faced certain problems related to the search of the materials, time constraints and restrictions on extraction of information.

One of the first challenges I faced was related to time constraints. Everything in the archives takes almost twice as much time as an unprepared scholar expects. I anticipated that I will be able to collect all the necessary data during a six-week field work trip. Eventually, it took me two trips and a total amount of about three months to gather data that I wanted to extract from the archival materials. There are special schedules for when to order archival materials



and how fast these materials will be delivered. In my case, the waiting period from order to delivery varied from two to three working days. Additionally, a scholar cannot order all the required files at once. Usually, I could not have ordered more than 20 folders or cases per day. This all significantly increased the time required for me to complete my data collection.

I selected necessary folders and files in advance using the online archival catalogs. Most of the files and cases contained broad names and catalogs did not provide a proper description of the folder materials. This created additional pitfalls. For instance, I ordered a set of files that, according to the name, included statistical overviews for the Russian provinces; however, they turned out not to have information that I needed. I was looking for detailed microlevel statistics, but those files only included short summaries of some statistical findings. Furthermore, in some of the files multiple pages were damaged or missing. One of my orders was even canceled due to the very poor physical condition of the materials with a note that the file was currently undergoing restoration.

I expected that I can easily extract necessary data by copying the archival materials. However, professional copying takes several weeks and is very expensive, and unauthorized copies or photocopies of the materials is prohibited, unless the materials are already scanned or microfilmed, i.e. available in a digital format. Some of the statistical reports are not processed to a digital format and are not allowed to be photocopied. I had to go over all the files, decide which information and data are relevant for my study, and then rewrite or retype it. Luckily, most of the elections data required for my study was in the microfilmed format. This sped up the process of data collection because I was able to pho-

tocopy most of the materials for future coding. However, sometimes the quality of the scans, microfilms and machines for using microfilms was quite poor, which also increased the time required to extract the information.

Even when I got access to the data I needed, it was still hard to predict exactly how this data would look like and in what format it would be presented. In my project, I expected that all the electoral protocols would be structured in the same way and contain the same information for all the districts and precincts. But the data was presented in a highly variable form. For some of the precincts elections results protocols included tables with the precinct population information (split of the population by sex, number of the eligible-to-vote population, information about spoiled ballots) and the number of votes received by each party, while others contained protocols with a full description of the election procedures in a given precinct with a short summary of the elections results.

Additionally, I was specifically interested in the vote share of regional and ethnic parties for the district and precinct-level. Frustratingly, almost all election results protocols did not contain the names of the parties, but instead only the parties' registration number. To be able to identify the vote shares, I had to find parties' registration numbers, which required me to order additional folders and files. Usually this information was provided in a separate file for each province, alongside the full lists of the parties' candidates. However, for several provinces such a file was absent and I had to look over supplementary files and election preparation materials to find the protocols of the parties' registration process. After I matched parties' registration numbers and their names, I had to decide whether I should collect the number of votes for each

individual party or I should aggregate these votes for all the regional and ethnic parties in a given province. Since it was important to identify which parties actually can be considered ethnic and regional, I ended up collecting the disaggregated number of votes for each party in every province. Later on, relying on the election preparation protocols and secondary sources of information about these parties, I was able to identify which parties can be considered ethnic and regional.

Archival work is not an easy task. It comes with biases and limitations, and even for an experienced scholar it can be very unpredictable. Based on my personal experience, I wish to note that archives can turn out to be time-inefficient. My first trip to the archives was very unproductive. I went there with a vague research idea. I

was not sure what materials to look for and was not prepared for how disorganized the information there is. As a result, I learned that archives are a poor source for very preliminary exploratory work, and they require from the scholar a sophisticated understanding of what data to obtain.

Despite all these limitations and difficulties related to archival work, I believe that everyone who works with historical data needs to explore archival materials to have a better sense of data origins, context and the roots of the secondary sources and off-the-shelf data. For my own research, archives provided valuable experience, and they have become the primary source of my fine-grained precinct-level data, that has allowed me to better understand the 1917 Russian election. ●

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RECOVERING LOST FUTURES: Contemporaneous Sources and the Study of Past Possibilities¹

by *Sofia Fenner*



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*If the owl of Minerva flies
at dusk, what use is it to
know what people were
thinking at noon?*

Comparative politics can ask much of us: languages and dialects to learn, new locales to call home, and unfamiliar social norms and government processes to navigate. Many of us spend years (or decades) building familiarity with a particular country or community, only to find that a promising theoretical lead requires us to tackle an entirely new case. Given these challenges and the pressures of publication, it is not surprising that practitioners of historical comparative politics often turn to secondary works to build their arguments. Gathering and analyzing historical sources ourselves can seem overwhelming—or a risky investment of time, especially if our hunches about that new case turn out to be wrong. We gratefully rely on the work of other scholars, often written many years after the events we seek to understand.

The comparative tendency to rely on after-the-fact analyses, however, is not motivated by convenience alone. As political science has become increasingly invested in prediction (Blyth 2006), assessments made in the midst of political events are viewed with mounting suspicion. In my own subfield, Middle East politics, scholars no sooner finished be-

rating themselves for failing to predict the Arab Spring than they began berating themselves for their optimistic takes on the early uprisings. As events wore on, early expressions of possibility from protesters and analysts came to be seen as misplaced, or even naïve. We now commonly invoke the Arab Spring as a cautionary tale, its moral that waiting and seeing is the best analytical choice. If the owl of Minerva flies at dusk, what use is it to know what people were thinking at noon?

Political actors themselves, however, do not have the luxury of waiting for dusk. They are in their historical moments, unable to see clearly what will happen next. Therefore, relying on post-hoc accounts risks imparting a subtle but powerful bias to our analyses. Knowing what happens next—indeed, being unable to un-know it—we risk missing “lost futures”: envisioned paths that never materialized, events that were anticipated but never occurred. These lost futures may have been “bad” analysis, but they nevertheless informed actors making consequential decisions. When we ignore them, our histories run the risk of becoming just-so stories, in which events were always bound to turn out the way we know they did.

1. The author would like to thank Yuna Blajer de la Garza, Amy Gais, and Diana Kim for their insightful comments.



In what follows, I offer one example of how after-the-fact histories lead us astray, and how contemporaneous sources can reveal a completely different story. In doing so, I draw on my own research into co-opted opposition parties in authoritarian North Africa. Co-optation is widely recognized as a pillar of durable authoritarian rule, but we know surprisingly little about the circumstances under which opposition parties agree to be co-opted (that is, to participate in the formal structures of an authoritarian regime). Conventional accounts argue that opposition agrees to co-optation when it sees no chance for radical change in the future, but contemporaneous sources reveal the opposite—parties agree to co-optation precisely when they think real reform *is* possible. I illustrate this argument through the experience of Morocco's Istiqlal Party, which was co-opted in the wake of the so-called "Green March" in the mid-1970s. I then consider potential objections and close by suggesting strategies for balancing the manifest benefits of doing our own history with the equally real costs.

Co-optation and the Future

Conventional accounts of authoritarianism paint it as a losing proposition for opponents. The term itself, which once referred only to bringing a new actor into a system or body, now commonly also means "weakened" or "domesticated," as if being co-opted neutralized opponents by definition. But if co-optation is so bad for advocates of political change, why would they ever submit to it in the first place? Dominant theories are clear on this point: opponents will agree to co-optation when they accept that far-reaching change is impossible. As Gandhi and Przeworski put it in their agenda-setting article on the subject, "when an opposition sees no chance to overthrow a dictator

in the foreseeable future, they may prefer limited influence [through co-optation] to interminable waiting" (2006,14). This explanation has the advantage of seeming correct when viewed from the present: in many regimes that co-opted serious opponents decades ago, democratic reforms have indeed never materialized. It thus makes sense to conclude that parties, accurately predicting this future outcome, agreed to co-optation because they realized that autocratic systems were unshakeable.

A detailed reading of texts contemporaneous with the co-optation of one such group, however, reveals exactly the opposite. Morocco's Istiqlal Party is a paradigmatic case of co-optation, commonly drawn upon to illustrate the damage co-optation can do to once-threatening opponents (e.g. Lust 2005, Gandhi 2008). Yet the party's own texts from a crucial period in the late 1970s reveal that they agreed to co-optation precisely when they thought that political change had become *more* likely, not less. Co-optation did not involve setting aside their anti-authoritarian aspirations; instead, it was a plausible way to pursue them.

Istiqlal's Co-optation

Istiqlal's true co-optation followed on the heels of a watershed moment in modern Moroccan political history: the Green March. In 1975, King Hassan II led a procession of several hundred thousand unarmed demonstrators into what was then the Spanish Sahara, a colonial territory south of internationally-recognized Morocco. The goal was to "liberate" the Sahara from Spanish rule and "return" it to Moroccan control. The gambit worked, at least vis-à-vis the Spanish, who renounced their claim to the territory within days. While the Green March (so named because many protesters bore copies of

the Qur'an) turned out to be just the beginning of a decades-long, violent dispute over the territory, it was a major achievement for the king. Territorial issues have historically been highly salient in Morocco, so pulling off a photogenic victory against a colonial power was no small achievement.

Existing accounts in both history and political science thus reasonably consider the Green March a victory for the regime. Problematically, however, they conclude that it must therefore have been a defeat for the opposition—including the Istiqlal Party. Founded in 1944 as an anti-colonial nationalist movement, Istiqlal had long been the most powerful party in Moroccan politics. During the 1950s and 1960s, it acted like an opposition party, pushing the King to adopt a constitution and to expand parliament's role. While Istiqlal never called for the end of the monarchy (many Moroccan nationalists see unifying value in a symbolic, ceremonial king), the party opposed many of Hassan II's initiatives and strongly rejected the marginalization of parties and parliament that attended Hassan's declaration of a state of exception in 1965. Elections were suspended, parliament essentially shuttered, and cabinets staffed with military officers and loyalists. Istiqlal found itself on the outside of an increasingly authoritarian system.

The Green March is widely viewed as an inflection point in Istiqlal's relationship with the regime. When the King reopened the political system with local elections in 1976 and parliamentary ones in 1977, Istiqlal participated avidly—essentially, agreeing to be incorporated into the revived electoral authoritarian system. Though they expressed concerns about electoral fraud, the party agreed to join the cabinet that resulted from the 1977 elections, something

Hassan had been trying to convince them to do for years. Its participation helped facilitate and lend credence to the King's attempt to shift political power away from an unreliable military.

The leading explanation for Istiqlal's co-optation in the late 1970s is that the party abandoned its democratic aspirations. In the words of one scholar, "[a]fter almost a decade of violence and repression, the parties realized they were unable to topple the King...Consequently, they established their willingness to accept the [1972] Constitution, the King called for new elections, and a political bargain was struck" (Lust 2005, 58). For Moroccan political scientist Muhammad Radwani, the Green March was the moment when the nationalist parties—Istiqlal first among them—"gave up on their visions and imaginings of modernity," settling instead for the status quo (2011, 14). Knowing the shape Moroccan politics took in subsequent decades, these explanations may seem convincing: viewed from a future in which democracy did not materialize, it seems plausible that Istiqlal members might have anticipated that future.

Contemporaneous sources, however, suggest that they were thinking something else entirely. Istiqlal texts describe the Green March as the fulfillment of a long-standing party demand. The return of the "violated lands" still held by European powers and the restoration of Moroccan "territorial unity" were recurring, central features of Istiqlal's party platforms and leaders' major speeches from independence onward. The successive failures of palace-backed governments on the Sahara were regular targets of Istiqlali critique. As early as 1959, Istiqlal president Allal al-Fassi scolded the authorities (*al-sulta*) for their "silence on major national issues," including territorial unity (Al Fassi 1959). One of al-Fassi's last public speech-



es alerted Moroccans to (from his perspective) an impending catastrophe: Spain seemed to be preparing to call a referendum on Sahrawi self-determination. Such a referendum, he warned, could lead to outright independence from both Spain and Morocco (Al-Fassi 1974).

Istiqlal pressure for action on the Sahara outlived al-Fassi, who died suddenly in May 1974. The party offered two public memoranda to the King that same year, urging prompt action to preempt a move toward Sahrawi independence. Even as new party head M'Hammed Boucetta and other Istiqlal elders joined the regime's diplomatic efforts, other party spokesmen kept up their criticism. In the pages of the party newspaper, *al-'Alam*, one Istiqlal luminary criticized the administration (*al-idāra*) for its "silence" at the United Nations, and for its "lateness" and "confusion" in coordinating diplomatic efforts at the International Court of Justice in the Hague (Al Youssoufi 1975a). Istiqlal's recommendations grew increasingly specific over time: four days before the King announced the March, a front-page party editorial announced that "before us are only two options:" either do nothing or "change our methods," which up to that point had been primarily juridical and diplomatic (i.e. not popular) in nature (*al-Iftitahiyya* 1975a).

Thus, when the March was finally announced, Istiqlal leaders described it not as monarchical initiative but as a concession to their repeated demands. Boucetta noted in an October 23, 1975 interview, reprinted in *al-'Alam*, that the upcoming march was "an important step," but cautioned that "we must not forget that Istiqlal had been demanding for more than a year that the people be enabled to enter into the battle against the colonial forces that are holding our Sahara." Rather than allowing a moment

of nationalist fervor to overshadow the party's ongoing oppositional aspirations, Boucetta continued his calls for political reform: as far as domestic politics were concerned, he argued, "nothing has changed, and problems are still waiting for solutions, whether on an economic or a political level." As another party leader put it, it was al-Fassi's death—the loss of a nationalist hero for whom the Sahara was a paramount, life-long priority—that had persuaded "officials to radically change their path and their positions, and to join the caravan of the Moroccan people on the issue of the Sahara" (al-Yousoufi 1975a). Later observers paint the Green March as Istiqlal's acquiescence to the King's initiative; for Istiqlal, it was the other way around.

Moreover, Istiqlal argued that the success of the March could be parlayed into success in achieving its other political goals, including those relating to parliamentary power. Four days after the King announced that the Sahara had been returned to Morocco, *al-'Alam* ran a lead editorial entitled "A New Morocco...and Elections." The piece draws an analogy between the "popular mobilization" that fueled the March and the mobilization of voters needed to ensure the success of the country. The King had announced that elections, long suspended under the state of exception, would be held soon, and Istiqlal's rhetoric on the coming vote echoed its discussion of the Sahara: "holding elections at a professional, regional, and national levels is the fulfillment of a demand for which Istiqlal has always called" (*Al-Iftitahiyya* 1975b). Party sources framed progress on democracy, like progress on territorial unity, as the enactment by the regime of Istiqlal's longstanding demands: that is, success, not failure. As these demands were met by popular action on the Sahara, so too might they be met on other issues. Far from

abandoning its visions of the future, the party was finally starting to articulate how, after years of authoritarian retrenchment, those visions might be realized in cooperation with both King and people. It was the language of possibility—not defeat or impossibility—that coincided with Istiqlal's deepening co-optation.

Potential Objections

Political scientists often distrust contemporaneous accounts. Such accounts often come from political actors themselves and are likely to be public-facing, since newspaper articles, speeches, and official statements are more likely to be preserved in archives than private communications (which may never have been written down at all). As a result, skeptics often dismiss contemporaneous accounts as spin: perhaps Istiqlal's leaders knew that there was really no hope of democratic reform but claimed there was in order to mask their actual (presumably self-serving) motivations. If we cannot know whether actors' contemporaneous discourse is sincere, are we not better off relying on what we know happened later?

Scholars working with contemporaneous accounts can respond to these concerns in several ways. They can triangulate, working to show that other political parties and non-party local observers shared similar assessments of what the future would bring. But not all assessments of the future that matter are shared: it is entirely possible for one political faction to think revolution is nigh while another views the status quo as stable. My point is not that contemporaneous sources are without bias; it is that later sources are *also* biased, against the importance of lost futures. When we assume that predictions are self-serving because we know them to have been proven wrong, we anachronistically

read our own certainty back onto historical actors. Contemporaneous sources are a valuable check on our deep-seated just-so tendencies. In this case, they help us see that the conventional narrative of co-optation as an act of surrender is inaccurate; parties agree to co-optation not when they think they have lost, but when they think they are winning.

Conclusion

The work of analyzing contemporaneous sources is not easy. I worked in four different archives in Morocco, spending months combing through newspapers and party texts—not to mention years it took (indeed, is still taking) to learn Arabic. I only discovered Istiqlal's long-standing activism on the Sahara because I began my archival research at independence, in the 1950s, rather than jumping straight to the 1970s. Had I not done so, I might have interpreted the party's rhetoric on the Green March as nothing more than hastily-constructed spin. This was a time-consuming choice, but it allowed me to work through history in the order that it was lived, making me more sensitive to what was (not) known at any given time.

Doing our own history helps us recover lost futures and their theoretical implications, but it is costly. How should scholars manage this tradeoff? I offer four suggestions. First, all research is more efficient when we are well-trained. Graduate courses on comparative historical analysis should cover not only research design and case selection but also historiography and strategies for approaching primary sources. Incorporating methodological reflections from political scientists (e.g. Kim 2017) as well as texts written primarily for historians (e.g. Farge 2015) would help students situate themselves in the interdisciplinary quest to under-



stand the past. Second, some historians explicitly aim to reconstruct lost futures and recover past senses of possibility. By seeking out their analyses—even when they do not tell us what we want to hear—we can build on the work of others and avoid duplicating their efforts.

Third, scholars can prioritize, asking where in our arguments contemporaneous sources might add the most value. I have suggested here that past actors' assessments of the future are one such area, but there are sure to be others. And finally, such work should be recognized and valued for the original contributions it can make not just to political science but to history as well. We should notice, appreciate—and, yes, interrogate—each other's historical research.

We may also want to reconceptualize our own work when political events overtake us. Rather than fearing that we might be proven wrong, or refraining from public comment in the interest of not embarrassing our later selves, we could embrace the role our near-term analyses will play for future historians. Without scholars "naïvely" imagining successful Arab uprisings, to return to an earlier example, it would be easy to write the real optimism and possibility of 2011 out of history entirely. Our "errors" are data for collaborators who will come later. At a time when various forces—from climate change to neoliberal hegemony to resurgent authoritarianism—sabotage our ability to imagine our futures, there is value in recording what we once believed them to be. ●

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WE HAVE HISTORY – AND HOW IT CHANGED ME¹

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*Bringing history into my
work changed my view
about what are the main
questions that should
guide a comparative
politics of education
research agenda.*

If I had taken on a research agenda on education without knowledge of or interest in the history of education systems, my agenda would have taken for granted several ideas that older scholars have popularized:

1. That primary education is a form of pro-poor redistribution, and democracy increases its provision
2. That education empowers individuals
3. That teacher unions are a major force shaping education policy decisions

But I came into comparative politics with some contemporary and historical knowledge of education systems, acquired both from things I had

read while pursuing graduate studies in education and from first-hand education policymaking experience in developing countries. For better or worse, this knowledge was enough to prompt me to dig deep into these common claims rather than take them for granted.

Digging deep entailed delving into history. Delving into history led me to question popular ideas about education provision that I myself had also internalized.

The historical perspective of my work is not something I or others who know me could have easily predicted. What drove me away from a career at the World Bank and into political science and comparative politics was a passion to understand the political reasons why *today* many education systems fail to teach even basic reading and math skills—much less critical thinking. With this passion intact, bringing history into my work changed my view about what are the main questions that should guide a comparative politics of education research agenda, and gave me a valuable perspective on today's education issues.

The lessons I learned about the benefits of a historical perspective are general lessons that I suspect will be valuable to others in comparative politics—from graduate students who are beginning to carve out their scholarly identity, to more senior scholars who maintain as much curiosity and excitement for new discoveries as in earlier days. I will flesh out these lessons by referencing my own research on education and what it implies for the state of knowledge and future research on education, but they apply to almost every topic of interest in comparative politics—autocracies, democracies, conflict, identity, economic development, state capacity, etc.

1. I thank David Laitin, Eugene Finkel, and Gareth Nellis for helpful and extremely rapid feedback.



To preview, learning more history helped me, and can help you:

1. Identify new compelling empirical patterns that deserve an explanation
2. Assess the validity of existing theories' assumptions
3. Clarify the mechanisms by and conditions under which X leads to Y, and thus refine existing theories of what explains Y, or how X affects it
4. Test predictions of the form X increases/decreases Y
5. Assess the plausibility of your proposed mechanism linking X and Y, and rule out alternative explanations for why X might increase/decrease Y

That is, historical knowledge can help in every step of the research process, from choosing a compelling research question (#1), to developing new theories or refining existing ones (#2 and 3), to testing the empirical validity of those theories (#4 and 5).

In my own work, studying history—gathering and analyzing quantitative historical data, immersing myself into qualitative primary sources to better understand the process leading to major policy decisions—has led me to substantially revise my thinking on each of the ideas listed at the beginning, and develop and test new ideas about the determinants of education provision.

The rest of this note is organized into three main sections corresponding to the three popular ideas about education listed earlier. Each section begins by briefly reviewing the common claim, then I introduce historical patterns or evidence I have discovered that challenge that claim, and finally, I identify new puzzles that emerge from this evidence. I conclude by dis-

cussing what I think is the most valuable contribution of historical research for comparative politics.

History's challenge to redistributive theories of education provision

Popular claim: Almost every comparative politics student, at some point in their graduate training, reads a seminal study that claims that democratization incentivizes governments to provide more education, especially primary education. Examples include Acemoglu and Robinson's (2006: 64) book on the redistributive (economic) origins of democracy, and Bueno de Mesquita and colleagues' (2003: 187) book on selectorate theory. A few students may also read Lindert's (2004) *Growing Public*, the seminal book that these authors cite to validate that claim. Lindert looks at school enrollment rates for about 20 OECD countries from 1870 to 1930 and argues that the spread of democratic voting rights to the poor "played a leading role" in explaining the rise of primary schooling (Lindert 2004: 105).

A key yet untested assumption in these studies is that, before democratization, the median voter lacked access to primary schooling.

Even if they have not read Lindert's work, most comparative politics students become familiar with studies on education provision that build on it, such as Stasavage's (2005) "Democracy and Education Spending in Africa", Ansell's (2010) *From the Ballot to the Blackboard*, or Brown and Hunter's (2004) "Democracy and Human Capital Formation." These influential studies extend the thesis of *Growing Public*, showing that, since the 1960s, enrollment and public spending on primary education have been greater among democratic than non-democratic countries.



What I discovered: In “Democracy and Educational Expansion: Evidence from 200 Years,” I present two facts that fly in the face of the popular belief that democracy played a leading role in the rise and spread of primary schooling. The first fact concerns the rise of schooling: barring a few exceptions like the U.S., almost all state-controlled primary education systems in the world emerged before democracy. Historians have devoted extensive attention to the case of Prussia, where already in 1763, under an absolutist regime, Frederick II passed a Royal School Ordinance imposing compulsory primary schooling for children in rural areas. By the early-19th century, Prussia had become *the* world model of public primary education; sending officials to learn about Prussian schools was considered a “must” for any state seriously interested in developing its own primary education system. This was true even in democratic countries like the U.S.

What my research shows is that Prussia was not an anomaly; around the world, most central governments began to regulate and monitor primary education well before there was a transition to democracy. This conclusion comes from comparing the year when a country first transitioned to democracy (measured in three different ways) and the year when the central government first intervened in primary education. For a sample of 109 countries from all regions, I found that central governments began to systematically monitor primary schools about 60 years before democratization. In European and Latin American countries, for which I used Ph.D. dissertations, articles and books written by historians to identify the timing of additional forms of state intervention, I found that, on average, central governments began to fund and build

primary schools a century before democratization; established teacher certification requirements and a mandatory curriculum for all primary schools, and took direct charge of teacher training, nine decades before; and passed compulsory schooling laws over five decades before democratization.

The second historical fact goes directly to the heart of median voter theories about the spread of primary education. My work shows that the common assumption that the median voter lacked access to primary schooling before democratization rarely holds. Among countries that transitioned to democracy at some point, primary school enrollment rates had already reached on average 70% *before* democratization. This is not driven by a few outliers. In most countries, a majority of the population already had access to primary schooling before democracy emerged.

Curious readers can refer to the paper for an estimate of the effect of democracy on primary school enrollment rates. Suffice it to say that, in most parts of the world, democratization did not play a major role in the spread of primary schooling.

Research puzzles that emerge: The empirical patterns I identify suggest that the literature has overemphasized democracy’s role as an explanation for the expansion of primary schooling, and that a key puzzle for future work in comparative politics is: Why was there so much provision of primary schooling under non-democratic regimes? And what does the autocratic origin of public schooling imply for the characteristics of education systems today?



History's challenge to the idea of education as empowerment

Popular claim: Theories that predict that democratization leads governments to increase the provision of primary schooling usually conceptualize schooling as a “good” or “service,” something that citizens want and demand, and governments are reluctant to provide unless they have political incentives to be responsive to citizens, as in a democracy. Development is freedom, says Amartya Sen, and access to education is a key component of development. Education empowers us; it gives us the knowledge and skills we need to pursue the kind of life we want to live. It also makes us more assertive and enhances our critical thinking skills, which is why an educated citizenry is bad news for autocrats (Lipset 1960; Almond and Verba 1963) and why autocratic regimes will usually limit its provision (Bourguignon and Verdier 2000).

What I discovered: As I have discussed, autocratic regimes did much to expand the provision of primary schooling for the masses. They built schools, recruited and trained teachers, designed curriculums, established compulsory schooling laws so that all families would send their children to school, and deployed inspectors to enforce education laws. While one set of theories in comparative politics suggests that autocrats may have provided primary education to appease angry citizens by giving them something they wanted, the historical cases I have studied do not support this view of the origins of education. In 1760s Prussia, 1830s France, 1860s Chile, and 1880s Argentina—the founding moments of state-controlled primary education in each of these cases—rulers believed that rural families were uninterested in sending their children to school and would resist schooling, partly because they relied on their children for

work (Paglayan 2017). Nor have I found much evidence to support the modernization view that the reason these rulers wanted to promote mass schooling was to empower people with useful knowledge and skills that would, in turn, contribute to urbanization and industrialization (cf. Gellner 1983; Weber 1976).

The main argument that elites espoused in favor of schooling poor children had to do with the benefits of shaping their minds to control their future political behavior and beliefs and, with that, promote political stability (Paglayan 2017). François Guizot, the mastermind behind France's first national law of primary education, passed in 1833, writes clearly about this goal: “The state must provide primary education to all families and give it to those who cannot afford it; and in this he does more for the moral life of peoples than he can do for their material condition” (Guizot 1860: 63-64). And he continues: “When men have learned from childhood to understand the fundamental laws of the country and to respect its sovereign, the sovereign and the laws become a kind of property which is dear to them, and they do not refuse the obligations that it imposes upon them ... Thus the public mind is formed, thus a true patriotism is maintained, thus fortifying and consolidating societies and thrones” (Guizot 1860: 86). The Guizot Law of 1833 is arguably the most important piece of French legislation concerning primary education; it led to the fastest expansion of primary schooling in French history (Diebolt et.al. 2005; Squicciarini and Voigtlander 2016)—much more so than the 1880s Jules Ferry Laws which are the subject of Weber's (1976) often-cited work.

In Prussia, the evidence that rulers saw primary education as a means to indoctrinate the masses, and not as a policy to promote industrialization or urbanization, is even more com-



elling. Johann Felbiger, an education reformer and politician who played a key role in designing Prussia's compulsory primary school regulations under Frederick II, argued that schools' most important task was to "induce pupils to obey. They must be convinced that it is useful and correct to follow the schoolmaster's wishes . . . In this way, the schoolmaster accomplishes his most important task: his pupils will observe their duties not only in school, but throughout their lives" (Felbiger, cited in Melton 2002). Frederick II was concerned that educating rural children might lead them to "rush off to the cities and want to become secretaries or clerks." He wanted children to "be taught in such a way that they will not run away from the villages but remain there contentedly." To prevent migration, separate curriculums were established for rural and urban primary schools.

It is entirely possible that, despite the intentions of educational reformers, primary schooling did end up contributing to urbanization in 1760s Prussia or social mobility in 1830s France—it would not be the first time that education policies lead to outcomes that go against their intended goals (Fouka 2016). But we should not take these effects for granted, nor should we confuse the effects of a policy with its goals as envisioned by those who designed it.

Research puzzles that emerge: If political stability was a central goal of primary schooling under non-democratic regimes, the question that emerges is: Did it work? Under what conditions did primary schooling help promote political order and delay democratization?

History's challenge to the obsession with teacher unions

Popular claim: Another explanation of the variation we see in education provision patterns

stresses the role of teacher unions. A common argument is that in places where unions have the right to negotiate their working conditions through collective bargaining, they are able to obtain better salaries, lower class sizes, and more money for schools. Why? Because politicians will cave in to unions' demands to avoid public-sector strikes, workers' main form of leverage during collective negotiations (Freeman 1984; Moe 2005; Anzia and Moe 2015).

What I discovered: To quantify the effect of collective bargaining rights, I used official reports from the U.S. Department of Education to assemble a historical dataset of teacher salaries, class size, and education expenditures across states and over time. In the U.S., bargaining rights for teachers were introduced in 33 states during the 1960s and 70s. My [dataset](#), available at AJPS Dataverse, covers all 50 states from 1919 on (Paglayan 2019).

Graphing the data reveals a striking pattern: when we look at the evolution of average teacher salaries, class sizes, and per-pupil education expenditures in states where teachers were given collective bargaining rights in the 1960s and 70s, and compare it to states with no such rights, what we see is that collective bargaining rights were introduced in states that had exhibited higher teacher salaries and education expenditures since at least the 1920s; however, on average, the introduction of collective bargaining rights for teachers did not amplify these historical differences. This is what I show in "Public-Sector Unions and the Size of Government." Difference-in-difference analyses also suggest that, on average, the introduction of collective bargaining rights for teachers had no effect on their salaries, class size, or education spending—in line with what Lovenheim (2009) and Frandsen (2016) find using different datasets.



The null average findings make sense once we examine the political process that gave way to the public-sector labor laws of the 1960s and 70s. Recall that existing theories assume that teacher unions are able to make credible threats of strike during collective negotiations. However, in the U.S., this is not the norm. Most of the labor laws of the 1960s and 70s were designed to curtail unions' ability to strike. At a time when public-sector strikes had reached an all-time high, these laws introduced collective bargaining rights to appease teachers but they continued to prohibit strikes and established new costly penalties for striking such as suspension of collective bargaining, union decertification, monetary fines for unions, salary reductions for employees, and/or suspension of automatic dues deduction. Where collective bargaining rights came coupled with the ability to strike, teachers obtained a modest increase in education spending—but this coupling was not the norm (Paglayan 2019).

