

# Analytical Narratives and Institutional Analysis

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During the time that I was in the Economics PhD program at George Mason University (2006-2010), it was one of the most intellectually diverse economics departments in the United States. Peter Boettke has argued that it's the best "weird place" to study economics in the world.<sup>2</sup> Students studied the standard sequence found in mainstream economics programs – micro, macro, math econ, and econometrics – but there was also an incredible range of other substantive and methodological approaches to engage with as well. GMU has long been home to scholars working in the Virginia Political Economy tradition, most notably Gordon Tullock and Nobel-laureate James Buchanan. At the same time, Nobel-laureate Vernon Smith and colleagues focused on experimental economics in the lab. There were several people applying economics to the study of religion, economic historians working on a variety of time periods and questions, and several faculty members trying to understand emergent systems and complexity with the use of agent-based models. Finally, there were the approaches most closely identified with Boettke and colleagues: market-process economics and comparative institutional analysis, both of which often relied on qualitative evidentiary sources.

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<sup>2</sup> [https://www.coordinationproblem.org/2007/04/narratives\\_of\\_a.html](https://www.coordinationproblem.org/2007/04/narratives_of_a.html)

For me, Boettke's advocacy of comparative institutional analysis and the use of analytical narratives was most formative. Like many important ideas, analytical narratives are underrated in both economics and political science. In this essay, I explain what analytical narratives are, why they can offer advantages in our positive analysis of institutions and institutional change, and how the questions we study can and should shape the methods that we use. In advocating the pursuit of particular questions in political economy, Boettke shaped what many of us think are useful empirical tools, what constitutes rigorous analysis of evidence, and which theories provide a solid analytical foundation.

Boettke encouraged us to ask "big" questions in political economy, ones that have long histories and wide applicability in trying to understand fundamental challenges facing society. Some were questions going back to classical economists like Adam Smith: why are some countries rich, but others poor? Some were of fundamental relevance to political institutions, like how can we create institutions that empower political actors but also constrain them from abusing this power? Some research interests were influenced by Boettke's own work and the perennial debate between different economic systems, such as how did the Soviet socialism function and why did reforms fail? A major research initiative at the time was motivated by immediate concerns raised with the destruction wrought by Hurricane Katrina in New Orleans and surrounding areas: how can communities respond successfully to large-scale natural disaster? A common theme across these questions is the importance of institutions. The centrality of institutions is at the core of – if not the defining feature of – the study of political economy.

However, the desire to understand institutions and institutional change raises the question of *how* one can research them rigorously. We often lack clean natural experiments for many important institutional questions. Instead, much of the analysis must be historical and comparative

(Boettke et al 2013).<sup>3</sup> Nevertheless, because much of our knowledge of historical institutions is qualitative in nature, we cannot always rely on quantitative evidence and statistical estimation to test causal claims (Skarbek 2020a). To pursue this area of research, we must be open to a wider range of methodological approaches, including engagement with qualitative evidence and case studies. But, which cases should we study and why?

One piece of advice that Boettke is fond of suggesting is that we should “Strive to find puzzles where it appears that history defies what logic dictates and then solve the puzzle by demonstrating with the tools of rational choice theory and institutional analysis [that] the defiance was only an illusion.”<sup>4</sup> The motivation is thus often based on explaining a puzzling or unique case, rather than starting from abstract theories or developing new iterations on familiar models. Instead, these research questions often arise from a deep knowledge of a particular case that reveals some interesting outcome or institution (Bates et al 1998; Levi 2004). One consequence of this method of inquiry is that many of us gravitated to studying institutions that were unusual, bizarre, or fascinating in some way and then trying to see if there was actually a rational explanation for its existence. For example, Emily Schaeffer (2008) wanted to understand how the *Hawala* system – an informal, money transfer system with global scope and deep historical roots – was able to flourish despite the inability to rely on state-based contracts or legal institutions. The conventional wisdom would seem to suggest that high-value financial transactions between loose-knit communities of people – many of whom were actually strangers – would require state-enforcement. Nevertheless, this was not true. Schaeffer discovers and documents how reputation mechanisms make the Hawala network highly successful. In doing so, this case study provides

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<sup>3</sup> Kimbrough, Smith, and Wilson (2008) have shown that lab experiments provide fertile ground for testing institutional arguments about property rights.

