

Inventing the Economy
Or: How We Learned to Stop Worrying and Love the GDP

by

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A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Sociology)
in the University of Michigan
2016

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For Mom, Dad, Sam, & Jen.

Acknowledgments

In the Fall of 2008, I walked into Greta Krippner's office with a question. I had just read Timothy Mitchell's *Rule of Experts* and I didn't know what to make of his claim that "the economy" was somehow new to the 20th century. Greta encouraged me to spend some time trying to figure it out and, after seven years, this dissertation is the result. I thank Greta, Gabrielle, Mark, Jason, and John for overseeing its completion.

Along the way, I have received generous feedback and assistance from too many people to list. I do want to specially acknowledge two of my co-authors: Elizabeth Popp Berman and Isaac Reed. Many of the ideas that informed my approach in this dissertation were worked out in conversation with them, and I am thankful for their constant support and sheer intellectual firepower.

About halfway through the process, I was fortunate enough to find another source of constant support and intellectual firepower: Jamie Budnick. Thank you for copyediting my prospectus, reviewing my job market paper the night my first application was due, and agreeing to marry me.

This dissertation is dedicated to my family. I can't imagine going through this without you. I am who I am because you are who you are.

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CHAPTER 1

Introduction

1.1 Inventing the Economy

Before the 1930s, the economy did not exist. In this dissertation, I explore what it means to make a claim that the economy did not exist before the 1930s, the evidence for this claim, and some of the consequences of the crystallization of the economy as an object of knowledge and policymaking in the 1930s-1940s.

Science studies scholars have a long tradition of studying the formation of new scientific objects (Pickering 1993, Latour 1999, Knorr Cetina 2001, Hacking 2002, Mol 2002). The consensus of this research is that objects are *enacted* (brought into being) through specific practices (Mol 2002). Scientists, or other experts, construct objects through the practices that identify those objects, draw their boundaries, establish their properties, and so on. Work on the social sciences has identified numbers and statistics as particularly capable of making things “hold together” (Desrosières 1991, Porter 1995).

Following this tradition, I identify one set of practices — national income statistics — that enact the economy. National income statistics here refers to a broad family of aggregate income and production measures, including national income, gross national product, gross domestic product, and close relatives (net national product, etc.). Historically, governments began to produce official national income statistics in approximately the same moment (1930-1950) that the economy took shape. I argue that this timing is not coincidental, but rather causal. Specifically, I argue that the routine, official production of national income

statistics was a cause of the formation of the economy. The economy came into being, and is sustained by, a particular *regime of perceptibility* (cf. Murphy 2006, Hecht 2012), a collection of people, tools, and ideas that provided sustained attention to particular facets of the economic world. This dissertation is thus a member of the genre of *formation stories*, a kind of causal argument about the coming into being of new social kinds. Drawing on my work with Isaac Reed (Hirschman and Reed 2014), I discuss below how formation stories provide causal accounts that differ from traditional approaches to causal inference.

In some ways, the economy is a strange sort of object to study via this practice-centered approach. Many of the objects analyzed by science studies scholars are mundane and eminently physical like the veins inside a leg. Scholarly analysis is motivated by the need to unpack the complexities of seemingly simple and obvious things. Atherosclerosis seems like something you can point to — here’s plaque inside a vein in a pathology lab. But, as Mol (2002) shows, atherosclerosis is necessarily enacted in different and incompatible ways in different settings — the definition in the pathology lab is fundamentally unavailable to patients and primary care doctors, as it requires the removal of the limb in question. The analytical payoff from these studies comes from rejecting the obvious “there it is!” character of the object. But the economy is a bit different. Where is it? Everywhere and nowhere. What is it? Everything and nothing at once.

And yet, we also think of the economy as the most material thing — indeed, “material” is often code for “economic” in social theory. Similarly, despite the economy’s potential vagueness, in practice, the economy exists as something to which we can point, refer, politic, and so on. When it comes to presidential elections, as President Clinton’s strategist, James Carville, famously said, “[It’s] the economy, stupid.” And a large literature in political science would agree — the economy does determine elections (e.g. Kramer 1971, Achen and Bartels 2004, Healy and Malhotra 2013). More mundanely, the economy is something we constantly invoke and characterize — “the American economy grew 2% last year” or “The Japanese economy never recovered from its lost decade.” As Suttles

(2010) has shown, the economy exists inside a dense landscape of terminology. It can be a machine or an organism, it can grow or shrink, it can be healthy or weak. It can consist of manufacturing or be service-based or financialized (Krippner 2005). Thus, in a very important sense, the economy has become an object according to Star's (2010: 603) useful definition: "something people... act towards or with" whose "materiality derives from action, not from a sense of prefabricated stuff or 'thing'-ness."

My purpose here is to ask how the economy came to be the sort of object it is — "how the leopard got its spots." The critical insight will be that the economy — unlike "culture," "politics," or "society" — has a well-defined size. The concept of an "economic sphere," separate from and analogous to the political or cultural spheres, dates back to the 18th century, give or take (Rothschild 2014). In the 1930s, the economic sphere became something measurable in the aggregate, and in whose aggregate performance everyone was interested. No similar transformation happened to the political or cultural spheres. The economy thus has a dual character: on the one hand, an area of life or mode of action (analogous with politics or culture), on the other, a well-defined, bounded object.

This introduction proceeds as follows. First, I clarify what kind of a dissertation this is (and thus what the formation of the economy is a "case" of). I offer a detailed discussion of the concept of formation stories, and how my approach to tackling the formation of the economy is informed by methodological debates in historical sociology and science studies. Second, I outline how this dissertation makes a broader contribution to sociological debates about the performativity of economics, and the relationship between expertise, quantification, and politics. To do this, I discuss and elaborate the concept of regimes of perceptibility, first put forward in the science studies literature. Third, I provide brief summaries of the empirical chapters of the dissertation, focusing on how these chapters make contributions to discussions to the historiography of macroeconomics, the evolution of political responsibility for economic outcomes, the economic value of housework, and the rise of top income inequality as a social problem.

1.2 Formation Stories in Sociology

This dissertation provides a formation story, a causal account of the coming into being of a new social kind which can then, in turn, participate in other causal relationships.¹ Formation stories are a venerable and widely-practice form in sociological explanation, but they have often been misrecognized as interpretive, descriptive, “merely historical”, or other noncausal derogatories (Hirschman and Reed 2014: 276).² The genre of formation stories includes accounts of the emergence of new organizational fields (e.g. Medvetz 2012 on the “crystallization” of the space of think tanks), analyses of the shaping and reshaping of sociotechnical objects (e.g. Eyal 2010 on the history of autism), and even work on the formation of the individual as an autonomous actor (Meyer and Jepperson 2010). Formation stories stand in stark contrast to more conventional *forcing cause* accounts. In this section, I briefly summarize the three dominant modes of forcing cause accounts, and then explain how formation stories necessarily violate key assumptions of these forcing cause approaches while still aiming to provide causal explanations. I conclude by arguing that my formation story, like many others, is doubly causal: it provides both a causal account of the emergence of “the economy” as a new social kind, and a causal account of the consequences of that emergence.

The dominant approaches to causality in sociology can all be characterized by the metaphor of “forcing.” These approaches emphasize how some aspect of the social world pushes or pulls on another aspect to move an object of analysis from one state to another. Following Goldthorpe (2001), we can group these approaches into three rough groups: “variable causality,” “experimental causality,” and “mechanisms.” Variable causality searches for robust associations between two variables, such as the link between father’s

¹This section draws heavily on Hirschman and Reed 2014. I provide in-text citations only to specific claims and quotes.

²Note, I am not claiming that descriptive work *should* be derogated. In contrast, I think descriptive work is highly undervalued in our current disciplinary quest for causal explanation. My point here is that formation stories are not just descriptive, and that allegations to the contrary both confuse the issue and reinforce the denigration of both formation stories and true descriptive accounts.

education and occupation and son's occupation in the famous Blau and Duncan path model. Experimental causality relies on the experimenter's agency in designing an intervention in the world (or statistically identifying a natural experiment) in order to tease out the relationship between a treatment and a cause. Mechanistic causality takes diverse forms, but always involves searching for an account "one level down" from the correlation or outcome to be explained. Mechanistic accounts include rational choice approaches that focus on microfoundations (explanations of aggregate outcomes in terms of simple rules of behavior at an underlying level), and critical realist accounts that treat mechanisms as components of contingent conjunctures. All of these approaches rely heavily on what Abbott (1988) has called the "fixed entities" assumption, the idea that the objects whose movements are to be explained are fixed across the explanatory timeframe. In other words, these accounts rule out the possibility that new social kinds or outcomes could come into existence. As such, they are incapable of addressing questions like the one posed in this dissertation: how did we come to live in a world that contains "economies" as object of knowledge and policymaking? How did economies come to be the kinds of things that could be growing or shrinking, industrialized or financialized?

Tackling the question of formation requires making four breaks with forcing cause approaches. First, we must abandon the fixed entities assumption, and allow for the possibility that new social kinds may come into existence. We must embrace the historically variable ontology of the social. Note that this is not a call for complete relativism, but rather a recognition of the historical character of social construction. Autism exists *because of* the social and technical matrix in which it is embedded (Eyal 2010). Similarly, I show how the economy comes into being at the center of a collection of statistical practices, economic theories, and policy regimes.

Second, we must treat history as eventful (Sewell 2005). New objects come into being through both the slow accretion of structural changes and through the chrysalis of transformative moments. The history of social kinds is one of discontinuous ruptures layered on

top of slow evolutions. In this dissertation, I point to several key events whose unfolding shaped the economy. In particular, the responses to the Great Depression and the mobilization for World War II catapulted a relatively uninfluential academic preoccupation (national income statistics) into the policy limelight, and in so doing transformed our understanding of “the economic” into a particular concrete social kind, “the economy.” I also take pains to note, however, that this eventful transformation occurred on top of structural changes in the organization of production, of academic knowledge production, and of the state which made possible the statistical practices that gave the economy its shape.

Third, drawing on the tradition of actor-network theory and the broader conversation in science studies on the formation of technical objects discussed above, we must accept that formation stories proceed through messy assemblages of actors of different scales and different kinds. Eyal (2010) offers a fantastic example here, showing how autism as a social problem emerges from the intersection of psychiatric knowledge, state imperatives to take care of troubled children, the lay expertise of parents, the tangle of billing and reimbursement practices of the American health care system, and more. In my account, I focus on the intertwined assemblages underpinning the production and dissemination of national income statistics and macroeconomic theories.

Finally, inspired by the “dynamic nominalism” of Ian Hacking (2002), we must foreground assemblages involving representation, self-understandings, and culture, broadly writ. To create categories is both an eminently technical act (Bowker and Star 2000) and an eminently cultural one (Durkheim and Mauss 1967). The categories we create have the power to reshape existing social kinds, and to create entirely new ones. Here, I argue that how economists, politicians, and the general public came to understand aggregate economic affairs concretized “the economy” as capturing the sum of all economic activity and also providing a convenient shorthand for evaluating the success of political programs. As Yarrow (2010) argues, in the US the economy came to stand in for American exceptionalism (for a similar story in post-war Japan, see O’Byrne 2009). The economy was thus

simultaneously (if unsurprisingly) a technical object and a political one, and its representation as a political object transformed its possibilities as a technical one.

To summarize “[formation stories] refer to explanatory accounts of how social kinds are shaped, reshaped, or brought into being; in contrast to forcing causes, these stories take as their points of reference the nonfixedness of social entities, the eventfulness of social life, the emergence of social entities from processes of assemblage, and the dependence of such assemblage and nonfixedness on representation.” (Hirschman and Reed 2014: 268) These formation stories, I argue, are doubly causal. First, they produce causal accounts of *how* new social kinds come into being, and come to take particular shapes. These accounts are messy — social kinds emerge through eventful processes of heterogeneous assemblage — but they are still causal. Second, formation stories provide causal explanations for the *consequences* of the new social kind coming into existence. In my case, I show how the assemblages that produced and sustain the economy made possible new kinds of precision in economic governance, while simultaneously obscuring from view potential social problems.

Given their limitations and messiness, what can we learn from formation stories? Beyond their value as explanations of specific historical events (which need not be trivialized), I think formation stories typically offer two sources of value beyond the matter at hand. First, they are capable of providing boundaries for forcing cause accounts. As I argue in chapter 4, scholars in political science and political sociology have long linked the performance of the economy to voting behavior (a classic example of “variable causality” at work). This analysis, and others like it, tends to treat the economy anachronistically, assuming that measures like GDP or unemployment meant the same thing at all times and places. Yet, as my analysis shows, before the economy existed, these measures were largely unavailable, or only available with a severe temporal lag, and not widely circulated. We would expect a different relationship to obtain between GDP growth and presidential vote share after the economy comes into being as a recognized object (measured by national

income) than we would before it. Second, a particular formation story is not readily generalizable in its details, but it may provide a starting place for future analysis. For example, Ian Hacking's (1995, 2007) work on "the looping effect of human kinds" has identified a general pattern relating diagnostic categories and the behavior of the diagnosed. This work in turn formed one of the bases for Eyal's (2010) research on autism. Similarly, work on the performativity of economics has described particular examples linking new economic theories to the creation of new markets (MacKenzie 2006). These accounts can and do form the starting point for future work on the influence of economics, providing a rough outline for a subgenre of formation story, if not a tightly specified causal mechanism.

In the following section, I outline how the formation story told in this dissertation contributes to two related conversations at the intersection of economic sociology, political sociology, and science studies.

1.3 Learning from the Formation of the Economy

Sociologists, political scientists, and science studies scholars have been increasingly interested in the consequences of economic knowledge for the functioning of markets and states. In this section, I discuss how this dissertation contributes to two important strands of this literature: research under the umbrella of "the performativity of economics" and work on the intersection of experts, expertise, and politics.

1.3.1 Making Performativity Macro

In 1998, Michel Callon published a pair of influential essays (Callon 1998a, 1998b) that introduced the concept of the "performativity of economics." Drawing on his previous work in the tradition of actor-network theory, Callon argued that sociologists had misunderstood the relationship between economic knowledge (broadly construed to include accounting, marketing, etc.) and economic life (buying, selling, producing, consuming).

Economics, Callon argued, was not (just) an inaccurate representation of an underlying reality, but rather a toolkit that provided individuals and organizations new kinds of calculative agencies, new ways to act economically. Economic sociologists had argued that economic action was embedded in social structures (Granovetter 1985); Callon (1998a: 30) argued in contrast that “economy is embedded not in society but in economics.” Donald MacKenzie elaborated this perspective in a series of influential papers and books on the role of financial economics in financial markets, most notably in *An Engine, not a Camera* (2006) which documents the role of particular theories, and particular economists, in constituting the Chicago Board of Exchange options market. Economics was thus an engine that powered economic activity, not a camera that reflected a pre-existing economic world (more or less accurately).

Thus far, research on performativity has tended to focus on the intersection of particular economic theories and particular markets, especially financial markets (for some notable exceptions, see Muniesa 2014). Despite Callon’s call to embed “economy” in economics, little work in this tradition has focused on the construction of the economy as a whole. Instead, the performativity literature tends to take for granted the existence of a category “the economic” and asks how certain objects or ways of acting are brought under the rubric of the economic. That is, performativity research is interested in how grain is transformed from a particular product born of nature and human labor into a standardized commodity, and later an abstract futures market (Cronon 1991)³. Similarly, Holm (2007) looks at how fish become economic through their transformation into transferable assets. These works tacitly assume the existence of the economic domain (which is generally equated with “the economy”), and ask how different objects are brought into that domain.

While incredibly generative, and useful for analyzing many cases of performativity, these accounts unintentionally ignore the historical construction of the economic itself, and simultaneously collapse the distinction between the economic and the economy. In chapter

³Cronon’s work predates the performativity literature, but is a key touchstone for it.

2, I synthesize the historiography of (macro)economic thought to situate the emergence of the economic as sphere of social life and the economy as a sociotechnical object of knowledge. My work thus extends discussions of the performativity of economics to the macroeconomy as an object, while offering a gentle reminder that acts of economicization (Caliskan and Callon 2009) only make sense in light of the historical emergence of the economic as a category.

In chapter 4, I further extend the performativity perspective by reuniting Callon's work on performativity with earlier research in the actor-network theory tradition that emphasizes how macroactors are constructed from networks of suitably black-boxed smaller actors. In "Unscrewing the Big Leviathan", Callon and Latour (1981) argue that the so-called macro actors of sociology, such as states and large corporations, are not ontologically different from micro-actors. Rather, macro actors set atop networks of control, making it possible for them to influence larger amounts of the social world. These networks take the form of black-boxed relationships, where those at the center are capable of issuing orders outwards without having to take into account all of the details of what happens underneath. These links are simultaneously social, political, and technological, and it is the technological links that most distinguish modern large-scale forms. Put differently, "technology is society made durable" (Latour 1991). Social and political alliances are unstable; technological linkages help to stabilize them, and thus make society more durable.⁴

I bring together this work on macro-actors with Callon's more recent perspective on the embeddedness of economy in economics to make sense of modern states. With the invention of national income statistics, and the stabilization of the economy as a technical object, states gained a powerful new way of governing as macro-actors. The economy is made simultaneously "big" (encompassing all economic activity) and "small," manipulable, understandable, measurable in tables and charts and reports that fit inside an office.

⁴This is not to say that technological linkages are flawless or incapable of being disrupted. Rather, this perspective highlights how technologies form another kind of connection between actors that tends to strengthen networks. On the possibilities for technological failure, see Akrich 1992.

Thus, this dissertation continues the work of unscrewing the biggest leviathans, and making sense of how seemingly small practices (statistical computations in a single office in a single Bureau) can join a network of actors to make possible a new mode of governance.

1.3.2 Policy Devices, Styles of Reasoning, & Regimes of Perceptibility

While performativity scholars have debated the effect of economics on markets, a somewhat separate group of scholars in political science and sociology have analyzed the influence economists on *policy*. In Hirschman and Berman (2014), we summarize this research at length, and point towards ways of integrating ideas from science studies (including performativity) into the analysis of the policy effects of economic expertise. In particular, we argue that economic expertise influences policy through three distinct routes: professional authority (the broad legitimacy of economists), institutional position (the presence of economists in the decision-making apparatus of the state), and “cognitive infrastructure” (how policymakers think about and measure the world they govern).

This last route of influence requires the most explanation, and relates most closely to the themes explored in this dissertation. Cognitive infrastructure “refers to economic *styles of reasoning* prevalent among policymaking elites, as well as the establishment of economic *policy devices* that produce knowledge and help make decisions.” (Hirschman and Berman 2014: 781) Styles of reasoning, a term borrowed from Ian Hacking (1992) are basic tools of understanding applied to a given domain. For example, the concepts of incentives, opportunity costs, and maximizing behavior are key components of the modern economic style of reasoning. One route by which economics (as a field of knowledge production) influences policy is through the diffusion of economic styles of reasoning. As economists gained professional authority in the 20th century, they gained positions teaching in law schools and policy schools, and thus helped to diffuse basic economic concepts to future policymakers. Similarly, economists in institutional positions of power (such as the Council of Economic Advisers) spend much of their time expounding basic economic

principles.

Policy devices, in contrast, refer to specific sociotechnical tools used to see the economic world, or to make decisions about economic affairs. Policy devices for seeing include economic statistics, like the national income statistics which are the focus of this dissertation. Hirschman and Berman (2014: 797) describe the influence of policy devices for seeing:

As scholars have long noted, policymakers suffer both from incomplete information and from too much of it (March and Simon, 1958; Lindblom, 1959). Faced with a vast array of choices, they rely on various tricks to narrow their field of vision enough to make it possible to act (Scott, 1998). The production of numbers that describe the world—numbers like GDP, the inflation rate, the unemployment rate—help policymakers to see crisply certain facets of it that, without the devices that produce them, would be blurry at best.

This dissertation extends the account in Hirschman and Berman (2014) by focusing on what economic statistics reveal *and* on those aspects of economic life that remain out of focus. Here, I draw on the concept of regimes of perceptibility introduced by Michelle Murphy (2006) and Gabrielle Hecht (2012). Regimes of perceptibility are bundles of people, ideas, and tools that provide sustained attention to certain aspects of social life while, implicitly, rendering other aspects invisible. Hirschman and Berman (2014) discuss policy devices for seeing in relative isolation; the concept of regimes of perceptibility, elaborated more fully in chapter 6, helps to connect the dots between particular policy devices and the actors that employ those devices to surveil the economic world.

In chapter 3, I narrate the construction of a particularly influential regime of perceptibility around macroeconomics, with national income statistics at its center. This chapter links the emergence of the “macroeconomic style of reason” to the statistics that make macroeconomic outcomes visible. In chapter 4, I show how this macroeconomic regime of perceptibility, and the associated style of reason, helps policymakers “see crisply” macroeconomic outcomes (especially economic growth), which then become targets of governance, and political obligations. In chapters 5 and 6, I show how this macroeconomic regime of per-

ceptibility leaves out unpaid housework and the distribution of income. These aspects of economic life are not completely invisible — other statistical techniques are capable of rendering them visible — but they are capable of being ignored, not automatically subject to intense scrutiny. Together, these chapters offer a novel conception of the political power of economic experts which complements existing accounts in political science and sociology.

1.4 Data and Methods

A compelling formation story must offer a comprehensive account of how the object or kind in question emerged, and trace the links between the specific details of that emergence to consequential outcomes. In order to approach this task, I began in typical ANT fashion by “following the actors” — in this case, national income statistics and national income statisticians. To follow the actors, I made trips to the archives of the most important national income statisticians in the early 20th century including, in the US, Wesley Mitchell, Simon Kuznets, the Bureau of Foreign and Domestic Commerce (at the National Archives) and in the UK, James Meade, and Richard Stone. I also read influential primary sources that presented national income estimates, including the 1921 NBER publication, the first Department of Commerce estimates authored by Simon Kuznets in 1934, as well as many others. I consulted extensively the “grey literature,” published conference proceedings, working papers, and edited volumes, especially the NBER *Studies in Income and Wealth*, and the journal *Review of Income and Wealth*. These sources, along with textbooks and other book-length treatments, provided important insights into the development of national income statistics as a field, and how practitioners themselves saw the value of their work, and the challenges they faced.

To understand the place of national income statistics in the broader space of economic discourse, I expanded my analysis to include a wider sample of economic works. Most systematically, I analyzed 42 introductory textbooks (constituting approximately 20,000

pages of text) published between 1890 and 1960 to understand the evolution of debates over large-scale economic phenomena, and the data mobilized to make sense of those phenomena. I detail this sample and my approach for analyzing it in chapter 2. I complemented this reading of economic textbooks with an analysis of count data of key macroeconomic terms in three large N corpora: JStor's Data for Research, Google Books, and the New York Times Chronicle. I searched the three main economics journals of the period, *American Economic Review*, *Journal of Political Economy*, and *Quarterly Journal of Economics* to understand how they mobilized aggregate statistics, and how they discussed aggregate economic phenomena. Finally, I consulted a smaller collection of archives of prominent economists who worked with national income data and brought national income analysis into policymaking, especially Paul Samuelson and John Maynard Keynes.

These works helped to inform my understanding of the emergence of the economy and the role of national income statistics within the field of economics. To understand how national income statistics traveled into policymaking, I also analyzed works that intersected more directly with the political arena. In addition to the archival data (which yielded many fruitful discussions between economists and policymakers), I read through presidential speeches and party platforms (largely collected in UCSB's Presidency Project). I also searched the Congressional Record for debates around national income and the economy, reading through every early reference to understand the motivations behind the push for national income statistics in the 1930s. I was unable to travel to the LaFollette family collection, but a helpful archivist provided several key documents which informed my analysis of the 1932 resolution authored by Senator LaFollette calling for official national income estimates. I also examined the popular reception of macroeconomic aggregates in the media, focusing on *The New York Times* and the *Washington Post*.

My primary data sources are summarized below:

Archival Collections

- Simon Kuznets

- John Maynard Keynes
- Richard Stone
- Alvin Hansen
- Economist Papers Project
- James Meade
- Wesley Mitchell
- National Archives (Department of Commerce)
- Paul Samuelson
- La Follette Family Papers

Published Primary Sources

- Economics Textbooks Sample (see Chapter 2)
- NBER Studies in Income and Wealth
- Review of Income and Wealth
- American Economic Review
- Journal of Political Economy
- Quarterly Journal of Economics

Large N Discursive Corpora

- JStor Data for Research
- Google Books
- The New York Times Chronicle

Popular Discourse

- The New York Times
- The Washington Post

Political Discourse

- Congressional Record
- Party Platforms
- Presidential Speeches

1.5 Preview of Coming Attractions

I mobilize these data, along with very helpful secondary sources that narrate at great detail some of the more consequential events discussed in the dissertation, across five empirical chapters. I summarize each chapter briefly here, highlighting how it mobilizes my data to make arguments about performativity, perceptibility, and the formation of the economy.

In chapter 2, I offer a novel interpretation of the historiography of “the economy” in order to resolve a tension between two scholarly camps. The first, centered around Margaret Schabas and perhaps Michel Foucault, argues that the economy emerged in the 18th century with the birth of political economy as a form of discourse. The second, most associated with Timothy Mitchell, places the birth of the economy in the 1930s, with macroeconomics and Keynes. I argue that both camps have captured essential discontinuities in the history of economic thought, but that they have not properly characterized the transformation of the 1930s. I draw on the large N discursive corpora, and the introductory economics textbook sample, to show how writers in the late 19th century had both a well-developed conception of “the economic” and a rich vocabulary with which to describe large scale economic phenomena but that they did not yet aggregate these phenomena into a single object called the economy. Aggregate statistics were occasionally produced as far back as the 17th century, but they were not incorporated into economic theorizing, and were produced irregularly in unstandardized fashions. Only with the emergence of state-led efforts to produce routine, standardized economic statistics do economic texts begin to invoke “the economy” and to begin to see a role for the government in managing that economy.

Chapter 3 moves to the history of national income statistics. Drawing on the archives of national income statisticians, as well as published estimates, the published methodological literature, and limited data on the political and public reception of national income statistics, I analyze the conditions that facilitated the emergence of the practice and its widespread adoption. National income statistics were relatively unimportant as late as the 1920s, and only cemented their place at the center of macroeconomic thought and economic

governance in the crucible of World War II. This chapter thus explains how a particular macroeconomic regime of perceptibility came to dominate postwar economic debates.

Chapter 4 examines the positive consequences of the rise of national income statistics and the formation of the economy as an object of governance. By positive here I mean “what was made possible,” without narrowly judging these transformations as wholly beneficial (or harmful). Drawing on major political party platforms and presidential speeches, and in dialog with extensive secondary discussions of the history of economic policymaking, I show how politicians came to assume responsibility for narrowly defined macroeconomic outcomes, especially the “size” of the economy. Before the 1920s, economic policy debates focused on the government’s role in creating the proper institutions for economic prosperity, broadly and fuzzily conceived. By the 1940s, “managing the economy” offered a new vocabulary for economic claims-making, alongside a new set of policy tools that seemed capable of reacting to short-term shifts in the economy. This chapter thus explains how the emergence of the economy, and the macroeconomic regime of perceptibility that constituted that economy, influenced policy and politics by reshaping the terrain of political debate.

The next two chapters turn from what was made possible to what was excluded. In chapter 5, I look at the history of debates over the inclusion of unpaid housework in estimates of national income. Drawing primarily on debates in the published methodological discourse, supplemented by archival data, I show how national income statisticians continually confronted the same intractable problems around the inclusion of housework. National income statistics relied on market data to produce timely and objective measurements — two major preconditions for their political value. Unpaid housework, by definition, lacked a clear market price. Yet the principles of national income accounting, in particular the “principle of invariance,” suggest that housework ought to be included in order to avoid misleading estimates of economic well-being as the boundaries of the market change over time. Economists searched — and continue to search — for appropriate market prox-

ies, but different estimation techniques yield widely divergent (and very large) valuations. Thus, economists agree that housework would constitute a large portion of national income were it to be included, but lacking an unequivocal estimate of its value, they continue to exclude housework.

Chapter 6 turns from housework to inequality. Recent debates over inequality have highlighted the growth of top incomes, as the top 1% and .1% of income earners take home an increasingly large share of the total. Economists began to pay attention to this increase in the early 2000s, nearly two decades after the massive takeoff began. Why did it take so long for this simple fact of inequality to become apparent? This chapter shows how the particular decisions made about the construction of the macroeconomic regime of perceptibility — and a parallel regime for labor economics and wage income — ended up occluding the distribution of income from view. Drawing on published primary sources, newspaper articles, congressional hearings, and economic journal articles, I show how macroeconomists came to ignore the distribution of income, while labor economists paid attention to the distribution of *earnings* using data that were incapable of seeing movements at the very top. This chapter further elaborates the concept of regimes of perceptibility, and offers a model for understanding how such regimes can influence policymaking and economic theorizing through their capacity to shape the construction of “stylized facts.”

Finally, in the conclusion, I speculate about counterfactuals and alternatives. In order to make sense of the importance of the formation of the economy, and the particular details of its formation, I pose a series of increasingly implausible or distant counterfactuals. This exercise highlights how small changes in national income statistics would likely have had local effects (changing the politics of data collection around housework, reshaping our narratives of inequality), while counterfactuals involving radically different statistical regimes imply much sharper deviations and thus highlight the overall importance of the invention of “the economy” as an sociotechnical object with a well-defined size routinely measured by national income.

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CHAPTER 2

From Economic Life to Macroeconomy

What did we talk about before we talked about the economy? What difference does it make? This chapter resolves a debate between two groups of historians of economics about the emergence of the economy as an object of knowledge. The first camp, identified with the work of Margaret Schabas, dates the emergence of the economy to approximately the 18th century. The second, identified with Timothy Mitchell, argues that the economy did not emerge until the 1930s. I argue that these two groups have largely talked past each other and thus mischaracterized the history of economic thought. In the 18th-19th centuries, “economic life” emerged as a recognized domain, separate from but connected to political life and nature. In the 20th century, “economic life” crystalized into an object called “the economy.” The economy was *both* a domain of life parallel to politics or nature and a reified sociotechnical object with a definite size and investigable dynamics. I support this argument with an analysis of economic discourse, drawing on both simple nGrams and in-depth readings of 42 economics textbooks published between 1890 and 1960.

2.1 Introduction

When did the economy emerge as an object of public discourse and scholarly analysis? And what did we talk about before we talked about the economy? In this chapter, I analyze the evolution of American economic discourse from 1890 and 1960. I use this analysis to adjudicate between two competing positions on the emergence of the economy. The first,

associated most strongly with Margaret Schabas (2005) and certain readings of Michel Foucault (Dix 2014), places the emergence of the economy in the 18th to early 19th century, with the birth of political economy as a field of inquiry. The second, expounded most prominently by Timothy Mitchell (1998, 2002, 2011), argues that the economy did not emerge until the 1930s with the rise of Keynesian macroeconomics.

I argue that both positions capture important transitions in economic discourse, but neither correctly characterizes the mid-20th century: the first downplays the period and sees largely continuity from the early 19th century to the present, while the second exaggerates the rupture. Informed by historical sociology on the one hand, and science studies approaches to the formation of technical objects on the other, I argue that the 18th and 19th centuries saw the formation of “the economic” as a sphere of social life and a kind of social action distinct from politics on the one hand, and nature on the other. In the 20th century, the fuzzy conceptual space of “the economic” crystallized into a technical object, “the economy.” The economy, as distinct from the economic, is characterized by having a well-defined size and boundaries measured through national income statistics. That the economy, unlike politics or nature, has a well-defined size made possible new forms of discourse about its short and long term dynamics, as well as new political obligations. In chapter 3, I trace in detail the history of national income statistics and how we came to institutionalize a particular version of that practice as “the size of the economy.” In chapter 4, I then show how the availability of timely measurements of the economy’s dynamics made possible new forms of government intervention, while simultaneously obligating governments to take responsibility for outcomes that would have been previously invisible.

In this chapter, I begin by laying out the positions advanced by previous scholars on the emergence of the economy. I then further elaborate my interpretation of the formation of the economy as an object in the mid-20th century and explain how it resolves the tensions between the two existing approaches. The second half of the chapter presents empirical support for this argument. I first draw on a three large corpora of discourse—

Google Ngrams, JStor’s Data for Research, and the New York Times Chronicle—to show the trends in the usage of key economic terms from 1800 to 2014, with an emphasis on the transition period of 1890 to 1960. I connect these large-scale trends to a brief analysis of influential statistical reports, and a more detailed analysis of a sample of introductory economics textbook published between 1890 and 1960.

These diverse sources of economic discourse offer a more nuanced understanding of the formation of the economy than previous accounts, which have generally relied on either a single news source (e.g. Emmison 1983’s analysis of *The Economist*), or iconic texts (Mitchell 1998). This analysis shows that by 1920, unemployment, business cycles, and inflation—but not economic growth—were all topics of widespread discussion and attempted measurement. In the 1930s-1940s, these topics merged into the study of macroeconomics, unified by a small set of newly-available official statistical measures.

2.2 The 18th Century Economy?

Historians of economics have long argued that the economy had to be invented. As Dumont (1977: 24) put it, “It should be obvious that there is nothing like an economy out there, unless and until men construct such an object.” This kind of statement takes on two connected, but distinct, meanings. First, that the economy is itself a human construction—in the absence of exchange relationships, property rights, and all the rest, there would be no such thing as an economy. Let’s call this the “trivial realist” perspective—trivial because it is obviously correct, not because it is unimportant. Second, the claim is taken to mean that even in the presence of functioning markets, monetary systems, and other hallmarks of economic exchange, a concept like “the economy” is not *necessary*. People seemed to have managed fine without such a concept for centuries. As Finley (1974: 21) noted, the ancient Greeks and Roman appear to have “lacked the concept of ‘economy,’ and, a fortiori, they lacked the conceptual elements which together constitute what we call ‘the economy;’” and

yet they managed to run cities, states, and empires for hundreds of years (see also Singer 1958, Leshem 2013). Finley continues,

Of course they farmed, traded, manufactured, mined, taxed, coined, deposited and loaned money, made profits or failed in their enterprises. And they discussed these activities in their talk and their writing. What they did not do, however, was to combine these particular activities conceptually into a unit, in Parsonian terms, a ‘differentiated sub-system of society.’ (Finley 1974: 22)

This quote nicely defines the second kind of claim about the invention of the economy, the economy as concept. When did writers begin to discuss what we now think of as economic activity as part of a distinct conceptual unit, a separate societal sub-system?

A large collection of scholars have identified this transition with the birth of political economy in French and British scholarship in the 18th and early 19th century (Foucault 1966, Tribe 1978, Buck Morss 1995, Hindess 1998, Firth 1998, Schabas 2005, Rothschild 2014, Dix 2014). Some place the foundational moment with the French physiocrats writing in the mid-18th century (e.g. Rothschild 2014: 1057). The physiocrats are most famous for their influence on Adam Smith, and for the *tableau economique*, a theoretical accounting system that traced the circulation of goods and money between different groups of workers, entrepreneurs, and landlords that is often seen as a precursor to modern national income accounts (Studenski 1958, see Milanovic 2015 for a detailed discussion).

Other authors have placed more emphasis on developments in the 19th century. In *The Order of Things*, Foucault (1966) argued that the human sciences emerged in a rupture in the early 19th century, as biology replaced natural history, linguistics replaced general grammar, and, most important for our discussion, political economy replaced the analysis of wealth. Foucault (1966) singles out David Ricardo as the key figure in this transformation, and more recent scholarship has extended this analysis (Hindess 1998, Dix 2014).

These two perspectives differ more in emphasis than substance. For example, Foucault himself, never one to fetishize consistent chronology, points to the physiocrats (rather than Ricardo) as the source of a “new conception of the economy” and “maybe even ... the

foundings act of economic thought and economic analysis” (Foucault 2009: 55).

Taken together, we have a reasonably clear picture: political economy emerges in the mid-18th century (starting with the physiocrats) and settles into a reasonably stable form in the early-19th century (culminating perhaps in Ricardo’s [1817] *Principles of Economics*). The economy, then, was the object of this new discourse.

Margaret Schabas provides perhaps the most compelling and thorough account of this transformation. Schabas (2005, 2015) adopts the same chronology as the above authors, but focuses her analysis on how the emergence of the economy separates economic analysis from *nature*:

The discourse of economics in the eighteenth century was thus primarily an attempt to understand specific features of the commercial and agrarian world, with the understanding that these phenomena (money, husbandry, manufacturing, trade, population growth) were imbedded in a larger system of nature and not the economy per se. Only gradually, over the course of the nineteenth century, were the appeals to the natural framing dismantled and replaced by the concept of a relatively autonomous and self-equilibrating economy with humans as the proximate cause. (Schabas 2015: 3-4)

For Schabas, the emergence of the economy as an autonomous sphere separates economic activity from nature. As Finley (1974:22) notes, ancient economic thinkers conflated what we would understand as natural and economic causes. “Modern” thinkers—those writing since the mid-19th century—understand the economy as a domain with its own dynamics separate from, albeit related to, the dynamics of nature.

In his analysis of Ricardo’s work, Dix (2014) agrees with Schabas on the separation of the economic from the natural in the writings of 19th century political economist. Dix adds to this discussion an important analysis of the separation of the economic from the *political*. In Ricardo, “The state . . . becomes an actor that artificially intervenes in a domain with its own natural laws and tendencies. Thereby, the state is no longer an integral part of the economy but something that stands outside it.” (Dix 2014: 16)¹ I return to this theme of the

¹Dix’s account of the separation of the economic from the political coincides provocatively with a reading of Polanyi (1944, 1968), who argues that early 19th century Britain saw the disembedding of economic and

awkward role of the state as both in and outside of the economy in Chapter 5, in the context of debates over the measurement of government output in the national income statistics.

Following scholars like Dix, Foucault, and Schabas, then, the story of the emergence of the economy is relatively clear. From ancient times through the 17th century, “the economy” as concept did not exist. Between the mid-18th and early 19th century, the economy came into being as an object of discourse, separated from, but connected to, nature and politics. This transition can be clearly seen in the canonical works of the new field of political economy, including the physiocrats and their *Tableau Economique*, Smith’s *The Wealth of Nations*, and Ricardo’s *Principles*. In the late 19th century, the vocabulary changes a bit as “economics” replaces “political economy” as the dominant name for the field of inquiry, but the basic story is one of relative coherence from the early 19th century to present.

Not everyone agrees.

2.3 The 20th Century Economy?

A contrasting strand of scholarship on the emergence of the economy begins with an important observation: no one seems to have used the phrase “the economy” in anything like its modern sense until the 1930s. Timothy Mitchell has made this claim most prominently, in a series of books and articles (Mitchell 1998, 2002, 2005, 2011), and earlier work by Emmison (1983) supports the basic claim, as do my own analyses of large bodies of popular and scholarly discourse reported later in this chapter. Mitchell argues that the novelty of “the economy” as a phrase undermines the consensus position that the economy emerged in the 18th or 19th century:

The consensus that the economy became a distinct object of intellectual knowledge and government practice in the late eighteenth or the nineteenth century overlooks a surprising fact. No political economist of that period refers to an

political institutions. That is, Polanyi argues that the economic and political became separated institutionally for the first time in this moment, while Dix argues that they also became separated in economic thought. For a detailed discussion, see Hirschman 2011.

object called ‘the economy.’ In the sense of the term we now take for granted, referring to the self-contained structure or totality of relations of production, distribution, and consumption of goods and services within a given geographical space, the idea of the economy emerged more than a century later, in the 1930s and 1940s. Both in academic writing and in popular expression, the meaning of the term came into common use only during the years around the Second World War. (Mitchell 2011: 125)

Mitchell (correctly) notes that the term “economy” has an older meaning, referring to good management, proper governance, and efficiency (Mitchell 2011: 124). The word economy has ancient Greek roots, descending from “Oikonomia” which referred to proper order (“nomos”) of the household (“oikos”, for details see Singer 1958, Leshem 2013). Political economy, in the sense of Adam Smith or David Ricardo, was understood as the proper management of the state. For example, Smith (1776, Book IV.I.1) offers this definition:

Political œconomy, considered as a branch of the science of a statesman or legislator, proposes two distinct objects: first, to provide a plentiful revenue or subsistence for the people, or more properly to enable them to provide such a revenue or subsistence for themselves; and secondly, to supply the state or commonwealth with a revenue sufficient for the public services. It proposes to enrich both the people and the sovereign.

For Smith, the object of political economy was the enrichment of both the people and the state, but political economy itself was a subset of the art of the state. Mitchell argues that throughout the 19th century and early 20th century, “economy” kept this connotation of good or efficient governance. This usage persists right through the early days of the Great Depression—one of F.D.R.’s first pieces of legislation was the “Economy Act of 1933”, a bill designed to cut government expenditures as part of his less well-remembered campaign pledge to balance the budget.

Mitchell argues that this terminological transformation represents a fundamental shift in the both economics as an academic discipline, and in the political management of economic affairs: “‘economy’ was not just a new word for an existing sphere or underlying substance; its arrival marked the emergence of novel forms of sociotechnical practice that

formed and performed the economy as a new object of professional knowledge and political practice.” (Mitchell 2005: 126) In other words, Schabas and Foucault are wrong. It was in the 20th century, not the 18th or 19th century, when the economy emerged, displacing “population” and other older objects as the central focus of government (Mitchell 1999). Lepler (2012, 2013) similarly argues that early 19th century actors lacked the concepts of capitalism, business cycles, and the economy, and that our persistent tendency to read those concepts back onto past explanations leads us to systematically misunderstand contemporary accounts of financial panics.

