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ABSTRACT

U.S. income inequality has varied inversely with union density over the past hundred years. But moving beyond this aggregate relationship has proven difficult, in part because of limited micro-data on union membership prior to 1973. We develop a new source of micro-data on union membership dating back to 1936, survey data primarily from Gallup (N ≈ 980,000), to examine the long-run relationship between unions and inequality. We document dramatic changes in the demographics of union members: when density was at its mid-century peak, union households were much less educated and more non-white than other households, whereas pre-World-War-II and today they are more similar to non-union households on these dimensions. However, despite large changes in composition and density since 1936, the household union premium holds relatively steady between ten and twenty log points. We then use our data to examine the effect of unions on income inequality. Using distributional decompositions, time-series regressions, state-year regressions, as well as a new instrumental-variable strategy based on the 1935 legalization of unions and the World-War-II era War Labor Board, we find consistent evidence that unions reduce inequality, explaining a significant share of the dramatic fall in inequality between the mid-1930s and late 1940s.

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1 Introduction

Understanding the determinants of the U-shaped pattern of U.S. income inequality over the twentieth century has become a central goal among economists over the past few decades. While most economists agree that both redistributive institutions such as unions and taxation as well as market forces such as technology and trade have roles to play in explaining this pattern, there remains widespread disagreement over the relative importance of the two. While there is a substantial literature in labor economics and sociology that argues for a causal relationship from labor unions to lowered labor market inequality (Card, 2001; DiNardo, Fortin, and Lemieux, 1996; Western and Rosenfeld, 2011), another view holds that more fundamental drivers, namely technological developments that increase the demand for educated labor faster than increases in educational attainment, better explain the time-series variation in inequality (Acemoglu and Autor, 2011; Goldin and Katz, 2008; Goldin, Katz, and Autor, 2020).

In the aggregate, there is a well-documented inverse relationship between income inequality and union membership in the US (see Figure 1). But moving beyond this aggregate relationship has proven difficult. While aggregate measures of union density date back to the early twentieth century, it is not until the Current Population Survey (CPS) introduces a question about union membership in 1973 that labor economists have had a consistent source of microdata that includes union status. Put differently, it is not until unions are in steady decline that they can be studied with U.S. microdata. By contrast, the U.S. Census has tracked Americans' education and wages consistently since 1940, allowing historical analysis of models emphasizing supply and demand of skill as determining levels of inequality (see, e.g., the seminal work by Goldin and Katz, 2008).

In this paper we bring a new source of household-level data to the study of unions and inequality. While the Census Bureau did not ask about union membership until the 1973 CPS, public opinion polls regularly asked about household union membership, together with extensive questions on demographics, socio-economic status and political views. We harmonize these surveys, primarily Gallup public opinion polls, going back to 1936. Our new dataset draws from over 500 surveys over the period from 1936-1986 and has over 980,000 observations, each providing union status at the household level. We combine these data with more familiar microdata sources (e.g., the CPS) to extend the analysis into the present day.

These new data sources allow us to revisit the role of unions in shaping the income distribution and to contribute to the long-running "institutions versus market forces" debate on the causes of inequality, particularly their role during the mid-century "Great Compression".[1] The competitive model focusing on the supply and demand for skilled workers offers hypotheses on the joint movement of relative wages and relative quantities and can be used to assess the economic forces at work. Given the increase in relative college wages since the 1960s, authors in this tradition (with a long pedigree stretching back to Douglas (1930), Tinbergen (1970), and Freeman (1976)) have focused on changes in demand resulting from technology (Katz and Murphy, 1992; Autor, 2014; Card and Lemieux, 2001; Katz and Autor, 1999; Autor, Katz, and Kearney, 2008)) interacting with the rate of schooling increases. Adaptations of the relative skill model to account for recent patterns in wage inequality include Beaudry, Green, and Sand (2016), Acemoglu and Autor (2011), Autor, Levy, and Murnane (2003), and Deming (2017). On the institutions side, the literature includes Bound and Johnson (1992), DiNardo, Fortin, and Lemieux (1996) and Lee (1999), with recent literature incorporating firms as important determinants of inequality (Song et al., 2015; Autor et al., 2020; Card, Heining, and Kline, 2013). A third strand of literature has attempted to "horse race" these two forces in ecological regressions across countries (Blau and Kahn, 1996; Jaumotte and Osorio Buitron, 2020), albeit with limited identifying variation. Bringing new micro data to the study of unions allows us to present several new results suggesting unions played a significant role in reducing income inequality at mid-century, when unions were at their peak and inequality at its lowest. These results fall into two broad sets.

Our first set of results replicates many of the stylized facts about unions established with CPS data and extends them back to earlier decades. We begin by showing that patterns of selection into unions has varied substantially over time: the education of union members relative to non-union members has followed a marked U-shaped pattern, mirroring the pattern of inequality itself and the sharp inverse of union density. That is, at mid-century, when density was the highest, unions were drawing in the least educated workers. Today, as in the 1930s, unions are smaller and union and non-union households look similar in terms of education. A similar pattern emerges for minorities: unions were relatively less white at mid-century than either before or after, even conditioning on education.

A key stylized fact about CPS-era unions is that members enjoy a wage premium, but did this advantage exist as union density was growing in the 1930s and 1940s and at their peak in the 1950s and 1960s? We show that the income advantage accruing to union households relative to non-union households with the same demographics and skill proxies holds quite steady (between ten and twenty log points) over our eighty-year period, despite the huge

[1] Collins and Niemesh (2019) is another recent paper emphasizing the role of unions in the Great Compression. They use the industry measures of union density constructed by Troy (1965) and form proxies of union density using 1940 IPUMS industry allocations within state economic areas. We build on this by providing direct measures of household union membership at the annual level over this period.

swings in union density and composition. As unobserved selection is typically a challenge in interpreting cross-sectional union premium regressions, we use a panel survey from 1956–1960, and we can show that the cross-sectional and respondent fixed-effects estimates are very close in magnitude.

The household union premium is larger for the less-educated households, decreasing by four log points for every additional year of education, and effects for non-white households are also remarkably stable over our entire sample period. We show that unions not only reduce the differentials paid to observable traits such as education and race, but shrink the differentials associated with non-observable traits as well: the ratio of *residual* income variance in the union sector to that in the non-union sector remains stable at roughly 0.60 over our entire sample period.

Together, the U-shape in selection by education and relatively constant patterns in union premia suggest that during the middle decades of the twentieth century, unions were conferring a substantial advantage to what would otherwise have been low-income households, thus compressing the income distribution. In our second set of results, we move beyond these stylized facts consistent with the role of unions depressing inequality, and instead more explicitly model the relationship of inequality to union density.

We begin by decomposing household income distributions, following DiNardo, Fortin, and Lemieux (1996)'s analysis of modern CPS data. We model selection into unions and then re-weight the non-union distribution to look like a deunionized counterfactual. We show that unions significantly compressed the mid-century income distribution: the Gini coefficient would have been 0.025 higher in 1968 had no household been unionized—a large effect, equal to the increase in the Gini from 1980 to 1990.[2] This exercise is most directly related to the stylized facts we document on selection and the union premium.