<sup>4</sup> <https://www.coordinationproblem.org/2007/09/hard-work-pays-.html>

evidence about how, how well, and how robustly extralegal institutions can function. This, in turn, informs debates over the comparative institutional effectiveness of state-based and extralegal institutions.

However, Boettke definitely did not advocate the use of cases studies in general. He had strong views about the analytical framework that should be used within case studies. In particular, he emphasized the importance of using rational choice theory and institutional analysis, in what is often called an “analytical narrative.” Analytical narratives are a methodological approach that combines a parsimonious model with rich, qualitative evidence. The model can be presented either formally or in words, but what is necessary is that it clearly communicates the nature of the interaction and generates comparative static predictions (Levi 2004, 202). By definition, analytical narratives are based on a rational choice approach, and most people use game theory to describe the key actors, their preferences, and the payouts from their interactions (Levi 2004, 204, 208). This work thus draws heavily on extensive form games and subgame Nash equilibrium concepts.<sup>5</sup> The goal of the model – like all models – is to simplify, but also to capture the core features of the interactions or institution that matter (Rodrik 2015).

In order to know if a model is successful in this task, one should examine “narrative” evidence. This can take the form of qualitative evidence based on written records (such as constitutions, laws, statutes, company manuals, archival materials, private correspondences, and diaries), interviews, surveys, participant observation, and ethnography. Data and statistical analysis can also inform one’s narrative, but it is not a necessary component. Moreover, in many cases quantitative evidence is actually inferior to qualitative and historical materials for assessing

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<sup>5</sup> Carpenter (2000, 655) questions why this specific game theoretic approach is necessary. Levi (2004, 205) seems to suggest that other concepts and approaches can also be helpful.

models and explaining the world (Leeson 2020). The narrative part of analytical narratives pays close attention to “detailed and textured account of context and process, with concern for both sequence and temporality” (Levi 2004, 208). In doing so, this evidence provides “the necessary information for causal assessment” (Levi 2004, 209).

For example, we might use narrative evidence to study the relationship between coups and whether the president of a country is a socialist. One way of answering this question would be to construct a dataset of socialist and non-socialist presidents and see if there are statistically significant differences in the rates of coups. This exercise might provide valuable insights about the average treatment effect across many different cases. This is sometimes described as an “effects-of-causes” approach because we are trying to identify the effect of a cause across many cases (Mahoney and Goertz 2006, 230-232). The emphasis thus tends to be on maximizing cross-case explanatory power.

However, an alternative is to focus on a single case and to examine evidence to identify the “causes-of-effects.” This approach is more interested in maximizing within-case explanatory power by identifying which factors jointly caused an effect or outcome in particular case. Consider the example of the *coup d'état* against President Allende in Chile in 1973. We can test the hypothesis that he was overthrown, at least in part, because he was a socialist by documenting a number of empirical implications that would follow if true. For instance, it would help to provide evidence on the following claims (Coppedge 1999, 471-472):

- Allende claimed to be a socialist
- He proposed socialist policies
- These policies became law
- These laws adversely affected the economic interests of certain powerful actors
- Some of these actors moved into opposition immediately after certain quintessentially socialist policies were announced or enacted

- Allende's rhetoric disturbed other actors
- These actors issued explicit public and private complaints about the socialist government and its policies
- Representatives of some of these actors conspired together to overthrow the government
- Actors who shared the president's socialist orientation did not participate in the conspiracy
- The opponents publicly and privately cheered the defeat of socialism after the overthrow

Narrative evidence would be helpful – and in many cases necessary – to document these empirical implications. If there was substantial qualitative evidence to support each of these claims, then it would be reasonable to conclude that there was a strong relationship in this case between being a socialist president and a coup.

Causal inferences in an analytical narrative, therefore, come not necessarily from statistical identification with “thin” data, but from deep engagement with rich evidence, which often identifies *causal mechanisms*. This rich narrative evidence also informs model construction, allowing us to update our models as we learn more about the inner workings of an institution, and to assessing the accuracy of the comparative static predictions. However, unlike approaches that emphasize deductive model construction, analytical narratives tend to be more inductive. Ideally, this process of model creation should be based on deep knowledge of a history, institution, or topic. As in case studies more generally, intensive knowledge of a case helps one to build a more insightful model and to generate greater explanatory power within a case.