Mitchell, following the work of Radice (1984), emphasizes the role of John Maynard Keynes in concretizing the economy, and in establishing the nation as its natural unit of analysis. The publication of Keynes’s influential *General Theory of Employment, Interest and Employment* in 1936 is taken to mark the birth of the economy as object (Mitchell 2011: 134) as well as the the subfield of macroeconomics. Along with Keynes, Mitchell (2005) highlights the role of the early “econometricians”, especially Ragnar Frisch and Jan Tinbergen who developed the first macroeconomic models of national economies in the 1930s. Both Keynes’s macroeconomics, and Frish and Tinbergen’s econometrics were resolutely national, constructing their object of analysis in terms of the boundaries of the nation-state. Mitchell argues that this construction only makes sense in the context of the collapse of European empires in the early 20th century: “Before the 1930s it would have been difficult to describe something called the “British economy,” . . . because the forms of trade, investment, currency, power, and knowledge that might be constituted as an economy were organized on an imperial rather than a national scale.” (Mitchell 2002: 6)² In addition to emphasizing how Keynes invented a *national* economy, Mitchell further argues that Keynes’s new conceptualization opened up the new possibility of limitless economic

²Historian Adam Tooze (1998: 212-218) argues that the national focus of the field actually goes back into the 19th century, at least in Germany, and to at least World War I in the United States. Tooze argues that economic statistics were collected at the level of the nation-state, and these in turn informed the creation of economic knowledge at the same level of analysis. I discuss debates over the level of analysis for the economic system more in Chapter 3.

growth: “The conceptualisation of the economy as a process of monetary circulation defined the main feature of the new object: it could expand without getting physically bigger.” (Mitchell 2011: 139) I will argue that although Mitchell is correct to emphasize dramatic shifts in the understanding of economic growth as a result of Keynes’s work and the new discourse of “the economy”, Mitchell’s analysis places too much weight on monetary circulation as the defining feature of the economy, ignoring the multiplicity of definitions of the economy that existed in the 1930s-1940s (and continue to exist to present).

Other writers agree with Mitchell on the general timeline, but emphasize slightly different actors, especially statisticians. Suzuki (2003) highlights the essential role of national income statistics in laying the statistical groundwork for the emergence of the (macro)economy in the 1930s, while McVety (2012) makes a similar argument in the context of the rise of development economics. Breslau (2003) and Shenk (2015) point to slightly earlier developments, including statistical research on business cycles conducted by Wesley Mitchell in the 1910s, and continuing under the auspices of the National Bureau of Economic Research in the 1920s. These authors agree with Mitchell that “the economy” emerged in the 1930s, and that such transition captures some meaningful change in both economic science and governance.

We are thus left with two contrasting stories. Schabas, Foucault, and Dix argue that the economy emerged in the 18th-19th century. Mitchell refutes this contention, and places the birth of the economy squarely in the 1930s. How are we to reconcile these positions?

2.4 The Economic Sphere and the Economy

To make sense of the apparent tension in the literature, I adopt a perspective informed by science studies approaches to the formation of objects (Latour 1999, Hacking 2002, Mol 2002) and historical sociology approaches to temporality and the role of culture (Abbott 2001, Sewell, 2005). I argue that both camps have identified important turning points in the

history of economic thought, and specifically, the formation of new social kinds. In particular, I claim that the first camp has identified the separation of the economic sphere (broadly conceived) from the political and the natural, while the second camp traces the crystallization of that diffuse atmosphere into a more precisely defined “sociotechnical object” (Hirschman and Reed 2014: 271) bounded and made visible through novel measurement practices.³

The concept of the economic as a sphere of social life may make more sense in explicit dialog with other such conceptualized spheres. In “Religious Rejections of the World and Their Directions,” Weber (1946) identifies distinct value spheres, including the economic, political, esthetic, erotic, and intellectual. Similarly, Boltanski and Thevenot (2006) identify six “orders of worth,” or logics of justification, corresponding to different regimes of value. Palonen (2006) offers a particularly useful account of politics as a territorial and an activity concept—that is, we think of politics simultaneously as a space and as a kind of action. Reading Schabas (2005) and Dix (2014) alongside these accounts, we can rephrase their finding as the coming into existence of a distinct economic sphere—a way of dividing up the world in which some activities are economic, some spaces are economic — as opposed to natural or political. For example, Schabas (2015: 3-4) describes the emergence of the economy in terms of the attribution of causation: “over the course of the nineteenth century. . . appeals to the natural framing [were] dismantled and replaced by the concept of a relatively autonomous and self-equilibrating economy with humans as the proximate cause.” Nature was supplanted by a new autonomous sphere in which humans were capable of causing economic events on their own. It becomes possible to explain particular features

³Hirschman and Reed (2014) suggest that formation stories — accounts of the coming into existence of particular social kinds (objects, forces, and actors) — are an under-appreciated genre of explanation in contemporary sociology, and present tools for both telling better formation stories and for categorizing the kinds of object formed. Formation stories start from four basic premises: (1) the set of social entities is not fixed, the ontology of the social is variable (Abbott 2001), (2) history is eventful and marked by ruptures, including the coming into being of new social kinds (Sewell 2005), (3) social kinds come into being through the heterogeneous assemblage of individuals, texts, practices, organizations, and more (Latour 2005), and (4) new social kinds involving knowledge about people have the capacity to reshape the actions of those described (Hacking 2002, see Hirschman and Reed 2014: 266–268 for details).

of the world in terms of their economic properties.

The economic in the 18th-19th century is thus both a collection of spaces (“economic life”, “the market” conceptualized both as a physical and a metaphorical space, eventually “business”, “industry”, etc.), and a kind of activity (“rational action”, “economic action”, etc.), defined in contrast to nature and politics. Political economy, and eventually economics starting in the 1890s, is the study of this mode of action and this set of spaces. But the economic sphere in this period is not yet a sociotechnical object.

The science studies tradition has a long history of trying to understand what it means for something to be an object (Daston 2000, Hacking 2002). Here, I use the term in the sense of Star (2010: 603) in her discussion of boundary objects: “something people... act towards or with” whose “materiality derives from action, not from a sense of prefabricated stuff or ‘thing’-ness.” Sociotechnical objects are understood as both real *and* historical, as knowledge practices layer on top of already existing, heterogeneous things, and in so doing transform that mess into something more obdurate, crystalline, fixed — objective. The adjective sociotechnical reinforces the key role of technical practices in shaping and defining the boundaries of objects, of “enacting” objects (Mol 2002), while simultaneously reminding us that technical practices are also social practices, that the two are always intermingled (Latour 2005).

The analogy of public opinion may be useful in understanding how the economic sphere might have transitioned into a sociotechnical object, the economy. As Osborne and Rose (1999), and Sarah Igo (2007) have shown, new social scientific research techniques transformed the public’s understanding of itself in the early to mid-20th century. Public opinion ceased to be a nebulous signifier, but rather was attached to the findings of pollsters and sociologists who rigorously investigated what the “average American” thought of politics, social life, and economic affairs. The French elite of the 18th century had learned to ask, what does the public think of the King? But their answers took the form of a heterogeneous collection of circulated books and pamphlets (Hirschman and Reed 2014: 273). The

American elite of the 20th century could ask, what does the public think of the President? and receive answers in the form of precise estimates of public approval. We now routinely rank leaders based on how high their approval stays during the course of the presidency, or how low it goes. Public opinion is no longer just a piece of a diffuse cultural atmosphere, it has become a technical object amenable to precise investigation, scholarly research, and professional manipulation.

I argue that we can understand the formation of the economy in the 1930s in similar terms. In the 1930s, the economic sphere crystallizes into the economy as a sociotechnical object. A key aspect of this transformation is the creation of national income statistics which come to be understood as measuring “the size of the economy.” Along with other macroeconomic indicators developed in this period, including the unemployment rate and inflation measurements, we come to see and act on the economy as an object. When 19th century actors wanted to know how well the economic sphere was doing, they had no easy metrics for success or failure. In contrast, 20th century economists have a battery of measures available in nearly real-time, which take the “pulse of the economy” (Landefeld et al 2008). This process of transforming a broad cultural atmosphere into a bounded sociotechnical object has no direct parallel in politics or biology. There is no ‘size’ of politics or ‘size’ of nature (or culture, for that matter). When political scientists study politics, they necessarily identify political objects of study — voting, volunteering, campaigning — that are understood as political in nature. They do not study “politics” as an aggregate whole. Similarly, economists in the 19th century studied economic objects of various sizes (from individual behavior to commercial crises, as we will discuss below), but only in the 20th century did they begin to *also* study the economy as an object in its own right.

Of all of the writers who have tackled the subject, the work of historian Adam Tooze (2001) comes closest to the position advanced here. Tooze shows how German economic discourse, and economic statistics, transformed in the 1920s, often in advance of similar developments in the US and UK. Tooze (2001:9) similarly argues that the 1920s-1940s

represent a period of crystallization or reification of the economic brought about by official government statistics:

Taken together these interrelated statistical innovations constituted a new matrix of economic knowledge, which gave substance to a new conception of the economy. First of all ‘the economy’ was envisioned as a separate system, distinct, for instance, from ‘the social’, ‘the cultural’, or ‘the political’. It was a measurable entity, a ‘thing’. This conception of ‘the economy’ as an autonomous social system was more restricted than that embodied in eighteenth-century ideas of a commercial society, or Marx’s totalizing conception of the mode of production. But it was also more concrete than those earlier formulations. Linguistic changes signal this shift to a more reified idea of the economic world.

I agree in large part with Tooze’s formulation, and in particular the importance of statistics, and the claim that linguistic shifts in how we discuss the economy signal the concretization of the economy-as-object. Tooze, unlike the two bodies of literature described above, nicely connects 20th century developments with their 18th and 19th century precursors without minimizing the transformations of the 20th century. But Tooze, like Mitchell, errs in attributing the separation of the economy from the social, cultural, and political to the 20th century. Tooze also downplays or ignores the potential multiplicity of the economy — the economy as object does not simply replace older notions of “economic life”, but coexists with them.

Economics in the 20th century thus becomes somewhat more like demography (with its well-defined object, population), but at the same time, the economy retains its broad character of a fuzzy conceptual space and mode of action. The economy comes to mean all of these things at once. It’s a *multiple* object (in Mol’s [2002] sense), defined by different practices in different contexts which give potentially discordant accounts. The economy becomes a sociotechnical object, but we should resist the temptation to equate objectivity with coherence, or unanimity (see also Star and Griesmer 1989). Mitchell (2002) and others are correct to signal an important change in the early 20th century, but overly dramatize that transformation by downplaying or ignoring the importance of the formation of the

economic sphere in the 18th-19th century which laid the groundwork for the formation of the economy in the 20th.

How would we know this argument is right? First, I'm going to show that there were a host of terms that meant something like the totality of economic stuff in use by the 1920—well before Keynes, but also a bit before national income statistics take off. Economists and the public *did* talk about economic issues, and even macroeconomic ones (especially monetary issues and inflation, but also unemployment and business cycles and industrial depressions). They did lump these together as a discrete field of political economy or economics (as shown in textbook data), but they did not lump them together as the analysis of a singular object called “the economy.” Additionally, I'll demonstrate that discussions of economic growth were largely absent (in accordance with Mitchell's (2011) claims). In fact, economic discussions had very few statistics to go on (even less than were available, because they lacked a framework for putting it all together). And they especially lacked *timely* statistics. The availability of timely data about the size and performance of the economy will be how I characterize the importance of the formation of the economy in the 1930s. Chapter 3 will look at the emergence of official national income statistics and how they came to be understood as the timely measures of the size of the economy. Chapter 4, in turn, focuses on the consequence of the emergence of “the economy,” and especially its timely measurement, for governments and how they are evaluated.

2.5 Studying Economic Discourse

Previous studies of the emergence of the economy have generally relied on hermeneutic or qualitative analysis of canonical texts. A few exceptions analyze a single data source systematically — generally a single news publication, including Emmison's (1983) analysis of *The Economist* or Suttles' (2011) analysis of the front page of the *New York Times*. This relatively informal approach to the analysis of economic discourse may have contributed

to the conflicting narratives both across and within the two camps discussed above. Here, I endeavor to take a more comprehensive approach by drawing on newly available sources of discursive data, as well as a more traditional deep reading of a sample of prominent economics textbooks. I focus my analysis on the period 1890-1960. This period begins well after the end of the process described by Schabas (the emergence of political economy and the economic as a conceptual space), but before the transformation described by Mitchell (the birth of macroeconomics as a subfield and the institutionalization of macroeconomic statistics), and ends after that transformation should have completed. Analyzing this period in depth should enable us to better understand the formation of the economy as a sociotechnical object.

For the first part of my analysis, I chart the usage of 12 terms related to macroeconomics and the emergence of the economy in three large datasets: Google Ngrams, the New York Times Chronicle, and JStor's Data for Research. These three sources sample different kinds of media: a sample of all books published in English, the entire corpus of the New York Times, and the entire corpus of academic journal articles archived in JStor. In general, trends visible in one data source are repeated in the others. These data show that by 1920, economic discourse included a variety of aggregate objects of analysis including unemployment, industrial depressions, and business cycles. I supplement these aggregate discussions with a few exemplars of influential statistical reports that analyzed the relevant phenomenon in detail.

This analysis relies on simple counts and relative frequencies of nGrams rather than more sophisticated quantitative or qualitative methods (e.g. topic models, formal coding procedures). This analytical choice is inspired by Lee and Martin's (2015) discussion of the importance of presenting textual data in as transparent a mode as possible. Simple, transparent methods "condense information to facilitate an intersubjectively valid interpretation" (Lee and Martin 2015: 1). Perhaps the simplest possible method is counts of words and phrases, i.e. ngrams. These counts condense large amounts of information, but make

very few interpretive assumptions. In so doing, analysis of nGrams foregrounds the interpretive act of the scholar, and equips critical readers with sufficient information to evaluate the interpretive claims made, and to offer alternative readings. In contrast to sophisticated qualitative coding, which “*disguises* the interpretation and moves it backstage”, a transparent, count based-method “*delays* the interpretation, and then presents the reader with the *same* data on which to make an interpretation that the researcher herself uses” (Lee and Martin 2015: 24, see also Biernacki 2012).

In the second part of my analysis, I examine a sample of introductory economics textbooks published between 1890 and 1960. Following the model of Levy and Peart (2011), I first downloaded bibliographic information on every book in the Library of Congress catalog with the classification HB 171.5, which includes primarily introductory economics textbooks along with a few related works (problem sets, companion texts, etc.). This initial pass yielded 696 candidate texts. Circulation data are difficult to acquire for this period, so in order to select texts that achieved at least some readership, I restricted my sample to books with multiple editions.⁴ I also excluded texts from this list that were not introductory textbooks intended for first year college courses (primarily excluding texts intended for high schools). This reduced sample included 42 texts.

Comparing this list with texts mentioned in the small literature on pre-World War II economics textbooks (e.g., Walstad et al 1998, Colander 2010) provides some face validity to the sample: most of the textbooks cited as influential were captured (Seligman, Ely, Fairchild, Garver), along with many other less well known texts. I attempted to acquire the first edition of each textbooks, though in some cases was only able to acquire a later edition. The final analytic sample includes books from every decade and multiple positions within the field (institutionalist, marginalist, etc.).⁵ I will return to this sample in Chapters 3

⁴Note that in some cases, books changed names between editions. To be conservative, I included any book whose author published multiple books in the sample even if they were not explicitly labeled as second editions.

⁵At least two prominent institutionalist texts (Slichter’s *Modern Economic Society* and Tugwell, Munro, and Stryker’s *American Economic Life and the Means of its Improvement*) seem not to have been categorized with the bulk of introductory texts, possibly inducing a bias against that perspective. Other institutionalist

and 4 to substantiate arguments about the historical trajectory of national income statistics, and the rise of a new understanding of economic governance. Here, I focus on discussions of “economic life” and “the economy”, along with brief discussions of business cycles and economic growth as emergent topics in this period.

2.6 Economic Discourse 1890-1960

In this section, I examine trends in the usage of 12 important terms in the period 1890-1960. These terms were identified through reading both primary and secondary sources on economic discourse in this period. My emphasis is on establishing how actors talked about large scale economic aggregates before and after the 1930s. I thus selected terms that either capture the economic sphere as a whole (“American economy”, “economic life”, “economic system”, “money economy”), some large dynamic within that sphere (“industrial depression”, “business cycle”, “commercial crisis”, “financial panic”), or some variable that would later become identified as part of the core of macroeconomics (“inflation”, “cost of living”, “unemployment”, and “economic growth”).

Together, the data presented here establish three important claims. Figure 2.1 presents trend data on the use of 12 terms in JStor’s database of academic research articles from 1890-1960. First, discussions of the economy as such rise dramatically in the 1930s and especially 1940s. This confirms previous research by Emmison (1983), Suttles (2011), and the intuitions of Mitchell (2002) and others. Second, in contrast with the strongest version of Mitchell’s argument, discussions of other large scale aggregate phenomena seem well-established by 1920, including unemployment, industrial depressions, business cycles, and inflation. The discussion of “commercial crisis” dates back even further (as seen in Figure 2.2, showing Google Ngram data back to 1800). Third, in agreement with Mitchell (2011), I find that discussions of economic growth only emerge post-World War II. The

texts were included, however (e.g. Edie’s *Principles of the New Economics*, see Rutherford 2011: 42-43 for a discussion of institutionalist textbooks).

dramatic rise in the use of the term itself is suggestive, but requires supplementary analysis of the textbook data to show just how absent economic growth was from the theoretical agenda of economists up through the 1940s (for a similar argument about the absence of economic growth from the literature at this time, see Warsh 2006).

2.6.1 The Economy

Almost no one talked about something called the “American economy” until the late 1930s, and widespread discussion did not take place until the 1940s. As seen in Figure 2.1, academic use of the term begins around the mid-1930s. Similarly, Figure 2.2 shows the dramatic takeoff in usages in the Google Books sample in the 1930s. I trace the term “American economy” here because “the economy” captures many older usages, e.g. “the economy of steam engines” referring to their relative mechanical efficiency. Nonetheless, trends in that phrase follow a similar pattern (if not quite as stark). This timeline accords very nicely with the story laid out by Timothy Mitchell. And yet, that story downplays the extent of macroeconomic-like discussions taking place before the 1930s, as I will document more below.

One very early use of the term occurs in *Recent Economic Changes*, a 1929 report published by the National Bureau of Economic Research on behalf of the President’s Conference on Unemployment chaired by Herbert Hoover.⁶ In the foreword to the report, the authors frame their task in terms of understanding the economy as a whole:

The committee was directed to make a critical appraisal of the factors of stability and instability; in other words, to observe and to describe the American economy as a whole, suggesting rather than developing recommendations. (NBER 1929: v)

The terminology used throughout the report is inconsistent; “American economy” does not reappear again, although related phrases appear a handful of times.

⁶For detailed accounts of Hoover’s role in promoting economic statistics as Secretary of Commerce, see Alchon 1985, Barber 1985.

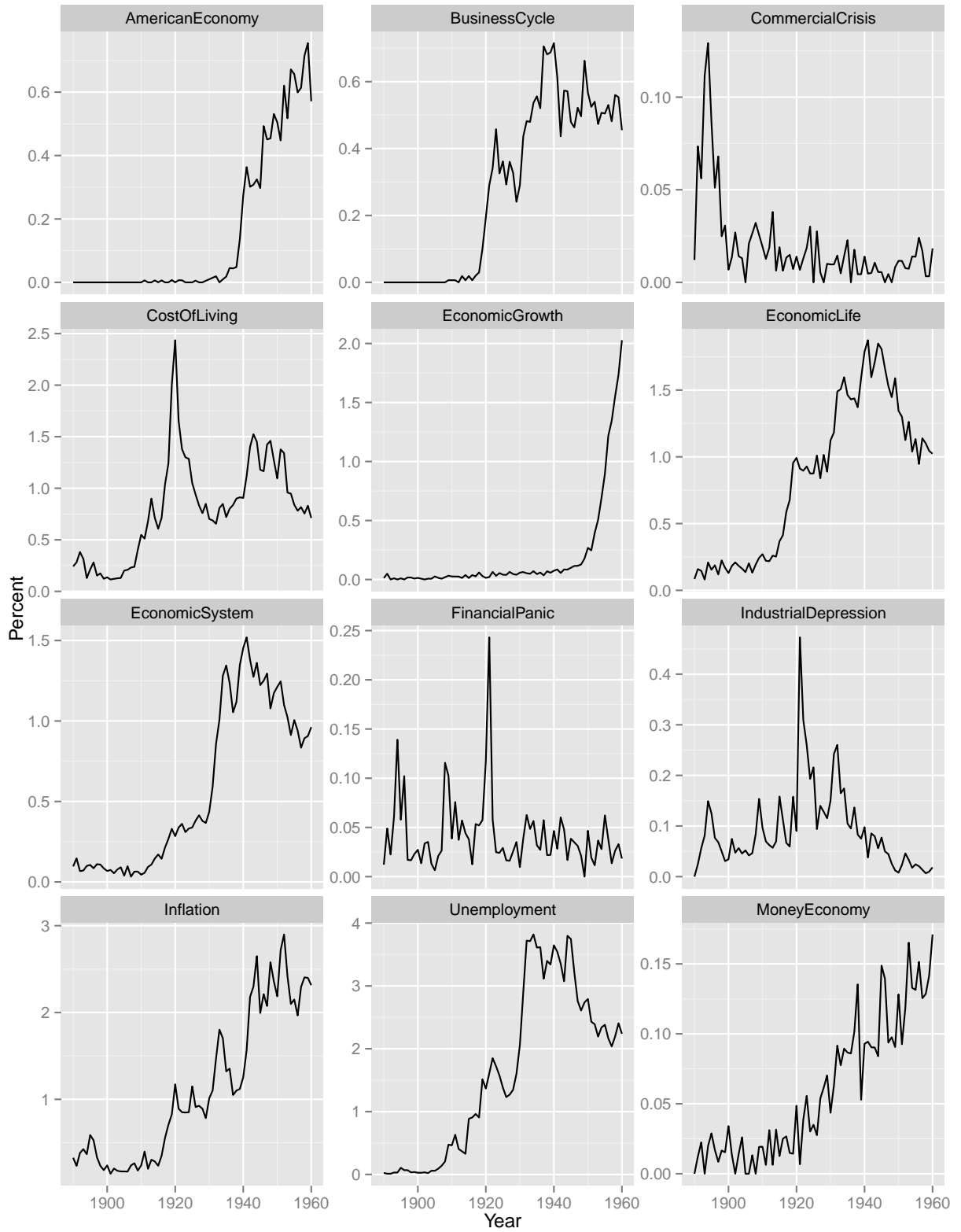


Figure 2.1: The relative frequency of 12 phrases, as a percentage of research articles in JSTor 1890-1960. Note that the vertical axes vary; the intent of the figure is to show relative, not absolute, trends.

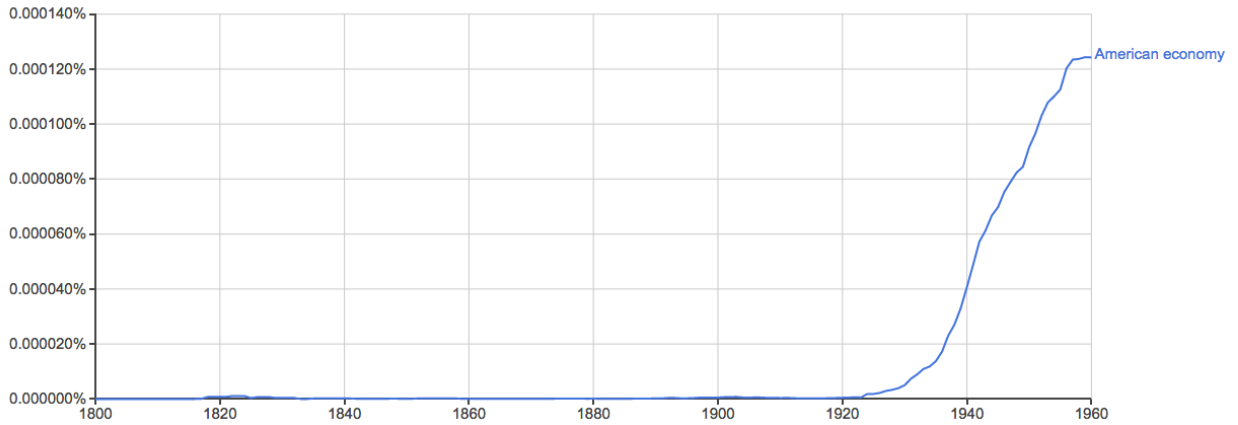


Figure 2.2: The relative frequency of the phrase “American Economy” in English-language books, 1800-1960.

By the end of the 1930s, terminological confusion about the economy blossomed. Nowhere is this heightened level of discourse about the “American economy” clearer than in a 1939 New Deal report titled *The Structure of the American Economy*. Published by the National Resources Committee of the Department of the Interior, *The Structure of the American Economy* reports on everything from consumer demand to the geography of agricultural production. Interestingly, the document opens with a definition of the economy, suggesting the authors knew the term to be of relatively recent vintage:

The American economy is the organized activity through which the 130 million people in this country obtain their daily living. Farmers raising food and fiber, miners extracting ore and coal, industrial workers fabricating raw materials into finished products, whole-sale and retail distributors making goods available to consumers, and a host of workers performing the other countless tasks required by modern living, all of these are combined in a huge and highly complex producing organization which constitutes the national economy. (NRC 1939: 1)

Of note, this report opens with a discussion of national income as its first statistical measure of the economy, in a chart titled “Loss in potential real national income due to depression unemployment of men and machines, 1930-1937.” (NRC 1939: 2) Here we see a fully-fledged macroeconomic understanding of the relationship between aggregate output measured as national income and resource utilization measured by unemployment, similar

to more recent discussions of “potential GNP” (Okun 1962).

2.6.2 Macro Before Macro

Discussions of large-scale aggregate economic phenomena did not begin in the 1930s, however. Already by 1920, we see in Figure 2.1 discussion of unemployment, inflation, business cycles, and industrial depressions, to name a few. How do we make sense of these discussions taking place in the decades before “the economy” and the birth of macroeconomics?

The term “money economy” plays an especially interesting role here. In contrast to McVety’s (2012) claim that the economy shifts from being an adjective to a noun following the rise of macroeconomics, we see that already by the 1910s and 1920s, “economy” served (at least occasionally) as a noun describing an aggregate economic system. For example, Wesley Mitchell (1913: 21) frequently referenced the term “money economy” to contrast with an older “barter economy.” And even earlier, Veblen (1904: 150-151) contrasted “money economy” with “credit economy”, with credit economy being an advanced form of money economy containing a well-developed capital market. Both terms are translations from German phrases with the root “wirtschaft.”⁷ Similar examples can be found from these decades of uses of “world economy”, and even “national economy” (usually as a translation of the German phrase *Nationalökonomie*, see Tooze 1998). Reading these discussions carefully, as I will further document in the textbook analysis below, shows that “economy” already served as a synonym for the whole economic system or economic life among some authors a couple decades before the 1930s. What it lacked was a size—in this period, no author that I can find makes references to the “size of the economy” or “size of the money economy” or any variation. Economic life was a distinct area of life, economic action was a distinct form of action, but the economic did not yet have a well-defined or

⁷Shenk (2015), drawing on Mitchell (1949:546), dates the first German usage of money economy and credit economy to an essay by Bruno Hildebrand published in 1864, “Naturalwirtschaft, Geldwirtschaft, und Creditwirtschaft.”



Figure 2.3: The relative frequency of the phrase “commercial crisis” in English-language books, 1800-1960.

measured size.

Other aggregate phenomena were measured by 1920, however. In particular, there was a large body of research on periodic crises and business cycles that dates back to the 19th century but crescendoed in the early 20th (Morgan 1990). Famously, in 1878, William Stanley Jevons (a British scholar best known as one of the founders of marginalist economics) identified commercial crises occurring approximately every 10 to 11 years. Jevons argued that this periodicity matched the recently identified cycle of sun spots, and argued that variations in solar output might explain the cycle of ups and downs in overall commerce. Figure 2.3 shows that discussions of commercial crises were not uncommon in the mid-19th century.

By the early 20th century, “commercial crisis” were joined by discussions of “industrial depressions”, “business cycles”, and “financial panics.” In 1886, the newly-formed Bureau of Labor Statistics in the United States published a 500 page document entitled “Industrial Depressions” as its first annual report. Industrial depressions were distinguished from, but considered related to, commercial crises and financial panics, which were understood as potential causes of a depression:

A panic or a crisis is usually short, sharp, and decisive in its results. A depression is a condition which has duration of time attending it. Panics and

crises may occur without a resulting industrial depression, as has been the case many times, and an industrial depression of much severity may occur without producing a financial or commercial crisis or panic, although financial conditions are always more or less disturbed during the continuance of an industrial depression. The terms crises, panics, and depressions are used under these distinctions. (Labor 1886: 15)

So here we can see some of the terminology already in use by 1886 for understanding large scale economic movements. As Lepler (2013) notes, this vocabulary was largely absent during the Panic of 1837. This report suggests that the industrial depressions of the 1830s to 1880s were a new phenomenon (Labor 1886: 12-13).⁸ We see then that between the 1830s (post-Ricardo) and the 1890s (pre-Keynes), a rich vocabulary emerges for describing and measuring the novel phenomena of industrial depressions.

In 1913, Wesley Mitchell published an influential volume on “business cycles” which endeavored to unite studies of financial panics and industrial depressions under one systematic framework. Mitchell begins his theory building with the concept of “money economy,” which is defined not simply as “the use of money as a medium of exchange ; but the fact that economic activity takes the form of making and spending money incomes.” (Mitchell 1913: 21) Several points are worth noting here. First, Mitchell clearly has a well-developed sense of what constitutes “economic activity”, and assumes the reader does as well. Economic activity is not itself a problematic or new category by the early 20th century. On the other hand, “money economy” is introduced as a relatively new concept, and one likely to be misunderstood. Mitchell sees money economy as important to understanding the workings of the business cycle because the money economy structures the behavior of business enterprises, but fails to sufficiently coordinate between businesses: “The money economy provides for effective coordination of effort within each business enterprise, but not for effective coordination of effort among independent enterprises.” (Mitchell 1913: 38)

Although the term “money economy” did not enter into as widespread (or sustained) use as did “business cycle”, it’s important to note how well-developed discussions of the

⁸Mitchell (1913: 584) places the birth of the business cycle a bit earlier, at the end of the 18th century.

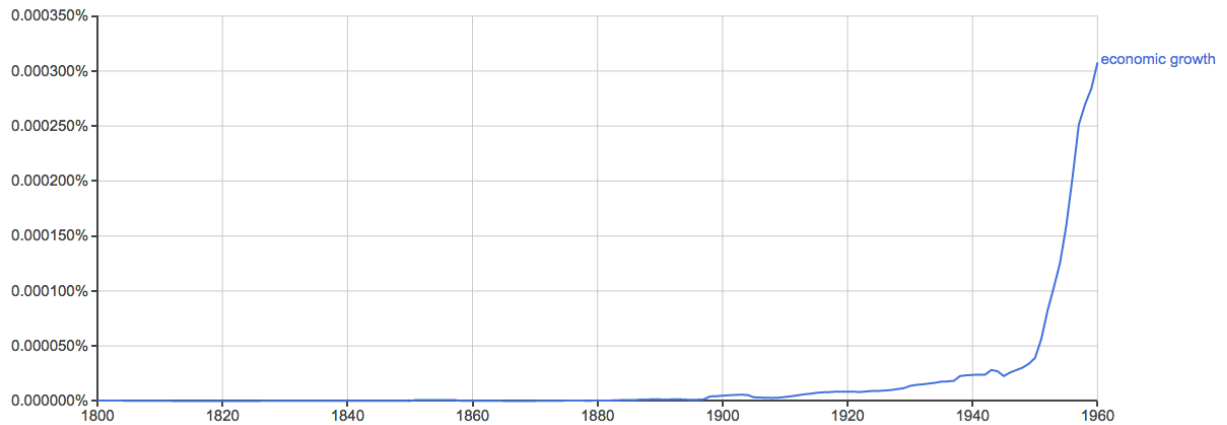


Figure 2.4: The relative frequency of the phrase “economic growth” in English-language books, 1800-1960.

aggregate economic system were by 1913.

2.6.3 The Growth of Growth

Third, and finally, Figure 2.1 and Figure 2.4 document the absence of explicit discussions of economic growth. Without a *size* by which to measure aggregate economic activity, scholars writing before World War II had no easy way to describe overall increases in prosperity. Certainly, the topic was not new—Adam Smith’s *Wealth of Nations* discusses differences in prosperity between various European nations back in 1776. But these discussions were rarely cast in quantitative terms, and were not related to the overall size of some economic pie. In the post-war years, we can see the dramatic takeoff of those explicit discussions of economic growth. Figure 2.4 shows that nicely in the Google Books sample. To fully understand how scholars understood economic well-being and its measurement in the period before comparable national income statistics were available, I turn now to the textbook sample.

2.6.4 Textbook Economics, 1890-1960

The above analysis of nGrams painted a broad picture of economic discourse. In the section, I fill in that picture with details from introductory economics textbooks. I draw on these sources to confirm the previous analysis, to add context to particular discussions, and to point towards moments of transformation. Following my strategy for analyzing the textbooks, I focus here on discussion of the economic system as a whole and the changing meaning of national income.

First, in line with the widespread usage of terms like “economic life” in the 1890s-1930s, we see in the textbook sample a rich vocabulary for describing the economic sphere as a whole. The terms vary, but the most prominent include economic life, economic system, and economic order. For example, Ely (1901: 81) discusses the relationship between economics and other social sciences by breaking apart their subjects into eight domains:

Man has been busy from the first in several lines of effort. He has talked, worshiped, fought, studied, etc., and each of these lines of effort has developed its own faculties and institutions. For convenience we may arrange these in eight groups, as follows: language, art, education, religion, family life, society life, political life, economic life. Each of these is the subject of a science of a science more or less developed.

Ely’s distinctions here are very similar to the classic Weberian discussion of spheres discussed above. This similarity is, in some ways, not surprising: like many early 20th century economists, Ely studied economics in Germany. Similarly, Seligman (1905: 6) contrasted “the economic” with “the ethical, the legal, the political,” all of which were seen as subsets of the social. And Blackmar (1912: 17) distinguishes “social, individual, intellectual, moral, religious, political, or economic life.”

These early texts also invoked the term “economy” to characterize the economic system as a whole. The precise taxonomy varied, depending on the aspects of the economic system that authors wished to emphasize. For example, Seligman (1905: 67) (similar to Veblen and Mitchell, discussed above) divided economic history into “three periods of barter, money

and credit economy.” Later in the same text, Seligman offers a different set of “economies”:

If we regard economic conditions from the standpoint of the relations of production to consumption — for these are the fundamental economic facts — we may divide the world’s history into three great stages, known respectively as the self-sufficing economy, the trade or commercial economy and the capitalist or industrial economy. From another point of view these may be called isolated economy, the local or village economy and the national economy.

Here we see national economy as a synonym for industrial economy, with both terms being used to describe a system in which relationships of production and consumption were integrated at a national scale, in contrast to earlier systems. This usage was a bit atypical, but discussions of barter and money economies were quite common in the pre-World War I period (see, for example, Johnson 1909: 14, Seager 1910: 7) and persist into the interwar period (e.g. Brown 1929: 145, Deibler 1929: 260).

In the post-war era, the usage of “the economy” becomes the familiar one, no longer just characterizing broad economic systems, but also describing a somewhat timeless object with a size and particular dynamics. Although textbook authors do not remark much upon the novelty of their usage, several offer definitions of “the economy.” For example, Burns et al. (1948: 3) write, “The economy of a country is a term referring to the organization of the countrys resources for the production of commodities and services. Thus, the original term and its modern derivatives all stress the underlying facts of scarcity and the consequent need to conserve and manage resources.” Umbreit et al (1952: 16) offer a similar definition: “By the economy we mean the whole system of economic institutions by means of which society provides for the production and distribution of economic goods.” And Murad (1953: 3) usefully distinguishes economy as system from economy as efficiency:

We say that we practice economy when we are being thrifty and sparing in the use of our resources. When we speak of an economy (for instance, the American economy, the national economy), we mean the orderly arrangement and management of production, exchange, and consumption in a household, business establishment, community, or nation.

Beyond offering explicit definitions, these later texts also directly link national income

and the economy. As noted above, the “barter economy” and “money economy” never had a *size* in the the 1890-1930 period, and there was no particular relationship between national income and the economy. Texts in the 1940s and 1950s began to describe the economy explicitly in terms of national income. Before the term macroeconomics came into widespread use, textbook authors outlined a “national income approach,” (Burns et al 1948: xv) a term used sufficiently widely to be incorporated into a survey of introductory economics professors undertaken by the American Economic Review (reported in Taylor 1950, a book-length survey of undergraduate economics education). The most famous textbook associated with this approach was Samuelson’s (1948) *Economics*, but it was not alone in emphasizing national income (see, e.g. Burns et al 1948, Hicks 1960). Most importantly for our purposes, these texts began to speak of the size and growth of the economy measured in terms of national income statistics. For example, Burns et al (1948: 85) equate the national output (a variant of national income, what we’d now call national product) and the performance of the economy in their discussion of the causes of variation in economic performance:

Population, geographic and technological factors, the sociological environment, and especially the attitudes or outlook of the people, the types of incentives, and the role of governmental policy — all influence the performance of the economy. This performance is the national output.

Samuelson (1948: 225) neatly summarized the most important post-war uses of national income statistics in economics: “By means of statistics of national income, we can chart the movements of a country from depression to prosperity, its steady long-term rate of economic growth and development, and finally its material standard of living in comparison with other nations.” These post-WWII uses of national income as measures of the performance of the economy as a whole contrast sharply with discussions of national income in the preceding decades.

National income measures, and quantitative statistics more broadly, are largely absent from the earliest textbooks in the sample. This absence is not surprising given the relative

paucity of statistics produced by the federal government or prominent private actors up through the end of the 19th century. Even quantitatively-minded economists had to resort to guesswork and hand-waving if they wanted to quote a measure of national income, as we can see in Irving Fisher's (1916: 99) resignation: "Unfortunately, there are no available statistics for income in the United States. We can only guess as to what the amount of it may be."

In the 1920s, we begin to see more detailed discussions of national income statistics (along with other indicators and measures), but the context of those discussions remains confined to the problem of *distribution*. That is, national income statistics were seen as useful ways to discuss the distribution of income between land, labor, and capital, a problem of great theoretical and practical concern in 19th and early 20th century economics (e.g. Lutz and Stanton 1923: 292, Edie 1922: 175-178).⁹ This emphasis on national income as something to be distributed persists into the 1940s. For example, Froman (1946: 468) cites national income data to chart the dramatic increase in the share of income going to employees (from 56% in 1910 up to 72% in 1945).

Discussions of national income in the 1920s and 1930s are notably divorced from discussions of *business cycles*. To the extent that authors discussed national income—and many did not—they did so in chapters on distribution. In contrast, business cycle discussions largely followed from discussions of the nature of money and the modern banking system. Echoing Wesley Mitchell's (1913) argument that the business cycle emerges with the "money economy", authors in the interwar period discussed the importance of bank credit in determining the ups and downs of the business cycle. To the extent that they offered measurements of the cycle — and not all did — they chose other indicators, such as the Federal Reserve Board's Index of Industrial Production (e.g. Deibler 1929, Cronin 1939, Gemmill 1939).

Finally, in line with the discussion above of the absence of the term "economic growth",

⁹Some authors also drew on the publications of the National Bureau of Economic Research to discuss the *personal* distribution of income (e.g. Edie 1922: 186), a topic I return to at length in chapter 6.

we see almost no discussion of growth as a *quantitative* phenomenon up through the immediate post-WWII period. Rather, we see routine discussions of economic progress through the lens of historical transformations in the economic order. Most often, this takes the form of the linear development of modes of production in Western Europe (especially England) and the US, tracing the history from the manorial system in the Middle Ages, through the rise of guilds and towns, to the modern factory system. We already saw hints of this framing in the common discussions of the transition from “barter economy” to “money economy” found in the 1900s-1920s textbooks. The relative absence of economic growth discussions persists into the 1940s, however. Even after national income approach comes to dominate, textbooks in the 1940s and 1950s emphasized the *stability* of national income as a primary concern, not its growth.

This concern shows through most clearly in debates over economic policymaking. Before the 1920s, economic policymaking discussions largely consist of unconditional claims about the value of tariffs, the importance of maintaining the rule of law, or the dangers of monopoly. That is, good economic policies are seen as good no matter what is happening in the economic system that particular day. As discussions of the business cycle increase in the 1920s and 1930s, economic policymaking turns to focus on conditional policymaking: what should the government do, *given* the particular moment in the business cycle? That is, economists come to argue that good policy in a depression might be bad policy in a boom, and vice versa (e.g. Edie 1922: 431, Brown 1923: 129, Fairchild et al 1926: 501, Deibler 1929: 355). This concern with countercyclical policymaking carries over into the 1940s and 1950s, with national income (as the best measure of the economy’s performance) replacing the business cycle as the object to be stabilized. For example, Mussey and Donnan (1942: 665) write, “Our central concern is the instability of employment and of real income which results from unstable production; our ultimate question is How can productive operations be so stabilized as to give more continuous employment and a more regular national income?” Similarly, Froman (1946: 751) worries about *maintaining* a high level of income

post-WWII: “We are hoping to maintain a national income of some \$150,000,000,000 a year, which is nearly as high as the national income figure for the peak war years and about double the average of the 1930’s.”

This emphasis on stabilization is not surprising in light of the recent experience of the Great Depression, and the pervasive worries that military demobilization would bring about another major downturn, but it contrasts sharply with the interest in growth that would emerge a bit later (Collins 2000, Yarrow 2010). For our purposes, however, the most important transition occurs by the 1940s: the government is understood as acting on “the economy” in aggregate, with national income as the preferred aggregate measure of the performance of that economy. As Nordin and Salera (1950: 17-18) put it, “the government is expected to act on the economy as a whole rather than on individual businesses.” I return to this argument in chapter 4 to analyze the emergence of this new political rationality of governmental intervention in economic affairs.

2.7 Discussion and Conclusion

What can we learn from this analysis of the history of economic discourse? First, we can resolve the debate in the history of economics on the origins of the economy. “Economic life” was recognized as a separate sphere of social life long before “the economy” crystallized into a sociotechnical object. And yet, that crystallization in the 1930s-1940s marks an important transition in economic measurement, and economic government. Governments in the 19th century did not act on “the economy as a whole”, nor did they measure the size of that economy in terms of national income (or anything else).