For each year of our sample period, we can estimate the effect of unions on the unconditional household income distribution, accounting for the changing position of union households in the counterfactual non-union income distribution as well as any changes in the union income premium. Across our eighty-year sample period, we find a consistent negative effect of re-weighting the full income distribution toward the union income distribution on both the Gini coefficient and the 90/10 ratio. As would be expected given the changes in selection and density documented earlier, the negative effect of unions on inequality is especially large at mid-century, when unions were organizing the most negatively-selected workers. We also document a role for spillovers using Fortin, Lemieux, and Lloyd (2018)'s extension of DiNardo, Fortin, and Lemieux (1996), suggesting that the micro-effect of unions

[2]In 1980, the U.S. household Gini in the CPS was 0.403 and after a decade of rapid growth in inequality, it stood at 0.428 (data from FRED).

on union members does not capture all of the effects of unions on the income distribution.

Next, we turn to regression analysis where instead of microdata we employ annual aggregated data from a variety of sources and include union density as an explanatory variable. We begin by simply adding union density to the canonical regressions estimated by Katz and Murphy (1992), Autor, Katz, and Kearney (2008) and Goldin and Katz (2008), who use aggregate time-series regressions to show that the supply of educated workers is a strong, negative predictor of the college-wage premium. We then refine our time series analysis by adding geographic variation, regressing state-year measures of inequality on state-year union density. While the aggregate-level analysis could have been performed without the data sources we have developed, the state-year regressions are made possible by state identifiers in our Gallup microdata. In both the annual and the state-year regression analyses, union density has a negative effect on standard measures of inequality such as the college premium, the 90/10 ratio, the Gini coefficient, the top-ten-percent income share, and the labor share of net income. While these exercises depends on a different set of (admittedly strong) identifying assumptions, they each yield a negative and significant effect of union density on measures of income inequality, in many cases comparable to or larger than the effect of skill shares.

Finally, we move to an explicitly causal estimate of the effect of union density on inequality, leveraging state-level heterogeneity in the two national policies most responsible for raising union density in the twentieth century: the Wagner Act and the National War Labor Board. The relatively fine annual and geographic variation in our data allows us to examine annual changes in union density and inequality across states in response to the immediate post-Wagner Act membership increase and World War II production contracts. We show that equality and union density differentially and robustly increased in states with high latent union demand and war production in the 1935-1947 period, with no other differential change in any other period.

We see three key contributions in extending microdata analyses of unions back to the 1930s. First, economists' understanding of the basic economics of U.S. labor unions—the size or stability of the union premium, selection into unions by education or other proxies for non-union wages, differences in residual wage variance between the covered- and non-covered sectors—relies almost entirely on CPS data and is thus limited to 1973 and later. We use our new micro-data to examine these stylized facts going back to 1936 (Sections 3 and 4). Importantly, tracing out how selection and the union premium varies during the decline, at the nadir, and then during the rise of U.S. income inequality sheds light on whether unions are a plausible factor in explaining the time-series pattern of inequality. These findings motivate our second contribution, in which we model inequality as an explicit function of union density (Section 5) in regression analyses. Finally, constructing state-year measures of union density back through the heyday of union growth allows us to leverage identifying variation from the two historical moments that account for almost all the sustained increase in private sector union density in American history: the Wagner Act and World War II. Cross-state variation in the effects of these national policies (increasing union density while simultaneously decreasing inequality) adds further evidence that unions played an integral role in the steep reduction in inequality that look place from the 1930s to the 1940s.

The rest of the paper is organized as follows. In the next section, we describe our data sources, in particular the Gallup data. Section 2 also presents our new time-series on household union membership. Section 3 analyzes selection into unions, focusing on education and race. Section 4 estimates household union income premiums over much of the 20th century, and Section 5 presents our evidence on the effect of unions on the shape of the overall income distribution. Section 6 offers concluding thoughts and directions for future work.

2 Household union status, 1936 to present

In this section, we briefly describe how we combine Gallup and other historical microdata sources with more modern data to create a measure of *household union status* going back to the 1930s.

2.1 Gallup data

Since 1937, Gallup has often asked respondents whether anyone in the household is a member of a labor union. This question not only allows us to plot household union density over a nine-decade period, as we do in this section, but also to examine the types of households that had union members and whether union membership conferred a family-income premium, as we do in subsequent sections. Before beginning this analysis, we highlight a few key points about the Gallup and other historical data sources that we use. A far more complete treatment can be found in Appendix B.[3]

Before the 1950s when it adopts more modern sampling techniques to reach a more representative population, Gallup data suffers from several important sampling biases that tend to over-sample the better-off. First, George Gallup sought to sample *voters*, meaning under-sampling the South (which had low turnout even among whites) and in particular Southern blacks (who were almost completely disenfranchised). Further, the focus on voters resulted in over-sampling of the educated (due to their higher turnout). Second, survey-takers

[3]Much of the information summarized here and presented in more detail in Appendix B comes from Berinsky (2006).

in these early years were given only vague instructions (e.g., "get a good spread" for age) and often found it more pleasant working in nicer areas, further oversampling the well-off. Even after 1950, these biases remain, though become smaller. We compare the (unweighted) Gallup data to decennial Census data in each decade in Appendix Tables B.1 and B.2.

As we are interested in the full U.S. population, we seek to correct these sampling biases to the extent possible. We weight the Gallup data to match Census *region* $\times$ *race* cells before 1942 and *region* $\times$ *race* $\times$ *education* cells from 1942 (when Gallup adds its education question) onward. Moreover, in Appendix D, we show that all of our key results are robust to various weighting schemes, including not weighting at all.

As we can only compare Gallup to the Census every ten years, we also seek some annual measures to check Gallup's reliability at higher frequencies. In Appendix Figure A.1, we show that our Gallup unemployment measure matches in changes (and often in levels) that of the official Historical Statistics of the United States (HSUS) from the 1930s onward, picking up the high unemployment of the "Roosevelt Recession" period. As another test of whether Gallup can pick up high-frequency changes in population demographics, Appendix Figure A.2 shows the "missing men" during World War II deployment: the average age of men increases nearly three years, as millions of young men were sent overseas and no longer available for Gallup to interview.

Beyond sampling, Gallup's standard union membership survey question deserves mention, as it differs from that used in the most widely used modern economic survey data, the CPS. Gallup typically asks whether you *or your spouse* is a member of a union, so we cannot consistently extract individual-level union membership as one could in the CPS.[4] In Appendix D, we compare our key results whenever possible using individual instead of household union measures—while occasionally levels shift, the changes over time are remarkably similar.

2.2 Additional Data Sources

While we rely heavily on the Gallup data, we supplement Gallup with a number of additional survey data sources from the 1930s onward. Gallup does not ask family income for much of the 1950s, but the American National Election Survey (ANES) asks both family income and union household status throughout that period, so we augment our Gallup data with the ANES in much of our analysis.[5]

[4] In some but not all cases they will then ask who (the respondent or the spouse) but to be consistent across as many surveys as possible, we create a harmonized *household* union variable.