The term “analytical narrative” goes back at least to Milton Friedman and Anna Schwartz's use in their classic 1963 book *A Monetary History of the United States, 1867-1960*. In the preface, they explain that a colleague suggested that they include an historical chapter as a useful complement to their statistical analysis. As they write, “[t]he chapter, which we began to write only after we had completed a first draft of the remaining chapters, took on a life of its own. The

one chapter became two, then a separate part, and has now become a separate book” (Friedman and Schwartz 2008, xxi). What began as a “foray into analytical narratives” to supplement statistical work became important in its own right and then began to “significantly affect” their statistical analysis. This is a nice example of how the deep knowledge obtained from narrative evidence can inform our understanding and use of parsimonious models and numerical predictions. Given its status as one of the most celebrated books in its field, it also shows that analytical narratives can be rigorous and influential within the discipline of economics.

Contemporary use of this method is tied more closely to *Analytical Narratives* (1998), a book written by political scientists and economists Robert H. Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry R. Weingast. Many of these authors’ past work had been in the style of analytical narratives. As such, it makes sense that this volume constituted a focal methodological statement of the approach. It was intended, writes Daniel Carpenter, as “the most thorough and methodologically self-conscious wedding of formal rational choice theory to historical narrative that has yet been attempted” (Carpenter 2000, 654). The introduction of the book discusses the use and value of analytical narratives, followed by chapters written by each author demonstrating their application in a variety of historical and contemporary settings. The book also served as a useful intervention in a debate raised by critics of rational choice theory, in particular in Donald Green and Ian Shapiro’s 1994 book *Pathologies of Rational Choice*, where they argue against the idea that we can ever develop a single, universal model of politics (Green and Shapiro 1994). *Analytical Narratives* responds, in part, that this was never the goal. Models *should* be context specific, though perhaps based on general principles, such as methodological individualism and some minimal conception of rationality.

In one of Boettke’s discussions of *Analytical Narratives*, he provides a useful 2 x 2 matrix to locate analytical narratives within different social scientific approaches (see Figure 1) (Boettke 2000). He distinguishes between thin and thick theories, where the former are parsimonious with clear comparative static predictions and the latter that are more multifaceted, multidimensional, and not as straightforward to measure and test (Coppedge 1999). He likewise distinguishes between “clean” empirics – the everyday data of a working academic economist – and the “dirty” empirics of narrative evidence. Combining these two distinctions generates four possible approaches. Thin/clean work is the standard economic approach of formal models and statistical estimation. Critics of law & economics have described this approach as a “desert” because of its lack of richness (Ellickson 1994, 147). Thick/dirty research is the traditional approach found in classic works of sociology and anthropology, such as Bronislaw Malinowski’s famous study of tribal communities (Malinowski 2002). Critics of this type of law & society research have likewise described this approach as a “swamp” because of its overwhelming and uncultivated richness, which can be just as inhospitable as a desert (Ellickson 1994, 147)

	<b>“DIRTY” Empirical Work</b>	<b>“CLEAN” Empirical Work</b>
<b>THIN description</b>	ANALYTICAL NARRATIVE	Standard economics analysis <ul style="list-style-type: none"> <li>● constrained optimization</li> <li>● statistical significance</li> </ul>
<b>THICK description</b>	Traditional anthropological, sociological, area studies political science work <ul style="list-style-type: none"> <li>● social forces and cultural analysis</li> <li>● case study and ethnography</li> </ul>	Statistical sociology and political science <ul style="list-style-type: none"> <li>● “Kitchen sink” statistical analysis which throws everything in the right side of the equation in search of explaining the left side.</li> </ul>

Figure 1: Matrix from Boettke (2000)

Boettke thinks the least of thick/clean research approaches, which he refers to as “kitchen sink” regressions. This approach is problematic for a few reasons. Thick theories often lack clear

comparative static predictions, so it is less clear how to, or what type of evidence, would falsify them. Quantitative data likewise often does a poor job of mapping onto thick concepts and theories. The data is poorly matched for describing or testing the underlying theory. Taken together, finding statistically significant results is probably not terribly informative for testing thick theories. I do not think it is accurate to place much of political science in this quadrant any longer since many people in the field use both thin/clean and thick/dirty approaches. However, existing work in other areas, such as much of contemporary criminology, would fall within this quadrant (Bruinsma 2016). Finally, analytical narratives fill the final quadrant: the combination of thin models with dirty empirics. Thin/dirty approaches draw on the best of theory and narrative evidence to produce a more balanced, hospitable, and productive environment, much like a fertile river valley.

