Law and Inequality

Rebecca Johnson

Advised by: Professor Paul Starr

Contents

Course Overview 2

Week One: Defining Inequality, General Trends, and Alternate Lenses 5

Week Two: Explaining Growth in 50-10 and 90-50 Inequality—Change in Technology versus Changes in Wage-Setting Institutions? 11

Week Three: Explaining Growth in Top-End Inequality—Pay Practices and Marginal Taxation 19

Week Four: Beyond a Single Nation as the Unit of Analysis—Income Inequality Between and Within Nations 23

Week Five: The Everyday Texture of Income Stagnation: Precarious Employment and the Retrenchment of Policies to Buffer Precarity 29

Week Six: Overview of U.S. Trends in Intergenerational Mobility 36

Week Seven: “Pre-Institutional” Inputs to Mobility: Genetics, Early Language Exposure, Family Structure 40

Week Eight: Educational Institutions as a Stalled Engine of Mobility 46

Week Nine: Labor Market Institutions as a Stalled Engine of Mobility 55

Week Ten: Inequality in Criminal Law 60

Week Eleven: Inequality in Civil Law—Attrition in the Dispute Resolution Pyramid 63

Week Twelve: Inequality in Civil Law—Goals for and Actors Behind Enforcement 66
Course Overview

The present course, a 12-week seminar for graduate students and upper-level undergraduates, aims to investigate three sets of inter-relationships between inequality and the law. The course is divided into thirds.

The first two thirds are focused on inequality and the law. In the first third of the course, we focus on law, institutions, and income inequality. This first third is centered around three stylized trends in U.S. income inequality that we review in Week One:

- **Week One** focuses on the first two segments: explanations that implicate a variety of laws and institutions; roles of union and other wage-setting institutions; legislatures and partisan politics; executive compensation committees. While Weeks One through Three focus on nations as the geographical unit for which inequality is estimated, Week Four adopts two alternate spatial lenses: first, how do the descriptions of and explanations for inequality change when we measure inequality on a global scale? Second, and returning to the U.S., how does the federalist structure of policymaking contribute to regional inequalities in income and other inputs to wellbeing? Finally, while the first four weeks largely focus on characterizing and explaining formal trends in income inequality, Week Five focuses on the experience of living in a highly unequal society. Chief among these experiences is the shift for some workers from a post-WWII Golden Era defined by the **standard employment relationship (SER)** to employment characterized by greater risk and social policies that have failed to keep pace with that shift in risk.

The second third of the course, also falling within the general topic of inequality and the law, focuses on law, institutions, and intergenerational mobility. A theme throughout is that in the U.S., ideas about the value of social mobility carry greater cultural resonance than ideas about the value of equality. As a result, many social policies aim towards mobility into different segments of the income distribution rather than a state of equality. Week Six characterizes trends in intergenerational mobility, illustrating that despite growth in inequality during the post-1975 era, intergenerational mobility has neither increased nor decreased. While one gloss on this stall is optimistic—intergenerational mobility has not followed the **Great Gatsby** curve phenomenon of increases in inequality causing decreases in mobility—another gloss is pessimistic: the same era has been marked by a variety of policies and institutional efforts to weaken the link between social origins/ascriptive social characteristics and income/educational attainment. Before turning to these institutional levers on mobility, Week Seven focuses on inputs to mobility that often lie beyond the purview of formal institutions: genetics, linguistic enrichment, and family structure. Turning to institutional levers, Week Eight focuses on attempts to reform elementary/secondary and higher education to better promote mobility into various segments of the income distribution and why mobility may continue to stall despite these efforts. Week Nine focuses on similar dynamics in labor markets. Throughout the two weeks, we see a bifurcation in policies based on which segment of the income distribution they aim to launch individuals into—a better position in the income distribution...
that might still fall in the middle ranges versus the *elite* segment of top 1 and top 0.1% that holds a growing share of resources.

The final third of the course shifts focus from inequality *and* the law to inequality *in* the law, with [Week Ten](#) focusing on inequalities in criminal law and [Weeks Eleven](#) and [Twelve](#) focused on civil law.

Each week contains between 150 and 250 pages of reading that should be read prior to class in the order listed, as well as “Questions to think about” prior to class to inform discussion. Note that in the *pdf* version of the syllabus, to illustrate interconnections between each week, clicking on the **bolded name** of a Week takes you directly to that week’s contents—e.g., clicking on [Week Five](#) should take you to that week’s readings.

### Learning Objectives

By the end of the course, students should understand:

1. How to critically evaluate the analytic decisions that go into *measuring* inequality and mobility, and identify how a researcher’s focus on *explaining* inequality and mobility through a certain lens drives these analytic choices

2. How different types of forces—technological change; institutional change; changes in social norms and moral frameworks—interact to affect inequality and mobility

3. How the effect of a given policy on inequality/mobility depends on various factors: the policy’s formal text; on-the-ground implementation and adjustments; monitoring and enforcement
Section One: Law, Institutions, and Income Inequality
Week One: Defining Inequality, General Trends, and Alternate Lenses


The above readings for this first week fall into three topics.

**Topic one: how to define income inequality (**CBO, 2016)**

When defining inequality, we can distinguish between the following, with different definitions having different implications for trends over time in levels of inequality (Congressional Budget Office (CBO), 2016):

1. **Market income**: this consists of labor income, business income, and capital gains (profits that derive from the sale of assets), as well as income received in retirement from past services (CBO 2016, “Notes and Definitions”)

2. **Pre-tax or before-tax income**: this consists of market income plus government transfers in the form of cash payments (e.g., SSI; TANF; Social Security; unemployment insurance; and other transfer programs)[1]

3. **Post-tax income**: this consists of pre-tax income minus federal taxes.

The readings that follow generally focus on either market income or pre-tax income. And important to keep in mind when focusing on what type of income the author focuses on when analyzing trends in inequality is that authors may choose to focus on a particular definition of “income” in order to highlight a particular set of contributors to trends over

---

[1] The report also includes some non-cash transfer programs like Medicare where the value of benefits is translated/imputed to cash. In particular, the CBO estimates the average cost to the government providing benefits such as Medicare and uses these average values (CBO, 2016, p. 5, footnote 8)
time. For instance, Lemieux (2008) focuses on trends in inequality in market income. The reason for this focus is that the review’s purpose is to adjudicate between debates about different labor market contributors to these trends. Did interactions between technological change and workforce skill composition leading to greater returns for skilled relative to unskilled labor drive the trends? Or were changes to wage-setting institutions—for instance, declines in unionization or increased use of pay-for-performance incentives in top-tier occupations—more responsible? Week One and Week Two’s readings discuss these competing explanations in greater detail, but important to keep in mind is that researchers’ and policymakers’ choices about how to define income for the purposes of measuring inequality lead themselves to examining different contributors: changes to wages and other labor-market institutions; changes to transfer programs; changes to the rate at which income is taxed.

Questions to think about on this topic:

1. Which features of the U.S. tax system mean that the distribution of post-tax income will generally be more equal than the distribution of pre-tax income?

2. When defining government transfers for the purposes of calculating pre-tax income, the CBO and other entities include payments from “social insurance and other government assistance” programs (CBO, 2016, p. 3). This means that certain forms of transfers are included—for instance, Social Security payments—while other forms of transfers are not included—for instance, food stamps (Supplemental Nutrition Assistance Program) and Medicaid. How might what’s included in versus excluded from this definition of pre-tax income affect our estimates of how policy changes to tax and transfer programs have changed the landscape of U.S. inequality?

3. Some measures of pre-tax income rely on individuals to report the amount of transfer payments they receive (CBO, 2016, p. 5). How might social processes such as the stigmatization of welfare recipients (discussed in later weeks) affect this self-reporting of transfer income receipt? How would under-reporting of transfer income receipt affect estimates of inequality in pre-tax incomes?

Topic two: trends in U.S. income inequality since the mid-1970’s (Lemieux, 2008; McCall and Percheski, 2010)

Focusing on market income (Lemieux, 2008) and a market income and pre-tax income depending on the section (McCall and Percheski, 2010), the two readings agree on the same set of three stylized facts about trends in U.S. income inequality since the mid-1970’s. First is that income inequality has increased since the 1980’s, with some trends beginning in the late 1970’s (McCall and Percheski, p. 4; Lemieux, 2008). This overall trend masks the fact that different periods within this broader time range are marked by growth in inequality within different portions of the income distribution. In particular, the 1980’s were marked by growing lower-tail or bottom-end inequality, defined as inequality between those in the 50th income percentile and those in the 10th income percentile. These 50-10 gaps stopped growing in the late 1980’s, and growth in inequality since then is marked by growth in top end inequality: inequality between those in the 90th income percentile and those in the 50th percentile, with further differentiation at the very top of the income distribution.
While the two readings agree on these three stylized facts about trends in U.S. income inequality, each also focuses on nuances in these three general trends. These nuances will come into play in later weeks as we shift from describing trends to explaining sources of these trends.

**Lemieux (2008)** points to an important aspect of trends in income inequality that are difficult to square with technological explanations for these changes that we will review in Week Two. First, many advanced industrialized countries with similar workforce skill compositions faced the same technological changes as the U.S., and would presumably face the same effects of that change on higher relative demand for skilled workers (e.g., France, Japan, and Germany). Yet inequality has grown more rapidly in the U.S., and to some extent, the U.K., than in these other advanced industrialized contexts. The puzzle then becomes: why have advanced economies exposed to the same technological forces shown different patterns in income inequality? This puzzle will motivate the shift from technology-based explanations for this trend to institutions-based explanations for this trend in **Week Two** and **Week Three’s** readings.

Meanwhile, **McCall and Percheski (2010)** point to a different nuance in how inequality: the unit of analysis for measuring market or pre-tax income (p. 332). In particular, we can distinguish between different units of analysis:

1. Individual incomes for male or female full-time, year-round workers
2. Individual incomes for male or female working-age adults\(^2\)
3. Total household income (adjusted or unadjusted for household size)
4. Total family income (adjusted or unadjusted for household size)

The authors find interesting divergences in trends based on the unit of analysis one uses. For instance, there is greater inequality in total household income than inequality in total family income, in part due to higher rates of marriage and entrance into family units among more highly educated individuals. Their analysis suggests that in addition to variation in what counts as income contributing to variation in the measurement and tracking of inequality, variation in who/what is analyzed as the holder of income also matters.

**Questions to think about on this topic:**

1. Both sets of authors distinguish between income inequality in different segments of the income distribution: the 50-10 ratio; the 90-50 ratio; further differentiation within the top end of the 90th percentile. Which of these areas of the income distribution is the bulk of social stratification research focused on? If there is a lack of attention to certain areas of the income distribution in current social stratification research, what methodological challenges might contribute to this?

2. Which unit of analysis—workers; working-age individuals; households; families—do you think is the best lens for examining trends in inequality and why?

3. Which data sources do the authors and other researchers use to examine trends in inequality? What are some strengths and shortcomings of different data sources?

\(^2\)So including individuals who are working-age but do not report formal employment.
4. Later weeks will focus on the “institutional” explanations for increases in inequality that can respond to Lemieux’s challenge of how to account for different trends in income inequality in advanced industrialized democracies where workers with similar skill composition face the same technological pressures. Before we get to these readings, what lay explanations for trends in inequality do you see in political rhetoric and the media? Do these lay explanations focus more on technological forces or on political/institutional contributors?

**Topic three: two alternate lenses on inequality—wealth and volatility** (*Saez, 2017; Western et al., 2012; Hannagan and Morduch, 2015*)

The readings in Topic One and Topic Two make two important choices when analyzing income inequality. First, the readings generally examine *income* rather than wealth. Second, the readings examine *levels* of income an individual or household possesses rather than within-individual (or within-household) volatility in that income. While the remainder of the course will generally remain with a focus on 1) *levels* and 2) *income* (rather than *volatility* and *wealth*), the readings in the present section point to other directions the course could have taken.

**Wealth versus income** (*Saez, 2017*)

Focusing on trends in *wealth* inequality as apart from trends in *income* inequality, *Saez (2017)* first distinguishes between income—a flow measured often on an annual basis, with “streams” in the flow coming both from labor income and capital income (returns on wealth)—and wealth—a stock of economic resources accumulated from savings of income and inheritances (p. 8). Defined in this way, U.S. wealth inequality is significantly higher than U.S. income inequality because, for the most part, the bottom 50% of families in the income distribution have zero wealth (and thus zero capital income). And just as one can investigate the *share of income* held by different percentiles of the income distribution, one can investigate the *share of wealth* held by different percentiles. This investigation shows a declining percentage of total wealth held by those in the bottom 90% of the wealth distribution and an increasing percentage of wealth held by the top 10% and further top-end percentiles. For instance, the top 0.1% of families hold more than 20% of household wealth.

Though Saez’ focus on wealth inequality yields similar *substantive* conclusions as a focus on income inequality—namely, a growing share of the resource held by top percentiles—inequality in wealth has different contributors to its persistence. It therefore requires different legal and policy levers to mitigate. One driver of a declining share of wealth held by the bottom 90% of the wealth distribution is growing debt related to home ownership and consumer debts. These debts leave many members of the bottom 90% with essentially zero in savings. While savings are partly a function of an individual’s *level of income*—it is easier for those with high incomes to save a portion of that income than those with lower incomes—policies meant to reduce debt burdens and increase wealth accumulation in the form of savings among those outside the top 10% of the income distribution can play a role in reducing wealth inequalities holding income levels constant.
Volatility (Western et al., 2012; Hannagan and Morduch, 2015)

The processes that Saez describes mean that many individuals across the income distribution have very little savings. The lack of savings to serve as a buffer for shocks to income leads us to focus on another alternate lens with which to view income inequality: inequality in the degree to which that income is stable versus fluctuates in ways that create financial challenges and reduce wellbeing. Hannagan and Morduch (2015), who frame this instability in incomes as volatility, define volatility as within-year fluctuations in income, and operationalize it as the coefficient of variation (CV) of monthly income, or the following where $i = \text{month } i$ from 1, 2, … 12:

$$\frac{\sigma}{\mu} = \frac{s}{\bar{x}} = \sqrt{\frac{1}{12} \sum_{i=1}^{12} (x_i - \bar{x})^2}$$

A higher coefficient of variation implies that the level of income in certain months deviates more from the mean income across the year. Using granular financial diaries that collect income and expenditures every 2-4 weeks, the authors find significantly higher income volatility among households below the poverty line, and note that this includes both upward volatility (fluctuating income boosts, or months where $x_i > \bar{x}$) and downward volatility (income shocks/losses, or months where $x_i < \bar{x}$). The authors argue that inequality in volatility—top incomes have less volatility; lower incomes have more volatility—is an important alternate lens through which to view inequality in levels of income. Volatility, the authors argue, is distinct since increased volatility can lead to mismatches between spikes in spending (e.g., when certain bills are due) and spikes in income that can cause higher degrees of stress and lower levels of subjective wellbeing among households experiencing these mismatches.

While Hannagan and Morduch largely focus on the impacts of income volatility for individual-level wellbeing and insecurity, Western et al. (2012) broaden the lens to analyze the impacts of income volatility for broader patterns of social stratification. Before doing so, they argue that it is important to distinguish between income volatility and income insecurity using two dimensions:

1. **Dimension one**: direction of income change (loss or gain)

2. **Dimension two**: how predictable was the income gain (less predictable v. more predictable)

Income volatility includes both losses and gains, and does not distinguish between when these gains and losses are expected/predicted—for instance, through a planned withdrawal from the labor force when one’s spouse returns to work—or unexpected/not predicted—for instance, through a sudden layoff or the quick onset of a disability. Income insecurity is thus a narrower concept that focuses on income losses that are unexpected/not predicted.

Operating with this narrower concept, Western and co-authors focus on a range of unexpected adverse events that contribute to unexpected income losses: labor market insecurity (which they argue is well-studied), family instability such as the risk of poverty for women following union dissolution (which they argue is less well-studied by those concerned with volatility/insecurity), and the sudden onset of severe health problems. Importantly, and foreshadowing work we will read in [Week Five] by Hacker, Kalleberg, and Weil, they also highlight the weakening of various institutional mechanisms that once better insulated individuals facing these unpredictable shocks.
Questions to think about on this topic:

1. One barrier to policy interventions meant to alleviate inequality is the perceived justness or legitimacy of the distribution, or the extent to which individuals in the distribution are thought to deserve to have the level of resources that they possess. What implications might this have for public opinion about the importance of reducing inequalities in income versus the importance of reducing inequalities in wealth?

2. What is the relationship between wealth (the focus of the Saez reading) and volatility in incomes (the focus of Hannagan/Morduch and Western et al.)? In particular, how does wealth provide a buffer against the type of spending-savings mismatches that Murdoch documents as an undesirable consequence of income volatility?

3. Which of the main contributors to increased income insecurity that Western et al. outline—labor market insecurity; family instability/union dissolution; health—do you think is most amenable to policy intervention? What might serve as an example policy in each domain that would reduce the impact that a shock in that domain has on month-to-month or year-to-year levels of income?

4. Hannagan and Morduch very explicitly choose to focus on inequality in month-to-month fluctuations in income, while Western et al. focus on inequality in either month-to-month fluctuations or year-to-year fluctuations in income. What are the different implications for individuals/households of each type of fluctuation? What are examples of policies that might focus more on month-to-month fluctuations; policies that might focus more on year-to-year fluctuations?
Week Two: Explaining Growth in 50-10 and 90-50 Inequality....Change in Technology versus Changes in Wage-Setting Institutions?