Nonetheless, recognizing the long continuity of economic thought is important for dispelling some of the more overblown claims about the importance of the 1930s. Perhaps most important, we can refine Timothy Mitchell’s account. Mitchell (1998: 92) argued that “To create the economy meant also to create the non-economic.” (Mitchell 1998: 92)

This is wrong, or more accurately, it's right but off by a century or two. The non-economic was defined well-before "the economy" became a technical object, and included variously nature, politics, culture, art, religion, etc.

Making the economy a technical object did require refining those distinctions for some practical purposes. It was no longer simply a matter of asserting that the political was separate from the economic, but of practically accounting for the economic presence of the state and somehow creating aggregate measures of economic performance that simultaneously recognized the importance of the state while separating off the economy as an object. I turn to these problems in chapter 5, in the discussion of how national income statisticians reconciled hard to count objects, especially those in the household (housework) and in the political sphere (government). Of course, the precise demarcation of economy, politics, nature, and so on required of national income statisticians did not clarify the usage of those terms in other contexts; the economy remains an object multiple, as analysis of the blurred boundaries surrounding the state's economic activities has shown (Maryl and Quinn 2015).

My account also provides a better periodization that clarifies continuities and disjunctions in economic thought. For example, this history highlights the slow build of precursor concepts to macroeconomics. Business cycles are identified in the 19th century, and business cycle analysis increased in prominence and sophistication in the 1910s-1930s. Similarly, analysis of the price level (above all, Irving Fisher's work) picked up in this period. Acknowledging these precursors to modern macroeconomics helps to identify precisely what changed in the 1930s: the economy comes to have a *size* and that size is knowable, measurable in a timely fashion.

The relative lack of interest in national income statistics in mainstream economics textbooks from the 1920s and 1930s clarifies our explanatory burden: We must ask not only how the economy became an object with a size, but why national income in particular (and measured in one particular way) won out as the measure of that size. In the 1920s, national income did not hold a privileged place in economic analysis. By the end of World War II,

it did. What changed in between? How did national income come to be the measure of the economy? I tackle this question in the following chapter.

2.8 References

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2.9 Appendix: Introductory Textbook Sample

This chapter draws on a sample of 42 introductory economics textbooks published between 1890 and 1960. These textbooks were sampled following a procedure inspired by Levy and Peart (2011), as described above. The complete list of textbooks is presented here, after a brief outline of the analysis scheme used to guide my reading.

2.9.1 Analytical Scheme

Mindful of the critiques of qualitative coding laid out by Biernacki (2012, see also Lee and Martin 2015), my approach to reading the textbook sample was thematic and holistic. The goal was to capture how broad themes were treated, not to label and count every instance of particular phenomena. I focused my attention on four broad themes: definitions, data, government, and topics we would now consider to be macroeconomics. The list of themes and subthemes is presented here:

1. Definitions.
 - A. Of the field of study (economics, political economy)
 - B. Of the object of analysis (wealth of nations, trade, commerce, prosperity, “the economy”, etc.).
2. Data.
 - A. What quantitative statistics are presented, if any?
 - B. What time period are those statistics covering? How frequent are the intervals?
 - C. Who produced those statistics? (individual scholars, government, private entities, etc.)
 - D. What is their object of analysis (trade cycles, specific industries, inflation, etc.)?
3. Role of the government.
 - A. What kinds of actions are possible?
 - B. How does the government know when to do what? What data does it look at?
 - C. What interventions are conditional on some state of the world rather than always a good idea? Are any of these conditional on timing?
4. Specific macro topics.
 - A. Business & trade cycles. Fluctuations. Etc.
 - B. Monetary theory.
 - C. Growth & long-run prosperity.

2.9.2 Textbook Sample

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CHAPTER 3

How the Economy Got its Size

3.1 Introduction

Looking back from our current vantage point, it's difficult to imagine just how unimportant national income statistics (NIS) were before the 1930s. We swim in an economic and political discourse that takes for granted both the existence of "the economy" as an object, and the existence of comprehensive measures like Gross Domestic Product that measure the economy's size. Before the 1930s, however, NIS were almost entirely absent from our discourse. NIS were not measured by governments, and only infrequently measured by private actors. Economists did not theorize national income and its movements. Economics textbooks did not have chapters devoted to national income, nor did they identify NIS as an important area of research. To the extent that economists of the 1910s and 1920s attempted to characterize movements of the entire economic system, they turned to a heterogeneous collection of indicators that they hoped would predict the rise and fall of the business cycle. By the 1940s, NIS were ubiquitous, and they had come to be understood as measures of prosperity and power, an essential tool of governance and the ultimate dependent variable of the new macroeconomic theory. Moreover, NIS had become associated with a new object, "the economy", whose size they measured.

In the previous chapter, I showed that "the economy" emerged as an object of discourse in the 1930s-1940s, augmenting and crystallizing previous notions of "economic life" as a separate conceptual space from politics and nature. One of the key distinctions between

the economy and fuzzier notions of economic life is that the economy has a size. You can't ask how big economic life (or political life, or nature) is and get a reasonable answer; on the other hand, the economy's size is readily knowable and widely discussed. In particular, national income statistics define the size of the economy and in so doing enact the economy as the kind of object that has a definite size.

In this chapter, I show how the economy got its size. I attempt to address the following related questions: Why did we start routinely measuring national income when we did? Why did national income become the size of the economy and not some other measure? Why did we fixate on an understanding of the economy as an object with a unitary measure of size? What alternatives were available?

To answer these questions, I make forays into the history of economics, economic history, the history of the state, and military history. I focus on the work of national income statisticians, and their interactions with businesses, politics, governance, and the new field of macroeconomics. Following national income statisticians helps to explain how it became possible in the 1930s to produce timely measurements of relevant aggregate statistics, which in turn made NIS practically useful for managing war mobilization, politically useful for appraising the success of the New Deal and other programs, and theoretically useful for characterizing different states of the economy. In the following chapter, I extend this analysis by focusing specifically on how the availability of timely NIS transformed the capacities and responsibilities of the state. Here, I focus on the process by which these statistics came to be routine productions of the state, and how their initial successes cemented their position as both iconic indicators of economic well-being, and practical tools of statecraft.

To understand the timing of the emergence of national income statistics, I briefly chart the growing *calculability* of the economic system in the late 19th and early 20th century. Following a similar roadmap to Tooze (2001), I argue that a combination of technological innovations, changes in the organization of production, and changes in the organization of

the state made possible the routine production of aggregate economic statistics. Understanding why NIS were possible in the 1930s and not in the 1830s is a necessary but insufficient first step towards understanding how they came to measure the size of the economy. I document why the ultimate success of NIS was somewhat surprising from the vantage point of the 1910s-1920s. As noted above, NIS were not yet important parts of economic theory, political discourse, or business practice. I focus on the United States to document how the official production of NIS emerged from the political debates of the Great Depression, but soon became a tool for managing the war economy of the 1940s. NIS proved useful for managing the economy, which in turn won out as the dominant paradigm for economic planning.

3.2 The Growing Calculability of Economic Life

The idea of calculating aggregate economic statistics for use by policymakers dates back to at least the 17th century, and the “political arithmetick” of William Petty. Despite repeated efforts by individual scholars, aggregate economic statistics made little headway in the 18th and 19th centuries.¹ Writing a century after Petty, Adam Smith (1776) famously dismissed the endeavor: “I have no great faith in political arithmetick.” Between Adam Smith’s *Wealth of Nations* and Paul Samuelson’s (1948) *Economics*, the field of aggregate economic statistics went from a backwater to an essential practice for economic theory and policymaking.

The emergence of aggregate economic statistics in the 20th century was not simply a matter of will. Recent attempts to reconstruct data series according to modern standards highlight the impossibility of calculating the economies of the early 19th century.

¹Hoppit (1996) offers a critical analysis of the historiography of political arithmetic and, drawing on Studenski (1958), notes 15 attempts to calculate England’s national income between 1667 and 1812, which Hoppit argues had at least some influence on policy debates of the time. Hoppit thus points to more of a gradual increase in interest rather than a sharp break. That said, timely, official, aggregate statistics are absent until the 20th century.

Economist Joseph Davis (2004:1178) highlights the difficulty of analyzing this period: “Quite frankly, constructing a trustworthy U.S. GDP series is simply impossible before the Civil War owing to the comparative deficiency of annual data on agriculture, merchant and wholesale trades, and service industries.” Similarly, Porter (1995: 36) writes of Napoleon’s difficulties collecting usable statistical information: “France was not yet capable of being reduced to statistics.” What changed between the 18th century and early 20th century that facilitated the calculation of economic aggregates?

Here, I follow the lead of historians Adam Tooze (2001) and Timothy Shenk (2015) in exploring changes in the calculability of the economic system. By calculability, I mean the susceptibility of a given arena of social life to quantification by outsiders. In this sense, changes in calculability result from changes in both the organization of economic life and in changes of the capacities of economists and statisticians and the bureaucracies in which they were embedded.

The period in question—roughly “the long 19th century”—saw many more changes than can reasonably be summarized here. Historian Jurgen Osterhammel (2014) characterizes the period as *The Transformation of the World* in his massive overview. I focus on four important transformations that all facilitated the production, collection, and analysis of economic data by organized groups of experts, and I will largely limit my analysis to the United States (though similar trends took place in much of Western Europe). The first two transformations, the rise of the large corporation and the increasing prevalence of wage labor, occurred in the organization of production. The second two transformations, the growth of administrative capacity and the collection of income taxes, concerned the role of the state as well as technological innovation.² For each, I will discuss the rough timing of the transformation and how it facilitated the production of timely aggregate economic statistics.

²For a similar analysis focused on Germany, see Tooze (2001: 17).

3.2.1 The Rise of the Large Corporation

The calculability of the economic system, from the perspective of government bureaucrats or private economists, is partly a function of how much data is already generated as a by-product of the functioning of that system. Put differently, it's much faster and cheaper to aggregate data that has already been aggregated. The late 19th century saw the emergence of the "modern corporation" (Berle and Means 1932, Chandler 1977, Lamoreaux et al 2003), which was large, professionally managed, often owned by distributed shareholders, and most notably, centrally administered. Large corporations employed accountants to track the flow of money inside the firm, and between the firm and its suppliers and customers. For firms owned by shareholders, these accounts were even made public through annual reports.

For statisticians, the large corporation was a tremendous boon. If an industry was organized entirely into large firms, and if those firms could be convinced or coerced into releasing their data, statisticians would have access to everything needed to produce comprehensive aggregates. In the construction of the first official US national income estimates, internal correspondence shows how much statisticians relied on the records of large firms to make sense of certain industries, and how much easier it made their jobs when a small number of firms controlled a large market. For example, an internal letter noted that the radio industry could be easily measured with the cooperation of a single firm, RCA, as "They control the entire situation here so that if we get reports from them, we will have accurate data."³ Few industries were as concentrated as radio in the 1930s, but to the extent that production had been increasingly centralized in a small number of accountable units, the task of creating aggregate statistics was made substantially easier.

³Burnstein, letter to Kuznets, March 9, 1933, National Archives, RG 151, Index 600, Box 2562, Folder: "Finance and Investment - United States - National Income - 1933 - January - June."

3.2.2 Wage Labor

The long 19th century saw dramatic changes in the location and structure of production. One major trend was urbanization (Osterhammel 2014). In the United States, about 95% of the population lived in rural areas in 1790; by 1920 that number was less than 50%.⁴ Along with this transition from rural to urban came the rise of new forms of employment. In the 19th century, most workers were either self-employed or slaves.⁵ By the early 20th century, working for wages or salary had become the dominant form of labor. Lebergott (1964), for example, finds that by 1900, about 55% of the labor force worked as hired labor, and that number increased rapidly in the following decades.⁶

The existence of wage labor facilitated the collection of data about economic activity. To the extent that workers were employed by large firms, statisticians could learn their numbers and wages directly from the firms. More generally, the more fully production was commodified, the easier it was to measure. Individuals who were self-employed were more difficult to track (especially those not covered by the income tax, discussed below). For example, statisticians in the Commerce Department invested significant time and resources into producing better estimates of the incomes of professionals (accountants, lawyers, dentists, etc.) who tended to be self-employed, but did not always earn enough to be captured in income tax statistics. Noted economist Milton Friedman gathered data for his dissertation working with Simon Kuznets to improve data collection on professionals, and the Commerce Department worked through professional associations (e.g. the American Bar Association) to try to survey every professional as to their income.⁷

⁴Data from the United States Census, see <http://www.census.gov/prod/cen2010/cph-2-1.pdf>, Table 13.

⁵Arguably, slave labor was the most calculable category, as the extensive records from slave sales and plantations have made clear. See, among others, Levy (2012) on the relationship between slavery, calculation, and capitalism in the 19th century.

⁶Margo (1992: 26) notes that “there are no reliable statistics on self-employment early in the nineteenth century.” We are reasonably certain that self-employment declined dramatically in the 19th century, but we cannot know (by modern standards) exactly how much. In a sense, this absence of data further supports the point that the early 19th century was much less calculable.

⁷See, for example, National Archives, RG 151, Index 600, Box 2563 Folder: “Finance and Investment - United States - National Income - 1934 - July - December.”

Relative to wage laborers, professionals were difficult to aggregate, but at least professional associations kept lists of members, and professionals themselves earned money incomes that could be reported on standardized surveys. In contrast, as I will discuss at length in chapter 5, national income statisticians struggled throughout the past century to deal with “nonmarket production,” including everything from the value of produce grown by farmers for their own consumption to the unpaid labor of housewives. Labor bought and sold in the market, in contrast, was theoretically easy to value, and practically easy to measure.

3.2.3 Administrative Capacity

The growth in administrative capacity, and the production of official statistics, began earlier in the 19th century. As Hacking (1990) noted, the period from 1820-1840 saw an “avalanche of printed numbers.” These early 19th century efforts produced a great mass of data, but they tended to do so at irregular intervals or with significant lags between data collection and publication, and with constant shuffling of categories. Tooze (1998: 216) summarizes:

The giant censuses of occupation carried out in most countries roughly every decade provided basic information on the changes to the social structure wrought by industrialisation and urbanisation. However, they had little value as guides to current economic conditions. . . Rough estimates of agricultural production were prepared for most countries on an annual basis; exceptionally well-organised and publicity-friendly industrial associations might publish annual reports as well. However, the kind of up-to-date statistical information which we take for granted today was almost entirely non-existent.

Official statistics proliferated throughout the 19th century, but they did not become useful for thinking about the current state of the economic system until the end of the century. In part, early 19th century efforts were limited by a lack of resources. For example, although the US Census has been conducted decennially since 1790, the Census Bureau was not funded as a permanent organization until 1902 (on the history of the US census, see

Anderson 1990). Similarly, the Bureau of Labor Statistics opened in 1884 (see Stapleford 2009 for details). More generally, the late 19th and early 20th centuries saw a dramatic increase in the size and professionalization of state and federal bureaucracies (Skowronek 1982, Tolbert and Zucker 1983). Thus, at the same time the rise of large corporations and the shift to wage labor made the organization of production easier to calculate, dramatic changes in the administrative capacities of the state provided new resources with which to calculate.

These resources included increased funding, organizational changes, and new technologies. The most iconic technological change in the late 19th century was the emergence of Hollerith machines for automated processing of punch cards starting with the 1890 Census. In the 1930s, when economists at the Department of Commerce were tabulating the first national income estimates, they relied on punch card processing machines borrowed from the Census Bureau to perform specific calculations with industry-level data.⁸ The Hollerith machine may be the best remembered change in administrative data processing capacity in the late 19th century, however, as Tooze (2001: 25-27) notes, punch card processing relied on an arguably more important organizational innovation in the division of human labor. In particular, the Census Bureau and other administrative agencies began to receive reports of raw data from enumerators, rather than relying on local level offices to aggregate data. Having centralized the raw data, the Bureau could then process it more efficiently, and more flexibly.

3.2.4 Mass Income Taxes

In addition to increases in the state's capacity to collect and process data, the late 19th and early 20th century also saw the rise of new forms of taxation which in turn generated data for more accurate, timely economic statistics. In particular, income taxes provided

⁸Letter from Feiker to Bureau of the Census, February 24, 1933. Nation Archives RG 151 Index 600 Box 2562, Folder "Finance and Investment - United States - National Income - 1933 - January - June."

both a motivation for measuring income (to predict tax revenues) and, as a by-product, the necessary data. Prior to the routine collection of income taxes, state knowledge of individual and corporate income was spotty at best. As Tooze (2001:21) summarizes, “Tax records were the fundamental source for most of the early estimates of national income. As the share of national income going through the state coffers increased, it became easier to assemble the data from which to compile a national income estimate.”

In the US, the creation of the income tax in 1913 provided the data needed for National Bureau of Economic Research’s 1921 estimates of national income (which in turn laid the groundwork for the official Commerce Department estimates). The modern alternative to tax data—statistically representative household surveys—were not in widespread use until the 1940s (see chapter 6 for details). In its initial form, the US income tax had a relatively high threshold and thus covered a very small percentage of households. In the 1930s and 1940s, the tax base expanded until the majority of households were covered (Jones 1988), which in turn improved the ability of statistics based on tax data to track flows of income.

Together, these changes in the calculability of the economic system and the capacity of the state to calculate help explain why timely, official aggregate economic statistics emerged in the late 19th and early 20th century, and not a century or two earlier. Yet these changes in calculability cannot explain why national income statistics, in particular, became so dominant.

3.3 Why National Income?

National income statistics were virtually absent from economic theory and economic policymaking before the 1920s. The period from 1890 to 1920 saw an uptick in large-scale economic measurement, but very little activity around national income. Instead, theoretical and practical efforts around large-scale economic measurement were devoted primarily to cost-of-living indices (Stapleford 2009) and business cycle indicators (Friedman

2013). These measurement enterprises reflected both the theoretical interests of academic economists and the practical concerns of business and labor. National income statistics, in contrast, had no obvious constituency in this moment. Macroeconomics was not yet a term, or a coherent body of theory. The economics of growth was virtually nonexistent (as discussed in chapter 2, see also Warsh 2006). NIS were not yet understood as directly relevant to the business cycle. Instead, the impulse for measuring national income came from efforts to assess class conflict through objective measurement. By the 1930s, economists and policymakers began to connect national income to the problem of managing the depression as competing visions of economic planning eventually resolved into something recognizable as macroeconomic management.

3.3.1 Inflation and Business Cycles

The measurement of inflation is an old and difficult problem.⁹ In the late 19th and early 20th century, economists and bureaucrats began to devote increasing resources to its measurement. Theoretically, inflation was important to debates surrounding the Quantity Theory of Money.¹⁰ Versions of the quantity theory date back to the 16th century (Landreth 2012), although the most influential version is usually attributed to Irving Fisher (1911). Generally, quantity theorists argued for “monetary neutrality,” the idea that increasing or decreasing the supply of money will have no effects on the output of goods and services, but will instead only affect their price. Critics of the quantity theory argued that monetary movements frequently have “real” effects. Key to resolving these theoretical debates was good measurement of both the money supply and the price level. For example, if a doubling of the money supply (due to debasement, the discovery of new gold mines, etc.) doubled the price level, then the Quantity Theory would be taken to hold.

⁹This language is somewhat presentist—“inflation” had varying meanings in the 19th and early 20th century and was not very commonly discussed (see chapter 2); less anachronistic terms would be “the purchasing power of money” and “the cost of living.” For simplicity, I use the term inflation in its modern sense.

¹⁰There is an extensive literature on the quantity theory and its history. Interested readers might consult Laidler 1991, Humphrey 2001, Landreth 2012, and De Boyer Des Roches & Betancourt 2014.

At the same time as academics were interested in measuring the purchasing power of money to assess the validity of the Quantity Theory, labor activists and their business opponents wanted better measurements of the changing cost of living (Stapleford 2009). Efforts to measure changes in the price level began at the state-level Bureaus of Labor in the 19th century, and increased over the next few decades, and a final push induced by World War I resulted in a national Consumer Price Index, produced by the federal Bureau of Labor Statistics since 1921. These official measurements were invoked by both labor and business to justify demands for higher (or lower) wages.

Business interest in aggregate economic statistics was not limited to measurements of inflation, and academic discussions of aggregate dynamics were not limited to the quantity theory. In particular, there was a healthy tradition of business cycle theory dating back to the 19th century discovery that inspired a variety of measurement efforts (Morgan 1990), including the business forecasting movement of the early 20th century.

One of the first attempts to quantitatively assess the business cycle was also one of the most famous. In 1878, the English economist William Stanley Jevons published “Commercial Crises and Sunspots,” which argued that commercial crises followed a similar timing to sunspots. Jevons’ sole quantitative data source was the value of merchandise exported to India by the English East India Company (Jevons 1878: 36), an early example of the importance of large corporations as sources of data. Business cycle theory grew dramatically in the late 19th and early 20th century and business cycle theorists employed a diverse collection of data sources to analyze the cycle. Wesley Mitchell’s influential (1913) *Business Cycles* contained 150 tables on topics ranging from unemployment among union members to the prices of raw materials to bank clearings. Notably, Mitchell (1913: 21) made extensive use of the concept of “money economy” (defined as a community where “economic activity takes the form of making and spending money incomes”) to characterize the structural conditions needed to produce a business cycle. National income, in contrast, was not mentioned even once. Money economy was a kind of economic life, not an object with a

size.

Work on measuring business cycles was not limited to academic debates. Businesses in the United States were intensely interested in short-term forecasts of economic conditions, and a new industry emerged to provide them. Friedman (2009, 2013) documents the rise of the business forecasting industry, and how it benefitted from newly available data series, such as the Federal Reserve’s monthly index of industrial production. Major players in economic forecasting included Babson’s (first published in 1909), the Harvard Economic Service (starting in 1911), and the Index Number Institute run by economist Irving Fisher himself. These forecasting services popularized terms like leading and lagging indicators to describe how some data series seemed to predict larger movements in the cycle, while others followed behind. Their approaches varied—the Harvard group produced three series that aimed to track speculation, business activity, and banking separately, while Babson produced a single aggregate index—but they all focused on the timing of the ups and downs of a seemingly inevitable cycle, and not the overall size of the economic system (nor on the possibility for mitigating the cycle through proactive economic policy). And none of the forecasts looked at national income.

To summarize, in the 1910s-1920s, national income was not integrated in the major theoretical debates in economics, nor was it seen as a priority for business and labor. Where, then, did the impetus for producing better national income estimates come from?

3.3.2 NBER and National Income

Before 1920, estimates of national income were relatively scarce, and produced by lone estimators working with inconsistent definitions and limited data.¹¹ The most prominent economist working in the field was probably Willford King. In 1915, King published *The Wealth and Income of the People of the United States*. King (1915: 2) motivated his

¹¹For detailed histories of NIS in this period, see Studenski (1958), Carson (1971), Vanoli (2005), and Mitra-Kahn (2010). Much of the framing of the later half of this section on the origins of the NBER follows Rutherford (2011) and Shenk (2015).

research around the classic question of political economy, the question of distribution and the possibility of the elimination of poverty:

If there has been an increase in the riches of the nation as a whole, has the increase been distributed to all classes of the population, or have the benefits been monopolised by a favored few? Are the landlords, the capitalists, the captains of industry, or the wage- earners receiving the principal share of the fruits of progress?

King noted the general paucity of studies bearing directly on this question, and how newly available data from the Census Bureau and other governmental agencies had dramatically improved the possibility of providing a satisfactory answer, especially for the later decades of the 19th century (King 1915: 3-4). King (1915: 138) estimated the national income for the United States decennially from 1850-1910, and even estimated the price level to attempt to produce measures of changes in the real value of the national income. King went on to break down the national income by industry, by factor of production (wages, rent, interest, and profits), and by family income. This set of breakdowns reflected King's interest in the distribution of income, and was more or less the template followed by national income estimates for the next 20 years. King ended his text with a dramatic call to nativist action—arguing that the relative increase in standard of living documented in his table would disappear under the weight of “the subtle assaults of the low-standard alien invaders.” (King 1915: 255)

King's 1915 book does not seem to have had much influence on its own, but King himself continued to work in the field and was recruited by the newly-formed National Bureau of Economic Research (NBER) to assist in their first ever study, an estimate of national income and its distribution. NBER was founded by a group of businessmen, labor leaders, and academics who believed that social problems (and especially the biggest social problem of all, inequality) could be solved with better, more objective data (Rutherford 2011, Shenk 2015). NBER was founded in 1919, in part as a response to the elimination of the Central Bureau of Planning and Statistics, a federal agency which had been organized to

help mobilize the United States in World War I. Wesley Mitchell was selected as the first research director. Together, Mitchell, King, Frederick Macaulay, and Oswald Knauth undertook a new estimate of the national income, which was seen as both informing debates over the distribution of income and providing a framework to organize much-needed statistical information for military mobilization (Shenk 2015). For Knauth, the measurement of national income was essential to understanding class conflict. Knauth wanted to know if the national income were “so large as to make its proper distribution the primary source of public interest, or... so insufficient that its increase is the question of prime importance.” (quoted in Alchon 1985: 60).

In 1921, NBER published *Income in its Amount and Distribution, 1909-1919*. Unlike King’s (1915) earlier book, NBER was able to produce annual estimates, in part based on newly available income tax data. The emphasis of the book tracked King’s, with a bit less nativism. The first page opened with a list of “The Questions to Be Answered,” which focused on inequality, year-to-year fluctuations, real vs. nominal movements, and the comparison of the US per capita income to other nations. The report also noted how scarce comparable estimates were: as of 1921, only Australia had produced an official estimate of national income (see Studenski 1958 for details).

The 1921 book was well-received by the field, and NBER continued producing estimates of national income through the 1920s. Its estimates were cited in textbooks, especially those with an institutionalist bent that wanted to report data on the distribution of wealth and income (e.g. Edie 1922, Fairchild et al. 1926). Other organizations began to produce national income estimates as well. In 1926, the Federal Trade Commission published the first estimate of national income by a governmental agency, which covered the years 1918-1923, but the research was never continued and the report was largely forgotten (Carson 1971). National income did not yet have a strong constituency inside the federal bureaucracy. In 1927, the National Industrial Conference Board, a non-profit research organization funded by business interests, began to publish a competing estimate of national

income, using somewhat different definitions than NBER (Carson 1971). National income estimates were still disconnected from discussions of monetary theory and business cycles, and from economic planning, but there was growing interest.

In the 1920s, the US government began to take more of a leading role in gathering and publishing economic data. As Secretary of Commerce, Herbert Hoover pushed a model where the government supported business by providing information which businesses could, in turn, use to predict economic movements and moderate the business cycle (Barber 1985). In 1921, the Department of Commerce began publishing its monthly *Survey of Current Business* to provide up-to-date reports to business leaders. In response to the 1920-1921 downturn, Hoover organized the President's Conference on Unemployment, which mostly reported the disappointing state of unemployment data, but did eventually vote that between 3.5 and 5.5 million Americans were unemployed (see Duncan and Shelton 1978 for details). Hoover's passion for economic statistics, and private-public partnerships, made him an easy ally for NBER. In 1929, NBER published a massive volume of economic statistics on behalf of the Conference on Unemployment, titled *Recent Economic Changes*. The report covered everything from consumption to transportation to marketing. The final empirical chapter of the report, written by Morris Copeland, treated national income directly. Although Copeland reported similar information to prior publications, his emphasis was squarely on how national income data revealed changes in the economic system that might promote stability. Copeland (1929: 839) noted in his conclusion:

The two great impersonal forms of economic organization, corporations and governments, now disburse nearly one-half of our national money income, and during the war the proportion was even higher. There has been an appreciable increase in the proportion of total money income which may be called "fixed incomes"—salaries, interest, and rents—a change making for the increased stability of business conditions.

The detailed data provided by national income statistics were seen as useful for Hoover's distributed vision of planning where large corporations would establish policies that naturally, and voluntarily, tamped down economic fluctuations. This vision of planning was

tested and eventually abandoned over the following decade, as competing planning ideals emerged in the Great Depression (Balisciano 1998, see chapter 4 for a longer discussion). In that process, national income statistics would be repurposed as the central variable for a new vision of planning associated with John Maynard Keynes and the management of economic aggregates.

3.4 Making it Official: The 1934 US National Income Statistics

By 1932, everyone knew the nation's economic system was functioning poorly. But how bad was it? Available statistics at the time were inadequate to settle even relatively straightforward disputes about the number of individuals out of work, or the respective reduction in incomes of owners, landlords, and workers. Business cycle forecasters famously failed to predict the crash, as immortalized in Irving Fisher's infamous statement that "stock prices have reached what looks like a permanently high plateau" made just a week before the 1929 crash (Friedman 2013: 80). The failures of economic forecasters to predict the crash, and their continued enthusiasm in the face of a worsening of the economic situation led most of the major forecasters to close up shop (Friedman 2013: 199-201). The depths of the Depression and the failures of the available data and tools for understanding business cycle led academics and the government alike to look for both new data and new theories.

One of the leaders in the push for new governmental statistics was Senator Robert La Follette, Jr. La Follette, a Republican from Wisconsin, sympathized strongly with organized labor and supported the New Deal. In an interview given on March 15, 1932, La Follette laid out the dismal state of economic statistics.¹² On unemployment La Follette noted:

¹²The following quotes are from "Radio interview between Mr. Charles Ross and Senator Robert M. La Follette, Jr., over N.B.C., March 15, 1932, 8p.m.", La Follette Family Papers, Library of Congress, BOX I:C557.

It is a sad commentary on our statistical information that in the third winter of the depression we have absolutely no authoritative official figures on unemployment. The only data we have are those collected by the census in 1930 for the country as a whole and for certain cities in January 1931.

Better unemployment measurement would prove surprisingly difficult until the federal government adopted the technique of randomized household surveys, and it was not until 1940 that anything comparable to modern unemployment statistics became available (Card 2011). La Follette went on to call for official measurement of national income statistics and, in line with existing understandings of their meaning and use, connected this call to debates over wages and profits:

Likewise, with all the talk we have heard from bankers and others about the need for cutting wages and with all the actual wage-cutting that has taken place, we are woefully lacking in any adequate wage statistics. Also, while we are discussing the wages of labor, it is startling that we have no accurate information on the wages which capital is taking in the form of net profits from the point of view of industry as a whole. Furthermore, do you know that we have never had any official estimate of the total national income of the United States and the only authoritative information we have to go on is the estimate of an unofficial agency in 1929?

Interestingly, La Follette appears to have been unaware of the Federal Trade Commission's national income estimate published in 1926, which reinforces how marginal the effort was. In any event, its data would have been almost a decade out of date by 1932, and as La Follette noted, no estimates, private or official, were available for 1930 or 1931. La Follette connected the need for national income measurement to debates over the share of total income going to wages and profits, and how those may have changed in the wake of the Depression.

La Follette soon translated his public statements on the need for better statistics into legislative text. In June, La Follette authored and secured the passage of Senate Resolution 220 that called on the Department of Commerce to produce:

estimates of the total national income of the United States for each of the calendar years 1929, 1930, and 1931, including estimates of the portions of the

national income originating from agriculture, manufacturing, mining, transportation, and other gainful industries and occupations, and estimates of the distribution of the national income in the form of wages, rents, royalties, dividends, profits, and other types of payments. (Quoted in Duncan and Shelton 1978: 77)

The history of the production of these estimates has been well-told (Carson 1971; 1975 offers the most authoritative account, see also Philipsen 2015). Commerce sought the aid of NBER to help organize the estimates, and Simon Kuznets was tapped to lead the effort. Kuznets had been a student of Wesley Mitchell, and had just published an agenda-setting article on theoretical issues relating to national income statistics (Kapuria-Foreman and Perlman 1995). Kuznets built on and improved the work of the NBER, aided by both a small full-time professional staff, and the ability to acquire information and assistance from other governmental agencies (as noted above).

In January, 1934, Commerce published *National Income, 1929-1932*, to much acclaim. This report was widely read, selling an unexpected 4,500 copies in less than a year (Carson 1975: 159). The estimates produced became the basis for a continual series of national income statistics, which Commerce published annually, and then quarterly, in the *Survey of Current Business*.

Although Kuznets did not employ the exact language of “size of the economy,” he did invoke the term “national economy” in its modern sense multiple times in the opening pages of the report. For example, Kuznets (1934: 1) writes:

If all commodities produced and all personal services rendered during the year are added at their market value, and from the resulting total we subtract the value of that part of the nation’s stock of goods which was expended (both as raw materials and as capital equipment) in producing this total, then the remainder constitutes the net product of the national economy during the year. It is referred to as national income produced, and may be defined briefly as that part of the economy’s end-product which is attributable to the efforts of the individuals who comprise a nation.

This usage of “national economy” and “the economy” was not yet typical (as noted in Chapter 2), and Kuznets’ report may have played some role in popularizing the term-

nology. Note how “the economy” now has an end-product, a measurable size of output, denominated in dollars, and derived from market prices. The economy is no longer simply a placeholder describing a kind of economic system (“money economy” vs. “barter economy”), but a sociotechnical object whose “end-product” is measured according to a particular standard. This usage would grow considerably in government reports, academic publications, and popular usage over the following decade.

Kuznets, like Mitchell and King before him, was forced to make many difficult choices about what to include in the national income, and how to include it. Commodities bought and sold on the market presented the least challenge, especially given the increasing ability of the federal government to collect data on market transactions. Government itself proved more difficult to include. In addition to somewhat surprising data collection challenges, Kuznets faced a thorny theoretical challenge: how much was government’s output worth? For Kuznets, some of government’s activity clearly constituted “intermediate production,” necessary for final production but not valuable on its own. Counting all of government as final production would thus create a double counting problem, as if the final national income counted separately the value of a car, and the steel used to make the car. Unfortunately, there was no ready solution to the question of *how much* of government’s output was intermediate, and Kuznets’ ad hoc solution (subtracting the value of taxes paid by businesses) satisfied no one. Eventually, Kuznets’ successors at Commerce abandoned this treatment in favor of counting all of government as final production, a move which frustrated Kuznets who felt that it sacrificed too much for the sake of convenience (Kuznets 1948, Kuznets 1951, Gilbert et al. 1948).¹³ Apart from the treatment of government, most of the decisions made by Kuznets as to how to count and what to count were institutionalized in the official US standards in the 1940s, and further reinforced in the 1953 United Nations System of National Accounts. Small changes in the standards continue to present, but the basic logic

¹³In chapter 5, I return to the question of problematic inclusions and exclusion with an extended discussion of the treatment of unpaid housework, another area that has vexed national income accountants since at least the 1920s.

of the system has been fixed.¹⁴

3.5 Early Uses of National Income Statistics

In addition to being widely read, the official Department of Commerce national income statistics were picked up by policymakers, politicians, and businesses alike. Although they were not yet widely understood, and their presence was nowhere near as ubiquitous as it would later become, national income statistics made their way into discussions of New Deal programs, business planning, and even campaign speeches.

In 1935, just one year after Commerce published its first estimates, Lauchlin Currie authored an influential memo titled “Comments on pump priming.”¹⁵ Currie was an influential economic thinker during the New Deal, first at the Treasury Department and later at the Federal Reserve and the White House itself. Even before John Maynard Keynes’ (1936) *General Theory*, Currie advocated the use of fiscal policy (taxes and spending) to fight the Depression in aggregate. In “Comments on pump priming”, Currie drew on Kuznets (1934) to calculate how much the production of durable goods had fallen since 1929, and thus how much money the government would need to spend to restore full employment. Currie’s approach to macroeconomic management did not immediately win out in the competition

¹⁴This opinion is somewhat controversial. For example, there have been some changes in the measurement of finance (Christophers 2011) and intellectual property (van Ekelén 2015). But compared to the relatively large changes proposed and executed in the 1920s-1940s, the national accounts have been incredibly stable (see Waring 1999 for a feminist version of this argument, highlighting the continuity from NBER 1921 to the present UNSNA, also discussed below in Chapter 5). Perhaps the most notable shift has been the transition from discussions of national income to gross national product (GNP) and later gross domestic product (GDP). GNP came into use during World War II itself. It differs from national income primarily in that national income subtracts a measure of depreciation, while GNP does not. During the war, GNP was seen as the more relevant measure as the central question concerned the output of new war material, and short-run running down of productive equipment was not a concern. After the war, GNP stuck, although both measures continue to be produced. Similarly, GDP came to replace GNP in the 1990s (in the US) as the central aggregate. GDP tracks economic activity that takes place within the boundaries of the country while GNP tracked economic activity that “belonged” to the nation (e.g. the two measures differ in the treatment of multinational corporations). That said, GNP and GDP track each other very closely in the US, rarely differing by more than 1% (although this close equivalence does not hold for smaller countries with more foreign trade or investment). See Hirschman (2012) for a detailed explanation.

¹⁵This memo was reprinted in *History of Political Economy* as Currie (1978).

among competing visions of the New Deal, but as I show in the next chapter, by the end of the 1930s, macroeconomic management was ascendant (see also Lee 1990, Brinkley 1996, Balisciano 1998), with national income statistics as a critical component of this vision.

The use of national income statistics for planning was not limited to the federal government. Federal agencies, local governments, and business all wrote to Commerce asking for updated statistics and detailed breakdowns. A recurrent theme was an interest in speeding up the timing of their production. For example, in a November 5th, 1934 telegram, Fortune magazine wrote to ask for updated national income estimates through the end of 1933; Robert Martin (a senior analyst) replied that the estimates were not yet ready but would be soon.¹⁶ The following year, Robert Nathan, who took charge of the national income statistics following Kuznets' return to academia, corresponded with Herbert Bratter of the investment firm Loomis, Sayles & Company about the possibility of creating a monthly index of national income. Nathan reported that Commerce was working on a monthly series but so far had had little luck, as the data for many important components of national income did not readily lend themselves to a monthly series.¹⁷ Commerce never managed to produce a monthly index of national income, but the immediate interest in timely data suggests that end users were interested in up-to-the-minute assessments of the current economic situation through the language of national income.

Politicians too saw a use for timely national income statistics. In his 1936 re-election campaign, Roosevelt invoked increases in the national income as markers of the success of the New Deal and particularly the idea that deficit spending produced an increase in income:

The national income of the United States this year will have risen from about thirty-eight billion dollars in 1932 to well over sixty billions in 1936. In other words, it seems to me like a fairly simple mathematical question to put to you. If somebody that you trusted were to come to you and say, Look here, will you

¹⁶National Archives, RG 151, Index 600, Box 2563, Folder: "Finance and Investment - United States - National Income - 1934 - July - December."

¹⁷Letter from Robert Nathan to Herbert Bratter, July 5, 1936. National Archive, RG 151, Index 600, Box 2564, Folder: "1936 July - December."

borrow \$800 so as to get an increase in your annual income of \$2,200?' would you do it or not?¹⁸

Here, we see how national income has become rhetorically linked to both purchasing power and the success of government initiatives. Roosevelt invoked similar statistics in at least eight other speeches in September and October of 1936, all to reinforce the success of his economic programs.¹⁹ An October 1st speech in Pittsburgh, PA went into greater detail, and offered a quick definition of national income suggesting Roosevelt did not believe that his audience would be entirely comfortable with the concept:

Our national income had declined over 50 percent—and, what was worse, it showed no prospect of recuperating by itself. By national income I mean the total of all income of all the 125,000,000 people in this country—the total of all the pay envelopes, all the farm sales, all the profits of all the businesses and all the individuals and corporations in America.²⁰

The entire speech focused on changes in national income, on how Roosevelt took office in a period when national income was on a declining spiral, and how his deficit spending managed to arrest that spiral. Seeing the large uptick in national income from 1932 to 1936, Roosevelt was now certain that the time for deficit spending was over, and that the federal budget could be balanced as tax incomes automatically increased and temporary spending measures came to a halt (a strategy that arguably led to a second depression in 1937, which in turn cemented the place of Keynesian analysis inside the White House, see Brinkley 1996).

By the end of the 1930s, national income had become a significantly more central statistic to our collective understanding of the economic system, which in turn increasingly went by the name “the economy.” When the National Resources Committee, a New Deal planning group, published a major 1939 report they titled it *The Structure of the American*

¹⁸Remarks at the State Capitol, Lincoln, NE. October 10th, 1936. Available at <http://www.presidency.ucsb.edu/ws/?pid=15161>. Also cited in Carson 1975: 160.

¹⁹A search for references to “national income” in 1936 in The American Presidency Project yields 11 results, including 9 campaign speeches. See <http://www.presidency.ucsb.edu/ws/index.php>.

²⁰Address at Forbes Field, Pittsburgh, PA, October 1st, 1936. Available at <http://www.presidency.ucsb.edu/ws/index.php?pid=15149>.

Economy (including a rare definition of this new use of the term “economy”, as discussed in Chapter 2). The first statistic cited in this report was a measure of national income, including an estimate of how much income the country had lost due to the Depression (NRC 1939: 2). National income was seen as the right measure to capture the overall economic devastation of the 1930s.

The 1930s thus saw significant changes in both the production of national income statistics, and the beginnings of their widespread use. World War II would cement their place as the most important measure of the size of the economy, and prove the utility of national income analysis for managing that economy.

3.6 National Income Goes to War

By 1940, national income statistics had begun to work their way into military planning. Although the US had not formally entered the war, planning for wartime mobilization had begun in earnest. Throughout 1941 and continuing into 1942, generals, civilian officials, and economists debated how many men and how much materiel the American economy was capable of devoting to the war effort. National income statistics became central to this debate, as three civilian economists—including Simon Kuznets and Robert Nathan—debated top military planners in an effort to create a “feasible” mobilization plan. Military historian Jim Lacey (2011: 40) argues that “It is almost impossible to overestimate the effect of Kuznets’ work on the planning and conduct of America’s wartime mobilization.”²¹

The most visible site of the influence of national income statistics on US mobilization came in 1942. In January, 1942, President Roosevelt announced ambitious goals for the construction of planes, tanks, and ships to be delivered by the end of 1943. Nathan and

²¹This section relies heavily on Brigante (1950), Edelstein (2001), and Lacey (2011), who all narrate these events in detail. Brigante offers an immediate retrospective drawn from documents and oral histories, Edelstein analyzes the debate from the perspective of economic history (how accurate were economists’ predictions?), and Lacey offers a detailed account rooted in military history. All three agree on the details salient for the account here.