It is easy to see why thick/clean approaches are problematic, but why then do we need more than thin/clean and thick/dirty approaches? Political scientist Margaret Levi argues that, unlike the thick theories found more often in anthropology, sociology, and political science, analytical narratives privilege parsimonious models (Levi 2004, 212). That is, thin economic theories might be more informative than the thicker theories found in anthropology or sociology. Nevertheless, dirty empirics – narratives – might be necessary to explain topics that other disciplines usually study. There might be no relevant clean evidence available, or narrative evidence might be better able to describe and explain some topic or institution. If so, engaging with narrative evidence is necessary for economists to be able to apply thin theories in these areas. In short, the capacity to use narrative evidence creates the opportunity to bring economic theories to bear on a wider range of questions.

This type of economic imperialism is less hostile, and potentially far richer, than simply applying thin/clean empirics to topics studied in other disciplines. It is less hostile because it

accepts that dirty empirics do have evidentiary value in the social sciences. It likewise recognizes that the questions and topics that other fields engage with are worthy of study. In addition, narrative evidence expands the scope of social scientific evidence available for economic inquiry. As an example, consider two “law and economics” style analyses of Japanese sumo wrestling. Mark Duggan and Steven Levitt (2002) apply the standard thin/clean approach to try to tease out whether sumo wrestlers intentionally lose in certain rounds of tournaments. The authors start by “acknowledging that sumo wrestling is not itself a subject of direct interest to economists,” but believe there is nevertheless insights to be gained from studying it. Based on their statistical analysis, they find that wrestlers who are just below the threshold to advance in an important round are far more likely to win than they otherwise would be. The authors provide quantitative evidence that this is more likely caused by “rigging” than by changes in effort. This is an interesting finding and tells us something valuable about strategic interactions and corruption.

But, why stop there? There are many other potential economic lessons to derive from sumo wrestling. However, one might need to grapple with dirty empirics to address them. For example, legal scholar Mark D. West (1997) brings an economics perspective to the study of the legal rules and social norms in Japan’s “secret world of sumo,” but he engages extensively with narrative evidence. He studies the organizational structure of the Japan Sumo Association, focusing on the ownership and transfer rights of 105 shares of so-called “elder stock” that determine how the organization is governed. Because this organization operates outside of Western culture and corporate law, and it is a sport with deep cultural and historical roots, some scholars assume that it operates in vastly different ways. Nevertheless, West convincingly shows that owners of elder stock arrange governance institutions to maximize the aggregate wealth of the group. One way that shareholders accomplish that is by selective use of either state-based legal rules or their own

extralegal norms and practices based on their comparative efficiency. Because of the qualitative evidence needed to describe this organization, this type of analysis would be excluded by thin/clean approaches alone.<sup>6</sup>

In addition to reducing perceived constraints on the scope of social scientific analysis, thin/dirty approaches can also sometimes simply do a better job of describing and explaining institutions and institutional change. Dirty empirics are not always second-best. Consider, for example, how we might measure institutions. Even if we define institutions simply as the “rules of the game,” that opens a wide range of possible important attributes. Institutions can vary in many ways, including ranging from legal to extralegal, formal to informal, centralized to decentralized, flexible to rigid, permanent to temporary, and fragile to robust to antifragile. If the best descriptions of an institution must often be multifaceted and multidimensional, then clean empirics will do a worse job describing institutions than dirty empirics will. This is one reason for why so many of the most influential studies of institutions have been based on qualitative historical evidence (for example, Greif 2006; North, Wallis, and Weingast 2009).