Brynjolfsson, Erik, and Andrew McAfee. The second machine age: Work, progress, and prosperity in a time of brilliant technologies. WW Norton & Company, 2014. Focus on:
• Chapter 9- 'The Spread'


• Chapter 1- 'The New Gilded Age'
• Chapter 2- 'The Partisan Political Economy'


Topic one: Skill-biased technological change and job polarization (*Autor, Katz, and Kearney, 2008; Brynjolfsson and McAfee, 2014*)

Lemieux (2008) from *Week One* argues that a “1990’s consensus” emerged within the economics profession to explain the first two stylized trends in U.S. income inequality that we reviewed in that week: growth in overall inequality and growth in lower-tail inequality. In this consensus, researchers argued that the 1970’s saw increased demand for skilled labor but that there was a sufficient supply of skilled labor to keep pace with this demand. Since the 1980’s, demand for skilled labor has continued to grow but educational attainment has failed to keep pace, leading to a growing wage premium for skilled workers.

This theory, focused on the interaction between demand for skill and workforce educational composition, prompts the question: during this period, what caused the increase in relative demand for skilled labor? An initial theory—skill-biased technological
change (SBTC)—argues that advances in computing technology led to increased demand for skilled labor relative to unskilled labor due to greater complementarities with this skilled labor. Thus, the period was marked not by skill-neutral technological change but technological change with effects on productivity and wages biased towards skilled workers.

SBTC provides a reasonable explanation for stylized trend one and two—overall growth in inequality and the contribution of both 50-10 and 90-50 gaps to this growth. But it largely fails to explain why, beginning in the 1990s, 50-10 gaps stagnated while 90-50 gaps continued to rise. Autor, Katz, and Kearney (2008), acknowledging this shortcoming of SBTC’s original formulation, propose the job polarization thesis as an updated version of SBTC.

In job polarization, jobs and their component tasks fall into three categories, and each interacts differently with computer capital, or technologies that provide a close substitute for human labor in routine cognitive and manual tasks (p. 8). First, computer capital complements jobs involving abstract reasoning and these jobs see increased productivity and higher wages. Second, computer capital substitutes for jobs that involve routine tasks and these jobs see falling or stagnating wages. Third, computer capital neither substitutes nor complements jobs involving non-routine manual tasks such as truck driving, waiters, and other service workers. Since these jobs tend to fall within the upper, middle, and bottom end of the wage distribution respectively, job polarization explains why in the recent decade, the 90-50 gap in income continues to grow even as the 50-10 gap in income has stayed constant. Higher-earning “abstract reasoning” jobs and lower-earning “non-routine” service jobs are to some degree protected from the technology that depresses the wages and availability of middle-earning “routine” jobs. In later topics in Week Two and later weeks, we will review a series of institutional challenges to SBTC/job polarization, so it is important to first understand the theory on its own terms.

While Autor et al. largely focus on job polarization theory in the abstract, outlining general complements between computing capital and higher-skilled work, Brynjolfsson and McAfee (2014) vividly illustrate some of its dynamics in recent years, and delve into more details about how exactly the rise of computing technology complements higher-skilled work and substitutes for jobs in the middle of the income distribution. They begin with the case of TurboTax, a software for performing taxes. The software made its founder a billionaire, and creates value for all who use it to do taxes more inexpensively, yet leaves thousands of human tax preparers out of work. As such, it illustrates two facets of job polarization. First is the bounty created by new forms of computing capital: the ability to create algorithms that require little human upkeep is enormously profitable for the creators. Yet second, and relevant for the present week’s focus on contributors to 50-10 and 90-50 gaps, as well as Week Three’s focus on growth in the upper income percentile, is the spread of this bounty and its concentration in a smaller range of individuals.

How exactly does this concentration occur? A strength of Brynjolfsson and McAfee (2014) in relation to Autor et al. is in showing when in economic cycles—growth, recessions, and post-recession growth/re-hiring—job polarization tends to occur. In particular, citing qualitative interviews with CEO’s and other business managers, they argue that these managers often recognize that a job falling into Autor et al.’s “routine” category might be substituted for by information and computing technologies. But during periods of revenue growth, it can be challenging to eliminate these jobs. When recessions occur, all jobs face layoffs. Yet when companies engage in post-recession hiring, “jobs doing routine work were not restored.” As such, job polarization occurs during specific
macroeconomic periods that mean that in addition to a recession’s impact being unevenly distributed in terms of which workers face layoffs, the bounty of post-recession recoveries can fail to benefit workers in the middle of the income distribution whose jobs are substituted for.

**Questions to think about on this topic:**

1. Going back to the different ways of defining inequality that we covered in Week One, Autor and co-authors describe how they use data from the CPS to focus on inequality in a specific type of outcome among a specific unit of analysis:

   Real weekly earnings for workers ages 16 to 64 who participate in the labor force on a full-time, full-year (FTFY) basis, defined as working 35-plus hours per week and forty-plus weeks per year.

   Returning to the CBO discussion of market income, pre-tax income, post-tax income, describe how Autor and co-authors describe inequality using a definition that is narrower than market income. Going back to the McCall and Percheski reading, what unit of analysis do they use? How might these two choices affect their descriptions of trends and explanations for these trends?

2. Autor et al. contrast job polarization as an explanation for trends in inequality with what they call “institutional contributors” to inequality like changes to the minimum wage. In contrast to institutional contributors, they describe job polarization as a “demand-side phenomenon, induced by rising relative demands for both high- and low-skill task.” What social, legal, and institutional forces might shape demand for high and low-skill labor (or the perception that this labor cannot or should not be replaced by technology)?

3. Autor et al. argue that high-skill jobs that involve ‘abstract reasoning’ are complemented by rather than substituted for by computer technology. How might advances in algorithms/AI in various higher-skilled domains—for instance, radiology/medical diagnosis (article here); legal discovery (article here)—affect the extent to which this prediction holds going forward? Will these new forms of automation complement the abstract reasoning that occurs in these fields or substitute for it?

4. Brynjolfsson and McAfee (2014), as a specific example of the general trends they outline, describe the shift from Kodak as a dominant photography-oriented company to Instagram and Facebook as dominant companies in the same market space. Kodak employed over 145,000 workers and created a robust middle-class community around its Rochester, New York headquarters, while Instagram had a founding team of 15 and Facebook employs fewer than 1,000 engineers. Beyond the implications of these differences (replicated across many industries) for the distribution of U.S. income inequality, what other implications might the stagnation of large companies that serve as important economic touchstones of cities/suburbs have for the civic life and resources of these places?

**Topics two and three:** Challenges to SBTC/job polarization

SBTC and job polarization faces a large challenge as an exclusive explanation for the three stylized facts about U.S. income inequality we reviewed in Week One: evidence from
cross-national research that finds that advanced industrialized democracies with similar workforce skill composition (and exposed to similar technological pressures) exhibited different trends in inequality. Lemieux from Week One summarizing earlier research by Piketty and Saez, describes the “puzzle of why some advanced countries experienced a sharp increase in inequality while others did not,” with work showing that English-speaking countries (United States, Canada and Britain) experienced a steep growth in inequality, as measured by the share of income or earnings going to the top 10 or 1% of the distribution. By contrast, these shares have remained remarkably stable in France and Japan over the post-war period.” (p. 25).

This challenge–how to explain divergent trends in inequality amidst countries exposed to similar technological forces–has led researchers to focus on a variety of institutional explanations that we will delve into for the remainder of the Week Two readings and in Week Three. Here, we focus on the two sets of institutional explanations most relevant for growth in 50-10 inequality during the 1980’s (bottom-tail inequality) and growth in 90-50 inequality throughout the 1980’s to present day. First is the wage-setting institution of labor union density. Second is partisan politics that not only influences the post-tax income distribution through taxation regimes, but also can influence the pre-tax income distribution through expansionary macroeconomic policy. In Week Three we shift to focus on institutional explanations for inequality at the very top end of the income distribution.

Topic two: Declines in unionization (Western and Rosenfeld, 2011; Farber and Krueger, 1992)

One main set of institutional explanations is changes in the practices and societal strength of various ‘wage-setting institutions.’ And focusing on wage-setting institutions that impact inequality as measured in 90-50 and 50-10 ratios, labor union density emerges as an important institutional contributor to trends in inequality. Western and Rosenfeld (2011) document how between 1973 and 2007, private sector union membership declined from 34 to 8 percent among men and 16 to 6 percent among women. Focusing on these trends, we can ask two questions. First, what role did law and institutions play in this decline, especially since the decline occurred long after a major historical setback to unionization in the form of 1947s Labor-Management Relations Act (Taft-Hartley Act), which included union-weakening provisions such as granting the executive branch greater power to investigate whether a worker strike is harming national health/safety and prohibiting so-called closed shops? Second, regardless of the source of decline in union membership, what role has this decline played in increases in income inequality?

Focusing on whether and how law and institutions have contributed to declines in unionization, Farber and Krueger (1992) attempt to disentangle how much of the decline is attributable to changes in demand for unionization–are workers simply less interested in union representation than in the past (p. 7)–versus how much of the decline is attributable to changes in the supply of potential unions: do employers have greater legal and institutional supports to counter unionization? The authors, focusing on the period from 1977 to 1992, present evidence in favor of a declining demand explanation and find that declining demand for unions among nonunion workers is driven by increases in job satisfaction during this period. They note that this raises the puzzle of why job satisfaction has increased amidst stagnating wages for those at risk of unionization during this period.
Regardless of the source of declines in unionization, unions themselves are important institutions that shape wage and benefit setting. What role has the decline of unions played in increases in inequality, and in which segments of the income distribution have these declines had the largest impact? Western and Rosenfeld (2011) find that declines in unionization explain little of the growth in between-group inequality (wage inequality among persons with different educational attainment) but explain about one-third of the growth in within-group (wage inequality among persons with the same educational attainment), with effects in both union and nonunion sectors. Western and Rosenfeld do not focus on the question of whether declines in unionization explain lower-tail inequality, top-end inequality, or both. But others (e.g., Fortin and Lemieux, 1997, who we are not reading in the course) argue that unions predominantly affect wages for those in the median range of the income distribution—the same workers that the job polarization thesis argues are most susceptible to substitution by computer technology—and account for the growing 90-50 inequality in the 1990s.

Questions to think about on this topic:

1. Farber and Krueger’s article highlights a potential self-reinforcing dynamic behind declines in union density: there might be institutional forces that make unions more difficult to organize and certify (e.g., a set of more conservative appointees to the National Labor Relations Board). But once this decline starts, employees with less exposure to unions may no longer be convinced of a union’s benefits and further decline in unionization may be more driven by a lack of interest in unions than by major institutional forces. Using examples from present-day unionization efforts, do you see evidence of these dynamics?

2. Western and Rosenfeld argue that declines in unionization have spillover effects on the nonunion wage distribution—as they put it, ‘unions not only equalize union members’ wages, they also equalize the nonunion wage distribution by threatening union organization and buttressing norms for fair pay’ (p. 532). What other social mechanisms might produce norms for fair pay in nonunion industries (e.g., social movement action)?

3. Previewing readings in [Week Four] that focus on non-standard/precarious forms of employment—jobs defined by features such as high movement between employers rather than a stable employer-employee relationship; workers who operate as contractors with fewer legal protections than employees—what variation do we see across different industries in the density of unionization? What challenges does non-standard/precarious forms of employment pose for unions as an inequality-mitigating institution?

Topic three: partisan politics as underlying contributor (Bartels, 2008; Kelly and Enns, 2010)

The case of different NLRB stances on unions under different Presidents highlights the role of partisan politics in shaping a broad range of institutions that affect income inequality. Returning to the CBO categories from [Week One] earlier research on the role of partisan politics on income inequality often focused on how redistribution in the form of progressive taxation and transfers shaped inequality in post-tax/transfer income.
This earlier wave of research focused on the median voter theorem (MVT) of politics and redistribution.³ MVT predicts that the level of taxation/redistribution a government adopts will correspond to the level of redistribution that a voter with the median income would endorse. More inequality will lead to a greater demand for redistribution as the median income falls and the median voter prefers a higher level of taxes/transfers on higher income individuals. Less inequality will lead to a lesser demand for redistribution through the opposite process. Throughout, the focus of MVT is on how inequality in pre-tax income will self-correct through the mechanism of voters’ preferences for higher levels of taxation, which will then equalize the distribution of post-tax income.

The puzzle then becomes: why did this self-correction fail to occur? The trends in Week One show that rising inequality in pre-tax income has been accompanied by rising inequality (though at a less sharp rate) in post-tax income, raising questions about whether increasing income inequality is self-reinforcing through mechanisms within partisan politics rather than self-correcting.

Bartels (2008) argues that there are self-reinforcing dynamics to rising U.S. income inequality. He argues that rather than treating inequality as the “passive, technical, and distinctly apolitical issue” sometimes present in media framings of the phenomenon and the framing by proponents of SBTC who pay little attention to “social and political factors” (p. 25), partisan politics plays an important role.

Focusing first on pre-tax income, Bartels shows that there is significantly higher income growth for lower and middle-income individuals during Democratic administrations than Republican administrations; during Republican administrations, there is growth in pre-tax income for the top 10% of the income distribution but little growth for other deciles. Bartels also highlights that these patterns are robust to controlling for time-variant characteristics like oil price shocks and changes in female labor force participation that may be correlated with the party in office and also correlated with income shares.

Bartels notes that the patterns are striking given that they occur in growth in pre-tax income shares, which are often framed as a “natural system existing prior to, and largely separate from, the political sphere” (p. 29). In Bartels’ account, presidents affect the pre-tax income distribution through mechanisms such as whether the president responds to crises such as the OPEC price shock with an expansionary macroeconomic policy focused on stimulating growth (while risking inflation) or a policy emphasizing fiscal restraint (while lowering the risk of inflation). Bartels shows through examples that Democratic presidents tend to pursue the former response to uncontrollable events, while Republican presidents tend to pursue the latter, leading to different patterns of pre-tax growth under each regime. In turn, growth in income inequality under Republican presidents can have self-reinforcing dynamics through policymakers’ greater responsiveness to the preferences of middle and upper-middle income citizens.

Bartels highlights that partisan politics is not only relevant for post-tax income share, but also relevant for the growing inequality in pre-tax income shares that Week One reviews. Bartels offers some empirical explanations for the self-reinforcing dynamics of this growth in pre-tax income inequality—where, as he puts it, “most voters—including most Republican voters—have experienced much more economic prosperity under Democratic


⁴Through stagnation in lower and middle income shares; growth in top decile income shares
presidents than under Republican presidents” (p. 98). For instance, he argues that voters suffer from ‘economic myopia’—focusing on economic performance during the shorter time span of the year prior to the election rather than the more relevant time span of the incumbent’s entire term.

Building on Bartels’ idea about a self-reinforcing dynamic, Kelly and Enns (2010) point out that his analysis shows how voters respond (or fail to respond) to changes in macroeconomic performance rather than how voters respond (or fail to respond) to changes in income inequality. Investigating how changes in income inequality affect public support for expansions in government, the authors find a paradoxical relationship: increases in pre-tax income inequality (measured via the Gini coefficient) are associated with support for less expansionary government policy and vice versa. The relationship holds across both wealthier and poorer citizens, and the authors show that since perceptions of the degree of inequality in the U.S. track the actual degree of inequality, the pattern is not due to lower income individuals’ ignorance of the reality of income inequality.

As such, the self-reinforcing dynamics that Bartels documents might be made more pronounced by the fact that voters do not respond to increases in the degree of income inequality with support for a more expansive set of government policies to mitigate that inequality.

Questions to think about on this topic:

1. Bartels argues that explaining inequality with reference to SBTC/job polarization can make trends in inequality seem like ‘natural’ or ‘apolitical’ phenomena. Focusing on the Autor et al. reading from Topic One, do you agree with this characterization? Why or why not? Could one keep the main arguments of SBTC/job polarization—interactions between tasks for jobs at different levels of the income distribution and technology that substitutes for or complements those jobs affects trends in inequality—while adding a supplementary focus that places more of an emphasis on how various laws and institutions affect the workforce skill composition and the substitution of technology for certain jobs?

2. Bartels notes that he faces an empirical challenge that limits him to studying one form of partisan politics: the partisan makeup of the presidency, since there is not enough variation in partisan control of the Senate/House during the period under study to meaningfully disentangle the effects of Republican versus Democratic majorities. Previewing readings from Week Four that focus on U.S. regional inequality created by federalism, what role do you see for state legislatures—which do change partisan composition more rapidly during the period under study—for trends in income inequality? Since state legislatures focus less on macroeconomic-related policies and more on taxation/transfer decisions, through what levers might state legislatures impact inequality?