[5] The ANES has a relatively small sample size in any given year so that our ability to use the ANES to provide detailed breakdowns of union status and income by geography or demographics

We have found one survey that includes a union question that pre-dates our Gallup data. This 1935-36 survey was conducted by the Bureau of Home Economics (BHS) and Bureau of Labor Statistics (BLS) to measure household demographics, income, and expenditures across a broad range of U.S. households, and we will henceforth refer to it as the 1936 Expenditure Survey. The survey asks about *union dues* as an expenditure category, which is how we measure household union membership. Rather than sampling randomly from the whole population, the agencies chose respondents from 257 cities, towns, and rural counties within six geographic regions. In most communities, the sample was limited to native-white families with both a husband and wife, though blacks were sampled the Southeast and blacks a single individuals in some major Northern cities.[6] To mitigate the effects of this selective sampling on our estimates, we employ the same cell-weighting strategy as we do in our Gallup sample.

We further supplement our sample with a 1946 survey performed by the U.S. Psychological Corporation that includes state identifiers, family income, union status and standard demographics.[7] In 1947 and 1950 we use data from National Opinion Research Corporation (NORC) as a check on our union density estimates from Gallup, but, as these data do not have state identifiers, we do not use them in our regression analysis. We also use the Panel Survey of Income Dynamics (PSID) for the late 1960s and early 1970s. From 1977 onward, we can use the CPS to examine household measures of union membership.[8]

Summary statistics for the CPS, ANES, and these additional data sources appear in Appendix Table B.3. In general, at least along the dimensions on which Gallup appears most suspect in its early years (share residing in the South, share white, education level), these data sources appear more representative. The table shows all data sources unweighted, though we will use ANES and CPS weights in years they are provided, to follow past literature. We weight the 1936 Expenditure survey and the 1946 U.S. Psychological Corporation survey in the same manner that we do Gallup.

is limited.

[6]Black families were included in New York City, Columbus, OH, and the Southeast, and single individuals were included in Providence, RI, Columbus, OH, Portland, OR, and Chicago, IL. Note that Hausman (2016) uses these data in studying the effects of the 1936 Veteran's Bonus.

[7]The Psychological Corporation survey was a public opinion survey conducted in April 1946, in 125 cities with 5,000 respondents (plus an additional rural sample). See Link (1946) for a description of the survey and cross-tabulations.

[8]Beginning in 1977, the CPS includes both the union-membership question *and* individual state-of-residence identifiers. As most of our analysis conditions on state of residence, we generally do not use CPS data from 1973–1976, which has the union variable but only identifies twelve of the most populous states plus DC, and groups the rest into ten state groups.

2.3 The union share of households over time

Figure 2 plots our weighted Gallup-based measure of the union share of households, by year, alongside several other series (Appendix Figure D.1 shows that the weighted and unweighted Gallup measures are very similar). The Gallup series bounces around between eleven and fifteen percent from 1937 to 1940. Between 1941 and 1945, the years the U.S. is involved in World War II, the household union-membership rate in our Gallup data roughly doubles. The union share of households continues to grow at a slower pace in the years immediately after the war, before enjoying a second spurt to reach its peak in the early 1950s. After that point, the union share of households in the Gallup data slowly but steadily declines.

Also presented in Figure 2 are our supplemental survey-based series. Note that each of these series generally has fewer observations per year than Gallup. The ANES sits very close to Gallup, though as expected is noisier. The 1936 expenditure survey is very close to our earliest Gallup observation, in 1937. The U.S. Psychological Corporation appears substantially lower than our Gallup measures in 1946, whereas the two NORC surveys (from 1947 and 1950) are very close to the Gallup estimates for those years.

To avoid clutter and to focus on the earlier data, we end our series in the 1980s and do not plot our CPS series in this figure, instead plotting the official CPS/BLS individual worker series, divided by the number of households, in blue for comparison. Appendix Figure A.3 shows the Gallup and CPS household-level series from 1970 until today, allowing readers to more easily assess their degree of concordance during their period of overlap (1977-1986). Reassuringly, in the years when Gallup and the CPS overlap, they are quite close.[9] As we emphasized in Section 2.1, our measure of union density is based on whether a *household* has a union member, as the Gallup data do not always allow us to examine respondent-level membership. Appendix Figure D.2 shows how our household notion of density compares to the more traditional *individual* measure of density within the ANES and CPS, where both measures can be computed. The household measure is always above the individual measure, as we would expect. But in both datasets, the household and individual measures track each other in changes quite closely.

2.4 Comparison to historical aggregate series

Finally, Figure 2 plots two widely-used historical *aggregate* data series, the BLS series (based on union self-reports of membership) and the Troy series (compiled by Leo Troy for the NBER

[9]Given the labor-intensity of reading in the Gallup data, we do not continue past 1986 and beyond this point rely on the CPS. We cut off at 1986 in order to have a ten-year period where Gallup and CPS overlap, which allows us to check consistency of Gallup over a substantial period of time.

and based on union's self-reported revenue data).[10] While the Gallup measures do not always agree with the BLS and Troy series in levels, they are, for the most part, highly consistent in changes. We describe these existing historical data sources in greater detail in Appendix E, summarizing key points below.

The density measures based on existing historical aggregate sources are everywhere above our microdata-based series until the 1950s, at which point they converge. As we document in Appendix E, labor historians believe the union self-reports of their own membership (which the BLS series uses) are significantly biased upwards. Especially from 1937-1955, when organized labor in the US was split into two warring factions—the American Federation of Labor and the Congress of Industrial Organization—the two federations over-stated their membership in attempts to gain advantages over the other. Membership inflation became such an issue that the federations themselves did not know their own membership. The CIO felt the need to commission a 1942 internal investigation into membership inflation, privately concluding that its official membership tally was inflated by a factor of two.

Leo Troy was aware of the membership inflation issue, and thus where possible bases estimates on dues revenue (from which he can back out membership using dues formulae). But as we discuss in Appendix E, revenue reports are missing for much of the early CIO, and the same incentives likely led unions to inflate dues revenue as well.

That respondents polled by Gallup did not share these incentives to overstate union membership is an advantage of our data. However, there is an important reason why Gallup and other opinion surveys may *understate* true union membership: individuals can be in unions without knowing it, especially during certain historical moments. As we discuss in greater detail in Section 5.4, during World War II, the government gave unions the authority to default-enroll workers when they started a job at any firm receiving war-related defense contracts and to automatically deduct dues payments from their paychecks. Thus, some workers during this period of rapid growth in density may not have known they were members and thus answered Gallup survey enumerators honestly (though incorrectly) that they were not in a union. It is not surprising that the Gallup data most undershoots the Troy and BLS numbers during the war years. Similarly, moments of high unemployment complicated calculations of union density. Until Congress mandated annual reporting in 1959, unions had great discretion in how to count a union member who became unemployed, whereas an unemployed respondent in Gallup, no longer paying his union dues, might honestly consider

[10]These series give aggregate union *counts* of membership, so we divide by estimates of total U.S. households (geometrically interpolated between Census years) to make the numbers as comparable as possible to Gallup. This transformation will obviously overstate the union share of households if many households had multiple union members.

himself no longer a member.[11] Indeed, Figure 2 shows that Gallup shows essentially no net growth between 1937-1940, which includes the period after the upholding of the NLRA, but also includes the Roosevelt Recession, whereas the BLS and Troy show robust growth.[12]

In summary, while the microdata-based versions of household union density we develop and the more widely used measures based on aggregate data differ slightly in levels (in a manner consistent with their non-trivial differences in methodology), they in almost all years firmly agree in changes. Like the Troy and BLS series, the Gallup data exhibit the same inverted U-shape over the twentieth century. Moreover, as we will show in Section 5, the relationship between aggregate union density and inequality is very similar whether we use our new, micro-data-based measures of household unionization rates or the traditional, aggregate measures.[13]

An important advantage of our series, however, is that it is based on microdata, which allow us to examine *who* joined unions and how this selection changed over time. It is to this task we now turn.