Kuznets, working for the War Production Board, were convinced that Roosevelt's goals could not be met, and that in attempting to produce too much, the US economy would actually produce less than it could, as the misallocation of resources would exacerbate scarcities and hold up production. Nathan and Kuznets grounded their analysis in national income statistics, which provided a single framework to unify discussions of the civilian and military labor forces, civilian consumption needs, and different forms of military production (including production for military allies).

In March, 1942, Kuznets authored a memo arguing that the existing plan was not feasible, and that either civilian consumption would have to fall dramatically or the military would fall about 20% behind in its production goals. Kuznets' memo was poorly received by the military brass, especially General Brehon Somervell, who was in charge of the procurement and distribution of supplies for the Army. Somervell distrusted national income statistics, and believed Kuznets and Nathan were far too pessimistic.

The bureaucratic debate raged until a heated meeting on October 6, 1942.²² Based on updated analysis, Kuznets and Nathan were now convinced that the military program was close to 30% too large to be feasible. They presented their analysis to the War Production Board, including Robert Patterson (the Under Secretary of War), Isador Lubin (an influential advisor to FDR), and Leon Henderson (another influential New Dealer, and then Administrator of the Office of Price Administration).

The turning point of the meeting, according to Brigante (1950: 30) came when Henderson stepped in to support Kuznets and Nathan. Kuznets and Nathan had argued that the economy could support no more than \$90 billion in war production; Somervell had countered that \$90 billion was insufficient for the plans of the Joint Chiefs. Henderson remarked that "The amount in question, 90 billion dollars, was interesting... because it exceeded by far the value of our entire national product both for 1933 and 1934. ... Maybe if we can't wage a war on 90 billions, we ought to get rid of our present Joint Chiefs, and find some

²²Lacey (2011: 173-201) reprints several memos leading up to the meetings, as well as their minutes.

who can.” (quoted in Lacey 2011: 112) After that, the matter was largely settled: the military would have to roll back its plans to what the civilian economists thought was feasible based on the national income data.

Although the planning committee itself was soon dissolved, the scaled back production plans were seen as a tremendous success, enabling the US to efficiently mobilize the maximum feasible military in the quickest time (Edelstein 2001). The debate also demonstrated the value of national income statistics for planning government expenditure in a much more vivid fashion than the halting attempts at macroeconomic planning in the late 1930s. After the war, national income statistics would only increase in their centrality to federal policymaking.

The United Kingdom experienced an even more dramatic transformation in terms of the production and use of national income statistics in World War II. As of 1939, the UK had no official estimates, nor did it have unofficial estimates akin to those routinely produced by NBER. In 1940, John Maynard Keynes wrote *How to Pay for the War*, an influential pamphlet that drew on estimates of UK national income produced by Colin Clark to sketch a plan for war finance that would avoid the hyperinflation of previous wars by strategically reducing consumer demand through a mix of taxes and bonds (see Skidelsky 2005 and Tily 2009 for details). The following year, Keynes convinced the UK government to begin to produce its own official national income statistics for wartime budgeting, and recruited Richard Stone and James Meade to compile the first estimates. These estimates, much like those of Kuznets and Nathan for the US, were seen as highly effective tools for managing the wartime economy, and proof of Keynes’ economic ideas.

Germany experienced a somewhat different trajectory, adopting national income statistics a bit earlier than the US and UK, but abandoning them before the end of World War II (Tooze 2001). National income statistics took off under the Weimar regime as part of the effort to pay reparations. Under the Nazi regime, the German statistical bureau shifted towards more disaggregated forms of planning (akin to something like input/output ac-

counts, or Soviet central plans). This trajectory nicely demonstrates the non-inevitability of national income statistics in this period: many statistical alternatives were available, and it was a particular alliance of actors and circumstances that led to the success of national income statistics in the US and UK (and their failure in Germany). Just as the US and UK and won World War II, national income statistics in the form adopted by the US and UK would come to dominate in the post-war era.

In 1944, representatives from the US, UK, and Canada met to discuss standardizing the production of national income statistics (Denison 1947). These meetings laid the groundwork for the international standards designed by Richard Stone in the late 1940s and 1950s, and eventually published as the United Nations System of National Accounts. The standards developed in the 1930s and 1940s to deal with the Great Depression and World War II would be enshrined as the global standards (Ward 2004, Herrera 2010). National income, having proved its worth in World War II, would be propelled into global use as the measure of the size of every nation's economy.

3.7 Aftermath

Before World War I, national income statistics were a backwater of research, absent from public debates, and disconnected from economic theory. By the end of World War II, national income statistics were produced on a quarterly basis by the major English-speaking governments, were seen as critical policy tools for planning in both war and peace, and were central to the theoretical debates in the newly-named subfield of macroeconomics. To riff on Charles Tilly, here war made statistics.

In 1948, Paul Samuelson published the best-selling economics textbook of the era (Skousen 1997). Samuelson's *Economics* departed from pre-war textbooks in several respects, but one of the most notable was its increased emphasis on aggregate topics.²³ Samuelson

²³Samuelson's textbook was not unique in this departure; other late 1940s and 1950s textbooks placed national income analysis first. See Burns et al. 1948, Nordin and Salera 1950.

went so far as to write that “National income provides the central unifying theme of the book” (1948: v) and “It is the first task of modern economic science to describe, to analyze, to explain, to correlate these fluctuations of national income.” (1948: 5) Macro topics preceded micro—students were expected to understand Keynesian theories of full employment, inflation, and the movements of national income *before* they could make sense of traditional topics like supply and demand. Samuelson (1948: 225) made clear how national income statistics made economic movements visible:

By means of statistics of national income, we can chart the movements of a country from depression to prosperity, its steady long-term rate of economic growth and development, and finally its material standard of living in comparison with other nations.

National income, then, had replaced the earlier business cycle indicators as the most important measure of short-term ups and downs while simultaneously providing a metric for comparing affluence over time and between nations. National income unified the short-run and long-run, and brought together macroeconomic theories and policymaking. National income, as the measure of the size of the economy, became an obligatory passage point for economic debates.²⁴ And in turn, “the economy” became an object with the capacity to grow or shrink.

The 1940s and 1950s saw a massive increase in both academic research on economic growth and the explicit politics of growth and development. Academic research on the measurement of growth included early efforts by Clark (1940), whose *Conditions of Economic Progress* may have been the first to use national income statistics to show just how large the gap between poor and rich countries had become, and a long string of research by Kuznets and collaborators (e.g. Kuznets 1956) that showed just how much the rich countries had grown in the 19th and 20th centuries. Theoretical work included the famous Solow (1956)

²⁴The explicit use of the phrase the “size of the economy” takes off in the 1950s. Hollinger (1954: 225) provides a clear example, one that explicitly defends the use of national income statistics as the measure of size for understanding developing countries: “To get our initial insights into the size of the economy how much there is there to work with, and the rate of growth of the economy, national income tables are appropriate...” (Hollinger 1954: 225)

model, which became the workhorse theoretical model of growth (Warsh 2006, Boianovsky and Hoover 2014). Politically, economic growth came to dominate domestic politics in rich countries like the US (Collins 2000, Yarrow 2010) and Japan (O’Byran 2009), while its close cousin economic development dominated discussions of rich countries’ obligations to poor ones (Ferguson 1990, Mitchell 2002). The growing theoretical, practical, and political use of national income statistics tended to fix the statistics in the form they took in the immediate post-war period, and to increase their importance.

3.8 Conclusion

Before the 1920s, we did not talk about “the economy” and we did not measure economic life according to a single metric. In the 1920s-1930s, national income statistics took off as an empirical enterprise in part fueled by old class concerns, and made feasible by transformations in the capacities of the state, the organization of business, and information technology. By the end of World War II, discussion of “the economy” was pervasive, and national income was its measure, yoked to new forms of macroeconomic planning. Post-war, national income was fixed into a system of national accounts, which in turn became a global standard used in a variety of debates around growth, development, and planning. Academic research continues along many paths, but “the economy” as object remains indelibly linked to national income statistics that measure its size, and thus provide its operational definition. For many purposes, the economy is what national income statistics measure (and vice versa).

As Porter (1995: 17) has eloquently argued, “Numbers... create new things and transform the meanings of old ones.” Giving economic life a *size* created “the economy”, a powerful new thing. Quantifying economic life transformed it into the economy, adding a bounded object onto an existing domain of social life. As with other forms of quantification, this act made economies commensurable with another. Commensuration, in turn,

inspires comparison: “the truly generative aspect of commensuration is that it creates precise distinctions at the same time that it unifies... Precise distinctions force comparisons and stratification. It is impossible not to notice who or what is higher or lower.” (Espeland and Lom 2015: 34) Adam Smith could point to the apparent affluence of the Netherlands as a topic of debate; 20th century economists would measure the precise gaps between the US and Mexico, Germany and France, Africa and the rest of the world, even when this precision appears unwarranted given the quality of the underlying data (Jerven 2013).

In the next chapter, I focus on another consequence of the proliferation of timely macroeconomic data: a transformation in the presumed capacities and political economic responsibilities of the state. Then I will turn to two chapters about what was left out of national income statistics—paths not taken, alternatives that were abandoned due to the particular contingencies that led to the institutionalization of a particular version of national income as the size of the economy. In the conclusion, I return to questions of alternatives and counterfactuals.

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CHAPTER 4

Managing the Economy in Real Time

4.1 Introduction

In 2007 and 2008, the American financial system was in turmoil. Unexpected declines in housing prices translated into a widespread panic as previously liquid derivatives suddenly became toxic.¹ The response from the Federal Government was swift and massive. The Federal Reserve lowered interest rates from 5.25% in 2007 to 0-0.25% by the end of 2008. The Fed also dramatically increased its purchases of other financial assets to provide liquidity to the market and increased its lending to troubled institutions. In October 2008, after overcoming brief Congressional resistance, President George W. Bush signed the Troubled Asset Relief Program into law, which promised to provide another \$700 billion in government funds to prop up the financial industry. Despite these efforts, the American economy began to decline. By the time Barack Obama won the presidential election, it was becoming clear that stemming the panic on Wall St. would be insufficient to prevent a recession.

In meetings with his economic advisors, President-Elect Obama began to draft the outlines of a proposed economic stimulus bill.² In order to determine how much money the federal government should spend, Obama's economic advisors looked to the available macroeconomic indicators to see how badly the financial crisis was affecting the rest of the economy. The first estimate of GDP for the third quarter of 2008 had just been released,

¹For details on the panic and alternative accounts of the causes that produced it, see Gorton 2010, Financial Crisis Inquiry Commission 2011.

²This section draws heavily on the account of journalist Michael Grunwald (2012).

and it showed a small decline, $-.5\%$.³ Based on this data, economist Christina Romer (soon to be appointed Chair of the Council of Economic Advisers) argued for a robust stimulus package, much larger than the one proposed by Obama during his Fall campaign. By January 2009, the President and his advisers settled on \$800 billion as the minimum stimulus needed to right the economy.

On January 9th, 2009, Romer and Jared Bernstein (Vice President-Elect Biden's top economic adviser) published a report detailing how they expected the economy to fare with and without their stimulus proposal, and justifying the size of the program in terms of the best available estimates of the economy's decline. Romer and Bernstein (2009: 4) focused on projections of the unemployment rate (see figure 4.1). Without the stimulus, unemployment would soar to 8.8% ; with it, unemployment could be kept to just 7% . Their projections translated stimulus spending into jobs: the stimulus would increase GDP 3.7% over two years, which in turn would create or save 3 million jobs.

In February, the President's stimulus was passed as the American Recovery and Reinvestment Act, which allocated approximately \$800 billion dollars in aid to states, tax cuts, and direct spending on public works, infrastructure, and simple direct cash payments to individuals. Unfortunately, the data on which Romer's calculations were based turned out to be overly optimistic. The $-.5\%$ growth for the third quarter of 2008 would be revised into -4% . And the estimates were even worse for the fourth quarter. As Grunwald (2012: 248-249) reports: "At the time the stimulus passed, the official GDP estimate for the fourth quarter of 2008 was -3.8 percent. That was disastrous, but a couple weeks later, it would be revised to an unthinkable -6.2 percent. And it would eventually be rerevised to -8.9 percent. That's Great Depression territory."⁴

Romer and Bernstein's unemployment forecasts proved to be as optimistic as the initial

³Note that quarterly GDP data are reported on an annualized basis in the US. For example, if the BEA reports a 4% growth rate, that means if growth were sustained at that pace for a full year, it would correspond to an increase in yearly GDP of 4% .

⁴See Reamer (2014) for a more detailed discussion of what statistics were available to policymakers at different moments in 2008-2009.

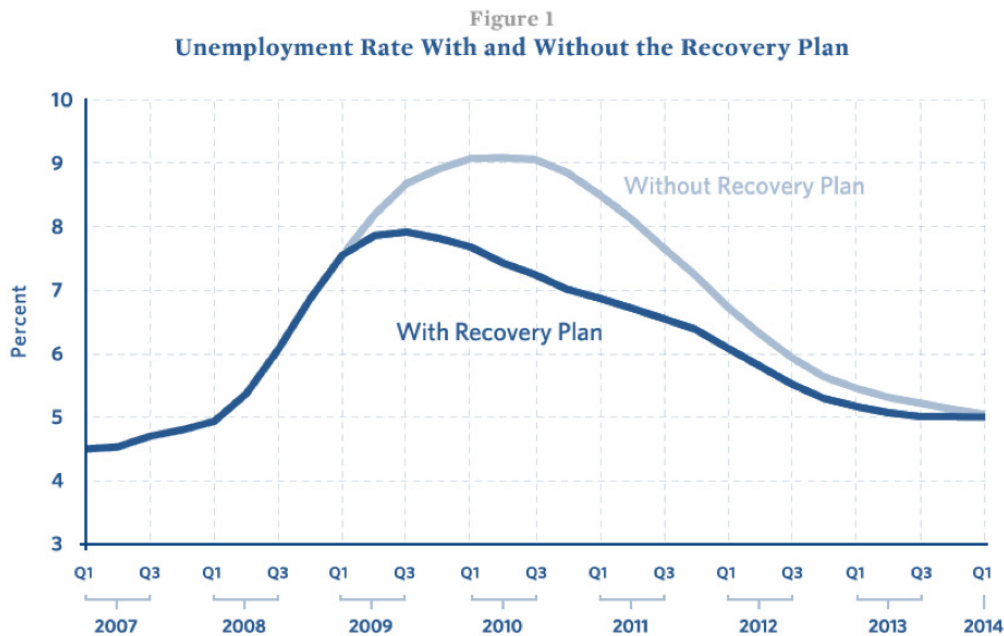


Figure 4.1: From Romer and Bernstein 2009.

GDP estimates. Even with the stimulus, unemployment hit 10% before the end of 2009. Although recent evaluations suggest that the stimulus and bailout were incredibly successful, keeping unemployment to 10% instead of 16%, and holding declines in GDP to 4% rather than 14% (Blinder and Zandi 2015), political commentators routinely judged the stimulus a failure, and the Democrats fared poorly in the 2010 elections. As political scientists predicted, voters punished incumbents for the short-run economic situation, independent of the blame that incumbents “rationally” deserved (Achen and Bartels 2004, Bartels 2013, Healy and Malhotra 2013).

This chapter investigates the history of government responsibility for the short-term performance of the economy. As the story of the 2008 bailout and 2009 stimulus demonstrates, contemporary policymakers have come to understand their role in terms of timely estimates of economic aggregates. In the language of the Federal Reserve, economic policy has become “data dependent,” conditional on the best estimates of the current state of economic affairs. When and how did the state become responsible for the economy and what

exactly did it become responsible for? When and how did policy become “data dependent”? In the post-war era, the federal government has taken responsibility for “managing the economy” (or “macroeconomic management”). The availability of timely measurements of (what came to be understood as) macroeconomic outcomes helped to change how governments perceived their economic responsibilities without necessarily equipping them with the capacity to execute those responsibilities. Although economists and politicians have debated the best means for managing the economy throughout the post-war period, and the exact targets for good economic performance, the debate has largely rested on the assumption that the federal government is capable of both seeing the economy in real time and acting directly on the economy, albeit with limitations.

The playbook that Presidents Bush and Obama followed in responding to the financial crisis was written over the course of the 20th century. In particular, I focus on the interwoven development of economic theories (macroeconomics, growth theory), economic policies (monetary policy, fiscal policy), policymaking institutions (the Federal Reserve, the Council of Economic Advisers), and economic statistics (national income statistics, unemployment statistics) from about 1890 to about 1964. Covering such a large swath of time and expanse of domains requires a synthetic, survey approach that necessarily excludes a detailed analysis of each period. The advantage of taking such a broad view is that it highlights dramatic transitions from the early to the late 20th century that might be occluded by focusing on a narrower slice of time. It also allows for the possibility that policy instruments may be developed before or after the theories justifying and quantifying their use, and policymaking institutions may be repurposed from one task to another.

This analysis offers new insights into debates over the state’s role in economic life. The state has long been responsible for “governing economic life” (Miller and Rose 1990) in various forms. Highlighting the emergence of macroeconomic management showcases the dramatic transformation in the scale of government’s responsibility for economic performance. This policy paradigm made visible small quantitative changes in economic per-

formance, and obligated the state to account for those changes. Every tiny movement of inflation, unemployment, and economic growth is made visible to the public and trumpeted rhetorically by politicians. Romer and Bernstein drew up a massive intervention to respond to a movement in unemployment that would have been difficult to detect a century earlier. Smaller recessions in the postwar era have occasioned more modest responses, but nonetheless have obligated states to at least offer explanations and plans cast in precise, quantitative terms.

The emergence of “managing the economy” as the dominant discourse of government’s relationship to economic life also offers a novel interpretation of the origins of the political economic crisis of the 1970s. Krippner (2011) identified a triple crisis for the state: increasing demands for resources in the face of declining growth, state expenditures outstripping revenues, and declining public confidence. Financialization, in Krippner’s account, emerged as a partial solution to these crises, a means by which the state offloads some of its responsibility for managing the economy back onto the market in general, and finance in particular. Here, Krippner, following Offe (1974), argues that the state’s triple crisis resulted in part from its expanded role in managing the economy, which redefined economic events “as the product of state actions rather than the blind operation of the market.” (Krippner 2011: 21) This chapter offers a new characterization of the process by which the state came to manage the economy, and in particular, how managing the economy came to mean the simultaneous obligation to measure, report, and control inflation, unemployment, and growth in precise terms.

The remainder of the chapter proceeds as follows. First, I briefly review existing treatments of the state’s responsibility for economic affairs. Second, I describe the sources and analytical approach used for my analysis. Then I proceed to an analysis of economic policy from 1890 to 1964, broken into four periods. In each period, I assess changes in the government’s stated responsibilities, the institutions charged with making economic policy, the economic theories guiding (or at least available to guide) the formation of policy, and the

production of economic statistics.

4.2 The Role of the State in Economic Life

How has the state's responsibility for economic life changed over the course of the 20th century? My approach in this chapter draws on work by Marxian political theorists, American economic sociologists, and scholars of governmentality. Specifically, I combine insights from Offe (1974) and Krippner (2011) on the political responsibility of the state with work by Miller and Rose (1990) and Scott (1998) on the technological aspects of government. I focus especially on how these authors make sense of the history of 20th century economic policymaking.

Writing in an explicitly Marxist tradition, Offe (1974, 1975) argued that the state was not simply influenced or constrained by the political power of capitalists, but rather that the state is *structurally* capitalist. In this account, the state serves to protect the interests of capitalists against both explicitly anti-capitalist forces (socialist movements) *and* the short-sighted self-interest of individual capitalists. Through a combination of selection filters, the capitalist state prevents radical or destructive items from reaching the policy agenda. To succeed in this task, the state must maintain a certain level of legitimacy with the public; it cannot be too closely identified with the capitalists whose interests it serves. Offe argues that, in the post-WWII era, capitalist states have been pushed by these structural necessities to take responsibility for economic outcomes while simultaneously attempting to reduce the size of the state. The consequence is that the state transforms economic outcomes into state responsibilities, independently of the state's ability to secure those outcomes. Offe (1974: 50) highlights full-employment policy as a particularly important example of these tensions:

A suitable case to illustrate this situation may be found in the proclamation and practice of "full-employment policy." At least in the form of a declaration of the aims of economic policy, a State policy of job-security has since the Second

World War been part of the stock-in-trade of almost all the parties in every developed industrial state in the political assurance of corresponding laws and programmes related to business, educational and structural policies it would never have been possible to secure the degree of social peace and integration of the trade-unions which, above all in West Germany, was a prerequisite for a capitalist restoration. Independently of the failure or (except in the USA) relative success of this programme its mere adoption alone has an important effect which modifies reality: the actual fact of unemployment changes its social definition wherever it appears; it is no longer perceived as a periodic event in a blindly operating economic cycle but as a “culpable” and therefore “actionable” failure on the part of political-administrative direction.

Offe sees full-employment policies as both essential to securing the legitimacy of post-war capitalist states, but also setting up an impossible expectation that the state would actually manage to achieve jobs for all. Unemployment no longer results from a “blindly operating economic cycle” but rather from the failures of governments to properly manage the economy. Whether or not we accept Offe’s arguments about class conflicts as the source of the state’s newfound responsibility, his analysis of the increasing responsibility of the state for economic outcomes provides a valuable starting point for understanding the history of macroeconomic policymaking. Offe identifies how post-WWII states made many new economic guarantees — full-employment, growth, stability — that they could neither routinely deliver, nor readily withdraw from without occasioning protest and loss of legitimacy.

Building on Offe’s analysis, and other treatments of the 1970s crisis, Krippner (2011) identifies particular strategies through which the American state grappled with its newfound responsibility for providing full-employment and managing the economy. Krippner (2011: 16-20) characterizes the 1970s crisis as simultaneously a social, fiscal, and legitimation crisis. The social crisis stemmed from increasing demands for economic resources in the face of declining growth, which in turn produced inflationary pressures. The fiscal crisis resulted from asymmetric political forces which pushed for increased expenditures (to solve the social crisis) and stable or reduced taxes (demanded by capitalists). And the legitimacy crisis flowed from reduced public confidence in the state, attributable to its failure to deliver

on its many new promises.

Krippner (2011) identifies financialization as the outcome of policymakers grappling to solve these crises. The free flow of credit across sectors, and increased foreign capital inflows combined to relieve credit constraints and, at the same time, the Federal Reserve managed to fend off the threat of inflation through higher interest rates while simultaneously justifying that policy in terms of market forces. Summarizing, Krippner (2011: 22) argues “these developments allowed policymakers to avoid politically difficult decisions about how to allocate limited resources between competing social priorities.” The social and fiscal crises were solved through the increased availability of credit while the legitimacy crisis was solved, in part, through a rhetorical inversion that devolved some responsibility for macroeconomic conditions back to the “blindly operating economic cycle” (in Offe’s words).

These perspectives offer a useful starting point for understanding the transformation of the state’s responsibility for economic outcomes in the post-war era. They identify an important end-point, the post-war settlement of the 1960s, and some of its key features, including the promise of full employment, management of the economic cycle, and direct responsibility of the state. Extending this analysis, I argue that not only had the state become responsible for broad economic outcomes, but it had become accountable for relatively small deviations from a very optimistic ideal. Fluctuations too small to even see in the 19th and early 20th century became political crises warranting response in the mid-20th. “Managing the economy” as a paradigm for the state’s role in economic life implied a kind of precision, even as it resolved tensions about liberal governance by creating a new object, the economy, which the state could manage instead of more directly controlling production. Put differently: the precise tensions identified by Offe and Krippner were new to the post-war era, and they emerged from transformations in the technologies states used to see and act on economic life. These technologies, especially aggregate economic statistics, allowed capitalist states to take on responsibility for macroeconomic outcomes

without taking direct planning responsibilities increasingly associated with socialism or communism.

My approach to understanding the technologies of governance draws heavily on the writings of Miller and Rose (1990). Their approach, inspired by Michel Foucault, “draws attention to the fundamental role that knowledges play in rendering aspects of existence thinkable and calculable, and amenable to deliberated and planful initiatives: a complex intellectual labour involving not only the invention of new forms of thought, but also the invention of novel procedures of documentation, computation and evaluation.” (Miller and Rose 1990: 3) In order to govern an area of social life, it must first be rendered governable. As we saw in the previous chapter, the statistical capacities of states dramatically increased in the first half of the 20th century. By the end of World War II, the United States published monthly unemployment and inflation statistics, and quarterly estimates of Gross National Product. At the start of World War I, it produced none of these.

These statistics (re)constituted economic life as “the economy,” crystallizing an immensely complicated domain into a technical object whose performance could be assessed through a small set of relatively simple statistics. The economy as object is big enough to include the entirety of production and exchange in a given country, but small enough to be displayed on a single graph. Following Miller and Rose (1990: 7, cf. Latour 1988), this sort of simplifying transformation is necessary to make phenomena governable:

The events and phenomena to which government is to be applied must be rendered into information — written reports, drawings, pictures, numbers, charts, graphs, statistics. This information must be of a particular form — stable, mobile, combinable and comparable. This form enables the pertinent features of the domain- types of goods, investments, ages of persons, health, criminality, etc. — to literally be re-presented in the place where decisions are to be made about them (the manager’s office, the war room, the case conference and so forth).

In order for the President and the Council of Economic Advisers to “manage the economy” they must be able to make it small enough to fit inside the Oval Office. Aggregate

economic statistics developed in the 1920s to 1940s did the trick. Older, competing visions of the government's role in economic life (discussed more below) did not disappear entirely, but "managing the economy" came to hold a dominant position in our understanding of the government's responsibilities and capacities.

To be slightly more precise, I define "managing the economy" as a kind of political rationality that takes responsibility for the performance of a particular sociotechnical object (the economy) measured by specified calculative devices (national income statistics, unemployment rates, etc.).⁵ Managing the economy is associated with a suite of technologies of government, including fiscal and monetary policy, which are in turn new names and new understandings of (primarily) older forms of government actions (e.g. taxing, spending, buying and selling government debt, printing currency). Managing the economy is not a singular program, but a terrain of discursive debate in the post-World War II era. From the ebbs and flows of fiscal and monetary policy (Stein 1969), to debates over appropriate targets for economic growth (Collins 2000), to the rise and fall of Monetarism (Krippner 2011), economists and policymakers have debated the best way to manage the economy and the limits of how far the economy could be pushed.

Perhaps no author has been more influential for our understanding of the limits of state action than James Scott (1998). In *Seeing Like a State*, Scott (1998: 2) argues that "legibility" is "a central problem in statecraft." While Miller and Rose emphasized how processes of documentation and the creation of new statistics enabled new forms of government, Scott shows how many aspects of social life remain illegible to central planners despite their best efforts. Scott's work focuses on the deadly combination of "high modernist" ideologies and authoritarian regimes, which combine together to create massive human tragedies.

By contrast, "managing the economy" presents an alternative ideological vision of the role of the state (authoritarian or otherwise). Riffing on Scott, we might call managing the economy a form of "low modernist" ideology, one that draws on a particular legibil-

⁵On political rationalities, see Miller and Rose 1990:8. On calculative devices for policymaking, see Hirschman and Berman 2014.

ity project (macroeconomic statistics) to implement a much more modest vision of state control. The economy is envisioned, and calculated, as an object composed of, yet distinct from, the plans of individual firms and households. Acting on the economy, at least in theory, avoids the strong requirements of high modernist projects for complete knowledge and complete control. Instead, states trying to manage their economies act on aggregate flows and let individuals work out the details for themselves.

Perhaps no single object embodies this low modernist vision more clearly than MONIAC — the Monetary National Income Analogue Computer — designed by the New Zealand economist Bill Phillips in 1949 (see Colander 2011). Phillips is most famous for his eponymous Phillips Curve (Phillips 1958), a mathematical relationship between unemployment and inflation that has been the source of much debate ever since it was first identified (see Forder 2014). MONIAC, pictured in Figure 4.2, modeled the flow of national income through the economy as a literal flow of water through the tubes of the machine, and through manipulation of various apertures allowed an economist, student, or central banker to compute the effects of various policies. MONIAC was hydraulic Keynesianism in its most literal form. Managing the economy involves acting on the economic system in a way that is both timely and invasive (not simply “hands off,” nor entirely “structural” and long term) without requiring the kind of detailed microeconomic planning schemes that Scott identifies as failing due to lack of metis, local and tacit knowledge. Of course, avoiding the pitfalls of high modernist planning does not imply the easy success of macroeconomic management.

Miller and Rose (1990: 9) argue that democracies rely on “indirect mechanisms of rule... that have enabled, or have sought to enable government at a distance.” The economy itself, and the techniques to manipulate it, are examples par excellence of this rule at a distance. In the following sections, I document how policymakers and experts in the United States discovered macroeconomic management as a strategy of indirect rule, and how it came to dominate over competing visions of planning in the 1930s and 1940s.

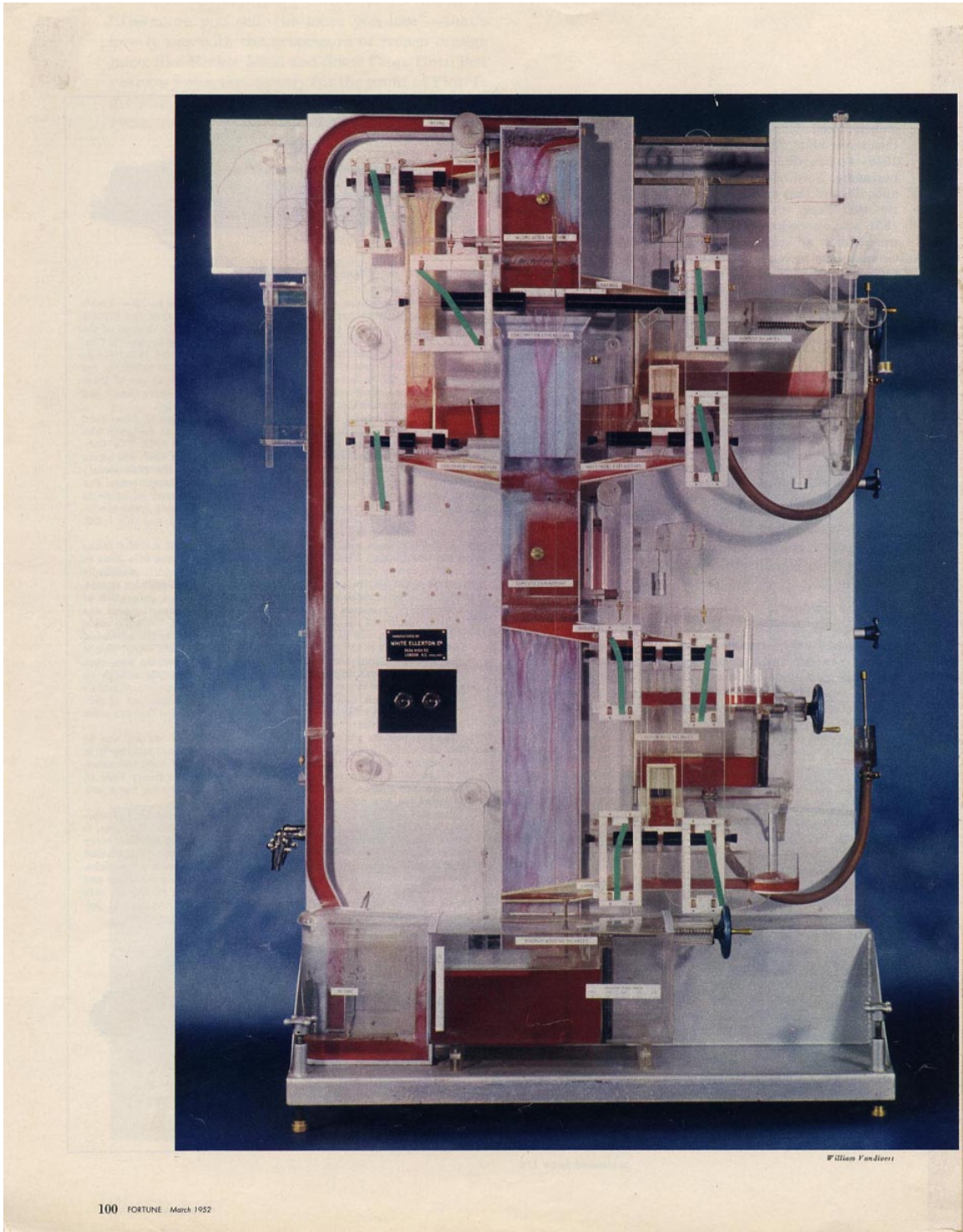


Figure 4.2: A photo of MONIAC from Fortune Magazine (1952).

4.3 Analytical Strategy

In the following four sections, I trace the history of the federal government's involvement in managing large scale economic phenomena from about 1890 to 1964. Studying the end of this period presents relatively few difficulties. By 1946, the major players, theories, measures, and debates were all established. If you want to understand 1950s thinking about the state's role in managing the economy, you can simply look at the debates between the President's Council of Economic Advisers and the Congressional Joint Economic Committee, read the policy briefs of the Committee for Economic Development, or study the macroeconomics portions of economic textbooks. These stories are well-told, and I rely heavily on the excellent treatment provided by Collins (2000), Yarrow (2010), Stein (1969), and Bailey (1950), as well as my own reading of primary sources.

Similarly, the New Deal itself has been extensively researched, including very useful accounts of the debates within the Roosevelt administration surrounding different methods of planning (see Balisciano 1998, Brinkley 1996, Lee 1990, Stapleford 2007, and Reagan 1999 for a sample). A smaller but useful literature explores 1920s precursors to the New Deal, and especially Hoover's pioneering, but often forgotten, efforts to engage in large-scale economic planning (Alchon 1985, Barber 1985, Metcalf 1975).

Before the 1920s, however, the trail is much more difficult to follow. In some sense, this difficulty is reassuring: the larger argument of this dissertation is that the 1920s-1930s are the beginning of a sizable rupture in how economists and policymakers think and act on large-scale economic phenomenon. Establishing this rupture requires studying a much more diffuse policy domain as it begins to coalesce. In order to do so, I rely on a combination of sources. Two of the most useful are histories of the financial system, including the debates surrounding the Federal Reserve (Greider 1987, D'Arista 1994) and histories of the concept of unemployment (Garraty 1979, Sautter 1991). By the 1940s, monetary policy and unemployment would be linked together in the discourse of macroeconomics; in the 1900s and 1910s the two discussions transpired largely independently.

Since the goal of this survey is to contrast the earlier and later periods, I also draw on two long-running genres to highlight those changes: introductory economics textbooks and political party platforms. The textbook sample (described in detail in chapter 2) provides a convenient index of shifting understandings of the role of the state in economic theory. I use these textbooks to show how the economic policy in the 1890s-1910s was largely framed in “structural” and “institutional” terms, not “data dependent” ones. By this, I mean that economists debated how the state should support the institutions and structures of the economic system, but economist did not identify a role for the state conditional on the current performance of that system. Economists who advocated the Gold Standard or low tariffs appealed to criteria of long-term principles, not short-run performance.

Similarly, I read and analyzed the presidential party platforms of the major political parties from 1892 to 1960.⁶ At each presidential nominating convention, the major parties announce a platform containing their promised agenda for the coming term. These platforms identify the major economic policy debates of each election, and proposed solutions (along with a laundry list of other topics). Through my analysis of these platforms, I show how the promises made by political parties became increasingly pitched in terms of short-term, precise, quantitative macroeconomic indicators, in stark contrast to the varieties of economic claims made in the 1890s and 1900s.

4.4 1890-1910: Tariffs, Trusts, and the Money Question

Economic debates in the 1890s-1900s were dominated by three policy issues: tariffs, trusts, and the money question. In my brief discussion of these three themes, I highlight how these debates were characterized as fundamentally structural or institutional.⁷ Advocates of

⁶These platforms are conveniently available from UCSB’s American Presidency Project at <http://www.presidency.ucsb.edu/platforms.php>. My analysis included the Democratic and GOP platforms from each election year 1892-1960, along with the Populist party in 1892 and the Progressive party in 1912 and 1924.

⁷My analysis here is not intended to contribute to the historiography of the era and for that reason my engagement with that historiography is minimal. Rather, I discuss this period to highlight contrasts with

higher (or lower) tariffs, busting (or allowing) trusts, and gold (or bimetallism) believed that these policies were unconditionally better than the alternatives. The state was understood as playing a tremendously influential role in structuring economic life, but there was not yet a sense that the state's role could or should involve responding to short-run conditions with policies justified solely in terms of those short-run conditions. Economic textbooks largely mirrored these concerns, and although business cycle theories were advanced during this period, they remained largely unconnected to government policy (Dorfman 1949: 116). To the extent that political actors or economic theorists invoked current conditions, they did so to support arguments about the long-run consequences of these policies. And they did so largely without the aid of timely aggregate statistics.

In the late 19th century, tariff duties loomed large in national politics. In the absence of an income tax, tariffs served as the primary source of revenue for the central government. For manufacturers, tariffs also served as a form of protection, insulating them from direct competition with cheaper British goods. In contrast, some farmers and urban laborers saw the tariffs as a primary source of the high cost of living. These positions were manifested in the party platforms of the Republicans and Democrats respectively. As the 1896 GOP platform put it:

We renew and emphasize our allegiance to the policy of protection, as the bulwark of American industrial independence, and the foundation of American development and prosperity. This true American policy taxes foreign products and encourages home industry. It puts the burden of revenue on foreign goods; it secures the American market for the American producer. It upholds the American standard of wages for the American workingman; it puts the factory by the side of the farm and makes the American farmer less dependent on foreign demand and price; it diffuses general thrift, and founds the strength of all on the strength of each.

In contrast, the Democrats argued that tariffs should not be used as a form of industrial policy (aimed at favoring or promoting particular industries), but should only be used to

later debates. This analysis relies on what should be uncontroversial claims about the major economic policy debates of the era, drawing primarily on the party platforms to dramatize the forms these claims took. For a recent survey of the politics of the period, see Prasad 2012.

generate the minimum revenue necessary to run the government. Their 1896 platform states:

We hold that tariff duties should be levied for purposes of revenue, such duties to be so adjusted as to operate equally throughout the country, and not discriminate between class or section, and that taxation should be limited by the needs of the Government, honestly and economically administered. We denounce as disturbing to business the Republican threat to restore the McKinley law, which has twice been condemned by the people in National elections and which, enacted under the false plea of protection to home industry, proved a prolific breeder of trusts and monopolies, enriched the few at the expense of the many, restricted trade and deprived the producers of the great American staples of access to their natural markets.

This debate — reiterated throughout the 1890s and 1900s — echoes discussions among economists back to at least Adam Smith and David Ricardo on the value of free trade and the possibility for protective tariffs to promote domestic industry. Note that the tariff was not discussed in terms of a particular state or condition of the economy, as we now understand it. Democrats argued for low and non-discriminatory tariffs, Republicans for high and differential tariffs, both on the basis of the structural effects of such tariffs, not their short-run or conditional effects. In particular, Democrats connected the tariff debate to another central policy concern of the period: trusts.

As noted in the previous chapter, the late 19th century witnessed the dramatic growth of large businesses. This growth spurred intense political concern.⁸ In 1890, Congress passed the Sherman Anti-Trust Act in a nearly unanimous vote. The Act did little to end political concern with big business. Democrats in particular expressed concern about the persistence of industrial trusts and monopolies throughout the period. The 1900 Democratic platform asserted: “We pledge the Democratic party to an unceasing warfare in nation, State and city against private monopoly in every form.” And, as noted above, the Democrats saw the tariff as intimately linked to the trust question — they accused Republicans of using the tariff to protect domestic monopolies from foreign competition. The 1900 Platform

⁸See Sklar (1988) for a history of debates over anti-trust law in this period.

continues: “Tariff laws should be amended by putting the products of trusts upon the free list, to prevent monopoly under the plea of protection.” As with the tariff question, the issue of trusts and monopolies was not a question about the short-run conditions under which monopolies should be permitted, but rather one of long term structures and institutions.

Perhaps the most iconic economic issue of the period was the money question. Bankers and farmers faced off over the issue of whether the government should hew to a simple gold standard, or should allow for the coinage of silver. In general, Democrats supported bimetallism and Republicans supported the gold standard. In 1896, the issue rose to its maximum prominence when the Democrats nominated William Jennings Bryan, who made the free coinage of silver a central issue of his campaign, and famously declared to defenders of the gold standard: “you shall not crucify mankind upon a cross of gold.” The 1896 Democratic platform linked the gold standard to a host of evils:

We declare that the act of 1873 demonetizing silver without the knowledge or approval of the American people has resulted in the appreciation of gold and a corresponding fall in the prices of commodities produced by the people; a heavy increase in the burdens of taxation and of all debts, public and private; the enrichment of the money-lending class at home and abroad; the prostration of industry and impoverishment of the people.

Here we see how long run structural issues are brought into dialogue with current conditions. Democrats argued that free silver would improve the nation’s economic system, and that the return to the gold standard had caused all manner of short-run problems. I am not arguing that economic debates of this period ignored current economic conditions. Rather, short-run conditions tended to be mobilized in favor of one or more long-run, structural solutions. Democrats here blamed rising inequality and indebtedness on a structural change (demonetizing silver) made more than 20 years earlier.

Similarly, Republicans linked high tariffs and the gold standard to short-run prosperity. In their 1900 platform, looking back on the first McKinley administration, Republicans argued that:

The expectation in which the American people, turning from the Democratic party, intrusted power four years ago to a Republican Chief Magistrate and a Republican Congress, has been met and satisfied. When the people then assembled at the polls, after a term of Democratic legislation and administration, business was dead, industry paralyzed and the National credit disastrously impaired. The country's capital was hidden away and its labor distressed and unemployed. The Democrats had no other plan with which to improve the ruinous conditions which they had themselves produced than to coin silver at the ratio of sixteen to one. The Republican party, denouncing this plan as sure to produce conditions even worse than those from which relief was sought, promised to restore prosperity by means of two legislative measures—a protective tariff and a law making gold the standard of value.

