

Rediscovering the 1%: Economic Expertise and Inequality Knowledge*

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Abstract

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.

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 ever received, equaling their peak just before 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 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 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 experts monitored trends in

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.

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 attention to the influence of economists' technical tools and styles of reasoning in shaping policy

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.

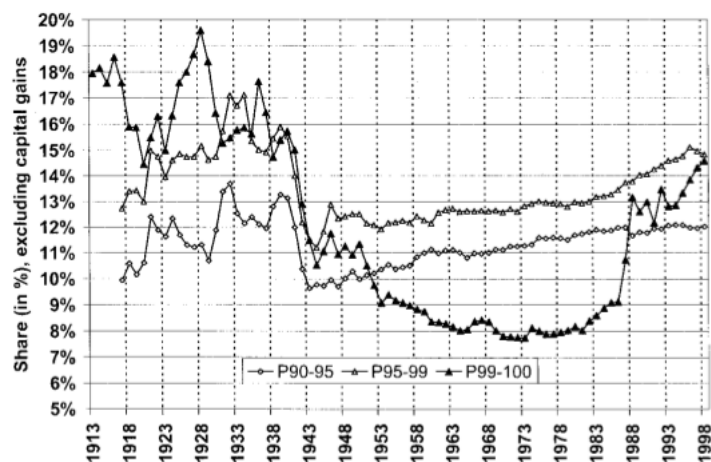


FIGURE II
The Income Shares of P90-95, P95-99, and P99-100, 1913-1998
Source: Table II, columns P90-95, P95-99, and P99-100.

Figure 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

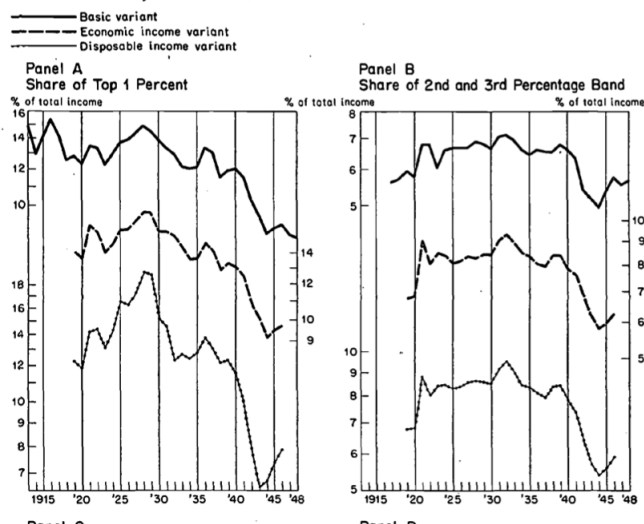


Figure 2: Kuznets (1953:33) less famous chart showing the downward trend of top incomes in the 1930s-1940s.

(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.

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

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.

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 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

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.

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 dosime-

ters 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 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.

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.

⁶I expand on this argument in Chapter 4.

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 (Keynesian 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.

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 3 for an example of their findings).⁷ In the 1930s, work on national income was transferred from NBER to the Department of Commerce, and distributional issues would take a backseat 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’

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

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 has 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.

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.

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 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

⁹For an extended discussion of the history of macroeconomics and what came before, see Chapter 1.

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 Senate, 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

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.

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

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 macroeconomics 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

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.”

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 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

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.

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.

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 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

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.

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 simultaneously 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 productivity. 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”:

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.

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 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 “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 labors (firms) to the study 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 big advantage of including all the covariates a researcher could want, especially the a range of 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.

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.

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

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 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 *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 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

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

¹⁹*New York Times*, 1953, October 12.

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

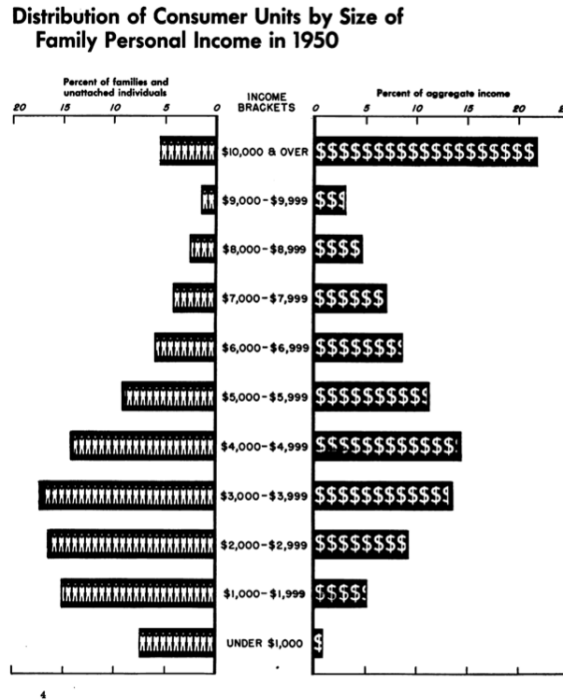


Figure 4: A graphical representation of the distribution of pre-tax income in the United States in 1950 (BEA 1953: 4).

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

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.

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 reaction (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 relatively incapable of tracking movements at the top of the income distribution. In terms of data, economic growth and income inequality were delinked — no dataset tracked both in comparable ways. This

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.

delinking came just as a thirty-year span of decreasing inequality would itself come to an end. As researchers began to notice the increase in inequality, they would frame their explanations in terms of productivity, and sociodemographic analysis, but would largely miss the dramatic growth of top incomes.

5 The Stylized Facts of Inequality

Having established two disjoint regimes of perceptibility for inequality, and having abandoned attempts to integrate the data underlying those regimes, we are now better positioned to understand how economists knew what economists 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 increases 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

²³Similarly, work by sociologists interested in inequality would focus on persistent wage gaps. Leicht

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 researchers began to notice the rise in 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.