3 Selection Into Unions

Labor economists have long debated the nature of selection into unions. We focus on selection into union by education, and then by race. Less-educated and non-white households have on average lower income than other households, and thus selection along these margins into unions reveals whether or not unions historically excluded or included the relative less advantaged. Besides being of independent interest, the nature of selection into unions is an indirect test about whether union density was causally related to the Great Compression: if union members were, say, more educated and whiter than non-union members in mid-century, it would be difficult to argue that the increased union density was exercising equalizing pressure.

[11] As noted, Gallup and ANES did *not* skip over the unemployed or those otherwise out of the labor force when fielding their union question, and many unemployed and retired respondents in these surveys nonetheless identify as union members.

[12] Indeed, it is well documented that at least among the largest locals where data are available, *dues payments* plummeted for CIO unions during the 1938 recession, as millions of workers were laid off (Lichtenstein, 2003). We speculate that unions continued to report these laid-off workers as members.

[13] Of course, it is possible that Gallup's non-representative sampling contributes to the gap between it and the BLS and Troy series. We suspect non-random sampling is not an important factor. First, the sampling biases with respect to calculating average density go in both directions (e.g., Gallup's oversampling the well-off creates negative bias but under-sampling the union-hostile South creates positive bias). Second, as noted, the weighted and unweighted versions of the Gallup union density series are very similar (see Appendix Figure D.1).

While we focus on selection on observables, there is likely selection on unobservables that bias our results. These unobserved traits could include uncredentialled trade skills or raw ability. Lewis (1986) wrote "I have strong priors on the direction of the bias....the Micro, OLS, and CS wage gap estimates are biased upward—the omitted quality variables are positively correlated with union status." Abowd and Farber (1982) and Farber (1983) enriched the model of selection into unions to include selection by union employers from among the pool of workers who would like a union job. They argue that, because unions confer a larger wage advantage to the less skilled, the the marginal cost of skill to union employers is lower than for nonunion employers. The result is that most skilled will not want a union job, and employers will want to hire the most highly skilled from among those workers who do desire a union job. Thus, low observed skill workers will be positively selected into union jobs by employers based on their unobservables and high observed skill workers will be negatively selected into union jobs by workers based on their unobservables. This two-sided selection results in the union sector being composed of the center of the (observed plus unobserved to the econometrician) skill distribution for a particular job. Card (1996) presents evidence consistent with this two-sided view of selection, and argues that the resulting biases cancel each other out resulting in a relatively unbiased cross-sectional union premium.

3.1 Selection into unions by education

We begin our analysis of who joined unions by estimating the following equation, separately by survey-source d (e.g., Gallup, ANES, CPS) and year y:

$$Union_{hst} = \beta_{dy} Educ_h^R + \gamma_1 Female_h^R + f(age_h^R) + \mu_s + \nu_t + e_{hst}. \tag{1}$$

In this equation, subscripts h, s, and t denote household, state and survey-date, respectively (our Gallup data provides many surveys per year, so survey date t will map to some unique y and survey-date fixed effects subsume year fixed effects). The superscript R serves to remind readers that in many cases, a variable refers specifically to the *respondent* (not necessarily the household head). $Union_{hst}$ is an indicator for whether anyone in the household is a union member (and is the underlying household-level variable we use to construct the aggregate time-series in the previous section). $Educ_h^R$ is the respondent's education in years.[14] $Female_h^R$ is a female dummy, $f(age_h^R)$ is a function of age of the respondent (age and its square when respondent's age is recorded in years, fixed effects for each category when it is recorded in

[14]Where a specific survey does not collect information directly on years of schooling but reports specific ranges or credentials, we use simple rules to convert these measures to years of schooling. The note to Figure 3 describes how we impute years of schooling in these cases.

categories), and μ_s and ν_t are vectors of state and survey-date fixed effects, respectively.

The vector of estimated β_{dy} values tells us, for a given year y and using data from a given survey source d, how own years of schooling predicts whether you live in a union household, conditional on basic demographics and state of residence.[15] Note here that we are not yet controlling for race.

Figure 3 shows these results across our key datasets. A clear U-shape emerges, with the year-specific point-estimates remarkably consistent across all data sources.[16] In the earliest years (1936 through approximately 1943) the coefficients suggest that an additional year of education reduces the likelihood of living in a union household by only two to three percentage points. At the trough of the U (around 1960), we estimate that an additional year of education reduces the likelihood of living in a union household by roughly *five* percentage points. Since the 1960s, the negative marginal effect of education on the probability of living in a union household declines steadily: it reaches zero around 2000 and is now positive and in some years statistically significant, though small.

The differential increase in education among union households in recent decades may reflect, in part, the substantial growth of relatively highly-educated public sector labor unions since the 1960s. Indeed, as we show in Appendix Figure A.7, before President Kennedy's 1962 executive order giving federal employees the right to organize, the share of union members in the public sector was nearly negligible, hovering around five percent, while today one in every two union members works in the public sector.[17] While we do not know sector for the Gallup, Psych. Corp., and 1936 expenditure surveys, we can compare our baseline selection patterns from the ANES and CPS to those when we drop any household with a public sector worker. As Appendix Figure A.8 shows, while the *levels* of the selection effect change slightly for this sample, the *increase* in the education of union households from 1970 onward is unchanged. While we do not have data from before 1950, any effect of public-sector unions is likely to be tiny, as both the public sector workforce was smaller and public-sector unions were essentially nonexistent.

Another possible explanation for the relative up-skilling of union households is the steep

[15] For the ANES, given the small sample sizes, we constrain the coefficients on education (β_{dy}) to be equal across six-year bins in order to reduce sampling error. For the Gallup and other surveys, we estimate the coefficients on education (β_{dy}) by estimating separate regressions for each $survey\ source \times year$ combination.

[16] This pattern holds when other education measures are used instead of years of schooling. Appendix Figures A.4, A.5, and A.6 show similar patterns when, respectively, a high-school dummy, college dummy and log years schooling serve as the education measure.

[17] Over the period from 1973-2016, tabulation of CPS data indicates that 5.3 percent of college graduates employed in the private sector were members of labor unions. In contrast, fully 39.7 percent of college graduates employed in the public sector are union members.

decline since the 1960s in the share of union members in manufacturing employment—also depicted in Appendix Figure A.7. The manufacturing share of union members is the rough inverse of the public-sector share, falling from nearly fifty percent in the 1950s to less than ten percent today. Appendix Figure A.8 also shows the education selection patterns after dropping households with *either* a public-sector or a manufacturing worker. A large majority of the up-skilling effect remains.[18] We return to this pattern in the conclusion when we discuss questions for future work.