Here, Republicans blame Democrats' silver coinage for the disastrous economic conditions of the early 1890s, and argue that their own policies of returning to the gold standard and raising tariffs ensured renewed prosperity. But the point of this argument was not to claim that protective tariffs and the gold standard were the proper policies *only* when economic conditions were bad. The gold standard and protective tariffs were understood as always the best policies, and short-run performance merely provided evidence for these claims.

Economics textbooks from the era follow a similar pattern to the political debates. Discussions of the role of the state tended to focus on the state's role in organizing the conditions under which markets could function well: law and order, property rights, enforcing contracts, building roads and bridges and other public works, and managing the money supply (e.g. Bullock 1897: 154-157).

Even those economists who saw themselves arguing for an expansive role for the state framed that role in structural terms. Richard Ely, a leader in the Progressive movement and economic theorist in the very interventionist German Historical School tradition, wrote in his influential textbook that "The scope of state action is necessarily great." (Ely 1901: 254) His book devoted chapters to the role of the state in enforcing patents and copyrights, fostering industry through land grants and subsidies, protections for workers, anti-monopoly legislation, and even justifies state-run industry in the case of natural monopolies (Ely 1901:

296). Nonetheless, his book contains almost no discussion of a role for the government in responding to short-run crises with short-run solutions. His short discussion of recurrent economic crises of overproduction makes no mention of specific government responses, but rather suggests that “Whatever improves industrial society in any respect is a partial remedy” to the problem (Ely 1901: 243). Short-run crises do not call for special solutions, and there is no sense that government policy should be responsive to timely data on the current state of economic affairs.

The Panic of 1907 marked one turning point that increased discussion of short-run government responses. Financial panics were a recurrent problem in the 19th century, but previous panics were handled primarily by bankers themselves. In particular, JP Morgan was seen as playing an instrumental role in managing the fallout from the 1893 panic and in the end Morgan and the Rothschilds bailed out the US Treasury, rather than the reverse. The 1907 Panic called this system into question, as the aging Morgan was seen as incapable of continuing to provide order to the market and no one else could fill his shoes (Bruner and Carr 2007). The political debate increasingly turned to the role of the federal government in responding to financial crisis, which in turn produced some of the earliest discussions of conditional policymaking—including in the 1908 party platforms. The Democrats used the occasion to call for “an emergency currency... issued and controlled by the Federal Government, and loaned on adequate security to National and State banks” while the Republicans lauded “the emergency measures adopted by the Government during the recent financial disturbance.” The 1907 Panic shifted debates about the government’s role in managing the financial system and the money supply, beginning a discussion that would end with the creation of the Federal Reserve.

4.5 1910-1929: Unemployment, Public Works, and the Fed

Comparing the political discourse of the 1910s and 1920s to the preceding decades highlights three related dramatic shifts: changes in the discussion of the money question related to the founding of the Federal Reserve, the increasing awareness of temporary unemployment as a problem susceptible to and meriting government response, and more generally an uptick in discussions of the business cycle drawing on newly available economic statistics. The 1910s and 1920s saw the first attempts to use two of the most important tools of what would become understood as macroeconomic management: countercyclical public works to fight unemployment and open market operations to control interest rates. These techniques were not yet combined into a theory of fiscal and monetary policy (as they would be in the 1930s and 1940s), but the period does mark a sharp break with the preceding era in terms of the recognition of a potential for short-run government intervention justified in terms of temporary conditions.

The history of discussions of unemployment perhaps most fully highlights these shifts. As Garraty (1979: 4) notes, the term unemployment itself comes into widespread usage in English only in the 1890s (see also my analysis in chapter 2), although discussions of the unemployed go back a few decades earlier. Unemployment as a concept emerged in an era that saw simultaneous heightened levels of worker independent (formally free labor) and dependence (with an increasing proportion of the population working as wage laborers, and a decline of self-employment). Connected to discussions of unemployment were issues of measurement and of the appropriate government responses.

The first efforts to measure the number of unemployed took place in the late 19th century, led by Carroll D. Wright (first as chief of the Massachusetts Bureau of Statistics of Labor and then as the first US Commissioner of Labor). The 1880 Census was the first to ask about joblessness, but the data from this question were practically useless for policymaking, as they were never even processed (Sautter 1991: 41-45). Questions on unemployment were asked in the next three Censuses, but the answers were published years later, and seen

as unreliable. The 1920 Census did not even bother to ask. These early efforts, and others undertaken by various state bureaus of labor, were seen as unsatisfactory by all involved, but did lead to growing awareness of at least the problem of measuring unemployment, if not consensus on the magnitude or dynamics of unemployment itself (Sautter 1991, Card 2011). Interest in the problem of unemployment grew in the 1920s even without a firm grasp of its size. Famously, when then-Secretary of Commerce Herbert Hoover called a Conference on Unemployment in 1921, experts debated the level of unemployment before voting on a consensus estimate of 3.5-5.5 million.⁹

In this period, debates around policy responses to unemployment also increased. In the UK, the 1909 report by progressive economist William Beveridge titled *Unemployment: a Problem of Industry* led to the creation of the country's unemployment insurance system in 1911. Beveridge framed unemployment as a question of the industrial system as a whole rather than an issue with individual workers: "the inquiry must be one into unemployment rather than into the unemployed" (Beveridge 1909, cited in Garraty 1979: 137). The American economist Wesley Mitchell adopted a similar perspective in his influential 1913 book *Business Cycles*, treating the rise of unemployment as one feature of the larger problem of business cycles and industrial instability, and one that government should address through its efforts to mitigate the business cycle.

Debates about the government's role in responding to unemployment recurred throughout the period. Although no national measures were adopted in the 1910s, various politicians and activists suggested every major form of governmental response that would later be attempted: unemployment insurance (like that adopted in the UK), state-run labor exchanges to help the unemployed find work, public works to directly employ the unemployed, and better measurement to guide the timing of assistance (Sautter 1991: 119-120). In the 1920s, these debates only increased. Following the 1920-1921, recession, Herbert Hoover organized the above-mentioned Conference on Unemployment. In addition to de-

⁹Report of the President's Conference on Unemployment, 1921, p. 19.

bating measurement issues, the Conference also discussed proposed solutions. One proposal involved planning public works such that government would spend the most money when unemployment was highest: “In a growing country like the United States the aggregate volume of public works of cities, counties, States, and of the Federal Government is so great that if a larger proportion were executed in years of depression than in years of active industry a powerful stabilizing influence would be exerted.”¹⁰ The idea had wide political appeal: the government was not proposing new or unnecessary works, but simply shifting by a year or two the timing of large projects to coincide with periods of high unemployment.

In 1928, FG Dickinson of the University of Illinois published a study which attempted to quantitatively measure “the possible maximum power of public construction to prevent cyclical unemployment” (Dickinson 1928: iii) from the scanty available data on the magnitude of both unemployment and public construction from the 1920s. Dickinson found that if the government had managed to perfectly allocate public works, it would have been able to make up for all of the declines of factory wages in bad years. Although this result seemed encouraging, Dickinson was quick to note that practical difficulties would have made perfect countercyclical planning unrealistic: the business forecasters of the 1920s (discussed in chapter 3) were simply not accurate enough to use as the basis for countercyclical planning. Dickinson imagined a future in which a clerk at the Bureau of Labor Statistics would watch an improved index of employment to determine the precise timing of countercyclical public works in terms of measured aggregates: “His duty would be to watch the index of employment and immediately inform the numerous public officials throughout the country whenever the index approached 5 per cent above or below the average of the preceding years.” (Dickinson 1928: 200)

Although Dickinson’s study, and similar proposals in the 1920s, did not yet involve the macroeconomic frameworks developed in the 1930s (with debates over multiplier effects,

¹⁰*Report of the President’s Conference on Unemployment*, 1921, p. 96.

and a broader understanding of fiscal policy than just public works), these discussions offer a clear example of a proposal that obligated the government to take responsibility for a precisely measured short-run economic outcome and respond with policies justified in terms of current aggregate economic conditions.

These new responsibilities appeared in political party platforms and other venues where politicians made claims about the responsibility of the state. For example, the 1924 Republican platform describes the situation in 1921, when the Republicans came to power, in terms of short-run economic performance, and unemployment in particular: “When the republican administration took control of the government in 1921, there were four and a half million unemployed; industry and commerce were stagnant; agriculture was prostrate; business was depressed; securities of the government were selling below their par values.” Note the 4.5 million unemployed figure—the midpoint of the guess arrived at by debate at Hoover’s unemployment conference. By 1928, the Democratic platform called for precisely the sort of countercyclical public works program proposed by Dickinson:

We expend vast sums of money to protect our people against the evils of war, but no governmental program is anticipated to prevent the awful suffering and economic losses of unemployment. It threatens the well-being of millions of our people and endangers the prosperity of the nation. We favor the adoption by the government, after a study of this subject, of a scientific plan whereby during periods of unemployment appropriations shall be made available for the construction of necessary public works and the lessening, as far as consistent with public interests, of government construction work when labor is generally and satisfactorily employed in private enterprise.

A year before the 1929 crash and the beginning of the Great Depression, politicians were already debating the creation of large-scale national programs to respond to unemployment, guided by research (“after a study of the subject”).

At the same time as economists debated how best to measure and respond to unemployment, and politicians began to accept responsibility for it, transformations in the institutions of monetary policymaking dramatically increased the capacity of the government to intervene in a timely fashion. In 1913, after years of debate following the Panic of 1907,

Congress passed the Federal Reserve Act.¹¹ The Federal Reserve system was designed to be a private-public hybrid, with banks scattered across the nation, accountable to local business interests, but also authorized to act on behalf of the nation in order to prevent financial panics. In the 1910s and 1920s, the bankers of the Federal Reserve began to understand their capacity to influence short-run economic performance through the buying and selling of government securities. Conceived as a system for promoting financial stability, the Fed soon took on a broader role in managing economic conditions.

In the 1910s, individual reserve banks began to buy government debt simply as a source of revenue in so called “open market operations” (buying and selling government debt on the open market). Benjamin Strong of the NY bank realized the possibility for the Fed to influence interest rates more broadly through open market operations as early as the 1910s (D’Arista 1994: 21). This possibility was put into practice beginning in 1919 when the Fed first uses open market operations as a policy tool rather than a source of revenue, aided by the dramatic increase in the size of the market for US debt following the government’s increased spending during World War I (D’Arista 1994: 80). By 1923-1924, the Fed’s Open Market Investment Committee, under Benjamin Strong’s leadership, began to take credit for using open market operations to help shorten the length of the recession by easing money (D’Arista 1994: 116) The Fed’s role was recognized even in the party platforms, as when the Democrats denounced the Fed’s policy in 1924 as unduly contractionary:

We denounce the recent cruel and unjust contraction of legitimate and necessary credit and currency, which was directly due to the so-called deflation policy of the republican party, as declared in its national platform of June, 1920, and in the speech of acceptance of its candidate for the presidency... The contraction bankrupted hundreds of thousands of farmers and stock growers in America and resulted in widespread industrial depression and unemployment. We demand that the federal reserve system be so administered as to give stability to industry, commerce and finance, as was intended by the democratic party, which gave the federal reserve system to the nation.

Here we see the Democrats linking the federal reserve and its management of credit and

¹¹For detailed histories of the Federal Reserve, see Greider 1987, D’Arista 1994.

currency to both financial stability *and* unemployment. And we see another link between conditional policymaking and short-run economic conditions, although it is worth remembering that the federal reserve of this period was both more fractured (lacking a central governing board) and more controlled by local business interests, and thus only partially a creature of the state.

Open market operations and countercyclical public works were two of the most important forms of conditional policymaking debated and (to varying degrees) attempted in this period and show a significant break from early policy debates around tariffs, the gold standard, and trusts (all three of which remained objects of political debate). Beyond these particular policies, the 1920s also saw the articulation of some of the first broad visions of the state's role in planning the economic system to promote stability (Metcalf 1975, Alchon 1985, Barber 1985). Major figures in these "New Era" debates included economists Wesley Mitchell and Edwin Gay at the newly-founded National Bureau of Economic Research. Working with and sometimes on behalf of Secretary of Commerce Herbert Hoover, Mitchell and Gay oversaw an ambitious research program throughout the 1920s which aimed to measure the business cycle and theorize interventions in order to moderate it. Hoover's vision of national planning involved businesses taking advantage of this newly available statistical data to better plan their investment and production decisions and, in so doing, moderate the ups and downs of the business cycle. The government's main role would be to facilitate businesses planning on their own behalf, through the distribution of up-to-date information in publications like the *Survey of Current Business* (first published by the Commerce Department in 1921).

By December, 1929, a month after the great Wall Street Crash, the *Survey of Current Business* claimed significant progress in its mission: "While it may be too early to say that the utilization of business data has entirely eliminated the business cycle, there is agreement today among business leaders everywhere that the wider use of facts will mitigate in a large degree many of the disastrous effects of the one-time recurrent business cycle." (quoted in

Barber 1985: 82) Somewhat ironically, Herbert Hoover may bear more responsibility than any other single figure for the growing belief that government could and should manage the business cycle. Alchon (1985: 169) summarizing the importance of the 1920s planning debates argues that, “New Era techno-corporatism was thus an important chapter in the larger development and acceptance of the idea that statistical analysis is the appropriate basis for public policy; that the business cycle can be understood and controlled through such analysis; that public works and improved business management are legitimate countercyclical measures.” Competing visions of planning advanced in the wake of the seeming failure of Hoover’s model would go much further to embrace a strong role for the central government as the primary force responsible for moderating the business cycle.

4.6 1929-1946: From Pluralist Planning to Macroeconomic Management

In the 1930s, “planning” was everywhere. Hoover and the NBER had laid the groundwork for the debate by measuring the business cycle and by offering one vision of how those measurements might be used (voluntarily) by scientifically-run businesses to mitigate the cycle. As the Great Depression raged, competing notions of planning were debated, and the early aspects of the New Deal incorporated features of many of them. These competing notions relied on different kinds of data, different politics, and different objects of governance. By the end of the 1930s, macroeconomic management would be ascendant, with John Maynard Keynes as its foremost theorist, and FDR and his administration avowed converts. The data needed to execute this macroeconomic vision also came together in this period, especially national income statistics (as described in detail in the previous chapter). The major tools of government countercyclical action discovered in the 1910s-1920s would be incorporated together into the theory of macroeconomic management under the headings of fiscal and monetary policy. In the 1940s, the center of the debate was the ques-

tion of *how* the government should use macroeconomic tools to manage the economy, not whether it should. As the federal government took an increasingly active role in managing the economy, so too did politicians increasingly articulate a governmental responsibility for the economy. In 1946, Congress signed the Employment Act, which clearly announced the federal government's responsibility for macroeconomic performance.

The intellectual history of economic policy in the Great Depression has been well-told.¹² My short summary here highlights how a specifically “macroeconomic” vision emerged in this period, how it relied on newly available aggregate statistics, and how it offered a middle-ground between doing nothing and forms of planning that were seen as more invasive. Balisciano (1998) usefully identifies four visions of economic planning debated in the 1930s: social management planning, technical-industrial planning, business economy planning, and what came to be called macroeconomic planning. Each recognized the responsibility of the government for short-run economic performance, but suggested different policy responses linked to different political interests and theoretical understandings of the underlying economic problems. Their contradictions help to explain why the early New Deal programs seemed incoherent.

These four visions provided different diagnoses of the source of economic problems, and thus different economic prescriptions (Balisciano 1998: 158). Social management planners believed that the Depression resulted from a lack of coordination and a reckless pursuit of profit; they proposed planning boards in the style of the World War I Industries Board to better manage the economic system. Technical-industrial planners identified a similar problem (lack of rational control of industry, lack of balance) but sought a solution in the techniques of scientific management, proposing that technical experts (in industry and government) should be given the authority to run industry in a more rational manner. Business economy planners offered a vision most similar to the 1920s Hooverian associationalism; they suggested that business simply lacked the information to prevent cycles

¹²For a sampling focused on the United States, see Tobin 1966, Stein 1969, Barber 1985, Lee 1990, Brinkley 1996, Reagan 1999, Stapleford 2007, Alacevich et al 2015.

of overproduction and crisis, but that with some increased industrial combination (cartels) and better data (provided by the state), business would be able to plan production on its own. Finally, the macroeconomic planners identified imbalances between aggregate saving and spending as the source of crises and depression, and proposed countercyclical spending to stimulate the economy. All four visions influenced the New Deal, as Balisciano (1998: 166) summarizes through the example of the National Industrial Recovery Act, one of the most sweeping early New Deal acts:

Business economy planners got the suspension of antitrust laws by allowing businesses to enter into collusive price contracts. Social management planners got the promise of centralized industrial control, with government as the main partner in a collaborative union of economic actors. Technical-industrial planners got expert technicians in positions of authority; the act established a planning staff of economists and other professionals who would ostensibly chart and guide industrial development. Macroeconomic planners got Title II, the act's \$3.3 billion public works provision.

This pluralist approach to planning collapsed over the next four years. By 1935, when major portions of the NIRA itself were ruled unconstitutional, “none mourned its passing” (Balisciano 1998: 167). The failure of the NIRA and related planning efforts struck a major blow against forms of planning that relied on business cooperation and government planning through detailed regulation at the industry level. At the same time, the technical sophistication and appeal of macroeconomic approaches increased, while competing approaches premised on more fine-grained regulation at the level of industries floundered due to political resistance and a lack of data (Lee 1990, Stapleford 2011).

In 1934, as detailed in the previous chapter, the Department of Commerce published official estimates of national income. These estimates were quickly turned into an annual and quarterly series, which provided detailed information on aggregate economic performance used by macroeconomic planners to advocate for specific targets for government spending. In early 1935, Lauchlin Currie, an economist working at the Treasury Department, wrote an influential memo titled “Comments on Pump Priming” (reprinted as Currie 1978). The

memo drew on the newly available official national income data to arrive at quantitative estimates of the amount of money the government would need to spend make up for the declines of the Depression. Based on comparisons between 1929 and 1934, Currie argued that New Deal deficit spending “was too small to have significant effect on the trend of business in the conditions then prevailing.” (Currie 1978: 527) The deficit would need to nearly double from its already historically large size to appreciably alter the course of the Depression. Although Currie did not yet incorporate the concept of a multiplier (the idea that government spending in a recession increases national income some multiplicative factor, through secondary employment effects), his memo illustrates nicely the emergence of a calculative regime that treated fiscal policy as a tool for managing the aggregate economy in quantitative terms.¹³ Public works were no longer understood as simply a means of employing the unemployed, but part of a broader concept of fiscal policy to prime the pump of the economy.

In the late 1930s, the place of macroeconomic management was cemented by the publication of Keynes’ *General Theory of Employment, Interest, Money* and the recession of 1937 (Stein 1969). As the title suggested, Keynes (1936) offered a unified account of the relationships between national income, employment, and interest rates. The debate over the true meaning of Keynes’ theory has raged ever since, but his work inspired a generation of economists to focus on the problem of macroeconomic management (to the exclusion of older visions of planning) and build a shared language and body of statistics for understanding the economy. Keynes’ theory was seen as making sense of the renewed downturn in 1937-1938, when the Roosevelt administration’s balanced budget led to a sharp recession. This recession convinced Roosevelt to more fully adopt fiscal policy as a tool for managing the economy (Stein 1969, Brinkley 1996). This recession was visible to policymakers as it unfolded, through improved data collection on both employment and national income.

These transformations in the conceptions of economic governance were visible in the

¹³On the history of the multiplier in economic theory, see Boserup 1969, Dimand 1994. At a minimum, the idea was floating in the air in the late 1920s, and published in some version by Kahn (1931).

party platforms of the Depression. For example, the 1936 Democratic platform boldly asserted government's responsibility for unemployment:

We believe that unemployment is a national problem, and that it is an inescapable obligation of our Government to meet it in a national way.

Due to our stimulation of private business, more than five million people have been reemployed; and we shall continue to maintain that the first objective of a program of economic security is maximum employment in private industry at adequate wages. Where business fails to supply such employment, we believe that work at prevailing wages should be provided in cooperation with State and local governments on useful public projects, to the end that the national wealth may be increased, the skill and energy of the worker may be utilized, his morale maintained, and the unemployed assured the opportunity to earn the necessities of life.

Public works to fight unemployment had moved from a marginal debate in the 1910s to the center of public discourse and policy in the 1930s. Appropriate policy had become increasingly understood as conditional on the state of the economy, and changes in the economy were in turn attributable to successful policy. In both 1936 and 1940, Democrats would argue for the success of their programs in terms of the rebounding of national income and the reduction of unemployment. For example, the 1940 Democratic platform claimed: "The full force of our policies, by raising the national income by thirty billion dollars from the low of 1932, by encouraging vast reemployment, and by elevating the level of consumer demand, has quickened the flow of buying and selling through every artery of industry and trade."

The failure of the Federal Reserve to prevent the crash of 1929 and ensuing financial chaos led to reforms of the system. A series of laws passed under FDR centralized authority over the Federal Reserve in the hands of a central Board of Governors largely appointed by the President, not the bankers themselves. This board oversaw a unified monetary policy through the Federal Open Market Committee, which took responsibility for buying and selling government debt and, in so doing, affecting interest rates. Monetary policy was now coordinated through a single body who took as its task stabilizing the economic system as

a whole, not just the financial system (Greider 1987: 313).

The government's role in managing the short-run economy only grew during World War II. As discussed in the previous chapter, macroeconomic tools, and national income statistics in particular, played a key role in aiding the allied mobilization through both management of wartime finances and direct planning of military production. In *How To Pay For the War*, Keynes (1940) laid out a vision of wartime macroeconomic management for the UK which involved a quantified combination of government borrowing, taxing, and printing currency in order to minimize inflation while maximizing employment and production. In the US, Simon Kuznets and Robert Nathan shaped the overall plans for arms production and the size of the American military through calculations based on national income statistics. These efforts, and other wartime successes, earned economists increased prominence in public debates, and especially in the administrative branch of the federal government (Bernstein 2001).

Even before the war was won, economists began to debate *demobilization*, and how the US and UK could secure full employment and prevent a post-war depression. For example, Alvin Hansen, a leading American economist best remembered for his popularization of Keynes and co-creation of the Hicks-Hansen ISLM model, wrote a report for the National Resources Planning Board in 1942 titled "After the War — Full Employment." Hansen, espousing a relatively conventional position at the time (cf. Samuelson 1944), argued that major wars are usually followed by major slumps and that the government would need to plan for the peace well before the war had ended.¹⁴ Hansen's proposal, and others like it, recognized the potential for the enlarged national government to play a strong role in stabilizing the economic system through some combination of active intervention (countercyclical public works, monetary policy) and automatic stabilizers (unemployment insur-

¹⁴Somewhat ironically, the predicted post-war depression never came after World War II. This prediction was arguably the first major failure of the American Keynesians (see Sapir 1949 for an immediate retrospective), and echoes the difficulty faced by macroeconomic managers to present. While improvements in measurement had made present economic conditions much more visible than in the 1920s, forecasting remained and remains very difficult.

ance, progressive income taxes). Stein (1969) documents in exhaustive detail the debates in this period as different economists and political actors champion variations on fiscal and monetary policy. The broad outlines of this discussion feel familiar: should fiscal policy act primarily through increased expenditures and public works or raising and lowering taxes? Should monetary policy or fiscal policy be the first resort when a recession looms? Which political actors should decide the timing and extent of interventions?

In 1944, both political parties included employment policy in their party platforms, although the subtle differences between the two platforms are revealing. The Democrats summarized their economic platform simply: “To speed victory, establish and maintain peace, guarantee full employment and provide prosperity...” The Republicans eschewed the contentious phrase “full employment,” but still acknowledged government’s role in supporting the economy:

We shall adopt a program to put men to work in peace industry as promptly as possible and with special attention to those who have made sacrifice by serving in the armed forces. We shall take government out of competition with private industry and terminate rationing, price fixing and all other emergency powers. We shall promote the fullest stable employment through private enterprise.

The “fullest stable employment” was not a promise of guaranteed jobs, but it did represent an acceptance of the government’s ultimate role managing the economy. In 1945-1946, these debates culminated with the eventual passage of the Employment Act of 1946.¹⁵ The act began life as the Full Employment Bill, but as in the discrepancy between the 1944 platforms, “full employment” proved contentious for conservative factions, who put up staunch resistance to the government adopting such a strong mandate. Full employment was also difficult to define — early drafts of the bill included attempts at a technical definition written by economists, but these were seen as unsatisfactory and removed. In the negotiations over the act, the more liberal factions in Congress sacrificed the explicit adoption of full employment as a policy goal (and especially the use of federal investment as

¹⁵Bailey (1950) documents the political maneuverings surrounding the proposal, modification, and eventual passage of the Act in detail.

the primary method of maintaining full employment) in order to pass a bill establishing an economic planning mechanism in both the legislative and executive branch. The Employment Act created the Council of Economic Advisers, who are tasked with helping to draft the Economic Report of the President, and Congress's Joint Committee on the Economic Report, which was to respond to the report and propose appropriate legislation. The Employment Act also offered a full-throated embrace of the government's role as manager of the economy, even if it did not obligate particular measures:

The Congress hereby declares that it is the continuing policy and responsibility of the Federal Government to use all practicable means ... to coordinate and utilize all its plans, functions, and resources for the purpose of creating and maintaining, in a manner calculated to foster and promote free competitive enterprise and the general welfare, conditions under which there will be afforded useful employment opportunities, including self-employment, for those able, willing, and seeking to work, and to promote maximum employment, production, and purchasing power.

Maximum employment corresponded to a low unemployment rate, maximum production to a high GNP, and maximum purchasing power to a low inflation rate. Although the means and extent of government's responsibility were still hotly debated, the basic terms of the debate were set. The Act also marked the extension of governmental responsibility from responding to exigent, temporary crises (the Great Depression, the need for wartime mobilization) to an embrace of the government's role in managing fluctuations in "normal" times.¹⁶ Government's role in managing the normal peacetime economy would continue to expand in the following decades, and would subtly transform as growth joined stability as a primary objective of macroeconomic management.

¹⁶I thank Greta Krippner for this characterization.

4.7 1946-1964: Stability & Growth

Over the following two decades, the debate would continue over the proper *means* for government to manage the economy, and the appropriate targets or goals.¹⁷ Perhaps the biggest changes in this period would be the importance of monetary policy (deemphasized in the 1940s), the rise of tax cuts as the preferred means of executing fiscal policy, and especially the emergence of economic growth as the most salient aim of economic policy (or at least, co-equal with low unemployment and inflation). Economic growth held an especially privileged place in American politics because it brought together Cold Warriors concerned with a “growth gap” and liberals concerned with ending poverty and raising standards of living. Growth offered the possibility of everyone getting what they wanted, and thus politicians in this period began to promise growth (Davis 1988, Collins 2000, Yarrow 2010). I focus here on the emergence of growth to highlight the unique role played by national income statistics as measurements of the size of the economy. Most importantly for my story, not only did politicians begin to emphasize growth, they did so in precise, quantitative terms. “Economic growthmanship” (as Collins 2000 called it) involved the promising of higher rates of growth *today*, based on the superior economic policies of particular candidates.

In the 1930s and 1940s, economists (and the public) were largely worried about economic *stability*; by the 1950s the discussion would shift to focus more on growth. In the earlier period, the overriding question had been how to prevent recurrent depressions and financial panics, and whether or not it was possible to stabilize the economy without enacting some form of socialist or communist government. Some economists, most prominently Alvin Hansen, went so far as to claim that the economy was entering a period of “secular stagnation,” characterized by increased unemployment and no increases in output (see Rosenof 1998 for details). In the 1950s and 1960s, stability seemed surprising easy to assure and predictions of secular stagnation seemed naive. The post-WWII depression pre-

¹⁷See Stein (1969) for the blow-by-blow.

dicted by Keynesians in the mid-1940s never arrived. In the 1950s, the economy was booming; yearly real GNP growth exceeded 5% in 1950, 1951, 1955, and 1959, with only two isolated years of mild recession.¹⁸ This growth was not simply discussed by economists and statisticians. As Yarrow (2010: 64) notes, the short-run rate of growth of the economy became a point of national pride and intense discussion: “Headlines, congressional hearings, scholarly and popular reports, and political rhetoric pounded into Americans the radically new idea that they should be deeply concerned about whether U.S. GNP rose by 3 percent or 5 percent this year.”

This measured growth coincided with the emergence of theories of growth in economics. As noted in chapter 2, and discussed in detail in Warsh (2006), economists had few ways of talking about economic progress in the late 19th and early 20th century, and even fewer theories that explicitly examined growth (in productivity, output, or standard of living). In the 1940s and 1950s that changed, most famously with the publication of Robert Solow’s neoclassical growth model (Solow 1956) and related empirical work (Solow 1957), which in turn drew on newly available employment and GNP data (see Boianovsky & Hoover 2014 for details). Solow himself would go on to serve on the Council of Economic Advisers in 1961-1962 under President Kennedy, in a key period when Kennedy would begin to push for a large tax cut in order to promote economic growth.

The overt politics of growth is easily visible in the party platforms of the era. For example, Democrats in 1956 connected economic growth directly to the Cold War:

The Republican Party has not grasped one of the dominant facts of mid-century — that the growth of productive power of the Communist states presents a challenge which cannot be evaded. The Democratic Party is confident that, through the freedom we enjoy, a vast increase in productive power of our Nation and our Allies will be achieved, and by their combined capacity they will surmount any challenge.

The fear of the Soviet Union surpassing the US economically was visible everywhere

¹⁸Source: BEA Table 1.7.1. “Percent Change From Preceding Period in Real Gross Domestic Product, Real Gross National Product, and Real Net National Product.”

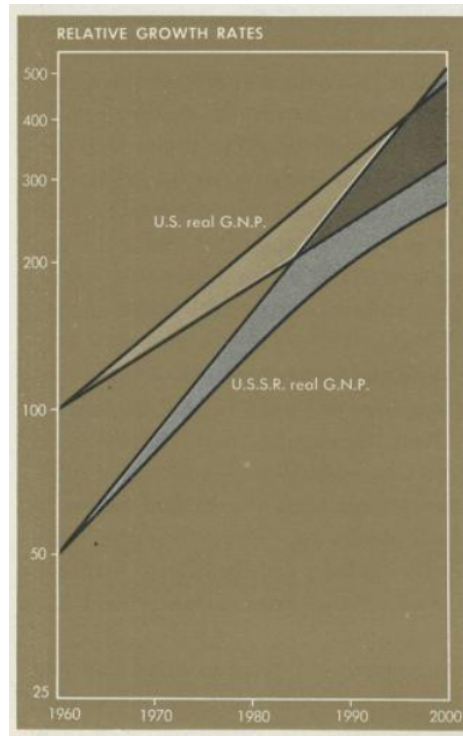


Figure 4.3: “America leads Russia, but will the gap narrow?” From Samuelson (1961), also cited in Levy and Peart (2009).

from political statements like these to economic textbooks themselves. As Levy and Peart (2009) document, American economics textbooks of the 1960s were fascinated and terrified by Soviet economic growth. In the 1961 edition of his best-selling textbook, Samuelson famously projected that the Soviet Union would overtake the US economically in between 23 to 36 years (see Figure 4.3). For the next 20 years, Samuelson’s textbook would continually place the catchup time of the Soviet Union somewhere 20 to 30 years in the future.

The 1956 Democratic platform also illustrated the *specificity* of growth promises. The Democrats claimed their economic program would obtain, by 1960, “A 500 billion dollar national economy in real terms.” The Democrats lost and President Eisenhower was re-elected. In 1960, the Republicans would note in their platform precisely what Eisenhower’s second administration accomplished: “Despite the lamentations of the opposition in viewing the economic scene today, the plain fact is that our 500 billion dollar economy finds more Americans at work, earning more, spending more, saving more, investing more,

building more than ever before in history.” The Democrats and Republicans met on the field of competing claims to provide economic growth, in the shared language of GNP and the size of the economy. Meanwhile, the Democrats alleged in 1960 that the Republican record was still insufficient to best the Soviets: “They have slowed down the rate of growth of the economy to about one-third the rate of the Soviet Union.” The Democrats went on to promise 5% growth under their administration, along with improved stability, lower inflation and unemployment.

Even before President Kennedy took office, he was apparently nervous about how his administration could meet this ambitious target.¹⁹ Kennedy consulted top economists like Paul Samuelson, and eventually formulated an ambitious plan centered around a large tax cut designed to stimulate investment (rather than increased spending). Kennedy first proposed a stimulative tax cut in 1962, but the final bill was only passed in 1964 under President Johnson. The tax cut was not sold as countercyclical stimulus needed quickly to fight an already existing recession, but a correction aimed at inching a well-functioning economy towards higher growth and higher employment. As Davis (1988: 345) documents, both the CEA and the Treasury Department agreed on the size of the needed tax cut, drawing on macroeconomic data to arrive at a figure of \$7-10 billion. This cut would reduce the “full employment surplus,” the amount that the Federal Government’s revenue would exceed its spending *if* full employment were achieved, a key metric in the 1960s for debates around the Federal Government’s role in encouraging or discouraging growth.²⁰ In this sense, the tax cut plan relied on new macroeconomic understandings that linked short-run conditions (full employment) and long-run concerns (growth).

Davis (1988) documents the debates surrounding the proposal and passage of the tax cut in exhaustive detail. Part of the reason for the act took more than a year to pass through

¹⁹See Stein 1969, Davis 1988, and Collins 2000 for detailed discussions of the Kennedy administration’s fiscal policy.

²⁰That is, if the Federal Government were running too large a surplus under conditions of full employment, it was seen as discouraging growth. The initial tax cut proposals aimed to reduce but not entirely eliminate the full employment surplus. See Costantini (2015) for a history of debates over evaluating the government’s budget in terms of the business cycle.

Congress was that the proposal combined across the board cuts in tax rates with more politically controversial reforms in the tax cut to close loopholes, cap itemized deductions, change the standard deduction, and more. Despite relative bipartisan support for across-the-board tax cuts to stimulate growth, the exact details of tax reform were bitterly fought.

In some sense, then, the debate over the Kennedy tax cut marks a slight return to concerns with long-run structural problems that dominated the 1890s-1910s, but recast with the quantitative precision of macroeconomic statistics, and focused on the *size* of fiscal policy and how it relates to the *size* and growth of the economy. Managing the economy now linked both short-run policy aimed at controlling unemployment and inflation and long-run concerns about economic growth into a unified framework of measurement and intervention. By the 1960s, the conditions were in place for the state to make incompatible, or at least difficult to achieve, precise economic promises, and thus call into question its own legitimacy, setting the stage for the triple crisis of the 1970s.

4.7.1 Coda

The idea that a President would be responsible for the overall growth rate of the economy emerged in the 1950s and 1960s but it persists to this day. In the run-up to the 2016 Republican primary candidate Jeb Bush published an op-ed titled “My Tax Overhaul to Unleash 4% Growth.”²¹ Jeb Bush may be a conservative Republican, but he still campaigns in the discourse of managing the economy, and especially the vernacular of growthmanship developed more than 50 years ago.

4.8 Discussion and Conclusion

In this chapter, I documented a transformation in the political rationalities of economic governance in the first two thirds of the 20th century. In the late 19th century, debates over

²¹Available at <http://www.wsj.com/articles/my-tax-overhaul-to-unleash-4-growth-1441754195>.

the government's role in economic life took on a structural or institutional character. The dominant issues of the day concerned how government could best establish the conditions for prosperity through the maintenance of the proper institutions to support trade and industry. In the early 20th century, attention turned increasingly to short-run concerns that demanded policy responses framed not in terms of their long-run superiority but rather in terms of their capacity to meet a need of the moment. "Managing the economy" in the 1930s and 1940s meant using taxes, spending, and monetary policy to promote employment and stem inflation. In the 1950s and 1960s, managing the economy expanded to encompass growth as a particular goal of policy, alongside maintaining economic stability and preparing to fight recessions as they occurred.

In his presidential address to the American Economics Association, former Chair of the CEA under Eisenhower Arthur Burns (1960: 1) nicely summarized the transformation documented above:

The American people have of late been more conscious of the business cycle, more sensitive to every wrinkle of economic curves, more alert to the possible need for contracyclical action on the part of government, than ever before in our history. Minor changes of employment or of productivity or of the price level, which in an earlier generation would have gone unnoticed, are nowadays followed closely by laymen as well as experts.²²

No longer was the state simply aiming to create a propitious environment for capitalist growth, but rather was judged according to narrow performance criteria: 4% unemployment, 5% GNP growth, 2% inflation and so on. While the optimal targets and policy instruments were debated throughout the postwar era, the sense that the state was obligated to meet those short run targets was not. The success or failure of these policies were measured in timely, official, standardized ways and reported to the public at large. Precise macroeconomic outcomes became the responsibility of the state and key elements of campaign promises. Macroeconomic management had arrived.

This analysis sheds light on the crises of the state that emerged in the 1970s. First, in

²²Also quoted in Collins (2000: 43).

a narrow sense, this chapter identifies the conditions of emergence of the tax and spending policies that led to the fiscal crisis of the state were in some part derived from the macroeconomic theories of the time. Second, in a somewhat broader sense, this chapter showed how the political responsibilities which governments failed to meet in the 1970s only took shape in the 1930s and 1940s. At the same time as “the economy” itself crystallized as a sociotechnical object, so too did the state’s responsibility for managing that economy.

Taking a longer view of the transitions in the state’s role in economic life also suggests shifts in the notion of *laissez-faire*. In 1900, arguments against governmental intervention in economic life were arguments against tariff favoritism or industrial policy. By the 1960s, arguments against government intervention took the form of rejections of counter-cyclical fiscal policy to fight recessions. In some sense, “the economy” and its management emerged as a third way between the extremes of unfettered capitalism on one hand, and full-out socialist planning on the other. By acting on the economy, the state could fulfill its obligations to high economic performance without running afoul of the technical (cf. Scott 1998) or political difficulties of more invasive forms of intervention. Yet this move did not long satisfy critics of state intervention. A full exploration of the pushback against “managing the economy” is outside the scope of this chapter, but at a minimum this argument suggests that the history of arguments for *laissez-faire* must be understood against the backdrop of shifts in the political rationalities of economic government.

This chapter makes relatively minimal claims about the political forces that spurred the adoption of particular policies or rationalities. Broadly, I showed that new forms of measurement were necessary preconditions for government accepting short-run responsibilities for small movements. In the absence of routine, official, standardized macroeconomic statistics, it is difficult to see how, say, the politics of growthism could have emerged and taken the form it did. In making this argument, I bracketed the detailed politics of particular moments, and thus am incapable of making claims about the relative importance of social movement actors, party elites, business interests, state officials, and economists

themselves. Many of these stories are well-told for particular moments, such as the detailed histories of the early and late New Deal which document the shifting policy frameworks and political coalitions that eventually led to the decline of programs like the National Recovery Administration and the embrace of explicitly Keynesian frameworks. A more detailed examination of other similar turning points might add to our understanding of alternative formulations of state responsibility for economic outcomes, alternative policy approaches for achieving economic goals, and why those alternatives did not win out.

As noted in the introduction, political scientists have increasingly linked short-run economic performance and electoral outcomes (see Healy and Malhotra 2013 for a recent review). My historical account suggests that this link may have changed in the first half of the 20th century. Although research on retrospective economic voting has grown aware of the possibility that economic voting itself may be contingent on other factors (Anderson 2007), thus far researchers have not focused on historical transformations — events, in Sewell’s (2005) useful terms — so much as mediating factors understood in an experimental temporality (for a partial exception, see Lin 1999). Future work on the links between macroeconomic conditions and economic voting should pay extra attention to the distribution and consumption of information on economic conditions. Put bluntly, we would expect voters to react differently before and after macroeconomic indicators became widely available and understood.

One recent finding generally supports this hypothesis. Kayser and Leininger (2015) find that election forecasts perform better when they rely on the economic data available at the time of the election, rather than the revised data produced months later. In other words, “voter perceptions of economic growth are influenced more by media reports about the economy, which are based on initial economic estimates, than by the actual state of the economy” (Kayser and Leininger 2015: 1). The same logic underlying this finding would suggest that voters would respond differently to economic conditions in 1900 (before the availability of timely macroeconomic aggregates) than in 1960. Future research

could explicitly account for the information available at different historical moments, perhaps drawing on a closer reading of the historical record to see which indicators (if any) politicians themselves pointed to in making political promises and assessing the success or failure of policies. In so doing, this research could help further identify how politicians and the public have understood the state's role in managing economic life over the past century.

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CHAPTER 5

How Much is a Housewife Worth?

This chapter shows how the economy as current enacted is an irreducibly market object. I focus on the history of debates over counting unpaid labor, primarily understood as women’s housework. Tracing the history of partial attempts and near misses demonstrates that housework could be included without fundamentally disrupting “the economy,” but it would have to be included on market terms. The practical and epistemic justification for national income statistics relied on markets as truth-telling devices, but flows of money and resources are not confined to market transactions, and thus in order to accurately measure the whole economy, national income statistics are constantly challenged to go beyond the market.

5.1 Introduction

“This treatment, whereby commercial products are valued at market price, government services are valued at cost, and unpaid household activities are simply ignored, is not a matter of principle but of practical convenience. It can be defended, therefore, only on practical grounds.” — Sir Richard Stone, 1984 Economics Nobel Prize Lecture

The previous three chapters described the formation of the economy as a sociotechnical object, and how the crystallization of the economy transformed how we think about and act on the economic system. This chapter begins to explore what was left out of the national income statistics, and in turn, the economy. In chapter 3, I discussed in detail the difficult work that was required to assemble satisfactory national income statistics and

make them usable for planning in both war and peace. In accomplishing that task, national income statisticians faced difficult tradeoffs between theoretical desiderata and practical constraints. This chapter focuses on one particular form that tension took, one that has never had a complete resolution: the treatment of unpaid housework.

Feminist economists have long criticized the exclusion of unpaid housework from national income statistics (Folbre 1991, Waring 1999). In particular, they argue that excluding unpaid housework distorts the measurement of economic growth and development, and generally leads to undervaluing women's work. The feminist economics account suggests that the men who created and institutionalized national income statistics (and they were almost entirely men) did not consider women's work productive, and thus worthy of inclusion, but as yet there exists no definitive historical treatment of this decision (see O'Brien 1998 for a partial exception).