3. Kelly and Enns’ preferred explanation for their counterintuitive finding of less support for redistribution under periods of rising income inequality is a media framing explanation. They argue that during times higher inequality, media frames might emphasize the link between individualism and economic success; during times of lower inequality, frames might emphasize the role of government-supported schools and other public goods in fostering good economic outcomes. The authors
note, however, that this explanation is speculative. How might you go about designing a study that tests this explanation for their findings?
Week Three: Explaining Growth in Top-End Inequality- Pay Practices and Marginal Taxation

Brynjolfsson, Erik, and Andrew McAfee. The second machine age: Work, progress, and prosperity in a time of brilliant technologies. WW Norton & Company, 2014. Focus on:
- Chapter 10- 'The Biggest Winners: Stars and Superstars’

- Chapter 8- Two Worlds
- Chapter 9- Inequality of Labor Income


The readings in Week Two focused on the first two stylized trends about U.S. income inequality that we introduced in Week One: growth in 50-10th percentile inequality in the 1980’s; followed by this form of inequality holding constant while 90-50th percentile inequality continued to rise.

The readings in this week focus on the third stylized trend: continued growth in inequality since the 1980’s in top-end incomes, defined as the share of income held by the top 1% and top 0.1% of earners (for shorthand, I will call this top-end inequality). We can divide the readings into three topics.

Topic one: characterizing top-end inequality; labor versus capital income for top earners (Piketty, 2014, Chapter 8; Keister, 2014)

While in Week One we focused on one distinction in the way that researchers can measure inequality—the distinction between market income, pre-tax income, and post-tax income—Piketty (2014) and Keister (2014) argue that to understand growth in top-end inequality, we need to further disentangle between different sources of pre-tax income. In particular, Piketty distinguishes between:

- *Income from labor*: this is income from wages
- *Income from 'capital'*: this is income stemming from returns on 'all types of wealth owned by the individual: stock, bonds, savings accounts, housing, etc.'
Piketty (2014), comparing France and the U.S., shows that much of the decline in inequality over the course of the 20th century in France came through declines in income from capital, or as he puts it, a shift in the composition of top earners from a class of “rentiers” to a class of “managers.” Turning to what Piketty calls the post-1980’s “explosion in inequality” in the United States, trends we reviewed in Week One, Piketty notes that while gains in capital income account for about $\frac{1}{3}$ of the growth in top-end inequality since the 1980’s, the main driver of growth has been “extremely high remunerations at the summit of the wage hierarchy, particularly among top managers of large firms” (p. 298). Calling these high earners a new class of super-managers, Piketty notes that in contrast to prevailing market for superstar talent theories, which argue that high renumeration results from new global audiences to which superstars market their talents (e.g., Rosen, 1981, Mankiw, 2013), fewer than 5% of the top 0.1% are “athletes, artists, or actors”; instead, most are executives and financial services professionals at large firms. This characterization directly challenges the portrait of top earners that Brynjolfsson and McAfee (2014) offer, who, updating Rosen’s theory of superstar talents to the era of new computing and information technology, argue that the top percentiles of the U.S. income distribution are those with ideas and talents that can be shared with newly global audiences through advances in technology.

Keister (2014) confirms this characterization of top earners as “super-managers” rather than superstar talents; using data from the Survey of Consumer Finances, she agrees with Piketty that “the most significant change for top earners” during the growth in top-end inequality has been that “earned income became a much more significant portion of total income, rising from just more than 45% of the total in 2001 to 50% of the total in 2010,” in part due to “rising salaries for very top earners” (p. 356). Both Piketty and Keister highlight that high renumeration for top earners is an important driver of recent growth in U.S. top-end inequality, these patterns are unlikely to change in the short-term wake of the Financial Crisis that resulted in large reductions in capital gains for some earners.

Topic two: bargaining and compensation for top earners (Piketty, 2014, Chapter 9; Diprete, Eirich, and Pittinsky, 2010)

Topic One highlighted the importance of growing renumeration for U.S. individuals in the top 1 and top 0.1% of the U.S. income distribution. Topic Two now turns to the puzzle of why executives/professionals in the U.S. (and to a lesser extent, other English-speaking Anglo-Saxon countries like the U.K.) have been able to successfully negotiate for and receive this high renumeration amidst rising inequality, while executives/professionals in countries within Continental Europe and Japan have not experienced the same growth in renumeration. Piketty ultimately settles on a bargaining and social norms explanation for why U.S. top earners have been more successful at achieving high renumeration (decoupled from high performance/high productivity) than counterparts in Continental Europe. Decreased marginal tax rates mean that top earners now have a reason to spend more effort on negotiating for higher pay with institutional actors in charge of compensation (e.g., corporate boards and their compensation committees). The fact that earners will retain more of this compensation after taxes makes these bargaining efforts more attractive. Though Piketty et al. do not describe the institutional contexts in which this more intensive bargaining takes place, the theory does highlight how another set of institutions—federal and state legislatures that negotiate and set tax rate—may play a role
not only in redistribution of income that affects the post-tax income distribution, but also in the pre-tax income distribution among top earners through giving these earners more or less incentive to negotiate for high pay.

While Piketty gives a high-level overview of how top earners negotiate for high compensation, DiPrete et al. (2010) delve more into the organizational factors that affect these negotiations. The analysis focuses on CEO’s, who compose a small percentage of the top 0.1% but who are illustrative for the way it sheds light on how the social norms that Piketty describes as driving increased executive compensation are enacted through organizational practices. DiPrete and co-authors argue that increases in CEO pay are partially driven by a process where compensation committees and Boards of Directions benchmark pay based on a cognitively relevant peer group (e.g., CEO’s at similarly large firms) and where a process of leapfrogging often occurs: if a small group of firms raises CEO salaries significantly (the focal CEOs), this has ripple effects that drive the entire distribution of CEO pay upwards as committees upwardly adjust the compensation of CEO’s in the focal CEOs’ peer groups. Thus, rather than explaining high compensation solely as a result of individual bargaining and social norms that make high renumeration acceptable, DiPrete and co-authors show how organizational practices like peer group benchmarking can lead to the compensation increases that contribute to top-end inequality.

Questions to think about on Topic One and Topic Two:

1. We can return to the topic posed in Week One: one barrier to policy interventions meant to alleviate inequality is the perceived justness or legitimacy of the distribution, or the extent to which individuals in the distribution are thought to deserve to have the level of resources that they possess. How might the fact that U.S. top earners, while earning a higher share of income from capital than those in the rest of the distribution, now earn a substantial share from labor income (Keister and Piketty) affect the perceived legitimacy of this form of inequality?

2. Piketty notes the empirical trend of English-speaking Anglo-Saxon countries exhibiting a sharper increase in the “supermanager” phenomenon in financial and nonfinancial sectors than countries in Continental Europe and Japan. What sociological mechanisms might lead English-speaking countries to share similar norms and practices towards the acceptability of high compensation—e.g., is it greater exchange between political elites in more culturally similar countries?; greater exchange of academic ideas that frame inequality and renumeration in a particular way?

3. DiPrete and co-authors focus on compensation committee benchmarking as one practice within organizations that can lead to processes of diffusion where a leap in CEO pay in one firm has ripple effects across other organizations. What other organizational practices might lead to 1) high renumeration for some workers; 2) the framing of that renumeration as legitimate; 3) the spread of those renumeration practices across organizations?
Topic three: is higher taxation on this top-earner compensation self-defeating? (Young et al., 2016)

Topics One and Two highlight the presence of and contributors to growth in top-end inequality. The final topic/article addresses the question: suppose that this inequality in pre-tax income persists, is it futile for governments to mitigate this inequality in pre-tax incomes through increased marginal tax rates on top earners? In particular, as federal tax policy shifts “away from taxation of the elite” (Young et al., p. 421) and state governments attempt to find new revenues from the top of the income distribution, does increased marginal taxation on top earners—“millionaire taxes” for short—result in capital flight as individuals seek the most advantageous tax jurisdiction? This question is important because traditional economic models, which often assume that there is relatively little friction in geographic mobility, frame variation in marginal tax rates within a federalist system as self-defeating from a revenue-generation standpoint: faced with higher tax rates, high-income individuals will migrate from higher tax rate states to lower tax rate states, and the state that originally proposed the tax increase will end up with no meaningful long-run increases in revenue to fund redistribution or other transfer programs.

In contrast to this standard economic model, Young and co-authors find that millionaires are more stuck in place and less mobile in response to increased taxation than the models predict. More specifically, using a gravity model of migration that models the flow of migrants from state $i$ (origin) to state $j$ (destination) as a function of both variables that mechanically predict increased interstate migration between $i$ and $j$ (e.g., distance; shared border), and the variable of interest—the difference in top income tax rates between $i$ and $j$—the authors find few statistically/substantively meaningful effects of higher tax rates in state $i$ in inducing migration to state $j$, especially once Florida is removed as an attractive destination for high earners. The authors findings have implications for how we interpret the efficacy of government intervention to redistribute both the labor/capital income that Piketty, Keister, and DiPrete et al. focus on: namely, that even if federal efforts at redistribution stagnate due to the processes that Bartels from Week Two documents, efforts at redistribution that vary across localities in a federalist system are not necessarily self-defeating.

Questions to think about on Topic Three:

1. What sociological factors might make interstate migration in search of lower tax rates less “frictionless” than in traditional economic models? Put another way, what might explain “the most striking finding” of “how little elites seem willing to move to exploit tax advantages across state lines” (p. 439)

2. Previewing next week’s readings on regional inequalities, what are some drawbacks of variation in state legislatures’ willingness to impose higher tax rates on top incomes for revenue-generation, or the size of the population on which to impose these taxes? What implications might this have for the generosity of transfers/redistribution that lower-income individuals living in different states receive?
Week Four: Beyond a Single Nation as the Unit of Analysis- Income Inequality Between and Within Nations


• Section IV- ‘Socioeconomic Rights’


• Chapter 9- 'The Deep Story’
• Chapter 10- 'The Team Player: Loyalty Above All’

The readings in Weeks One through Three chose a specific geographic unit of analysis: the nation. Largely focused on the U.S., but bringing in other countries such as the U.K., France, and Japan for comparative leverage, the analysis was on the distribution of income and wealth across an entire nation.

This week’s readings shift that lens in two ways. Topic one focuses on global inequality: is the salient form of inequality within the income distribution of nations like the U.S., or between nations? Milanovic argues the latter is the dominant form of contemporary inequality. Topic two focuses on regional inequality in the U.S. As Hochschild notes at the beginning of her book:

Across the country, red states are poorer and have more teen mothers, more divorce, worse health, more obesity, more trauma-related deaths, more low-birth-weight babies, and lower school enrollment. On average, people in red states die five years earlier than people in blue states. Indeed, the gap in life expectancy between Louisiana (75.7) and Connecticut (80.8) is the same as that between the United States and Nicaragua (p. 8)

This highlights that one’s place in the income distribution, and place in the distribution of other markers of wellbeing such as life expectancy, is tied not only to one’s country of residence as Milanovic argues but also to one’s state of residence within the U.S.. The readings in this topic focus on U.S. federalism as one contribution to these between-state inequalities.
Topic one: between-nation inequality (Milanovic, 2016)

The readings in previous weeks focused on the challenges associated with being at the bottom of the income distribution within the U.S. as higher percentiles gained a larger share of growth in national income. Milanovic argues that even if an individual is at the bottom of his or her own nation’s income distribution, he or she may still occupy a relatively privileged position within the distribution of global income. For instance, he notes that individuals at the bottom of Germany’s national income distribution still have higher levels of income than 60% of individuals in China. Recognizing these descriptive patterns, Milanovic builds a model to ask: what is the locational premium associated with living in various nations, defined as the gain in an individual’s income relative to individuals in the world’s poorest nation (the Democratic Republic of Congo)? Individuals derive this locational premium from two sources that adhere to nations rather than to individuals and are thus not traceable to individual effort/desert:

1. A certain mean level of income (operationalized alternately as GDP or educational attainment)
2. A certain level of income inequality

Milanovic finds that these two features of a country explain two-thirds of the variability in individual income percentiles across the world, and the locational premium is greatest for those in the 50th-80th income percentiles. In terms of how a nation’s level of income inequality matters differently for individuals in different places in that nation’s income distribution, Milanovic finds a U-shaped curve. Those at the bottom of the income distribution benefit most from a lower Gini coefficient (less inequality) while those at the top of the income distribution benefit most from a higher Gini coefficient. Meanwhile, for those in the middle of the income distribution, the Gini coefficient matters little for their overall income attainment: a country’s GDP and other absolute levels of income are more important.

Milanovic does not delve into causes of the highly skewed global income distribution, but his perspective does show how depending on the country in which one lives, one’s economically-rational response to inequality might differ. Using the metaphor of a flagpole of the world’s income distribution where each country has a plaque that represents the range from incomes in that country, he notes that an individual can try to improve his place on the pole by moving up within his country’s income distribution (shifting up within the plaque), by increasing the country’s whole GDP or placement on the pole (shifting the entire plaque upwards), or by trying to migrate (jumping between plaques). For many of the world’s individuals, migration is the most economically-rational response.

Questions to think about on Topic One:

1. Milanovic shifts the unit at which we analyze inequality from an individual’s nation to the world. To explore the benefits and drawbacks of that analytic shift, think about the distinction between absolute deprivation—the level of resources someone has—versus relative deprivation—perceptions of how one’s resources compare to resources held by other individuals. When judging relative deprivation, individuals construct a cognitively salient reference category with whom to compare the level of resources they own. What geographic scale do you think people use for this

---

5 Adjusted for purchasing power parity.
cognitive reference category to judge whether they are poorly or well off—the nation? The world? One’s local community? How might the empirical answer to this question affect our choice of geographic units within which to measure inequality.

2. Thinking of three options available to individuals in Milanovic’s flagpole analogy—moving within their country’s plaque; shifting the entire country’s plaque upwards; jumping plaques—what policies do you see directed at each?

3. Focusing on “jumping plaques”—moving from a country with lower mean GDP to one with higher mean GDP—how do tensions within higher income nations’ migration policies (e.g., between helping the least well off individuals like refugees; attracting skilled workers to fill labor market gaps) facilitate or constrain this option for certain individuals hoping to migrate?


Milanovic’s article expands the geographic lens with which we analyze income inequality from the scale of the nation to the scale of the world. Yet we can also narrow the geographic lens in a fruitful way: within the U.S., what regional inequalities exist that affect an individual’s level of income and access to social policies that alleviate some of the negative consequences of income shortfalls?

Meyerson (2015) and readings that follow (e.g., Hochschild, 2016) argue that the division between largely ‘Blue’ Northern states and largely ‘Red’ Southern states is important from the standpoint of income inequality. While the readings in Weeks One through Three focused on the hollowing out of manufacturing employment that once fell in the middle of the income distribution through forces that included substitution by technology, political decisions about taxation and redistribution, and other factors, Meyerson notes a surprising reversal of this trend in recent years. Auto industry manufacturing jobs once exported to China have returned to the U.S. due to two forces: rising wages in China and falling wages in the U.S. in states that lack state-specific minimum wage legislation. Meyerson shows how the productivity-adjusted pay of Chinese low-wage workers has risen from 36% of the pay of Mississippi low-wage workers in 2000 to 69% in 2015, making relocation of jobs from China back to the U.S. more appealing.

The author notes that this relocation of manufacturing jobs has not been evenly spread across the U.S. but instead has been concentrated in a few states in the South that have the weakest set of state-level workplace protection (e.g., minimum wage; unionization rights) legislation: Alabama, Louisiana, Tennessee, South Carolina, and Mississippi. And just as Northern States in the pre Civil-War era benefited financially from the slave labor-induced productivity of Southern States, Northern states are not immune from benefiting from the weak workplace protections that have led to a revival of very low wage manufacturing in the U.S. South. Policies weakening workplace protections spread from Southern states to traditionally union-dense states like Wisconsin, Indiana, and Michigan. Despite this diffusion, Meyerson emphasizes that a “divided government at the federal level” that has blocked measures like large federal minimum-wage hikes has created diverging destinies for individuals living in two sets of U.S. states/localities: individuals living in states and cities with weak labor rights that lead to low wages for
those at the bottom of the income distribution versus individuals living in states and cities where liberal coalitions “have taken it upon themselves to enact their own minimum-wage increases, paid sick-day legislation, and statutes making it easier to vote.”

While Meyerson (2015) notes that lower-income workers living in two sets of states/locales—states with weak labor rights and protections; states with stronger labor rights and protections—have experienced diverging destinies in the absence of federal legislation that applies to workers regardless of their place of residence, Versteeg and Zackin (2014) illustrate why this is the case. Constitutional scholars have long pointed to the exceptionalism of the U.S. constitution in the domain of what the authors call positive or socioeconomic rights such as “rights to free education, labor rights, social welfare rights, and environmental rights” (p. 1645). While most other national constitutions contain these rights, they are notably absent from the U.S. federal constitution.

Reviewing all U.S. state constitutions produced and revised between 1776 and 2011, and focusing on nine positive/socioeconomic rights (e.g., education; minimum wage; rights or guarantees for the poor), Versteeg and Zackin highlight a wrinkle in this exceptionalism: mentions of these rights are prevalent within these U.S. state constitutions. Yet the pattern they find about the number of positive rights contained in different state constitutions, reproduced in Figure 1 below, introduces a puzzle when contrasted with the lack of these rights in U.S. Southern states that Meyerson and Hochschild document: there are not clear regional patterns where Northern states formally outline a higher number of positive rights. This raises the question: if Southern and Northern state constitutions contain similar counts of formal positive rights, what leads to the regional divergences in the strength of these rights in action that others document?