As noted in Section 2, we use a household and not an individual concept of union membership. In the discussion above, we have implicitly assumed that the selection patterns over time reflect less-educated workers joining unions in the middle decades of the 1900s, but in principle they could instead reflect changes in marriage patterns whereby union members, for whatever reason, became more likely to marry less-educated spouses during this period.

We address this concern in two ways. First, we reproduce the selection-by-education analysis (Figure 3) after excluding observations where the respondent is female. In this sample we do *not* rely on the education of the spouse as a proxy for the education of the likely union member. Appendix Figure D.4 shows that selection into unions by years of schooling for the male-only sample yields the same U-shape as we saw with the full sample. Second, in the CPS era, we can directly compare results using the household- and individual-based union membership concept. While we can only examine more recent years with our CPS data, both the individual and household selection series (plotted in Appendix Figure D.3) show the same marked increase in terms of selection by years of schooling from the 1970s until today.

All of this evidence suggests that union members were substantially less educated than non-members until quite recently and especially so in the 1950s and 1960s. While "skill" is multi-dimensional and has unobserved components, so long as unobserved dimensions of skill correlate with education, then the historical data from mid-century challenges Lewis' conjecture that "omitted quality variables are positively correlated with union status."

3.2 Selection into unions by race

We next examine selection by race, which is important for at least two reasons. First, given that school *quality* is an often unobserved dimension of skill (Card and Krueger, 1992) and blacks have always attended lower-quality schools than whites, race may serve as another proxy for skill and thus further inform the selection evidence in the previous subsection.

[18]These results use our standard weights as described in Section 2 and B, but Appendix Table D.1 shows robustness to other weighting schemes, including not weighting.

Second, selection of union members by race over time is an important (and unresolved) historical question. Historians disagree on the degree to which unions discriminated against black workers over the twentieth century (Ashenfelter, 1972, Northrup, 1971; Foner, 1976; King Jr, 1986; Katznelson, 2013).

We analyze selection by race in the same manner as selection by years of schooling, and simply replace $Educ_h^R$ with $White_h^R$ in equation (1).[19] The estimated coefficients on $White$ across time and data sources are presented in Figure 4. Again, a U-shape emerges, though it is noisier than that in the selection-by-education analysis. In the beginning of our sample period, whites are (conditional on our covariates) more likely to be in union households than non-whites. This advantage diminishes during the war years and continues to grow more negative until about the 1960s. While noisy, at this point, whites are about ten percentage points *less* likely to be in a union household than are other respondents. Since then, whites gain on non-white households and the differential attenuates toward zero as we reach the modern day.

While not quite as consistent as for education, selection by race again agrees for the most part across data sources. There is some disagreement between Gallup and CPS, whereby Gallup shows minimal selection with respect to race by the early 1980s, whereas CPS shows that whites are still somewhat less likely to live in union households. However, by the end of the sample period, there is no remaining selection by race in the CPS either. As we noted in the previous Section, Gallup's sampling of the South changes over time, so in Appendix Figure A.9 we replicate the analysis dropping all observations from the South, finding very similar results.

We believe it is an important contribution to show that, at least with respect to *membership*, blacks were not underrepresented in unions throughout most of the twentieth century. But this result must be viewed in context. First, part of the over-representation of blacks in unions is merely a byproduct of unions organizing lower-skilled areas of the economy, which are disproportionately non-white. Appendix Figure A.10 shows that controlling for years of schooling reduces the negative effect of the *White* coefficient in most years, though the basic U-shape remains.[20]

Second, membership alone does not summarize how unions treat non-white workers. While the mid-century leaders of the industrial unions of the CIO committed themselves publicly to policies of racial equality (Schickler, 2016), leadership roles remained overwhelm-

[19]Results are essentially exactly the inverse when instead of $White$ we use a black dummy. We use $White$ instead because sometimes Gallup uses "negro" and sometimes "non-white" and thus $White$ would appear, in principle, a more stable marker.

[20]For completeness, we also show (in Appendix Figure A.11) that the pattern of selection by education we see in Figure 3 barely changes if we simultaneously control for race.

ingly white, and U.S. labor history is littered with ugly examples of the white rank-and-file walking off the job in reaction to integration. By the early 1960s, over 100 locals of AFL-CIO unions (mostly in the South) remained explicitly segregated (Minchin, 2017). The 1964 Civil Rights Act led to large unions, even ones with Black leaders such as the UAW, being sued under Title VII. The AFL-CIO did not have a black officer until 2007.

Nonetheless, at mid-century, unions were organizing groups that were disproportionately non-white. Moreover, during most of the twentieth century the *non*-unionized sector practiced *de facto* or *de jure* racial discrimination, a topic we explore in the next section when we examine the union premium and in particular the premium by race.

4 The Union Family Income Premium Over the Twentieth Century

Estimating the union premium—the wage differential between union and otherwise-similar non-unions workers—is at the core of the modern empirical neoclassical approach towards measuring the effect of labor unions, pioneered by Lewis (1963). The early analysis by Lewis generally focused on industry-level differences, as consistent sources of microdata were not yet available. Freeman and Medoff (1984) were among the first to use CPS microdata to estimate determinants of union membership and the union premium with individual-level data. They find a union premium of roughly sixteen percent, averaging across studies in the 1970s. In general, a ten to twenty log-point union premium—controlling for Mincer-type covariates and estimated on cross-sectional wage data such as the CPS—has been found consistently in the literature. As noted in the introduction and in the Lewis (1986) review of the literature, there is almost no microdata-based estimates of the union premium prior to the 1968 PSID.[21] An important exception is a recent paper, Callaway and Collins (2018), which uses detailed microdata from a survey of six cities in 1951 to estimate a union premium comparable in magnitude to what we find. The advantage of our data is that it is nationally representative as well as available over long stretches of time, and includes income from all sources not just earnings. The disadvantage is that our income data is binned household data rather than continuous individual worker data.

A key challenge in this literature is separating any causal effect of union membership

[21] While cross-sectional estimates of the union premium go back at least to the 1960s (see Johnson (1975) for a summary of research from that period), many are based on ecological regressions (e.g. Rosen (1970)) between union density and average wages at the industry or occupation (often not labor market) level. These macro estimates are summarized and critiqued in Lewis (1983). The one pre-PSID exception to our knowledge is Stafford (1968) who estimates a union premium of 16% in the 1966 Survey of Consumer Finance.

on wages from non-random selection into unions. On the one hand, if higher union wages create excess demand for union jobs, then union-sector employers have their pick of queueing workers and unobserved skill could be higher in the union sector, overstating the union premium. On the other hand, a higher union wage premium for less-skilled workers and union protections against firing might differentially attract workers with unobservably less skill and motivation. Naturally, researchers have turned to panel-data estimation to address this selection bias, though Freeman (1984) and Lewis (1986) warn about attenuation bias due to misreported union status, which fixed-effects regressions exacerbate. Card (1996) uses CPS ORG data to examine workers as they switch between the union and non-union sectors (using the 1977 CPS linkage to employer data to correct for measurement error), showing that the union premium remains significant even after accounting for negative selection at the top and positive selection at the bottom.[22]

4.1 Baseline results

To construct a union premium series back to 1936, we use all the datasets employed in the selection analysis so long as they contain family income, which excludes most of Gallup data from the 1940s and 1950s. We also drop surveys with severe income top-coding (which we defined as more than 30 percent of observations in the top category), which results in losing some Gallup data from the 1970s.