Here, I examine how it was that economists came to exclude housework. Somewhat surprisingly, I find that economists were intensely conflicted about the exclusion of housework, and that conflict persists in various forms to present. In the 1920s-1950s, economists largely agreed that theoretical principles called for the inclusion of unpaid housework, but practical concerns made that inclusion difficult. In order to include housework, economists needed to know both its amount and value, neither of which was easily quantifiable with available data. After the 1950s, most economists considered the form of the accounts fixed, but specialists in national income statistics continued to search for better ways to count and value unpaid housework in order to include it in the accounts, or, at a minimum, add housework to new "extended accounts" (Eisner 1988). Estimates of the value of housework were consistently large, but different methods and data produced quite divergent estimates. Research on unpaid housework continues, furthered in part by newly available time use data, but has not settled into a stable and agreed upon measurement technique.

Thus, I find that unpaid housework was not simply overlooked, or deemed irrelevant, by national income accountants. Rather, unpaid housework was initially excluded from na-

tional income statistics, and by extension from the economy-as-object, in part to guarantee the mechanical objectivity of the economy, and in part because economists believed that unpaid housework did not vary sufficiently quickly to help manage the economy in the short run.¹ That is, economists believed unpaid housework should, ideally, be included in order to produce the “truest” measure of economic performance, but that the costs of producing a less accurate measure, and possibly distorting long-run or international comparisons, were not seen as pressing enough to overcome the practical difficulties of settling on a single estimate. In later periods, institutional inertia placed significant barriers on changing national and international standards, barriers worsened by the lack of consensus on the best technique for valuing housework.

5.2 Data & Methods

This data for this chapter are drawn from the larger project on the history of national income statistics. Sources of data include scholarly publications (journal articles, conference volumes, and books), government documents (especially the Department of Commerce’s *Survey of Consumer Business*), newspaper reports, economics textbooks, and archival documents from six relevant collections.² I read these documents looking for methodological discussions, and in particular debates over the inclusion and valuation of nonmarket goods. I draw heavily on the public presentations of national income estimates — final reports and methodological treatises — both because they are consistent with archival correspondences, and because they are the context in which national income statisticians were forced to most clearly justify their work.

¹O’Brien (1998) similarly emphasizes economists’ belief that the lack of variability in housework implied that including it in the accounts would not help resolve short-run economic issues.

²The relevant archives here are the papers of the Bureau of Foreign and Domestic Commerce (the precursor to the Bureau of Economic Analysis) housed at the US National Archives, along with the papers of Simon Kuznets (Harvard University), Wesley Mitchell (Columbia University), Richard Stone (University of Cambridge), John Maynard Keynes (University of Cambridge), and James Meade (London School of Economics).

I divide my analysis into two rough periods: 1920-1947 and 1947-present. In the first period, national income statistics transitioned from being a relatively low-status pursuit of individual scholars into a widely-used, high-status product of government bureaucracies in the US, Canada, and Western Europe. In the second period, national income statistics were largely standardized, and diffused across the globe. In both periods, statisticians worried about the perceived objectivity and legitimacy of national income statistics, and in both periods the exclusion of housework was noted and debated. I focus on the United States as an influential, albeit non-representative, case to highlight the relationship between the quest for objectivity and the exclusion of housework.

5.3 The Rise of National Income, 1920-1947

The idea of calculating the total income of a nation for a given year dates back to at least the 17th century. William Petty produced the first estimates of national income for England and Ireland in the late 17th century, and his work was followed by intermittent estimates over the next two hundred years (Studenski 1958).³ This period is characterized by lone scholars in a handful of countries, often unaffiliated with any government, producing estimates based on scarce data. The era of the lone investigator then gave way to a period of official, routinely produced, institutionalized statistics in the 20th century. While only eight nations had any estimates whatsoever before 1900, 92 nations had official or unofficial estimates by 1957 (Studenski 1958: 156-157). The rise of official national income estimates followed the massive expansion of government statistics offices in Europe and the United States beginning in the 19th century and continuing through the early 20th cen-

³Like most accounts of the history of national income statistics, this one relies heavily on Paul Studenski's (1958) *The Income of Nations for the pre-20th century era*. Additionally, this necessarily brief review omits many important topics. For example, in the 20th century, the Soviet Union and China developed a competing accounting framework to the SNA known as the Material Production System. Omitting this story obscures the role of geopolitical struggles in shaping modern national income statistics. Readers interested in more detailed histories should consult Studenski (1958), Carson (1971), Vanoli (2005), Bos (2009), Mitra-Kahn (2010) and Herrera (2010), and see the discussion in chapter 3.

ture (Desrosieres 1998; Duncan and Shelton 1978). These new offices, along with private non-profit institutions, replaced the lone scholars of the first period.

World War I sparked more interest in and debate about national income estimates, along with measures of inflation or cost of living (Stapleford 2009) and other economic aggregates. In the years immediately following the war, a group of high-profile economists and activists founded the National Bureau of Economic Research (NBER) with the goal of improving economic data. NBER brought together politically diverse actors who were convinced that objective data was the key to solving pressing political and social issues (Rutherford 2011). Even before the war, influential actors were convinced that an objective measurement of the distribution of national income between different elements of society would be critical to resolving class conflicts (Shenk 2015). The NBER embodied that vision, bringing together academics with experts from business and labor to produce statistics that political rivals would find mutually acceptable. To achieve that vision, the data NBER produced would have to be as objective and authoritative as possible.

For its first major research project, NBER tasked noted business cycle theorist Wesley Mitchell and three collaborators with the production of a comprehensive estimate of the national income of the United States. Published as *Income in the United States: Its Amount and Distribution, 1909-1919* (NBER 1921), this report emphasized the technical difficulties of calculating national income and the various theoretical and methodological debates that surrounded such a calculation. Unlike past national income publications, the report deferred making specific policy recommendations. The methodological decisions made for this calculation, about what to count and how to count it, have been largely re-made ever since. Although there have been numerous changes, according to one prominent critic of national income statistics: “The definition of income used in this report corresponds in most major ways to what is used today. It included essentially the market economy (where money must change hands), with the addition of home-produced food and fuel, and an estimate of the rental value of owner-occupied homes.” (Waring 1999: 41-42)

In particular, as I will discuss below, the NBER 1921 estimate laid the groundwork for the official national income estimates produced by the Department of Commerce in the 1930s, and continued up to the present. Importantly, NBER 1921 also provided the first known estimate of the value of unpaid housework, although this estimate was kept separate from the final total (Hawrylyshyn 1976). How did NBER define national income? Why did they feel compelled to estimate the value of unpaid housework, but exclude it from their national income measure?

Following an extended discussion of various alternatives, NBER (1921: 42) defined the national income as follows:

[T]he National Income is taken to consist of the commodities and services produced by the people of the country or obtained from abroad for their use, with the omission of goods for which no price is commonly paid, for example the services of housewives. Agricultural produce consumed by the families that produce it, mainly food and firewood, is included, and so also is the rental value of homes occupied by their owners.

This definition was guided by pragmatic as well as theoretical considerations. In particular, the authors of the report worried about the quality of the data available: “it is hardest of all to frame a definition which will include both of these concepts and at the same time enable one to use the exceedingly miscellaneous data from which an estimate of the aggregate must be made.” (NBER 1921: 4) Thus, the report offered a definition that was meant to be unambiguously estimable, whether or not that definition was theoretically ideal.

The definition adopted invoked the market to solve one of the central methodological problems faced by economists trying to measure national income: the “production boundary.”⁴ Social life does not neatly divide into productive and non-productive behavior. Economists must decide which activities count as productive in order to then estimate the value of production. Sale in the market provides a clear criterion for inclusion — anything sold in the market should clearly be included.

⁴My discussion of the methodological principles of national income draws on a large body of literature, including the primary sources and historical works cited here, along with various textbooks. For an excellent primer, see Lequiller and Blades 2006.

Yet this criterion was insufficient to define a stable, authoritative measure of national income. The problem, as economists had long known, is that the boundaries of the market are not fixed over time. Housing offers an important example. Rental income is easy to include in the national income. Renters pay a fee for the use of housing in a given year, and this rental income can be added to the total national income. Owners, in contrast, do not pay a market price for the value of the shelter provided by their homes. What happens when renters turn into buyers? The amount of housing services produced remains the same, but the total measured market income goes down. This paradox suggests another principle of national income estimation: the total estimate should be “invariant” to purely legal changes in the organization of production. Renters buying their own homes should not change the total national income because the same housing services are produced.

Already in 1921, the conflict between the principle of invariance and the market criterion for inclusion was clear. Excluding all nonmarket production was simply not an option. But how far should the production boundary extend? Here, a second market-based criterion was invoked. If a nonmarket good had a market equivalent whose price was readily known then the value of the nonmarket good could be easily imputed. So, the value of owner-occupied housing was estimated based on the rent for similar housing. And the value of agricultural produce consumed by the farmers who grew it was similarly imputed.

In contrast, unpaid housework, or more precisely “the services of housewives to their families” (NBER 1921: 57), was not so readily brought into comparison with an existing market. The problem was not the complete absence of a market for housework. Rather, the problem was that the market did not provide an unequivocal estimate of either the *amount* of unpaid housework performed by housewives nor of the *value* of that housework: “How much was their [housewives’] contribution to the National Income worth, on the average? As much as the average pay of domestic servants? Somewhat more? Perhaps \$500 per annum before the war, and more than that after servants’ wages rose? We do not know.” (NBER 1921: 59) Over the following decades, economists would routinely confront these

problems and offer a shifting array of methods of inferring the value of housework from market price, none of which stabilized into an unequivocal (and thus potentially authoritative and objective) standard.

NBER recognized that the exclusion of housework also presented serious problems for the comparability of national income estimates across time and space. As housewives began to purchase on the market goods and services that they used to produce themselves “the range of goods not commonly paid for in money gradually shrinks. Hence figures such as we get for the National Income in successive years tend to exaggerate the increase in economic welfare.” (NBER 1921: 57) Here, NBER recognized the tradeoff between the theoretical superiority of estimates including housework, and the practical difficulties of estimating the value of housework in the absence of a market.

In the end, NBER did use the wages of servant as a rough proxy for the services of housewives and produced an estimate of their value (see figure 5.1). NBER went to great lengths to highlight the uncertainty of the estimate — the words “assumption” or “assumed” appear four times, and “conjectural” twice. But, as noted above, this “conjectural estimate” was excluded from the final total of national income.

NBER continued intermittent work in the field of national income in the 1920s and early 1930s, as did the National Industrial Conference Board. In 1932, in the wake of the Depression, Senator La Follette authored a resolution calling on the Department of Commerce to produce its own estimates of national income. The Bureau of Foreign and Domestic Commerce (BFDC, the precursor to the Bureau of Economic Analysis) brought in Simon Kuznets, an NBER affiliate and student of Wesley Mitchell’s, to lead the effort (see Carson 1971 for a detailed account). Through Kuznets, the methodological decisions of the NBER were brought into the official measures of national income.

The official estimates were published as *National Income, 1929-1932* (Kuznets 1934), and then updated as a yearly, and eventually quarterly, series in the Commerce Department’s *Survey of Current Business*. Following the lead established by the NBER, Kuznets

TABLE 7
**CONJECTURAL ESTIMATE OF THE MONEY VALUE OF
HOUSEWIVES' SERVICES ON THE ASSUMPTION
THAT SUCH SERVICES WERE WORTH \$500 PER
ANNUM ON THE AVERAGE IN 1909 AND
ROSE IN VALUE WITH THE AD-
VANCE IN WAGES ¹**

Year	Estimated Number of Housewives	Assumed Average Value of Housewives' Services	Conjectural Total Value of House- wives' Services
	In Millions	In Dollars	Billions of Dollars
1909	17.7	\$500	\$ 8.85
1910	18.0	500	9.00
1911	18.4	500	9.20
1912	18.7	525	9.82
1913	19.0	525	9.98
1914	19.4	525	10.19
1915	19.7	550	10.84
1916	19.9	600	11.94
1917	20.2	650	14.30
1918	20.4	750	15.30
1919	20.5	900	18.45

¹ The number of housewives is based on Dr. Edward's rough approximation for 1910, on the assumption that this number varied as the total population, and on Mr. King's estimate of the total population in inter-censal years. The assumed average value of their services corresponds with Mr. Knauth's estimate of the average incomes of persons engaged in "Domestic and Personal Service"—a group that includes many other occupations besides female domestics.

Figure 5.1: From NBER 1921, the first national income estimate of the value of housework.

(1934: 1) defined national income as “all commodities produced and all personal services rendered during the year . . . added at their market value” less the cost of the intermediate goods (raw materials and depreciation) used to make the final goods and services. This definition sufficed for goods and services traditionally sold in the market, but Kuznets still had to reckon with the problem of nonmarket goods. Again following the logic of the NBER estimates, Kuznets recognized the theoretical problems of excluding unpaid housework and the practical problems of including it. Kuznets (1934: 4) further clarified that the services of housewives are not exactly comparable to those of servants:

the organization of these services render them an integral part of family life at large, rather than of the specifically business life of the nation. Such services are, therefore, quite removed from those which gainfully occupied groups undertake to perform in return for wages, salaries, or profits. It was considered best to omit this large group of services from national income, especially since no reliable basis is available for estimating their value.

Kuznets took great care in explaining how the reliance on the objectivity of market prices shaped the choices of where to draw the production boundary, and how to value various goods and services. For example, economists and policymakers debated whether or not various morally questionable goods and services should be counted as productive. Kuznets (1934: 5) eventually included any legal good bought or sold in the market, arguing that to do otherwise would be to “impose his own scale of values” on the statistics. As such, “all lawful activities are to be given that benefit of doubt which the market place is eager to bestow upon anything that succeeds in fetching a price.” (Kuznets 1934: 5) Here, the market served as a guard against the economist (or politician) imposing his own values onto the accounts.

The exclusion of housework from the official estimates was seen as controversial and notable enough to require discussion and justification. For example, a three page press release written at the time of the report’s publication listed just two major caveats. First, that data were available for the continental US only, and second: “Certain items that might be classified as income under concepts other than those employed by the investigators have

been excluded from the totals presented, i.e. . . . the imputed value of services of housewives and other members of the family. . . .”⁵The New York Times story reporting on the report picked up the press release language word-for-word, ending with the same caveat about the exclusion of housework.⁶

Similarly, influential economics textbooks in the 1930s-1940s — the first to focus their attention on macroeconomics and national income statistics — noted the exclusion of housework and argued that it flowed from practical considerations about data and valuation, not economic principles (Garver and Hansen 1937: 16, Burns et al 1948: 103, Nordin and Salera 1950: 57). As Paul Samuelson’s famous *Economics* (1948: 228) put it: “A wife’s housework services are not included simply because it is hard to find a market-price yardstick or any other to evaluate them. In all logic, they should be included, and national income should be increased by one-fifth or more.”

After his work on the 1934 publication, Kuznets returned to academic life and became an influential theorist of national income and economic growth. Throughout his writings in the 1930s and 1940s, Kuznets was careful to stress how any national income estimate required making tradeoffs. This multiplicity of possible national incomes led Kuznets to argue that a value-free, objective national income measure was not possible. Rather, each investigator must draw the boundaries of what is included in ways that make sense given the particular political problem that he hoped to address. For example, Kuznets listed five possible specifications of what kinds of goods could be included in a national income estimate, from only those goods produced by private actors and sold on the private market (ignoring completely governments, let alone housework or owner-occupied housing) all the way up to a measure that includes goods not appearing in markets such as the services of housewives to their families, and the services of individuals to themselves (Kuznets 1941: 9).

⁵“The National Income 1929-1932”, National Archives Record Group 151, Index 600, Box 2563, Folder “Finance and Investment - United States - National Income - 1934 - January - April.”

⁶“Our Income Fell 40% in Four Years”, New York Times, January 22, 1934.

Despite this care, Kuznets continued to justify the emphasis on the role of the market in determining the production boundary for the official Commerce Department estimates. Kuznets argued, “It is the market, with its vast mechanism for the disposition of diverse goods, that reveals the ties binding the separate units in the economic system and segregates economic goods from other.” (Kuznets 1941: 7) The market both identifies economic goods, and determines their value. Kuznets (1941: 21-22) further wrote:

The yardstick of economic value is fashioned on the market place. It is in markets that economic goods are brought together and their relative importance gauged for purposes of sale and purchase; that the members of the community vote, in terms of the common currency unit, upon the relative value to them of various commodities, services, and arrangements. In fact, to identify economic value with market price is, at least as the first step, the one possible solution of the problem [of valuation].

Although Kuznets immediately added a caveat to this claim, the basic logic guides the production of national income statistics. Markets are to the economy what elections are to politics. In the market, the members of the community vote with their dollars, and in so doing determine value. Fortunately for economists and statisticians, the outcome of these economic elections provides the data necessary to compute complex statistics like national income.

National income statistics were a relatively new and uncertain endeavor in the 1920s and 1930s. In the 1940s, they would prove their worth in World War II. In the United States, national income statistics were credited with helping guide military mobilization, as Kuznets and his colleagues convinced the military to moderate their demands for materiel to what the economy could feasibly produce in a limited time (see chapter 3 for details). In the United Kingdom, national income statistics were used to guide wartime fiscal policy and making it possible to avoid hyperinflation through a carefully planned combination of higher taxes and bond issues (Keynes 1940, Meade and Stone 1941). This transformation of the national income into a macroeconomic policy tool continued after the war with the rise of the politics of economic growth, with GNP as its main outcome of interest (Collins 2000,

Yarrow 2010). This process fixed the national income in a particular form, as the nitpicking precision favored by Kuznets lost out to the policy imperatives of the post-war era. In the next section, I discuss how national income statistics became fixed, as bureaucrats fought to maintain the status quo for fear of national income becoming politicized and losing its newfound value as a policy tool.

5.4 The Institutionalization of National Income, 1947-2015

In this section, I document how bureaucrats in the US Department of Commerce fought to keep GNP from becoming a so-called “welfare measurement” and how this fight led them to exclude unpaid housework, and other imputations of nonmarket goods.

In 1947, the Department of Commerce published *National Income: A New Version*. This document codified the improvements made during the war, established Gross National Product as the main aggregate reported, and entrenched the “accounting” framework that gives the national accounts their name. The 1947 standards also brought the US and UK accounts closer together, and helped to lay the groundwork for the United Nations System of National Accounts (UNSNA).⁷

This publication also occasioned a surprisingly critical response from Simon Kuznets (1948), who had returned to academia from his wartime post and was no longer directly involved in the production of the data. Kuznets made several criticisms of the 1947 accounts, including that the accounting metaphor itself made the entire enterprise appear more certain, and objective than it truly was. For Kuznets, it was important to highlight how and when judgment entered into the framework. The accounting framework, Kuznets

⁷In the 1940s and 1950s, national income statistics proliferated across the world. At the same time, the first international standards were created, modeled on the UK and US statistics. British economist Richard Stone wrote a series of reports that laid out the framework, first on behalf of the League of Nations and then most influentially the first UNSNA, published in 1953. The UNSNA was revised three times in following decades (in 1968, 1993, and 2008), but the basic principles and the practical resolution of many of the trickiest problems remained the same (Bos 1994). Importantly, every version excludes unpaid housework. The exact procedures used by different nations may vary from the UNSNA, but most attempt to follow the standards (Herrera 2010).

argued, distracted attention from the difficult problems of “scope, netness, and valuation” (Kuznets 1948: 154), to which it offered no helpful answers. Any measure of national income must make difficult judgments about what is worth measuring, and what counts as “final” production, and the new national income accounts covered up rather than revealing or improving those choices. In his view, any measure of national income was necessarily making judgments about economic welfare.

Kuznets’ argument fell on deaf, and somewhat confused, ears. The new generation of economists in the Commerce department were less concerned with the theoretical principles of national income statistics, and more concerned with establishing and maintaining a reliable policy tool. In particular they worried about subjectivity showing through in the standards and were surprised that Kuznets criticized them for effectively making the same decisions he had made in 1934. In their response, Gilbert et al. (1948) argued that Kuznets proposals would require moving away from the “observable demand of ultimate buyers for goods and services” (about which knowledge could be relatively certain), and in so doing would undermine the scientific objectivity of the statistics: “The moralistic flavor [Kuznets] wishes to inject into national income measurement might be in the tradition of Ruskin — it is not in the tradition of quantitative economics.” (Gilbert et al. 1948: 189)

This 1948 fight over the role of explicit moral and economic welfare considerations in the national accounts would be repeated over the following decades. Every time, economists inside government would successfully fight to maintain the status quo and keep out any modification that smacked of subjectivity. In turn, this status quo was buttressed by its reliance on observed market behavior as the hard methodological core, onto which modifications must be carefully grafted. As Edward Denison, one of Gilbert’s co-authors and former official statistician, wrote in the *Survey Of Current Business* in response to growing calls for welfare measurement argued, “We can obtain a generally acceptable measure only because market prices provide weights to combine them that are widely accepted as reasonable and objective.” (Denison 1971: 13) Another of Gilbert’s co-authors, George Jaszi,

went on to serve a long stint as Director of the Bureau of Economic Analysis. At the end of his career, Jaszi (1986:411) claimed that preventing GNP from becoming a politicized measure of economic wellbeing was perhaps his greatest accomplishment:

Over the 43 years at BEA, I feel that I made my principal contribution as an economic accountant. First, I resisted the will-o'-the-wisp of forging national output into a measure of economic welfare. I was a minority of one in a company that included such mental giants as Simon Kuznets and John Hicks, and at one point I had to defy a forceful Secretary of Commerce who had instructed the BEA to prepare a measure of welfare.

The fight over the exclusion of unpaid housework was central to this debate, although concerns over the treatment of government and especially the environment also shaped the debate (Denison 1971, Eisner 1988). In the 1970s and 1980s, influential economists continued to argue for the inclusion of unpaid housework, and offered innovative new ways of measuring its value. Each of these methods relied on a different way of reading the market, a different set of market numbers. Hawrylyshyn (1976: 101) summarized both the available methods and how they led, unfortunately, to a wide range of estimates: “The highest values are obtained with methods based upon the opportunity cost of women in paid employment, lower with methods based upon the cost of a single housekeeper, and lowest with methods based upon pricing individual services performed.” Three methods, three ways of analogizing unpaid housework to market labor, three different answers.

Although the BEA resisted efforts to transform GNP into a measure of welfare, research continued on the value of nonmarket work. For example, Martin Murphy, working at the BEA, tried to find a consistent estimate for the value of housework. Murphy (1978) argued that including housework in the accounts would change little because housework was a relatively stable percentage of measured GNP and that this finding was robust to different estimate methods. Just four years later, Murphy (1982: 35) overturned his own conclusions, arguing that “aggregate estimates for all persons are highly sensitive to the method of valuation.” More recently, Steve Landefeld (then-director of the BEA) and co-authors published estimates of unpaid housework using newly available time use survey data (Lan-

defeld et al 2009). And yet, despite this continued interest, and radically improved data sources, the lack of consensus around the value of the estimate, driven by the tenuous connection to the hard facts of market prices, continually militated against the inclusion of housework.

5.5 Discussion & Conclusion

The practical and epistemic justifications for national income statistics rely on markets as truth-telling devices, but the actual flows of money and resources are not confined to market transactions. In order to accurately measure the whole economy, NIS are constantly challenged to go beyond the market.

In the 1920s-1940s, statisticians were concerned with producing national income estimates that were free from bias and thus capable of objectively informing political debates. Nonmarket goods like unpaid housework seemed risky to include because they lacked the (purportedly) objective information provided by market prices. In the 1940s, as national income statistics gained a surer foothold in the policy apparatus, bureaucrats fought to preserve the political neutrality of the accounts. Despite protestations from outside academics that national income always already contained some implicit welfare criterion, the economists at Commerce argued that modifying the accounts to take into account subjective welfare criterion would undermine their usefulness and objectivity. At the same time, estimates of the value of unpaid housework based on different kinds of imputations never converged on a unique value.

Relying on market prices' revelatory powers thus helped national income statisticians to answer the question of valuation and the question of boundaries, of which activities should count at all. National income statisticians easily included goods regularly bought and sold by individuals and businesses, and thus about which market price data are available and credible.

Conversely, statisticians struggled throughout the 20th century to include non-market goods. It was not difficult for statisticians concerned with the amount and value of unpaid housework to find *some* number, but it was hard to find a *unique* number. Statisticians attempted to analogize unpaid housework to the labor of servants or to value housework in terms of a market opportunity cost, but these methods yielded inconsistent results. Lacking an unequivocal market estimate of the amount or value of housework, national income statisticians chose to exclude them entirely.

In terms of unpaid housework, this analysis suggests that statisticians chose to exclude housework from the national accounts *despite* theoretical principles, not because of them (cf. Folbre 1991, Waring 1999). Excluding unpaid housework brought real dangers in terms of comparability across time and space, and at least the potential to misinterpret trends in growth and well-being. The persistent stream of research by mainstream economists, including those working inside the agency tasked with constructing the national accounts attests to the serious concern with these dangers. Nonetheless, statisticians continually opted to avoid controversial numbers in order to shore up the authority of the national accounts.

My analysis here is limited by its focus on dynamics in the United States. As scholars of quantification have long noted, national culture deeply shapes the acceptance of different techniques (Porter 1995, Carson 2007, Fourcade 2011). In the case of national income, however, the connection between the US (and UK) estimates and the relatively influential UN standards suggests that here the rest of the world largely conformed to the US approach. Norway offers a brief but revealing counterpoint. In the 1940s, Norway experimented with including unpaid housework in its official national income estimates but ultimately removed the estimates to conform to international standards (Aslaksen and Koren 1996). The global principles of national income statistics are firmly those of Anglo-American economics, regardless of the national contexts in which they are embedded.

The decisions made by statisticians in the 1920s-1940s produced a set of incredibly suc-

cessful tools, in the standard sense of widespread adoption and use. At the same time, the quest for mechanical objectivity led statisticians to exclude unpaid housework despite theoretically principled arguments for its inclusion. National income thus exhibits what Sandra Harding (1995) would call a “weak” objectivity, one that encodes systematic, disciplinary biases in its quest to eliminate individual ones. Harding (1995: 16) cautions against resting too easily on the apparent success of weakly-objective measures: “Such information and explanations may well ‘work’ in the sense of enabling prediction and control. . . . One form of explanation that ‘works’ may at the same time obscure or draw attention away from other regularities and their causes that would suggest other possibilities for organizing nature and social relations.”

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CHAPTER 6

Rediscovering the 1%: Economic Expertise and Inequality Knowledge

In the 2000s, academics and policymakers began to discuss the growth of top incomes in the United States, especially the “top 1%.” Newly analyzed data revealed that top income earners in the 1990s received a larger share of income than at any point since the Great Depression, and that their incomes had begun a dramatic upward climb in the early 1980s. This paper investigates why it took two decades for this increase in top incomes to become politically and academically salient. I argue that experts assembled two “regimes of perceptibility” (Murphy 2006) for producing knowledge about income inequality, and that neither of these regimes was capable of tracking movements in top incomes. Macroeconomists focused on labor’s share of national income, but did not examine the distribution of income between individuals. Labor economists, on the other hand, drew on newly available survey data to explain wage disparities in terms of education, age, work experience, race, and gender. By relying on surveys, these scholars unintentionally eliminated top incomes from view: surveys top-coded high incomes, and thus were incapable of seeing changes in the top 1%. Studies of top incomes that relied on income tax data thus fell by the wayside, creating the conditions under which experts, policymakers, and the public alike could be surprised by the rise of the 1%. This historical narrative offers insights into the political power of economic expertise by clarifying the complex linkages between observations, stylized facts, causal theories, and policy attention.

6.1 The Surprising Growth of the Top 1%

In 2003, economists Thomas Piketty and Emmanuel Saez published a surprising finding: the top 1% of income earners in the United States were now receiving 15% of all income, almost twice as large a share as they had 30 years earlier. And, perhaps even more surprisingly, top income earners were now receiving the largest share they had received since the Great Depression. Even before the finding appeared in the prestigious *Quarterly Journal of Economics*, it had already been written up in the *New York Times Magazine* by columnist and noted economist Paul Krugman.¹ Although scholars in many fields had noted an increase in income inequality in the 1980s and 1990s, Piketty and Saez’s research reframed the debate by offering a new characterization of inequality that did not fit easily into existing debates about the role of education, globalization, race, or gender. Over the next decade, the growth of the top 1% thus spurred new research programs in economics, political science, and sociology. Beyond the walls of the academy, the growth of the top 1% entered into mainstream political discourse, and even helped to frame a new political identity as the Occupy Wall Street movement proudly declared, “We are the 99%!” (Gould-Wartofsky 2014).

My purpose here is not to investigate the history of income inequality in the United States. Instead, I trace the history of *inequality knowledge*, in the spirit of O’Connor’s (2002) poverty knowledge. How was it possible that Piketty and Saez’s finding was so surprising? The trend of growth in the top 1% of incomes began in the mid-1980s, and was at least potentially visible in publicly available IRS tax data (see Figure 6.1). Piketty and Saez did not invent any fancy new statistical techniques, but rather updated an analysis published by Simon Kuznets half a century earlier (see Figure 6.2). What was it about the way we produced inequality knowledge that made it possible to miss the growth of the 1%? To answer this question, we must explore the *regimes of perceptibility* through which

¹Krugman, Paul. 2002. “For Richer.” *New York Times Magazine*. October 20. Krugman, and other commenters, relied on Piketty and Saez’s NBER working paper, released in 2001.

experts monitored trends in inequality. Following Murphy (2006) and Hecht (2009), I define a regime of perceptibility as an assemblage of social and technical things that makes certain features of the world visible while rendering others invisible.

I argue that the two dominant regimes of perceptibility for making sense of the distribution of income, associated with labor economics and macroeconomics respectively, developed and institutionalized tools for seeing inequality that were well-matched to the particular theoretical problems faced by those fields in the mid-20th century, but which made top incomes invisible. Labor economists relied on survey-based measures of income which explicitly top-coded incomes, and thus were incapable of tracking changes in the extremes of the distribution, while macroeconomists stuck to the national income accounts to examine the share of income going to labor as a whole, but almost entirely ignored the personal distribution of income.² Economists, and other social scientists, interested in income inequality in the 1980s and 1990s lacked easily available, standardized, and timely data on the whole distribution. Although income distribution data had been seen as a top statistical priority in the 1940s and 1950s, official measures of the entire distribution received little attention and were discontinued in the 1970s — just before they might have revealed changes in top incomes.

Tracing the history of inequality knowledge, including both theories of inequality and the data used to assess and inform those theories, contributes new insights to a growing conversation on the political power of experts and their tools. Scholars in science studies have recently turned their attention to agnotology (Proctor and Schiebinger 2008, Croissant 2014, Frickel 2014), the social production of ignorance. That is, scholars have sought to understand how experts intentionally or unintentionally shape political outcomes by *not* producing knowledge about particular topics. At the same time, sociologists and political scientists interested in the political power of economics have increasingly turned their at-

²Top-coding refers to the practice of reporting incomes above a certain threshold, X, as simply “above X”. The actual dollar value of those high income is either not recorded at all or only recorded in data not released to the public. The top coding of incomes is usually justified as part of a collection of practices designed to protect the privacy of respondents with unusual characteristics.

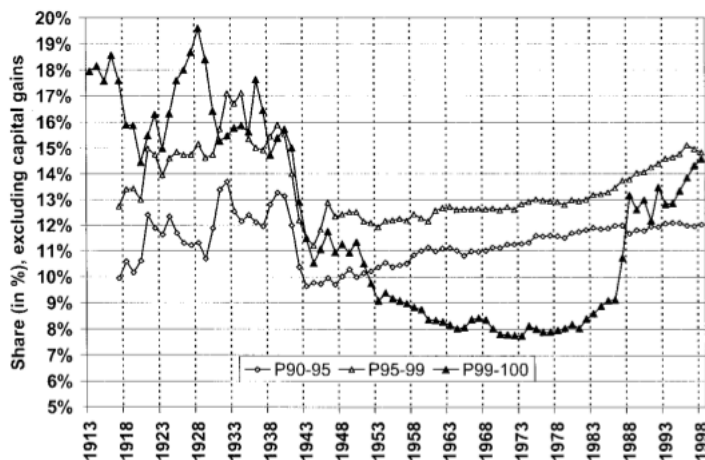


FIGURE II
The Income Shares of P90-95, P95-99, and P99-100, 1913-1998
Source: Table II, columns P90-95, P95-99, and P99-100.

Figure 6.1: Piketty and Saez’s (2003:12) famous chart showing the growth of top incomes in the 1980s-1990s.

Chart 2
Income Shares of Upper Income Groups, Total Population
Three Variants, 1913-1948

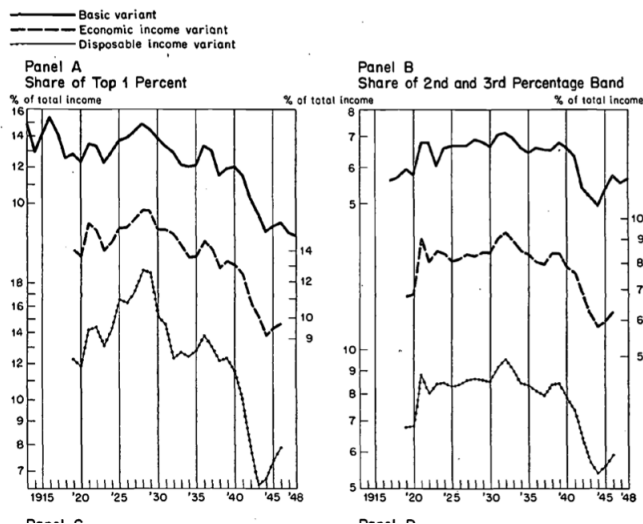


Figure 6.2: Kuznets’ (1953:33) less famous chart showing the downward trend of top incomes in the 1930s-1940s.

tention to the influence of economists' technical tools and styles of reasoning in shaping policy (Hirschman and Berman 2014). Here, I combine these emphases with an older tradition of research on social problem definitions (Hilgartner and Bosk 1988) to show how the regimes of perceptibility associated with different communities of economic inquiry collectively influenced policy debates by identifying and disseminating particular “stylized facts” about inequality, while rendering other forms of inequality invisible.³

The remainder of the paper proceeds as follows. First, I expand on the above discussions of regimes of perceptibility and the sociology of ignorance. Second, I outline an analytical strategy for approaching the rise of the 1% in inequality debates, arguing that it can be usefully understood as an example of the emergence of a new stylized fact. Third, I turn to the case itself. I explore the intertwined histories of macroeconomics, labor economics, and the production of official statistics. The case is divided into roughly two periods: the 1920s to the 1970s, when both labor economics and macroeconomics take shape and develop specific regimes of perceptibility for seeing inequality, and the 1980s to present when we can see the effects of those regimes in rendering largely invisible the growth of top incomes. In this later period, I rely on a partial counterexample — a brief moment when top income inequality became a matter of public concern — to theorize the importance of regimes of perceptibility for both observing economic life and for sustaining attention on particular features. In the discussion, I draw on insights from the case to propose a broader model for understanding economists' influence on policy debates.

³A stylized fact is a lightly abstracted empirical regularity, such as a trend or correlation. The term has been used widely in economics since the 1960s, and recently has seen some usage in sociology, but has thus far not been subjected to much theoretical analysis. I return to the concept in the discussion, but for a more complete account of stylized facts, including an analysis of the evolution of the term's usage, see Hirschman 2014.

6.2 Perceiving Economists' Influence

How does economic expertise shape policy debates? Here, I draw on theories of social problem definition and agenda setting to argue that economists are particularly important actors in providing facts that can dramatize social problems, and in turn their role as providers of facts gives economists leverage to shape the framing of social problems. Inspired by recent developments in the sociology of ignorance or non-knowledge, I then argue that economists shape policy through absence as well as presence: when economists don't study a particular problem, or don't study it in a particular way, then public discourse mirrors that absence. This analysis motivates a further discussion of economists' regimes of perceptibility: the combinations of people, ideas, and tools that structure what economists do (and do not) routinely observe.

Sociologists have long been interested in the process by which a particular aspect of the world becomes recognized as a "social problem" (Blumer 1971, Gusfield 1984). Hilgartner and Bosk (1988) provide a useful "ecological" model of social problems which highlights both the competition between different problems for public attention, and the struggle between actors to define problems in particular ways.⁴ The central question for researchers following this approach is, "Given the vast universe of possibilities, how do social forces select particular problem definitions?" (Hilgartner and Bosk 1988: 75) Although they do not focus primarily on experts and the production of knowledge, Hilgartner and Bosk's approach offers several important insights for understanding how economic expertise contributes to social problem definition. First, because public attention is assumed to be scarce, experts may play a role in selecting which problems come to public attention. Second, because multiple definitions exist for any particular problem, experts may play a role in shaping exactly how a social problem comes to be defined. Operatives attempting

⁴This model has many similarities to research on "agenda setting" in political science (Kingdon 1984, Baumgartner and Jones 2010) and "framing" in social movements (Benford and Snow 2000). For simplicity, I focus on Hilgartner and Bosk's work as exemplary of this broader cultural approach to social problems, rather than emphasizing the differences between these three approaches.

to promote a particular social problem framing rely on a combination of emotional performances and seemingly objective claims about the nature and extent of a problem. Thus, “‘cold, hard facts’ and an image of technical expertise become powerful resources for constructing authoritative presentations” of social problems (Hilgartner and Bosk 1988: 61).

Since experts produce those “cold, hard facts”, exactly which facts they produce — or fail to produce — influences the construction of social problems. In the past 10 years, historians and sociologists of science and expertise have become increasingly interested in the “non-production” side of this thesis, arguing that non-knowledge shapes public discourse just as readily as knowledge itself. Under the broad heading of agnotology (Proctor and Schiebinger 2008), scholars have studied the causes and consequences of both strategically and normatively produced ignorance (Frickel 2014). Strategic ignorance refers to the deliberate obfuscation of already produced knowledge by powerful interests, such as when tobacco companies disseminate misleading claims about the health consequences of smoking or when oil companies create the false perception of an ongoing scientific controversy about climate change (Oreskes and Conway 2010). Normative ignorance refers to a subtler, and perhaps more pervasive, form of ignorance. Normative ignorance is generated by disciplinary norms, standard operating procedures, standards of evidence, and so on that dictate which claims are accepted as authoritative, and which claims are either rejected or never even advanced (Kleinman and Suryanarayanan 2013). A related stream of research on “undone science” examines cases where social movement activists pressure experts to produce knowledge relevant to the movement’s goals, but which would require experts to alter their standard practices or accept alternative kinds of evidence (Frickel et al. 2010).

What determines the kinds of facts that experts produce and fail to produce? Murphy (2006: 12) introduced the concept of regimes of perceptibility in the context of debates over a controversial medical diagnosis, “multiple chemical sensitivity”, to describe “how arrangements of words, things, practices and people drew out and made perceptible specific qualities, capacities, and possibilities” and in so doing, pushed other possibilities to the

background. Similarly, Hecht (2009: 899) extends this concept of regimes of perceptibility to the context of the link between radon and cancer in African uranium mines, arguing that “In the domain of occupational exposures . . . instruments, labor relations, scientific disciplines, expert controversy, and lay knowledge combine to create . . . assemblages of social and technical things that make certain hazards and health effects visible, and others invisible.” Just as dosimeters and Geiger counters constitute key links in the regime of perceptibility for radioactive occupational health hazard, I similarly argue that economic statistics constitute key links in the regime of perceptibility for economic problems.

To make the concept of a regime of perceptibility slightly more tractable, we can analytically separate out three interrelated elements in the assemblage: people, ideas, and tools. A regime of perceptibility brings together a community of inquiry (in this case, subfields of economics), with particular theories about how their object of study behaves (i.e. Keynesian economics), and particular institutionalized devices for analyzing those objects (i.e. economic statistics; for more on the role of economists’ devices in policymaking see Hirschman and Berman 2014).⁵ This regime of perceptibility governs what kinds of trends are readily observed by shaping both the production and interpretation of data.

Thus, given that policymakers and the public always have limited attention, and given that the economic world does not present itself unproblematically in some ordered and logical fashion, economic theories and data collection practices shape both which aspects of economic life we view as important, and the precise ways in which we can detect or fail to detect changes. For example, since the 1940s, macroeconomists have emphasized the importance of economic growth, with Gross Domestic Product (GDP) serving as a simple measure of that growth that is easy for policymakers and the public to track. Yet GDP is also known to leave out or miscount potentially important aspects of growth, including the role of unpaid labor (especially unpaid housework performed primarily by women), the

⁵I use the term “community of inquiry” here in place of the narrower “epistemic community” (Haas 1992) to emphasize that the community is united by its object of study, rather than a particular set of policy positions. Subfields of economics contain individuals with wildly divergent policy positions, but who may be relatively unified in terms of the kinds of data they routinely analyze.

contribution of the environment, and even the value of government services.⁶ Similarly, as I will show, macroeconomists and labor economists in the mid-20th century constructed regimes of perceptibility that emphasized certain forms of inequality, but made others invisible, and thus influenced policy attention by failing to identify the growth of top incomes in the 1980s–1990s.

6.3 Analytical Strategy

Drawing on the language of the preceding section, we are now in a better position to characterize the argument of the chapter and thus to identify the appropriate strategy for establishing the claims needed to support it. In the early 2000s, economists identified a new “stylized fact”: the rise of the 1%. Stylized facts are simple empirical regularities in need of an explanation, such as trends and simple correlations. The trend of the rise of the 1% had fallen outside of the regimes of perceptibility established by macroeconomists and labor economists, and in particular, was invisible to the particular policy devices for seeing linked to those regimes. How did it come to be that the particular tools employed for analyzing inequality were incapable of seeing the whole distribution?