(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.

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 proceeded, 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.

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 estimates. 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

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

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 1990s (see Figure 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.

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 dominators (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

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.

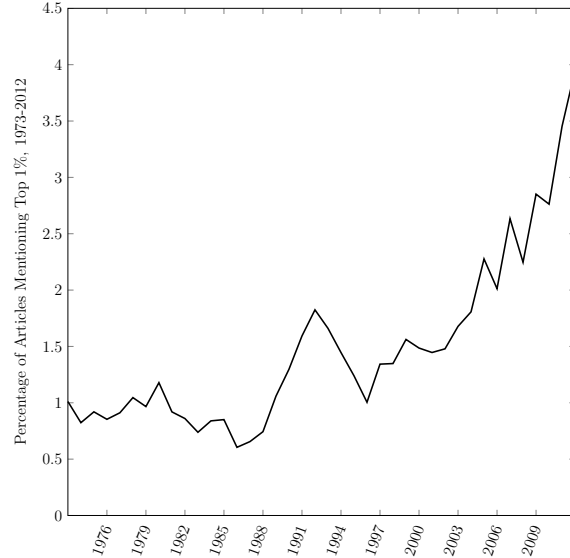


Figure 5: Three-year moving average of the percentage of all research articles in JStors *Data For Researchers* database that mention “income inequality” or “income distribution” and also “top 1%”, “top one percent” or “top 1 percent” from 1973–2012. The data are noisy — not every reference to the “top 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.

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

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.

the 97th percentile). Specifically, Katz and Autor (1999: 1468) cited growing evidence on the tremendous growth of CEO pay and athletes (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 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:

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*.”

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.

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 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-

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)

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 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

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.

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 Discussion

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 knowledge 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

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

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).
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. 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,

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

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 7). Narrow observations may inform policy responses, as when early estimates of the decline in GDP in 2008 caused by the financial crisis underestimated the extent of the recession, and arguably led to a smaller than necessary stimulus (Grunwald 2012). 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 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).



Figure 6: Stylized facts mediate the link between observations and theories.

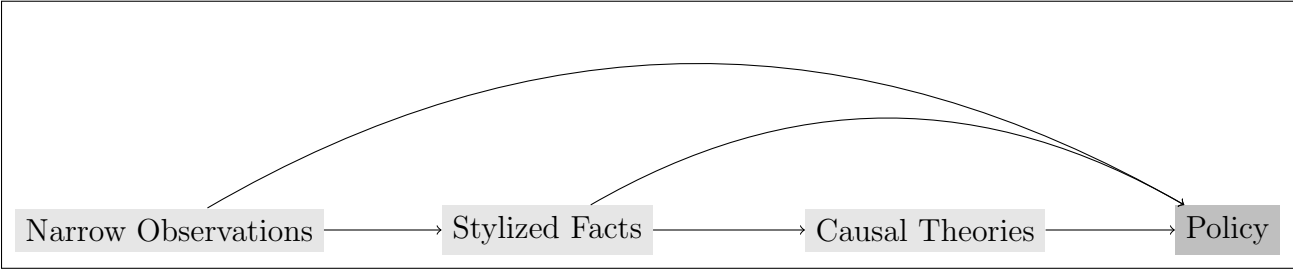


Figure 7: A more complex model highlighting the direct influence of observations and stylized facts on policy.

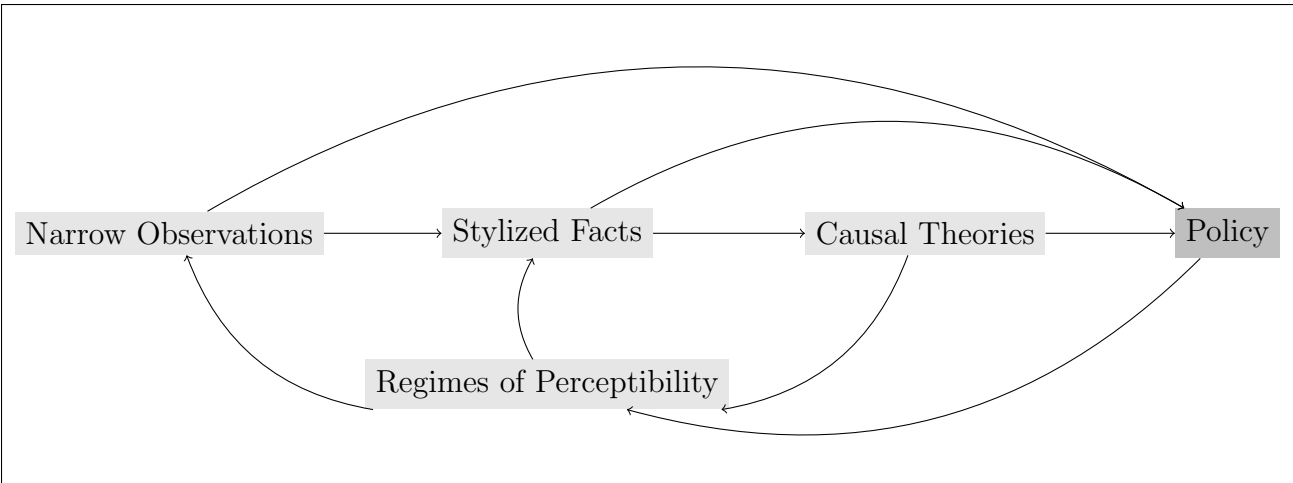


Figure 8: This model highlights how causal theories and policies together produce regimes of perceptibility, including institutionalized policy devices for seeing, that influence which observations are made and how those observations are combined into stylized facts.

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.

6.2 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 highlighting 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 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.

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

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.

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

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. 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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