Across all these surveys, we estimate the following regression equation separately by data source d and year y:

$$\begin{aligned} ln(y_{hst}) =& \beta_{dy} Union_h + \gamma_1 Female_h^R + \gamma_2 Race_h^R + f(age_h^R) + \\ & g(Employed_h) + \lambda_h^{eduR} + \nu_t + \mu_s + e_{hst}. \end{aligned} \tag{2}$$

While we are estimating a *household income* function, we do our best to mimic classic Mincerian controls. In the above equation, y_{hst} is household income of household h from survey date t in state s; $Union_h$ is an indicator for whether anyone in the household is a

[22]Lemieux (1998) performs a similar exercise using Canadian data, with the added advantage that he can focus on *involuntary* switchers. He finds estimates that are in fact quite close to OLS estimates of the union premium. Other scholars (e.g., Raphael, 2000 and Kulkarni and Hirsch, 2019) have used the Displaced Workers Survey (which records many *involuntary* separations thus lessening concerns about endogenous switching and is known to have limited mis-measurement of union status) to estimate worker-level panel regressions, again finding premiums close to cross-sectional OLS estimates (about 15 percent). Jakubson (1991) estimates longitudinal union premia in the PSID, getting estimates of around 5-8%, but does not account for measurement error.

union member; $Female_h^R$ and $Race_h^R$ are, respectively, indicators for gender and fixed effects for racial categories *of the respondent*; $f(age_h^R)$ is a function of age of the respondent (age and its square when respondent's age is recorded in years, fixed effects for each category when it is recorded in categories); $g(Employed_h)$ is a flexible function controlling for the number of workers in the household; λ_h^{educR} is a vector of fixed effects for the educational attainment of the respondent; and μ_s and ν_t are vectors of state and survey-date fixed effects, respectively. Note that for the 1946 U.S. Psychological Corporation and for the Gallup surveys from 1961 onward, we cannot control for the number of workers per household, but we show later that this bias should be small.

As with our selection results in the previous section, Figure 5 shows our union premium results separately by survey source and year. While not a perfectly flat line, the premium holds relatively stable. Of the sixty-some point estimates we report, only a handful are greater than 0.20 or less than 0.10. Not a single estimate has a confidence interval intersecting zero. Given the standard errors around each estimate, the family union premium does not appear to follow any discernible pattern over time, and in Appendix Table A.3 we check for heterogeneity by macroeconomic conditions, as in Blanchflower and Bryson (2004), but find little.

While the majority of our estimates are from cross-sectional data, there is a unique three-wave panel survey of the ANES (1956, 1958 and 1960) that allows us to estimate household union premium controlling for respondent fixed effects. The union premium estimated in this specification is almost identical to the cross-sectional estimate from the ANES in the same period, and statistically significant at the five-percent level despite a small sample. We provide more details and specifications in Appendix Table A.2. To our knowledge, this analysis yields the earliest panel-based estimate of the union premium, at least from U.S. data.

Card (2001), using CPS data, noted as a puzzle that the union wage premium was surprisingly stable between 1973 and 1993, even as private-sector union density declined by half. Our results, if anything, deepen this puzzle, as we show that the premium remains somewhere between ten and twenty log points over a *nine-decade* period that saw density (as well as the degree of negative selection by skill) both increase and then decrease.[23] We have no clear resolution of this puzzle and indeed find it hard to write down a model of collective

[23]While the unions literature is mostly empirical, the few theory papers on unions that do exist do not help rationalize the surprising pattern of declining density alongside steady premiums. Existing models in which SBTC determines union density rates predict that the premium should dwindle as density declines. This result is also hard to rationalize with models that assume a union objective function that is a positive function of both union wages and membership, such as Dinlersoz and Greenwood (2016).

bargaining outcomes with standard union and firm objective functions that yields a steady premium in the face of increasing then declining density. One simple explanation is that the union premium is bounded below by some minimum, say five percent, below which workers will not pay dues and attend meetings. It may also be bounded above by some amount of product market (or other input market) competition on the firm side.[24] We flag this question and the testing of this hypothesis as a potentially fruitful area for future research.

4.2 Robustness and Related Results

As a family union premium is a departure from the more familiar individual earnings premium estimated in past papers, Appendix Table A.1 shows the coefficients on the Mincer equation covariates in equation (2), so readers can compare it to standard earnings equations. In all cases, the coefficients on the covariates have the same signs and similar magnitudes as we typically see from an individual earnings regression.

As another check on whether the household nature of our inquiry creates biases, in Appendix Figure D.5 we use the CPS to compare our premium results with (*a*) the traditional worker-level earnings premium, where individual earnings are regressed on individual union membership and (*b*) a worker-level family income premium, where family income is regressed on individual union membership. Our premium results—family income regressed on household union membership—generally fall between these two other estimates. In almost all years, they agree in changes.

In Appendix Figure A.12, we show results after controlling for occupation of the household head. As noted, occupation categories vary considerably across survey sources so our attempts to harmonize will be imperfect, which is why we relegate this figure to the Appendix. The appendix figure reports coefficients that are somewhat larger than in the main Figure 5, consistent with unions differentially drawing from households where the head has a lower-paid occupation.

As noted earlier, we cannot control for the employment status of household members in the Gallup and the Psychological Corporation data. Appendix Figure A.13 shows that any bias is likely very small: in the ANES, *not* controlling for employment status increases the estimated union premium only slightly, relative to the baseline results where these controls are included.[25]

[24]Rios-Avila and Hirsch (2014) offer this explanation for the steady nature of the union premium, between ten and twenty points, across time and countries.

[25]Union households are more likely to have at least one person employed (likely the union member himself), which explains why controlling for household employment has a (slight) negative effect on the estimated union household premia. However, living with a union member is a *negative* predictor of own employment (results available upon request), which likely accounts for the fact

The family income premium may not fully capture changes in the household's economic well-being. Union families may benefit from other forms of compensation such as health benefits or vacation, as has been documented in the CPS-era (see Freeman, 1981 and Buchmueller, DiNardo, and Valletta, 2004 among others). Unfortunately, Gallup and our other sources do not consistently ask about benefits. One exception is from a 1949 Gallup survey that asked about paid vacation. As we show in Appendix Table A.4, Gallup respondents in union households are over twenty percentage points (about forty percent) more likely to report receiving paid vacation as a benefit.