Research puzzles that emerge: Comparative politics scholars are in a unique position to examine how additional institutional features besides collective bargaining and strike rights affect unions' ability to shape policy, given the large variation in union regulations that exists across countries. Yet the most important implication of my work is that present-day educational patterns, at least in the U.S., predate the emergence of modern teacher unions. If we want to understand the present, we need to explain the past.

Conclusion: Learning history's lessons

Quantitative historical research has rapidly become one of the most exciting and promising developments in comparative politics and political economy. The attractions of this kind

of work are varied. Causal identification opportunities arise from the natural experiments that abound throughout history. Historical sources are often full of detailed information we would never dream of finding in present-day datasets. But, most importantly, examining the long history of the present-day issues we care about, using both quantitative and qualitative data, helps us ask better questions and develop better theories about these issues.

There are certain historical facts that we have neglected and need to start incorporating into our theories of education provision. Most of the expansion of primary schooling around the world took place before democratization rather than as a result of it. In 19th-century Europe, which gave birth to the notion of state-controlled primary education systems, political elites originally conceived of public primary schooling more as an indoctrination tool than a form of progressive redistribution. The history of public schooling matters deeply even if we care about present-day education; as my work on the U.S. highlights (Paglayan 2019), current differences in educational investment often go far back in time.

In comparative politics, historical research at its best entails a combination of methods (e.g. quantitative causal inference, process-tracing, text analysis) and the use of multiple types of data. These help us make better inferences. Getting data might require costly travel to another country, but this isn't always necessary. In my own work, I have relied heavily on the huge collection of primary and secondary sources available in U.S. libraries, the digital repository HathiTrust, and the growing digitized content of some national libraries and parliaments, to obtain most of the statistical data and parliamentary debate transcripts I have needed. Visiting



national libraries and archives has allowed me to access other kinds of data not available elsewhere, such as 19th-century school inspectors' reports, school textbooks, newspaper articles, special reports commissioned by the central government, and essays and letters written by ordinary parents, teachers, priests, and industrialists.

The challenge of doing historical research pays off. Our desire to understand today's world is

likely to benefit from the big-picture insights that a historical perspective offers. Most present-day issues, not just education issues, have deep roots. Getting to know those roots will likely change your understanding of the issues you care about. Be warned: it will probably also make you more realistic and humbler about what you can do to change the world, especially as an academic. ●

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WHEN DEMOCRACY IS BROKEN, ROLL THE DICE: Lotteries in Political Selection

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In many ways, it seems that democracy is in a state of crisis. Politics is viewed as elite-driven, where elected officials are increasingly wealthy, high status, or even corrupt. Systemic inequality combined with polarization raises concerns that large populations in society aren't being represented or are shut out of the political process. Voters feel as if they have no influence on policy, and trust in both government and fellow citizens is low; in 26 nations across the world, citizens are more likely than not to say elected officials don't care what ordinary people think (Pew 2019). In tandem with these issues, populist parties are successfully campaigning with appeals to bring governance "back to the people," and across the world the use of direct democracy initiatives such as referendums have significantly increased (sometimes with unanticipated results, i.e. Brexit or the Colombian peace agreement in 2016).

Are there institutional reforms that can improve how we select political officials? Lotteries have been proposed as one solution—either the completely random selection of political officials or candidates for office, or some incorporation of a lottery into the institutional rules of selection. Lotteries have been suggested as a way to reform parliamentary politics (Dowlen 2009;

Gastil and Wright 2019), such as the UK House of Lords (Barnet and Carty 2009) or committee selection in the supranational parliament of the European Union (Buchstein and Hein 2009). They are also now increasingly used for deliberative democracy in various forms (Van Reybrouck 2016), and have been a part of the policy debate in countries such as Canada, the Netherlands, Ireland, the UK, Australia, and the US.

There is a long tradition in political science and law that analyzes the benefits of lotteries in political selection (Manin 1997; Elster 1989; Engelstad 1989; Dowlen 2009; Duxbury 1999; Ober 1993 among many others). Most readers will be familiar with selection by lottery – also called sortition – where individuals are randomly chosen for political office. In the past, this was a key part of governance for polities in ancient Athens, as well as the medieval and Renaissance northern Italian city states. Today, modern applications of sortition can be found in jury selection for courts of law, and the growing use of citizens assemblies. Beyond just sortition, it is also possible to incorporate an element of randomization in political selection, which is called a lottery-based procedure. For example, a lottery could be used to select groups, who then elect or appoint an individual to office. While falling



Early institution builders recognized the value of lotteries and experimented with the use of lottery-based rules in governance.

short of a pure lottery, it retains some of its desirable features; as evidenced by the fact a number of European countries employed such rules during democratization in the 19th century.

Why lotteries? They primarily help overcome the “aristocratic effect” of election—namely that wealthy citizens are better able to cultivate the reputation and public speaking skills necessary for office, as well as employ personal and state resources to maintain their hold on power (Manin 1997). By design, lotteries can ensure more equal access to political office. While individual draws

from a lottery might not result in a group that is completely representative of the population, each individual has an equal chance of being selected. Even if politics is still dominated by elites, lotteries ensure more voices represented than would be otherwise by election (Stone 2009). Lotteries can also help prevent corruption by introducing uncertainty, for any element of randomization makes it more difficult for elites to coordinate or have undue influence over the selection process. We see similar theoretical expectations for lottery-based procedures (Cirone and van Coppenolle 2019).

Lotteries are also a handy tool for social scientists, for they can easily draw on tools of causal inference. Random assignment of political office provides an ideal experiment to estimate the effect of the office on policy outcomes or political careers. Even lottery-based procedures can aid in identification, because partial lotteries can be used with quasi-experimental research designs (Karpowitz and Mendelberg 2011; Cirone and van Coppenolle 2018). But what do we know about the uses of lottery in political selection?

As this essay will show, we can look to existing research to better understand the effects of lotteries, both by looking to the past and the present. I first review a set of notable uses of lotteries in history, by highlighting studies that use micro-level historical data and methods of historical political economy to study how lottery-based rules affected political outcomes. I then discuss modern-day research on citizens’ assemblies that construct randomly assigned deliberative groups, and use survey methods and other data to analyze their effect. Finally, I connect the study of the past to the present, and discuss what can be learned from both.

Political Lotteries in the Past

Early institution builders recognized the value of lotteries and experimented with the use of lottery-based rules in governance. Here, I discuss two notable cases from which we can learn — Florentine Republic and the French Third Republic. In both cases, there was a fear that wealthy and influential elites would dominate the political process, at the expense of minorities (either citizens or political groups). Lotteries were also used for their anti-corruption effects, in periods of high uncertainty where political institutions were rapidly developing; in this case lottery-based procedures can play a “sanitizing role” (Stone 2009).

In the 14th century Florentine Republic, politics was dominated by networks of elite families who had vast influence over the political and economic systems. Abramson (2019) exploits a lottery-based selection rule in Florence from 1382-1434, in the context of an institutionalized oligarchy with limited franchise based on membership of occupational guilds. The Priorate, or the executive body, and its leader was chosen



by a rule that combined features of election and lottery. The subset of eligible candidates was voted on within a specially designed committee, and then the city's executive was randomly drawn from this pool of successful candidates (weighted by guild quotas). This also allows for the estimation of the causal impact of leader preferences on policy, and the study shows that the economic interests of the leader determined the currency pricing at the time.

But perhaps more importantly, it also shows that the process was, in fact, random and uncorrupted, and successfully ensured diversity in representation. In Florence, overall the lottery-based procedure, combined with frequent redrawing of the executive, meant that both low and high guild members were both ensured a fairer shot at power, and prevented from coopting and consolidating too much power overtime. This was well known to political theorists in this era – as McCormick (2010) notes, Machiavelli advocated for lottery-based selection rules to ensure the participation of a wide range of society in government, and Guicciardini claimed that even an element of randomization in the process helps attenuate elite bias (pure sortition was not necessarily needed). Here, the Florence case also shows us that random election to office had an educational mechanism; Machiavelli argued that common citizens became more informed about politics after serving (Machiavelli, of D I.47).

The French Third Republic (1870-1940) also employed a lottery-based procedure, in order to select members of the powerful budget committee in the Chamber of Deputies. In Cirone and van Coppenolle (2019), we show this selec-

tion rule was strategically used to prevent the capture of early institutions by party factions or groups of self-interested political elites, during the uncertain period of democratization. In this case, the chamber was divided into eleven randomly assigned groups; each group then met to elect three members to the committee. While falling short of pure sortition, randomly assigned groups in the first stage of the selection process helped even the playing field. This resulted in the appointment of young, skilled, middle-class deputies at the expense of influential elites. Further, once parties became stronger, they eliminated the lottery-based procedure and elites once more captured key posts. This result also travels; lottery-based procedures were used to select members of the Constitution Committees in both France in 1789 and Denmark 1848, and this selection rule once again emphasized skill over elite status (Cirone and van Coppenolle 2019). Thus, the innovative use of lotteries helped involve broader range of politicians in the policymaking process.

Political Lotteries Today

Today, lotteries are being used in initiatives of “deliberative democracy,” designed to involve a cross-section of citizens in a dynamic form of policymaking (Gastil 2018). It's incredible to see the sheer number of such initiatives being incorporated, in every region across the world; interested readers can access the searchable database of public participations at www.participedia.net/. One example is that of citizens assemblies, which consist of a randomly selected group of individuals¹, typically stratified across basic socio-demographic categories, tasked with debating an issue and developing a

1. Note this can be as a result of random sampling by government or private firms with replacement, or random selection from those who signal interest in participation; it varies across cases, and theoretical expectations would also differ depending on selection and attrition rates, see Gastil 2018 or Karpowitz and Mendelberg, 2011.



recommendation. Members are given balanced information, learn about the issue, and then have a series of facilitated meetings that involve discussion and policy formulation. Topics debated can be specific issues, or even significant institutional reforms, such as constitutional amendments or electoral law change.

Here, the benefit of selection by lot is clear – participants engage with a wider cross section of society, and in particular viewpoints they may not encounter otherwise. Studies of deliberative democracy have shown that it can help promote learning across divided societies, can negate the negative effects of polarization, and help citizens navigate misinformation and manipulation (Druzek et al 2019). Participants of deliberative activities are also more likely to willing to participate again, which perhaps could have spillover effects to other forms of democratic participation (Gastil et al. 2010; Gastil 2018). Random selection also insures that such participation in such initiatives aren't necessarily dominated by narrow or organized interests (as some events open to public participation are likely to be).

Scholars are still exploiting the experimental features of citizens assemblies, and the effects of participation in any type of deliberative exercise on participants or policy do vary by the context and the institutional design. Still, there is increasing evidence supporting the positive effects of deliberation. As part of a pilot of a citizens' assembly in Ireland in 2011 run by We the Citizens, an independent national democratic participatory initiative, scholars designed a quasi-experiment to separate the effect of informa-

tion from the impact of deliberation on opinion change (O'Malley, Farrell, and Suiter 2019). One group of interested participants just received informational materials about the issues being discussed, while another group received this information plus the opportunity to deliberate in a group setting. As expected, the authors found opinion change is more substantial in the deliberative setting.

The momentum behind citizens assemblies derives from idea that modern politics is perhaps broken and they are often advocated for alongside initiatives of direct democracy, such as referendums. Ireland is also an exemplar case for the use of both. Ireland convened two randomly selected citizens assemblies² – first, during the Convention on the Constitution in 2012-2014; and second, in the Irish Citizens' Assembly from 2016-2018. Members were asked to discuss a number of national policy issues, and notably both assemblies played an important advisory role in recommending constitutional change on abortion and marriage equality (Farrell 2019). The Irish case is also significant because both assemblies were key in calling for national referendums, and thus it linked deliberative democracy with direct democracy. Referendums suffer greatly from a lack of misinformation or biased information (Qvortrup 2019); citizen assemblies can potentially alleviate this shortcoming and improve the referendum process, before the vote occurs.

The United Kingdom has also recently experimented with citizens assemblies to tackle the notoriously complicated issue of Brexit. In 2017, a team of scholars in the UK and the Constitution

2. A professional market research company selected 99 regular citizens at random, but stratified based on four demographic targets: sex, age, social class and region. In the former, one third of the convention's members were politicians and the rest citizens; in the latter, it was entirely members of the public.



Unit at UCL facilitated the Citizens Assembly on Brexit, which brought together 50 randomly selected citizens (again, stratified over baseline demographics and EU Referendum vote choice) over two weekends to deliberate potential outcomes for post-Brexit. The assembly resulted in a report outlining the body's opinions, but scholars also implemented surveys to track attitudes before and after the process (Renwick et al 2019). Members saw some opinion shifts on EU trade and immigration, but more importantly members improved their understanding of the issues over time (as measured by their self perceptions and facilitators). Similar pilot initiatives are being undertaken in other parts of England, shedding more light on how to incorporate consultation in policymaking (Prosser et al 2019).

Today, there is great research potential for the study of any citizen's assembly or similar initiative using sortition or lottery-based selection. Effective experimentation typically requires a high level of researcher control – here, a researcher can exploit built-in randomization to actively innovate in studying both the effects of the assembly on the citizen, and the potential of the exercise to improve democratic policy or practice. Research teams can also experimentally manipulate features of the design – from the group members to the type of facilitator to the deliberative activities – which greatly adds to our ability to take an evidence-based approach to develop this as a democratic institution (Karpowitz and Mendelberg 2011).

Linking the Past and the Present

It is true that the very nature of political selection has both changed over time and varies significantly across cases; the rationale behind lottery-based procedures in the Florentine

Republic versus the 2016 Irish Citizens Assembly on abortion is driven by a myriad of different political, social, and economic conditions. Further, research questions involving political selection involve analysis at the level of the individual – from the chosen politician's characteristics to their resulting behavior in office. This often requires detailed data and analyzing the micro-foundations of each historical usage, and perhaps results in the interpretation of findings on a case by case basis. However, there are also generalizable incentives and behavior created by the use of lotteries, regardless of the case. For example, we know lotteries undermine elite coordination and capture – while the definition and types of elites in power change from the past to the present, this general prediction should hold across the various studies. For now, there are distinct benefits to a comparative approach.

We can also exclusively use historical cases to analyze the effects of lotteries at high levels of government, namely legislative office or governing assemblies. Today, this is almost impossible to replicate – researchers are unlikely to convince a modern parliament to randomly select its members or change its selection rules, because political parties and other organized groups have a vested interest in controlling political selection. However, thanks to innovations in early democratic republics who incorporated lotteries, we can see how they changed political incentives in the actual process of governing.

Further, there are important policy implications in the difference between the historical use of lotteries in parliaments, compared to modern use of lotteries in local citizens' assemblies.





Randomized selection machine (kleroterion), third century B.C.

Source: The Athenian Agora Museum, Greece;
http://www.agathe.gr/democracy/the_jury.html

For example, recent studies have found that deliberation in randomly assigned groups can help moderate opinions and mitigate polarization³. But should we expect these results to travel to modern day legislative behavior?

While the behavior of legislative bodies could be somewhat replicated in laboratory experiments or focus groups, the stakes are higher in real life institutions. In our work on the 1848 Danish Constituent Assembly, we look at the fact that members were assigned to random groups in order to discuss policy issues relating to the Constitution (Cirone and van Coppenolle 2019). As a result, the composition of policy deliberation is exogenous (for example, the number of Conservative or noble assembly members in a group). Using micro level data on legislator biographies, experience, and roll call voting during 1848-1849, we can look at this composition to see how it affected voting behavior (both a member's ideal point, and likelihood to

vote with their party). During the Constituent Assembly, we find no statistically significant effect of group composition on voting behavior; the main driver of policy decisions remains party affiliation. While only one case, this might suggest that the moderating effects of randomly assigned groups are larger in local and more advisory contexts. But only by exploiting historical cases, where randomization was used in parliaments, can we perhaps better identify potential scope conditions on the modern-day usage of lotteries in citizens' assemblies.

Conclusion

The element of chance in a lottery has always captured our imaginations.⁴ Yet from a policy perspective, lotteries are now being proposed in various forms to address democratic deficits. Lottery-based selection of high-ranking politicians have been suggested for the national parliaments of the UK and France, as well as for the supranational institutions of the European Union. Citizens assemblies have been implemented in a wide range of countries, at both the local and national levels (Fishkin 2011).

However, lottery-based political selection is no panacea. There are a number of shortcomings to these processes. First, no matter which selection rule, it is likely that elites can still be disproportionately involved in politics, and lotteries don't insulate all democratic institutions from partisan or corrupt pressures. Second, politics benefits from investment in expertise and career politicians; the uncertainty inherent in random selection of permanent institutions

3. See Druzek et al 2019, Gastil et al. 2010, O'Malley, Farrell, and Suiter 2019, Renwick et al 2019.

4. One only has to read Jorge Borges' "Lottery in Babylon," a dystopian short story about how a simple government lottery grows to dominate all aspects of civilian life; or the 1971 novel by Luke Rhinehart called "The Dicer," where the main character takes all life decisions by rolling a dice.

could disincentivize potential candidates from acquiring skills or experience. Alternatively, problems with recruitment and attrition from selected citizens will always be an issue with lottery-based selection; and randomly chosen officials might lack democratic legitimacy, which could impair their ability to do their job well. Third, even implementing lotteries in the form of temporary citizens assemblies require time, resources, and careful design of the process. Lotteries are also difficult to endogenously implement, particularly at top levels of governance

— political parties and other groups are too invested in current systems of selection, so it is unlikely we will see a return to the pure sortition of ancient times.

Still, there is distinct promise to the use of lotteries in political selection, to help include more citizens in the democratic process. By examining unique institutional experimentation in the past, and by adapting democratic initiatives based on more recent instances of lottery-based selection, it may be possible to alleviate current democratic shortcomings. ●

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INTERDISCIPLINARY TRAVELS

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Peter L. Berger, renowned for his sociological research on how humans construct meaning, once referred to the historian as the “one traveler whose path the sociologist will cross more often than anyone else’s on [their] journeys.” He then cautioned that “the sociological journey will be much impoverished unless it is punctuated frequently by conversation with that other particular traveler” (Berger 1963, 20).

As our discipline, alongside the broader social sciences, strives to satisfy increasing demands for quantification, these words inspire my consideration of how historical sociologists have maintained our ties to our cherished fellow travelers in history. They also inform my search for similar and dissimilar trends within our other kindred discipline, political science, and how its practitioners approach the past through historical research. Below, my reflections suggest the rough outlines of a path toward interdisciplinary and multimethod learning.

Outcomes and Processes

Why did X happen when Y was expected? Why here and not there? Why then? Political science is a puzzle-driven discipline peopled by scholars who tend to be motivated by “deviant”

cases. To solve the puzzles, the political scientist-cum-investigator compares the deviant with the “normal,” testing hypotheses, identifying variables, and otherwise seeking to explain varying outcomes. Hence, puzzle-posing and puzzle-solving are a sort of variation on Mills’ methods of difference and agreement: experimental in inspiration and driven by an outcomes-based logic of inquiry. As a result, when applied to historical research, the method is unavoidably a process of reverse-engineering, of back-casting, that moves from an outcome back to a point where an explanation begins to form.

Three recent, excellent examples of puzzle-based historical political science are Laia Balcells’ *Rivalry and Revenge*, Evgeny Finkel’s *Ordinary Jews*, and Lisa Blaydes’ *State of Repression* (this list is inflected, of course, by my own interest in political violence). The first questions why, in conventional civil wars, armed groups target civilians in some local settings and not others. Balcells combines archival documentation from the Spanish Civil War with quantitative statistics generated from this data. Then, like any up-to-date political scientist, she includes a brief analysis of an outside comparative case—here, Côte d’Ivoire—by drawing on lo-

Two other trends have arisen in sociology in recent years, pulling us away from the tidy, outcomes-based methodological positivism that undergirds puzzle-seeking strategies.

cal-level voting returns and information on non-combatant deaths. Balcells argues that citizens are targeted by armed groups in conventional conflicts when ideological commitments pre-dating the onset of war meet desires for revenge that intensify with ongoing violence.

Finkel's book asks why some Jews, confined to ghettos during the Holocaust, chose to cooperate and collaborate with Nazis, while others chose to cope and comply, evade, or organize resistance. What, he investigates, led to such varied responses? The threats and violence in each case were similar, but, he finds, pre-war political activism, which inevitably was tied to Jews' pre-war social integration and thus states' pre-Holocaust political regimes, shaped decisions to select some strategies over others. Finkel's data sources include over 500 survivor testimonies produced by 8 different organizations as well as published memoirs, primary and secondary sources published in four different languages, and three quantitative datasets: the Jewish Ghettos Dataset, the Zionist Elections dataset, and the Polish National Elections dataset.

Blaydes, too, analyzes archival material to tease out a puzzle: why did some Iraqis comply with Saddam Hussein's violent autocracy while others resisted? Using Ba'th Party Archives' documents captured by the US military in its 2003 invasion, Blaydes quantifies even School Registers once used by the regime to examine high school students' fitness for Ba'th Party recruitment as a way to evaluate the regime's ruling strategy, then works to map the chronol-

ogies and geographical distributions of that most tantalizing of data: rumors.¹ Her results reveal how the regime treated its Sunni, Shi'i, and Kurdish citizens differently in different places, shaping Iraqi's political identities in turn, but because these dynamics varied across contexts, Iraqi society at large cannot be neatly divided along sectarian lines. In turn, Iraqi communal identities cannot explain their behaviors under Hussein's dictatorship; rather, their behaviors were closely tied to the regime's actions—namely, its distributive and punitive policies.

The methodological pattern across these three books is exemplary of current historical political science more generally. The scholar identifies a puzzling variation in outcomes, then seeks out the factors causing such puzzles through a crafty combination of historical qualitative data, quantitative data, and the quantification of qualitative data. This approach is common to sociology as well: recent examples from historical sociology include Andreas Wimmer's "Nation Building: Why Some Countries Come Together While Others Fall Apart," Robert Braun's "Protectors of Pluralism: Religious Minorities and the Rescue of Jews in the Low Countries During the Holocaust," and Patrick Bergemann's "Judge Thy Neighbor: Denunciations in the Spanish Inquisition, Romanov Russia, and Nazi Germany."

Yet two other trends have arisen in sociology in recent years, pulling us away from the tidy, outcomes-based methodological positivism that undergirds puzzle-seeking strategies. First, the revision of comparative-historical sociology away from outcomes-focused work and toward comparisons across sequences of meaningful

1. These are but two examples of Blaydes' innovative methodological strategy.



action; second, the surge of scholarship in cognitive cultural sociology. The first is motivated by the “post-positivist” turn (Lichterman and Reed 2015; Reed and Lichterman 2019) and aims to focus not on varying outcomes, but on causal mechanisms that inhere across cases to *produce* outcomes of interest (Mayrl, n.d.). It argues against the identification of empirical similarities and differences across cases, insisting that because there may be multiple pathways to the same outcome, empirical similarities cannot, on their own, reveal casual mechanisms.

Relatedly, cognitive cultural sociology has influenced historical sociology through a growing recognition that individuals’ actions are often motivated by a mix of unconscious habit and explicit reflection (Type I and Type II thinking in dual-process models of cognition, respectively), and that these can change throughout the course of a single “event” (in quotations because events, too, are processes). Against the messy reality of history, these pioneering scholars also follow a venerable lodestar: variables-based logic cannot adequately make sense of how shifting geographic and temporal contexts influence cognition, therefore the historical sociologist must privilege interpretive analysis over attempts to impose control and order over history. Instead of applying elegant positivist designs to archives, it asks: how do actors’ meanings and interpretations of actions and interactions shape their subsequent actions and interactions, and how do these chains of action produce outcomes?

These approaches, in contrast to the puzzle-posing model, are forward-thinking. They also draw creatively on ethnographic research procedures, rather than rely on the methodological positivism inspired by experimental

research procedures to guide archival analysis. A prime example is Isaac Reed’s (2016) comparison of the Salem Witch Trials and the Whiskey Rebellion. Despite their differences—a witch hunt is not a rebellion—both, Reed argues, are examples of cases where actors struggled to make sense of crisis. The comparison sheds light on knowledge production in times of uncertainty.

In Western PA, the breakdown of sovereign order coupled with Hamilton’s attempts to impose an excise tax on whiskey led to violent rebellion. But what did the rebellion signify? Four muddled interpretations emerged, creating a “thematization” of the conflict whereby interpretations of the rebellion were as confusing as the crisis that caused it. Ultimately, however, the “Philadelphia Interpretation” won (Reed explains why) and directed the state’s action in response. Salem, however, was different: coherent ideological responses to political, legal, and religious uncertainty “fetishized” the crisis rather quickly by displacing the population’s anxieties onto women as scapegoats. The result was “‘the crisis’ became ‘the witch crisis’” (154; emphasis mine). Reed’s analysis reveals how actors’ interpretations of events were cognized as they were happening, how these cognitions shifted over time and why, and how they ultimately shaped action. Interpretive explanation, not yes/no outcomes, guide his analysis.

Replication and Reflexivity

Scholars studying the past necessarily introduce their audiences to their data sources. Increasingly, political scientists and sociologists not only present the “what” but also the “why”—which sources have been used in which ways? What biases influence the construction



and function of these sources? How has the scholar's positionality affected their choice of, access to, and interpretation of these sources?

In the first endeavor, political scientists are more tidily systematic. A strict division between theory and evidence simplifies things, with certain sources employed as theoretical starting-points and others as evidence to adjudicate among competing explanations. Historical sociologists tend to blur such distinctions and are relatively unlikely to discuss the specifics of their methodological practices. Yet as Damon Mayrl and Nicholas Hoover Wilson (2018) note in their analysis of 15,256 in-text citations from 37 award-winning publications in comparative-historical sociology, most historical sociologists use their sources as both theory *and* evidence, and both constructively *and* critically, though in different ways—what they call “methodological architectures.”

For example, the methodological architecture called “The Theoretical Frontier” tends to privilege the constructive use of theoretical citations, which are then pragmatically combined with secondary historical sources to build a case. In contrast, the methodological architecture termed “The Sociologist as Historian” tends to rely on extensive and detailed primary archival research, which more often than not results in findings that emphasize historical complexity rather than grand, sweeping arguments. The two other strategies are “Macro-Causal Analysis” and “Data-driven Theorizing.” Political scientists, I find, trend towards the latter two approaches, and with data-driven theorizing in particular, they often engage in explicit discussion regarding data collection and methodology.

Historical sociologists are much less likely to do so regardless of architectural strategy (Mayrl and Wilson 2018, 14). The result is it can be hard to trace how historical sociologists toggle between primary and secondary evidence and theory—a dilemma made more difficult when the same author uses the same source in more than one way in any given work, for example as corroborating evidence for an argument at one point but critiqued as theoretically flawed at another.²

There is, of course, nothing wrong with citing the same source for its evidence as well as for its theoretical argument, nor to argue in support of one while challenging the other. Sociologists and political scientists adjudicate among competing theories using the same data all the time. We critique data as flawed yet also useful. And, despite historical political science's tendency to draw stronger distinctions, such scholars certainly conflate theory and evidence as needed.

Still, the greater quantification we find in political science has led to more explicit discussions in historical work of data collection and methodology and sociologists would do well to hew to this trend. Replication is the very essence of science (political, social, or physical) and it should be possible for social scientists of any discipline to visit the same archives and read the same texts, and clearly determine how a given scholar arrived at their theory. Where there is disagreement, it should be straightforward to untangle another scholar's evidence and logic. These informed arguments can be nothing but good for research as a whole, and I believe sociology has a great deal to learn from political science in this regard.

2. A related dilemma is the potential for mismatch between data collected and theoretical strategy employed. For an example of this critique applied to recent historical research in sociology, see Elizabeth Popp Berman (2019).

On the other hand, sociologists are developing two practices that should be adopted by our fellow travelers in other disciplines. The first involves scholars' responsibilities to interrogate the political construction and symbolic implications of archives, and the second serious discussion of scholars' positionality when conducting historical research. Below, I briefly summarize two emerging developments from these efforts.

The institutional field of archives includes archivists, curators, users, and professionals who keep them going, but it also extends to more elusive actors such as philanthropists, donors, NGOs, governments and other political authorities. These actors have interests, beliefs, and ideas, and the archives they leave are politically built. We must ask whose history it is organized to preserve, whose boundaries are enforced by this body of records, and whose history has been excluded. Going further, we can ask who first envisioned the archive, who funded it, why it was sited in one place and not another, and who serves as its gatekeeper.³ These questions are unending—and important. The political construction of the archive shapes how history is gathered and contained, then quantified over time.

Relatedly, although archives are often thought of as neutral sites that contain objective evidence about time-periods passed, symbolic decisions of categorization and classification are embedded in the very production of the documents contained in the archive in the first place. As the archive collects and accumulates documents portending to portray history as it occurred, it simultaneously chooses to include

some voices as representative of the past while excluding, and therefore *erasing from history*, the voices of others. Sometimes, decisions to gather evidence from some kinds of people and not others are intentional—these voices are valid and count for the history we want to tell here, these voices do not. Yet other times, decisions to gather evidence from some kinds of people and not others are unintentional and reflect ways of seeing and dividing the world at particular moments in time. In both circumstances, sociologists suggest that scholars must consider how the documents contained in archives always reflect inequalities in who gets to tell their story by having their words and images institutionally preserved.

Understanding that archives are never neutral forces introspection about the scholar. Hence, sociologists are increasingly reflecting on how their own positionality shapes the collection and interpretation of archival data. Archival access, as with access to any kind of evidence, depends crucially on social, cultural, and economic capital. And, as a wealth of social scientific research shows, people with more capital in particular fields are more likely to successfully navigate, benefit from, and succeed in related others. This maintains for archival research. It is not enough to simply know one's case and the relevant language: one must also possess the right social and cultural characteristics, connections, and resources to access an archive and its contents.

Finally, when it comes to archival analysis, the interpretation of evidence is also shaped by positionality. Upon entering an archive, only sometimes is it clear precisely which documents will

3. I thank my colleague Karida Brown for her insights on this point.

help answer a question and, more often, the researcher is faced with a large body of documents containing information on various, and sometimes conflicting, behaviors, correspondences, interactions, claims, and value statements. It is difficult to know what documents best answer a question of interest or most accurately reflect the “truth” of what caused a particular phenomenon. Inevitably, then, our social positions shape what we do or do not notice in the materials—what voices, perspectives, and stories are or are not included—as well as how we weigh conflicting evidence in order to develop theoretical arguments. Critical reflection on one’s own position in relation to the archive is thus necessary for any serious discussion of historical research methods and analysis.