Figure 1: Count of positive rights in U.S. state constitutions. Taken from Versteeg and Zackin (2014), page 1687

One factor that both Versteeg and Zackin and Meyer (2007) focus on is how justiciable the positive rights are and if justiciable, which stakeholders have the incentive to use those rights to press for various distributional outcomes for citizens living in the state. Later Weeks of the course will delve deeper into the topic of inequalities that result from unequal enforcement of legal rights. For now, worth noting is Meyer’s discussion of the important role of state attorney generals (SAG) in filing litigation that draws upon vague rights such as a right to education or right to a healthy environment enumerated in state constitutions. He argues that SAG, who often take the role hoping to run for Governor or another higher office afterwards, have an incentive to take on litigation related to positive rights in order to appeal to interest groups beyond bar associations and
other narrower groups. As Meyer puts it, “by developing a record in areas such as health care or securities regulation, an attorney general can position himself to appeal to the median voter in an election for higher office” (p. 895). While all SAG have this general interest, the state political climate in which the SAG operates influences the appeal of this type of litigation, with Meyer citing research that Democratic SAG’s pursue more of this litigation than Republican SAG’s. Thus, we see two institutional sources of the labor protection and health-related inequalities that Meyerson documents and that motivate Hochschild’s in-depth focus on Louisiana:

1. Inequalities in the formal rights promised in either state constitutions or state-level legislation
2. Inequalities in the extent to which interest groups and political actors like state attorney generals enforce enumerated rights in ways that improve wellbeing for those living in the state

The previous articles focus on how the formal structure of federalism–state constitutions; state legislation; enforcement actions by state political actors–produces regional inequalities in the United States. Hochschild (2016) begins with a puzzle, or what she calls the 'Great Paradox': why do residents living in Louisiana, a state that falls at the bottom of most between-state rankings of income, life expectancy, and other markers of human development, oppose federal and state-level legislation that might improve those indices? While Hochschild’s main focus is on why the citizens oppose environmental regulations despite suffering from some of the worst exposures to industrial pollution of any individual in the U.S., her analysis helps us more broadly understand how certain cultural narratives lead individuals to oppose policies that might alleviate some of the worst regional inequalities.

Hochschild argues that behind this opposition is a common deep story about the causes of U.S. income inequality that the Tea Party members she observes recount. In this deep story, individuals are waiting in the middle of a line leading up to a hill and just over the hill is the American Dream. They feel stuck and the line isn’t moving. While stuck in the middle of the line failing to reach the hill of the American Dream, they observe line-cutters. These line-cutters include Black people whom they think benefit from various affirmative action policies, overpaid public sector employees who they think are largely women and minorities and who seem to work fewer hours but with larger pensions and more job security, immigrants who lower the pay of white Americans, and refugees who drain important money they pay in taxes. Those standing in line feel sympathy for these groups, but they think they are violating rules of fairness. Meanwhile, they feel that their own president (Obama at the time) has betrayed them by actively helping the line-cutters while ignoring those who have waited in line patiently.

Hochschild argues that this deep story highlights that there are two distinct cultural responses to U.S. income, each fueled by a different deep story about the causes of the type of stagnation in median incomes that Week One and Week Two document. On the left, class conflict is directed up the ladder towards the top 1% and perceived corporate greed. For those on the left, federal and state intervention becomes an ally, while unfettered free markets become an enemy. In contrast, on the right, class conflict is directed down the ladder towards lower-income individuals who they believe are draining their tax money with undeserved disability checks and SNAP benefits. As a result, for those on the right, federal and state intervention becomes an enemy for helping those
they perceive as line-cutters, while the free market becomes an ally. While this is no doubt a simplified portrait, it reiterates Kelly and Enns’ explanation in Week Two for why support for redistribution may decrease during times of rising inequality. During these times, cultural narratives emphasize individualism; these narratives may frame government intervention as an enemy that helps people “cut in line” in front of more individualistic strivers.

Questions to think about on Topic Two:

1. The Meyerson and Hochschild readings each demonstrate pitfalls of federalism, and how it has led to regional inequalities that leave residents living in Southern states with more precarious employment, fewer workplace protections, and more anemic social policies to alleviate some of inequality’s consequences. Yet proponents of federalism point to various virtues such as the idea that state governments might be significantly more responsible to constituents than the federal government, or the idea that federalism allows for values pluralism where individuals with different “deep stories” about the role of government can congregate in a state with similar individuals. How should we weigh these benefits and drawbacks from the standpoint of inequality?

2. Versteeg and Zackin’s article, as well as Meyerson, each focus on the role of positive/socioeconomic rights outlined in state constitutions in providing a tool for activism centered on redistribution. Yet as the first authors describe, these positive rights are often specified in broad/vague ways. For instance, the New York state constitution outlines the following positive right to care for the poor:

   The protection and promotion of the health of the inhabitants of the state are matters of public concern and provision therefor shall be made by the state ... by such means as the legislature shall from time to time determine (p. 1625)

From the standpoint of activism aimed at mitigating the effects of income inequality that uses these constitutions as tools, what are the advantages and disadvantages of rights that are broadly defined versus rights that are more clearly circumscribed?
Week Five: The Everyday Texture of Income Stagnation: Precarious Employment and the Retrenchment of Policies to Buffer Precarity


- ‘Vignettes from the Modern Workplace’
- Chapter 1- ’The Fissured Workplace and its Consequences’
- Chapter 2- ‘Employment in a Pre-Fissured World’


Edin, Kathryn J., and H. Luke Shaefer. $2.00 a day: Living on almost nothing in America. *Houghton Mifflin Harcourt*, 2015. Focus on:
- Chapter 1- ’Welfare is Dead’
- Chapter 2- ’Perilous Work’

---

**Week One** and **Week Two** focused on stylized facts about U.S. income inequality: the 1980’s were marked by growing lower-tail or bottom-end inequality, defined as inequality between those in the 50th income percentile and those in the 10th income percentile. These 50-10 gaps stopped growing in the late 1980’s, and growth in inequality since then is marked by growth in top end inequality: inequality between those in the 90th income percentile and those in the 50th percentile, with further differentiation at the very top of the income distribution.

The readings that followed focused on different *explanations for* these stylized facts. The readings in the present week investigate some of the *consequences of* these stylized facts—and in particular, the stagnation of 50th percentile incomes— for an important domain of human life: work/employment. How do the broad macro-level trends that previous Weeks focus on—the rise of computer technology that substitutes for more routine labor (*Autor et al.*; *Brynjolfsson and McAfee*); the decline of unions and other wage-setting institutions to sustain wages in the face of these technological shifts (*Western and Rosenfeld*); the lack of a robust political response to growing income inequality (*Bartels*)—manifest themselves in individual employment trajectories?

As Kalleberg (2009) puts it, employment relations serve as a useful site for linking these micro and macro levels of analysis:
Employment relations are useful for studying the connections between macro and micro levels of analysis...because they explicitly link individuals to the workplaces and other institutions...Moreover, employment relations are embedded in other social institutions, such as the family, education, politics, and the healthcare sector (p. 12)

Thus, a focus on employment relations can help shed light on some of the micro-level causes and consequences of the overall trends in income inequality and institution-level sources of those trends reviewed in previous weeks. The week is divided into two topics.

**Topic one: causes and consequences of precarious employment (Kalleberg, 2009; Weil, 2014; Rogers, 2016)**

Kalleberg (2009) defines precarious work as “work that is uncertain, unpredictable, and risky from the point of view of the worker.” He argues that the same macro-level changes reviewed in Weeks One through Three that drive macro-level trends in inequality—technological change; the decline of unions and other wage-settings institutions—have driven increases in precarious work since the mid to late 1970’s. But Kalleberg also argues that it is important to recognize that precarious work is not solely a twentieth and twenty-first century phenomenon. Instead, we can think of three general eras in U.S. employment relations.

First, work prior to the New Deal was highly precarious and characterized by a lack of wage and labor protections. Framed in terms of Polanyi’s ideas about a Double Movement, this first era of precarious work served as the first side of this double movement as labor was transformed into a fictitious commodity meant to take on the portability and flexibility of real commodities. But as this first move of the double movement began to disrupt individual wellbeing, there was the second move via New Deal and post-WWII policies meant to counteract the effects of precarious employment on individual’s income and health.

These policies, combined with the U.S.’s avoidance of the damage to capital from WWII that affected their European counterparts, lead to what Kalleberg calls the great compression of the post-WWII era in income inequality. He argues that a cornerstone of this post-WWII compression in income inequality was the idea of a standard employment relationship (SER), characterized by long tenures with a single employer, stability in employment, a perceived sense of job security, and risk sharing between employers and employees through workplace benefits such as health insurance and defined-benefit pensions. Precarious work results from the reversal of these elements of the SER—short tenures; a lack of stability in employment; a perceived sense of job insecurity; and risk shifted from collective pools to a more privatized/personalized burden. While Kalleberg does an admirable job of defining precarious work and outlining his features, the details of why there has been an unraveling of the standard employment relationship and a rise in precarious work are largely told in broad strokes. For more depth on those details, we turn to Weil (2014).

Weil (2014) outlines the same general unraveling of the SER as Kalleberg (2009). Describing this unraveling as the 'fissuring' of employment, the strength of Weil (2014) is in the detailed account of the institutional contributors to this process. Or as he puts it:

The forces leading to vulnerable work conditions...are not an inevitable result
of the nature of those jobs or industries. They arise from how those sectors have come to be organized (p. 3).

Weil shows how the organization of various business sectors has changed from the “pre-fissured world,” a world that Kalleberg characterizes as being marked by a higher prevalence of the standard employment relationship. In the “pre-fissured world,” business was characterized by large enterprises, or what Weil calls lead businesses, that sought to place many activities they conducted within the walls of the firm rather than outside the walls of the firm. Organizational theorists like Coase and Oliver Williamson noted that firms choose to conduct an activity inside their walls to economize on transaction costs: for various reasons, it would cost more to outsource these activities to a competitive market and monitor market actors through contracts than to conduct these activities in-house. The most important consequence of these large integrated enterprises for workers was the existence of internal labor markets that lead to job security for an individual worker and the compression of wages across the distribution. As Weil puts it:

For employees, once on the inside of an internal labor market, whether entering as a production worker in a steel plant...or a junior executive at a chemical company—one could look forward to certainty of employment, an established profile of wage or salary increases over time, and fairly clear expectations of what was required to retain employment and advance in the organization (p. 38).

Weil pinpoints three sets of changes that led to the fissuring of the workplace or what Kalleberg describes as the unraveling of the standard employment relationship. First is that increased access to capital markets led to the involvement of new stakeholders—investors, lenders, and others—who pressed for a new corporate strategy of businesses maintaining a “laser-like focus” on core competencies. Businesses turned to various forms of sub-contracting and out-sourcing for activities outside their core competencies such as janitorial work and human resources, and shifting who performed activities from employees of the firm itself to employees of sub-contracted agencies led to a reduction in the business’ cost as they decreased mandatory payments to social programs like unemployment and worker’s compensation and as they shed liability for workplace injuries. Weil is careful to note that while some of this shift from employees to sub-contractors was the result of a deliberate misclassification of the employment status of various workers, the shift also occurred due to vast improvements in information and communication technologies that made de-centralized supervision much easier.

Weil argues that lead businesses, conscious of their public image and brand, would not have been willing to engage in such extensive sub-contracting in the absence of the third factor, or what he deems the glue that holds fissured businesses together: standards for products/services and the technology to enforce those standards. Through technologies that helped with supply chain monitoring and other ways to monitor sub-contractors, lead businesses could monitor the quality of output of sub-contractors’ products or services while retaining limited legal liability for the employment climate and wage behavior of sub-contractees. Thus, large enterprises contribute to various features of precarious employment that Kalleberg and others document—lack of protection from layoffs; stagnating wages amidst the absence of robust internal labor markets; greater insecurity—but are largely exempted from public or legal scrutiny for these changes since they can point to decisions by smaller enterprises.
While Weil (2014) focuses on fissuring across a variety of sectors, Rogers (2016) focuses on a specific manifestation of this “fissuring.” He focuses not on how fissuring has unfolded in large enterprises that also existed in a pre-fissured era, employing workers using a more stable employment relationship, but instead on new enterprises in the “platform economy” that have relied upon arms-length contracting relationships since their founding as a key route to profitability. Rogers argues that the two legal standards that courts use to establish the presence of an employment relationship—control and dependency provisions—have faltered in at least two ways light of the changing dynamics of work that Kalleberg and Weil identify.

First, control and dependence provisions fail to reflect the fact that “large firms are often better positioned to ensure compliance with employment laws than their thinly-capitalized contractors and suppliers” (p. 486); thus, while a small, sub-contracting firm may formally employ an individual and meet the control/dependence test, it is the large firms that initially sub-contracted out the tasks who have the ability and resources to prevent workplace abuses such as wage theft and failure to adhere to minimum wage legislation. Second, the two provisions contain perverse incentives that lead to the fissuring that Weil documents: employers have an incentive to re-structure work relationships in a way that helps the firm avoid employment law obligations but that hurts workers by granting them fewer rights and protections.

**Questions to think about on Topic One:**

1. **Kalleberg** is careful to emphasize that while the standard employment relationship provided security and stability to many workers, there were certain sub-groups who were largely excluded: women and racial minorities. Previewing the next section of the course on intergenerational mobility, how might the SER either facilitate or constrain entry of marginalized sub-groups into certain occupations? Are there any benefits to these sub-groups of employment characterized by shorter tenures with specific employers and other features that Kalleberg labels as negative?

2. **Week Two’s** readings on skill-biased technological change illustrate one way that technology structures inequality: by substituting for or complementing work with different characteristics. **Weil’s** article on the role of communications and information technology that help large enterprises de-centralize tasks to sub-contractors whom they monitor highlights another way that technology structures inequality. Describe the difference between the impact of technology on inequality in Weil’s account and the impact of technology on inequality in the account by Autor et al.

**Topic two: the subterranean retreatment of policies to protect against precarious employments’ consequences (Hacker, 2004; Edin and Schaefer, 2015)**

The readings in Topic One show how the macro-level trends in income inequality on which Weeks One through Weeks Three focus are accompanied by a micro-level restructuring of the relationship between employers and employees. In turn, they show that this re-structuring imposes new forms of income insecurity and risks upon certain workers. The readings in the present Topic focus on the question: has social policy
adapted to what Hacker (2004) describes as “shifting constellations of risk” (p. 257) in ways that buffer the harm these risks pose?

Hacker (2004) argues that the answer is “no”—social policy has not adapted to the shifting constellation of risks individuals face. Yet this failure takes the form of a more subtle type of policy retrenchment than researchers are accustomed to analyzing. In particular, past researchers studying policy retrenchment—defined by Pierson (1994) as “policy changes that either cut social expenditure, restructure welfare state programs to conform more closely to the residual welfare state model” (programmatic retrenchment) or that “alter the political environment in ways that enhance the probability of such outcomes in the future” (systemic retrenchment)—focused on one type of retrenchment: formal revisions to policies.

Focusing on these formal revisions, Pierson himself argues that policy retrenchment has not occurred in the United States. So returning to our original question, one might conclude that the lack of either programmatic or systemic retrenchment in the form of major U.S. policy revisions means that workers are just as protected by social policies today as they were in the “pre-fissured world” of standard employment relationships.

Hacker argues that this conclusion would be misguided, because formal revision to policies is only one of at least four ways in which policy retrenchment can occur. The other three forms of policy retrenchment, while less overt, may have equally large consequences. Hacker argues that in the face of the shifting constellation of risks, U.S. social policy has had only an ‘anemic’ response, and this anemic response has been marked by all four types of policy retrenchment. First is the most well-studied type: formal policy revisions; in the U.S. case, this comes in the form of changes like the switch from Aid to Families with Dependent Children (AFDC) to Temporary Assistance to Needy Families (TANF) that Edin and Schaefer (2015) discuss in greater detail below. But more consequential than these formal revisions has been the other three types of retrenchment:

1. **Drift**: changes in the operation or effect of policies that occur without any significant changes in the policy’s structure. One example of drift that Hacker identifies is in a small provision of 1974’s Employment Retirement and Income Security Act (ERISA). The provision granted health insurance plans directly sponsored by employers—self-insured plans—an exemption from state-level regulations concerning healthcare benefits. Drift occurred because although few employers had self-insured plans at the policy’s outset, the exemption led to a marked increase in these self-insured plans with two consequences. First is that the effect of the exemption was magnified as more employers switched into self-insured plans to fall under the exemption’s reach. Second is that pool over which health risks were collectivized via insurance become smaller, as risks were pooled within firms covered by a self-insured plan rather than between a large number of firms/individuals. And since self-insurance was not feasible for smaller employers, individuals shut out of the large enterprises that Weil documents were shedding workers were left either without employer-sponsored health insurance or with insurance occupied with a riskier pool of individuals.

2. **Conversion**: Hacker argues that for those pursuing reform in the form of retrenchment, it is often easier to work within an existing policy rather than formally end

---

the policy or enact a new one. Conversion occurs when a policy is opaque and broad enough to allow actors to co-opt it for different ends. An example is two types of pension systems—public systems allow for little conversion due to detailed rules translating quantity of work quarters into benefits, while tax breaks for employer-provided pensions give employers discretion to shift benefits in ways that leave less of a retirement buffer for workers.