To answer this question, I trace the histories of labor economics, macroeconomics, and the production of official economic statistics in the United States over two rough periods (1920–1970 and 1970–present). I draw on a variety of sources, including published articles in prominent academic journals as well as the so-called “gray literature” of conference publications and edited volumes, as well as archival documents and Congressional Hearing transcripts. Especially important to the history of the economic statistics are the archives of the Department of Commerce, including discussions of statistical priorities and feedback from end users (including business and academic economists). I use these documents to show how, in the first period, the acceptance of particular economic theories (Keyne-

⁶I expand on this argument in Chapter 5.

sian approaches to macroeconomics and human capital theory in labor economics) shaped the kinds of data that economists collected and analyzed (national income accounts and household surveys, respectively), and the stylized facts that economists identified. Other kinds of data were not entirely excluded, but they were sent to the background, less clearly connected to major theoretical debates, and thus capable of being ignored. In the second period, I add to these sources an analysis of public debate around income inequality, and in particular, an important event in 1992 where top incomes briefly became a subject of political contention before receding into the background, incapable of being fully comprehended by any of the dominant regimes of perceptibility.

6.4 Establishing the Regimes

Before the 1920s, the United States produced none of the macroeconomic statistics we now take for granted. Data — official or privately produced — on employment, inflation, income distribution, and growth were largely unavailable throughout the 1800s, and only became available at the state or local level in the 1890s to 1910s (Stapleford 2009, Card 2011). In the 1920s–1940s, the US began to produce consistent series of national data on inflation (the Consumer Price Index), unemployment (via the Current Population Survey), and economic growth (in the National Income and Product Accounts).

In 1920, a group of economists founded the National Bureau of Economic Research (NBER) with the explicit goal of producing timely, objective data about the economic system for use by policymakers, businesses, and academics alike. NBER’s first study (NBER 1921) used newly available income tax data to examine both national income *and* income distribution. Somewhat ironically in retrospect, this study focused heavily on top income earners precisely because only top incomes were well-covered by tax data (see Figure 6.3 for an example of their findings).⁷ In the 1930s, work on national income was transferred

⁷In the early 1910s, approximately the top 4% of earners made more than \$2000/year and thus paid income tax (NBER 1921: 113). The income tax did not become a “mass tax” until World War II (Jones 1988).

TABLE 23
A CONJECTURAL ESTIMATE OF THE PERCENTAGE OF
THE NATIONAL INCOME RECEIVED BY THE HIGH-
EST FIVE PER CENT. OF INCOME RECEIVERS

1913-1919			
Including Farmers			
Year	Income of the Highest 5% of Income Receivers (Billion Dollars)	Total Individual Income (excluding Corporate Surplus) (Billion Dollars)	Per Cent. of Total Income Received by Highest 5% of Income Receivers
1913	\$10.6	\$32.3	33
1914	10.3	32.0	32
1915	11.1	34.3	32
1916	14.3	41.6	34
1917	14.7	50.5	29
1918	15.4	60.0	26
1919	15.5	64.7	24
Excluding Farmers			
1913	\$ 9.9	\$28.1	35
1914	9.6	27.8	34
1915	10.4	29.6	35
1916	12.8	35.8	36
1917	13.6	41.7	32
1918	13.9	49.5	28
1919	14.4	53.8	27

Figure 6.3: A 1921 estimate of the income received by the top 5% of income earners based on tax data (NBER 1921: 116).

from NBER to the Department of Commerce, and distributional issues would take a back-seat to better, more timely measurement of total national income.

National survey-based statistics began in earnest with efforts to measure prices and expenditures during World War I (the beginnings of the Consumer Expenditure Survey, used to calculate the Consumer Price Index; see Stapleford 2009), but did not become widespread until the 1940s. Survey-based measurement of employment and unemployment, for example, began late in the Great Depression; for most of the 1930s, there were no nationally representative data on unemployment. In the post-war period, the Bureau of Labor Statistics' unemployment survey was transformed into the modern Current Population Survey (CPS) which also began to collect income data.

In the same period that official statistical productions took off, the academic field of economics was itself transformed. Just as there was little of what we now recognize as economic data before the 1900s, there was also little “empirical” work in economics, at least in the sense of analysis of large-scale datasets. Inspired by the German Historical School, the

American institutionalists began to produce and analyze such data in the 1900s to 1920s. Wesley Mitchell was both a central figure in the institutional movement and the founder and first research director of the NBER. Institutionalists would play an important role in integrating private and public data collection efforts, in part through the influence of Herbert Hoover who advocated such efforts strongly from his position as Secretary of Commerce and President (Alchon 1985, Barber 1985, Rutherford 2011). The institutionalists' direct influence was relatively short-lived, however. The modern divide of macroeconomics and microeconomics emerged in the 1930s, alongside the terms themselves. Institutional economics faded away in the immediate post-war era, replaced by the synthesis of ever-more mathematical neoclassical microeconomics and Keynesian macroeconomics (Yonay 1998, Rutherford 2011). While the institutionalists had been heavily involved in the creation of official statistics, it would be the Keynesians and neoclassicals who would take charge of finalizing the regimes of perceptibility built around those data.

In the following sections, I explore in greater detail how macroeconomists and labor economists came to theorize and measure inequality. Although inequality never acquires a single iconic statistical representation analogous to GDP for growth or the Consumer Price Index (CPI) for inflation, macro and labor economists did pay attention to inequality, and build tools for observing it into their regimes of perceptibility. Macroeconomists focused on the traditional question of *factor shares*, and drew on the national accounts to track labor and capital's relative shares of the total economic pie. Personal income distribution was outside of the scope of most macroeconomic research. Labor economists, in turn, relied on survey-based measures to track "gaps" between different kinds of workers, with a special focus on the gaps between college-educated and less-educated workers, along with continued interest in race and gender gaps. Personal income distribution was thus an important topic for labor economics, but understood primarily through the lens of the differential productivity of different kinds of workers. Although tax data had formed the basis for early studies of income distribution, by the 1960s, both macroeconomists and labor economists

had settled on other devices for observing inequality. The Bureau of Economic Analysis (BEA) did produce tax data–based statistics on income distribution, but these statistics were ignored — they were not capable of answering the questions economists were interested in — and eventually the statistics were defunded.⁸ Thus, by the 1970s, the two dominant regimes of perceptibility for income inequality were both theoretically incapable of making sense of such changes and practically incapable of seeing changes in the top of the income distribution.

6.4.1 Factor Shares in Macroeconomics

Macroeconomics, as such, did not exist before the 1930s. Macroeconomics as we know it emerged in the 1930s and 1940s at the meeting point of monetary theory and business cycle theory. These older theoretical concerns were combined with a new interest in economic growth, and a variety of new economic theories embodied in mathematical models and measured through new forms of data, most prominently the national income accounts.⁹ Here, I focus particularly on how macroeconomics came to largely ignore the personal distribution of income in favor of continued analysis of labor’s share of national income.

The distribution of income between the three great classes was arguably the central question of classical political economy (Sandmo 2013). Starting with the writings of the physiocrats in the mid–18th century, political economists divided the economic system into roughly three groups: landlords (who earned rents), employers (who earned profits), and laborers (who earned wages). Quesnay’s (1758) famous *tableau economique* modeled the flow of goods (or money) from landowners to agricultural workers to artisans and merchants. Although Adam Smith focused less sharply on questions of distribution, David Ricardo (1817: 2–3, see also McNulty 1980: 64) went so far as to claim that: “But in different stages of society, the proportions of the whole produce of the earth which will

⁸For much of this period, the BEA was known as the Office of Business Economics, and before that the Bureau of Foreign and Domestic Commerce. For simplicity, I refer to it as the BEA throughout.

⁹For an extended discussion of the history of macroeconomics and what came before, see Chapter 2.

be allotted to each of these classes, under the names of rent, profit, and wages, will be essentially different. . . To determine the laws which regulate this distribution, is the principal problem in Political Economy.” Twentieth-century macroeconomics was not nearly so focused on distributional questions as Ricardo or his 19th-century successors, but when macroeconomists looked at inequality they did so with a Ricardian lens.

Although Ricardo and his followers theorized about the distribution of income between the classes — what we now call “factor shares” or “the functional distribution of income” as opposed to the personal distribution of income — they did so without much recourse to economic statistics. Estimating the distribution of income between the great classes became a primary motivation for the construction of national income statistics. This motivation is visible in the first national income statistics published by the Department of Commerce in 1934. The 1934 estimates were produced in response to a 1932 Senate resolution authored by Senator Robert M. LaFollette Jr. of Wisconsin.¹⁰ LaFollette was an outspoken proponent of the need for better economic measurement in general. In an interview given in March of 1932, LaFollette connected his push for better measurement of national income to debates about wage cuts in the Great Depression:

Likewise, with all the talk we have heard from bankers and others about the need for cutting wages and with all the actual wage-cutting that has taken place, we are woefully lacking in any adequate wage statistics. Also, while we are discussing the wages of labor, it is startling that we have no accurate information on the wages which capital is taking in the form of net profits from the point of view of industry as a whole. Furthermore, do you know that we have never had any official estimate of the total national income of the United States and the only authoritative information we have to go on is the estimate of an unofficial agency in 1929?¹¹

LaFollette’s language echoes Ricardo; the “wages of labor” and the profits of capital are at issue, not the wages of individual laborers or the incomes of particular capitalists. This focus on factor shares was reflected in the presentation of the 1934 report to the Sen-

¹⁰Senate Resolution 220, 72nd Congress.

¹¹“Radio interview between Mr. Charles Ross and Senator Robert M. LaFollette, Jr., over N.B.C., March 15, 1932, 8 p.m.” LaFollette Family Collection, Box I:C557, Library of Congress.

ate, usually characterized as the first official publication of national income statistics in the United States.¹² To estimate the sum total of income payments, Simon Kuznets (who led the team that produced the 1934 national income estimates) separated incomes first into “labor incomes” (wages), “property incomes” (rent), and “entrepreneurial incomes” (profit) before dividing them into finer subcategories (Kuznets 1934: 2). The first chart in the report similarly reports incomes broken down by type of payment — rents, interest, dividends, salaries, wages — operationalizing the three primary divisions of income into slightly more tractable subdivisions (Kuznets 1934: 15). Interestingly, although the 1934 report did not attempt to estimate the personal distribution of income, Kuznets noted the importance of such estimates for evaluating total national income data as a measure of well-being: “Economic welfare cannot be adequately measured, unless the personal distribution of income is known” (Kuznets 1934: 6). Kuznets’ later work, discussed below, made great strides in estimating that personal distribution, but such efforts were not integrated into the production or analysis of national income statistics.

Between the 1934 estimates and the end of World War II, national income statistics took on a decidedly Keynesian character. In part due to the influence of Keynes himself, who oversaw the production of the first official national income statistics in the UK in the early 1940s, national income statistics became increasingly interwoven with macroeconomic theory (Tily 2009). Rather than emphasizing factor shares of *income*, the national income accounts would increasingly focus on consumption, investment, and government *production*, leading to the famous formula $Y = C + I + G (+ NX)$ (gross national product equals consumption plus investment plus government spending [plus net exports]).

To the extent that post-war macroeconomists, and national income statisticians, did care about the distribution of income, they continued to focus on the role of factor shares. For example, as discussed above, macroeconomist Nicholas Kaldor (1961) identified the stability of labor’s share of national income as one of six “stylized facts” about macroe-

¹²This framing ignores an admittedly uninfluential and often-forgotten 1926 report by the Federal Trade Commission, see Carson 1971 for details.

conomics that later models would attempt to explain. The influential research program of Cobb-Douglas regressions looked at the relationship, in the aggregate, between labor's productivity and labor's share of income (McNulty 1980: 180, Biddle 2012). Similarly, the dominant macroeconomic forecasting models of the 1950s and 1960s included an analysis of labor's share of national income, but did not include measures of the distribution of personal income (Metcalf 1969).¹³ In this sense, data and theory were reinforcing: econometricians and macroeconomists theorized factor shares, which were conveniently already measured in the national accounts, which in turn had been shaped by macroeconomic theorists.

The path from the rise of Keynesianism to the exclusion of personal income data from macroeconomics was not entirely straightforward. One of the central contributions of Keynes' (1936) *General Theory of Employment, Interest, and Money* was the concept of the "marginal propensity to consume." Keynes argued that many dynamics, including perhaps most importantly the "multiplier" that related increases in government spending to increases in total output, depended on how much consumers would change their spending in response to receiving an additional dollar of income. If the marginal propensity to consume was high, then a bit of extra income created by government spending would multiply many times. Conversely, if the propensity was low, then extra government spending would do little to stimulate the economy. From the very beginning, macroeconomists theorized that the marginal propensity to consume was a function of personal income — individuals who made a lot of money were more likely to save additional income, while poorer individuals would likely spend almost all of it. Such theorizing suggested that the personal distribution of income might be relevant to Keynesian models, and there were some investigations along those lines (e.g. Stone and Stone 1938; see Fixler and Johnson 2012: 26 for details). These early efforts revealed small differences between models including and

¹³Metcalf's own work represents a partial exception; his dissertation attempted to integrate personal distribution of income into standard forecasting models. As Metcalf (1969: 657) notes, "The Brookings model, for instance, limits its consideration of the income distribution to a determination of factor shares. Issues relating directly to how families or income units are distributed by size of income were never raised."

excluding the distribution of income, and macroeconomists largely abandoned the study of the relationship between personal income distribution and the multiplier (Thomas 1992).¹⁴

Between 1930 and 1950, macroeconomics emerged as a fully-fledged field, with distinctive substantive theoretical concerns formalized in particular mathematical models, estimated with data drawn from the national accounts. Inequality was not central to macroeconomists' theorizing, and to the extent that macroeconomists did care about inequality, they measured it in terms of the classical formulation of labor and capital's respective shares of national income. The policy device that macroeconomists relied on to see the world — national income accounts — did not track personal distribution of income, and by the 1950s there was so little interest from macroeconomists that when the Commerce Department attempted to incorporate such measurements into the national accounts framework, the measures went unused. In contrast, labor economists did theorize and measure the personal distribution of income, but the regimes of perceptibility they constructed to track distribution ultimately limited their capacity to see changes in top incomes.

6.4.2 Gaps in Labor Economics

Like macroeconomics, labor economics emerged as a recognizable subfield in the first half of the 20th century. As McNulty (1980: 2) notes, in the early 19th century, less than 15% of American income earners were employees; by 1980, 90% of income earners were employees. The field of labor economics thus grew as the phenomenon of wage labor itself became dominant. When a recognizable subfield of labor economics did emerge in the late 19th and early 20th century, it remained outside of the mainstream. Labor markets were seen as something other than the idealized competitive market, dominated by regulations and unions (McNulty 1980). Unsurprisingly, institutionalist economists studied labor markets extensively in the first half of the 20th century, while neoclassical economists

¹⁴Fixler and Johnson (2012) present a recent exception that ultimately supports this claim. For prior studies on the effect of the personal distribution of income on the marginal propensity to consume, they cite only a handful of papers from the 1950s.

largely left the field unexamined. In the 1950s, as the institutionalist movement faded away, a new generation of neoclassical economists turned their attentions to the problems of labor, and in particular, the determination of personal incomes.

Neoclassical economists turned the question of the individual wages into a central focus of labor economics. Why did some people earn more money in the labor market than others? The answer developed in the 1960s was “human capital”, differential productivity developed through formal education and on-the-job training. Human capital theories displaced older approaches that either treated the personal distribution of income as resulting from largely random forces, or from inherited ability (Blaug 1976, Teixeira 2007, Sandmo 2013). In this section, I trace the history of human capital theory and how it came to study the personal distribution of income through large-scale surveys focused on measuring wages, work experience, and education. This history points to a deep irony: in the 1920s–1950s, studies of personal income distribution focused on top incomes *because* there was good tax data about them. In the 1960s–1990s, studies of personal income distribution *ignored* top incomes because there was no good survey data about them.

To understand the rise of human capital theory, we must first trace two earlier developments: early statistical studies of the personal distribution of income and the marginal productivity theory of distribution. Although the personal distribution of income was not a central topic to economics in the 19th and early 20th century, a small body of statistical work on the subject accumulated. In 1897, Vilfredo Pareto (most famous for the concept of “Pareto optimality”) identified a statistical pattern in the distribution of incomes now known as the Pareto distribution, a particular functional form that seemed to match the shape of the income distribution and in particular its heavy right-tail (see discussion in Champernowne 1952). Future works attempted to estimate the parameters for that distribution, and others, which best fit the observed distribution of income. For example, Champernowne (1952: 598) fits a modified Pareto distribution to distribution of income data from the US in 1918, while Rutherford (1955) fits a probit distribution to the log of income. To the extent

that personal incomes seemed to follow particular statistical patterns, economists sought to explain those patterns with random or “stochastic” models. For example, Champernowne (1973)¹⁵ and Wold and Whittle (1957) both develop stochastic models of income determination that reproduce the stylized fact that incomes follow a Pareto distribution. Notably, this whole style of research eschewed analysis of the characteristics of particular individuals, but rather focused on producing a model that matched the observed shape of the whole distribution.

In the same period that statistical research on the distribution of income took off (1890–1960), the mainstream of economic theory became increasingly dominated by an approach to understanding wages and productivity known as the “marginal productivity theory of distribution” (Pullen 2009). In general, “marginalism” refers to an understanding of economic decision-making in terms of the “margin” — how much is one more unit of a good worth? How much more could a business produce if it hired one more worker (or paid for one more hour of work)? And so on. The marginal revolution dates to the 1870s with nearly simultaneous publication of works in England (by Jevons), France (by Walras), and Austria (by Menger) espousing some form of marginalist analysis (Mirowski 1984). Although these authors differed dramatically in their methodological approaches and substantive conclusions, their insights were eventually unified into a seemingly coherent school of economics called marginalism (Blaug 1972, Jaffe 1976).

One of the most important substantive conclusions derived from marginalist analysis was the marginal productivity theory of distribution, developed by the American economist John Bates Clark. Clark, writing at the end of the 19th century, used calculus to conceptualize the marginal product of labor in terms of its partial derivative (Pullen 2009). In the context of distribution of personal income, marginal productivity suggests that, assuming a competitive market economy, an individual’s wage will be a function solely of their produc-

¹⁵This book was based on Champernowne’s undergraduate research completed in 1936, updated and republished as a book much later and thus reflects this older approach to the question of the distribution of income.

tivity. This theoretical link between wages and productivity persists into the present, and forms the backbone of most neoclassical analysis of wages and labor markets. In macroeconomics, such approaches were linked to the Cobb-Douglas regressions discussed in the previous section. In microeconomics, the marginal productivity theory was transformed in the mid–20th century with the development of human capital theory. Rather than explaining the statistical distribution of all incomes in terms of some underlying law, human capital theories would seek to explain differential productivity between groups of workers based on their education and work experience.

In 1958, Jacob Mincer published an article based on his doctoral dissertation, titled “Investment in Human Capital.” This article is widely regarded as kickstarting the human capital turn in labor economics (McNulty 1980, Teixeira 2007). In it, Mincer explicitly frames his research against the older, statistical models of personal income distribution:

Economists have long theorized about the nature or causes of inequality in personal incomes. In contrast, the vigorous development of empirical research in the field of personal income distribution is of recent origin. Moreover, the emphasis of contemporary research has been almost completely shifted from the study of the causes of inequality to the study of the facts and of their consequences for various aspects of economic activity, particularly consumer behavior. (Mincer 1958: 281)

By “study of the facts”, Mincer refers to the empirical estimation of the distribution of incomes associated with Pareto and Champernowne. Research on the “consequences for . . . consumer behavior” includes the short-lived line of research in macroeconomics connecting the personal distribution of income to the marginal propensity to consume. Mincer argued that economists needed to focus more attention on the causes of the distribution of income, and to connect such research to the paradigm of rational choice and marginal productivity. In other words, Mincer argued that economics could understand the distribution of income as a consequence of the choices individuals make about how to invest in their own productivity, especially by seeking formal education or on-the-job training. In a 1997 retrospective, Mincer also connected the rise of human capital theory to the inability of

1950s macroeconomics to adequately account for the sources of economic growth (Mincer 1997, quoted in Teixeira 2007: 17).

Soon, Mincer was joined by Theodore Schultz and Gary Becker in leading the charge for human capital theory. In 1962, they produced a special issue of *Journal of Political Economy*, which brought human capital theory squarely into the limelight.¹⁶ Blaug (1976: 827) documented the dramatic takeoff of human capital theory studies in the 1960s and 1970s, finding 800 articles on human capital theory published by 1966, and 2000 articles by 1976. Blaug characterized the dominant form of research in the human capital approach in terms of its exploration of the “earnings function”:

The [human capital research] program adds up to an almost total explanation of the determinants of earnings from employment, predicting declining investment in human-capital with increasing age, and hence lifetime earnings profiles that are concave from below. No wonder the bulk of empirical work inspired by the human-capital framework has taken the form of regressing the earnings of individuals on such variables as native ability, family background, place of residence, years of schooling, years of work experience, occupational status, and the like—the so-called “earnings function.” (Blaug 1976: 832)

Several important observations follow for our story. First, human capital theory was interested in *earnings* or wages, not in all sources of income. Second, human capital research was interested in explaining variation in individual earnings in terms of a small set of important covariates, including especially education. Third, the human capital approach virtually invented the idea of “earnings regressions.” As strange as it may seem in retrospect, before the 1960s, economists did not run — and were largely not capable of running — regression models predicting earnings with a set of individual covariates. In part, economists avoided earnings regressions because the variations in individual earnings were not seen as an important theoretical topic (as opposed to, say, the aggregate labor share). But economists were also limited by the relative paucity of data. Fortunately, just as the

¹⁶There is a bit of a priority dispute over which of Mincer and Schultz should receive more credit for human capital theory. Some of Schultz’s work in the 1950s makes similar conceptual moves to Mincer’s dissertation (e.g. Schultz 1950). That said, commenters agree that the 1962 special issue marked the widespread takeoff of the approach. I thank Beth Berman for drawing my attention to Schultz’s early work.

human capital revolution was underway in economic theory, the microdata revolution was taking place in official statistics.

The rise of the large-scale representative survey is a relatively recent phenomenon (Igo 2007). The first major representative surveys took place during the Great Depression, such as the famous Gallup Poll predicting Roosevelt's 1936 electoral victory over Landon. The technique expanded dramatically in the 1940s, including the 1940 launch of the "Monthly Report of Unemployment", which became the influential Current Population Survey in 1948 (Bureau of Labor Statistics 1984: 8). The CPS, along with a handful of other large-scale, government-funded, representative surveys like the Panel Study on Income Dynamics (PSID, launched in 1968), would become the tools of choice for labor economists interested in understanding not just the overall shape of the income distribution, but also the covariates that predicted income (age, education, gender, and race). The availability of individual-level "microdata" contributed to the shift in the field of labor from studies of the demand for labor (firms) to studies of the supply of labor (individual laborers), and especially to the success of human capital theory and its earnings regressions. According to Stafford (1986: 388), in the 1970s, "about two-thirds of labor articles in major journals were on the broad subjects of labor supply and wage determination", and by the early 1980s, about two-thirds of empirical labor economics articles in top journals used individual-level microdata, and half of those drew on just three surveys (CPS, PSID and the National Longitudinal Survey) (Stafford 1986: 395).

Surveys fit the research program of human capital theory nicely. Surveys were relatively less effective than tax data at capturing financial income and profits, but were relatively good at measuring earnings from labor. And large-scale demographic surveys like the CPS had the significant advantage of including all the covariates a researcher could want, especially education variables. As human capital theory increased in influence, its interests became even better reflected in the data collection practices of government surveys:

Prior to the advent of human capital models of lifetime earnings, most sets

of microdata did not have much information on work histories of individuals. Early work on earnings functions was commonly based on years of potential labor market experience, defined in terms of age and years of formal schooling. Because the theory emphasized the importance of on-the-job training through various types of job market experience, new and on-going data collection efforts obtained extensive information on job market experience. Variables such as years of full time experience, years of part time experience, and years in military service became widely available in cross-sectional data. (Stafford 1986: 398)

Conversely, because advances in survey-based microdata were driven by pressure from labor economists, and labor economists did not particularly care about non-wage income, improving measurement of such income was not a priority. Similarly, labor economists interested in the returns to a college degree or on-the-job training were satisfied with surveys that captured the middle 90% of the population well at the expense of known problems with the reporting of income at both the top and bottom of the distribution. Top incomes were especially poorly captured due to the problem of top coding. Thus, for researchers relying on the CPS, incomes at or above the 97% (approximately) were simply unobservable, and researchers either ignored these incomes or made static imputations about their size (i.e. assuming that there was no trend in top incomes, as in Katz and Autor 1999: 1471).

By the late 1970s, then, the modern regime of perceptibility for labor economics had come together. Routinely conducted, large-scale, government-led surveys provided individual-level microdata which provided good measurements of labor earnings and theoretically important covariates, but poor coverage of top incomes. These data were analyzed with the goal of understanding how individuals chose to invest in education and job training over the life course, and to measure the returns to a college degree, along with other predictors of individual earnings.¹⁷ That surveys were technically incapable of measuring top incomes was simply not seen as a pressing problem.

¹⁷These data were also well-suited to other questions of policy concern, including those asked by sociologists studying income inequality, such as measuring racial and gender gaps. See discussion below.

6.4.3 From First Priority to Orphan Estimates

Contemporary discussions of the microdata revolution in empirical economics contrast microdata to older, aggregate statistics, including the national income accounts. Historically, the situation is a bit messier. As discussed in this section, during the 1950s to 1970s, government statisticians worked hard to integrate microdata and the national accounts in order to produce comprehensive statistics on the distribution of income that would be more useful than either on their own. These statistics were considered the number one statistical priority by economists in government in the 1940s. Yet, when the BEA began to publish such data in the 1950s, they were barely noticed. When funds to update and maintain the data could not be secured, the BEA ended the estimates, again, to little fanfare. What happened? This section traces one possible way that the personal distribution of income could have been incorporated into prominent policy devices in a way that would have made changes in top incomes more visible, and connects the previous discussions of theoretical trends in macroeconomics and labor economics to the failure of this incorporation.

Following World War II, Congress's newly established Joint Committee on the Economic Report (later renamed the Joint Economic Committee, or JEC) laid out a series of "statistical gaps" that federal statistical agencies were charged with filling. Number one on this list was information on the personal distribution of income (JEC 1949: 86). In the 1950s, the BEA turned its resources to this task. In 1953, the BEA published estimates for the distribution of income in the United States from 1944–1950 (see Figure 6.4 for a graphical presentation of the 1950 estimate).¹⁸ By this time, national income data were published quarterly, and each of these quarterly reports received news coverage, much as they still do today. In contrast, of the 71 stories that referenced the "Office of Business Economics" in the *New York Times* in 1953 and 1954, just one mentioned the income distribution study. The story, "Family Income Up \$850 in Six Years", emphasized the gains in

¹⁸Office of Business Economics. 1953. *Size Distributions of Income in the US 1944–1950*.

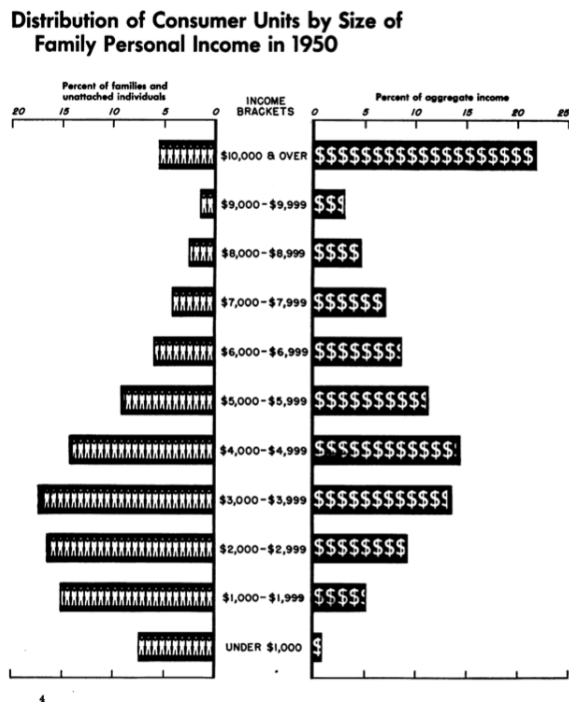


Figure 6.4: A graphical representation of the distribution of pre-tax income in the United States in 1950 (BEA 1953: 4).

average income, discussing the distribution of income in just a few sentences at the end.¹⁹ The BEA report itself argued for the importance of distribution data in “market analysis”, with little mention of academic or policy implications.

Later efforts by the BEA to produce data on the distribution of income would meet with similarly little interest, at least publicly. The BEA produced annual estimates of income distribution from 1955 to 1964, each updating the series to approximately two years prior (i.e. the 1964 report covered up to the year 1962).²⁰ The BEA sought funds from Congress to update the series, and better integrate survey data from the Census and Federal Reserve, but the funds were not forthcoming. Despite being deemed the most accurate and comprehensive available measurements of the distribution of income by a report to the JEC (Schultz 1964), and despite personal income distribution having been the number one statistical priority as recently as 1949, Congress denied a request for \$60,000 per year to

¹⁹*New York Times*, 1953, October 12.

²⁰See, e.g., *Survey of Current Business*, March 1955 and *Survey of Current Business*, March 1964.

fund four permanent positions dedicated to the income distribution data. The BEA tried to link its research to the newly prominent “war on poverty”, but failed to sway skeptical representatives of the need for the data.²¹ The BEA would have been hard-pressed to point to massive interest from outside; for example, the *New York Times* makes no mention of the BEA’s 1963 or 1964 publications estimating the distribution of income. The series then stopped abruptly.

In 1974, the BEA once more restarted its work on income distribution. The publication included estimates through 1971. Unlike the more recent Piketty and Saez data, the BEA’s estimates included some sociodemographic variables, as they combined data from the Current Population Survey alongside tax data and other sources (Radner and Hinrichs 1974). Thus, in some sense, these data could have been useful to labor economists interested in “gap research”. It is difficult to speculate exactly why labor economists did not draw on these data, but we can see why their priorities were already better served with the widely available survey data – the BEA had a few demographic covariates, but not detailed work histories, and the BEA’s major work was to reconcile survey-based measures of income with the national accounts’ particular definition of income. But labor economists, interested primarily in *earnings* and not total income, would have little to gain from these reconciled measurements. Once again, the BEA’s estimates received little to no public reac-

²¹Congressional Hearing Transcripts, Budget Hearings for the Department of Commerce, 1965. In a 50 year retrospective on the BEA’s monthly publication, the Survey of Current Business, Lebergott (1971: 117) made a similar argument connecting the war on poverty to the need for good income distribution data:

In the early sixties the Nation’s concern with poverty began escalating. Publicists offered explanation after expostulation, Congress passed program after program. And OBE stopped publishing the only adjusted, and the most reliable, estimates of the U.S. income distribution. Now these data — indeed, any good data — may have been irrelevant either to public concern or to policy choice. But the flow of references to what ‘official’ income distribution figures show continues unabated. Hence it would seem desirable once again to have income estimates that allow for known limitations of the field survey and IRS data. (The adjustment procedure tended to reduce the proportion of families that were reported as, but were not actually, low income.) One hopes that the tiny amount of expenditure and the mild extent of interagency cooperation required to reinstitute the series on a solid basis will be forthcoming during the next fifty years.

In spite of Lebergott’s optimism, such expenditure has not been forthcoming.

tion (e.g. no mention in the *Times*). The 1974 estimates were not repeated in the following year, and the BEA has not published estimates of the distribution of income since.

By the end of the 1970s, the Federal Government had flirted with producing income distribution estimates compatible with the National Income and Product Accounts. These efforts met with little interest from scholars and the public, and were discontinued. What remained were survey-based measures derived from the Census's Current Population Survey, and similar surveys conducted by the Bureau of Labor Statistics and the Federal Reserve to measure prices and consumption. These surveys captured the bulk of income — although with some recognized problems of underreporting at the top and bottom²² — and they were ideally suited to the research agendas of those interested in sociodemographic forms of inequality rather than characterizing the distribution as a whole. These statistics did not, however, accurately measure all forms of income (such as capital gains), and they suffered from the problem of top-coding, thus making them largely incapable of tracking movements at the top of the income distribution. In terms of data, economic growth and income inequality were unlinked — no dataset tracked both in comparable ways. This disconnect in the data used to study inequality and growth came just as a thirty-year span of decreasing inequality would itself come to an end.

6.5 The Stylized Facts of Inequality

Having traced the construction of two disjoint regimes of perceptibility for inequality, we are now better positioned to understand how economists knew what they knew about inequality in the 1980s–1990s, and how they were capable of missing new trends in top incomes.

In the 1980s and 1990s, labor economists both noticed and attempted to explain in-

²²The BEA argued before Congress that the Census surveys only captured about 90% of all income, due to problems of underreporting, top-coding, and some categories that were simply not measured. Congressional Hearing Transcripts, Budget Hearings for the Department of Commerce, 1965.

creases in inequality in the United States. The dominant characterization of this increase focused on the growing gap between the wages of college-educated and non-college educated workers. One influential, and reasonably representative example is Bound and Johnson (1992). Bound and Johnson document the stagnation of average wages in the 1980s, as well as dramatic shifts in the relative wages of individuals. Examining CPS data, Bound and Johnson measure changes in the relative wages of individuals based on years of work experience, education, and sex. In the end, they conclude that “skill-biased technological change” has driven major increases in the wage gap between college and non-college educated workers. The emphasis here, as in much of the literature, is specifically on wage inequality, which is understood as an indicator of productivity (following from the marginal productivity theory of distribution and human capital theory).

Later work in the 1990s would debate the relative influence of international trade and globalization as alternatives to technological change as explanations for the observed gaps, as well as the persistence of race and gender gaps (see Card and DiNardo 2002 for one summary of the literature).²³ While labor economists in the 1990s recognized the increasing right skew in the income distribution, they did not link it specifically to the growth of incomes at the very top, nor did they marshal data capable of explaining — or even *seeing* — movement at the very top. For labor economists (and sociologists) interested in inequality, the growth of top incomes was almost invisible.

The focus of inequality researchers on “gaps” is mirrored in popular coverage of inequality during the 1980s–1990s. McCall (2013: 53–95) analyzes news coverage of inequality during this period and finds it to be very fragmented. Explicit discussions of economic inequality or the overall distribution of income are largely absent. Instead, like labor economists, public discourse focused in earnings disparities and education:

[The] earliest debates about income inequality seeped into the media as re-

²³Similarly, work by sociologists interested in inequality would focus on persistent wage gaps. Leicht (2008) summarizes sociological research on inequality in the 1990s as “dominated by studies of racial and gender gaps”, in contrast with economists’ focus on education, technology, and trade. See Morris and Western (1999) for an excellent example that, like much work by economists, relies primarily on the CPS.

searchers began to notice the increase in the 1980s. Because earnings are the main component of income, and earnings disparities appeared to be rising in the 1980s, researchers focused at the outset more on the widening disparity in hourly *earnings* between high-skilled and low-skilled workers in the *labor market* than on disparities in *incomes* between rich and poor *families*. (McCall 2013: 59, emphasis in the original)

McCall did not find an increase in discussions of inequality in the popular press as inequality itself increased throughout the 1980s–2000s. Rather, the nature of the discussion shifted. McCall (2013: 59) finds that inequality debates shift from a focus on earnings and the labor market to a focus on incomes and tax policy: “Remarkably, the role of tax policies in affecting top incomes did not emerge as a focal part of the academic story until the early 2000s, when new ‘facts’ about income inequality became available.” In other words, at least in this case, popular discussion followed from the trends in academic research. In the 1980s–1990s, labor economists identified earnings disparities as the novel stylized fact of interest, and public discourse focused on issues like the minimum wage, trade, and education. In the 2000s, as we will see, the identification of the stylized fact of increasing top income inequality would alter the landscape of public debate about inequality.

Macroeconomists, for their part, mostly ignored the personal distribution of income in favor of continued debate about the stability of labor’s share of national income. One partial exception was the continued debate over the Kuznets’ curve in development economics. In his 1954 presidential address to the American Economics Association, Simon Kuznets (1955) theorized an upside-down U-shaped relationship between economic growth and income inequality. As countries developed, inequality would first increase (as workers moved from the rural to urban sectors) and then decrease (as various forces pushed up wages). Kuznets relied on sketchy data from the late 19th and early 20th century in the US and UK, which roughly followed this pattern. In the 1970s, consensus held that Kuznets’ curve was real, although better longitudinal data produced in the 1980s and 1990s troubled this consensus. The early acceptance of Kuznets’ hypothesis ironically led researchers to argue that increasing inequality in development would resolve itself as development pro-

ceeded, and thus incentivized researchers to *not* focus much on inequality (see Moran 2005 for a detailed history). Most of the debate about the curve itself concerned inequality in developing countries (not the US), and ultimately relied on survey-based measurements of inequality, which were still incapable of seeing top inequality.

Those macroeconomists who were more squarely focused on the United States largely ignored inequality in favor of discussions of the big three macroeconomic variables: growth, inflation, and unemployment. Dominant macroeconomics textbooks, for example, included lengthy discussion of labor and capital's share of income, but no discussion of income inequality. Mankiw's (2003, 5th edition) intermediate textbook is an influential exemplar: the entire book contains just one reference to economic inequality, and it is in the context of unemployment (Mankiw 2003: 173). Labor's share, on the other hand, is discussed and even modeled prominently in the contexts of the Cobb-Douglas production function (2003: 72–73) and growth accounting models (Mankiw 2003: 229–231). Labor's share was also a continued object of research, from debates about how best to measure it (Krueger 1999) to empirical observations of its decline starting in the 1980s (Gordon 1988).

Thus, the stylized facts of inequality in the 1980s and 1990s included an impressive and important list of findings: the widening gulf between highly educated and less educated workers, the persistence of race and gender gaps, and the overall decline in labor's share of national income. But, in general, the two largest groups of economists potentially interested in income inequality were incapable of seeing, and theoretically disinclined to look for, changes in top incomes.

6.5.1 Krugman and the 1992 Presidential Election

Although most sociologists and economists conceptualized and analyzed inequality in ways that left the growth of top incomes of the 1% invisible, there were a few notable exceptions. Economists specifically interested in taxation followed more closely the IRS's Statistics of Income, and the various tax models used by the Federal Government to produce revenue es-

timates. Drawing on these data, Feenberg and Poterba (1993) noted the growth of incomes of the top 0.25%. This paper appears to have attracted some notice from fellow tax scholars, and from Piketty and Saez who cite the paper as a precursor, but had little resonance in the broader economics literature. Feenberg and Poterba would repeat their argument that inequality scholars failed to pay sufficient attention to the very top incomes through the 1990s, and explicitly noted the problem of top-coded survey data as an impediment to studies of this income group (Feenberg and Poterba 2000).

More surprising than this academic outlier is Paul Krugman's highly public, and highly political, 1992 calculation of the income gains of the top 1%. In the early 1990s, Paul Krugman was a noted international trade theorist in the process of transitioning to his current status as one of economics' leading public intellectuals. His first major book for popular audiences, *The Age of Diminished Expectations*, was published in 1990 and included a very short chapter on income distribution, noting the growing wage gap between more and less educated workers, and a brief analysis of the divergence between the incomes of the top 10% (Krugman 1990: 19–25). In an interview, Krugman (1998) later noted that his editors objected to this chapter's inclusion: "For complicated reasons, that book was initially a *Washington Post* project. And my editors at the Post tried to pressure me into taking the income distribution chapter out, saying that nobody cared about that issue."

Two years later, Krugman published a more surprising finding. Based on a Congressional Budget Office (CBO) publication with information derived from tax data, Krugman argued that between 1977 and 1989, 60% of the income gained by the country went to the top 1% of income earners. This finding was popularized in the *New York Times*, criticized by the *Wall St. Journal* and the Bush Treasury Department, and eventually featured in stump speeches by then presidential candidate Bill Clinton. The CBO eventually responded to the critiques with a study of its own, supporting Krugman's overall finding, with a caveat that the number dropped a bit when adjusted for the size of families (a common practice at the CBO, though not common in many other income datasets). One *Times* summary of the

debate is particularly revealing:

One reason that the issue remained relatively invisible for so long – in spite of broad agreement among academic economists that income and wealth have grown more unequal – is that statistics on distribution are among the mushiest and murkiest in economics. *There are literally dozens of ways to slice and dice the data, not to mention hundreds of different data series.* Confronted by Republican legislators after Governor Clinton started peppering his speeches with the statistic on the top 1 percent, the budget office was called upon to assess the Krugman calculation. Several weeks later, it issued a report that gave a number of alternate measures of the gains by the top 1 percent. Every measure showed that the top 1 percent of families reaped an outsized share of the gains. By one calculation 70 percent of the rise in average after-tax family income went to the top 1 percent, rather than the 60 percent figure that Mr. Krugman had initially estimated and that Governor Clinton has been using.²⁴ [Emphasis added.]

Krugman's 1992 calculation represents a significant challenge to the initial premise of this paper — that the growth of the top 1% was a surprising finding in the early 2000s. Clearly, there was some political awareness in 1992 that the incomes of the top 1% had grown dramatically, and thus the Piketty and Saez findings could not be a complete surprise. Yet, despite the political salience of Krugman's calculation during the 1992 election, the stylized fact that the top 1% had captured most of the income gains of the 1980s and grown to an historically unprecedented level did not take hold. Without a recognized place among academics, or even a tight connection to a particular statistical agency or dataset, it became just one more economic fact bandied about in a presidential election. One piece of evidence for this claim is Krugman's own later academic work. In 1995, Krugman published a foray into the debates about the role of trade in increasing inequality.²⁵ This paper makes no mention of the growth of the top 1%, instead sticking to the mainstream academic debate about the increasing education wage gap. Similarly, a broader sample of articles from JStor's database suggests a large increase in discussions of the top 1% in academic publications about income inequality in the 2000s as compared to the late

²⁴“Richest Getting Richer Now Top Political Issue.” *New York Times*. 1992. May 11.

²⁵“Growing World Trade: Causes and Consequences.” *Brookings Papers on Economic Activity* 1:327–377. Krugman's primary conclusion is that increased world trade is not responsible for growing income inequality.