To give three examples based on the texts mentioned earlier, Balcells’ (2017) book is powerful in part due to her emphasis on politics and emotions as significant for explaining violence against noncombatants in civil wars. She is able to emphasize these aspects of conflict due to the kinds of historical data she collected—Spanish national and local archives and memoirs, which provide qualitative evidence in support of her argument that rivalry drives violence early in civil war, while revenge explains violence later.

Yet her comparison of Spain with the civil war in Côte d’Ivoire relies mainly on secondary sources: to examine direct violence against civilians, Balcells builds a dataset that combines various human rights organization reports (Balcells 2017, 165). This in and of itself is not problematic—scholars, especially those who study violence, frequently use such evidence to examine patterns in conflict. But as Balcells herself mentions in a special issue on conflict archives with Christopher M. Sullivan, these sources tend

to privilege easily observable acts of violence, leading us to know significantly more about the urban core of conflicts and visible acts of violence than about conflicts on the periphery and clandestine operations throughout war (Balcells and Sullivan 2018).

On the other hand, conflict archives can be subject to their own biases, including with regards to their availability as evidence of violence can be hidden, destroyed, or otherwise manipulated by conflict’s victors, or strategically released for political purposes (more on this below). Given Balcells’ sensitivity to these concerns, it would have been helpful to read about the promises and pitfalls of comparing Spain and Côte d’Ivoire—two very different cases—with two very different kinds of data. I suspect the insights garnered from a comparison not just of the cases but of the data used to examine them, including how the evidence in each case was originally collected and organized and how this might have shaped the results, would be insightful for future scholars of violence.

Finkel (2017) is rare in that he provides an extensive appendix wherein he discusses the construction of his various sources, including the different political contexts that may have shaped the kinds of information provided in the oral testimonies that undergird part of his analysis (Finkel 2017, 199-207). He is also upfront about his personal connection to the history he analyzes (Finkel 2017, 18-20). Two aspects of the study that I keep thinking about, however, relate to my own struggles researching the Holocaust as a grandchild of survivors. First, how does he think his biography shaped the kinds of questions he asked and attended to in his research, and what kinds of issues might he have overlooked as a result? Second, given that he constructed the largest existing data-

set on Jewish ghettos in interwar Poland using previously unseen documents from the United States Holocaust Memorial Museum, how does he think his positionally influenced his ability to access these documents and how might future scholars interested in doing original research on the Holocaust (or on other instances of violence for that matter), but perhaps without the same social connections, be able to access such sources?

Finally, Blaydes (2018) examines Ba'ath Party Archives captured by the U.S. Military during the 2003 Iraq Invasion. Numerous archivists have written on the troubling ethical dilemmas involved in collecting and disseminating these documents (e.g., Caswell 2011; Cox 2011; Montgomery 2012). Blaydes also relies on documents and first-hand testimonies produced by the Iraq Memory Foundation between 2003-2008, the latter of which aired on the *al-Iraqiyya* public television network. This evidence is troubling from an empirically practical as well as an ethical perspective: Iraqi exile Kanan Makiya who formed the Iraq Memory Foundation collaborated with President George W. Bush's administration to produce and disseminate these testimonies, as well as other evidence of Saddam Hussein's violence against Iraqis, to justify the Iraq War to Americans *and* to Iraqis themselves. The goal of the Iraq Memory Foundation archive, then, was to "powerfully impart the brutalities of the former regime to the public and scholars" (in Alshaibi 2019:292). Blaydes does not discuss the potential problems involved in working with these sources and whether, as a result, there might be significant social and political biases in her findings. She asserts that the data she relies on for her analysis "are not attitudinal, but based on...actions of individuals, as collected or documented by the regime's single party," but this does not, in my estimation, adequately ac-

count for the fact that the archives were intentionally constructed to justify US military intervention in Iraq (Blaydes 2018, 12). The data may reflect observations of actions, but the data themselves were organized and compiled for violent and controversial political purposes. This merits further discussion.

Conclusion

Sociologists' lives may be impoverished if they leave their historian peers behind but, as I hope this essay demonstrates, we have much to gain by attending to our peers' historical research in political science and vice versa, as well. The emphasis on puzzling outcomes that drives much of political science has resulted in some of the most exciting and innovative work in recent years. But, sociologists caution, desires for methodological positivism must not cause us to lose sight of the significance of meaning and interpretation. Among other reasons, this is because the search for empirical patterns across cases can elide the important fact that behind any outcome, multiple mechanisms are possible.

Likewise, attention to heuristics should compel even more methodological precision concerning historical data collection practices. The messy reality of theorization—especially when toggling between archival data and analysis—can make replication especially difficult compared with tidy variables-and-outcomes-based designs. Subsequently, the process of creating, preserving, archiving, and accessing evidence should be central to methodological discussions rather than ignored, and issues of potential bias should be emphasized rather than elided. Each of these lessons emerges when considering our disciplines' similarities and differences, and each suggests potentially innovative approaches to historical social science moving forward. ●



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STATE CAPACITY, ECONOMIC DEVELOPMENT, AND THE ROLE OF HISTORY

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There are major differences in income levels across modern-day nations. Explaining such differences is a key challenge for the social sciences (Acemoglu 2009, 3-8). Modern levels of state capacity also differ dramatically. Understanding why is another major social science challenge (Besley and Persson 2011, 1-2). Indeed, there is a striking relationship between the two phenomena: *Figure 1* shows a strongly positive correlation between the tax/GDP ratio – a basic measure of the state’s extractive capacity – and per capita GDP.

The study of history can significantly improve our knowledge of the relationship between state capacity and economic development. First, at a fundamental level, the development process concerns the institutions in society that structure incentives for political and economic interactions (Acemoglu et al. 2005, 388; North 1990, 3). This process, moreover, is dynamic by nature (Bates 2017, 2). True knowledge of the development process therefore involves the analysis of institutional change over a long time horizon. Second, practically speaking, only a small number of nations (e.g. South Korea) have joined the developed world since World War II (Bates 2017, 2-3, 115-16). Taking a historical perspective enables in-depth study of the long-run process of development both within and across nations. Third, over the past decade, there has been a technological revolution in comparative politics – drawing in part on the established literature in economic history – in both the construction of original historical databases and the use of rigorous statistical methods (Stasavage 2014a). Historical research is thus well-placed to provide rich new answers to many of the most profound debates in comparative politics.

In this essay, I offer a brief take on the relationship between state capacity and economic development in historical perspective, relying

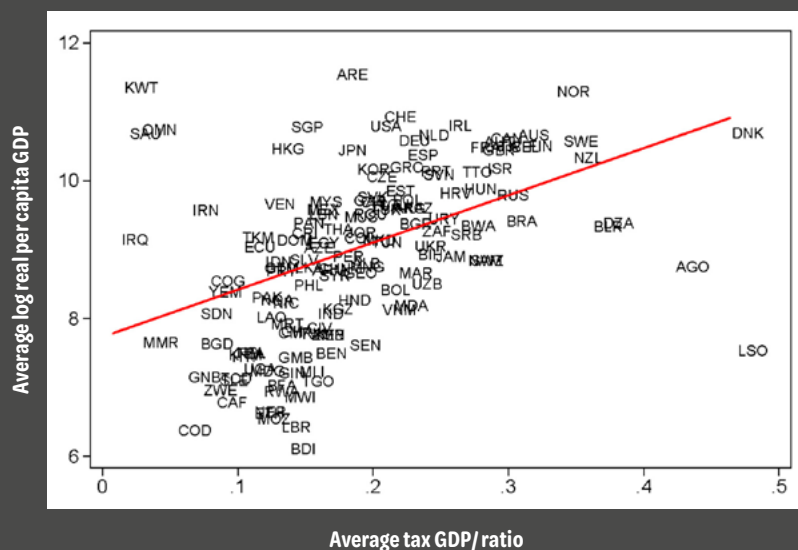


Figure 1:
Total Taxation and GDP Worldwide, 2000-2009

Historical research is well-placed to provide rich new answers to many of the most profound debates in comparative politics.

heavily on the material in Dincecco (2017). First, I put forth a conceptual framework that highlights the government's challenge to exert proper authority over both its citizenry and itself. Next, I evaluate long-run state development in Western Europe – the birthplace of both the modern state and modern economic growth. I argue that greater state capacity promoted economic development in this context. Finally, I characterize the basic historical features that helped make the state development process in Western Europe different from other world regions, and describe new trends in research on this topic.

Rules of the Game

To start, there are several channels through which the state may help promote economic development. They include the ability of the state to provide a free and competitive market for domestic exchange, transportation infrastructure, and mass education. For brevity, I focus on the state's ability to provide what North (1981, 24) calls the basic rules of the game: law and order, private property rights, and external defense. By reducing the likelihood of expropriation – whether by thieves, a predatory state, or a rival nation, the state's provision of the rules of the game can incentivize individuals to make private investments – in physical capital, education, and/or innovations – that support the development process.

Effective Statehood

Following Mann (1986, 113), I define *state capacity* in terms of the national government's ability to accomplish its intended policy goals – economic, fiscal, or otherwise. By *effective statehood*, I mean the political arrange-

ments that enable the government to best accomplish such policy goals. Thus, effective statehood will enhance state capacity. My conceptualization of effective statehood follows Madison's (1788, 257) classic statement regarding the government's dual challenge to exert proper authority over both the citizenry and itself. Several prominent arguments about effective statehood – including Fukuyama (2004, 21-6), North et al. (2009, 21-5), Besley and Persson (2011, 6-7), and Acemoglu and Robinson (2012, 79-81) – highlight political conditions akin to those of Madison, lending credence to his observations.

I conceptualize the state's ability to exert authority over the citizenry in terms of its control over taxation. Fiscal strength is central to state power (Levi 1988, 2). Furthermore, historical fiscal data are systematically available, allowing for comparisons across time and place. Specifically, the national government must have both the political authority and administrative ability to implement a standard tax system with uniform tax rates throughout its territory. Without fiscal centralization, revenue will be small due to local tax free-riding, reducing the state's ability to effectively accomplish its policy goals. The process of fiscal centralization took several hundred years in Western Europe. Thus, Weber's (1946, 78) classic definition of the state in terms of its monopoly over violence does not make much sense for historical institutional analysis, since it represents the outcome of a hard-fought process rather than a starting point (Hoffman 2015, 306-8).

The government's ability to exert authority over the citizenry is necessary for effective statehood, but not sufficient. Even if fiscal centralization enables the state to gather more revenue, there

is no guarantee that it will spend the new funds in growth-promoting ways. I conceptualize the state's ability to exert authority over itself in two parts. The first concerns institutional impartiality with respect to fiscal matters. There must be an institutional player within the government – think parliament – that has the formal and permanent authority to monitor public finances at regular (i.e. yearly) intervals. The second concerns the distributive politics behind parliament's fiscal role: to be an effective monitor, this charge must actually be in the interest of influential parliamentary groups. If this condition does not bind, then parliament's formal fiscal authority will not have much bite. By improving the government's ability to productively spend funds, parliament's fiscal supremacy should further promote revenue gathering, given the logic of fiscal contracting (Levi 1988, 52-67).

Overall, if the state succeeds to exert proper authority over both the citizenry and itself as described above, then it should not only be able to gather enough revenue to accomplish its policy goals, but should spend funds in ways that support the development process (versus wasteful spending). Thus, the state will be effective.

Historical Roots

I now analyze the historical relationship between state capacity and economic development in Western Europe. The starting point is the aftermath of the ninth-century demise of Charlemagne's Empire. This demise resulted in long-lasting political fragmentation marked by instability and warfare. Indeed, warfare is a common explanation for institutional change (e.g. Tilly 1992, 67-95). To secure new revenue for military purposes, rulers were willing to establish both local freedoms (e.g. urban self-govern-

nance) and national parliaments, thereby granting elite taxpayers formal roles in policy-making.

The historical bedrock of effective statehood is to be found in medieval city-states rather than territorial states. Due to compact size, city-states were more likely to establish fiscal centralization. Similarly, due to low communications and travel costs, parliamentary elites were more likely to meet frequently. City-states were the first to establish long-term public debt, and could borrow at more favorable terms than territorial states (Stasavage 2011, 77-93). Effective local governance, moreover, has been linked with greater technological innovations and economic development in European history (Mokyr 1995; van Zanden et al. 2012; Stasavage 2014b; Cox 2017; Dittmar and Meisenzahl 2019).

By the early sixteenth century, many nation-states had territorial borders that resembled their modern borders. However, they did not satisfy any of the above conditions for effective statehood. Contrary to the conventional wisdom, national rulers were generally weak. Due to political fragmentation and warfare, they often granted partial control over governance to local elites in exchange for new funds to be put toward the military. Such bargains, however, enabled city-states to obstruct later national-level centralization efforts. Early modern nation-states are therefore properly viewed as "mosaics" constructed on a medley of traditional local institutions (Strayer 1970, 53). In early modern Europe, there was a military revolution, making battlefield success more dependent on high revenue (Gennaioli and Voth 2015). Local institutional fragmentation, however, made it difficult for national governments to extract more funds. Local elites were inclined to oppose national-level fiscal reforms that threatened



their traditional tax rights, since control over taxation was a fundamental part of self-governance. Given state weakness, national rulers could not simply impose a standard tax system with uniform tax rates throughout their territory, but were forced to bargain region by region over local tax rates. Local elites hoped to free-ride on outside tax contributions, paying less while other regions took up the slack. Since elites across all regions acted in this manner, national governments could only extract low revenue per capita.

Fiscal centralization was a long and difficult process. In 1660s France, for example, Finance Minister Jean-Baptiste Colbert undertook fiscal reforms. His “success” was to divide France into “only” eight tariff zones. Within the largest customs zone, moreover, there were still five local tariffs.

Fiscal centralization typically took place from the time of the French Revolution onward. French military conquests were key catalysts for such structural changes. England was unique in this respect: there the Norman Conquest established a relatively high centralization level early on.

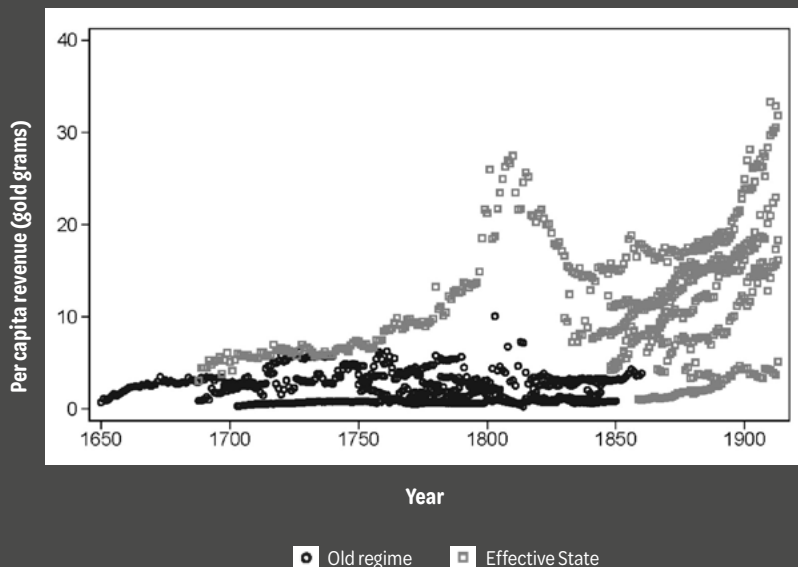
By the end of the Napoleonic era in 1815, most nation-states still had not established impartial institutions. National parliaments existed, but did not generally exert fiscal supremacy. Beyond the problem of geographic scale, national parliaments were only convened at the ruler’s request. Furthermore, fiscal authority was divided: parliament controlled taxation, while the ruler controlled spending. Thus, parliament was hesitant to grant appeals by the ruler for new funds, which it feared would be wasted on military adventures. To avoid parliament, rulers often turned to fiscal predation (e.g. forced loans).

Institutional impartiality typically took place over the nineteenth century, decades after fiscal centralization. Both England and the Dutch Republic were exceptional in this regard. The Glorious Revolution of 1688 in England is the archetype for the establishment of fiscal supremacy by parliament, while the Dutch Republic is commonly characterized as constitutional.

Figure 2 summarizes the evolution of state revenue in early modern Europe. First, as described above, the establishment of effective states at the national level was not widespread until the nineteenth century. The anomaly was England (i.e. the pre-1800 grey squares). Second, per capita revenue was much higher under effective statehood than under the Old Regime. Greater revenue extraction was not just a by-product of economic growth. In England, for example, the Industrial Revolution did not take place until after 1750 (on the Continent, it did not take place until after 1870). Yet per capita revenue there grew by more than 80 percent between 1650 and 1730.

By better enabling nation-states to provide the rules of the game (along with free and compet

Figure 2:
Evolution of Regime Type
and Revenue in Europe,
1650-1913



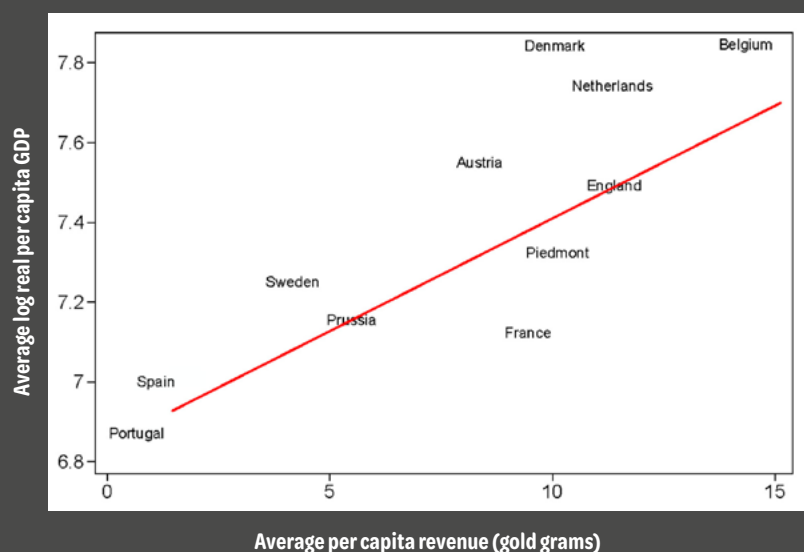


Figure 3:
Revenue and GDP in Europe,
1650-1913

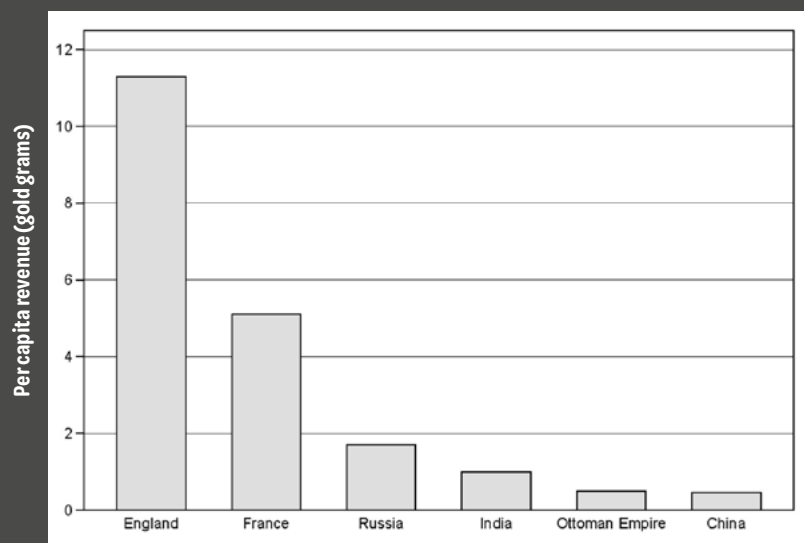


Figure 4:
Per Capita Revenue across
Eurasia, 1780s

itive markets), effective statehood could promote economic development. In line with this view, *Figure 3* indicates that the correlation between the government's ability to extract revenue and per capita GDP was strongly positive in European history. Rosenthal (1992), Acemoglu

et al. (2011), O'Brien (2011), Cox (2016), and Dincecco and Katz (2016) each show detailed evidence of the consequences of national-level institutional reforms for economic development in Europe's past.

Effective statehood at the national level formed the institutional bedrock on which the twentieth-century welfare state was constructed. Prior to the mid-1900s, spending by national governments on housing, healthcare, retirement, unemployment, and family assistance was generally very low. Social spending soared, however, from World War II onward (Lindert 2004, 12-13). Greater direct taxation of income and/or wealth, which calls for a high level of administrative capacity in order to enforce compliance, helped support this increase.

Why Europe?

Figure 4 suggests that the extractive capacity of historical states in Western Europe was large relative to other parts of Eurasia. On the eve of the French Revolution, per capita revenue was highest in England, at approximately 11 gold grams. While this amount was more than double that of rival France, both nations gathered far more revenue per head than other national governments in Eurasia. In Russia, per capita revenue was less than 2 gold grams, and in China, India, and the Ottoman Empire, it was less than 1 gold gram.

Two historical features specific to Western Europe may help explain such differences: high political fragmentation and the low land-labor ratio. The imperial government in China, for example, established lasting administrative rule over large swaths of territory by the late thirteenth century. Thus, the emperor may have been able to rely on coercive resource extraction rather than fiscal contracting. Administrative

centralization, moreover, meant that elites could not play rulers off against each other by threatening to switch allegiances. Finally, unlike in Western Europe, external attack threats in China were unidirectional (i.e. from the Steppe), further reducing the bargaining power of elites. For all three reasons, parliamentary representation (i.e. institutional impartiality) may have been less likely to develop in early modern China than in Europe, hindering further state development (Dincecco and Wang 2018).

Much like early modern Europe, pre-colonial Sub-Saharan Africa was ethnically and politically fragmented. Unlike Western Europe (and China), however, the land-labor ratio in pre-colonial Sub-Saharan Africa was high: in 1500, for example, there were only approximately two people per square kilometer, while in Western Europe this ratio was approximately ten (Herbst 2000, 16). In the Sub-Saharan African context, control over territory may have gone unchallenged, due to the ease with which individuals could migrate to virgin land. Thus, it may have been difficult for governments to establish both the political authority and administrative ability to rule over large swaths of territory, thwarting institutional centralization. The transatlantic slave trade and European imperialism in the late 1800s further impeded the state's ability to

broadcast institutional power in Sub-Saharan Africa.

The brief analysis above helps bring into focus the sorts of contextual features that made the historical state development process in Western Europe stand out relative to other regions. An exciting new batch of research highlights a host of other factors that have influenced global patterns of state development. This recent body of work often exploits local variations within individual nations, and commonly centers on non-European contexts. Competition over public policy among different types of elite actors is one important factor that this new literature highlights (Mares and Queralto 2015; Garfias 2018; Beramendi et al. 2019; Hollenbach 2019; Pardelli 2019). Another such factor is colonial-era fiscal and political structures (Suryanarayan 2017; Lee 2018; van Waijenburg 2018). Other factors include civil war (Paglayan 2017), statistical legibility (Lee and Zhang 2017; Brambor et al. 2019), and technological change in agriculture (Callen et al. 2019). This new batch of research stands to further improve our understanding of the historical roots of state capacity, as well as the relationship between past state capacity investments and current patterns of economic development. ●

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CHINA'S STATE DEVELOPMENT IN COMPARATIVE HISTORICAL PERSPECTIVE¹

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The collapse of the Chinese state in the early twentieth century was surprising. China was a pioneer in state administration: it established one of the world's most centralized bureaucracies in 221 BCE, two hundred years before the Roman Empire.¹ In the seventh century, it produced a quarter of the world's GDP (Maddison 2007, 381) and became the first country to use a civil service examination to recruit bureaucrats. Max Weber described the Chinese examination in great detail (Weber 1951 [1915], 115), which became an essential part of his definition of a modern bureaucracy – the “Weberian” bureaucracy (Weber 1946 [1918], 241; Evans and Rauch 1999, 751).

At that time, Western Europe was experiencing large-scale dislocation, crisis, and a real break in continuity. The Roman Empire had fallen, and the Carolingian Empire had yet to form. Commerce virtually disappeared, and the ruling dynasties could barely maintain a salaried administration (Barraclough 1976, 10). In the medieval period, elites in Europe obtained their status primarily by inheriting feudal titles, and meritocratic recruitment did not emerge until the nineteenth century.

Why, then, did China suffer a dramatic reversal of fortune, given its early bureaucratic development?

Here I document, and then explain, the rise and fall of the Chinese state. I show that two standard explanations for state development – economic development and war – both fall short. I offer my own explanation, which focuses on how the civil service examination transformed the Chinese elite from an encompassing interest group to a narrow interest group. This elite transformation accounts for the initial rise, but the ultimate decline and fall, of China's state capacity.

I use a historical perspective that allows me to uncover continuities and changes that I would not have observed in a short time frame. States, like most institutions, require time to develop. The Chinese state, for example, took centuries to rise and centuries to fall. Studying a short period will risk missing the forest for the trees. As Daniel Ziblatt argues, *temporal* distance – moving out from single events and placing them within a longer time frame – can uncover previously undetectable patterns (Ziblatt 2017, 3).

1. For China's early state building, see Hui (2005) and Zhao (2015).

Fiscal policies (Wakening-Strengthening)

Per Capita Tax (dane of rice)

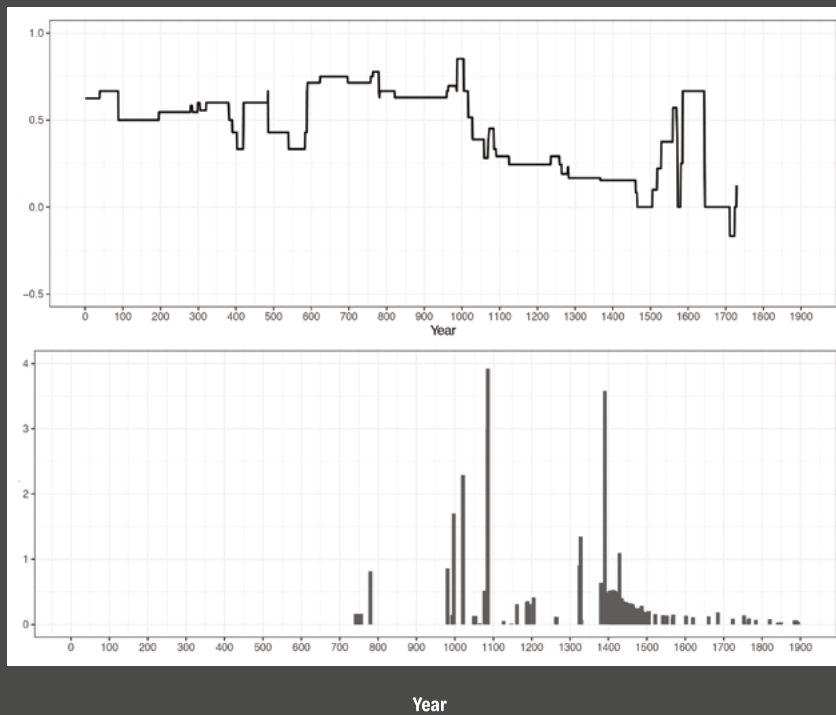


Figure 1: China's Fiscal Policies (top) and Per Capita Taxation (bottom), 0-1900

The Chinese case is worth studying on its own merits. Much work on long-run political development centers on Western Europe. Yet Western Europe might be an outlier, and its political path may have been an accident (Stasavage 2016, 146). Historical China, on the other hand, might

be more representative of today's developing world: an agrarian economy, prevalent violence, strong family institutions, and a weak state. Although history does not repeat itself, it often rhymes. The regularities I discover from the Chinese case enables us to draw on what is known about a historical case to shed light on contemporary

cases. As I discuss in the conclusion, China's historical development produces important lessons for understanding contemporary China and the developing world more generally.

The Rise and Fall of the Chinese State

Figure 1 shows China's fiscal development from 0 AD to 1900. The upper panel presents the evolution of major fiscal policies. I code each policy according to whether historians consider it to be state strengthening (+1), neutral (0), or state weakening (-1).² The lower panel presents per capita taxation, based on estimates from archival materials.³ Both graphs demonstrate that China's fiscal capacity peaked in the eleventh century, started to decline afterwards (with transitory increases), and diminished toward the end of the period.

The comparison with Europe is striking. At its peak, China's fiscal capacity – proxied by revenue as a fraction of GDP in 1086 – was more than ten times that of England (Stasavage Forthcoming). But by the start of the nineteenth century, England taxed 15–20 percent of its GDP, while China taxed only 1 percent (Guo 2019).⁴

Another striking comparison is ruler survival. Figure 2, below, presents the duration and probability of deposition for Chinese, European, and Islamic rulers.⁵ Despite declining state capacity, Chinese rulers enjoyed longer tenures, on a par with European rulers. Both Chinese and

China's historical development produces important lessons for understanding contemporary China and the developing world more generally.

2. Data on China's major fiscal policies comes from Wang's (1981) *History of Finance in Imperial China*. I consider a policy that increased tax extraction to be state strengthening and one that decreased tax extraction to be state weakening. A policy that maintained the status quo is considered neutral. The graph shows the moving average of these policies.

3. I collect data on historical taxation and population from Chinese official histories, Liang (2008), and various primary and secondary sources. A complete list of references is available upon request.

4. For empirical studies of China's financial situation in the late imperial era, see Sng and Moriguchi (2014) and Ma and Rubin (2019).

5. The data on European and Islamic rulers comes from Blaydes and Chaney (2013) and Kokkonen and Sundell (2014). For Chinese rulers, see Wang (2018). The lines denote moving averages.