3. **Layering**: this involves layering new requirements or rules on top of existing policies; an example is rather than repealing a policy, the federal government granting states waivers to ignore various provisions in ways that constitute retrenchment.

By highlighting these subtle, and perhaps more insidious, forms that retrenchment takes, Hacker shows how the new sets of risks that workers in the lower and middle ends of the income distribution face are not buffered by compensatory social policies. 

Hacker is careful to note that the four forms of retrenchment are not mutually exclusive but often occur in conjunction with each other. Edin and Schaefer (2015), focusing on a segment of the income distribution most in need of policies to buffer against various shocks—those earning less than $2 a day—highlight how two forms of retrenchment worked together with negative effects on inequality. In the chapter on “The End of Welfare,” the authors document on-the-ground practices after a large formal revision of cash welfare policy: the shift from AFDC to TANF block grants. While the formal shift itself had negative consequences for individuals’ income stability, these negative consequences were exacerbated by the type of subterranean adjustments by state legislatures and street-level bureaucrats about which Hacker theorizes. For instance, loose federal guidelines governing how TANF block grants were allocated resulted in states shifting the money to cover budget shortfalls far beyond the original program’s aims. At the local level, bureaucrats told some recipients that there was not enough money to go around despite state surpluses. The qualitative research thus highlights the impact that more subtle forms of retrenchment have on security of those subject to the worst forms of precarious work documented in Topic one.

**Questions to think about on Topic Two:**

1. Relating to Hacker’s arguments, researchers often focus on retrenchment through formal policy revisions but not the other three types not only because such revisions are thought to be more substantively important. Part of this focus is greater challenges detecting and measuring the more subtle forms of retrenchment. What research strategies might you pursue to analyze drift, conversion, and layering of a social policy?

2. Edin and Schaefer document how in addition to formal changes to cash welfare reducing the caseload, shrinkage of TANF is also driven by non-awareness of the policy within certain social networks and local communities. How might retrenchment thus have self-reinforcing dynamics where cutbacks lead to fewer recipients, and fewer recipients means fewer “seeds” in a social network to spread information and awareness about potentially beneficial social policies?
Section Two: Law, Institutions, and Intergenerational Mobility
Week Six: Overview of U.S. Trends in Intergenerational Mobility


Weeks One through Five focused on various explanations for three stylized facts about U.S. income inequality—a general increase in income inequality beginning in the 1980s; an initial contribution of bottom-tail inequality to this increase that stagnated in the 1990s; growth in top-end inequality throughout the period with further differentiation among top earners—and the readings include both explanations such as job polarization that focus on the interaction between technological change and workforce educational composition and a set of more institutional explanations ranging from declines in unionization to new pay-setting preferences and practices among top earners.

The readings in Section Two of the syllabus, which encompasses Weeks Six through Nine, shift focus slightly from inequality to mobility. Mobility is important to examine because empirically citizens might tolerate high levels of income inequality if there is high mobility: that is, if a given individual’s placement on the distribution is relatively uncorrelated with features outside of that individuals control such as the socioeconomic status of her parents, her race, her gender, or other ascriptive characteristics. The stylized trends in inequality we reviewed in Week One included the fact that income inequality has increased since the early 1980’s. The present week’s readings explore: during this time, has intergenerational mobility increased (as predicted by theories that see higher levels of inequality, by motivating effort and increased human capital investment, increasing levels of mobility), decreased (as predicted by theories like the idea of a Great Gatsby curve where higher levels of inequality dampen mobility through various mechanisms), or stayed constant?

Breen and Jonsson (2005), while taking a comparative lens, highlight important U.S. mobility trends during the post-WWII period. First focusing on the relationship between social origins (fathers’ occupation; fathers years of education) and an individual’s educational attainment (years of education; transitions from primary to secondary education; secondary to higher education; etc.), Breen and Jonsson note that the U.S. experienced stability in this relationship in recent decades. That is, amidst rising inequality, intergenerational mobility in terms of educational attainment has neither increased nor decreased.

Focusing on income tells a similar story, with mobility fairly stable in recent decades. However, contextualizing the United States’ degree of intergenerational mobility in in-
come, measured either using father-son income, father-daughter income, or sibling correlations in income, the U.S. has higher elasticities (less mobility) than most Nordic countries, with father-son elasticities of roughly 0.45 compared to 0.13 to 0.34 in Sweden and Germany respectively, and sibling correlations in income of 0.4 compared to roughly 0.25 in various Nordic countries. Thus, while the level of intergenerational income mobility has remained largely stable in the U.S., this level remains higher than in other advanced industrialized democracies.

The authors note that these patterns of stability stand in contrast to two prevailing theories that offer two sets of predictions about mobility in the modern era. The first—modernization theory—argues that the effects of social origin on one’s educational attainment should become weaker over time (mobility should increase) as institutions seek to become more meritocratic in their selection criteria. Week Eight of this syllabus, which focuses on horizontal stratification within both elementary/secondary schools (in terms of financing) and colleges (in terms of post-graduating earnings and debt repayment), highlights why this modernization hypothesis was overly optimistic about educational institutions as an engine for mobility. The second—reproduction theory—argues that inequalities based on social origins may decrease at educational transitions earlier on in an individual’s life (e.g., the transition from high school to entering college) but will be more than compensated for by an increase in inequalities based on social origins in later educational transitions (e.g., the transition from college to professional degree). Stability in the relationship between social origin and educational attainment across levels of educational transitions goes against each of those hypotheses, with the stability amidst rising inequality a puzzle that remains to be explained.

Breen and Jonsson (2005) review intergenerational mobility trends for a slightly older cohort of U.S. individuals affected by social policy reforms such as the post-WWII expansion in higher education that Week Eight will focus on more deeply. Meanwhile, Chetty et al. (2014) use tax micro-data to examine levels of intergenerational mobility for more recent cohorts of U.S. individuals: 1971 to 1993 birth cohorts. For children born between 1971 and 1986 (old enough to be between 26 and 39 at the time of the 2012 data cutoff), the authors estimate two measures of intergenerational income mobility:

1. The correlation between a parent and child’s percentile ranks (e.g., 1 to 100) in the U.S. income distribution for those of a similar age

2. The probability that a child “reaches the top fifth of the income distribution conditional on her parents’ income quintile”

For children born after the 1986 cohort, who are still in the midst of educational completion that affects later earnings, the authors focus on the correlation between a parent’s income rank and the child’s rate of college attendance.

Using this data, the authors’ findings are consistent with the stability story rather than the narratives of increasing mobility (modernization hypothesis) or decreasing mobility (reproduction hypothesis). As the authors summarize:

These rank-based measures of intergenerational mobility have not changed significantly over time. For example, the probability that a child reaches the top fifth of the income distribution given parents in the bottom fifth is 8.4% for children born in 1971, compared with 9.0% for those born in 1986. Children born in the highest-income families in 1984 were 74.5 percentage points more likely to attend college than those from the lowest-income families.
The corresponding gap for children born in 1993 is 69.2 percentage points (p. 1).

How do the authors interpret this stable degree of intergenerational mobility (that remains stable at low levels), especially in light of fellow economists’ *Great Gatsby* hypothesis that higher levels of *societal inequality* (which the authors do confirm in their data) will lead to a *decrease in* (rather than stability of) intergenerational mobility?

The authors’ interpretation takes us back to the contrast between the stylized trends in inequality we reviewed in *Week Two*—growth in 50-10 and 90-50 inequality—and the stylized trend in inequality we reviewed in *Week Three*—growth in inequality at the very upper tails of the income distribution. The authors note that when they look at the relationship between U.S. income inequality and U.S. mobility across areas (defined as census tracts), they find a strong relationship between increased inequality *when one restricts the income distribution to the lower 99%* and decreased mobility (p. 11). In contrast, they find no relationship between increases in inequality in the “extreme upper tail” (the top 1%) and decreases in mobility. As they put it, “the correlation between inequality and mobility is driven primarily by ‘middle class’ inequality,” meaning that the trends in upper-tail inequality in recent decades have not yet led to substantial declines in overall intergenerational mobility. And returning to the readings in *Week Four* on regional inequality, they argue that the most striking story amidst this stability is how the *level* of mobility one experiences is strongly related to one’s area of residence within the U.S..

There are at least three ways to interpret the *stability* in intergenerational mobility that *Chetty et al.* (2014) document. One lens is optimism that mobility is stable despite rising income inequality: the negative consequences of the “supermanager” dynamics that Piketty and others show contribute to top-end inequality affect *levels of income* but not how closely a child’s income is tied to that of his or her parents. However, another lens is pessimism given that the time period that *Chetty et al.* (2014) study—births between the early 1970’s and 1990’s—has been marked by a variety of social policies aimed at increasing mobility by intervening on important institutions like schools and labor markets. From early childhood education programs like Head Start to anti-discrimination policies that target workplaces, the fact that mobility has not increased amidst these policy interventions might lead to pessimism about the role of institutions in increasing social mobility.

The third lens is confusion that mobility has not *decreased* given evidence that socioeconomic gaps in the main input to mobility outcomes—educational achievement—have *increased* during this time period. As *Chetty et al.* (2014) themselves note:

> The stability of intergenerational mobility is perhaps more surprising in light of evidence that socio-economic gaps in early indicators of success like test scores (Reardon, 2011), parental inputs (Ramey and Ramey, 2010), and social connectedness (Putnam, Frederick, and Snellman, 2012) have grown over time...An important question for future research is why a such a plunge in mobility has not occurred (p. 11, emphasis added)

Turning now to *Reardon’s* (2011) in-depth overview of these growing socioeconomic gaps in the early indicators of future mobility, the finding of stability in mobility is

---

7 Given links between race/ethnicity and parental income, these might be expected to have an effect on intergenerational mobility.
more puzzling. As he summarizes, the achievement gap between children from high and low-income families is “roughly 30 to 40 percent larger among children born in 2001 than among children born 25 years earlier” (p. 91). Reardon operationalizes the two key constructs—1) the gap between high and low-income families and 2) academic achievement—in various ways and finds stable patterns. For instance, across a range of test reading test scores, there has been a consistent increase in the gap between children born to parents in the 90th income percentile and children born to parents in the 10th income percentile from the 1940 to 2001 birth cohorts. Similar patterns hold for gaps in math test scores across similar cohorts.

Reardon argues that one important way to contextualize whether the 1) size of the gaps and 2) increase in that size across birth cohorts is substantively meaningful is to compare it to black-white gaps in similar tests. There, a different story more consistent with the goals of social policy interventions emerges: the black-white gap in academic achievement narrowed by roughly one-half a standard deviation for birth cohorts from 1940 to the 1970’s and then remained relatively constant. This narrowing of black-white gaps during this period, and widening of 90-10 gaps, lead to what Reardon calls a “striking” crossover effect where the black-white achievement gap went from being substantially larger than the 90-10 achievement gap to being eclipsed in size by the 90-10 achievement gap. While this represents the success of legal and policy efforts such as de-segregation and efforts to increase resources for predominantly minority schools, questions remain about why the same policies have not also reduced socioeconomic gaps in mobility inputs.

Questions to think about:

1. What are your candidate explanations for the “puzzle” identified by Chetty et al. and Reardon of no decreases in intergenerational mobility despite growing socioeconomic gaps in elementary and middle school-level academic achievement?

2. Breen and Jonsson note that although trends in U.S. intergenerational mobility show few changes across recent cohorts, the level of intergenerational mobility in the U.S. remains low compared to some Continental European countries. Which way of measuring mobility—longitudinally or cross-sectionally—do you think is more effective for informing policy interventions?
Week Seven: “Pre-Institutional” Inputs to Mobility: Genetics, Early Language Exposure, Family Structure

- Chapter 1- 'Molecular Me'
- Chapter 2- 'The Durability of Heritability'
- Chapter 4- 'Genetic Sorting and Cavorting in American Society'


- Introduction
- Chapter 5- 'Labor of Love'

---

**Week Six** provided evidence that while U.S. income inequality has increased in recent decades, the (low) level of intergenerational mobility in both income and educational attainment has remained largely stable across birth cohorts since the 1970’s. One more pessimistic gloss on these findings is that the *failure to increase* intergenerational mobility represents a failure of various institutional practices meant to increase this mobility. **Week Eight** and **Week Nine** focus on these institutional practices in two major sets of organizations meant to serve as meritocratic engines of mobility: schools and labor markets. The weeks highlight factors that can make these organizations *stalled* rather than *effective* engines of mobility. In the present Week, however, we turn to what I call three “pre-institutional” inputs to intergenerational mobility: genetics, family structure, and early language exposure.

When I label these three facets of a child’s life “pre-institutional,” what do I mean? Here, it is helpful to distinguish between arguing that an input to mobility is *pre-institutional* versus *a-institutional*. Genetics, family structure, and early language exposure are not *a-institutional*: public policies incentivizing marriage, for instance, can shape family structure; likewise, home visiting programs that begin shortly after birth can influence a child’s early language exposure and development.

What I mean is that these inputs to or constraints upon mobility affect individuals before they enter the formal institutions that are the typical targets of public policy. Thus, the argument is not that *no institutions* affect these early exposures, but instead that these exposures occur within an informal institution—the family—prior a child’s entry into more formal and more highly regulated schooling and labor market institutions.
**Topic one: genetics (Conley and Fletcher, 2017)**

Conley and Fletcher (2017) begin by noting valid reasons for trepidation among social scientists about whether to study genetic contributions to inequality. Chief among these reasons is the **naturalization** argument. If differences in educational and income attainment can be partially attributed to genetics in a way that makes it unfeasible that society will be able to dramatically reduce intergenerational correlations in these outcomes, inequalities in those outcomes “may be argued to be natural, unchangeable, and thus outside the realm of policy intervention” (p. 2).

They cite Herrnstein and Murray’s *Bell Curve* as an example of the move from estimating genetic contributions to educational and income attainment to arguing against “social policy to promote equal opportunity” (p. 2). More specifically, those authors argued that thanks to the triumph of various *mobility-producing* policies, many of which we will review in **Week Eight** and **Week Nine** of the course—the role of the GI Bill and Pell Grants in the expansion of higher education; the rise in homeownership due to policies enacted by the Federal Housing Administration—the U.S. had transformed from a society with low social mobility to higher social mobility. In this higher social mobility society, assortative mating occurred not based on class and other noisy signals for underlying ability/intelligence, but on intelligence itself as high-intelligence persons met in newly-meritocratic institutions. Herrnstein and Murray argued that the result was **genetic stratification**: intergenerational mobility would return to low levels because the constraint on mobility was no longer race and class-biased organizations that public policies could target, but instead individuals’ genetic makeup that could not serve as policy targets.

Conley and Fletcher argue that while it would be easy to dismiss this argument, economists and others were developing more rigorous causal analyses that suggested that the influence of ‘environment’ on a child’s income and educational attainment (and thus their mobility relative to their social origins) had perhaps been overstated. Summarizing Susan Mayer’s *What Money Can’t Buy*, the authors argue that researchers were running into unmeasured/unobserved aspects of a family that seemed to influence the educational and income attainment of their children and that could be classified as **pre-institutional inputs**. Conley and Fletcher see three plausible candidates for these **pre-institutional** characteristics of a family that constrain mobility:

1. **Candidate one- cultural transmission that affects productivity**: these include knowledge about how to navigate institutions, difficult-to-measure skills like impulse control, or often-unmeasured exposure to complex language/cognitive stimulation (the focus of **Topic Two** below). This creates hope for mobility-focused policies because although these “cultural practices” are more challenging targets for policy interventions, they are still in theory mutable.

2. **Candidate two- cultural transmission that does not affect actual productivity but does affect perceived productivity through processes of discrimination**: public policies can increase mobility by targeting these through affirmative action and other programs meant to undo effects of discrimination.

3. **Candidate three- shared genetics**: while this does not need to lead to the abolishment of mobility-promoting policies, it does suggest that policies may need to be renewed in each generation.
The authors argue that all three factors likely play a role, and when investigating the contribution of genetics, our interest is in: how much of educational attainment (or income attainment) is heritable through genetics rather than shared family environment?

Arguing that the answer is substantially greater than zero—with current estimates in the range of explaining between 10 and 15% of the variation in attaining a B.A.—they also argue that there is interesting between-group variation in the relative degree of genetic heritability of different outcomes and that this variation contributes to our understanding of mobility. For instance, summarizing findings by other researchers that shows lower heritability of IQ for self-reported Black Americans than Whites, they note that one interpretation is that “environmental conditions—such as a lack of parental resources, poor schooling conditions, or simply racism—prevent the full realization of genetic variation in this population...potential intellectual ability is inherited but requires environmental conditions of human capital investment to be realized in the form of IQ” (p. 33).

Thus, understanding the contribution of genetics as a ‘pre-institutional’ input to mobility need not undermine the role that the school and labor market policies we discuss in later Weeks can play. Instead, the analysis of genetic contributions can help researchers understand where a lack of those institutional inputs to mobility might constrain the educational and earnings potential of disadvantaged individuals.