Of course, critics have raised several complaints about analytical narratives. The knee-jerk reaction from economists working in the thin/clean tradition is often to object that analytical narratives run the risk of being “just so” stories. These are ad hoc explanations or ex-post rationalizations that are difficult or impossible to verify. However, there are several reasons this need not be the case with analytical narratives. The comparative statics of the model provide a

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<sup>6</sup> While much of law and economics is based on neoclassical economics and welfare analysis, there is a long tradition of institutional analysis within law and economics that tends to favor thin/dirty approaches. Ronald Coase’s (1937, 1960) most famous papers fall within this approach. Harold Demsetz’s (1967) classic paper on property rights does so as well. Major works by both Robert Ellickson (a former president of the American Law and Economics Association) and Lisa Bernstein (a current director of ALEA) likewise use thin/dirty approaches (Bernstein 1992; Ellickson 1994).

falsifiable test. The logic of the model itself provides a check on the internal validity of the proposed relationships. The narrative, if done correctly and successfully, can provide ample evidence that the model applies and does so in the way that the intuition of the model suggests. Of course, analytical narratives could be used to tell just so stories, but that's no different than with any other approach.

A second concern is the belief, often implicitly held, that qualitative evidence cannot be analyzed or assessed in a rigorous way. The worry is that narrative evidence is no evidence at all and therefore cannot confirm or disconfirm theoretical predictions. This concern is largely misplaced. There is a large and thoughtful literature on how to deploy qualitative research methods in a rigorous way to make descriptive and causal inferences (Skarbek 2020a). Again, social scientists working with qualitative evidence can do a better and worse job, but there are many fine examples of analyses based on qualitative methods (see, for example, Leeson 2007).

Related to this issue, some economists seem to discount qualitative evidence not because it is qualitative but because it is associated with disciplines that use thick theories, which they see as less scientific or rigorous. Because of the overwhelming focus on thin/clean approaches, economists rarely make this distinction between types of theories and types of empirics. But a failure to distinguish the two can lead one to wrongly reject dirty empirics because of the perceived weaknesses of thick theories. Of course, the analytical narrative project is based on the argument that theoretical and empirical approaches can be used in different combinations. Adopting dirty empirics does not require economists to forget all that we've learned about maximizing behavior, equilibrium, and incentives. It would be a mistake to ignore narrative evidence simply because much of the research that engages with narrative evidence lacks the thin models that economists favor. In fact, this is one of the major benefits of using analytical narratives.

Political scientist Jon Elster (2000) makes a more substantive criticism of *Analytic Narratives* in his influential review in the *American Political Science Review*. He criticizes the assumption of rationality in the use of rational choice in general, suggesting that the presence of uncertainty or any of the litany of alleged behavioral biases make this a weak approach (Elster 2000, 692). However, unless economists plan to change disciplines, such a sweeping concern should not lead one to discount analytical narratives too heavily. Moreover, for economists like Boettke working within the Misesian tradition of economics, these concerns are far less relevant. Behavioral criticisms do not land a fatal hit if one's view of rationality is constituted primarily by methodological individualism and "purposive" action rather than highly specific and stringent models of decision-making. Elinor Ostrom (1998) likewise distinguishes between "first-generation" models of rational choice that are more vulnerable to complaints like those raised by Elster (2000) and Green and Shapiro (1994) and "second-generation" models of rational choice that offer more sophisticated insights into rational choice.

Elster also offers a set of criticisms against several of the empirical applications found in *Analytical Narratives*. He argues that many of the models take place at a level of abstraction that is too high. For example, he questions the applicability of models to states as actors. To do so, one would need to show that people have overcome a large number of collective action problems within states. Moreover, one would need to assume away classic problems of Arrowian cycling within these groups to argue that they have something approaching a coherent utility function. In addition, Elster questions the accuracy of the narrative in several chapters, arguing that the empirics are insufficient to substantiate the model. Elster's complaints are not without merit in many of these cases, but they are not damaging to the fundamentals of the analytical narratives

approach. Again, as with any method, analytical narratives can be applied more and less effectively.

A final debate about analytical narratives is to what extent we can generalize from one case to others (this is a more sophisticated cousin to the “just so” story complaint). It is certainly true that generalization is one of the primary weaknesses of case study research, and social scientists working with case studies are well aware of this (Gerring 2017, 244-245). Given this concern, is there still value in analytical narrative case studies? There are several possible responses. First, some people argue that analytical narratives provide a framework for explaining a unique event or outcome and that alone is a worthy endeavor (Levi 2004, 203). For example, Avner Greif’s analytical narratives are offered as a context-dependent description of key causal mechanisms that explain an important historical event (Greif 2006, Chapter 11). There is no claim to generalizability, but this is not acknowledged to be a significant shortcoming.