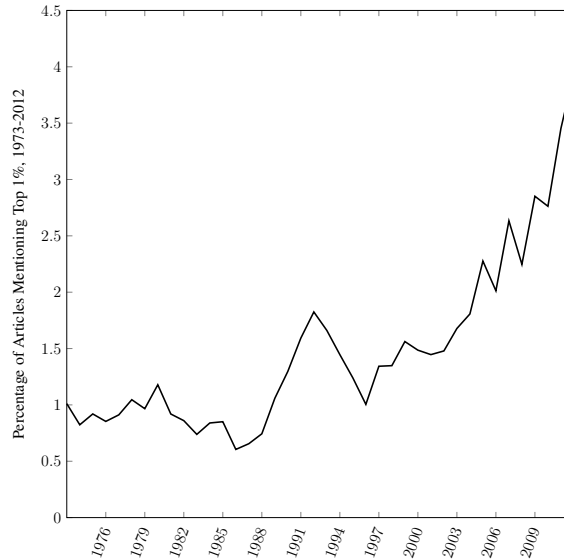


Figure 6.5: Three-year moving average of the percentage of all research articles in JStor’s *Data For Researchers* database that mention “income inequality” or “income distribution” and also “top 1%”, “top one percent” or “top 1 percent” from 1973–2012. The data are noisy — not every reference to the “top 1%” will refer to the growth of top incomes — but suggestive of a slight, temporary increase in 1992 followed by a large increase post-2000.

1990s (see Figure 6.5). Finally, Krugman’s calculation also focused on a very narrow time-window — 1977–1989, corresponding to the period covered by the CBO data. It would be Piketty and Saez who would go back to the IRS data at the source of the CBO model and construct a series that put contemporary top incomes into a much longer historical perspective.

6.5.2 Rediscovering the 1%

In 2001, Piketty and Saez (in this section abbreviated as P&S) published a working paper containing estimates of the share of income accruing to the top 5% and 1% of income earners in the United States from 1913 to 1998. Their working paper relied heavily on tax data, combined with national income accounts data to determine some of the relevant denominators (e.g. total wage income). Their estimates differed from Feenberg and Poterba (1993, 2000) in two interesting respects. First, P&S estimate a consistent series back to 1913, several decades earlier than Feenberg and Poterba. Second, in order to extend their series back

further, P&S use a slightly more complicated method to generate their denominators, in order to take into account the much smaller percentage of income that was reported on tax returns in earlier periods. All told, P&S produce a dataset capable of making the provocative claim that the share of income accruing to the top 1% had returned to levels not seen since the Great Depression. Additionally, Piketty and Saez note that the top 1% increasingly derived their incomes from wages and salary, and not simply from capital gains, thus rejecting a sharp division between their work on overall income inequality and the work of labor economists focused solely on wage income.

It is hard to say why Piketty and Saez's research had such a large academic and political impact.²⁶ That their data series extended back to the Great Depression may have played a large role in capturing attention. Additionally, the early 2000s saw researchers interested in more conventional wage inequality (the labor economists above) casting about for new explanations. Katz and Autor's (1999) review of the literature on changes in inequality in the United States noted some evidence that the top incomes had grown dramatically (even as all of their own quantitative evidence, relying on the CPS, analyzed only incomes up to the 97th percentile). Specifically, Katz and Autor (1999: 1468) cited growing evidence on the tremendous growth of CEO and athlete pay (e.g. Hall and Liebman 1998). It is possible that Piketty and Saez's work caught on academically because it helped make sense of increasing evidence from the labor economics literature that something interesting was happening at the top of the income distribution which had not previously been fully measured, and which did not easily fit within the framework of human capital theory.

Piketty and Saez also benefitted from early and positive attention from the media.²⁷ Specifically, Paul Krugman, by then a renowned columnist at the *New York Times*, wrote up

²⁶As of August, 2014, the main paper (published in working paper form in 2001, in the *Quarterly Journal of Economics* in 2003, and updated since) has 1825 citations on Google Scholar.

²⁷One critic, Alan Reynolds (2007: 2), claimed: "After 35 years of writing on economic issues, I do not recall any other private and unofficial estimates that were as widely and uncritically repeated as the Piketty-Saez estimates on income shares of the top 1 percent. ... Searching Google for "Emmanuel Saez" in early October turned up 51,700 entries, including 871 that also involved the *New York Times*. Similar searches yielded 814 joint references to Saez and the *Washington Post*, 568 for the *Wall Street Journal*, 375 for the *Financial Times*, and 319 for *USA Today*."

an extensive magazine piece drawing heavily on their research. In “For Richer”, Krugman (2002) argued that the US had entered a “new Gilded Age.” Krugman rehearsed 15 years of CPS-based studies that showed increases in income in the top 5%, starting in the late 1970s.²⁸ But Krugman then went on to claim that studies that relied on categories as big as the top 10% or 5% missed the big change in the new Gilded Age:

Most of the gains in the share of the top 10 percent of taxpayers over the past 30 years were actually gains to the top 1 percent, rather than the next 9 percent. In 1998 the top 1 percent started at \$230,000. In turn, 60 percent of the gains of that top 1 percent went to the top 0.1 percent, those with incomes of more than \$790,000. And almost half of those gains went to a mere 13,000 taxpayers, the top 0.01 percent, who had an income of at least \$3.6 million and an average income of \$17 million. (Krugman 2002)

Krugman continued on to say that the three main arguments (globalization, skill-biased technological change, and the so-called “superstar” hypothesis) economists had generated to try to understand growing income inequality in the 1980s–1990s seemed inadequate in the face of the Piketty and Saez data:

Globalization can explain part of the relative decline in blue-collar wages, but it can’t explain the 2,500 percent rise in C.E.O. incomes. Technology may explain why the salary premium associated with a college education has risen, but it’s hard to match up with the huge increase in inequality among the college-educated, with little progress for many but gigantic gains at the top. The superstar theory works for Jay Leno, but not for the thousands of people who have become awesomely rich without going on TV. (Krugman 2002)

Instead, economists would need to bring in social and political factors, like social norms, to make sense in the massive increases in executive pay that helped to drive the incomes of the 1%. Other economists, sociologists, and political scientists would build off of P&S’s findings to make claims for the importance of politics (Hacker and Pierson 2010), financialization (Kaplan and Rauh 2007), the rise of agency theory, the death of unions, and more. And Piketty (2014) himself would write a bestselling work connecting the growth

²⁸Perhaps surprisingly, Krugman makes no mention of his own earlier 1992 calculations on the growth of the top 1%, showing just how tangential they had been to the larger conversation on inequality in this period.

of top incomes to newly-discovered “fundamental laws of capitalism” relating the returns on capital, the rate of economic growth, and concentration of income at the very top. The stylized fact of the growth of the top 1% had entered into academic debates in force and gave force to new and different theories.

Not all economists bought into the P&S findings uncritically. Conservative think tanks continued their war on all findings of increased income inequality, much as they contested Krugman’s 1992 calculation. Reynolds (2007) published an influential critique in the Cato Institute’s journal *Policy Analysis*. Reynolds claimed that P&S dramatically overstated the rise of the top 1% by misusing tax data, and that their estimates were very inconsistent with the story told by survey data.²⁹ P&S show a change in recorded, taxable income accruing to the top 1%, but Reynolds argues that this observation is consistent with a shift in *how* the 1% earned their money rather than the total *amount* the top 1% earned: Top-earners responded to changes in the tax law (specifically, lowering the top marginal tax rates), by shifting income from corporate tax filings to individual tax filings. By relying solely on pre-tax income, P&S failed to capture the government benefits paid out to some of the bottom 90%, and thus underestimated the bottom at the same time as they overestimated the top. Reynolds summarized his findings:

[S]tudies based on tax return data provide highly misleading comparisons of changes to the U.S. income distribution because of dramatic changes in tax rules and tax reporting in recent decades. Aside from stock option windfalls during the late-1990s stock-market boom, there is little evidence of a significant or sustained increase in the inequality of U.S. incomes, wages, consumption, or wealth over the past 20 years. Reynolds (2007: 1)

P&S responded to Reynolds point-by-point (Piketty and Saez 2006), noting that the discrepancies between their findings and survey-based measures are completely logical, and in fact, the main novelty of their study:

The reason for the discrepancy is that the Census Bureau estimates are based

²⁹“If the Piketty and Saez estimates actually demonstrated a continuous and credible upward trend toward greater inequality since the late-1980s, all other estimates of income distribution would have to be wrong — including those of the Census Bureau, the CBO, and the Federal Reserve Board.” (Reynolds 2007: 18)

on survey data which are not suitable to study high incomes because of small sample size and top coding of very high incomes. In contrast, tax return data provide a very accurate picture of reported incomes at the top. Our key contribution was precisely to use those tax data to construct better inequality estimates.

My point here is not to claim that P&S are right and Reynolds is wrong, or vice versa. Rather, I simply want to document just how much was still up in the air in the mid-2000s (and indeed, even now) in terms of the measurement of inequality. Although extensive debates still exist about the measurement of unemployment, inflation, and economic growth, the existence of widely accepted, bureaucratically produced, official, and timely measures institutionalized in policy devices forestalls many of the sorts of challenges present in both the 1990s “Krugman calculation” debate and in the critical reception of P&S.³⁰ Basic definitional issues — what kinds of income should be studied (wage, capital gains, non-monetary remuneration, before tax or after tax and transfer, etc.), the units of analysis (families, households, individuals, “tax units”), and the relevant metrics for comparison (top 1% or .1% income shares, 90/50 or 90/10 ratios, Gini coefficients) — are all still live controversies in the study of income inequality. Inequality is not unknown, nor unknowable. But even now, it remains knowable at some remove, an ill-defined structural fact about the US political economy to be debated and pondered, not an immediately relevant indicator of overall economic well-being or policy success.

6.6 Discussion

6.6.1 Modeling Economic Influence

What can the rediscovery of the 1% teach us about the political power of economic experts?

Here, I draw on the case to produce a simplified model of policy-relevant economic knowl-

³⁰Note that the U.S. government still does not produce income distribution data similar to the Piketty and Saez data. For example, a recent controversial Congressional Research Service report (Hungerford 2012) on the relationship between top tax rates and economic growth relied on P&S’s data as the best existing data on top incomes.

edge claims focusing on the interactions of narrow observational claims, stylized facts, and full-blown causal or explanatory theories. I make two key interventions. First, I argue that all three kinds of economic knowledge claims may independently enter into the policy process and public debates. As in the case of the 1%, this model recognizes the capacity for stylized fact claims to enter into policymaking even in the absence of elaborated causal theories.³¹ Second, I argue that dominant causal theories from previous eras shape contemporary economic regimes of perceptibility, which in turn limit the range of data that may be observed, and shapes the interpretation of those data.

The model here relies on complicating the distinction between observation statements and causal theories through further analysis of the concept of stylized facts. A long tradition in the history and philosophy of science has complicated the clean divide between observations and theories, perhaps most famously in Kuhn's (1969) argument that data are "theory-laden". More recently, and concretely, Edwards (2010) has shown how modern climate data could not exist in the absence of theoretical models. Inspired by this research, I argue that economic knowledge claims can usefully be divided into three broad ideal types:

1. Narrow observational statements that are clearly bounded in time and place and linked to specific data. Example: "Real gross domestic product – the output of goods and services produced by labor and property located in the United States – increased at an annual rate of 4.2 percent in the second quarter of 2014."³²
2. The identification of simple empirical regularities in need of explanation, including trends, rates of incidence, and bivariate associations. In economics, such regularities are often explicitly named as stylized facts, following the usage pioneered by Kaldor (1961) who identified a collection of macroeconomic trends and argued that further research in macroeconomics should attempt to explain those broad trends, rather than focus on noisy data. Perhaps the most famous of these "Kaldor facts" is the claim that labor's share of national income remains roughly constant over time (a claim to which we will return in the discussion of macroeconomists' approaches to inequality). Similarly, the claim that top incomes increased dramatically constitutes a stylized fact (a trend abstracted from noisy data).

³¹Conversely, this model also recognizes the possibility that theories might become influential even in the absence of compelling support from observational data.

³²<http://www.bea.gov/newsreleases/national/gdp/gdpnewsrelease.htm>, accessed August 30, 2014.

3. Causal theories or explanatory models. While stylized facts abstract noisy data into clean trends, causal theories try to explain *why* the trends characterized by stylized facts exist. Stylized facts, in this sense, are the explananda for causal theories, and serve as analogs to “phenomena” in the natural sciences (cf. Hacking 1983). That is, while physicists might come up with theories to explain a newly observed phenomenon (a predictable reaction of the physical world to a particular intervention), economists come up with theories to explain relatively stable trends and rates identified as stylized facts. Families of causal theories constitute the broad policy paradigms we know and love — Keynesianism, monetarism, human capital theory, and so on.

Within the confines of the academic field, the relationship between these three kinds of knowledge claims is seemingly transparent. Narrow observation statements accumulate and are interpreted and processed into cleaner stylized facts. These stylized facts then serve as the explananda for causal theories. This process is depicted in Figure 6.6. In mainstream American economics, at least since the 1930s, causal theories usually take the form of explicit mathematical models (Weintraub 2002). Much as climate modelers seek to reproduce key features of the observed temperature record in their simulations (Edwards 2010), economic modelers try to reproduce stylized facts by varying their assumptions about, e.g., frictions in various markets, information asymmetries, and so on. Progress happens through the identification of new stylized facts, the construction of better explanatory models, and eventually, through the accumulation of new narrow observations which seem to deviate from our expectations (“anomalies” in Kuhnian terms) and which eventually may lead to whole new families of theories. In this sense, the rediscovery of the 1% fits nicely: labor economists observed irregularities in executive pay and other top earners, new data were analyzed to reveal a previously unspotted trend in top incomes, and this trend motivated new theories emphasizing political institutions in place of human capital.

These three kinds of knowledge claims do not remain confined to the academic field, however. All three may independently enter into the policy process or public discourse in influential ways (as depicted in Figure 6.7). Narrow observations may inform policy responses, as when early estimates of the decline in GDP in 2008 caused by the financial



Figure 6.6: Stylized facts mediate the link between observations and theories.

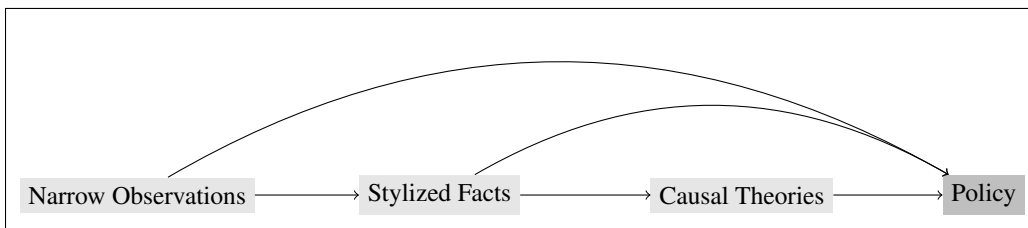


Figure 6.7: A more complex model highlighting the direct influence of observations and stylized facts on policy.

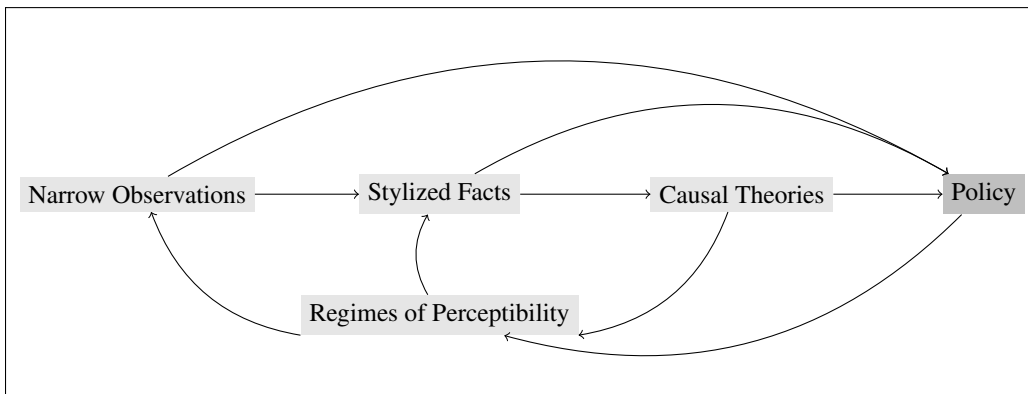


Figure 6.8: This model highlights how causal theories and policies together produce regimes of perceptibility, including institutionalized policy devices for seeing, that influence which observations are made and how those observations are combined into stylized facts.

crisis underestimated the extent of the recession, and (according to some commentators) led to the Obama administration pursuing a smaller fiscal stimulus than was needed to prop up the economy (Grunwald 2012, see chapter 4 for details). As we saw, stylized facts can have a powerful influence on public understandings of economic problems, as when the identification of the rise of the 1% reshaped contemporary American understandings of inequality. Finally, broad economic theories may guide the actions of economists in positions of institutional authority (like heads of central banks).

Beyond their direct influence on particular policy debates, and, crucially for understanding the history of inequality knowledge, prior theories may also condition the production of new methods of observation (i.e. policy devices for seeing) and thus limit the kinds of new narrow observations and stylized facts which accumulate. Put another way: if economists theorize that X is irrelevant to their current theoretical understanding of some process, they will not advocate for (or produce on their own) systems of observation capable of tracking X. Thus, our current regimes of perceptibility are shaped by past understandings of what was worth observing (as depicted in Figure 6.8). Human capital theory shaped the production of data about inequality, leading economists to advocate for better microdata produced by increasingly detailed surveys and, simultaneously, led economists to abandon the study of tax records (and the production of easy to use secondary data sets based on tax records).

Thus, in addition to incorporating our existing understandings of the role of economics in policy, this model suggests two novel paths of influence: the direct role of stylized facts in shaping policy debates, and the indirect role of economic theories on policy through their shaping of our regimes of perceptibility. The history of inequality knowledge in the post-WWII United States nicely demonstrates both of these paths.

6.6.2 Ignorance and Sustained Attention

Beyond this conceptual model of the economic knowledge and policy, the rediscovery of the 1% also contributes to ongoing discussions around the production of ignorance by high-

lighting the importance of sustained attention. Most of the existing research on agnotology follows one of two paths. The first studies the intentional, strategic production of ignorance, often connected to the profit motive of large corporations who benefit from political inaction and thus from the appearance of scientific controversy (Proctor 1995, Oreskes and Conway 2010). The second examines the unintentional or normative production of ignorance (“undone science”), often connected to social movement pressure to change disciplinary norms and thus alter the way science is done or expand the objects of scientific inquiry (Epstein 2007, Fickel et al. 2010, Kleinman and Suryanarayanan 2013). The rediscovery of the 1% has much in common with the “undone science” tradition, as the failure to observe trends in top incomes resulted from the particular ways that economists’ regimes of perceptibility excluded top incomes from view rather than a strategic attempt to obfuscate wealth at the very top.

Unlike the majority of undone science cases, the rediscovery of the 1% did not require the acceptance of new methods or new data, but rather a return to older, simpler methods that had been put aside because newer tools became available (and those tools were better suited to economists’ theoretical interests). If economists had simply updated the estimates from NBER (1921), Kuznets (1953), and Radner and Hinrichs (1974), the growth of top incomes would have been observed in the mid-1980s without resorting to new methods or new kinds of data. Similarly, if the growth of top incomes had occurred 20 years earlier, it would have been picked up in the Department of Commerce’s estimates.³³

Finally, that the 1992 “Krugman calculation” debate did not result in sustained attention to top incomes highlights the importance of linking narrow observations to routine methods

³³An analysis of comparable figures from Radner and Hinrichs (1974) and Piketty and Saez (2003) shows how closely matched the two datasets are. For example, Radner and Hinrichs (1974) find that in 1964, the top .1% received 1.9% of all income; Piketty and Saez (2003) find that in 1964, the top .1% received 1.97% of all income. For 1971, Radner and Hinrichs (1974) find that the top 1% of consumer units received 7.2% of all incomes; Piketty and Saez find that the top 1% receive 7.79% of all incomes. This closeness is, of course, unsurprising, as Piketty and Saez and Radner and Hinrichs (1974) both rely on the same underlying tax data. Differences are in part accounted for by slight variations in the income concept, and the definition of the relevant units of analysis — the SCB figures, for example, incorporate some microdata to estimate family incomes rather than tax units.

of observation and broader theoretical frameworks. As an isolated calculation, Krugman's calculation was a political claim used to criticize the economic legacy of Ronald Reagan. Piketty and Saez instead framed their calculation in the long sweep of the past century, and published their findings in academic journals rather than feeding them into op-eds and stump speeches. They also continued calculating the top 1% share, as part of a comparative research program that has now produced data about trends in top incomes for more than 25 countries.³⁴ In short, Piketty and Saez built a new regime of perceptibility for top incomes that both analyzed data and interpreted it (at least enough to produce stylized facts and comparative evaluations of different countries), and thus provided sustained attention that made it impossible for top incomes to simply recede into the background as they did after the 1992 presidential election.

6.7 Conclusion

The facts of income inequality do not speak for themselves in the accumulated statistical frequency distributions. The facts must be recognized in the statistical constructs, and interpreted from them. (Mincer 1957 quoted in Teixeira 2007: 33)

In short, top income inequality has become one of the defining social problems of the 21st century. That top inequality was not one of the defining social problems of the 1980s and 1990s tells us something about the importance of economic experts in channeling public attention towards certain kinds of inequalities, and away from others. As Mincer (1957) noted, the facts of inequality — like all facts — do not speak for themselves, they must be recognized and interpreted. What Mincer did not highlight was precisely who gets to do that interpretation, who chooses which aspects of the statistics to highlight or ignore. Economists and government statistical bureaus jointly construct systems of observation that carefully track certain aspects of the economic system, while disattending to others.

³⁴<http://topincomes.g-mond.parisschoolofeconomics.eu/>, accessed September 8, 2014.

To understand why we care so much about top inequalities now, and why we seemed to care so little as recently as 20 years ago, we must understand both changes in the distribution itself *and* changes in the production of inequality knowledge.

What other facts about inequality are out there, waiting to be (re)discovered?

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CHAPTER 7

Conclusion

7.1 “You Can’t Feed a Family with GDP”

We’ve all heard the aphorism that “a rising tide lifts all boats.” If we think of national income — or GDP — as the economy’s tide, then this particular aphorism more or less held true in practice for decades.¹ For most of the post-WWII era, years of strong economic growth (when the economy’s size increased, the tide was up) were good across the board. Poor people did better, rich people did better. GDP was not quite a “sufficient statistic” for understanding economic performance, but it was a pretty reasonable approximation.² Since the mid-1990s (or perhaps a bit earlier), this story has changed dramatically. The link between measured economic growth and rising standards of living seems to have been severed. Take, for example, a recent *New York Times* (2014) story, “You Can’t Feed a Family with GDP.” The story documents the continued rise of the GDP per capita alongside the stagnation and decline of median household incomes in the late 1990s to present 2000s (as illustrated in Figure 7.1). As the NYT summarizes, “Around 1999, growth in the United States economy stopped translating to growth in middle-class incomes.”

The data here show another version of the “stylized facts” of income inequality laid out in the previous chapter. We know that the top 1% have received an increasing share of all

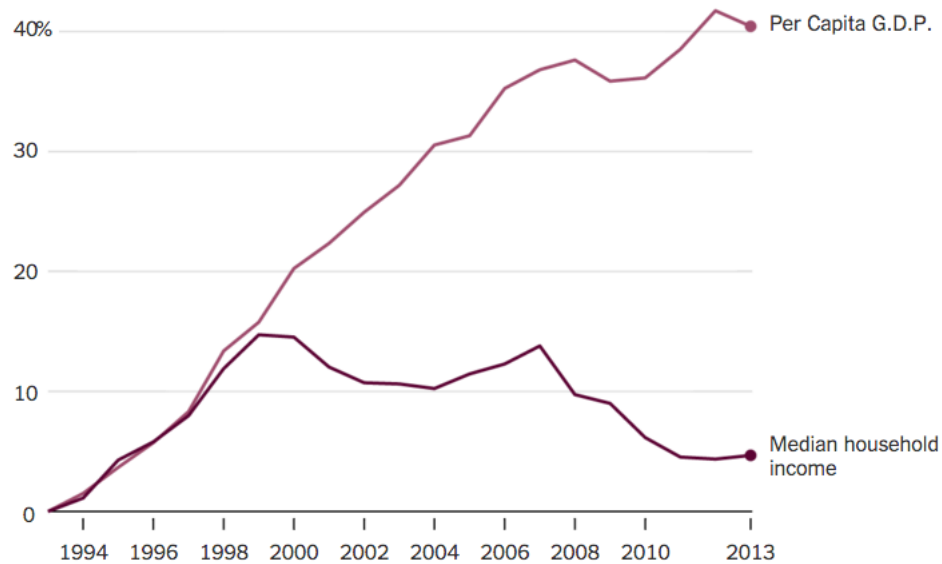
¹The use of this aphorism in connection with national prosperity is associated with President Kennedy, who often employed it in both his election campaign and presidency to describe how prosperity in one state would help lift the entire nation’s economy. See, for example, <http://www.presidency.ucsb.edu/ws/index.php?pid=60414&st=rising+tide&st1=>.

²In statistics, a “sufficient statistic” is a function of the data that contains all information necessary for inference about an unknown parameter.

Growth Hasn't Translated Into Gains in Middle-Class Income

Until around 1999, overall economic growth tended to correspond with growth in earnings for middle-income Americans. Since then, the two have diverged sharply.

Percent change indexed to 1993 level



Source: Census, Bureau of Economic Analysis

Figure 7.1: From New York Times (2014).

income, and we know that wages for typical workers have been flat or decreasing. What this framing highlights is how the takeoff of the 1% implies a rupture in the meaning of GDP. The American economy recovered well from the 2008-2009 financial crisis, but the American people did not. The rising tide now only lifts a few boats.

This narrative has become ubiquitous. As I was writing this conclusion, a colleague alerted me to yet another news story on the disconnect between the apparent performance of the economy and the lived economic experience of individuals: “My Economy: Hard work, risk and opportunity in Alabama.” (Marketplace 2015)³ The story opens:

The American economy looks pretty good on paper right now. We’re adding jobs — not as many as we’d like — but we’re adding them. The country’s gross domestic product is expanding — again, not as fast as we’d like, but it’s growing. Wall Street is doing pretty well. Corporate profits are up. The recession ended five and a half years ago.

But it sure doesn’t feel that way to a lot of the country.

This tension surrounding the inability of the GDP to capture even narrowly-conceived economic performance for the majority of people is somewhat different from older criticisms of the GDP’s exclusion or mistreatment of important areas of social life (for a review of criticisms of GDP, see Stiglitz et al 2010). As discussed in chapter 5, national income statistics have never adequately accounted for unpaid labor, mostly performed by women. The output of government is treated in an ad hoc fashion that may downplay the importance of public works and infrastructure by treating them as final consumption (Kuznets 1948). The environment is left out, leading to well-known paradoxes such as spending on pollution reduction leading to increases in measured economic growth (Nordhaus and Tobin 1972). These criticisms generally take the form of accusing GDP of leaving something out, and in turn, of “the economy” being incomplete.⁴

³See <https://twitter.com/StephenMolldrem/status/662364161439956992> for the notifying tweet, November 5, 2015.

⁴Feminist geographers Cameron and Gibson-Graham (2003) argue that these criticisms call for “completing” the economy by including more and more within its boundaries, but in so doing fundamentally reinforce a narrow vision of economic production located in the for-profit enterprises and markets.

Rather than engaging in this variety of critique, this dissertation has been an exercise in historical ontology (Hacking 2002) in the service of understanding the present space of economic debate. How did we come to have an object called “the economy”? Why do certain statistics play such a central role in defining it? How might things have been otherwise?

The five substantive chapters of this dissertation begin to answer these questions. Along the way, they offer insights relevant to several contemporary debates in sociology, political science and science studies, as well as the history of economics. In chapter 2, I drew on a large sample of economic discourse to adjudicate between two competing positions in the historiography of macroeconomics. I showed how both camps had captured part of the story. By the end of the 19th century, political economy had emerged as a well-developed field of thought, and “the economic” had formed as a sphere of life complementary to politics and nature. In the mid-20th century, “the economy” as technical object forms on top of this economic sphere. The economy is distinct from economic life (and from politics or nature) in that it has a well-defined, routinely measured *size*, meaning that its growth (or decline) can be (seemingly) objectively measured and controlled. Chapter 2 thus established the timing of the emergence of the economy and of economic life, and helped to make sense of the competing narratives of historians of economic thought who have variously emphasized either the late 18th or mid-20th century as the moment of the economy’s emergence.

In chapter 3, I engaged in the messy work of tracing the assemblage that formed the economy as a technical object. I traced some of the conditions that made possible the emergence of timely aggregate economic statistics. This historical exercise helped make sense of why, for example, the Depression of 1893 or early 19th century financial panics did not lead to the same outburst of novel statistical practices, fields of economic thoughts, and modes of economic governance. I then trace the history of how national income statistics in particular emerged as the key aggregate for defining and bounding the economy

in the 1930s and 1940s. The Great Depression amplified existing interest in measuring economic outcomes, and led to the institutionalization of official national income accounting. National income proved its worth in World War II, as By the end of World War II, the modern macroeconomic regime of perceptibility was largely complete, with quarterly measurements of GNP, along with monthly measurements of unemployment and inflation. This regime made visible (and thus potentially governable) small changes in what came to be understood as the size of the economy, changes that would simply have been invisible in earlier eras.

Chapter 4 examines how the availability of timely aggregate economic statistics reshaped the capacities and obligations of the state. Government had long been understood as playing a central, constitutive role in economic life. Populists and progressives debated a wide range of economic interventions in the late 19th and early 20th century, and belittled the old dogmas of *laissez-faire*. These interventions focused on the structures of economic life — the institutions that enabled markets and firms to function, including property and contract law, and anti-trust enforcement. Economic debates in this period did not yet envision a “data dependent” role for the state, where the state’s proper course of action would be dictated by the current conditions of “the economy” or something similar. In the 1920s and 1930s, planners of various political affiliations began to propose and enact more timely forms of economic governance, with proposals like countercyclical public works to combat unemployment, and open-market operations by the Federal Reserve to control financial markets. In the 1940s and 1950s, fueled by the newly visible economy, politicians began to embrace a macroeconomic vision of economic governance. “Managing the economy” supplemented older visions of the state’s role, and politicians began to promise reforms couched in terms of precise targets for unemployment, and especially economic growth. Politicians were simultaneously increasingly responsible for delivering on these promises, independent of the capacity of their proposed interventions to actually achieve their precise macroeconomic objectives. By the 1970s, politicians increasingly sought ways to offload

their newfound responsibilities back onto the market, thus setting up some of the dynamics explored in recent work on neoliberalism and the turn of finance.

The following two chapters turned away from what was made possible by the formation of the economy to what was left out, or made more difficult. Chapter 5 showed why unpaid housework continually floats on the edge of inclusion in national income (and thus in the economy), never quite yielding to the national income accountants' demands for unequivocal, objective, market-based valuations. Chapter 6 looks at the treatment of the distribution of income in both the macroeconomic regime of perceptibility, and in the parallel regime associated with labor economics. Macroeconomists largely ignored the distribution of income, and efforts to include measures of distribution in the national accounts were ignored and defunded. Labor economists, in contrast, examined the distribution of earnings, but did not care about wider measures of incomes, and were content to leave unexamined the extreme tails of the distribution in exchange for richer information about the sources of variation in the middle. Together, these two regimes of perceptibility produced compelling and influential stylized facts summarizing our understanding of the dynamics of aggregate income and sources of variation in earnings, but failed to observe changes in top incomes. Only when public finance economists began to construct a new regime of perceptibility using tax data to specifically examine top incomes did the stylized fact of the rise of the top 1% receive sustained attention, and thus become a matter of both academic and political concern.

7.2 Multiplying Economies

Having shown how we got here, I now briefly turn to where else we might have gone. Mol (2002) sensitized us to the importance of practices in enacting our technical objects. National income statistics enacted the economy. It follows, then, that understanding how else we might have measured national income — or what we might have measured instead of

national income — should give us some insight into alternative shapes the economy could have taken. These alternatives veer from “plausible worlds” (Hawthorn 1993) — events that almost happened, debates that could have gone otherwise — towards increasingly distant possibilities. I start by asking, what if we measured GDP a bit differently? What if the economy had a slightly different size? I then proceed to ask, what if the economy had no size? What if we measured economic life in some entirely different way? Next, I ask, what if “the economy” never came into being as a sociotechnical object, but rather remained a fuzzy conceptual space more akin to politics or nature? And last, I ask what if the economic had never become conceptually separated from the political or the natural? These accounts — especially the final two — are highly speculative. At a minimum, they should dramatize the transformations in economic discourse that have taken place over the past hundred years, and how difficult it is to imagine a world without well-defined, bounded, measured economies.

7.2.1 A Different GDP

The first set of counterfactuals are the most plausible, and the most informed by the historical record. Although GDP has settled into a relatively stable form over the past century, there have been many moments of contention about the particular choices made in its construction, and especially in the 1920s-1940s, there were many alternatives seriously considered. For example, it is relatively easy to imagine a world in which the advocates for the inclusion of unpaid housework were a bit more compelling in their arguments, and a bit less tentative in their worries about the underlying data. Similarly we can imagine a world in which the politics of inequality in the 1950s-1960s were slightly different, enabling the Department of Commerce to successfully acquire funding to better incorporate income distribution into the National Income and Product Accounts framework. How important would these changes — or similar alterations — have been? What would have changed (and what might change in the near future) if we measured GDP a bit differently?

My contention is that these changes would have had important effects on particular economic debates, but would not have changed the overall shape of economic governance or our broadest understandings of “the economy” and its importance. Take the example of housework. Suppose unpaid housework had been added to the official national income statistics in the 1930s and the official UN standards of the 1950s. How would that change have affected later New Deal planning efforts, the mobilization for World War II, or the post-war politics of growth? The increased attention to housework might have led to a sharper consideration of the tradeoffs made by women on the home front and a quicker understanding of the idea that one of the reasons the US economy was so productive during World War II is that women increased their paid work more than expected, in part due to the dramatic decrease in the birth rate. The need to measure unpaid household labor to produce accurate estimates of its value might have spurred earlier measurement of time use and perhaps an earlier, stronger debate around the problem of the “second shift” (Hochschild 1989) and issues of the productivity of unpaid labor (Folbre and Wagman 1993). These are important changes, and show the influence of economic experts and regimes of perceptibility on how we understand and act on the economic world.

And yet the basic structure of the national income statistics, and the economy it defined, would have remained the same. The basis for valuation would still be market prices, the boundaries of the statistics would still be nation states, and so on. The politics of growth could have proceeded apace, with a few more footnotes about the difficulty of measuring variations in unpaid work. The economy would still have had a size, knowable and governable in close to real time. “Completing the economy” would not fundamentally disrupt it (Cameron and Gibson-Graham 2003). We would likely still think and talk much as we do now.

7.2.2 The Economy, But Not As We Know It

Another way to dramatize the importance of the rise of the macroeconomic regime of perceptibility centered around national income statistics is to suppose instead that an alternate measurement system came to dominate our understanding of aggregate economic affairs. We can see the possibility for such alternatives in several places: in the Soviet Union's Material Product System (a variant of national accounting derived from a particularly restrictive reading of Marx's labor theory of value, see Herrera 2010), in Germany's abandonment of national income statistics during World War II in favor of more disaggregated measures (Tooze 2001), and the long tradition of research on input/output economics most prominently associated with Wassily Leontief (Dorfman 1973). In 1941, Leontief published an influential book on input/output economics titled *The Structure of the American Economy* — the same title as the 1939 NRC report, but with a vision linked to a very different statistical system than the national income-based discussions that eventually won out. All of these systems still relied on large scale quantification efforts, and produced detailed systems of accounts capable of organizing data about many aspects of the economic system.

These different statistical frameworks connect to different understandings of economic planning. For example, Leontief's version of the economy focused not on the total size of the system, but on the pattern of flows between sectors. Input/output economics are much more compatible with detailed, industry-level planning of the sort attempted in the mid-1930s, but abandoned in favor of Keynesian-inflected macroeconomic management after facing increasing resistance from business interests and the courts. Stapleford (2011) documents how National Resources Committee moved away from Gardiner Means' vision of "multi-industry planning" (Lee 1990) in 1937-1939 in part due to data limitations — Means could not generate the detailed statistics needed for more fine-grained control of industries. If Means had been a bit more successful at producing the data needed for his analysis, or if Leontief's empirical work had been completed a bit earlier, we could imagine "the economy" defined not in terms of its size but in terms of its *Patterns of Resource*

Use (as Means' 1938 book was titled). Such an economy might have foregrounded discussions of "balance" over discussions of "growth." Indeed, it's hard to imagine talking about growth without some singular measure of size. Without a single aggregate measure of economic performance, economies would be much more difficult to compare, and the political debates around economic performance would have had to change as well.

Such a system would have had to win over many skeptical converts — all of the forces that arrayed to fight industry-level planning — but if it had, the resulting economy would have looked substantially different. Perhaps the disconnect between industry-level flows and quality of life would have been large enough that another quality of life measure would have surfaced and become dominant, more cleanly separating economic performance and overall well-being. It's much harder to imagine what our economic debates would look like without recourse to a single measure of an economy's size.

7.2.3 Long Live Economic Life!

Even more alien is a counterfactual in which economic life never becomes so quantified. Or, put differently, where the various efforts to quantify bits of economic life are never amalgamated into a unified statistical framework. Nature and politics offer instructive parallels. The efforts to quantify nature in its various forms — from biodiversity to global climate — have been truly vast (Edwards 2010). Similarly, although perhaps not quite so strikingly, political scientists have quantified everything from partisan position to the correlates of war. And yet neither of these efforts has quite so transformed "nature" or "politics" into a technical object on its own.

Put this way, it's not so difficult to imagine a persistent discourse of economic life that made little or no recourse to unifying statistics. Macroeconomics would be very different or absent entirely. Economic life would still be quantified, measured, and manipulated, much as nature and politics are, but its boundaries would remain fuzzy.

7.2.4 Embeddedness Forever

Finally, we imagine a counterfactual in which the economic never discursively separates from the political or the natural. This counterfactual requires traveling back long before the 19th century (where my analysis begins), but again, I think it is worth spending a moment to ponder. As Polanyi (1944) long ago noted, the idea that the economy is separated (“disembedded”) from politics and nature has had very destructive consequences. It produced the fiction that land, labor, and money could be treated as commodities independent of their realities as nature itself, human beings, and a political convention. I do not know what a world without a separate economic sphere would look like, one in which the basic “stuff” of social life consisted of some ontology other than our own. In many ways, this possibility is much more foreign than any of those discussed above. We can imagine, with some difficulty, a world where the economy remains indistinct and conceptual, less like an object and more like a domain of life. But what would a truly embedded society look like, one that never convinced itself that economic things were different from political things? I leave descriptions of such a counterfactual as an exercise for the reader.

7.3 Limitations

Every research project has its blind spots and an unwieldy historical project whose shape twisted and turned over time has more than most. Here I briefly discuss three clear areas of concerns and opportunities for future research.

First, the dissertation has focused primarily on the United States, and to a lesser extent the United Kingdom. Developments in the rest of the world enter rarely. Two partial justifications for this focus are the dominance of Anglo-American economics in the global field (Fourcade 2006) and the influence of Anglo-American statistical conventions on the United Nations System of National Accounts, and thus the national accounts of the rest of the world. Despite these justifications, my analysis is clearly limited by an insufficient

exploration of alternative conceptions of economic life present in the 1900s-1930s in non-English speaking countries, and by the similar absence of discussion of the spread and implementation of the US-UK vision. Some of that work has already been completed by others, to whom I am indebted (e.g. Tooze 2001 on Germany 1900-1945, O’Byrne 2009 on Japan post-WWII). Much more remains to be done.

Second, much of my research examined the history of economics per se. Alternative discourses produced outside of the discipline received less attention. Again, this narrowness limits the space of possibilities investigated. For example, future research might explore the Technocracy movement of the 1930s, which envisioned a statistical system built entirely on *energy* rather than money as the unit of analysis. This and other movements proposed radically different objects of management than the market-based economy. Understanding their proposals, and how they failed, would enhance the story told here.

Finally, my analysis has been somewhat circumstantial in some of its causal claims. The overall argument is a *formation story* (Hirschman and Reed 2014), a narrative account of the coming into being of a new kind of thing and the consequences resulting from its emergence. The chapters of this dissertation vary in their approach, focusing on establishing the phenomena (especially chapter 2), unpacking the sequence of its events leading to the particular version of the economy that came to dominate (chapter 3), looking at the consequences of that emergence for political accountability (chapter 4), and examining two paths not taken (chapter 5 and 6). At times I offer only suggestive evidence for important claims, other times I make arguments about conditions for possibility rather than tight causal linkages. Grafting together such a miscellany produced an account that would likely not be described as “clean.” I hope that despite this messiness, the reader comes away convinced of the broad outlines of the story, or brimming with ideas for how to disprove it and in so doing tighten the analysis.

7.4 Learning to Love the GDP

In my time studying the history of national income statistics and the economy, I admit to have grown rather fond of the GDP (and the broader system of accounts institutionalized in mid-20th century). I empathize with the emotional tenor of Diane Coyle's (2014) recent book, *GDP: A Brief but Affectionate History*. The vast bulk of work written on GDP in recent years has been critical. We are keenly aware of what "the economy" leaves out. My initial forays into its history were motivated by these critical concerns. It was only in studying the history of what GDP accomplished, what "the economy" made visible and possible, that I began to appreciate it on its own terms. A bit less abstractly, I have come to appreciate the political power of (seemingly) unequivocal numbers. Politicians really are forced to account for economic performance, as imperfectly measured as it is. Try to imagine a world *without* the economy, as I did above, and it becomes easier to see how much we've learned within the current framework.

The economy is not a perfect invention and GDP is not a perfect measure. They reflect the time and circumstances of their creation, and the systematic biases of their inventors. Nonetheless, we can and should respect their accomplishments while simultaneously working towards something better.

7.5 References

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