**Topic two: early language exposure (Fernald, Marchman, and Weisleder, 2013)**

Conley and Fletcher argue that just as genetic transmissions from parents to child that may affect mobility are not traditionally measured in most survey research, exposure to a certain quantity and quality of language in early months of years may affect mobility and also often go unobserved in traditional surveys used to measure intergenerational mobility. The idea that differences in early language exposure may serve as one mechanism that constrains the mobility of children born to lower SES parents first came to public prominence with Hart and Risley’s 1995 publication that found that higher-SES children in the sample spoke over twice as many words by age 3 as lower-SES children, and that the mechanism producing these differences in a child’s complexity of vocabulary were SES disparities in the amount of child-directed speech from the child’s parents. In turn, they and later authors found that these early gaps in vocabulary were highly predictive of the later socioeconomic gaps in academic achievement that Reardon (2011) from [Week Six] discusses, gaps in academic achievement that constrain the future mobility of lower-SES individuals.

Fernald et al. (2013) build upon this research to address the question: how early in a child’s life do socioeconomic gaps in language development emerge? And just as Conley and Fletcher note that social scientists understandably approached research on genetic contributions to social stratification with trepidation, Fernald et al. argue that there was understandable trepidation to “pursue the question of early origins of SES-related disparities in cognitive skills that are relevant to school success” (p. 3) due to worries that evidence of early gaps would undermine schooling-focused public policies.

Using a measure of language development and processing adapted to younger children—a looking-while-listening (LWL) procedure that assesses vocabulary by measuring a child’s

---

response to visual stimuli—Fernald and co-authors find two main patterns. First is that there are SES gaps in language proficiency by the age of 18 months, and there is a significant SES × age interaction where the gaps increase in magnitude as children get older. The authors note that their study is agnostic on mechanisms by which lower SES is associated with lower language attainment in the 1.5-2 year olds, and could range from factors like the “physical conditions” of daily life in poverty such as “safety, sanitation, noise level, and exposure to toxins” (p. 11) to higher stress among lower SES parents that reduces the quality of parent-child interactions.

The authors ultimately conclude that while choices like how a parent interacts with his or her child are often beyond the purview of public policy—thus, my inclusion of this research in the week on “pre-institutional” inputs to mobility—that one could frame the findings as a “public health message...with the goal of helping caregivers understand the crucial role they can play in enabling infants to build and strengthen skills essential for optimal development” (p. 13). In other words, if SES differences in language exposure serve as one constraint on the mobility of lower-SES children, and if there is a reluctance to set up formal institutions that would interfere in parent-child interactions in a child’s early months to prevent these disparities from developing, policies could encourage behavioral change within the informal institution of the family.

**Topic three: family structure (Edin and Kefalas, 2005)**

Many of the mechanisms that Fernald et al. discuss as potentially lying behind variability in a child’s early language exposure and development relate to stressors associated with poverty, or as they put it, to “stress and instability in disadvantaged families” (p. 11). One source of instability correlated with socioeconomic status can be family structures beyond a two-parent household. Research on the relationship between family structure and outcomes like intergenerational mobility has to disentangle between two factors:

1. **Family structure effects**: The effect of family structure—e.g., being born to and predominantly raised by a single parent—on outcomes like educational attainment that serve as markers of intergenerational mobility

2. **Family resource effects**: The effect of income that is correlated with family structure, with higher SES individuals less likely to enter into certain family structures like single parenthood; in addition, two-parent households can pool resources in ways advantageous for child outcomes

Edin and Kefalas (2005), noting the difficulty of disentangling family structure effects from family resource effects, nevertheless argue that there are independent effects of family structure on child outcomes that serve as markers of mobility. The authors conduct in-depth qualitative research on why lower-income mothers view childbirth as a “necessity” but marriage as a “luxury” that they aspire to but fear is out of reach (p. 6). Motivating this research, they argue that even when family resource effects are adequately controlled for, explaining about “half of the disadvantage” between children raised in single-parent versus two-parent households, family structure has an independent effect on child outcomes:

Though many single mothers are admirable parents, it remains true that, on average, children raised outside of marriage typically learn less in school, are
more likely to have children while they are teens, are less likely to graduate from high school and enroll in college, and have more trouble finding jobs as adults (p. 3)

Family structure is included along with other pre-institutional inputs that constrain mobility because as Edin and Kefalas note, policies especially during the Bush administration have tried to ‘promote marriage’ and reverse the increasingly low prevalence of marriage among low-income individuals across racial/ethnic categories. Underlying these policies is the idea that lower-income individuals have lost interest in marriage, and that policy efforts to strengthen that informal institution can thus have a significant effect. Yet Edin and Kefalas find “astonishingly little evidence of the much-touted rejection of the institution of marriage among the poor” (p. 6). Given that lower-income mothers want to pursue childbirth in a two-parent household more advantageous for child’s inter-generational mobility, and given various barriers to realizing this aspiration, the role of family structure in constraining mobility can appear resistant to formal intervention by policies and organizations.

Edin and Kefalas’ research, however, sheds lights on how policies might mitigate some of the consequences of raising a child in a more challenging single-parent environment. As they note, given the concentration of single-parent families in communities with fewer resources, there are important interactions between a parent’s own type of investments in children and the quality of institutions in the surrounding community. For middle-class parents who often raise children in two-parent households, community institutions supplement the positive investments of time and attention they put into their children. In contrast, lower-income single parents must allocate this same time and attention to protecting their children from negative influences within the neighborhood. As the authors describe:

Most middle-class youths are raised in communities with a wide array of valuable resources that are often taken for granted: good schools, plentiful opportunities for after-school and summer enrichment, and peers who graduate from high school, go to college, and establish careers before they marry and have children. Thus, for middle-class parents, the community supports and extends their efforts to parent their children well. For mothers living in impoverished, inner-city communities, however, the neighborhood is often the greatest impediment to their aspirations for their children (emphasis added, p. 149).

Thus, while family structure might be beyond the purview of many institutional interventions, efforts like mixed-income housing, anti-gang interventions, and other measures that change neighborhoods, can help single parents allocate time and energy investments into promoting their child’s development in ways that facilitate mobility rather than spend this same time and energy guarding their child from neighborhood threats.

Questions to think about on this topic:

1. Conley and Fletcher: In biomedical research studies that conduct genetic sequencing, researchers debate an ethical obligation to return incidental findings—things like a mutation that the research is not explicitly designed to study but that the patient might want to know. As large surveys incorporate genetic sequencing and construct individual-specific, polygenic scores for social outcomes like
educational attainment, should researchers disclose these scores to participants? What might be drawbacks in terms of phenomena like *stereotype threat*, *self-fulfilling prophecies*, and other theories that focus on how *thinking* one is at risk for a bad outcome might bring that outcome into being?

2. **Fernald et al.**: we can think of two classes of policies to increase the linguistic stimulation that lower-SES children receive. One are *child-directed* policies that increase access to more linguistically rich environments through programs like early childhood education or home visiting. Another are *parent-directed* policies that, for instance, increase the educational attainment of a child’s caregivers. What are benefits and drawbacks of each type of policy?

3. **Edin and Kefalas**: when motivating their study, the authors argue that single-parent family structure has an effect that is independent from the mother SES with which it is correlated. Yet the examples they draw upon—for instance, the quote above—sometimes mix mechanisms that are SES-based (e.g., exposure to dangerous neighborhoods) with mechanisms that are family structure-based. Given how closely the two are correlated, what are benefits and drawbacks of trying to isolate an “independent” effect of family structure? What empirical designs might you leverage for such an analysis?
Week Eight: Educational Institutions as a Stalled Engine of Mobility

- Introduction
- Chapter 4- 'Court Interventions in School Finance'

- Chapter 2- 'School Finance Reform and Educational Ideology: A Guide to Law, Politics, and Policy'


- Introduction
- Chapter 18- 'The Battle over Merit'


While the previous week reviewed what I call “pre-institutional inputs” that constrain mobility, the hope of social policy is that, for instance, the gaps in vocabulary that Fernald documents in early life can be reduced through high-quality public elementary/secondary education in ways that facilitate mobility. In turn, given the trends in income inequality documented in Weeks one through Three that increasingly favor high-skilled workers with a college degree, a range of policies frame higher education as an important engine of mobility. The present week focuses both on the *ideals* of these policies as well as reasons they may fall short in ways that contribute to the stalled mobility that *Week Six* documents.
Public elementary and secondary schools are one of the main institutions focused on breaking the link between ascriptive characteristics and mobility, and the extent to which they can achieve this aim depends in part on how much the financing a school receives to spend on pupils is linked to or de-linked from the ascriptive characteristics of local parents. The readings in this section document two institutions social movements use to accomplish that de-linking: legislative changes (“statehouses”) and legal challenges (“courthouses”). Each of these strategies relates to the findings from Versteeg and Zackin (2014) in Week Four. While the U.S. federal constitution does not contain positive rights regarding education, many U.S. state constitutions do, and these state provisions have served as useful and durable tools for activists to leverage in litigation to break the link between parent SES and the quality of a child’s public education.

Paris (2010) begins with a puzzle: at a time of “general retrenchment in most other areas of social provision” (p. 35), why have state and local expenditures on education continued to increase? As he puts it, the bipartisan ideology behind support for education is rooted in its depiction as an important engine of intergenerational mobility:

In this ideology, the role of government is often analogized to that of an umpire ensuring a fair game to make the pursuit of success possible for everyone. Within this view, public education easily becomes a primary vehicle for bringing each individual up to the starting line in the race for the good things in life and for securing the conditions of fair competition (p. 36)

Paris argues that school finance litigation is rooted in the notion that public education provides this fair starting line. Reformers argue that in order for that line to be in the same place for all students, schools should either receive equal per-pupil financing de-linked from surrounding wealth or more disadvantaged schools should receive higher/compensatory financing on account of their students’ greater disadvantage.

Meanwhile, Hanushek and Lindseth (2009) frame school financing as an issue of both national competitiveness—the U.S.’s relatively low rank in reading and math proficiency in international rankings based on common tests—and intergenerational mobility, as gaps based on various ascriptive characteristics—race; socioeconomic status—have either persisted (in the case of race) or grown (in the case of socioeconomic status, and as documented more fully by Reardon (2011) in Week Six).

Reardon (2011) documented two distinct patterns in how educational achievement—achievement that is important for intergenerational mobility—has changed for students with certain ascriptive characteristics. The first pattern was a narrowing gap in academic achievement between Black and White students in the 1940’s through 1970’s/1980’s birth cohorts, with some stalling of that narrowing in more recent cohorts. And socioeconomic-directed school financing litigation had its roots in the race-directed school de-segregation litigation that is arguably responsible for the dramatic narrowing of that racial gap. Hanushek and Lindseth document how the first wave of school finance-related litigation played out in the federal court system as an offshoot of changes to what sort of remedies plaintiffs could seek in school de-segregation cases.
black states/cities, even extensive busing of White students into Black districts would still leave districts/schools majority Black. Second was that in locations where busing could have a significant impact, the standards for the court mandating busing within a district were stringent and required “proof of complicity of the suburban districts in causing the segregated schools in the central city” (p. 85).

For these two reasons, activists pursuing litigation turned to financing as a tool for either reducing the presence or mitigating the effects of segregated schools. They argued that increased financing to fund higher-quality teachers, smaller classes, and enrichment programs could both make Black schools more attractive for White students to voluntarily attend (reduce the presence of segregation). In addition, increased financing could remedy some of the consequences of segregation’s history for schools/districts that remained majority Black (p. 86). Thus, the first wave of school finance litigation was motivated by the desire to increase intergenerational mobility based on the ascriptive characteristic of race. But the litigation’s focus on per-pupil financing created spillover effects for a different ascriptive characteristic: socioeconomic status.

While the first wave of school finance litigation played out in federal courts as part of de-segregation efforts, developments in case law made a federal-focused strategy untenable. While activists initially tried to ground cases concerning between-district funding disparities in the 14th amendment’s equal protection clause, the Supreme Court ruled in Rodriguez v. San Antonio, 1973 that education was a matter left to the states and not a right guaranteed by the U.S. constitution. Activists thus shifted the venue of their legal battles to state courts and in doing so, shifted the focus of these battles from race to socioeconomic status.

Litigation over school financing in state courts played out in two waves, with each wave drawing upon arguments regarding the importance of schools for mobility. The first wave of cases activists filed in state courts used equity arguments; using rights to education outlined in individual states’ constitutions, activists argued that the large funding inequities between districts with more versus less local taxable wealth prevented the realization of this right for poorer students. While those behind the suits hoped that legislatures would engage in leveling up in response—increase the funding for poorer districts to match that of wealthier districts—some of the rulings ended up undermining taxpayer incentives to support any schools due to this redistribution. Activists thus shifted to another wave of state court cases grounded in adequacy arguments: courts “decide whether the state appropriations for K-12 education are sufficient to pay for an ‘adequate’ system, and if not, how much more funding is needed” (p. 96), and in some cases, not only re-distribute between districts but re-distribute from other expenditures to school-directed expenditures in state budgets.

These continued legal battles, focused on competing notions of how a school’s level of per-pupil expenditures does or does not provide a fair starting line, have complicated consequences for mobility. On the one hand, one can imagine clear negative consequences of under-resourced schools in terms of teacher quality, class size, and other “inputs” to education. But as Hanushek and Lindseth argue, funding levels are a blunt instrument for measuring school quality, and the authors ultimately recommend a shift from more unconditional funding formulas to funding formulas more conditional on school performance.
**Questions to think about on this topic:**

1. **Paris** notes the strong ideological roots of debates over school financing. These strong roots mean that researchers and policymakers who criticize the impact of school finance-focused litigation often do so by disagreeing on *empirical* rather than *ideological grounds*. They argue not with the mobility-focused justification but instead that financing serves as a blunt/ineffective tool to change academic performance: **Paris** summarizes, that “variations in several school inputs do not now have any systematic relationship to measurable student performance or outcomes” (p. 48). How might litigation, if shifting away from a focus on financing only, incorporate empirical evidence on the impact of various inputs to student performance?

2. Both authors highlight the “courthouse” versus “statehouse” dynamics of school funding. From the standpoint of inequality, which institution might serve as a more receptive venue for grievances articulated by lower-SES individuals? Which institution might produce more *durable* distributional changes?

**Topic two: two forms of horizontal stratification in higher education**

The quality of the public elementary and secondary schools a child attends, which is impacted by how resources are distributed between and within school districts, matters for outcomes such as whether a student graduates from high school. After high school graduation, a range of mobility-focused policies focus on promoting college enrollment, retention, and completion.

As the readings in [Week Two] by **Autor et al.** and **Brynjolfsson and McAfee** underscored, even the “less institutional” theory of job polarization contains an implicit focus on how college as an institution affects trends in income inequality. For instance, job polarization argues that jobs found in the “abstract reasoning” category are complemented rather than substituted for by new forms of computer technology, and that these jobs often require a college degree. The mobility question then becomes: which laws and institutions weaken the link between ascriptive characteristics—race, gender, parent SES and educational levels—and a person’s probability of college completion?

Public elementary and secondary schools are one important set of institutions, since the quality of ones elementary/secondary education affects both ones probability of graduating from high school graduation and ones ability to keep pace with college coursework. Another set of important institutions that I focus on in the present section try to make college more affordable for persons from a wide range of family backgrounds. While historically the G.I. Bill was an important driver of the post-WWII increase in college entry and graduation, important for more contemporary mobility are federal loans provided under Title IV of 1965s Higher Education Act (HEA) and federal Pell grants under the 1972 HEA amendments directed towards students with financial need. These uniform federal efforts were supplemented by state efforts such as budget allocations to public university systems to keep tuition costs low in ways that increase access and mobility.

Yet as **Gerber and Cheung (2008)** point out, while one form of stratification in educational attainment is *vertical*—which individuals do or do not receive a college degree?—as federal policies expand access in general to higher education, another form of stratification has emerged: horizontal stratification. Using **Gerber and Cheung**'s
definition of horizontal stratification as “the different types or quality of education received at a particular level” (p. 300), the present topic focuses on two forms of horizontal stratification that impact mobility into different areas of the U.S. income distribution.

Form one: between-institution differences in quality (Deming, Goldin, and Katz, 2012)

The first form of horizontal stratification maps on to the trends in income inequality covered in Week Two: growth in inequality between the 50th and 10th income percentiles followed by growth in inequality between the 90th and 50th percentiles. Focusing on differences in quality between institutions that students attend, I review research showing how the rise of for-profit colleges that leave students with significantly fewer post-graduation opportunities and significantly more post-graduation debt than at more traditional universities weakens the value of a college degree as an engine of mobility.

Amidst the post-1970’s rise in inequality that led economists to argue that college degrees were increasingly valuable, there were major changes to the institutional landscape of U.S. higher education. In particular, the for-profit postsecondary school sector became the fastest growing part of U.S. higher education from the 1990s to 2010 (Deming, Goldin, and Katz, 2012, p. 161). These institutions are important to examine for mobility purposes because they disproportionately serve those most in need of upward mobility: students with low incomes, minority students (African-American and Hispanic), and older students returning to complete a college education. Likewise, these institutions are important to examine from the perspective of the role of law and institutions because federal student aid plays a substantial part in financing these institutions: for instance, 95% of students in Title IV-eligible for-profit colleges receive Pell grants compared to 27% of students in public community colleges, and federal grants and loans account for 73.7% of the revenue of for-profit higher education institutions (Deming, Goldin, and Katz, 2012). As a result, government statutes and enforcement of those statutes play a role in the existence and practices of for-profit institutions that enroll students most in need of mobility, making the institutions an important case foreshadowing the Week Eleven and Week Twelve readings on how differential enforcement of statutes mitigates or exacerbates inequality.