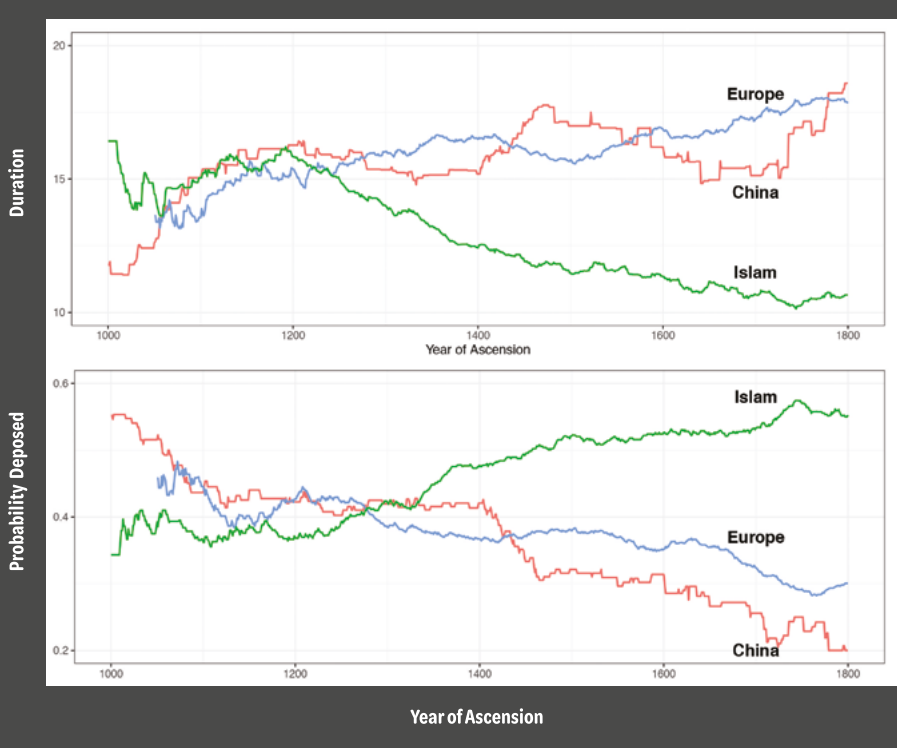


Figure 2: Ruler Duration (top) and Deposition (bottom) in China, Europe, and the Islamic World, 1000-1800

European rulers outperformed their Islamic counterparts. In other words, as China's state capacity became weaker, its rulers stayed in power longer.

Standard Explanations for State Development

Given its early development of statehood, how should we explain the rise and fall of the Chinese state? According to the literature, state institutions tend to evolve in response to either a growing economy or the need to mobilize for war. However, I explain in this section why these standard answers do not fully explain the Chinese case.

Economic development

Modernization theory predicts that as a country's economy develops, society will put more demands on the state. State institutions will then evolve in response to these societal de-

mands to provide public goods and services, which requires fiscal extraction and modern public finance.

Yet the historical evidence suggests that China's economic (under)development was a consequence of state (under)development, rather than the other way around. Scholars of the California School argue that China was the world leader in economics as well as science and technology until about 1500. Before the Renaissance, Europe was far behind and did not catch up to and surpass China until about 1800 (Pomeranz 2000; Wong 1997). Thus, China's economic decline appears to have occurred *after* its state decline, which is consistent with the new institutional economics notion that the state needs to provide security and protect property rights in order to promote long-term economic development (North 1981; Acemoglu and Robinson 2012).

War

External war and internal conflict can both "make" the state. To prepare for external war, which became more expensive in the medieval era, European kings must extract resources from society, establish a centralized bureaucracy to manage state finances, and bring local armed groups under the control of a national army (Tilly 1975). Internal conflict may also promote state development. Mass demands for radical redistribution can induce elites to set aside their narrow interests and form a collective "protection pact"; a broad-based elite coalition that supports greater state strength to safeguard against popular revolt (Slater 2010, 5-7).

But China had fought more wars than Europe; while there were more than 850 major record-

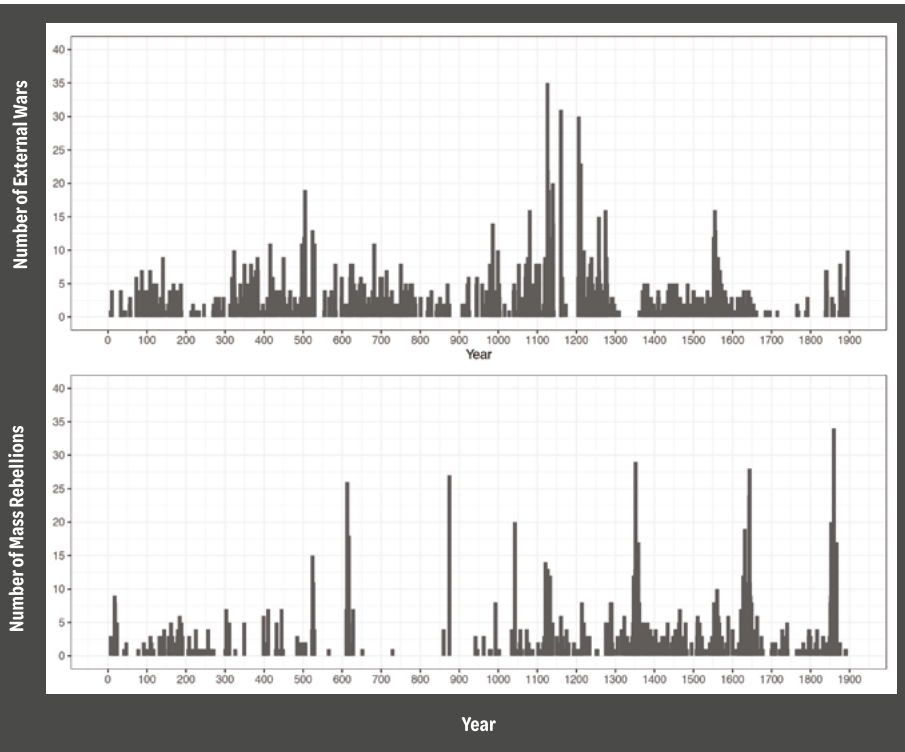


Figure 3:
Number of External War
Battles (top) and Mass
Rebellion Battles (bottom)
in China, 0–1900

ed land conflicts in Europe between the years 1000 and 1799, China experienced 1,470 land-based conflicts during this period (Dincecco and Wang 2018: 343).

In addition, if external or internal war explains state development, we should see state strengthening around or after conflicts. *Figure 3* presents the number of external war battles (upper panel) and mass rebellion battles (lower panel) in China from 0 AD to 1900.⁶

The timing of external wars challenges Charles Tilly's argument that such conflicts force the state to tax its citizens, establish a bureaucracy, and create a national army. The number of external war battles peaked between the twelfth and thirteenth centuries, while state-strengthening policies had started to decline by then. Similarly, mass rebellions occurred frequently

and intensively from the mid-fourteenth century to the late nineteenth century, when taxation was declining continuously.

Elite Transformation and State Development

The turning point in China's rise and fall was in the eleventh century. At the time, China was ruled by the Northern Song Dynasty, which faced existential threats from the Khitan and Tangut nomadic tribes in the north. There was the danger that a war could break out at any moment.

In 1065, defense expenditures consumed over 80 percent of the state's income, which caused the government to register its first overall financial deficit. Aged and inexperienced Song soldiers were hired from the flotsam of the marketplace and were unfit for active combat.

Four years later, Emperor Shenzong and Wang Anshi – a politician – enacted reforms designed to strengthen the country's fiscal capacity and establish a national standing army. They conducted a national cadastral survey to obtain an accurate account of land holdings, which they used to impose taxes on the landed elite who had been hiding properties and evading taxes. The reform also sought to eliminate private armies and organize the population into a national army.⁷

Emperor Shenzong and Wang Anshi were state builders: when faced with external threats, they tried to “make” the state. But many politicians opposed the reform and recruited the empress to their cause. Reform opponents sent letters attacking Wang Anshi, and local officials sabo-

6. For more information about the dataset, see Dincecco and Wang (2018).

7. For details about the reform, see Wang (2019).

tagged the reform and delayed implementation. Wang resigned in 1074 after a prolonged drought, which the critics exploited to blame the reform and persuade the superstitious emperor. The critics then undermined many reform policies. The opposition leaders completely abolished the reform after the emperor's death in 1085, with support from the dowager empress.

Charles Tilly might be wondering: Facing severe external threats, why do elites oppose state building? Traditional, structural factors cannot explain individual-level differences among elites. I offer a new framework.

To buy, or to make: that is the question

My framework starts with the presumption that elites need protection. Such protection involves a bundle of services, including defense against external and internal violence, insurance against weather shocks, justice in dispute resolution, and social policies that protect people from risks.

Elites can obtain protection in two ways. They can "buy" public protection from the state by paying taxes. They can also "make" private protection by relying on private order institutions, such as kinship groups.⁸ Public protection exhibits economies of scale and scope, so the marginal cost of protecting an additional unit is small. If elites need to protect a large area, it is cheaper to "buy" public protection. Private protection has a unit cost, and each unit pays the same price for its own protection because of the rival and excludable nature of private protection. For example, if protecting one unit (e.g., 100 square kilometers) requires one garrison with one unit of labor and capital, then the cost

of protecting two units will double to two units of labor and capital (constant return to scale). If elites only need to protect a relatively small area, then private protection is more efficient, because the marginal costs of funding a private army to protect a small area are relatively low compared to the taxes paid to support a national army. "Making" their own protection also gives elites some autonomy from the state.

This simple logic suggests that elites' level of support for state building depends on the geographic span of their social networks. If they must protect a geographically dispersed network, it is more efficient to support state-strengthening policies. These elites have an encompassing interest (Olson 1982, 48). If they need to protect a geographically concentrated network, it is more efficient to rely on private protection and oppose state strengthening. These elites have a narrow interest (Olson 1982, 48).

From encompassing interest to narrow interest

Applying the framework to the Chinese case, we can now understand why the state started to decline in the eleventh century.

A hereditary aristocracy ruled China during the medieval period from the seventh to the ninth centuries. The aristocracy consisted of a group of large clans whose genealogies were included in the official clan list approved by the imperial state. The emperors recruited bureaucrats almost exclusively from this list, and men from these clans could inherit their fathers' positions. Although these clans were located across the country, their core male members formed a national elite coalition by intermarrying their

8. Firms must similarly decide whether to make a component in house or buy it from the market. See Coase (1937, 390) and Williamson (1981, 556).

children. During the Tang Dynasty (618–904 AD), this national elite was based in the capital cities and became a self-perpetuating institution (Tackett 2014, 25).

Thus, before the 11th century, a network of national elites ruled China. Since their kinship networks were spread out across the country, they were motivated to build a strong central state so they could protect their kin. These elites constituted an encompassing interest group.

The Huang Chao Rebellion (874–884 AD) captured the capitals and killed most members of the aristocracy (Tackett 2014, 187–234). Local elite gentry families, which traditionally held many lower bureaucratic offices, filled the power vacuum left by the demise of the aristocracy.

After the aristocracy was decimated, the Song emperors introduced the civil service examination as an alternate way to identify bureaucratic talent. During this time, members of the local gentry had to recommend prospective candidates to the local magistrate before they were even eligible to sit the initial exam (Hartwell 1982, 419). The expanded civil service examination system therefore reinforced the gentry's strategy to contract marriage alliances with wealthy local neighbors, exchanging prestige and political opportunity for economic advantage. The civil service examination then brought many locally embedded elites into the central government. These elites became "local advocates" who, in order to influence the government's actions, intervened directly and openly with central officials as a native, with a native's interest in (and knowledge of) local affairs (Hymes 1986, 127–128).

Locally embedded elites who served in the central government no longer supported a strong

central state. They were better off protecting their kin using private organizations. They started to form kinship organizations, uniting their kin members around common ancestors and compiling genealogy books to manage kin membership (Faure 2007, 68). They intervened in national affairs to benefit their hometowns (Beattie 1979, 72). Their relatives became local strongmen who organized defense, repaired dikes, and funded schools (Zheng 2008, 183–194). In the late imperial period, these elites became a narrow interest group.

As the elites' social networks became localized, they also fragmented; they found it difficult to organize cross regionally. A fragmented elite contributed to a despotic monarchy because it was easier for the ruler to divide and conquer. Historians have noted the shift to imperial despotism during the Song era, as the emperor's position vis-à-vis his chief advisors was strengthened (Hartwell 1982, 404–405). The trend further deepened when in the late fourteenth century the founding emperor of the Ming Dynasty abolished the entire upper echelon of his central government and concentrated power securely in his own hands (Hucker 1998, 74–75). This explains the increasing security of Chinese rulers.

The despotic monarchy and the narrow interest elite became a self-enforcing equilibrium: the rulers were secure, while the elite used the state to protect their local interests and enjoyed their autonomy. Yet this arrangement led to the gradual decline of the Chinese state.

Lessons for Today

China's historical experience suggests two important lessons for understanding contemporary China and the developing world more

generally. First, it helps us understand how the Chinese Communist Party built a modern state. The key to the party's success in the mid-twentieth century was that it eliminated or neutralized local elites through a *social* revolution. The party achieved this mainly through land reforms in which local landed elites were deprived of their land—and sometimes their lives. Meanwhile, a prolonged and hard-fought revolution helped forge a close-knit network of party elites from all over the country. This national team conquered the country and imposed on it a centralized elite structure.

Second, many developing nations face a challenge in state building as China did historically:

traditional authorities and powerful local families subvert state power. Many of the policy interventions carried out by the international community, such as the World Bank and the International Monetary Fund, focus on strengthening the bureaucracy. But as the Chinese experience demonstrates, state weakness is a *social* problem that cannot be resolved with a bureaucratic solution. When Chinese emperors began using a civil service examination to recruit bureaucrats, the Chinese elites became more fragmented and opposed to state building. This experience shows that building a strong state requires social changes, which are generally missing from today's international programs. ●

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THE STUDY OF ARMED CONFLICT AND HISTORICAL DATA¹

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Why use historical data to study armed conflict? Quantitative studies of armed conflict, and more specifically civil wars, have grown exponentially in the past few years (Cederman and Vogt 2017). However, so too, has the use of historical data to study armed conflict. The use of historical cases to study armed conflict is well established (Tilly 1978; Skocpol and Theda 1979; Petersen 2001) with other areas of Comparative Politics experiencing a “historical turn” nearly ten years ago (Capoccia and Ziblatt 2010). In recent years, the systematic collection of historical sources to generate new datasets for the quantitative study of armed conflict has risen to prominence as a result of the awareness that civil wars, rebellions and all forms of conflict involving non-state actors can be fruitfully studied using spatially and temporally disaggregated data (Cederman and Gleditsch 2009). Kalyvas (2006) inaugurated this trend using a mix-methods approach to leverage historical data from the Greek civil war. More recent books have followed this path (e.g. Balcells 2017; Finkel 2017; Kopstein and Wittenberg 2018), and historical data has been effectively harnessed in a range of recent articles (Costalli and Ruggeri 2015; Zhukov 2017; Kocher, Lawrence, and Monteiro 2018).

Today researchers have more opportunities to create large N-datasets from historical sources than just a few years ago, thanks to a combination of factors that include the opening of some large once-private, secret, or protected archives, the evolution of technical digitalization, and the availability of powerful software to manage text and its codification. In this brief piece, we take stock of the publications of the past decade and provide some preliminary thoughts on why using historical data to study armed conflict is beneficial for political scientists. Further, we also highlight some limitations and issues that should not be underestimated by those attempting this kind of research, before closing with a few suggested remedies.

Historical Data and Object of Study: Legacies of Conflict and Legacies on Conflict

Historical data may be intrinsically necessary to study specific phenomena, such as the long-term effects and legacies of conflict (Wittenberg 2015; Costalli and Ruggeri 2018). In these cases the “legacies of conflict” are represented by past patterns of violence or conflict that can be core explanatory factors for a range of subsequent social, economic or political forms (Lupu and

Today researchers have more opportunities to create large N-datasets from historical sources than just a few years ago.

Peisakhin 2017; Rozenas, Schutte, and Zhukov 2017; Dincecco and Wang 2018). Alternatively, “the legacies on conflict” are about how previous institutional, social and economic factors

and patterns can influence the risk of armed conflict (Wig 2016; Paine 2019).

On this point, while research on legacies is growing, the term “legacy” can be applied to different time lengths: Lazarev (2018) investigates the legacies of conflict on legal institutions in Chechnya about 15 years after the end

of the war, while we discuss the electoral legacies of civil war in repeated observations over 25 years (Costalli and Ruggeri 2019). Other studies consider longer historical legacies stretching back to a country’s colonial past that can affect the likelihood of conflict (Blanton, Mason, and Athow 2001; Wucherpfennig, Hunziker, and Cederman 2016), or relations between forms of governance and conflict that span centuries (Xi 2019). We neither argue nor suggest that we should define a specific span for what a legacy is. However, we need to be more explicit in our works on the mechanisms and conditions that make legacies last for different time spans. If historical data help us understand legacies and theorize about long-term effects and path dependency (Mahoney 2000), we suggest the nature of these empirical sources should also push scholars to develop specific theoretical themes.

Historical Data and Theory-building: Micro mechanisms, “Place” and Multi causal paths

A unique feature of recent quantitative studies on conflict that use historical data is their emphasis on individual data and a detailed account of war-related events at the local level. For instance, geocoding the information on conflict

events contained in Soviet archival documents allowed Zhukov (2017) to study the dynamics of violence against civilians in German-occupied Belarus during World War II in innovative ways. These detailed information include monthly data about train derailments caused by partisan bands and data on the numbers of houses razed and civilians killed by Nazi forces at the district level. The significant wealth and details of the data concerning personal information and conflict dynamics that have been collected coding archival resources, should stimulate scholars to develop and be more aware of at least three important theoretical themes: the micro-foundations of violent mobilization; local spaces understood as “places” (Agnew 2002) that highlight the importance of context and the location of actions; and the multiplicity of mobilization paths.

First, the increased use of rich historical data has favored more precise reasoning around individual decision-making regarding risky and costly actions such as joining an armed rebellion. Theorizing the micro-foundations of conflict means developing explicit assumptions and causal mechanisms that pertain to individuals’ heuristics practices, preferences and decision-making processes. Historical conflicts often allow researchers to better exploit detailed data on individuals, their groups and networks, and the broader social, economic and geographic context. While it is often difficult to access the identities and lives of individuals involved in violent activities during or in the immediate aftermath of conflict, it can be easier to obtain this information and study relations within and between armed groups when the actual use of violence is only a memory of the past. Historical data also presents a chance to leverage information on individ-

uals and their networks in such a fashion that scholars are able to investigate and update theories on conflict in light of non-material factors such as individual and group emotions or ideology. These factors have become (again) more central in recent years in our theoretical frameworks. For instance, in our work on armed mobilization against Nazi-fascist forces in Italy between 1943-1945, we propose a theory on the decision-making process that leads individuals to join an armed rebellion in which indignation and radical ideological networks play a central role (Costalli and Ruggeri 2015). We have been able to test our theory thanks to spatially and temporally disaggregated data on the residence of the Italian soldiers who died in World War II as a result of the Fascist regime's military choices and on the strength of different ideological networks. In addition, we leveraged individual-level data on the persons controlled by the Fascist political police because they were considered communists or socialists and we triangulated these quantitative data from historical sources with interviews with former partisans.

Second, historical quantitative data provide verified information about localities where action took place. However, studying historical cases of conflict also allows political scientists to go beyond pure location and situate quantitative data within a detailed historiographical apparatus around these specific events. Consulting and comparing the works of historians allows political scientists to acquire thick knowledge of the cases, better locating the conflict actions in context. Especially in civil wars and armed rebellions, conflict areas are not mere geographical arenas, but places where participants have symbolic and identity roots, social rela-

tions, and past histories. Therefore, the role of place should encourage us to think more carefully about the risks of unspecified abstraction and the omission of social relations with other groups, local institutions and past experiences.

Third, research on individuals' trajectories of mobilization, based on large-N historical data, has revealed considerable heterogeneity of these paths, and pushes scholarship to reflect more on the effect of intertwined causal factors (Cederman and Vogt 2017). Such sensitivity might also allow the possibility that heterogeneous paths can still push actors towards homogenous decisions and actions (Humphreys and Weinstein 2008). This insight does not imply we should avoid the creation of new "ideal types" of paths, general tendencies, or sequences to armed mobilization. Rather, thicker knowledge of the cases—based on detailed micro-level information and historical accounts—is leading researchers towards to develop healthy skepticism about linear and unidirectional theories of armed mobilization (Viterna 2013). For instance, Balcells (2017) shows how local information of political allegiances prior to the war affected the dynamics of violence in the Spanish civil war combining rich and fine-grained quantitative data from historical sources with qualitative evidence. Though, this informational explanation is also paired with an emotional mechanism triggered by revenge. Local historical information, also analyzed with quantitative methods, provides opportunities to elaborate more nuanced explanations of conflict in which material and ideational factors jointly play a role. At the same time, this type of thicker knowledge, aided by historical data, is allowing scholars to show and explain counterintuitive conflict dynamics (Zhukov 2017).



Historical Data and Dangerous Liaisons: measurement bias, a-historical identification and the weight of the micro

Thus, scholars must critically assess the state of historical data: Are these data better than information collected on contemporary conflicts? And what is their data generation process? Lustick (1996) clearly stresses some risks of using historical material in social science research. Above all, political scientists must be aware that works of historians are rarely unanimous when presenting a given fact, giving rise to concerns about “selection bias” in historical work that feature historians who use “implicit theories about how events unfold and how people behave [that are] very similar to the theory under consideration by the social scientist” (Lustick 1996, 607). This remains a risk, but not an irreparable defect. Indeed, this risk can be reduced by comparing and triangulating between several different sources, and the process of triangulation is often more practical for historical cases that have produced rich historiographical literature. Moreover, while selection bias is a serious risk if political scientists work mainly on secondary sources, it can further be reduced when performing central empirical analyses on quantitative data, especially if the datasets have been constructed collecting data from primary, archival material. The findings of statistical analyses performed on these data should sustain various robustness tests and may be fruitfully enriched by carefully handled secondary sources. Another possible source of bias is tied to the policies and institutional settings that permitted the collection and storage of high quality data in the first place. Hence, the availability of these data is not random, but likely exists in countries with efficient state bureaucracies and sufficient state capacity. Further,

they may be preserved in national archives of past regimes that required high societal surveillance for repressive purposes. Hence historical fine-grained data could be skewed towards specific political regimes. Therefore, researchers should reflect on and be transparent about the likelihood of external validity when drawing conclusions from historical data.

Thus, the opening of archives paired with increasing software capacity digitalizing the new sources is akin to the opening of a new toyshop for political scientists. However, we should be careful and read carefully these toys’ instructions and their small prints. The context and historiographical debates surrounding specific historical data have to be studied, tackled and digested in order to avoid inferential fallacies based on a-historical assumptions and misused data. Shopping around for identification strategies using historical data can be risky business. One of the most important advantages of using historical data should be the possibility of acquiring thick knowledge of the conflict under scrutiny, otherwise the risk of making historical mistakes is high (Kocher and Monteiro 2016).

We mentioned above that high quality historical data are more likely to emerge in countries with specific characteristics. We should then acknowledge the findings of studies that leverage historical data are not automatically generalizable to contemporary conflicts. To be clear, comparing across space and time poses some hurdles (Bartolini 1993) and requires the definition of precise scope conditions. Nonetheless, we believe that history remains one of our best teachers, particularly in the field of conflict studies. Undoubtedly, warfare and its technology have changed over time and the international system has changed as well. As a

result, we need to be careful when we try to generalize theories and explanations with implicit premises embedded within specific war technologies or characteristics of the international system (Kalyvas and Balcells 2010). However, some features of war (such as uncertainty, the need of reliable information, secure supplies, etc.) are remarkably stable over time, despite they can acquire different concrete forms. Even more stable is human nature, as we have good reason to assume the micro-mechanisms and theoretical explanations of conflict based on individuals' decision-making processes should travel through time without the necessity of major adaptation.

Quantitative political science, history and cross-fertilization

This note looks to explain why historical data and historiographical literature, if properly handled and incorporated, can aid political scientists that work on armed conflicts. In a way, we could say that history can be "useful for political science", but does such a claim demand further work to distinguish knowledge as captured by political science from knowledge as generated in the study of history? Clearly, the two disciplines are based on different methods and different epistemologies. Further, we would never propose the simplistic and superficial mixing of research disciplines. However, both disciplines

converge in the creation of knowledge and often work on the same topics. From this more general perspective, we believe that increasing serious dialogue between these two disciplines and favoring conscious cross-fertilization is useful in itself. Moreover, as a result of serious cross-fertilization, political science might also be able to be "useful for history". In fact, political science, and especially quantitative methods, could allow scholars to evaluate arguments advanced by historians in a more structured and systematic way than usually possible with historiographical methods. In fact, historiographical research tends to reach deep knowledge about specific case studies rather than fostering systematic comparison and detecting recurring patterns. This is a common pattern in historical accounts of conflicts evaluating specific explanations through fewer samples and for restricted geographic areas. Quantitative studies based on historical data can amplify the full explanatory power of these local theories. In the end, using historical data to study conflicts is not without risks, but it is a fascinating and worthwhile path for inquiry, with the potential to increase our analytical capacity and further our understanding of armed conflict. Every approach and every method implies some risks. We believe using historical data to study armed conflicts is a risk worth taking. ●

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HISTORICAL SOURCES AND THE STUDY OF TRADE POLITICS IN DEVELOPING DEMOCRACIES

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Students of comparative and international political economy have long been interested in explaining how domestic political coalitions influence international economic policy outcomes, particularly in the context of trade policymaking (Rogowski 1989; Scheve and Slaughter 2001; Hiscox 2002; McGillivray 2004). The study of domestic politics on trade is illuminating, both because of the substantial societal welfare implications of these policy conflicts and because trade politics can provide insights into many other forms of redistributive policymaking that are of central interest to political scientists. The vast majority of the scholarship on the domestic politics of trade focuses on theory developed in the context of, and empirical evidence drawn from cases in, North America and Western Europe (Alt et al. 1996). This attention to advanced industrialized economies is understandable, given the oversize role that trade played in the historical economic development of the west as well as the considerable impact that domestic politics in these countries have had on global trade flows over the past two centuries.

Nevertheless, the spotlight on electoral politics surrounding trade in industrialized economies has correspondingly led to a dearth of scholarship on the sources of trade policy contestation

in developing democracies (Milner and Kubota 2005; Kohli 1989; Ahmed and Varshney 2012). This is a regrettable oversight. Institutional and cultural contexts in the Global South vary considerably, raising a fresh set of theoretical considerations regarding the channels by which political coalitions and interest groups can influence policymaking outcomes in the electoral arena. Empirically, too, qualitative and quantitative data collected from developing countries can allow researchers to test the external validity of findings from advanced democracies, while subjecting theoretical conjectures that are distinct to legislatures in emerging economies to rigorous evaluation.

In this essay, I will begin by discussing how historical data on the politics of trade can allow researchers to investigate questions that are difficult to answer with more contemporary data sources, and point to opportunities for data collection in archives and libraries in developing countries. I will then draw on my research on trade politics in South Asia to highlight salient ways in which the historical study of trade in the developing world can complicate conventional narratives and, in turn, add to our broader understanding of coalition politics surrounding redistributive economic policymaking.

Archival Sources and the Study of Trade Politics

Scholars interested in analyzing data related to either trade policymaking or trade politics in developing countries face steep challenges. Most publicly available datasets on trade policy measures only begin coverage in recent decades; for example, the United Nations Conference on Trade and Development (UNCTAD)'s Trade Analysis Information System (TRAIS) database, which makes product-level tariffs data available to researchers, has data starting in 1988, with many developing countries gaining coverage only much later.¹ Legislative debates

on trade policymaking, politicians' speeches and campaign pledges to labor unions and trade unions, correspondence between industry groups and policymakers, and other forms of evidence essential for studying the domestic politics of trade are similarly difficult to obtain in most developing country contexts.

Few governments systematically collect and make available these types of data to researchers; many actively restrict access to contemporary records related to policy deliberations. In interviews that I have conducted with officials at the Ministry of Commerce and Industry in India, for example, respondents regularly requested confidentiality, stating that political calculations related to foreign policy issues were not suitable for public consideration. These challenges are more acute when researchers attempt to study informal sources

of policy influence. Kochanek (1996) provides considerable evidence, for example, to show that firms and industry associations in India "developed a highly sophisticated mode of discrete lobbying designed to achieve particularistic benefits" when contesting policy changes during liberalization (see also Chari and Gupta 2008; Gaikwad and Scheve 2016).²

Scholars may be able to circumvent limitations in data access in contemporary periods by drawing evidence from historical sources. In South Asia, colonial authorities maintained extensive records of trade policy schedules, legislative deliberations on trade, commerce and tariff board reports, petitions for protection from firms and industry associations, and trade union discussions, for instance. Sensitive records, such as confidential correspondence between government authorities in England and India, which have long since been declassified, are readily accessible to researchers. In turn, archives can provide fertile soil for scholars seeking to unpack from a historical perspective the underpinnings of politics surrounding economic policymaking.

I have encountered a trove of historical sources related to the politics of trade in South Asia in repositories such as the National Archives of India (New Delhi), Ministry of Commerce Library (New Delhi), Central Secretariat Library (New Delhi), Indian Merchants' Chamber (Mumbai), and The British Library (London). These historical sources help recast conventional accounts regarding the domestic determinants of policymaking contests, as I discuss below.

1. <https://wits.worldbank.org> (Accessed October 1, 2019). See Ballard-Rosa et al (2016) for additional discussion.

2. Challenges regarding data access certainly also exist in industrialized country settings, but it is worth noting that in countries such as the United States, data on lobbying, campaign contributions, trade policies, legislative voting histories, and political speeches are publicly available and typically easier to access than in developing countries.

Scholars may be able to circumvent limitations in data access in contemporary periods by drawing evidence from historical sources



Trade Policymaking Under Colonial Rule

For much of the period between the mid-nineteenth and mid-twentieth centuries, a large class of developing countries were subject to some form of direct or indirect colonial rule (Mahoney 2010). A rich lineage of intellectual thought holds that colonization was driven primarily by trade, and that colonizers deployed political and military control over dominion territories in order to structure trading relations in ways that advantaged the metropole (cf. Hobson 2011; Lenin 1988). Kleiman (1976, 459) summarizes these claims, arguing that colonial powers, by “forcing the colony’s population to buy their imports for more and to sell their exports for less than going world prices” generated a trading system built on the “economic exploitation of colonial territories through trade.” In these accounts, trade policy served as a vital tool of the colonizer, brandished relentlessly to advance the economic interests of manufacturers in colonial metropolises to the detriment of citizens and producers in dominions.

At the same, many colonies—from India to South Africa to Malaysia—obtained limited forms of electoral and policymaking autonomy for significant periods while subject to colonial annexation. Trade policy was one among a select few policy levers over which legislatures in colonies had control (Tomlinson 1975). How did limited enfranchisement, granted for the first time from faraway metropolises, affect the aggregation and representation of economic interests related to trade in these legislatures? In cases where conflicts arose between manufacturers based in colonizer nations and producers in the colonies, whose voices prevailed and influenced policy?

Research questions such as these are unlikely to arise in the context of the historical study of

trade policymaking in the west, yet are central for understanding the origins of political conflict over trade in countries that were once subject to imperial rule. In a working paper, Don Casler and I set out to answer these questions by considering colonial-era data on industry-level import tariffs in British India, as well as an in-depth analysis of legislative debates and a plethora of contemporaneous sources pertaining to trade protectionism in the Indian parliament (Casler and Gaikwad 2019).