Second, Levi argues that successful analytical narratives are themselves a test of the broader rational choice framework. In this sense, case studies provide evidence on whether rational choice is successful as “a general theory of how structures shape individual choices and consequentially collective outcomes” (Levi 2004, 218). This is similar to an argument made in favor of lab experiments. Interactions in the lab are often incredibly different from the real world interactions that we are trying to understand, typically far more simplified. However, experimental economists argue that the theory that is applied and tested in the lab is what allows them to generalize, not the specific finding itself. The test in the lab informs the usefulness of a theory, and the theory has the potential to apply to a wider range of empirical cases.

A third response is that the extent to which we can generalize from one case is ultimately an empirical question about whether a mechanism in operation in one setting can be found in other,

similar situations. There is an increasing amount of evidence suggesting that this is, in fact, often the case. For example, in the positive economic analysis of anarchy – what Boettke (2005) calls “analytical anarchism” – we see very similar types of mechanisms generating cooperation across a highly diverse set of markets, countries, and communities (see review essay by Powell and Stringham 2009). Out-of-sample predictions to new cases are especially informative. No single analytical narrative can lead us to say that similar mechanisms are important in many other cases, but a series of case studies can provide insights about which particular mechanisms are more or less likely to operate. It would be as unreasonable to think you can generalize from one case to all cases, as to assume that you can infer nothing from a series of cases that generate consistent findings.

A final response is to caution against placing too much importance on generalizability. Following Russell Hardin (2003), Boettke often argues that there is a great deal of indeterminacy in social, political, and economic outcomes and institutions. High levels of indeterminacy suggest that no explanation or mechanism can reliably generalize to many or most other cases. This is precisely why we need close studies of mechanisms. One of Boettke’s responses to the problem of indeterminacy is Elster’s (2015) book *Explaining Social Behavior: More Nuts and Bolts for the Social Sciences*. In it, Elster provides a wide-ranging and insightful discussion of many types of individual, social, and institutional mechanisms. From this perspective, Boettke raises what he sees as a valid concern about indeterminacy, but then shows that a focus on mechanisms as studied in analytical narratives resolves this problem. Levi, like Boettke, argues that analytical narratives, because of their sensitivity to context and rich narrative evidence, are especially well suited to explaining how things like contingency and path dependence give rise to multiple equilibria (Levi 2004, 211). John Ferejohn makes an even stronger argument. To explain why one particular

equilibrium is chosen over others, economists often require evidence of an “interpretivist” nature about cultures, symbols, and meaning (Ferejohn 2000; see also Koppl and Whitman 2004). Narrative evidence is then either especially helpful for understanding mechanisms, or even perhaps necessary, for explaining why a particular equilibrium emerges.

While the analytical narrative has not become as common or accepted in economics or political science as it deserves to be, it still plays an important role in the scholarship happening at GMU. The reason for that has much to do with how Boettke and colleagues think high-quality work in political economy should be done. First, the best research questions are often found by looking “out the window,” at real-world institutional outcomes rather than toying with iterations of existing models or scanning the horizon for natural experiments from which one can back out a research question. Second, Boettke has always advocated the centrality of institutions in the positive study of political economy (Boettke et al 2013). This work often must be historical and comparative. Moreover, the importance that Boettke places on institutions – manifest in his focus on both political institutions and comparative institutional analysis – meant that many of us were more interested in causal *mechanisms* rather than causal *effects*. Narrative evidence and case studies helped us to understand these mechanisms.