Do for-profit colleges serve as this engine for mobility? Deming et al. (2012) ask whether for-profit colleges are what they call nimble critters—unbound by stagnant degree programs, the institutions might “train nontraditional learners for jobs in fast-growing areas, such as health care or information technology” (p. 143)? Or, are the institutions what they call agile predators, engaging in aggressive recruiting techniques that lead students to enroll without an accurate perception of the institution’s costs and benefits?

The authors follow and compare outcomes for a cohort of enrollees in for-profit colleges relative to demographically and academically similar students who attended community, public, and private nonprofit colleges. Deming et al., while trying to control for characteristics that stem from the fact that for-profit colleges serve a significantly more disadvantaged student population by matching each ‘treated’ for-profit college student with demographically similar control(s), find a range of negative outcomes. While they find that for-profit colleges help students obtain an Associates degree or certificate at a faster rate than community/nonprofit colleges, enrollees face significantly worse debt-related outcomes—significantly higher loan default rates and higher debt burdens—and significantly worse employment-related outcomes: higher rates of ‘idleness’ (neither
being in school nor working), results that hold when for-profit enrollees are only compared to students at nonselective/open admissions public/private institutions. Thus, the same B.A. has significantly different labor-market value depending on the institution that grants the B.A.

Meanwhile, other research using I.R.S. tax data that we are not reading for the course using student-level fixed effects—comparing a student to him or herself prior to entering a for-profit college since most work full-time before college entry—to show that certificate, associate’s, and bachelor degree candidates experience declines in earnings in the five to six years after attending a for-profit college, largely due to non-completion of the degree (Cellini and Turner, 2016).

In conjunction, these two studies shed light on how job polarization theories from Week Two which emphasized increasing returns to college education, play out in an organizational landscape of highly uneven institutional quality. In this uneven landscape, attending some colleges leaves one worse off earnings-wise than not attending college at all. Yet Deming et al. also highlight that the growth in the for-profit college sector is not a solely market-driven, a-institutional phenomenon. Instead, two sets of policies operating at different levels of government—state cutbacks to public university systems; federal loans with minimal standards for where the student uses the loans—have contributed to its growth.

Questions to think about on Topic One:

1. MacKenzie and others within the sociology of science discuss different forms of performativity, or how social science ideas come to affect the world they seek to describe. How might economic theories about the returns to a college education affect the college-attending behavior of various individuals? How might these theories be used by lower-quality colleges to recruit students who, by experiencing worse post-graduation outcomes than students who do not attend college at all, then serve as outliers to the theory’s predictions?

2. Deming et al. document the heavy reliance of the for-profit college sector on federally-funded student loans. What does this reliance reveal about unintended consequences, where a policy meant to increase mobility—loans that make college attendance accessible to a wide range of students—has unintended consequences? What changes to the policy might guard against these consequences?

Form two: within-institution differences in experiences (Karabel, 2006; Jack, 2016)

For-profit colleges are relevant for intergenerational mobility from and to a particular segment of the income distribution: mobility usually from the lower segments of the income distribution into middle percentiles. The second form of horizontal stratification maps on to the trends in income inequality covered in Week Three: growth in very top-end inequality and the rise of the individuals Piketty labels supermanagers commanding high remuneration within the top 1 and top 0.1 percent. To understand which individuals experience mobility into this class of supermanagers, working in fields

---

like financial services, elite law firms, and other professional occupations, we turn to the elite universities that Karabel (2005) describes as the Big Three: Harvard, Yale, and Princeton.

Karabel (2005), focusing on the history of admissions practices at the Big Three, argues that the institutions have long framed themselves as engines for intergenerational mobility. He describes how leaders of the institutions distinguished between two notions of equality, with the first notion mapping onto our readings on intergenerational mobility in Weeks Five through Nine, and the second notion mapping onto our readings on inequality in Weeks One through Four:

1. *Equality of opportunity*: the “principle that an individual’s chance to get ahead should not be limited by social origins or ascribed characteristics such as race or gender” (p. 4)

2. *Equality of condition*: the idea that society should minimize any inequalities of wealth, status, or power

The leaders of the Big Three endorsed ideas about *equality of opportunity* but largely rejected ideas of *equality of condition*; they believed the institutions should select individuals based on *merit* regardless of social origins/ascribed characteristics and in doing so, facilitate upward mobility for these individuals. As long as individuals had equal access to high wealth and status, the existence of this high wealth and status was not problematic in and of itself.

The main focus of Karabel’s analysis is on how the concept of merit mutated to suit different social climates and priorities. First the Big Three shifted from a pre-1920’s focus on academic achievement and mastery of the classics towards the idea of a man of all-around good character that resulted in lower rates of admissions of Jews amidst a rise in nationalism/anti-Semitism. Then, in the wake of the Cold War and events like Sputnik, the universities’ notion of merit morphed into the idea of intellectually gifted individuals who displayed excellence in at least one other domain. During the 1960’s and 1970’s, in response to civil rights, feminist, and other social movements, merit expanded to include the idea of women and racial minorities, with affirmative action put in place to address hardship and past discrimination minorities had experienced.

Karabel notes that amidst this increased racial, ethnic, and religious diversity, at the turn of the 2000’s, one gap in diversity remained: socioeconomic diversity. As he puts it:

Yet beneath this dramatic and highly visible change in the physiognomy of the student body was a surprising degree of stability in one crucial regard—the privileged class origins of students at the Big Three (p. 536)

Though reporting of socioeconomic attributes of a college’s student population is considerably more sparse than reporting of racial attributes, Karabel notes various statistics to support this claim of a lack of Big Three socioeconomic diversity at the time: at Princeton, students from the bottom 50% of the U.S. income distribution made up fewer than 10% of incoming students, while at Yale, fewer than 8% of students were the first in their families to attend college. This lack of socioeconomic diversity has implications for mobility into the class of *supermanagers* that are drawn from elite universities through the recruiting practices that Rivera (2016) in **Week Nine** will document. While these supermanagers compose a small part of the income distribution *numerically*, their large share of U.S. income and wealth makes it important to examine how an individual’s social
origins affect their trajectory into this class.

In the years since Karabel’s writing, the Big Three and other elite universities have made concerted efforts to increase socioeconomic diversity in ways that could promote upward mobility among lower-SES students. For instance, the percentage of incoming Princeton students receiving Pell Grants has increased from 7% of the Class of 2008 to 21% of the Class of 2020, and Princeton’s president frames this increase as important for intergenerational mobility:

> Research consistently shows that a college education is the single most powerful factor for enabling young people to achieve social mobility. A Princeton education is a rocket-booster for talented students from low-income backgrounds.

As Eisgruber notes, increasing the amount of racial and socioeconomic diversity at elite institutions has the potential to increase intergenerational mobility. And given the nature of post-graduation opportunities, it has the potential to increase mobility into the same upper percentiles of the income distribution that Week Three documents possess a growing share of the U.S. income distribution.

**Jack (2016),** however, documents a form of horizontal stratification that occurs within elite universities. While the form of horizontal stratification that for-profit colleges contribute to is stratification between-institutions in the quality of education they provide, the form of horizontal stratification that Jack documents is stratification within-institution in the resources and opportunities that individuals from different class positions are able to extract.

Focusing on a selective East Coast university, Jack finds that “doubly disadvantaged” students—lower-income students who attended “local, often distressed high schools”—struggle to seek help from and form positive relationships with professors and other authority figures at the university, a struggle less present among middle-class and “privileged poor” students (lower-income students who attended elite high schools as part of programs like Prep for Prep). His research shows that students who attend elite universities are academically prepared for its demands—undermining ideas like the mismatch hypothesis advanced in opposition to affirmative action. But these students struggle with cultural aspects of the university experience like a strong sense of individualism and the “expectation to proactively seek adults’ attention to curry favor with them” (p. 10). Though Jack does not follow these students to examine longer-term professional outcomes, he reports that some, for instance, abandon career goals such as medicine that could promote upward mobility because of challenges securing the necessary recommendation letters and faculty support. This shows that despite a new expansion in merit to now include socioeconomic diversity, gradients in the experience at these universities might affect class-related mobility.

**Questions to think about on this topic:**

1. Karabel notes the contrast between ideas about equality of opportunity and ideas about equality of condition. What form of equality does the current rhetoric surrounding socioeconomic diversity at elite institutions focus on? Do you agree with

---

Karabel’s argument that an emphasis on equality of opportunity can decrease political/social will towards more important efforts aimed at equality of condition?
Week Nine: Labor Market Institutions as a Stalled Engine of Mobility


• Chapter 1- ‘Entering the Elite’
• Chapter 4- ‘The Paper’
• Chapter 7- ‘Continuing the Interview: The Candidate’s Story’

After exiting the schooling system that Week Eight focuses on, students encounter a labor market. The readings in this Week focus on two facets of how that labor market may facilitate or constrain intergenerational mobility in earnings. Topic one contains readings that focus on the passage and implementation of laws that seek to weaken the link between the ascriptive characteristics of race and gender and labor market outcomes like hiring and salary determination.

Yet since the main metric of intergenerational mobility is mobility based on socioeconomic status, Topic two focuses on how amidst these formal anti-discrimination statutes, discretion in elite professional service firms’ recruiting practices mean that this hiring operates in class-biased ways that constrain mobility into a particular segment of the income distribution.

Topic one: legal regulation to weaken link between gender/race and labor market outcomes (Kelly and Dobbin, 1998; Hirsch and Kornrich, 2008)

Kelly and Dobbin (1998) document two significant policy measures passed during the Civil Rights era that were meant to weaken the link between ascriptive characteristics and labor market outcomes and in doing so, promote mobility. First was an executive order by President Kennedy that required federal contractors to use “affirmative action” measure to end employment discrimination. Second was Title VII of the 1964 Civil Rights Act, which prohibits employers from discrimination on the basis of race or sex and which empowers a federal administrative agency, the Equal Employment Opportunity Commission (EEOC), to enforce compliance. Relating to Hacker from Week Five’s distinction between retrenchment of policies through formal revision versus retrenchment through more subterranean adjustments to a policy’s implementation, the authors address the puzzle: when Reagan’s administration vowed to “curtail enforcement of anti-discrimination
laws” (p. 961), why did employers not end their own anti-discrimination efforts? The authors argue that to understand why employers maintained anti-discrimination/affirmative action programs in the wake of Executive Branch retrenchment, one should place this 1980’s change within distinct stages of employer responses to affirmative action (AA) and equal opportunity (EEO) law. The first stage in the 1960’s to early 1970’s was weak compliance amidst ambiguous guidelines. Although formal statutes had been passed, few employers established AA/EEO focused positions or internal policies. Second, the period from 1972 to 1980 was marked by expanded federal enforcement with the expansion of the EEOC’s powers to initiate enforcement action; this expanded federal enforcement, causing “anxiety among employers,” led to a sharp rise in the percentage of employers who allocated human resources professionals specifically to AA/EEO compliance and that constructed new internal policies.

Why amidst Executive Branch retrenchment on anti-discrimination statutes did these employer practices persist? Kelly and Dobbin argues that there was ‘subterranean’ persistence of these efforts by employers because human resource managers were able to justify the efforts using new arguments. These new arguments no longer drew upon the idea that firms should monitor discrimination in order to comply with federal policy/avoid EEOC crackdowns, since Reagan’s administration significantly reduced the threat of these enforcement actions. Instead, the new arguments drew upon the importance of anti-discrimination efforts for firm efficiency—it is more efficient for firms to clearly formalize hiring and promotion policies—as well as firm performance—a diverse workforce was important for firm profitability due to a diverse customer base. This persistence may have helped mobility by race and gender, serving as an example of a non-stalled engine along these lines.

Kelly and Dobbin’s analysis shows that firm-level anti-discrimination efforts persist even amidst weak enforcement conditions. Yet foreshadowing the readings in Week Eleven and Week Twelve, enforcement of these statutes still matters in order for the statutes to have continued relevance for the regulated entities. And as Hirsch and Kornrich (2008) document, there is widespread variation in “workers’ perceptions of unfairness and their willingness to mobilize their legal rights” (p. 1397). Both of these outcomes—a worker’s perception of unfairness and his or her willingness to mobilize legal rights—might depend on aspects of the organization that go beyond the discriminatory interaction(s) in and of itself. For instance, the authors find that minority and female workers are less likely to file grievances when there is evidence that the firm has engaged in efforts towards equitable employment practices—for instance, if the workplace is diverse with respect to race and gender and if women and minorities are well-represented in top management positions (p. 1424).

These findings might have mixed implications for the extent to which formal anti-discrimination statutes facilitate mobility for females and members of minority groups. On the one hand, the findings show that firms do engage in efforts like promoting females and minorities to positions of top management in ways that facilitate mobility. On the other hand, these efforts may suppress legitimate discrimination claims by facilitating the appearance of equity when that equity is applied to only a select few workers.
Topic two: who is recruited into the ranks of supermanagers? (*Rivera, 2016*)

The federal anti-discrimination laws and workplace policies that Topic one focuses on cover employers whose employees fall along a broad range of the income distribution. As such, they affect mobility into any segment. In contrast, building on the reading from Jack in Week Eight, which focused on how students from different class backgrounds extract different degrees of academic value from the same elite university, *Rivera (2016)* focuses on how students from different class backgrounds extract different degrees of post-graduation value from these universities. By focusing on elite professional services jobs (EPS)—top-tier investment banks, consulting firms, and law firms—that serve as “gateways to the U.S. economic elite,” Rivera highlights the dynamics of hiring into the sorts of jobs that launch individuals into the class of highly-remunerated supermanagers that *Piketty* notes contribute to top-end inequality.

Rivera’s study highlights how among the lower-income students who do make it to elite universities, there are further roadblocks to upward mobility via elite professional careers; the phenomenon she seeks to explain is why, among the pool of students who attend elite universities, “students from the most elite backgrounds tend to get the highest-paying jobs”?

She notes two mechanisms through which EPS hiring practices result in “elite reproduction” rather than upward mobility for lower-income students at elite universities. First is through screens, the sorting criteria that employers apply to the schools they target for recruiting and the résumés they screen. Second is through evaluative metrics, how the individuals making hiring decisions at firms measure a candidate’s potential. Rivera finds that although both screens and evaluative metrics are class-neutral at first glance, the way they operate in practice ends up favoring students who display signals “strongly correlated with parental socioeconomic status.”

For instance, in the résumé screening phase, EPC’s place more weight on exclusive extracurricular activities—and in particular, leisure activities that often required large monetary and time investments by the applicant’s parents during his or her child/teenage years—than on more widely accessible signals like grades and class rank. In the interview phase, the firms prioritized candidates who could tell emotionally rousing “stories” about where they had come from and their future trajectories, and wanted these stories to illustrate extreme dedication. Yet the extreme dedication they favored were narratives about pursuing a dream activity—for instance, going to Cordon Bleu to try one’s hand at becoming a chef after college graduation—rather than “narratives about doggedly working long hours at non-glamorous jobs.”

Thus, while firms used résumé screening and interview procedures that they argued were based on merit, the way these procedures were implemented in practice constrained mobility for lower-SES students at elite universities.

**Questions to think about:**

1. Kelly and Dobbin note the shifting arguments that professional use to justify diversity efforts in labor markets. What role do the ideological notions of “merit” that Karabel discusses in the previous week play in justifying these efforts?

2. Rivera notes constrained mobility into elite professional service careers due to implicit class biases in seemingly class-neutral hiring interactions. How might these
dynamics persist as an individual progresses through her career, with consequences for the “mature” earnings that feed into intergenerational mobility calculations?
Section Three: From Inequality *and the* Law to Inequality *in the* Law
Week Ten: Inequality in Criminal Law


Harris, Alexes. *A Pound of Flesh: Monetary Sanctions as Punishment for the Poor*. *Russell Sage Foundation*, 2016. Focus on:

- Chapter 1- ’The Criminal Justice System and Monetary Sanctions’
- Chapter 5- ’The Punishment Continuum’

The readings in the first nine weeks of the syllabus focus broadly on two forms of inequality and the law: the relationship between law/institutions and income inequality (Weeks One through Five) and the relationship between law/institutions and intergenerational mobility (Weeks Six through Nine).

The remainder of the syllabus focuses on a different relationship: inequality in the law. The present week focuses on inequality within different facets of the criminal justice system, while the two weeks that follow focus on inequality within civil law.

We can position each of the readings along a typical temporal process of criminal justice system involvement. Each stage of this temporal process can be marked by inequalities in the level or consequences of criminal justice system involvement, and in the U.S., the inequalities largely fall along racial lines. As Harris (2016) summarizes, the bulk of the burdens of incarceration fall upon Black men: while 1.8% of non-African American adults have been convicted of a felony, 7.7% of African-American adults have experienced a felony conviction. Citing research by Pettit and Western, she notes that for recent cohorts of African-American men, there is a higher frequency of incarceration than military service or college graduation.