The historical data that we collect and analyze in our research paints a nuanced story regarding democratization and trade policymaking that considerably revises conventional narratives, such as those articulated by Lenin and Hobson. We find that the devolution of political authority over trade policy to India’s legislature, starting in the 1920s, led to sharp changes in the balance of power between the interests of Lancashire and London and those of domestic manufactures in India.

For instance, declassified telegrams between British officials in England and the Viceroy of India acknowledge that London would soon need to begin accommodating political demands to safeguard Indian manufacturing interests from British competition:

“The steel industry in India is represented by the Tata Iron and Steel Company. It is common knowledge that this Company is in difficulties... it is generally believed that they are due to the dumping of cheap Continental and English steel into India, and many people think that this dumping is deliberate, and is designed to bring the Company down...There is the usual suspicion that we are more interested in British manufacturers than in an indigenous Indian industry,

and the protection of that industry is regarded as a matter of national importance and national pride...It would be a calamity if the Company were to fail.”³

Adverage Standard and Preferential Tariff Rates for British India, 1921-50

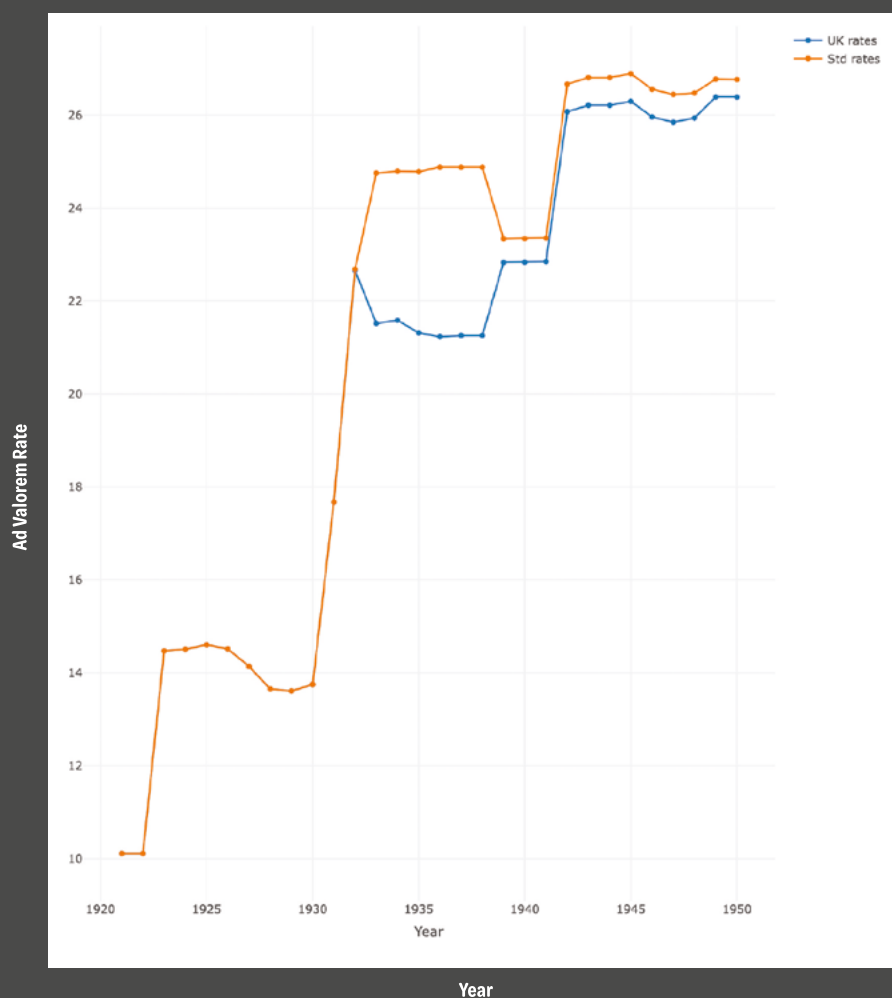


Figure 1:
Import Tariffs in British India
under Colonial Rule

The observations of these British agents were prescient. Soon after India's Central Legislative Assembly (CLA) commenced debate on steel industry policymaking, it enacted a series of protectionist measures to shore up domestic manufacturers. This represented a sea change

in India's trade policy; as Sir Purshotamdas Thakurdas, the representative of the Indian Merchants' Chamber in the CLA, remarked in 1924, "I think, Sir, that the introduction of this measure [in favor of trade protection] in the House marks a new departure in the policy of the British Government in India ever since the time of British rule in India."⁴ Evidently, the onset of limited democratic representation was marked by a brand of foreign policy assertiveness unseen during prior periods of colonial annexation.

In our paper, we document a steady rise in import tariffs on foreign products over the next three decades, with the average ad valorem tariff rate increasing from approximately 10 percent in 1921 to 26 percent in 1947 (i.e., an increase of about 160 percent), on the eve of India's independence. *Figure 1*, which plots the average ad valorem tariff rate on goods from around the world ("Standard Rate") and on goods from the United Kingdom under the system of Imperial Preferences which began in 1933 ("Preferential Rate"), makes this point clearly. Undergirding this rise in protectionism, we argue, was a steady increase in the representation of the interests of domestic actors in colonial legislatures.

Evidence from transcripts of parliamentary debates on trade policy in India—which were among the most vigorously contested policy debates taken up by elected representatives—buttresses this interpretation. In a 1926 debate over steel tariffs, Muhammad Ali Jinnah, leader of the All-India Muslim League and the future Governor-General of Pakistan, provided the following rationale for increased protectionism:

3. Telegram from Viceroy, Commerce Department to Secretary of State for India, March 10, 1924, Delhi (quoted in Casler and Gaikwad 2019).

4. Extract from the Legislative Assembly Debates, Vol. IV, No. 39, British Library, London, p. 20 (quoted in Casler and Gaikwad 2019).

"It is not the Government that want to give us protection. It is not the Government who are in love with this policy. The interests of India demand protection and without protection, let me tell you, there will be no labour, nothing to eat and there will be no Labour Members... [T]he greatest men that India has produced... have forced the hands of this bureaucratic Government at last to commit themselves to a policy of protection."⁵

These and related calls for protectionism certainly did not go uncontested in parliament. Legislative representatives pitted the interests of domestic manufacturers against those of labor unions and consumers in India. For example, Mr. Chaman Lall, the representative from West Punjab, argued vociferously against protectionism by pointing to the higher prices that consumers would need to pay as a consequence of import protection:

"Sir, I am really surprised at the nauseating atmosphere of self-congratulation in which we have been living through the whole day to-day. It seems to me that the gentlemen who represent the capitalists of India are thumping each other on the back at having produced a baby... and congratulating each other for having come upon a common platform, the platform of exploiting the common people of India."⁶

But a striking trend that we encountered pertained to the increasing attention that legislators paid to safeguarding and promoting Indian manufacturing interests vis-à-vis those of British and other foreign firms.⁷ As represen-

tative Jamnadas M. Mehta argued in 1926, "it is necessary that this House and the country should stand by these industries to whom we promised protection...[because] there is a deliberate attempt made by the manufacturers in Wales to kill this industry."⁸ In other words, electoral representation—even of the incipient kind that was afforded under colonial control—created avenues for domestic actors to influence policy outcomes and circumscribe, in turn, the rapacity of the colonial state.

This is not to say that conventional narratives about colonialism and trade exploitation are incorrect. Indeed, we do find evidence that Britain was able to negotiate preferential access for its goods (relative to the products of continental and other foreign manufacturers) in India through the system of Imperial Preferences enacted in 1933 (de Bromhead et al 2019). Yet, even preferential access was fought tooth and nail by domestic coalitions; the difference in India's import tariff rates between non-Commonwealth imports and British imports decreased from an average of three percentage points in 1933 to about half a percentage point by the 1940s, driven by domestic opposition to British manufacturers' preferential market access. Qualitative accounts underline the role of electoral representation in circumscribing preferential access, as evidenced by legislative minutes registering dissent to Imperial Preferences:

"[The] overwhelming majority of people of this country will refuse to countenance Imperial preference in any shape or form; this is not due

5. Extract from the Legislative Assembly Debates, Vol. VII, No. 17. British Library, London, p. 6 (quoted in Casler and Gaikwad 2019).

6. Extract from the Legislative Assembly Debates, Vol. IV, No. 39, British Library, London, p. 36 (quoted in Casler and Gaikwad 2019).

7. Extract from the Legislative Assembly Debates, Vol. IX, No. 20. British Library, London, p. 30-32 (quoted in Gaikwad 2019).

8. Extract from the Legislative Assembly, Debate, Vol. VII, No. 17, British Library, London, p. 1390 (quoted in Casler and Gaikwad 2019).

to any hostility toward the British people...but to our deep-seated conviction based on the painful experience of nearly two centuries that the British imperialists and capitalists are at the bottom of all our troubles.”⁹

The insights that we gleaned from this historical research can help shed light on coalition politics in colonial-era legislatures, yet have implications for our broader understanding of political competition on trade in a range of cases. Enfranchisement was a gradual process in many democracies outside of the colonial context, and notions of “national interest” were likely fluid and contested in other territorial units during transitions to nationhood, just as they were in the nascent Indian state during its path to independence. The exhaustive archival records developed and preserved by the British in India during the colonial era may thus provide a unique lens to examine how coalitions first emerged and wrested policy concessions in the legislative arena in other early democratizing states.

In a different vein, historical sources from the colonial period also hold the potential for explicating the behavior of coalitions in postcolonial democracies that were drawn into the orbits of great powers during the Cold War (Berger et al 2013) or have become de facto client states in contemporary geopolitical struggles between Asia and the west (Scheve and Zhang 2016). A historical turn in the study of trade politics in developing countries thus holds the potential of providing new theoretical and empirical insights into fundamental questions of subjugation and

resistance that are of considerable interest to scholars of international and comparative political economy.

Conclusion

Qualitative and quantitative data retrieved from repositories can provide researchers with evidence that might be difficult to obtain from more contemporary sources. These records are beneficial in myriad ways, helping scholars both test whether theories that have found support in advanced industrialized economies extend to developing country settings and interrogate the validity of new theoretical models that may better take into account institutional and cultural contexts in the Global South. In this essay, I have underlined some analytical payoffs that can accrue when archival evidence is used to advance the study of the domestic politics of trade and economic policymaking. Similar gains are also likely in other substantive areas of political inquiry.

At the same time, some notes of caution are in order. Archives across developing countries vary in scope and depth; some colonial authorities, for example, were more diligent than others in retaining contemporaneous records, and some postcolonial states have been relatively more committed to preserving their historical legacies. Bias in the availability of archival sources is therefore a key challenge for researchers relying on historical data to make evidentiary claims. Even in repositories where records have been preserved, resource gaps and staff shortages often leave source materials poorly organized, creating practical barriers to data access. Many

9. Legislative Department, February 7, 1927, p. 1 (quoted in Casler and Gaiwad 2019).

developing country archives, moreover, have stringent reproduction and copyright restrictions, challenging transparency and replicability goals in the knowledge production process (Gaikwad et al. 2019). Nevertheless, researchers who are attuned to these concerns can expand considerably the scope of their investigations by drawing on new forms of data available in repositories of the past. ●

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COMPARISONS ACROSS 'ERAS:' History and Inferential Leverage

by *Eric Hundman*



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*History can help us
sidestep the obstacles
that political controversy
and concealment pose in
our research.*

Pursuing work of contemporary relevance is, with good reason, a central concern for political scientists. However, it would be a mistake to believe that the only way to offer answers about contemporary phenomena is to draw on contemporary data. To the contrary, some research questions are actually better addressed from a point of substantial historical distance. In this essay, I draw on my research into military disobedience to highlight two such types of questions: those that are currently politically sensitive, and those that are rare or concealed. History can help political scientists by helping us sidestep the obstacles that political controversy and concealment pose in our research. Political scientists, in turn, can expand our understanding of history by offering new questions and theoretical perspectives.

The Challenges of Contemporary Political Sensitivities

Especially given the combination of authoritarian retrenchment around the world and the growing ease of censoring fast-expanding holdings of digitized data (Tiffert 2019), as scholars we often need to be creative and flexible in order to answer research questions that state ac-

tors seek to obfuscate. Pursuing our questions in history is one way to do so; if an analytic case can be made for comparison across eras, censorship can be sidestepped by looking at previous instances of a phenomenon.

Take, for instance, the question of loyalty and disobedience in China's military. As Beijing expands its territorial claims, sprints to upgrade the People's Liberation Army (PLA), and continues to threaten Taiwan, investigating the degree to which China's soldiers are loyal is crucial. Policymakers and scholars would like to be able to predict when and how China's soldiers are likely to obey their superiors. However, answering this question with contemporary data is virtually impossible given the PLA's opacity to outsiders. Reporting on the military in China is carefully controlled; while the PLA is far from a black box, my own research has demonstrated that public discussions of loyalty and disobedience in China's military are carefully circumscribed (Hundman 2019a).

A brief comparison across historical eras serves to illustrate how drawing more deeply from history can help sidestep the problems of access and data availability that inhere in politically sensitive research projects. Take first the case of China in the spring of 1989, when people



across the country had been protesting for months and student protestors had occupied Tiananmen Square, in the center of Beijing.¹ The leadership in the capital ultimately decided to respond using military force by sending tanks and troop carriers to disperse the occupation. They therefore summoned commanders of the PLA to issue the requisite orders. Many members of the PLA, we now know, were “troubled by misgivings, confusion, rumors and regrets about the task assigned to them” (Jacobs and Buckley 2014). In particular, Major General Xu Qinxian (徐勤先), commander of the 38th Group Army just outside Beijing, demurred and refused to lead his troops into the capital. He argued directly with his superiors, reportedly saying that “the protests were a political problem and should be settled through negotiations, not force” (Jacobs and Buckley 2014).

Xu’s resistance sent shock waves through Chinese officialdom, and merits explanation. Military disobedience can come in a number of different types (Hundman 2019b). In Xu’s case, he attempted to *refine* his orders; he maintained support for his superiors’ broad goals – in this case, a return to stability in the capital – but resisted the specifics of his orders to disperse the students. My book manuscript argues that commanders will choose this type of disobedience when they are in advantageous social network positions and remain strongly loyal to the superiors who issued their orders. In Xu’s case there exist some indications both of these factors were in place, but the events of 1989 are still among the most sensitive topics in the PRC and the data necessary for a rigorous test of my theory of disobedience *in this particular case* are simply not available.

A historical comparison from the late 19th century served, however, to test my theory. There is no question that 19th-century China was, in the aggregate, quite distinct from China today – it was ruled by the imperial Qing Dynasty, surrounded by powerful colonial forces, wracked with rebellion, and in a precarious financial state. Still, the commander of China’s forces on Taiwan during the Sino-French War (1883-1885), Liu Ming-ch’uan (劉銘傳), found himself in a very similar dilemma to that faced by Xu Qinxian a century later: he received an order to retake a beachhead from his French opponents, judged it to be strategically ill-advised, and refused to obey even as he argued with the emperor for a different approach to Taiwan’s defense (Hundman and Parkinson 2019).

While the temporal distance of Liu’s case presents challenges of its own, military disobedience during the Sino-French War is not viewed as a politically sensitive topic in China. To the contrary, Chinese historians have converged upon the judgment that Liu’s decision to disobey the emperor during the Sino-French War was the best among a bad set of options in the face of a superior foe (Huang Zhennan 1992). And Qing China offers voluminous data with which to test a theory of military disobedience. Thus, I was able to draw on thousands of pages of primary sources, including internal government documents, official communications between the throne and commanders in the field, personal letters between friends, and commanders’ diaries (Hundman 2016). This allowed me to directly model Liu’s social networks over a span of decades, while also investigating the details of his loyalty to the emperor and the Qing dynasty more broadly throughout his career. I found that he behaved as my theory predicts.

1. The best overview of these events remains Brook 1998.



In short, then, casting back into the imperial era in China for a comparison to commanders like Xu Qinxian allowed me to sidestep many of the problems caused by the fact that the military and questions about its members' loyalty are proverbial third rails in contemporary Chinese politics. Such comparisons across eras will of course not always be feasible or analytically advantageous – finding comparisons to investigate the effects of nationalism, for instance, would be impossible in the era before nationalism existed – but scholars should keep them in mind when investigating politically sensitive questions.

Studying Rare or Concealed Phenomena

The nature of military disobedience makes it both easy to hide and easy to recast as a behavior with fewer negative connotations. From an inferential standpoint, this means that it is not possible to identify the population of cases from which to sample, nor is it possible to identify a sample that will be convincingly random; officers themselves, military organizations, and states all have incentives to conceal disobedient behaviors. These substantial empirical challenges necessitate casting the widest possible net for cases through which to gain inferential leverage on such questions.

In my own work, in addition to highlighting attempts to *refine* orders, I also show how soldiers can choose to *grudgingly obey* orders they dislike. Because such a behavior so often appears identical to approving obedience, it is even easier to hide than more dramatic cases of disobedience, and empirical cases of grudging obedience are exceptionally challenging to identify. A comparison across eras, however, allowed me to establish the conceptual boundaries of this type of behavior.

One famous example of grudging obedience arose during the Vietnam War. On March 16, 1968, American troops had gone on a mission to Son My village with the goal of neutralizing a Viet Cong base there. They found no such base, but upon arriving, the Americans nonetheless gathered a group of villagers together. Lieutenant William Calley then told Private First Class Paul Meadlo “you know what to do” with them. Calley left briefly and upon returning, asked Meadlo, “How come they’re not dead?” Calley and Meadlo then together fired on the villagers, the latter crying as he did so (Kelman and Hamilton 1989, 6). Later in an interview, Meadlo explained, “Why did I do it? Because I felt like I was ordered to do it, and it seemed like that, at the time I felt like I was doing the right thing” (Richards 1979).

This interview aired on November 24, 1969, and its impact on American perceptions of the war is hard to overstate. By one account, it “ended the debate about what had happened at My Lai, and it also spawned a wave of Sunday feature stories by journalists about massacres they had witnessed in Vietnam” (Hersh 2018). Meadlo’s misgivings about a single wartime order sparked a sea change in American discourse about the Vietnam War.

Such weighty consequences do not always attach to grudging obedience, however. A comparison from the 19th century highlights how grudging obedience, when less publicized than Meadlo’s, can be very hard to identify without some historical distance. During the combined British, French, and Turkish attack on Sevastopol in 1854 during the Crimean War (1853-1856), Vice Admiral Sir James Whitley Dundas was in command of the British fleet and Lord FitzRoy James Henry Somerset Raglan was in command of the British land forces. They had been ordered to “act in concert” (Royle 1999, 142).

Raglan ordered Dundas to bombard Russian fortifications from the sea, and he acquiesced.

Lacking historical perspective and access to Dundas's personal writings, this would appear to be a simple case of an officer obeying orders from his superiors. We now know, however, that Dundas strongly disapproved of this order, writing that "I am quite sure this is an attack that will be of no service to the army...[but] my object will be to carry out Lord Raglan's wish, altho' contrary to my own opinion" (Lambert 2011, 158–59). Even so, he did not complain to Raglan or his superiors at the time. The consequences of this case also substantially differ from those in Meadlo's, as Dundas appears to have deeply buried his disapproval, writing directly to Raglan after carrying out the operation of which he disapproved that "I cannot repeat too often and with perfect sincerity that there is nothing in my power which shall not be done to assist your operations in the siege of Sevastopol" (Lambert 2011, 165).

Drawing a comparison across what would often be considered very different eras of warfare offers two analytical advantages that would not have otherwise been available to this project. First and most simply, doing so offered another useful data point that fell into the category of grudging obedience, which is enormously difficult to identify empirically.² Second, the differences between the two cases – in content, context, import, and motivation – were substantial. This, coupled with the fact that they involved identical behavioral choices by the two commanders, helped demonstrate the broad applicability of this conceptual category.

Conclusion

In this note I argued that historical comparisons are especially advantageous for two types of research questions: politically sensitive questions, and questions about phenomena that are rare, concealed, or difficult to identify. The core of my argument here is about data availability and using history to overcome the substantial challenges inference poses. One recent study of the international relations subfield found that "63% of all articles published in the past 27 years use empirical evidence from the 10 years prior to their publication" (Maliniak et al. 2011, 459). To some degree this is of course necessary to answer questions about contemporary institutions and cultures, but unnecessarily ignoring enormous swathes of human experience on the questionable basis of their irrelevance to contemporary questions closes off rich sources of perspective and understanding.

This is not to say that casting further back in history does not pose its own challenges in access, interpretation, and understanding. Furthermore, comparisons from different historical eras will only be analytically helpful if they fall clearly into the scope of the conceptual categories with which the researcher is concerned. But identifying appropriate, across-era comparisons will often be worth the effort, particularly for research projects that study individuals. Individual humans, as units of analysis, remain consistent and comparable across most accessible historical eras. For example, individuals' conceptions of self and networking activities in 15th-century Florence differed from ours today, but the connections to others and practices that people used to construct those conceptions are eminently recognizable.

2. See also the discussion of this challenge in Hundman and Parkinson 2019, Appendix.

to modern networkers. Despite these similarities, and “although no one in the social sciences seriously espouses a crude ‘traditional versus modern’ distinction any longer, it remains an all too readily invoked assumption in even the best analyses” (McLean 2007, 5).

The renewed interest among political scientists in studying individuals dovetails with this call for broader use of history. Political scientists typically focus on organizations, states, or other corporate actors in their research. However, particularly in international relations, there has been a resurgence of interest in the determinants of individual choice, as seen in recent work on leaders, foreign policy, and political psychology (Horowitz, Stam, and Ellis 2015; Saunders 2017; Kertzer and Tingley 2018). This “new behavioral revolution” therefore points to the exciting possibility of incorporating history more broadly into political science research (Hafner-Burton et al. 2017, S3).

Delving further back in history, when appropriate comparisons can be found, has the poten-

tial to help political scientists answer challenging, important questions. This is not a call to assume that contemporary beliefs, behaviors, ideas, or norms apply in the distant past; they often will not. However, the continuities across what scholars tend to think of as boundaries beyond which data becomes ‘historical’ – and thus not informative about contemporary questions – are much more substantial than commonly appreciated. “Benchmark dates,” when they are useful at all, must be adopted on the basis of the analytical needs of a given research project, rather than simply assumed as a matter of convenience (Buzan and Lawson 2014). Social scientists can contribute to our understanding of history and supplement historians’ work by asking new questions, bolstering incentives to preserve historical information, and offering new theoretical perspectives. Political scientists, too, can benefit by engaging more closely with historical data in order to offer better answers to many challenging, fascinating questions. ●

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TAXATION AND PUBLIC SPENDING IN 19TH CENTURY PRUSSIA

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What can explain differences in taxation and government spending? What caused some states to develop effective systems of taxation and tax collection? These questions have long been of interest to political scientists and economists alike. While our understanding has greatly improved, especially for democracies, we still know much less about the drivers of taxation and government investment in non-democracies. This is particularly important since many instruments of taxation were first introduced in non-democracies (Aidt and Jensen 2009; Mares and Queralt 2015). Moreover, investment in state capacity or the lack thereof may have long term consequences for subsequent regimes. Understanding under what circumstances economic elites in nondemocracies demand state investment in infrastructure and public goods, and when this demand translates to investments in fiscal capacity, will therefore have implications for learning about countries' long-run development.

In my recent work (Hollenbach 2019b; Hollenbach 2019a), I make a two-pronged argument about drivers and impediments to capacity investments. First, I contend that certain capital endowments can lead political elites in non-democracies to demand higher govern-

ment spending, in particular investments in education and infrastructure. As I expand upon below, this was the case for Prussian cities in the 19th and early 20th centuries. Secondly, as elite ownership of particular types of capital generates demand for government spending, it can also create incentives to invest in the capacity of the state to raise taxes. In contrast, redistributive threats, e.g., high inequality and a possible future regime change, can create incentives to curb fiscal capacity. I empirically investigate the two parts of this larger argument in multiple papers, drawing on newly collected quantitative data as well as qualitative evidence from local administrative units in Prussia in the late 19th and early 20th century.

Capital Endowments and the Demand for Public Spending

Capital and geographic endowments have long been identified as an important factor in determining countries' economic and political long-run development (e.g., Engerman and Sokoloff 2002; Rodrik, Subramanian and Trebbi 2004). Different types of factor endowments influence the economic activity of economic elites and, therefore, the type of capital elites are predominantly invested in. Naturally, the economic elites' political interests are shaped by their



economic activities, meaning they are directly related to the type of capital they own. It follows that the capital endowments of political elites in non-democratic regimes should directly influence political-economic decision making – in particular, fiscal policy.

The first part of my larger theoretical argument concerns the demand for government spending. Depending on the type of capital owned by elites, higher government spending may directly increase capital income. If complementarity between capital owned by elites and government spending is high enough, state investment will boost returns to capital. Owners of capital with sufficiently high complementarity to public investment will prefer higher government spending. This part of the argument builds directly on the formal model developed by Galor and Moav (2006). For example, public spending on education can be profitable when the marginal benefit of additional investment in physical capital is smaller than the marginal return of taxation paying for the education of workers. Similarly, investment in public health and sanitation can increase workers' health and reliability, reducing sick days and turn over (Brown 1989). As I argue, elites may prefer government investment to private investment in these goods for three reasons in particular. First, state investment is likely to benefit from economies of scale, especially where initial state resources already exist, e.g., the state is already supplying some level of public schooling. Second, government spending can serve as a commitment mechanism that prevents free-riding on the investments of others. Lastly, government spending financed through taxation raises revenue from actors opposed to increased investment, thus increasing total spending.

Consequently, I argue that where economic elites own physical capital with high complementarity to human capital and hold political power, non-democratic governments will pursue investments in public education, and possibly sanitation or public health. Where the supply of skilled labor is limited, the benefits to public investment will be even higher. In contrast, political elites who own capital with little complementarity to human capital, i.e., those that require cheap and easily replaceable manual labor, are likely to oppose public spending. These elites, for whom investments are unlikely to be profitable, are strongly opposed to paying for spending that will benefit others. I apply this general theoretical argument to the case of 19th century Prussia, where it pits industrial against agricultural elites. As I argue, industrial elites benefited from a more educated workforce and, therefore, pushed for state investment in education. In contrast, agrarian elites preferred little state investment.

Educational Investment in 19th century Prussian Cities

In a paper, recently published in *The Review of International Organizations*, I investigate this argument using data from a census of all Prussian cities with more than 25,000 inhabitants at the beginning of the 20th-century (Hollenbach 2019b). There are several advantages to using these historical city-level data. First, as with any subnational analysis, several potential confounders are held constant, as they do not vary within central states. For example, trade policy, threat of external war, or the political system are effectively the same across all cities in the sample¹. Second, cities were important political and administrative

1. There are slight differences in how the electoral rules were executed in different cities, however, the results are robust to controlling for these differences.

One major advantage of studying the historical Prussian case is the availability of high-quality data.

units during the time period studied. Decisions about public investment in infrastructure and lower level education were made at the municipal level. Third, the sample is comprised only of cities with more than 25,000 inhabitants. This ensures that observations are similar in terms of population density and economic

and are available online. Moreover, a large number of county level variables have already been digitized by Becker et al. (2014) and Galloway (2007). Aside from possible data transcription, the main potential difficulty is aggregating data to the correct administrative units, which change over time.

Most importantly, however, Prussia is an excellent case to study the theoretical argument, due to the large congruence of political and economic elites. In studies of nondemocratic politics, scholars often assume that economic elites also exhibit extraordinary political power. The political system of Prussia, especially at the local level, ensures that this is a valid assumption here. In 19th century Prussia, economic elites had immense political power. The three-class franchise, linking political (voting) power directly to tax payments, ensured that economic and political power largely overlapped. This was especially true for lower administrative units, e.g., cities. The franchise rules led to city councils in the most industrialized region of Prussia (Ruhr area) being mostly filled with industrialists, traders, and bankers. In the city of Essen, the family of industrial magnate Krupp by itself selected a third of council members (Hollenbach 2019b, 10). In rural areas, large landlords generally held vast local political power. The congruence between economic and political power at the local level ensures that Prussia is an excellent case to investigate the theoretical argument.