The use of analytical narratives also changed how many of us spent our time, with many of us going “into the field.” For example, many grad students and faculty did fieldwork in New Orleans in the wake of Hurricane Katrina to understand if and how recovery would happen. They interviewed residents who did not leave, those who returned quickly, and even those in other cities who never came back (Boettke et al 2007). Likewise, when I was working on my dissertation about prison gangs, I realized that there was not much useful data on gang membership or activity available. The California Department of Corrections and Rehabilitation even denied me access to

seemingly harmless data on other issues that were relevant to my theory, such as the share of certain ethnicities at a particular prison, “because of it’s [sic] sensitive nature in relation to gangs and gang activity.” Strict adherence to a thin/clean research approach would have meant the end of the study. Instead, I started reading law enforcement documents, gang rules and regulations, visiting prisons, and talking with gang members, formerly incarcerated people, police officers, and correctional staff. These sources of evidence provided insights that were not available in thin evidence sources. Moreover, even if I had the data I asked for, it would have said very little about how gangs organize, how influential they are, and how they change the social order of prisons. It would have been insufficient on its own to answer the questions I was asking. If I had to choose between the clean data and dirty narrative empirics, the latter were far more important in increasing my understanding of these institutions (Skarbek 2014, 2016, 2020b).

Of course, there is a professional risk for many young economists who deviate from the standard thin/clean approach. Dani Rodrik warns that the economics profession “is unforgiving of those who violate the way work in the discipline is done,” and this claim probably applies to using analytical narratives (Rodrik, 2015: 199). As a practical matter, many economics journals are skeptical about papers submitted with no quantitative evidence or statistical analysis. To some economists, that work is simply not considered economics (Leeson 2020). Nevertheless, I am convinced by the body of scholarship in political science and economics that thin/dirty approaches provide important insights about institutions that we simply cannot learn with thin/clean approaches alone. Moreover, there often seems to be a trade-off between focusing on finding good statistical identification and the importance of research question (Ruhm 2019). Scholars of political economy, especially junior economists, should think hard about how to manage this trade off.

However, if one is interested in writing about institutions and institutional change and is open to the use of qualitative evidence, the analytical narrative provides a useful way forward.

Boettke's advocacy of analytical narratives had several consequences that shaped the intellectual community at GMU. First, as noted above, by opening up the space to do qualitative research, many of us engaged in field work and relied on other forms of qualitative evidence. This also increased the scope of topics that we could write about. Although not always successful, the willingness to write papers with the thin/dirty approach creates two arbitrage opportunities. One is to bring thin theories into debates and literatures that primarily use thick theories. Peter Leeson, for example, uses parsimonious economic models to explain a wide range of unusual religious and superstitious practices (Leeson 2017). One can also bring narrative evidence to thin theories that previously engaged only with clean empirics. As Theda Skocpol argues in the context of comparative historical sociology, the large, existing secondary literature that is often available is a boon to someone arbitraging in these ways (Skocpol 1984, 382). Related to this, the analytical narrative method opened up the scope to write on topics that were far less conventional in economics. For example, Peter Leeson wrote a series of fascinating papers on 18<sup>th</sup> century maritime piracy while I was in grad school, which opened up the intellectual space for me to write about contemporary criminal organizations (Leeson 2007). Because of the methodological pluralism defended by Boettke and colleagues, grad students could take a chance and risk writing about topics that we found fascinating, even if unconventional.

Boettke's methodological vision also expanded the scholarly communities that we engaged with, especially with political scientists, who were now relevant discussion partners. In particular, Elinor Ostrom's work had a tremendous influence on many of us at GMU. She had been scheduled to visit prior to winning the Nobel Prize in October 2009, and when it was announced, I assumed

she would be too inundated with requests on her time to make it. Yet, not only did she come and give a lecture to a large audience, but she also spent more than an hour sitting around a small table with about ten of us grad students asking about *our* work. She listened carefully, asked probing questions, and offered suggestions. More generally, engagement with a wide range of scholars outside of economics is one reason why grad students at GMU are more likely than students in other programs to be well read across multiple disciplines, to engage with the empirical work in other fields, and to value interdisciplinary work in the tradition of Philosophy, Politics, and Economics.

In closing, Boettke has created an important intellectual community at GMU. It is focused on big questions about institutions and institutional change. It is methodologically pluralist, but committed to rational choice theory and institutional analysis. Boettke's tremendous breadth and depth of scholarship across economics – but also political science, sociology, history, and anthropology – created the opportunity to engage important debates across disciplines. He is the master arbitrageur, bringing analytical narratives to topics and problems raised in so many disciplines and debates. Finally, he maintains the highest level of charity and understanding in these debates, and I often feel he understands other side's arguments more fully than they do. Thank you, Pete, for championing the rigorous, positive analysis of political economy.

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