On the other hand, the union premium may also reflect compensating differentials for workplace dis-amenities, which would suggest that our estimated premia are *overstating* the differential well-being of union households. Some evidence against this claim comes from another Gallup survey in 1939 that asks respondents how easily they could find a job "as good" as their current one. As we show in Appendix Table A.5, union households are significantly more likely to say it would be hard for them to find a job just as good. Similar to the union premium, this tendency is similar to that in the modern day (the same table shows these results using the 1977-2018 GSS). To the extent respondents considered non-wage job characteristics (safety, working conditions, benefits, etc.) this result is an additional piece of evidence that union members, even in the early days of the labor movement, felt their jobs were better—in a broad sense—than non-union members.

Our estimates of a sizable union premium contrast with recent papers using regression discontinuities in close NLRB representation elections to estimate the causal effect of unionization on firm-level outcomes (DiNardo and Lee, 2004; Lee and Mas, 2012; Frandsen, 2020). These papers have found little evidence of positive union wage premia, although some have found effects on non-wage benefits such as pensions (Knepper, 2020). What explains the discrepancy? A possibility is that the LATE identified by the RD papers is not informative about the average treatment effect of unions. Importantly, most existing union workplaces were organized earlier and most elections are not very close. It is reasonable that a clear (sizeable) union victory in an election reflects workers' expectations of substantial advantage while a very close election reflects workers' expectations of more limited advantage. As such, the LATE identified by the RD papers is likely not informative (and likely understates) the average advantage of unionization. We do not mean to imply that we have identified the true average causal effect of unions on wages, but neither is it the case that the small effects found in the close-election RD analyses are appropriate when applied broadly.

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that controlling for total number of workers in the household has only a small effect on the estimated premium.

4.3 Heterogeneous Union Household Income Effects

We have so far assumed that unions confer the same family income premium regardless of the characteristics of the respondent. We now explore heterogeneity by years of schooling and race.

We begin by augmenting our family income equation (2) by adding an interaction term between years of schooling and the household union dummy. Figure 6 presents the coefficient on this interaction term, as usual, separately by survey-source and year. The results are consistent throughout the period and show that less-educated households enjoyed a larger union family income premium. Over the nine decades of our sample period, this differential effect appears relatively stable. For each additional year of education, the household union premium declines by roughly four log points.

The analogous results from adding $White_h^R \times Union_h$ to equation (2) instead of $Years\,of\,educ_h^R \times Union_h$ are shown in Figure 7. The interactions are not statistically significant in the earliest surveys (the 1936 BLS Expenditure Survey and the 1942 Gallup Survey), though their signs suggest that white workers enjoyed *larger* premiums. However, in the 1946 Psychological Corporation survey and in succeeding Gallup, ANES and CPS surveys, there is consistent evidence of a larger union family income premium for nonwhites over the next five decades. This racial differential in the union effect on household income has declined somewhat since the 1990s and in the most recent CPS data it cannot be distinguished from zero.

We saw in our selection analysis that some of the disproportionate membership of non-white households was merely driven by disproportionate membership of the less-educated, so we check whether the differential premium to non-whites is similarly explained. In Appendix Figure A.14 we reproduce the analysis in Figure 7 but include $Years\,of\,educ_h^R \times Union_h$ in all regressions.[26] The results barely change, suggesting that even for households with the same level of education, black households enjoyed higher union premiums. Of course, the union premium equation is only identified by comparing family income for unionized versus non-unionized households, so this result does not mean that non-white union workers were paid more than white union workers, just that the white pay advantage was significantly smaller in the union sector. Returning to our discussion at the end of Section 3, this result suggests that despite the many ways that the U.S. labor movement discriminated against non-whites, such discrimination appeared worse in the non-organized sector.

Our conclusion from the heterogeneity analysis is that, at least for most of our sample period, disadvantaged households (i.e., those with respondents who are non-white or less educated) are those most benefited (in terms of family income) by having a household member

[26] For completeness, we also reproduce the heterogeneity by years of schooling analysis in Figure 6 after adding $White_h \times Union_h$ interaction. The results barely change (see Appendix Figure A.15).

in a union. Ignoring this differential effect would tend to underestimate the effect of unions on inequality, especially from 1940–1990, when the differential premium for black households appears largest. We return to this point in Section 5.4.

4.4 Effects on Residual Income Dispersion

An influential view of unions is that they lower the return paid not only to observed skill, as we document above, but also to unobserved skill. Supporting this view is the fact that, at least in the CPS era, the union wage distribution is compressed even after conditioning on observable measures of human capital (e.g., Freeman and Medoff, 1984 and Card, 2001).

We implement an analogous analysis at the household level to determine if unions performed a similar function in earlier decades. Separately for union and non-union households, we regress log family income on all the covariates (except union) in equation (2). As before, we perform this analysis separately by survey-source and year. We then calculate residuals for each sector and compute the ratio of variances between the union and non-union residuals (which has an F-distribution with degrees of freedom given by the two sample sizes, allowing us to construct confidence intervals). If unions compress the distribution of unobserved skill, then this ratio should be less than one.

Figure 8 shows, over our sample period, the ratio of variance of residual log family income between the union and non-union sector, together with 95% confidence intervals. The ratio is uniformly below one, and often below 0.5, with confidence intervals that always exclude equality of the variances. Like the union premium estimates, there does not seem to be a strong pattern over time in the union-nonunion difference in residual income inequality. Instead, it appears that the CPS-era pattern of unions compressing residual inequality holds in a very similar manner throughout the post-1936 period.[27]

5 The Effect of Unions on Inequality

Empirically, we have so far documented that, in their effect on household income, unions have exhibited remarkable stability over the past eighty years. During our long sample period, the union premium has remained between ten and twenty log points, with the less-educated receiving an especially large premium. Moreover, the negative effect of unions on residual income variance is large and also relatively stable over time. By contrast, selection into unions varies considerably. From the 1940s to 1960s, when unions were at their peak and inequality at its nadir, *disadvantaged* households were much more likely to be union members

[27] For example, Card (2001) estimates a union-non-union variance ratio of around 0.61 in 1973 using individual male earnings, very similar to what we find in the 1970s for household income.

than either before or since. These pieces of evidence suggest, at least indirectly, that unions were a powerful force pushing to lower income inequality during the heyday of the labor movement.

In this section, we explore in a more direct manner the relationship between unions and income inequality, joining an extensive empirical literature examining how unions shape the income distribution. It is helpful to separate this literature into two conceptual categories. First, assume that unions affect the wages of *only* their members and that estimates of the union premium can recover this causal effect, putting aside selection and spillover issues discussed earlier. Then, simple variance decompositions can estimate the counterfactual no-union income distribution and thus the effect of unions on inequality. For example, so long as unions draw from the bottom part of the counterfactual non-union wage distribution, then their conferring a union premium to this otherwise low-earning group reduces inequality. Moreover, residual wage inequality also appears to be lower among union workers, suggesting that unions reduce inequality with respect to unobservable traits as well (Card, 2001). DiNardo, Fortin, and Lemieux (1996) and Firpo, Fortin, and Lemieux (2009) take this approach and find that unions substantially reduce wage inequality, especially for men.