While it is nearly impossible to know the exact capital ownership of city-level political elites, the overlap of political and economic power makes approximation possible. In general, I proxy for the strength of industrial elites with the share of workers employed in the industrial sector. In this particular context, Prussia in the 19th

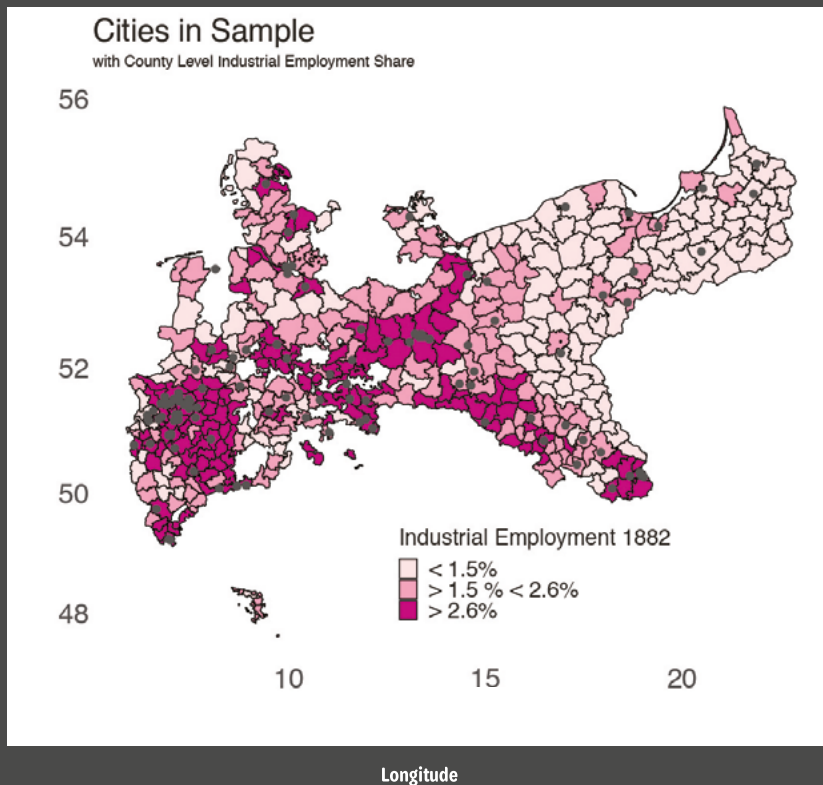


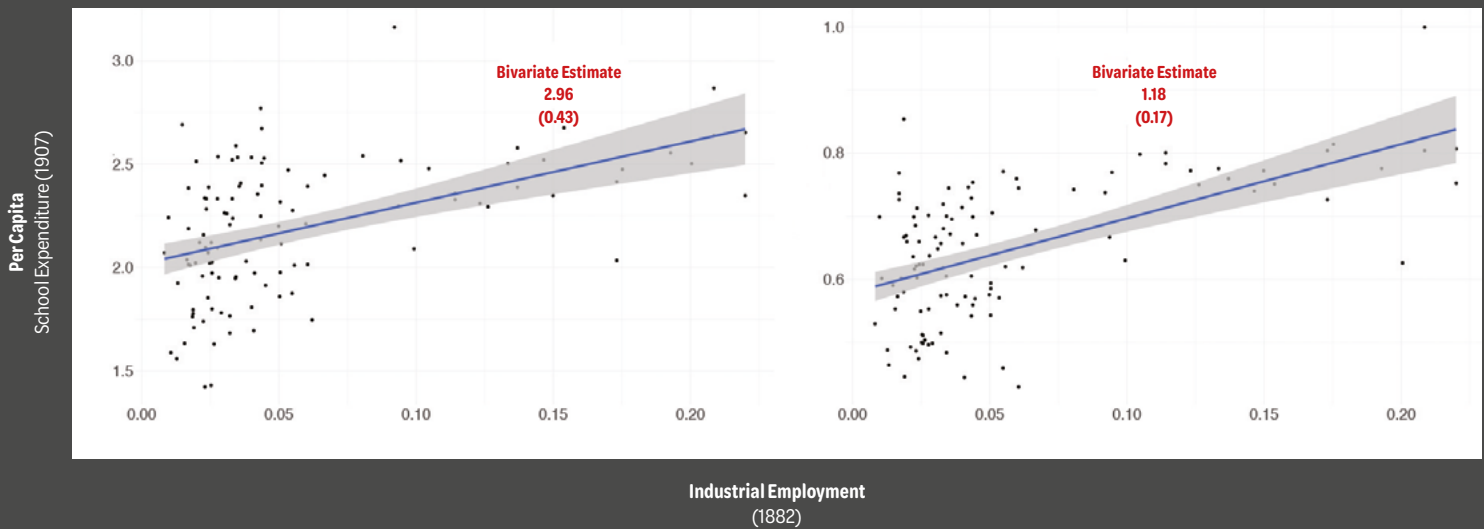
Figure 1:

Industrial Employment
Across Prussia

Note: This map shows the spatial distribution of the main proxy for capital ownership at the county level: industrial employment as share of total employment. Darker shades of purple indicate higher shares of industrial employment. Dark gray dots indicate the 125 cities in the sample and their location across Prussia.

characteristics, yet differ in whether they are located in more rural/agrarian or industrial areas.

One major advantage of studying the historical Prussian case is the availability of high-quality data. Prussia and the German central state were prolific data collectors and undertook regular censuses at various administrative levels. Most of the censuses are available as books in libraries, but many have now been scanned

**Figure 2:**

Bivariate Relationships:
Industrial Employment and
Education

Note: The left plot shows the bivariate relationship between logged per capita school spending with industrial employment.

The estimated coefficient in the bivariate regression is 2.96 with a standard error of 0.43 (clustered at county level). The right plot shows the bivariate relationship of school enrollment with the share of industrial employment. Here the estimated coefficient is 1.18 with a standard error of 0.17 (clustered at county level). The coefficient from the bivariate regression and clustered standard errors are shown in red in each plot.

and early 20th century, higher investment in industrial capital in a given locality ought to be associated with higher industrial employment.

Figure 1 shows the spatial distribution of the main proxy for capital ownership at the county level: industrial employment as the share of total employment. Darker shades of purple indicate higher shares of industrial employment. The 125 cities in the sample and their location across Prussia are marked as dark gray dots. Many of the cities are concentrated in the more industrial west, yet a significant share is located in the more agricultural east of Prussia.

I measure public investment in education using data on per capita school expenditures (logged) and primary school (Volksschule) enrollment at the city level at the beginning of the 20th century. Using standard linear regression and spatial autoregressive models, I show that the share of industrial employment at the county level in the 1880s, the main proxy for capital ownership of elites, is positively related to both school expenditures and enrollment. The relationship between cities' educational investment and the importance of industrial capital is estimated to be positive and substantively important for

both school expenditure and enrollment. The results are quite similar in bivariate models and when adding different sets of covariates, including province fixed effects. Only for school enrollment does the coefficient of industrial employment vary in size depending on the covariates included.

Figure 2 shows the bivariate relationship with industrial employment for both dependent variables. The coefficient from the bivariate regressions and their clustered standard errors are shown in red. If we take the results from the arguably most conservative models (i.e., the full set of controls and province fixed effects), the coefficient for industrial employment on spending and enrollment are estimated to be 2.45 (clustered SE: 0.5) and 0.4 (clustered SE: 0.16), respectively. For these models, substantive effects are such that a one standard deviation increase in industrial employment is associated with a six percent increase in logged per capita expenditure and a three percent increase in school enrollment.

To further add to the credibility of the results, I estimate the relationships in spatial two-stage least squares models (Betz, Cook and

Hollenbach 2019). I instrument industrial employment with the distance to carboniferous rock strata. These rock formations developed over three million years ago, are predictive of coal deposits today, and provide a strong instrument for local industrial development. The results from the instrumental variable models are quite similar to those in the original OLS regressions and lend additional credence to the findings.

Capital Ownership, Inequality, and Capacity Investments

In the paper discussed above, I establish the empirical connection between the industrial elite and government investment in public education at the local level. In work in progress, I further consider how the need for revenue to finance public goods might lead governments to invest in the fiscal and administrative capacity of the state. Here, I consider two factors. First, unless the state can raise revenue outside of taxation, the demand for spending identified above should bring about investments in the revenue raising capacity of the state. Elites that demand higher government spending should, therefore, also be proponents of state investments in the ability to raise revenues. When elites in power are invested in types of capital that are complemented by government spending, they should favor higher taxation. In particular, if taxes can decrease their share in paying for these goods.

On the other hand, should current political elites lose power, a state with higher administrative and fiscal capacity could potentially be used against them (Acemoglu, Ticchi and Vindigni 2011; Besley and Persson 2011). All else equal, higher levels of fiscal capacity should increase the level of taxation or redistribution after a potential regime change. Elites that fear redistribu-

tion after a potential regime transition should be more opposed to investments in fiscal capacity. Instead, these elites may have an interest in preserving a weak state to protect themselves from future taxation (Acemoglu, Ticchi and Vindigni 2011). As a consequence, if regime change is possible, high inequality should predict lower investment in fiscal capacity.

To investigate the fiscal capacity part of the larger theoretical argument, I collected data on the fiscal and administrative capacity at the county level in early 20th century Prussia. Specifically, I use data on income tax revenues as well as the share of state and municipality employees to total employees in 1907 to measure revenue collection and administrative capacity. Figure 3 shows the growth in state and municipal employees as the share of total employment from 1895 to 1907. Both this measure of administrative development and per capita income tax revenue at the county level, are positively related to the share of industrial employment in 1895, indicating support for the idea that industrial elites were supportive of higher capacity to tax. Evidence with regards to the second part of the theoretical argument is less clear. There is no evidence that land or proxies for income inequality have negative effects on growth in administrative employment or per capita income taxes.

On Generalizability and Scope Conditions

The results discussed above reflect the political situation during a specific period in a particular case. What can the analysis of historical Prussia or other historical cases tell us about politics today? As with studies of contemporary data, empirical investigations of historical cases allow us to learn about human behavior and politics beyond the particular case or time period. If po-

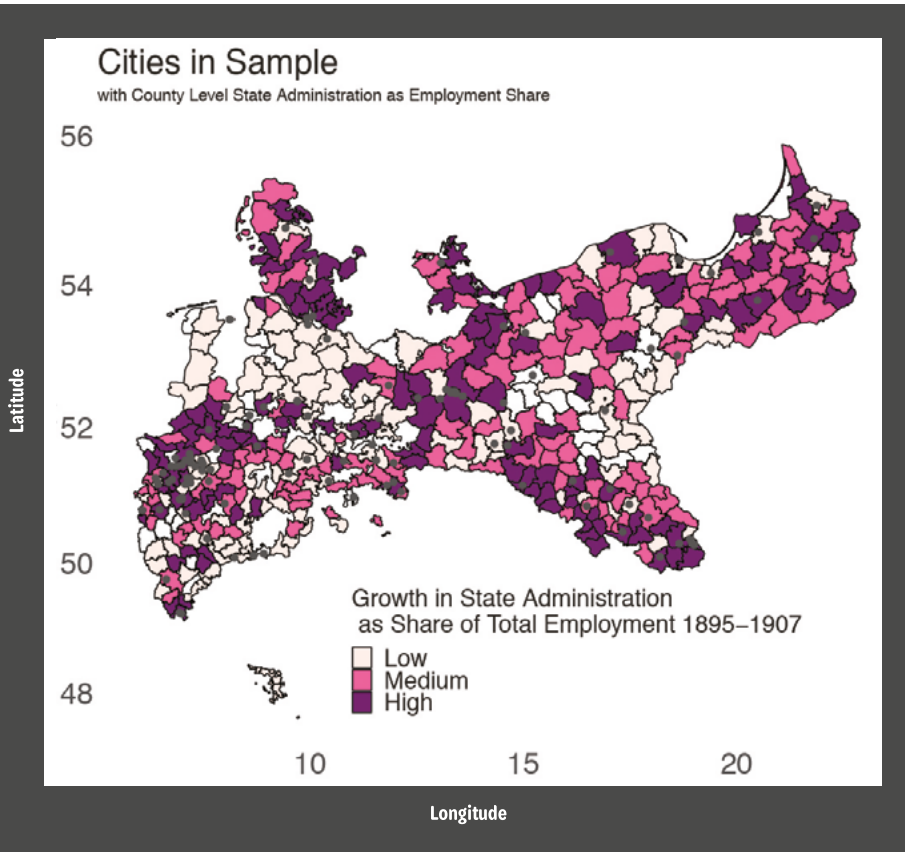


Figure 3:

Growth in Municipal Employees Across Prussia

Note: This map shows the spatial distribution of growth in state and municipal employees as the share of total employment from 1895 to 1907. Darker shades of purple indicate higher growth rates. Dark gray dots indicate the 125 cities in the sample and their location across Prussia.

litical institutions and incentives drive political choices and behavior, then similar institutions and incentives should predicate similar behavior independent of time and space. Moreover, by focusing on a single country and time period, scholars can gain more in-depth knowledge of the particular case. Similarly, more information and data may become available over time. Historical studies can thus help us to evaluate general theories, but also teach us in detail about specific cases and time periods. Given our different perspective and approaches, political scientists might view the same cases in a different light than historians and might uncover new insights. For example, my work provides new systematic evidence about when and where local elites in Prussia pursued the expansion of administrative capacity.

In a similar vein, single country studies, both historical and contemporaneous, raise questions

about generalizability. By analyzing variation within a single country, researchers can control for important confounders by design. In my work discussed above, using subnational data from Prussia means that the political system is the same for all observations. To some degree, within-country research designs compared to cross-national analyses involve similar – though less stark – trade-offs as those of experimental compared to observational research designs. While offering greater internal validity, results from a single country can raise concerns about external validity.

In my view there are two important lessons given the concerns about generalizability of single case and historical studies. First, it is important that scholars are clear about theoretical and empirical scope conditions. What are the types of cases where our theoretical arguments apply and what are the characteristics of cases that we can learn about given the empirical analysis. For example, Prussia is a good test case for the above discussed theoretical arguments given the high overlap in economic and political elites. And second, empirical investigations of theoretical arguments need to be replicated in other settings that fit the particular scope conditions. This implies, however, that we need more empirical studies of a single theory, i.e., not every published paper should present a new theoretical argument.

Conclusion

When do non-democracies increase government spending levels and invest in the ability of the state to collect taxes? While much of the existing work focuses on taxation as a threat to economic elites or war as a driver of fiscal capacity investments, I argue that the capital bases of political elites can provide incentives to

raise public spending on education and thereby increase demand for tax revenues. The political and economic environment of Prussia at the turn of the 20th century is an optimal case to investigate these arguments. During this time, Prussia underwent a massive, but geographically uneven, economic transformation. Moreover, the Prussian state introduced its first comprehensive income tax, which was administered at the local level. In addition to the large subnational variation, the political system ensured the political power of economic elites. Areas with strong industrial development were more likely to invest in primary education and the administrative development of the state. The theoretical argument and these findings are also in line with recent work by Becker and Hornung (2019), which shows that politicians from heavily indus-

trial constituencies were more likely to pursue liberal policies in the Prussian parliament. In this case, redistributive threats, or inequality, seem to be less important as determinants of state capacity development. One potential explanation is that Prussian elites, through the stability of the political system, felt sufficiently insulated from future redistributive threats. Additionally, many landed elites were able to use administrative rules (e.g., the status of manorial estates) to protect themselves from local taxation. In future research, I more systematically evaluate the role of political and administrative rules in undermining the central state's administrative reach, as well as the personal characteristics of local politicians and bureaucrats. ●

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THE HISTORICAL TURN IN THE COMPARATIVE STUDY OF POLITICAL ELITES

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Political elites are inherent to the political life of any complex society. Since the Neolithic revolution (10,000 B.C.), the “political class” has been a feature of agricultural societies with large populations and a specialized division of labor (Boix and Rosenbluth 2014). The division of labor extends to the political domain, where we can divide society between those whose main task is to govern and the rest; in short, between rulers and ruled, between the political elites and the “masses.” While research on political elites was central to comparative politics for decades, it dwindled after the 1970s. This piece examines the resurgence of quantitative comparative scholarship on political elites. In particular, I shall discuss a small but growing wave of research that leverages important historical events to study the origins of political elites as well as the consequences of their actions.

Some insights from early research on political elites

The study of political elites used to enjoy an impressive pedigree in political science. The importance of the political elite was not lost on

Gaetano Mosca, Wilfredo Pareto, and Robert Michels, the three classical elite theorists in the late 19th and early 20th centuries that shaped modern political science. In 1936, Harold Laswell—who famously defined political science as “the study of who gets what, when, and how”—equated the study of political elites with the study of politics (Rustow 1966, 692).

Earlier scholars such as Mosca, Pareto, and Michels tended to see the political elite as a unified group that dominated the “masses”, while later scholars find that political elites are often incoherent and fragmented (e.g. Dahl 1961; Quandt 1969).¹ Yet others use the concept of a “power elite” (Camp 2002; Mills 1956) to highlight that political, economic, and military elites are distinct but interwoven. Regardless, all agree on a minimalist definition: a member of the political elite possesses much more influence and power in public affairs than an ordinary member of society (Putnam 1976, 140).

Beyond Mosca, Pareto, and Michels, multiple new insights emerged from the comparative study of political elites between the 1950s and

1. For an empirical discussion on that point, see Putnam 1976, chap. 3.

the 1970s.² Many scholars collected data on the social background of elites. One important finding of this scholarship was to show that political elites emanated from socioeconomically advantaged backgrounds not only in Western countries but particularly in non-Western ones, from Morocco to Maoist China, for all its peasant rhetoric (Putnam 1976, 194). Quandt (1969, 19), for example, collected data on cabinet ministers and senior civil servants in post-colonial Algeria. While both emanated from relatively advantaged social backgrounds, he showed that ministers had more political power but civil servants enjoyed higher social status. Extensive knowledge of the Algerian case allowed Quandt to conclude that the incongruence between civil and political elites led to political instability after independence, an insight that likely travels to many other post-colonial societies. Most pertinent to this Newsletter issue, some scholars posed ambitious historical questions (Putnam 1976, chap. 7), even if they were difficult to answer in the pre-digital age, such as: How did the Industrial Revolution transform the composition of elites in Britain and elsewhere? Did the emergence of a postindustrial society lead to a preponderance of managerial and technocratic elites?

This wave of mid-20th century research appears to be sidelined or perhaps forgotten by some 21st century comparativists, including by top scholars who have recently reviewed some of the literature on leadership and political elites (Ahlquist and Levi 2011; Gerring et al. 2019). There are at least two reasons for this sidelining. First, much of the literature on elites in the 1950s-1970s was largely descriptive. The com-

mon “social background approach in elite studies” collected quantitative biographical information on the social background of elites in the hopes of inferring their behavior. However, as Searing (1969, 474) pointed out, this approach rarely connected background variables to larger historical and political transformations or to data on elite attitudes, behaviors, and policies.

Second, from historical institutionalists to survey researchers, the study of political institutions and of public opinion have dominated comparative politics since the 1980s. Many scholars of institutions and public opinion adopted rational choice theory: from Downs’ median voter to formal theories of elite decision-making, the inputs that mattered were the rules of the game (institutions) and the preferences of constituents (public opinion).³ Further, public opinion became easier to study as large surveys became easier to implement and analyze. The study of rules and structures has dominated over the study of agency and, among those studying agency, the study of ordinary citizens has dominated over the study of elites. There is no doubt that public opinion matters, especially in democratic settings, and that institutions are fundamental to understand “who gets what.” Rather, the point is that the volume of work on institutions and public opinion may have overshadowed pre-1980s research on political elites as well as research on elites since then.

It is unfortunate that elite preferences have long been relegated from center stage, at least outside the subfield of American Politics, because elite opinions probably matter more than those of the average citizen (that would not be true

2. Putnam (1976) provides an incisive and comprehensive synthesis that organizes our knowledge on political elites up to that point.

3. I thank Noam Lupu for this point.

in a direct democracy where governing elites strictly implemented their constituents' will, but no political regime is a direct democracy in theory or in practice). Further, in less democratic settings, where institutions are weaker, elites have even more agency precisely because they can bend the rules. That is consistent with what we have learned from a wealth of research that studies who benefits from distributive politics. Specifically, a wealth of studies on distributive

Research that directly engages political elites has resurfaced in comparative politics and political economy since the 2000s and especially in recent years.

politics show that governments in developing countries tend to disproportionately favor their home region(s) or ethnic group(s) via patronage, clientelistic politics, and otherschemes (Golden and Min 2013; Hodler and Raschky 2014; Kramon and Posner 2013). In that sense, then, we have learned a lot about the consequences of elite behavior for welfare and the public provision of goods, among other important outcomes. However, research on distributive politics typically speaks much more to the literatures on clientelism, ethnic politics, mass political behavior, and institutions, than to political elites per se. That is one reason why it is often unclear what these studies actually tell us about elites given that distributive politics and regional favoritism may be the result of elite agency, of institutional arrangements, of popular preferences, or of some mix of these factors.

A renewed interest in political elites

Perhaps as a reaction to the vast literature on institutions and public opinion, research that directly engages political elites—while never entirely absent—has resurfaced in comparative politics and political economy since the 2000s and especially in recent years (e.g. Ansell and Samuels 2014; Besley and Reynal-Querol 2011;

Camp 2010; Humphreys, Masters, and Sandbu 2006; Jones and Olken 2005; Rainer and Trebbi 2012; Reuter and Szakonyi 2019; Searing, Jacoby, and Tyner 2019; Truex 2014). There are likely other reasons for this renewed interest. For one, we cannot adequately understand political representation and the political process without directly studying elites, as Laswell told us. For another, “hot topics” in academic research often come and go in waves.

Some of these recent studies on elites improve upon the 20th century wave of elite research discussed above. Carnes and Lupu (2015) revisit the important question of whether education makes for better political leaders. They use cross-national and US Congress data to show that, contrary to earlier findings, “politicians with college degrees do not tend to govern over more prosperous nations, are not more productive legislators, do not perform better at the polls, and are no less likely to be corrupt” (Carnes and Lupu 2015, 36). Dal Bo et al. (2017) greatly improve upon the aforementioned social background approach in elite studies by combining fine-grained administrative data on Swedish politicians with similarly detailed and until recently unavailable data on the entire Swedish population. They argue that political elites in Sweden constitute an “inclusive meritocracy” because, “although politicians themselves are disproportionately high earning, their parents’ social class and earnings approximate a perfect replica of the entire population” (Dal Bo et al. 2017, 1881).

A historical turn in the comparative study of political elites

An important part of this renewed interest on political elites concerns research by mostly junior scholars that lies at the intersection of com-



parative politics and history (e.g. Abramson and Velasco Rivera 2016; Cirone and Van Coppenolle 2019; Dube and Harish 2017; Guardado 2018; Nathan 2019; Wilkinson 2015).⁴ This work reflects a growing interest in “micro-historical research” in comparative politics and historical political economy. Compared to macro-historical analysis (e.g. Moore 1968), micro-historical research switches the unit of analysis to the subnational level or even to the individual. This allows scholars “to examine the interplay of political and economic factors on the decisions of politicians” and to “disaggregate the multiple mechanisms implicit” in more macro-historical explanations (Mares 2015, 233–35).

In the remainder of this article, I highlight my research as well as that of three other comparativists whose work also intersects comparative politics, historical political economy, and elites. The goal is to present this growing field of research and some of its findings to all comparativists, and especially to those unacquainted with it. I select four articles that cover different periods (from the 18th to the 21st century) and regions (Africa, Europe, the Middle East, and Latin America) in order to present approaches and findings across a wide range of contexts.

Substantively, the four articles ask different questions but all leverage historical transformations to understand how profound socioeconomic changes impact political representation, the origins of political elites, and their behavior and policies. However, they do not simply leverage history for instrumental purposes. They also try to contribute to a better understanding of history. I am one among a growing number of po-

litical scientists who believe that understanding history is intrinsically interesting, and the rigorous study of elites is one important avenue to comprehend a region’s or a country’s political history.

Methodologically, all four try to creatively combine new tools of quantitative political science with extensive research of historical sources and archival research, including detailed data collection on elite biographies. This typically requires a combination of language skills (Arabic, English, French, and Spanish in these four articles) and statistical know-how (e.g. causal inference techniques for observational studies, text as data), thereby bridging humanistic and scientific skills.

Colonial investments, political elite formation, and regional inequality in East and West Africa (1900s-2010s)

My own work (Ricart-Huguet 2019) is motivated by the research mentioned above showing that elites disproportionately favor their home region. The biased provision of goods, and clientelism in particular, are problematic because they undercut programmatic policies. However, to the extent that power is distributed across regions in a manner that is roughly proportional to their population, one could argue that each region is receiving its “fair share.” Power is actually allocated in such a manner in legislatures, where seats are proportional to the population absent malapportionment. However, that need not be the case in cabinets, which are locus of political power and representation in semi- and non-democratic countries.

4. The title of this piece is inspired by Putnam’s (1976) *The Comparative Study of Political Elites*. The title does not imply that scholars had not studied this intersection before, it is rather a matter of shifting emphases. In fact, historian Ronald Syme went as far as back as collecting individual biographies of Roman elites to study how the composition of the oligarchy changed when Rome transitioned from a Republic to an Empire (Rustow 1966, 694).

I examine the extent to which power is unequally distributed across regions in a set of 16 former British and French colonies in sub-Saharan Africa that underwent similar colonial experiences and became independent around the same time, in the 1960s. To do so, I collect biographical data on roughly 5,000 post-independence ministers in these 16 countries to determine whether some regions have been over-represented and others under-represented in post-independence governments (1960-2010). I find that regional political inequality has remained stark in some countries such as Niger and Tanzania, where many ministers were born in a handful of regions, while it has been less pronounced in others such as Benin and Uganda, where ministers have hailed from various regions. In all countries, however, some regions have been systematically over-represented and others under-represented with respect to their population share over these 50 years. Why?

Existing literature focuses on important but short-term strategic considerations. Some argue that the leader selects ministers from his region or group disproportionately (regional favoritism) while others argue that leaders select ministers from multiple regions to reduce alienation and conflict (regional balancing). My research provides instead a long-term explanation. I combine the ministerial biographies with original colonial records and maps to show that post-independence regional representation in cabinets, as proxied by minister shares by region, results from colonial primary education rather than from other investments, economic development, or pre-colonial ethnic characteristics. Individuals from regions with more primary education were recruited disproportionately into the colonial civil service and the rubber-stamp colonial legislatures by adminis-

trators that were intent upon recruiting literate subjects. This selection criterion had unintended long-term consequences for regional political inequality: just as literacy and numeracy learned in school were transferable to the civil service and the legislature, the skills and status acquired in these two colonial institutions provided a political advantage to these incumbents' home regions after independence.

The importance of colonial education, I find, extends beyond the momentous post-colonial decade of the 1960s into the 2000s. For all the political instability and violent conflict in post-colonial Africa, colonial-era inequalities remain. One reason for this persistence is that political elites have generally reproduced rather than reduced the large regional inequalities inherited from the colonial past. In fact, political elites are particularly well-placed to maintain education and economic inequalities stemming from the colonial era so long as they continue to hail from districts that were already ahead during the colonial period.

Colonial land reform, political elites, and redistributive conflict in the Middle East (1920s-1960s)

Hartnett's (2019) work focuses on two former British colonies, Iraq and Jordan. Both were former UN mandates later ruled by two Hashemite brothers. However, the land reforms undertaken by the British to "modernize" their land regimes and land property rights were very different in the two neighboring colonies. In Jordan, the land reform provided property rights that were backed by the state to various social classes (peasants, sheikhs, and merchants). By contrast, in Iraq the land reform favored sheikhs, who were given larger holdings. The land reform



in Iraq formalized economic inequality and provided a political advantage to the landed elite already during colonialism that continued after independence.

Hartnett collects data on ministerial appointments in both countries from 1920 to 1967 to show that the consequences of the type of land reform for cabinet composition were stark. After independence, Jordanian cabinets were inclusive, diverse, and representative of different social classes. By contrast, Iraqi cabinets resulted from a power-sharing arrangement between the landed elite and tribal landowners. Unlike in British African colonies, the educated urban class (*effendiya*) were at first excluded. The political exclusion of social groups, notably the *effendiya*, led to much more redistributive conflict in post-colonial Iraq than Jordan.

Economic elites and public goods provision in Chile and Argentina (1850s-1960s)

Paniagua's (2019) work on the Chilean economic and political elite (1849-1907) also concerns the political effects of land inequality, but the take-away is very different from Hartnett's. Local landed elites that perceived a high risk of expropriation strategically diversified their portfolio holdings (finance, manufacturing) following the approval of an 1854 corporate law that regulated joint-stock companies and enabled investment diversification. Members of the economic elite who diversified were then more likely to become national—as opposed to local—politicians in order to “steer state intervention” towards policies that would support broader development and not only narrow rural interests. Districts with diversified elites experienced higher rates of public goods provision (proxied by literacy), thereby tying elite behavior to policy outcomes.

In Hartnett's work, the economic elite in Iraq has narrow interests because its holdings are in one sector: land. Paniagua, on the other hand, shows that landed elites and broad development-oriented public goods provision can actually coexist as long as the asset portfolios of these elites become diversified enough.

The Industrial Revolution and political representation in Britain (1700s-1880s)

Finally, Fresh (2018) revisits the central question of whether modernization facilitates greater political elite turnover by examining the most important economic structural transformation in modern history, the Industrial Revolution. She collects the biographies from the Members of Parliament in Britain between 1708 and 1884, along with other data, to show that the Industrial Revolution increased political competition, reduced the presence of political dynasties, and diversified the economic interests represented in Parliament from predominantly landed interests to international commercial interests.

Fresh leverages exogenous local variation in the presence of coal-bearing bedrock, a key source of energy to fuel the Industrial Revolution, to show that the above political changes are spearheaded by these areas in particular. The individual-level data allow her to show that these changes came about because new economic elites, rather than “old elites in a new economic guise”, became in turn political elites. Therefore, existing pre-industrial political elites were unable to leverage their positions of political power to limit economic change or capture it for their benefit, suggesting an interesting limit to how political elites can protect their positions of power.



Moving forward

Comparative politics has long learned from history and used historical approaches (e.g. Putnam 1993; Tilly 1975). The recent turn combining the historical *and* quantitative study of political elites is refining the questions that comparativists are asking and pushing the methods used to answer them.

This essay showcases new research on political elites that may be outside the purview of many scholars by highlighting the work of comparativists who, while working in different topics and regions, share a deep interest in history and in quantitative social science.

In 1966, Dankwart Rustow commented that “scholars in comparative politics have been turning away from the institutional-legal approach of a previous generation.” “Elite studies hold great promise” especially in developing countries, he argued, because most of them are not democratic and hence institutions are less constraining (Rustow 1966, 695). While elite studies remained important in comparative politics through the 1970s, the study of institutions and of public opinion became the core of comparative politics thereafter. Over 40 years later, and in a context where democratic backsliding seems to be affecting even developed countries, a renewed interest in the (historical) study of political elites suggests that Rustow may soon be right again. ●

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HISTORY, POLITICAL SCIENCE, AND TIME

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We are living in a golden era of historically oriented research in the comparative politics sub-field. Scholars have deployed a variety of quantitative and qualitative methods to explore heretofore-understudied causal relationships in countries around the world and extending back several decades or even centuries. Daniel Ziblatt's archival work and statistical analysis has established the importance of civically oriented conservative parties for the stabilization of democratic regimes (Ziblatt 2017). Scholars like Amal Ahmed and David Stasavage have explored the origins of electoral institutions and taxation systems back to the 17th, 18th and 19th centuries (Ahmed 2013; Stasavage 2003). Grigore Pop-Eleches and Joshua Tucker have shown

how a sophisticated quantitative analysis of public opinion data can establish the reality of "Leninist legacies" affecting the mindsets of contemporary populations across post-communist Europe (Pop-Eleches and Tucker 2016). Works by Anna Grzymala-Busse (2015), Jeffrey Kopstein and Jason Wittenberg (2016), Daniel

Slater (2010), and Kathleen Thelen (2004) have built on earlier findings in the tradition of comparative-historical analysis to produce major works on the origins and trajectories of church-state relations, interethnic violence, authoritarian regimes, and workforce training in different nations. Statistical studies controlling for a wide variety of intervening variables show remarkable correlations between specific historic events many decades ago and recent po-

litical phenomena (Darden and Grzymala-Busse 2006; Rozenas, Schutte, and Zhukov 2017). If there were ever any doubt that "history matters" for comparativists, it has surely been eliminated by now.