The first stage in the temporal process of criminal justice system involvement is contact with law enforcement officers; as a result, one of the first stages at which racial disparities can emerge is in different racial groups’ likelihood of experiencing this contact despite engaging in similar levels of crime. Gelman et al. (2007), focused on the New York City Police Department (NYPD)’s stop and frisk policy address this question. More specifically, does the count of members of three different ethnic groups—whites, blacks, Hispanics—actually stopped exceed the count that one would predict one would be stopped using two sets of predictors:

1. That ethnic group’s population proportions
2. That ethnic group’s crime rate, operationalized using a lagged measure of arrests (previous year’s arrests)

The authors find that blacks and Hispanics are stopped at roughly twice the rates of whites, a result that controls for that ethnic group’s previous year arrest rates. Since roughly 1 in nine stops leads to an arrest, this shows how racial inequalities in the first
temporal stage of criminal justice system involvement—contact with police—begin to contribute to the racial inequalities in incarceration that Harris reviewed.

Gelman et al. focus on racial inequalities in the level of criminal justice system involvement. And these racial inequalities persist through subsequent temporal stages of criminal justice system involvement—sentencing decisions; probation decisions. The next article shifts to a different form of racial inequality: racial inequalities in the consequences of criminal justice system involvement. Pager (2003) focuses on a consequence particularly important for economic inequality: how the ‘mark’ of a criminal record impacts employment outcomes. Using an audit study that sent in-person testers to apply to various low-wage jobs—waitstaff; laborer/warehouse worker; service or sales—Pager finds that the same criminal record has substantially different employment consequences depending on the race—white or black—of the applicant. While a criminal record reduced callback rates for both white applicants and black applicants, there were important interactions between race and incarceration status. In particular, the callback rate for black applicants without a criminal record was 14% compared to 5% for black applicants with a criminal record, a reduction of $\sim 36\%$. Meanwhile, the callback rate for white applicants without a criminal record was 34% compared to 17% for white applicants with a criminal record, a reduction of $\sim 50\%$. Two aspects of this finding are striking: first is that whites with a criminal record had higher callback rates than blacks without a criminal record. Second is that the low absolute callback rate that black applicant have means that even though there is technically a smaller effect of a criminal record on their callback rates ($36\%$ reduction compared to $50\%$ reduction for whites), those with a criminal record approach a negligible callback rate. The fact that the same event—incarceration, and the criminal record it leaves on one’s résumé—has such starkly unequal consequences for black men seeking low-wage work contributes to racial inequality at the bottom of the U.S. income distribution.

Pager highlights racial inequality in one economic consequence of incarceration: employment. Harris (2016) highlights racial and other forms of inequality in a neglected but important economic consequence: monetary sanctions. These monetary sanctions, also known as Legal Financial Obligations (LFOs), are administered to those with criminal justice system involvement for a variety of reasons: fees associated with the trial phase of criminal convictions such as those for DNA processing and court costs, fines involved in restitution for victims, interest paid on those legal debts if payments are not made in a timely manner. Harris documents how these legal debts exacerbate the effect of incarceration on inequality. As she describes, legal debt means that “criminal justice contact continues well beyond the one discrete moment of conviction or the period of incarceration” (p. 12), since legal debts that are almost never paid off while the individual is incarcerated follow that individual as he or she tries to reintegrate into the community and can beget further jailtime or incarceration for non-payment.

The existence of legal debt means the ‘arm’ of incarceration, and the effect it has on a person’s life chances, extends well beyond his or her release from jail or prison. In Chapter five, Harris documents how between-defendant variation in three phases of the assignment of monetary sanctions has additional implications for inequality. First is the phase during which a defendant is sentenced—or where, the amount of fees, fines, and restitution (LFO) is determined. Focusing on variation between Washington State counties, Harris finds that in some counties, the defendant’s race significantly affected the LFO amount, with white defendants assessed lower amounts than non-white defendants who had committed similar crimes and had similar past criminal histories. Other coun-
ties displayed no such patterns. After the sentencing phase comes the monitoring phase, where counties use different methods to classify defendants into three categories: non-payers, compliant with monthly payments (which in some counties, includes exceptions for defendants unable to pay due to being 'indigent'), and paid in full. Finally, among the nonpayers, judges and county clerks exercise significant discretion in determining the sanction such as whether a warrant is issued and whether the individual is jailed for 'willful non-payment.'

In addition to discretion in the sentencing phase that leads to racial disparities in some counties, disparities that Harris documents with quantitative analysis, Harris uses ethnography and qualitative methods to document discretion and resulting inequalities in the monitoring and sanctioning phases. The clearest inequality throughout is by socioeconomic status. Not only are lower SES individuals less able to pay off their legal debts, they also face inequalities in whether, conditional on being brought into court for nonpayment, they face further incarceration as a result. For instance, Harris documents how when defendants are offered a public defender in hearings concerning whether they will face incarceration for nonpayment, many decline on the grounds that they don’t want to pay more of the small fees associated with that defense.

**Questions to think about on this topic:**

1. **Gelman et al.**: why does using previous year’s arrest rates as a proxy for the actual level of crime committed by different racial groups bias the authors towards finding no stop and frisk disparities once these arrest rates are controlled for, making the continued presence of these disparities more surprising?

2. Pager’s study was conducted in a specific geographical context—Milwaukee—with certain racial and political dynamics. Do you think the pattern she found would change based on the geographical location of the audit study? Why or why not?
Week Eleven: Inequality in Civil Law- Attrition in the Dispute Resolution Pyramid


• Chapter 19- 'Little'

Parts I and II touched upon a variety of statutes meant to reduce inequality and improve mobility, many of which were passed during the rights revolution of the 1960s-1970s: various provisions of the Civil Rights Act, the Individuals with Disabilities Education Act (IDEA), rights provisions in state constitutions, the Elementary and Secondary Education Act (ESEA), the Higher Education Act (HEA), Fair Housing Act (FHA) provisions that affect tenants rights, and others. Yet the effect that these formal statutes have on inequality and mobility depends on whether they are enforced, by whom they are enforced, and the outcomes of enforcement. As a result, the federal government uses various methods to both encourage enforcement of statutes such as the IDEA, HEA, and FHA, and to prevent the emergence of disparities in enforcement.

In particular, Kagan argues that features of American culture have meant that enforcement of statutes is often entrusted to a de-centralized array of private actors rather than concentrated in centralized bureaucracies. Kagan argues that there is an ideal typical portrait of the relationship between the legislature, executive, and judiciary branches of government: the legislature and executive branch negotiate over fundamental policy decision and then entrust elaboration of these policies to specialized administrative agencies staffed by experts in a substantive area (e.g., the Department of Education). In this ideal typical portrait, the role of courts is limited to adjudicating disputes between citizens and agencies about narrow matters on the meaning of statutes, and to ensuring that a statute does not violate basic constitutional rights.

Kagan argues that in the U.S., courts play a much larger role in policymaking than in this ideal typical model; instead, Kagan argues that the U.S. is characterized by what he calls adversarial legalism that features the organized use of courts by interest groups as an alternative policy forum for pursuing policy goals, judicial boldness in second guessing administrative agencies, and persistent uncertainty surrounding public policy as interest groups seek to reverse legislative losses via use of the court system and reverse court losses via use of the legislative system. The question Kagan addresses is that if one accepts adversarial legalism as an adequate descriptor of the role of U.S. courts in relation to the legislature, and if one agrees with Kagan’s assessment that adversarial legalism has intensified since the 1960s, what explains this intensification if various features of the U.S. such as federalism have remained constant?

Kagan argues that the intensification of adversarial legalism in the U.S. since the
1960s, as well as its stronger manifestation in the U.S. compared to other advanced industrial democracies, is due to the collision between a new “American political culture” centered on widespread demands for a more activist government and an older, inherited American political culture centered on distrust of centralized political authority. Kagan argues that the collision of these two national political cultures resulted in efforts for new federal norms but citizen demands for litigation to supervise implementation of these norms rather than entrusting centralized bureaucracies with oversight.

What implications does adversarial legalism have for inequality? In the present week and the week that follows, we will discuss how the de-centralized nature of enforcing public policies is accompanied by two types of disparities in this enforcement with implications for inequality. In this week, we focus on the disparity of: who gets stalled versus progresses through what commentators call the “dispute resolution pyramid” of enforcement?

Felstiner, Abel, and Sarats (1980) dispute processing model—sometimes summarized as naming, blaming, claiming—shows how the same event might trigger different enforcement responses in two differently situated individuals. The first step is naming: transforming an unperceived injurious experience (unPIE) into a perceived injurious experience (PIE). The second step is blaming: transforming a perceived injurious experience into a grievance, which occurs when a person attributes fault for the injurious experience to some person or institution. Finally, there is claiming: the person with the grievance voices it to the person or entity believed to be responsible. Enforcement of civil law is marked by attrition at each stage. While a less institutional lens on this attrition would argue that, for instance, a persons probability of perceiving an experience as injurious is solely a function of randomly distributed psychological tendencies, Felstiner et al. argue that attrition at each stage is likely linked to differences in class, education, work situation, social networks(p. 633), but do not empirically investigate these. Felstiner et al.’s framework highlights that disparities in enforcement of a statute—disparities that have consequences for the extent to which a statute can serve its inequality-reducing or mobility-promoting aims—can have roots in more downstream disparities in who recognizes an experience as harmful and who a person blames for a harm. Thus, while a standard account of enforcement disparities by socioeconomic status and race focuses on features like disparate access to legal representation once a harm is already perceived, we can also look for demographic disparities in earlier stages of the enforcement process such as differences in whether a person blames him or herself for a bad outcome, which would halt the process at the blaming stage, or blames an institution.

We can see this attrition through the dispute resolution pyramid in Desmond’s Eviction. Individuals facing eviction have a variety of formal legal tools within civil law at their disposal to defend themselves against unfair evictions; in theory, they can enforce renters protection statutes by mobilizing these tools.

Yet Desmond’s ethnography highlights various reasons why renters are reluctant to mobilize these legal tools. For instance, Desmond highlights how the presence of children increases a family’s probability of eviction because “children caused landlords headaches...they could test positive for lead poisoning, which could bring a pricey abatement order. They could come under the supervision of Child Protective Services, whose caseworkers inspected families’ apartments for unsanitary or dangerous code violations”(p. 230). Families with children have formal laws protecting them from discrimination on this basis. As Desmond documents, although the initial 1968 version of the Fair Housing Act did not consider families with children a protected class, in 1988, Congress outlawed
discrimination against children and families. Yet even when the individuals in Desmond’s study faced explicit signals like being told outright by a landlord—“we don’t want your kids”—they stalled in the dispute resolution period somewhere between naming and claiming. They recognized that their children were causing difficulties in the housing search, and that landlords were discriminating on this basis in unfair ways. But the idea of claiming a harm and pursuing formal action was far from their mind as their dominant attention was on finding housing rather than enforcing abstract legal rights.

**Questions to think about:**

1. **Kagan:** Kagan uses the ideas of American political culture to explain adversarial legalism. How do we reconcile this national cultures framework with two empirical realities? First is statute by statute variation in the incentives the statute provides for adversarial legalism. Second is subnational—local and state variation—in rates of litigation and private enforcement of statutes. If there is a unified and distinctive American political culture that fosters legal enforcement of policies, is there also a unified and distinctive New York culture that explains why the state faces higher rates of litigation than say, New Jersey?

2. **Felstiner et al. and Desmond:** where in the dispute resolution pyramid do you think the individuals in Desmond’s study experience attrition? What policy interventions might address this attrition?
Week Twelve: Inequality in Civil Law - Goals for and Actors Behind Enforcement


Once an individual names, blames, and claims a harm—a process we discussed in Week Eleven—Galanters (1974) framework is useful for understanding how structural disparities in the organization of the legal system limit the extent to which persons can achieve redistributive aims. While the theory is sometimes simplified to imply that wealthier individuals are advantaged by the legal systems structure, the theories focus is less on the socioeconomic attributes of different parties in a claim and more on how a structural attribute—whether a party (person or organization) is a “one shotter” within the legal system or a “repeat player”—affect aspects of a case such as the party’s definition of success (e.g., rule development versus concrete resources), their preference for settlement versus trial, and their likelihood of relying on the general legal system for dispute processing versus developing private systems to adjudicate grievances. Thus, while socioeconomic attributes like wealth may be correlated with these structural attributes—for instance, wealthier one-shotters may be able to hire firms that are repeat players within an issue area—the focus is on how the structural aspect of being a one-shotter or repeat player encounters the structure of the legal system to produce certain patterns of enforcement.

Galanters four quadrants composed of different combinations of one-shotters and repeat players each are associated with certain patterns of enforcement. These quadrants lend useful insight not only into when enforcement disparities in civil law may emerge, but also an important type of disparity beyond unequal access to representation: disparities in the goals of enforcement, and a partys willingness to go to trial in order to pursue those goals.

Focusing on different goals for a case, Galanter argues that a repeat player has a stronger interest than a one-shotter in using the legal system to affect the long-term state of the law in ways that advance the repeat players interests. As a result, repeat players will settle cases where they expect unfavorable rule outcomes and appeal cases that they think will produce favorable rules, while one-shotters almost always prefer settlements.

Divergent interests between one-shotters and repeat players in seeing a dispute through to trial means that repeat players not only have an advantage in present disputes, they
also have an advantage in future disputes due to their ability to shape the law in their favor via strategic trials. In contrast, one-shotters’ focus on gaining what they can from the current case rather than using the law for rule-development makes it less likely that longer-term rule development will be in the one-shotters’ favor. Galanter points out that although organizations like legal aid have an interest in long-term rule development in one-shotters’ favor, the professionals duty to the present clients interests mean that the organizations face pushback if a lawyer for the organization tries to “play his series of OSs [one-shotters] as if they constituted a single repeat player” (p. 24).

The focus on divergent goals for a case for one-shotters, getting what they can out of the present case; for repeat players, sometimes sacrificing an easier and quicker settlement in the present case to strategically go to trial to advance rule development in their favor leads to broader disparities in how wide versus narrow-ranging the effects of enforcement are. In particular, one-shotters short-term goals make it difficult for their cases to proceed in ways that favor future one-shotters or that aggregate to lead to broader structural change, decreasing the extent to which enforcement can lead to more redistributive interpretations of various statutes.

Galanter’s essay was originally published in 1974. A 2003 volume, In Litigation: Do the Haves Still Come out Ahead?, points to two developments since the time of its writing that the essay, though prescient, did not fully address or anticipate. These developments are important for disparities in enforcement of inequality and mobility-focused statutes.

Development one: internalized dispute procedures for one-shotters versus repeat players (and not just repeat players versus repeat players)

Galanter’s framework argues that disputes involving two repeat players will often move out of public courts and into private remedy systems or informal bilateral controls since the players have an interest in minimal disruption to stable repeated transactions. These private remedy systems may rely on third parties to adjudicate disputes but take a form (such as arbitration or a domestic tribunal) detached from official sanctions and applying domestic rather than official rules (p. 21). In other words, repeat players use private systems for disputes with other repeat players with whom they share an interest in applying more local rules and in continuing beneficial transactions in the future. Yet the Silver-Greenberg and Gebeloff article on the proliferation of mandatory arbitration highlights the spread of these private remedy systems to transactions between a one-shotter and repeat player (e.g., a single resident versus a nursing home; a single consumer versus a credit card company; a single employee versus an employer). How does Galanter’s theory explain the rise of private arbitration not between two repeat players, but between a one-shotter and repeat player?

Perhaps one way to view the rise through the lens of Galanter’s framework is that these clauses are embedded in contracts that persons sign when they are seeking a stable continued relationship with an organization or company. Thus, even if the signee of a contract resembles a one-shotter when a dispute arises, they initially resembled a repeat player seeking a stable rather than one-shot transaction with the entity in question. However, since many have expressed concerns about the effect of private arbitration on the transparent public enforcement of civil law, theories explaining the evolution of private arbitration from repeat player-repeat player disputes to disputes between one-shotters and repeat players are worth examining in greater detail on the general exam.
Development two: changes in the organizational landscape of legal counsel

As Kritzer and Silbey note in the introduction to *Do the Haves Still Come out Ahead?*, Galanter wrote his essay during a particular historical period—an “age of hopefulness that saw the triumph of the civil rights movement and many experiments in creating greater access to justice”—and the essay was meant to temper some of the enthusiasm about the laws redistributive capacities.

The decades since Galanter’s essay have been marked by what Albiston and Nielsen (2014) call a shift from the expansion era of public interest law to the embattled era, where public interest law organizations (PILOs) funded through the federal Legal Services Corporation (LSC) faced restrictions on their activities and large foundations such as the Ford Foundation that had once funded a large ecosystem of public interest law nonprofits shifted their priorities. Galanter, in a section on the work of legal aid organizations in partnering with one-shotters against repeat players like education and housing entities, argued that these entities faced challenges in aggregating a series of one-shots to influence rule development in favor of future one-shotters. Adding to these challenges is an altered landscape where federal funding through the LSC now prohibits certain tools like class action used to aggregate one-shotters into cases oriented towards rule development.

Questions to think about:

1. Recently, federal agencies have used their funding leverage over various organizations to try to reduce the use of contracts containing mandatory arbitration clauses. These include efforts by the Department of Education to reduce the use of these clauses in colleges that benefit from student loans, as well as efforts by the Department of Health and Human Services to reduce their use in facilities like nursing homes that receive Medicare or Medicaid funding. Notably, these two types of organizations provide goods/services that are *incompletely commodified*—persons accept their provision through market mechanisms, but are simultaneously wary of some of the effects of this provision. What relationship do you see between the perceived acceptability of “commodifying” a good or service and the use of contracts that reduce a person’s right to publicize grievances with these goods or services?