Yet while there is a great deal of new and exciting work linking historical causes to long-

run political outcomes in particular cases, there is not much theoretical attention in the discipline to the question of how we might conceptualize the relationship between political science analysis and historical research in general. Even as political scientists dig deeper into archives in multiple languages and historians engage in

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and historical research.*

“global history” with an implicitly comparative perspective, there is little direct intellectual dialogue between social scientists and practicing historians. As any conversation across today’s disciplinary divide will quickly demonstrate, political science and history remain deeply divided by differing epistemological and ontological assumptions. Most political scientists, even if they take history seriously, still aim to produce valid generalizations; most historians instinctively reject any effort to make “causal inferences” in social analysis that might possess general validity across time and space.

Such a sharp division between the disciplines of political science and history is neither inevitable nor salutary. Indeed, during the 19th and early 20th centuries, debates about historiography as well as historically-oriented research were deeply entwined with the early development of social science theory—and vice versa. Pioneering social scientists such as Marx, Durkheim and Weber eagerly sought out the latest findings of historical research as they worked to build their overarching theories of social development. In turn, leading historians ranging from E.P. Thompson and Fernand Braudel to Eugen Weber and Lynn Hunt took direct inspiration from debates in the social sciences to explore new approaches to social and cultural history. Philosophical works by authors such as R.G. Collingwood, Karl Popper, Hannah Arendt, and Thomas Kuhn endeavored to show the possibilities as well as the limits of scientific approaches to human affairs as well as the sociological dimensions of the history of science itself—and their works were devoured by legions of political science graduate students in the post-WWII era. Yet today, metatheoretical and philosophical debates of this sort are no longer attractive to a data-driven social science com-

munity highly skeptical of all “grand theories.” We rarely ask ourselves the core question: can we truly be scientific about the analysis of the history of our own species?

I will argue here that exploring how political scientists should understand history requires us to reengage with the fundamental philosophical questions that animated academic debates in the 19th and 20th centuries. Ironically, then, successfully relinking political science and history requires us to look at more closely at the history of our own discipline. In doing so, we discover a central problem largely elided in the comparative subfield today: namely, a century and a half after the foundation of modern social science, we still lack any scholarly consensus about how to think scientifically about temporality in human affairs. Instead, political scientists can be roughly divided into three rival camps: a majority that tries theoretically and methodologically to abstract from temporal considerations; a second group that hopes to embed political science in theories of evolution; and advocates of an emerging approach that sees temporality and the construction of political memory as factors that must be explicitly taken into account. This last approach holds the key to relinking historical and social scientific research in a mutually beneficial dialogue.

Abstracting from Time

The idea of a modern science that could eventually explain all empirical phenomena in the universe, across time and space, dates back over two centuries—at least to the work of Pierre-Simon Laplace. As Laplace wrote: “We ought...to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow. Given for one instant an intelligence which could comprehend all the



forces by which nature is animated and the respective positions of the beings who compose it—an intelligence sufficiently vast to submit these data to analysis—it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes” (Laplace 1995 (1825), 4). Laplace’s vision of a fully deterministic universe, in which “timeless” scientific laws might explain the characteristics of empirical phenomena in all times and places, was subjected to intense philosophical criticism over the course of the 19th and 20th centuries. Yet his philosophy of science maintains its appeal for a significant subset of political scientists. Indeed, much of what we now accept as conventional social science methodology is, in one way or another, designed to take temporality—literally and figuratively—out of the equation.

The most common way to generate a “timeless” social science is to analyze all human behavior, in all times and places, as if it were driven solely by a single, universal form of motivation: the maximization of utility. Adopting this axiom allows scholars to develop sophisticated mathematical models of behavior that satisfy the properties of Nash equilibria, in which no single actor has any unilateral incentive to change her behavior—and thus the resulting outcome as a whole should persist in perpetuity, barring some “exogenous” shock. Time enters into such models only in the form of actors’ hypothesized “discount rates,” that is, the rates at which rational individuals discount the value of future payoffs relative to more immediate ones. Yet for reasons of mathematical tractability, the vast majority of rational choice models set a single invariant discount rate for all actors, rather than allowing expectations about the future to vary

among individuals and over time (as is presumably always the case in the real social world). Even if rational choice models are utilized to analyze particular time-bound political situations, in and of themselves, they are essentially static. For those who remain convinced that real scientists must abstract entirely from temporal and spatial contexts, the true test of a model’s success is its internal mathematical consistency, rather than its accuracy in predicting real world outcomes.

In recent years, the imposing edifice of social scientific model building in the disciplines of political science and economics has begun to erode under the weight of new neuroscientific and psychological findings about actual human cognition. Put simply: real world human beings are not only “boundedly rational” at best—as Herbert Simon taught us long ago—but seemingly quite irrational emotional states, ideological principles, and in-group myths frequently drive their behavior. The former assumption that “irrational actors” would always be “selected against” in social competition, and thus could be safely ignored, has given way to a growing realization that politicians who are able to generate and reinforce powerful ideas, identities and emotions can sometimes push entire societies in hitherto unexpected directions (Hanson 2010).

For these reasons among others, a growing number of social scientists have looked instead to the experimental method as a more reliable way to tease out “timeless” causal relationships from time-bound social contexts (Gerber and Green 2012). With proper experimental design, laboratory science allows a researcher in principle to control for all merely local and/or temporary features of an empirical environ-

ment, in order to observe the “pure” effects of a particular experimental treatment. Laboratory experiments have decisively proven, for example, that penicillin kills streptococcal bacteria not only in the United Kingdom in the 1920s, but also around the world and for as long as streptococci maintain the same biological structure. Conversely, given the absence of laboratory confirmation in other times and places, it is not a scientific defense of early claims of the discovery of “cold fusion” to say that it “really worked, but only for a short while in Utah.” Of course, ethical and practical constraints limit the ability of social scientists to subject human subjects—as opposed to strains of bacteria or subatomic particles—to rigorous tests in controlled laboratory conditions. Still, it is alluring to imagine that the cumulative results of carefully designed field experiments, online and in person, in societies around the world, might eventually generate robust causal findings about politics.

Certainly, the trend over the past decade toward the testing of social scientific hypotheses through field experiments in multiple countries has led to a great deal of fascinating research. Thus far, however, it is fair to say that the various findings of particular political science field experiments have yet to add up to any new, widely accepted general, cross-national propositions governing political behavior—notwithstanding some admirably rigorous efforts to synthesize multiple experiments across societies (Dunning et al. 2019). No matter how well-designed an experiment may be, and no matter how strong and statistically significant the results of any given experimental finding, there is no guarantee that it will turn out to be fully replicable across differing temporal and spatial contexts. In order for the results of field experiments around the world to cumulate, political scientists would

need a widely-accepted general theory identifying precisely which contextual factors to control for in varying societies situated in diverse geographic regions, with different historical legacies—precisely the sort of “grand theory” of political development that our discipline has largely abandoned.

Perhaps the advent of supercomputers capable of crunching enormous amounts of data, combined with new machine learning techniques to refine algorithms for analyzing it, might allow us to find truly “universal” correlations in human affairs, valid across all times and places? There can be no doubt that “big data” approaches to social science analysis are an enormously useful tool for evaluating the impact of policy choices in a wide variety of social settings (BenYishay et al. 2017). Yet it seems unlikely that such approaches will make the rest of comparative politics obsolete. The most ardent acolytes of “big data” sometimes sound very much like Laplace in their depictions of a future in which most aspects of human behavior might be successfully predicted (Harari 2015). The problem, however, is that social science datasets are obviously much more complete for advanced societies in contemporary times than for poorer countries or societies in the distant past, introducing potential omitted variable bias in even the most sophisticated big data analyses. Coupled with this well-known problem of data scarcity is a more fundamental philosophical issue concerning the nature of human agency. If human cognition and behavior are in fact reducible entirely to algorithms, perhaps big data researchers will eventually be able to predict political and social outcomes in countries around the world and far back in history. If not—if human beings are inherently capable of acting in fundamentally novel ways through non-algorithmic

forms of cognitive processing—even the most sophisticated big data analyses may generate results valid only for more limited historical periods, with temporal scope conditions that must be identified through some means other than inductive “machine learning” approaches themselves.

Evolutionary Approaches

If social scientists cannot eliminate temporality entirely from social analysis, perhaps it is better to model our discipline more explicitly after the one branch of the natural sciences that unabashedly takes time explicitly into account—evolutionary biology. There is no longer any serious debate among scientists (as opposed to politicians and theologians) that *Homo sapiens* evolved out of other species of great ape. Ever since Darwin, we have understood how the theory of natural selection helps to explain a remarkably diverse set of biological developments ranging from the extinction of the dinosaurs to the evolution of drug-resistant bacteria. If evolutionary theory in the biological sciences can help us discern relatively general laws of development over wide stretches of historical time, it certainly seems possible in principle that an evolutionary political science might eventually achieve the same result.

The idea of applying evolutionary theory to analyze human affairs is hardly new. On the contrary, it is arguably as old as social science itself. One could fill an entire library with works endeavoring to apply one or another approach to biological evolution to the subject of “social” or “cultural evolution.” Much of this work has aged very badly. The literature on the subject is riddled with outmoded jargon, logical fallacies, and unsupported hypotheses. Worse, politicians have

explicitly used theories of human evolution to justify racism, dictatorship, and even genocide. The presumed link between evolutionary theory in the social sciences and the philosophy of “social Darwinism”—that is, the idea that the laws of nature have doomed humankind to an endless struggle in which the “fittest” must survive at the expense of the “less fit”—makes the entire project of evolutionary thinking unpalatable to many, even if social Darwinism has little or nothing to do with Darwin’s actual theory. Against this background, one might reasonably conclude that attempting to rescue Darwinian thinking for social scientific purposes is a fool’s errand at best.

Yet evolutionary reasoning nevertheless keeps making its way back to the center of social scientific debates in a wide variety of disciplines. Social psychologists are increasingly interested in tracing how biological evolution may have shaped the human brain in ways that facilitate and steer social cooperation and conflict (Barkow, Cosmides and Tooby, 1995). Economists applying a historical approach to understanding the emergence of market economies have frequently deployed evolutionary metaphors and approaches (Nelson et al. 2018). Political scientists have used evolutionary theories to explore the role of gender in politics, the causes of war and peace, the efficacy of face-to-face diplomacy, and variation in types of state organization, among other topics (McDermott and Hatemi 2017; Gat 2009; Holmes 2018; Steinmo 2010). Thus, social scientists keep going back to Darwinian and biological thinking for theoretical inspiration, notwithstanding the often-sordid history of “social Darwinism.”

To date, however, progress in developing a consistent evolutionary theory has been hindered



by two key conceptual mistakes made by social scientists: social Lamarckianism and genetic reductionism. Social Lamarckians agree that human beings evolved out of earlier primates via natural selection, but then insist that the development of human “culture” has changed the nature of evolution among human beings to allow for the direct inheritance of cooperative institutions and behaviors through intergenerational learning. For most social Lamarckians, human culture has made “group selection” feasible in a way rejected by standard Darwinian evolutionary theory. Yet this line of reasoning is ultimately subject to the same functionalist fallacy that bedeviled earlier forms of Lamarckianism in the 19th and early 20th centuries. To say that a particular set of rules or actions are “functional” for the survival of a given group does not actually explain how such rules or actions emerge or are sustained in actual human communities. Nor can be sure any longer in the evolutionary axiom of modernization theory, namely, that “modern” impersonal institutional rules and individualist cultural values are ultimately destined to triumph over rival, “traditional” ways of life.

Genetic reductionists, meanwhile, correctly reject the arguments of the social Lamarckians as incompatible with core Darwinian principles, attempting instead to explain the central features of human social development directly in terms of biological causes. Yet this approach, too, does us little good in accounting for social change that unfolds at a much faster pace than these putative biological foundations. Social phenomena ranging from the rise and fall of nations to the emergence of new religions and ideologies clearly do not require any sudden transformation of the human gene pool. Indeed, as many genetic reductionists argue, the major

biological evolutionary influences on human cognition would have been pretty well set by the Pleistocene era, which ended over 10,000 years ago. How then can genetic explanations provide any insight into the specific dynamics of social change within recorded history?

For over 150 years, social Lamarckianism and genetic reductionism have been the Scylla and Charybdis of evolutionary theory in the social sciences, dooming countless theoretical voyagers in search of a social scientific approach to human affairs that might take temporality seriously. Advocates of each of these approaches has repeatedly pointed to the logical flaws of the other as a point of departure, thus perpetuating an essentially sterile debate. The way forward lies in embracing a third, relatively unexplored possibility: that biological evolutionary processes might have generated, among *Homo sapiens*, a parallel and autonomous dynamic of social change that has its own, independent temporality.

Toward a Contextual Social Science

Recently, political scientists and sociologists have questioned the idea that social analysis must always exclude temporal factors in order to be considered “scientific” (Hanson 2019). Of course, comparative politics as a subfield must always strive to advance hypotheses that have relatively broad applicability across multiple times and places. One need not believe that reality is entirely knowable and predictable though general laws to accept the more limited proposition that scientific knowledge, as a rule, proceeds by abstracting from local and contingent temporal and spatial contexts to discover more generally valid propositions. No science can generate collective knowledge if every sci-

entist can claim a local or temporal “exception” to a general finding willy-nilly.

Yet as scholars such as Paul Pierson (2004), Kathleen Thelen and James Mahoney (2010), Tim Büthe (2010) and Anna Grzymala-Busse (2011) have argued, taking social temporality seriously does not result in an unscientific free-for-all, but instead can help us discover new causal relationships in politics that are only visible over the long run. This emerging literature has shown clearly that there are generalizable principles at work explaining political and social outcomes over the course of historical time, which need not invoke any direct causal links to human biological evolution. Post-communist countries with deeper and more extensive social connections to the advanced West have clearly been more successful in political and economic reform—not withstanding recent setbacks—than their peers lacking such linkage (Kopstein and Reilly 2000; Levitsky and Way 2010). Twentieth-century elites in Southeast Asia who faced strong class and communal threats to their power and status have forged much stronger authoritarian states as compared to those established by elites in countries lacking well-organized opposition (Slater 2010). Countries in South America with indigenous empires, conquered in the first wave of European colonialism, have tended until recently to fare worse in economic and human development than those without such empires and colonized later on (Mahoney 2010). Such findings may not by themselves establish any “universal laws” of politics, but they are surely of profound importance for millions of people living across vast expanses of the globe.

Political scientists are also now beginning to explore patterns in the “subjective” aspects

of human temporality—that is, how we as a species collectively construct and experience time itself. Alan Jacobs has traced the reasons why politicians in some developed countries and not others have been able to take the “long view” in the creation and institutionalization of social welfare policies that, by their very nature, will not benefit their constituents until decades later (Jacobs 2011). Elizabeth Cohen has shown how the institutionalization of time in modern polities subtly reinforces established power structures while disenfranchising marginalized populations (Cohen 2018). Michael Bernhard, Jan Kubik and their collaborators have investigated the comparative politics of memory and commemoration in post-communist Europe, showing how particular types of memory construction generate specific kinds of political coalition (Bernhard and Kubik 2014). In my own work, I have explored the causal impacts of explicit ideological visions of time and history in the establishment of political regimes across Europe and Eurasia (Hanson 1997; 2010).

We are now in a good position to connect and synthesize findings from the comparative politics literature on “objective” temporal processes and studies investigating patterns in the formation of “subjective” political memory and pictures of the future. Indeed, I would argue that taking human temporality seriously is the key to understanding the specific, autonomous dynamics of social evolution, driven fundamentally by social rather than genetic processes and unfolding in a way that has no predetermined historical endpoint. Human beings evidently perceive of time and history in pattered ways, generating collective identities and related institutional attempts to order time and space that are either “selected for” or that “go extinct” due to structural factors discernable only over

the long run (Hanson 2017). If this sort of distinctly non-biological, non-Lamarckian evolutionary theory turns out to be correct, political scientists may have more reason than ever to study the formation of subjective political ideas as well as the contextual factors that limit or promote the spread of these ideas in different times and places—topics that have long been marginalized in the discipline.

These recent trends in comparative politics have brought us to an auspicious moment for reengaging our colleagues in history departments on a new basis of mutual scholarly respect. Reviewing the broad intellectual trends in our discipline as I have done here helps to explain the mutual suspicion between most contemporary political scientists and historians. Political scientists engaged in an effort to discover “timeless” forms of social equilibrium, who see changing historical parameters as “exogenous” factors of little intrinsic interest, clearly have little to say to scholars digging deeply into the quotidian details of human societies in diverse historical periods—and vice versa. Evolutionary theorists who see human history as either a story of adaptive progress or as a product of Darwinian genetic forces are unlikely to have much in common with historians

who wish to understand, interpret, and genuinely empathize with the diverse human perspectives they study. In contrast, our subfield’s recent embrace of human temporality in both its objective and subjective dimensions, along with the increasing use of archival and even archeological materials in research, should allow for the forging of more productive academic interconnections between practicing political scientists and historians. Political scientists may increasingly realize that they need to take historiographic debates more seriously in order to figure out the “scope conditions” that limit how far even our most powerful generalizations can travel (Lustick 1996; Hanson and Kopstein 2005; Kreuzer 2010). Historians engaged in global, comparative, and social history—as well as those who are increasingly interested in synthesizing such approaches—may discover to their surprise and delight that the best recent historically oriented political science sheds real light on long-term trends in regions they study (Eley 2002; Lepore 2018; Snyder 2010; Tooze 2018; Vinson 2012). Given the philosophical gulf separating political science and history departments at most universities, such a rapprochement will no doubt take time—but the potential intellectual rewards more than justify the effort.●

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HISTORY, MEMORY, AND POLITICS IN POST-COMMUNIST EASTERN EUROPE

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In October 2017, a commemorative plaque “In Memory of the 200,000 Poles Murdered in Warsaw in the German Death Camp KL Warschau” was unveiled in Warsaw. This was a somber ceremony, with the local priest performing Catholic rites and a representative of the Polish army honoring the dead.

The only problem – almost none of this was true. There indeed did exist a camp in Warsaw, where a few thousand Polish citizens died during the German occupation. But after the burning of the Warsaw Ghetto in 1943, this camp was turned into a concentration and extermination camp for Jews brought in from other parts of Europe, who were used as slave labor to clear the charred remains of the ghetto. A total of some 20,000 people died in this camp, most of them Jews. The Polish citizen movement behind this commemorative project was, therefore, not just commemorating victims of their own ethnic group at the expense of other victims – this is an unremarkable and largely ubiquitous feature of commemorative politics everywhere. What is, however, remarkable is that the very clear purpose of this commemoration was to put it in direct competition with the memory of the

Holocaust, especially in Poland, the geographic heart of the genocide.

This new historical remembrance in Poland has been going on for quite some time and has attracted much international attention (Charnysh and Finkel 2018). In 2018, the Polish government passed a law that criminalized the use of the phrase “Polish death camps” to designate German Nazi death camps in occupied Poland, such as Auschwitz, Treblinka and many others, but also criminalized any insinuation that individual Poles may have committed anti-Jewish crimes during the Holocaust.¹

Poland is hardly alone. As I document in my book, *Yellow Star, Red Star: Holocaust Remembrance after Communism* (Subotic 2019), this new historical revisionism has been flourishing across post-communist Europe, and is especially visible in historical museums, monuments and memorials, history textbooks, and rehabilitation and restitution laws. Over the course of four years, I sifted through hundreds of primary archival and secondary literature sources on the Holocaust and its remembrance in Eastern Europe, including newspaper coverage of com-

1. The law was amended in June 2018 to make the offense civil and not criminal.

I found a remarkably strong – but nationally flavored – trend of appropriation of Holocaust memory, especially its narrative and visual repertoire, to instead tell the story of communist oppression.

memorations, museum exhibitions and catalogs, oral testimonies, history textbooks, public speeches, theater, film, and literature sources. I conducted dozens of interviews, as well as museum and memorial site visits in six countries. I

conducted textual and visual analysis of these varied sources, and compared how they narrate the Holocaust differently over the decades. I looked for patterns of similar visual imagery across historical periods and documented when these patterns changed. While the cases I examined most closely – Serbia, Croatia, and Lithuania – varied considerably in historical and political circumstances of the Holocaust, the communist period, and the post-communist transition,

I found a remarkably strong – but nationally flavored – trend of appropriation of Holocaust memory, especially its narrative and visual repertoire, to instead tell the story of communist oppression.

For example, in 2014, the Historical Museum of Serbia in Belgrade put up a highly publicized exhibition *In the Name of the People – Political Repression in Serbia 1944-1953*, which promised to display new historical documents and evidence of communist crimes carried out by communist Yugoslavia in the first postwar years. The most stunning visual artefact displayed, however, was a well-known photograph of emaciated prisoners (one of them Elie Wiesel) in the Nazi Buchenwald concentration camp, taken by a United States Army soldier at camp liberation in April 1945. In the Belgrade exhibition, this canonic image – one of the most famous photographs of the Holocaust – was displayed in the section devoted to the Yugoslav communist era camp for political prisoners on the Adriatic is-

land of Goli otok, with the caption, “the example of living conditions of Goli otok prisoners.” The visual message of this display was, very clearly, that communist oppression *looked* like the Holocaust.

Similarly, in Hungary, the House of Terror museum that opened in 2002 in Budapest narrates the story of Hungary’s 20th century experience as a nation-victim of the foreign communist, and to a much lesser extent, foreign fascist regime. The House of Terror goes out of its way to bring home the message that fascism and communism were flip sides of the same coin – there are multiple visual representations of black totalitarianism and red totalitarianism – of the black arrow cross juxtaposed with the red star, of the fascist uniform juxtaposed with the communist uniform. Obviously, equation of the two totalitarian regimes is not new nor particularly surprising. What is more interesting is that the blunt message of this state institution is presented through the appropriation of not just Holocaust imagery, but also Holocaust museum visual displays. Most directly, the House of Terror uses the model of the “Tower of Faces” – portraits of Holocaust victims projected on to the entire length of walls in the United States Holocaust Memorial Museum in Washington, DC to project portraits of Hungarian “victims of communism,” while the “Hall of Tears” in the basement of the Budapest museum is a visual repurposing of the Children’s Memorial at Yad Vashem in Jerusalem (Radonić 2017).

Under the government of Viktor Orbán, Hungary has further embarked on a spectacular urban re-visioning of its 20th century history. The Memorial to the Victims of the German Occupation erected in Budapest in 2014 memorializes Hungary – the country – as the main victim of the German

occupation, by a not-very-subtle depiction of Germany's imperial eagle crushing of Hungary, which is symbolized by Archangel Gabriel. The memorial was unveiled overnight and with no accompanying official opening ceremony, in order to avoid any public debate and expected protests (Pető 2019). Indeed, immediately upon unveiling, Holocaust survivors or their family members placed hundreds of handwritten notes, pictures, and objects outside the Memorial that told the story of 430,000 Jews who were deported from Hungary, mostly to Auschwitz, the quickest rate of deportation in the history of the Holocaust, taking less than two months and done with the active participation of Hungarian civil servants (Braham 2016).

In a manner similar to many new public monuments, museums, and memorials across post-communist Europe, the Budapest Memorial uses architecture as a tool to express myths of nationhood, as part of a state strategy of new visual remembrance of the past. Specifically, it narratively replaces the memory of the Holocaust and the catastrophe of Hungarian Jewish annihilation as the central memory of World War II in Hungary, with the memory of Hungarian victimhood and innocence. It also purposefully replaces the responsibility for the murder of Hungarian Jews from Hungary's Axis-allied government and places it firmly with Germany, presenting fascism and its exterminationist policies as alien, foreign intrusions into the Hungarian body politic. This shift therefore completely removes the history of the Holocaust in Hungary before the German occupation in 1944, the period that left 60,000 Hungarian Jews killed as early as 1942, the extermination carried out not by Germans, but by Hungarian forces under the rule of regent Miklós Horthy (Braham 2016).

This new type of Holocaust remembrance I document in the book, then, is not exactly denial – Viktor Orbán even declared 2014 a Year of Holocaust Commemoration. However problematic, it does not prominently feature voices that deny the Holocaust as a historical fact, nor challenge its most established realities. It is also not quite the same as trivialization – while the emphasis always in on the larger ethnic suffering, it is relatively rare to hear outright belittling of Jewish victimization. A more nuanced way of understanding this type of Holocaust remembrance is as *memory appropriation*, where the memory of the Holocaust is used to memorialize a different kind of suffering, such as suffering under communism, or suffering from ethnic violence perpetrated by other groups.

As my book demonstrates, this process is not simply a byproduct of post-communist transitions; it is in fact an integral part of the political strategy of post-communist states which are basing their contemporary legitimacy on a complete rejection of communism and a renewed connection to the pre-communist, mythically nationally pure, and above all, ethnic, character of states. It is this rejection of communist doctrinaire supra-nationalism and its replacement with old fashioned ethnic nationalism that colors how the Holocaust is remembered. In a global environment of anticommunism, this nationalized Holocaust remembrance has also completely erased the memory of communist antifascist resistance as its constitutive part, and this exclusion provides contemporary anticommunist regimes their legitimacy shields. Holocaust remembrance, then, is no longer about the Holocaust at all, but is about very acute legitimacy needs of post-communist states which are building their identity as fundamentally anticommunist, which then in turn

helps them be perceived as more legitimately European (Mälksoo 2009).

To understand this process, my book explores ways in which states make strategic use of political memory in an effort to resolve their contemporary “ontological insecurities” (Mitzen 2006; Steele 2008; Subotic 2018) – insecurities about their identities, about their status, and about their relationships with other international actors. The book’s principal argument is that post-communist states today are dealing with conflicting sources of insecurity. They are anxious to be perceived as fully European by “core” Western European states, a status that remains fleeting, especially in the aftermath of the openly anti-East European rhetoric of the Eurocrisis and Brexit (Spigelman 2013; Favell 2017). Being fully European, however, means sharing in the cosmopolitan European narratives of the twentieth century, perhaps the strongest being the narrative of the Holocaust.

The European narrative of the Holocaust – which understands it as the foundational block of postwar European identity (Assmann 2014) – has created stress and resentment in post-communist states, which have been asked to accept and contribute to this primarily Western European account as members or candidate states of the European Union. The problem is that the “cosmopolitan Holocaust memory” (Levy and Sznajder 2002) as developed in the West does not narratively fit with the very different set of Holocaust memories in post-communist Europe. This lack of fit is evident primarily in the lack of centrality of the Shoah as the defining memory of the twentieth century experience across the post-communist space. Instead of the memory of the Holocaust, Eastern European states after communism con-

structed their national identities on the memory of Stalinism and Soviet occupation, as well as pre-communist ethnic conflict with other states. The European centrality of the Holocaust, then, replaces the centrality of communist and ethnic victimization as the dominant organizing narrative of post-communist states, and is therefore threatening and destabilizing to these state identities.

My book documents how, influencing European Union’s own memory politics and legislation in the process, post-communist states have attempted to resolve these insecurities by putting forward a new kind of Holocaust remembrance where the memory, symbols, and imagery of the Holocaust become appropriated to represent crimes of communism. The criminal past is not fully denied, but the responsibility for it is misdirected. This accomplishes two things – it absolves the nation from acknowledging responsibility for its criminal past while, at the same time, it makes communism, as a political project, criminal. By delegitimizing communism, post-communist states have also removed anti-fascist resistance from the core memory of the Holocaust, which has allowed for a revival and ideological normalization of fascist ideological movements in the present.

The centrality of the Holocaust as a foundational European narrative, however, is also soundly rejected across much of post-communist Europe because of its perceived elevation of Jewish victimhood above victimhood of other regional majority ethnic groups, a move that is increasingly openly resented (Baer and Sznajder 2017). Further, the European Holocaust memory’s focus on Jewish suffering is also begrudged in much of the region because it brings about discussion about extensive and deep local com-

plicity in the Holocaust and material and political benefits of the complete Jewish absence across Eastern Europe (Himka 2008). Jewish businesses, homes, and property have over decades of looting followed by communist seizures slowly morphed into the general economy, with difficult and sporadic attempts at restitution (Charnysh and Finkel 2017). Contemporary Holocaust remembrance practices – such as those sanctioned by the Polish or Hungarian governments – avoid these difficult discussions by deflecting all responsibility for the genocide of the Jews onto Nazi Germany, absolving the national past from any appearance of impropriety or crime.

My book aims to put these episodes of memory inversion in a contemporary political context by arguing that they are not isolated instances of competing memory, but instead critical elements of national strategies of political legitimacy. They serve to reposition national narratives in opposition both to those of communism but also those historically embraced by Western Europe, and instead reclaim a national identity that rejects multiculturalism and is rebuilt along ethnic majoritarian lines.

Memory politics should be of immediate and critical interest to comparative politics, which has so far remained somewhat disinterested in a systematic engagement with this topic, other than in discussions of nationalism (for a recent attempt to fill this gap, see Bernhard and Kubik 2014). But far from being important just in understanding nationalism, a focus on historical memory is vital in understanding the formation of domestic institutions, educational systems, and forms of civic representation. It is also critical in understanding state international behavior, and especially the interaction between state

action on the international and the domestic stage (Subotić 2016). More immediately, it has become quite obvious that the politics of memory have played a momentous role in the rise of populist and far right movements in Eastern Europe since at least 2010. The sharp tilt to authoritarianism and illiberalism in Hungary and Poland, and also encroaching illiberalism in Bulgaria, Serbia and Croatia, cannot be explained without understanding the strength of the appeal to a particular type of memory politics these regimes engaged in to gather up votes. History, quite clearly, has become a “handmaiden of populism” (Rév 2018: 621).

But here is the methodological problem – which way does the causality go? Did these populist movements emerge first, and then shape the politics of memory to further solidify electoral support? Or did this new/old/inverted memory already exist, or develop on its own, through various official and unofficial cultural institutions, and the new populists just dipped into the already deep pool of revisionism and anti-cosmopolitan resentment? My research suggests, instead, that public memory and populist leadership are best understood as mutually constitutive as they are both products of the social environment which is saturated with a particular notion of the past and a very specific national identity of victimization that both promotes this revisionist history and gives rise to populist movements that capitalize on it. The study of memory politics has the potential to further complicate linear causal accounts and enrich comparative politics with a renewed focus on narratively rich social environments that act as both agents and structures of political change.

It is quite clear that cultural issues of identity and history have been integral to the ascent and



consolidation of populism in post-communist East Europe, as the question of who are the real victims of history has been central to the populist enterprise. The fact that East European victimization under communism is not adequately understood and appreciated in the West is the central grievance of these movements, and it feeds into a new cycle of victimization – this time the perceived oppression by Western liberal ideals, such as “gender ideology,” feminism, LGBTQ rights, or even more dramatically, Middle Eastern migration and refugee flows (Mark 2019). The core of populist resentment is the issue of cultural imposition – and the deepest cultural imposition is the imposition of memory of their own pasts. ●

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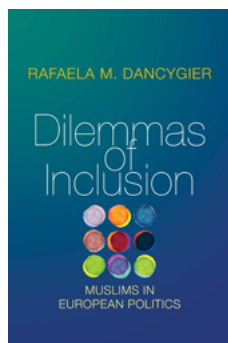
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LUEBBERT BOOK PRIZE

Dancygier, Rafaela. 2017. *Dilemmas of Inclusion: Muslims in European Politics*. Princeton: Princeton University Press.



Book Description (From the Publisher)

As Europe's Muslim communities continue to grow, so does their impact on electoral politics and the potential for inclusion dilemmas. In vote-rich enclaves, Muslim views on religion, tradition, and gender roles can deviate sharply from those of the majority electorate, generating severe trade-offs for parties seeking to broaden their coalitions. *Dilemmas of Inclusion* explains when and why European political parties include Muslim candidates and voters, revealing that the ways in which parties recruit this new electorate can have lasting consequences.

Drawing on original evidence from thousands of electoral contests in Austria, Belgium, Germany, and Great Britain, Rafaela Dancygier sheds new light on when minority recruitment will match up with existing party positions and uphold electoral alignments and when it will undermine party brands and shake up party systems. She demonstrates that when parties are seduced by the quick delivery of ethno-religious bloc votes, they undercut their ideological coherence, fail to establish programmatic linkages with Muslim voters, and miss their opportunity to build cross-ethnic, class-based coalitions. Dancygier highlights how the politics of minority inclusion

can become a testing ground for parties, showing just how far their commitments to equality and diversity will take them when push comes to electoral shove.

Providing a unified theoretical framework for understanding the causes and consequences of minority political incorporation, and especially as these pertain to European Muslim populations, *Dilemmas of Inclusion* advances our knowledge about how ethnic and religious diversity reshapes domestic politics in today's democracies.

Q&A with Rafaela Dancygier

What caused you to embark on this project?

Two phenomena got me interested in this project. First, when doing research for my first book, *Immigration and Conflict in Europe*, I noticed that in many instances, immigrant groups relied on links of kin and clan to mobilize politically, and politicians were often happy to tap into and reinforce these networks. Though this type of behavior is frequently written about in low-income countries where patronage is disbursed along ethnic lines, I did not expect to see these dynamics to play out in European cities that



supposedly feature programmatic parties and depersonalized bureaucracies. I wanted to find out more about when and why political parties mobilized minority electorates on the basis of ethno-religious ties and what consequences this would have for the party system and the nature of minority political inclusion.

Second, I got interested in multicultural dilemmas: How can liberal democracies recognize minority rights and practices when these violate norms of equality and justice? Public discourse was tackling these questions with respect to European Muslim communities, and political theorists had of course worked on multicultural dilemmas as well. I thought that these dilemmas were particularly salient – but not yet addressed – in the electoral context, where I saw parties empowering socially conservative and patriarchal “community leaders” for the sake of vote mobilization. I wanted to investigate why parties that increasingly tout adherence to liberal values and gender equality recruit candidates that undermine these principles.

What is one main thing you want the project to be remembered for ten years from now?

If I had to choose, I would hope that scholars who are interested in the incorporation of new groups into politics would make use of the larger theoretical framework. I distinguish between two inclusion types (aside from exclusion) – symbolic inclusion and vote-based inclusion – and argue that each type is connected to different electoral incentives and has different political consequences. Symbolic inclusion of minority candidates is meant to appeal to sections of the majority electorate who value diversity. It therefore leads to the election of minority candidates who are acceptable to these voters. By contrast,

parties engage in vote-based inclusion when they are after the minority vote. In many cases this occurs when minorities are concentrated in vote-rich enclaves. In the case of European Muslims, I show that symbolic inclusion leads to the election of secular, progressive women, whereas vote-based inclusion causes the election of religious, socially conservative men.

It would be great if my project was remembered for these distinctions, and I’d be especially interested to see what consequences these inclusion types have in the case of other groups with different traits.

What in your data or findings surprised you the most? Why?

Once I had theorized that symbolic inclusion should lead to the disproportionate selection of Muslim women (who can more easily signal progressive, secular bona fides than men), I collected candidate data across countries that varied in the extent to which parties should pursue symbolic vs. vote-based inclusion (based on the permissiveness of their citizenship regimes and hence the potential value of the immigrant-origin Muslim vote). I replicated a similar design across cities within countries, varying the size and social conservatism of the Muslim electorate. I thought this was a bit of a long shot, because I worried that unobserved factors might intervene that would make it difficult to detect these relationships. I was very pleasantly surprised when I did find that the predictions were borne out in the data.

What would you change or do differently if you went back and did this project again?

If I had to do this project again, I would conduct more field work. Time and logistical constraints



made it difficult to spend longer periods of time in the field. Thanks to my prior field research, I had a good grounding in these topics and settings, so I am pretty confident that I got “things right.” But more field work most likely would have generated additional insights.

What is the biggest still unanswered question that emerges from your research?

The biggest unanswered question relates to the long-term societal consequences of different types of minority political inclusion. In the concluding chapter I speculate that different inclusion types could affect how majority populations view minorities and how minorities view their position in society. The book focuses on the political repercussions, but it doesn’t ad-

dress social integration, and I’d be very interested to see how the latter unfolds.

If another scholar does the same project ten years from now, do you think their findings would be different from yours? And if yes, in which ways?

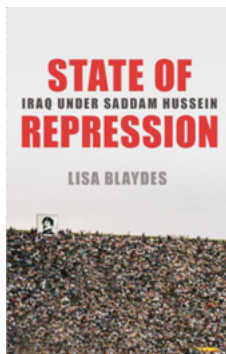
The dynamics I analyze in the book have been at work for the last several decades, so I do not think that the findings would be radically different. If there is a change, it may originate from within the Left. It is possible that progressive electorates become increasingly intolerant of the hypocrisy of leftist parties that recruit socially conservative candidates. If that is the case, we may only observe this type of recruitment among center-right parties, which would reinforce partisan polarization in interesting ways. ●





LUEBBERT BOOK PRIZE HONORABLE MENTION

Blaydes, Lisa. 2018. *State of Repression: Iraq under Saddam Hussein*.
Princeton: Princeton University Press 2018



Book Description (From the Publisher)

How did Iraq become one of the most repressive dictatorships of the late twentieth century? The conventional wisdom about Iraq's modern political history is that the country was doomed by its diverse social fabric. But in *State of Repression*, Lisa Blaydes challenges this belief by showing that the country's breakdown was far from inevitable. At the same time, she offers a new way of understanding the behavior of other authoritarian regimes and their populations.

Drawing on archival material captured from the headquarters of Saddam Hussein's ruling Ba'th Party in the wake of the 2003 US invasion, Blaydes illuminates the complexities of political life in Iraq, including why certain Iraqis chose to collaborate with the regime while others worked to undermine it. She demonstrates that, despite the Ba'thist regime's pretensions to political hegemony, its frequent reliance on collective punishment of various groups reinforced and cemented identity divisions. At the same time, a series of costly external shocks to the economy—resulting from fluctuations in oil prices and Iraq's war with Iran—weakened the capacity of the regime to monitor, co-opt, coerce, and control factions of Iraqi society.

In addition to calling into question the common story of modern Iraqi politics, *State of Repression* offers a new explanation of why and how dictators repress their people in ways that can inadvertently strengthen regime opponents.

Q&A with Lisa Blaydes

What caused you to embark on this project?

For those of us interested in understanding the inner workings of authoritarian regimes, getting access to government documents and data can be very difficult. The archival materials that I used to write *State of Repression* were drawn primarily from the Iraqi Memory Foundation collection at Stanford's Hoover Institution and include records of the Ba'th Party recovered by US-led forces after the invasion of Iraq in 2003. The collection became publicly available at Hoover in the summer of 2010, though a small number of scholars had been using the documents before that date. After spending some time reviewing the material, I knew that the documents provided an amazing opportunity to answer questions about governance, identity and repression in authoritarian Iraq, a country case

that has remained largely opaque to scholars of comparative politics. The archival collection at Hoover includes over 10 million digitized documents and is the only collection of its type for a Middle Eastern autocracy. To complement the material at Hoover, I also used materials from the now-closed Conflict Records Research Center at the National Defense University—particularly, transcripts of Saddam Hussein’s cabinet-level meetings—and the Iraqi Secret Police Files at the University of Colorado Boulder. Together, these documents allowed me to understand more about the types of high-stakes politics taking place in one of the most repressive authoritarian regimes of the late 20th century.

What do you want the project to be remembered for ten years from now?

I see the book as having two main contributions. The first relates to advancement of our empirical knowledge about the Ba’thist regime. Because the archival collection is so vast, scholars will be working through the material for years to come. Indeed, a number of outstanding monographs have already been published using the Hoover collection. But because most of the other scholars using the archives are historians by training, naturally they bring a historian’s sensibility to their empirical work. As a political scientist, my background led me to bring a different analytic lens to the materials and to tackle the archive in a distinctive way. I have tried to infuse the book with a broad set of empirical insights related to a variety of subjects, including the regional distribution of Iran-Iraq War casualties; information about the human cost associated with the international sanctions regime; statistics on the tribal composition of paramilitary groups; and estimates for the number of Iraqis who sought to dodge compulsory military service.

The second thing I hope will be remembered relates to the book’s core theoretical insight. In addition to providing new empirical findings about the Ba’thist regime, I also try to make a more general argument about the causes and consequences of repression in authoritarian regimes. I argue that authoritarian elites seeking to infer which citizens oppose the regime face an informational problem, and this is especially pronounced vis-a-vis groups that are culturally or linguistically distant from a dominant group. At critical moments of protest or political transgression, this monitoring problem leads to repressive actions targeted at entire classes of citizens, rather than targeted repression aimed at specific individual dissidents. This group-based repression reinforces group-based identities and can, over the longer term, pose substantial challenges to authoritarian stability.

What in your data or findings surprised you the most? Why?

Authoritarian regimes work tirelessly to render their societies legible, often with the goal of exercising more effective political control. The specific strategies for accomplishing this goal can be costly and, sometimes, surprising.

For example, one of the largest and most comprehensive collections within the Hoover documents are the School Registers, an annual accounting of the Iraqi high school population. For each student the Registers list the student’s name and other personal information as well as his orientation toward the Ba’th Party. In some years, the Registers also include additional material about whether the student (or his father) enjoyed the “Friend of Saddam” privileged bureaucratic status as well as whether the student had volunteered to join the “Fedayeen Saddam” paramilitary group. The School Registers are



the closest thing I have seen to a political census within an Arab authoritarian regime. Information from the Registers allowed me to characterize and map geographically the extent of stated opposition to the Ba'th Party at different points in time.

Another surprising discovery within the collection related to files that held more than 2,000 rumors the Ba'thists had gathered. Rumors were critical sources of information for ordinary Iraqis living under the Ba'thist regime. For example, some rumors provided information about anticipated price shocks during the sanctions period. Individuals used that information to stockpile sugar or other basic commodities. Other rumors provided information about how to avoid being targeted in government raids. In still other cases, rumors sought to mobilize people for participation in popular protest or other acts of political subversion. For example, sometimes protests or attacks on Ba'th Party offices were rumored to occur on the occasion of upcoming religious holidays, after a Friday prayer service or on the birthday of Saddam Hussein. One rumor even suggested that an anticipated solar eclipse would serve as the signal for coordinated riots to take place across a number of cities.

The rumors were full of surprising details, including stories about assassination attempts against Saddam Hussein and his sons as well as worries about what an American invasion of

Iraq might mean. One of the most persistently circulated rumors in the run-up to the U.S. invasion of Iraq was that the U.S. would deploy an aerial chemical spray that would put Iraqis to sleep. These "sleep bombs" would then provide the U.S. with an opportunity to attack Baghdad. Fear and uncertainty are persistent themes in the collection as well as the tremendous importance of information acquisition in an authoritarian context.

What would you change or do differently if you went back and did this project again?

If I were starting this project today, I would think more seriously about how to use optical character recognition to create machine-encoded text from the existing digital files. This would allow for the use of text analysis strategies to classify the information more efficiently. Regime memoranda could be keyword searched more easily and analyzed using topic modeling. In addition, one of the biggest blind spots in scholarly analysis of the collection relates to the Ba'th Party membership files. Given the huge number of files included in that collection (more than a million digital pages), automated and computer-assisted approaches provide the best possibility for extracting, organizing and analyzing these large quantities of text. ●





LUEBBERT ARTICLE PRIZE

Kruks-Wisner, Gabrielle. 2018. "The Pursuit of Social Welfare: Citizen Claim-Making in Rural India." *World Politics* 70(1), pp. 122-163.

Abstract

Who makes claims on the state for social welfare, and how and why do they do so? This article examines these dynamics in the rural Indian context, observing that citizens living in the same local communities differ dramatically in their approaches to the state. The author develops a theory to explain these varied patterns of action and inaction, arguing that citizen claim-making is best understood as a product of exposure to people and places beyond the immediate community and locality. This social and spatial exposure builds citizens' encounters with, knowledge of, and linkages to the state. This in turn develops their aspirations toward the state and their capabilities for state-targeted action. The author tests the theory in rural Rajasthan, drawing on a combination of original survey data and qualitative interviews. She finds that those who traverse boundaries of caste, neighborhood, and village are more likely to make claims on the state, and that they do so through broader repertoires of action than those who are more constrained by the same boundaries. The article concludes by considering the extensions and limitations of the theory and the role of the state itself in establishing the terrain for citizen action.

Q&A with Gabrielle Kruks-Wisner

What caused you to embark on this project?

The project is focused on citizen claim-making in Rajasthan, in northern India. I first became interested in exploring the pathways through which citizens pursue social welfare in a different setting: in south India in the aftermath of the 2004 Indian Ocean tsunami. While working in affected fishing villages, I observed that different people – men, women, and members of different caste communities – sought assistance through different channels: some turned to elected representatives, some to traditional caste leaders, some to NGOs. This variation in whether and in how directly citizens engaged the state was intriguing to me. A post-disaster setting is in many ways exceptional, and so I wanted to study similar dynamics under more quotidian conditions. That's what brought me to Rajasthan. I wanted to know: how do citizens in India's northern poverty belt navigate access to the state when seeking essential services and entitlements?



What is one main thing you want the project to be remembered for ten years from now?

I hope the project will be remembered for two things. First, for developing a bottom-up view of the state as it seen and experienced by citizens themselves. By documenting citizens' own accounts of claim-making, I hope to have called attention beyond the well-studied realm of elections to the day-to-day—but equally important—practices through which citizens navigate access to the state. Second, for highlighting the power of social and spatial exposure in shaping what citizens come to expect from the state. This is important, because it reminds us that citizens' beliefs and behaviors are constantly changing in response to what they see the state doing all around them. This highlights the possibility of a positive feedback loop—where more a responsive state helps to produce more active citizens—but also the specter of a negative one, where poor public performance reinforces low citizen expectations.

What in your data or findings surprised you the most? Why?

I was surprised to find seemingly similar people—living in the same villages, under similar structural conditions—express such different relationships to the state: they had different opinions about what the state *should* deliver, *if* it would deliver, and whether it was *worthwhile* to engage in claim-making. Most surprisingly, this variation did not conform to patterns that I initially expected (and that the literature on participation broadly predicted): it was not simply a matter of rich versus poor, or of high or low social standing. Citizens' approaches to the state varied within the same communities as well as within socioeconomic and caste groupings.

Poverty, in other words, was not uniformly constraining in its effects on citizenship practice.

What would you change or do differently if you went back and did this project again?

I would love to re-engage these questions with a broader comparative framing, both sub-nationally in India and cross-nationally. One of the strengths of the project is its depth: the ability to dig deep within one state to uncover micro-level variation—across and within villages—that otherwise might have gone overlooked. But this leaves important open questions about how the theory I propose works under different sets of macro conditions. I would also love to add a longitudinal element: my data are static, and so leave me unable to tell the story of how patterns will change over time.

What is the biggest still unanswered question that emerges from your research?

I think the biggest unanswered questions are normative ones: is the story in rural Rajasthan a “success” story? Are the high levels of citizen claim-making that I document a sign of a robust local democracy? Or are they a sign of an uneven, and often failing, state apparatus where claim-making is driven by necessity? This points to a second unanswered question, which is: what happens over time? How long will citizens continue to engage the state in these ways, and when might we see the scales tip towards higher levels of citizen exit rather than voice?

If another scholar does the same project ten years from now, do you think their findings would be different from yours? And if yes, in which ways?



Without a doubt! The theory I develop is that citizen claim-making is both socially produced and state-induced. This implies that citizenship behaviors will change as social conditions change (for example, if more women enter the workforce, or if caste barriers shift in certain arenas), but also—and perhaps most importantly—as the terrain of the state itself shifts. A lot depends on the state’s commitment and capacity to deliver in terms of social policy, but also on patterns of social and political exclusion. The adminis-

tration, the political climate, and the felt local presence of the state in India has already, by 2019, shifted dramatically since the time of my fieldwork in the late 2000s. In an increasingly polarized climate marked by rising majoritarianism, I might expect to see more bifurcated patterns of claim-making, rising among certain groups but falling for others. Broader patterns of claim-making might, over time, re-emerge with more inclusive discourses of citizenship. ●





SAGE PAPER PRIZE

Homola, Jonathan, Miguel M. Pereira, William Simoneau, and Margit Tavits.

“Legacies of the Third Reich: Concentration Camps and Outgroup Intolerance.”

Abstract

We explore the long-term political consequences of the Third Reich and show that current political intolerance, xenophobia, and voting for radical right-wing parties are associated with proximity to former Nazi concentration camps in Germany. This relationship is not explained by contemporary attitudes, the location of the camps, geographic sorting, the economic impact of the camps, or their current use. We argue that cognitive dissonance led those more directly exposed to Nazi institutions to conform with the belief system of the regime. These attitudes were then transmitted across generations. The evidence provided here contributes both to our understanding of the legacies of historical institutions, and the sources of political intolerance.

Q&A with Jonathan Homola, Miguel Pereira, William Simoneau, and Margit Tavits

What caused you to embark on this project?

The origins of the project were rather prosaic: we were looking for a project that would allow us to collaborate, i.e., one that would fit with our diverse research interests and skills. Between the four of

us, we were interested in attitudes toward immigrants and other outgroups, the success of the radical right, the history and treatment of Jews in Europe, including the Holocaust, and we were all fascinated by the line of research that tries to rigorously establish the long-term effects of coercive institutions. After multiple sessions of brainstorming and refinement of ideas, we settled on exploring the long-term effects of Nazi-era concentration camps on outgroup intolerance.

What is one main thing you want the project to be remembered for ten years from now?

The idea that even short-term institutions that promote hatred can sustain that hatred for decades, but that memorialization of victims and education about the atrocities may be able to overcome those effects.

What in your data or findings surprised you the most? Why?

One surprising finding, i.e., something that we did not knowingly look for but just stumbled upon, is the re-educational effect of memoirs. The effects of concentration camps are weaker around those camps that offer most vivid experiences about what conditions were like



in those camps. We did not expect that. Instead, we started looking into memorialization because a reviewer suggested that our effects may be due to contemporary factors: locals who see tourists visiting memorials may feel fingers pointed at them and, as a defensive reaction, they may start justifying the outgroup hatred promoted in the camps. Yet, we found the exact opposite: the effects disappear around camps with experiential memorials, those that have preserved some original physical structures, while they are strongest around camps with simple monuments and no original structures. These results were surprising to us at first. However, when we thought about them from the perspective of the re-education that the experiential memorials provide, they made sense.

What would you change or do differently if you went back and did this project again?

We aren't quite finished with the project yet, but we probably will not be changing any major aspects of it. There simply are inherent data limitations to doing anything more or different than what we already do. For example, we might wish to have a more powerful design that uses panel survey data from before and after the war, but that would be wishing for the impossible because those data do not exist. We tried to work with what is possible and get the most out of the data that could be gathered.

What is the biggest still unanswered question that emerges from your research?

One of the questions that needs further investigation is how to break the effects of historical legacies. Our finding that re-education through memorialization may have the power to do so is exploratory. Future research should more firmly establish this effect and explore other ways in which we can get out from under the shadow of oppressive historical institutions.

If another scholar does the same project ten years from now, do you think their findings would be different from yours? And if yes, in which ways?

We think that their findings would be similar. We use survey and electoral data from different time-points that are almost ten years apart: 2008 and 2016/2017. In terms of the level of anti-immigration sentiment, outgroup hatred, and radical politics, the world looked very different at these two time-points. In 2008, Germany did not have a significant radical right presence and immigration was less of an issue politically. By 2017, Germany (and the rest of Europe) had experienced an unprecedented refugee crisis, anti-immigrant sentiment was growing and immigration was a highly salient political issue, and radical right was gaining popularity. Yet, our effects are the same across these fairly different contexts. This increases our confidence that our findings are also likely to still be there with data from 2029. With that being said, if we think even further into the future, it is possible that some of the effects of these institutions get progressively mitigated. ●





LIJPHART/PRZEWORSKI/VERBA DATASET AWARD

Bryan D. Jones, *The Comparative Agendas Project (CAP)*

Dataset Description

The Comparative Agendas Project (<https://www.comparativeagendas.net/>) assembles and codes information on the policy processes of governments from around the world. CAP enables scholars, students, policy-makers and the media to investigate trends in policy-making across time and between countries. It classifies policy activities into a single, universal and consistent coding scheme. CAP monitors policy processes by tracking the actions that governments take in response to the challenges they face. These activities can take many different forms, including debating a problem, delivering speeches, (e.g. the Queen's speech in the United Kingdom), holding hearings, introducing or enacting laws (e.g. Bills and Public Laws in the United States) or issuing judicial rulings (e.g. rulings from the European Court of Justice).

Q&A with Bryan D. Jones

What caused you to embark on this project?

Frank Baumgartner and I had finished *Agendas and Instability in American Politics*, in which we did a series of case studies based on the existing literature assessing agenda-setting. We found

patterns of punctuated equilibria in each subsystem we examined. But we discovered that we could not do systematic comparisons, even qualitative ones, because contexts were so different. So, we decided to try to develop a set of codes based on policy content that were reliable across time and could be compared across the content categories—that is, the policy issues. We modeled the system roughly on budget codes and later the National Income and Product Accounts, in which the reliability of the system is the most important characteristic. Validity must take a second seat, or time series analyses can't be done. If new content categories are needed, and sometimes they are, we require back-coding of all items in the category. We started with Congressional hearings, because that seemed to be the most important venue for agenda-setting in the sense of moving from the systemic agenda to the formal, governmental agenda. It is still our "gold standard" series.

Other countries got added because of demand from European scholars willing to do the hard work of developing systems that could be compared with the US and were internally reliable. Christoffer Green-Pederson at Aarhus University started the Danish system and it diffused outward.



What is one main thing you want the project to be remembered for ten years from now?

I can offer a trinity of insights gleaned from this:

As I tell my students, good measurement often is much more important than fancy modeling. “Good theory, good data, good results”.

Second, no project of this magnitude is possible without cooperative activity, and the willingness to provide collective goods without being directly compensated for it.

Finally, the role of induction is so critical in science. When Sherlock Holmes says he “deduces” he actually means “induces”. Detecting patterns in data is critical to building theory. Yet it is often dismissed in the rush to “build models”, often based on faulty assumptions.

What in your data or findings surprised you the most? Why?

There is really so much stuff. I’ll list two. One is how important the seemingly meaningless question periods in European parliamentary periods lead issue attention and later policy action. They seem to act as distant early warning systems of emerging issues.

The second is what a powerful tool the Policy Agendas Project is in tracing quantitatively changes over time, in some cases causing me to re-think accepted wisdom in the discipline. Most of these data-generated insights are laid out in *The Great Broadening*, published by Chicago Press and co-authored by me, Sean Theriault, and Michelle Whyman. We document the critical role of social movements in policy change, show that the interest group system grew because of policy change, and was less important in generating policy change, and that

party polarization was in large part caused by policy change and did not cause it. Many more historical insights from this.

What would you change or do differently if you went back and did this project again?

Probably we’d still make the same mistakes. My biggest regret, however, is the failure of our research teams and the rest of those of us who count public policy as our basic interest is our inability to get across to political scientists the fundamental role past policy actions play in current politics. It is the major conclusion of so much work in comparative and American politics, including *The Great Broadening*, yet so much political science fails to incorporate this into current analyses.

What is the biggest still unanswered question that emerges from your research?

All I can think of is Donald Rumsfeld’s distinction between “known unknowns” and “unknown unknowns”. We don’t know what might emerge because we have not yet formulated that question. But if I had a hunch, it would be to try to put illiberal democracies and even authoritarian systems within the same policy frame as we used when starting the Policy Agendas Project: punctuated equilibrium. The theory developed in the *Politics of Attention* indicates that punctuations should be more severe in systems with high friction, and our comparative analyses of budgets from our participating teams show that to be the case in democratic systems. Our fine Hungarian team is already working on measuring policy change quantitatively as Hungary moved from Communist authoritarianism to democracy to illiberal democracy. Do illiberal

democracies and autocracies experience severe punctuations because of the “informational deficit” caused by blocking full discussions of issues, hence overly constraining the policy agenda? We have the conceptual tools to ask these questions, and several research teams are examining these issues in authoritarian systems. Especially interesting are systems that have changed from authoritarian to democratic, or vice versa.

If another scholar does the same project ten years from now, do you think their findings would be different from yours? And if yes, in which ways?

I’m not sure about the findings, but surely data collection and coding would be different. Our media data is so limited because we had to take samples due to a limited budget. That would be pretty easy to overcome. ●





LIJPHART/PRZEWORSKI/VERBA DATASET AWARD HONORABLE MENTION

Nils B. Weidmann and Espen Geelmuyden Rød. *The Mass Mobilization in Autocracies Database (MMAD)*

Dataset Description

The Mass Mobilization in Autocracies Database (MMAD) tracks incidents of anti- and pro-regime protest in autocratic countries. It contains detailed information on the date and location of these incidents, the protest actors and issues, as well as the level of violence involved. The currently available versions 1 and 2 cover the years 2003 - 2015, version 3 will extend coverage until the end of 2018. Data and documentation are available at <https://mmadatabase.org>

Q&A with Nils B. Weidmann and Espen Geelmuyden Rød

What caused you to embark on this project?

In 2012, Nils received a five-year research grant from the Alexander von Humboldt Foundation that brought him to Konstanz. Espen was part of this project from the beginning, and together we developed the MMAD. The reason for collecting the protest data in the MMAD came from our shared interest in studying the influence of digital technology on political mobilization. We ended up writing a book about the topic (*The Internet and Political Protest in Autocracies*, Oxford University Press, 2019)—a book that in-

troduces the MMAD and relies heavily on it for the empirical analyses.

What is one main thing you want the project to be remembered for ten years from now?

As many other data collection projects in political science, we rely on media reports about the phenomenon we're interested in (in our case, protest). A challenge that comes up in this process is that you often get multiple, sometimes contradicting reports about the same event. We believe that we found a nice way to incorporate the different sources of information into the coding process, simply by coding the variable in our data separately by source, and later aggregating them to the level of events. We certainly hope that the project will be used for a wealth of interesting substantive research, but also believe that our coding approach may be useful for many other data collections in our discipline – not just those about political protest.

What in your data or findings surprised you the most? Why?

In our book, we find that Internet technology overall suppresses protest in autocracies, but that it catalyzes ongoing protest. The opposite



direction of the effects surprised us. While our initial expectation was that the political effects of Internet technology can play out differently depending on timing and political context, we did not anticipate such a stark contrast. Another, perhaps even more surprising, finding is that the protest-suppressing effect of digital technology is larger in autocracies that have liberalized their political institutions than in closed regimes.

What would you change or do differently if you went back and did this project again?

In a project comparing different world regions and countries, availability of reports (from the media and other sources) is a major challenge. Some regions receive much less coverage than others. We did some initial analyses to see how severe this problem is, but cannot be sure that our final selection of news outlets could not be improved. In general, with more progress in the automatic coding of event data, the problem of uneven media coverage is likely to be reduced. However, we first need to work to improve the quality and granularity of automatically-coded data, which is why the final coding step in the MMAD is still done by human coders.

What is the biggest still unanswered question that emerges from your research?

Above, we described some of the core results presented in our book, which show that ICT can affect political protest in different ways. Still, the theoretical mechanisms accounting for

these findings remain unexplored empirically. We discuss several candidate mechanisms in the book, for example the use of digital technology for state surveillance, censorship, and propaganda or the use of online communication by opposition activists. However, with our research design and the data we have, we were not yet able to empirically probe the degree to which each of these contribute to the patterns we uncover in the book.

If another scholar does the same project ten years from now, do you think their findings would be different from yours? And if yes, in which ways?

We think that scholars attempting to collect data on protest in autocracies ten years from now will run into similar challenges related to potential biases in reporting. However, one can imagine that this bias changes over time, which would result in a different set of protest events than the one currently in the MMAD. For example, reporting of political events becomes ever more reliant on digital communication channels and social media. This could mean that the gap in reporting of protest between places with limited and advanced availability and use of technology grows over time. Another possibility is that technological development allows for the collection of data with less reporting bias. Whatever way it goes, we expect that bias in reporting may change, which could affect empirical patterns on the relationship between ICT and political protest and mobilization we find in the data. ●





THEDA SKOCPOL PRIZE FOR EMERGING SCHOLARS

by David Samuels (University of Minnesota) and
Rafaela Dancygier (Princeton University)

At the 2019 APSA meeting the Comparative Politics Section created a new Emerging Scholar Award and named it in honor of Theda Skocpol, the Victor S. Thomas Professor of Government and Sociology at Harvard University. We are now asking colleagues to help make this prize viable. To fund it, we need \$15,000. \$1,000 has already been pledged, and the Comparative Politics Section will match donations up to \$7,000. That means we need to raise another \$7,000.

We've set up an easy way to donate: go to the Comparative Politics section's webpage at <https://www.apsanet.org/section20> and scroll to the bottom - there's a box to "Donate Now!" to the Theda Skocpol Emerging Scholar Award. Once you log into your APSA account, you can pay by credit card. Any amount is welcome, thanks for your support. ●





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ABOUT

The Organized Section in Comparative Politics is the largest organized section in the American Political Science Association (APSA). The Section organizes panels for the APSA's annual meetings; awards annual prizes for best paper, best article, best book, and best data set; and oversees and helps finance the publication of this newsletter, APSA-CP.



The section website is:

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