A second category of papers argues that unions affect the wages of *non-union* workers as well. Unions can raise non-union wages via union "threat" effects (Farber, 2005; Taschereau-Dumouchel, 2020) or by the setting of wage standards throughout an industry (Western and Rosenfeld, 2011). Conversely, unions can lower non-union wages by creating surplus labor supply for uncovered firms (Lewis, 1963). Unions might also affect the compensation of *management* (Pischke, DiNardo, and Hallock, 2000; Frydman and Saks, 2010) and the returns to capital (Abowd, 1989; Lee and Mas, 2012; Dinardo and Hallock, 2002), thus reducing inequality by lowering compensation in the right tail of the income distribution. By targeting the largest "superstar" firms (Autor et al., 2020) that have high capital fixed costs and low labor share, unions may compress cross-firm dispersion as well as the capital-labor split of income. Frydman and Molloy (2012) show that, across industries, the fraction unionized was a significant determinant of CEO pay compression in the 1940s and 1950s. Finally, as an organized lobby for redistributive taxes and regulation, unions might affect the income distribution via political-economy mechanisms (Leighley and Nagler, 2007; Acemoglu and Robinson, 2013).

In this section, we add several new results to this literature. First, and most directly related to the results in the previous two sections, we conduct distributional decompositions following DiNardo, Fortin, and Lemieux (1996), where we show how measures of inequality change with the level and composition of union membership. This exercise jointly accounts for where union households are in the income distribution as well as the effect of union membership on a household's position in the income distribution. The identifying assumptions are as follows: first, that, conditional on our controls, union membership is not otherwise correlated with determinants of income and, second, that union membership affects *only* the income of union households (i.e., no "spillovers" to other workers or households). We show robustness to weakened versions of these assumptions, in particular showing evidence of spillovers using extensions to the reweighting methodology proposed by Fortin, Lemieux, and Lloyd, 2018.

Second, we turn to more aggregate analysis. We follow some of the canonical work on the effect of skills shares on the college premium, adding union density to these standard, aggregate, time-series estimations. Note here that aggregate analysis does not rule out spillovers, but instead rests on the (strong) identifying assumption that conditional on our time-series controls, union density is exogenous. Next, we use the state identifiers in the Gallup data to conduct a parallel analysis at the state-year level. Finally, we leverage the historical cross-state variation in union density generated by the Wagner Act and World War II to obtain instrumental variables estimates of the effect of union density on inequality.

5.1 Distributional Decompositions

In this section we present the historical impact of unions on inequality using distributional decompositions, following DiNardo, Fortin, and Lemieux, 1996 (henceforth DFL). First, we compare observed inequality in each year to what inequality would look like without any union members. The difference provides a measure of unions' impact on inequality within a given year. Second, we use differences in this measure across key years in our data to identify the total contribution of unions to changes in inequality over time. In other words, we estimate how much of the fall and rise in inequality can be explained by unions.

Both of these exercises require estimating a counterfactual income distribution that would have existed had selection into unions been different than what was observed. Assuming union membership is conditionally independent of household income, we can simulate this counterfactual using reweighting procedures. Specifically, we will construct "deunionized" counterfactuals in each year by reweighting the non-union population so that their distribution of observables matches that of the general population.[28]

In our first exercise, we consider the income distribution under the counterfactual that nobody joins a union and compare it to the unweighted income distribution in each year. The top panel of Figure 9 plots differences in Gini coefficients for true and reweighted populations

[28]While the DFL methodology is by now standard, we provide a more complete review of DFL reweighting methods in Appendix F.

over time, $\text{Gini}\,(F_{Y_t}) - \text{Gini}(F_{Y_t^{C_0}})$. Unsurprisingly, this within-year impact of unions tracks both the pattern of union density and negative selection into unions documented earlier. During the period of peak union density, unions reduced the Gini coefficient by 0.025 relative to the non-unionized counterfactual. More surprising is that even though union members are positively selected on education today, unions still exert a small equalizing force, suggesting that the within-union compression effect still dominates the union-non-union difference.

The bottom panel of Figure 9 shows differences in log income percentiles between true and deunionized counterfactual distributions for the three years where we have continuous income data (1936 consumption survey, PSID, and CPS). In 1936 and 2014, the differences in these distributions are small, but in 1968 there is a large compressing effect of unions. We show the densities themselves in Appendix Figure F.1. In addition to true and deunionized density plots, the bottom panel of Figure 9 shows dashed lines corresponding to a deunionized counterfactual that also accounts for potential spillover effects of unions. We construct these spillover-adjusted distributions following Fortin, Lemieux, and Lloyd (2018), who augment the standard DFL reweighting procedure to allow for labor-market-level union density effects on the household income distribution.[29]

The time series and percentile plots tell a similar story: unions had a small impact on overall income inequality during the pre-war and modern eras, when density was low, but significantly compressed income inequality during the period in-between, when density was high. How much of the absolute change in inequality can we attribute to this differential impact from unions? To answer this question, we decompose the absolute change in inequality into its "total union effect," the difference between observed changes in inequality and the change in inequality that would have occurred in the absence of unions. For the time period t_B to t, this total union effect is computed as the difference in within-year union effects,

$$\Delta^U = \Big[\text{Gini}(F_{Y_t}) - \text{Gini}(F_{Y_{t_B}})\Big] - \Big[\text{Gini}(F_{Y_t^{C_0}}) - \text{Gini}(F_{Y_{t_B}^{C_0}})\Big] \tag{3}$$

$$= \Big[\text{Gini}(F_{Y_t}) - \text{Gini}(F_{Y_t^{C_0}})\Big] - \Big[\text{Gini}(F_{Y_{t_B}}) - \text{Gini}(F_{Y_{t_B}^{C_0}})\Big]. \tag{4}$$

Table 1 reports the total union effect over different periods. The contribution of unions

[29]Our procedure consists of further reweighting the DFL-weighted distribution to look as it would without unions in the same labor market. Spillover-adjustment weights are constructed to remove the predicted impact of state-year-industry (in CPS) or state-year (in 1968 PSID) union density throughout the income distribution. Predictions are formed from an ordered probit of non-union household income against state-year-industry (in CPS) or state-year (in 1968 PSID) union densities. These labor market densities are only directly available in the CPS and PSID, and hence dashed lines are omitted for 1936, although we present results with predicted state-year shares (along with additional details) in Appendix F.

to the change in household inequality between 1936 and 1968 is considerable, with unions explaining 23% of the change in the Gini, 46% of the change in the 90/10, 18% of the change in the 90/50, and 80% of the change in the 10/50 (note that these are ratios of household income, not individual earnings). The contribution of unions to the change in household inequality since 1968 is smaller but not insignificant, with unions explaining about 10% of the increase in the gini, and between 12-18 percent of the change in the percentile ratios.

In the left columns of Table 1, we further decompose the total union effect into the portion attributable to changes in union membership (a "unionization effect") and the portion attributable to changes in union wages (a "union wage effect"). Note, however, that estimating these subcomponents requires predicting union membership in one year using estimates of union selection from another, which comes with considerable caveats in our mixed-dataset setting.[30]

In sum, the pure "micro" effect of unions on household inequality from 1936 to 1968 is considerable, even without accounting for spillovers, and larger than the effect of union *decline* on the recent rise in inequality. Further, even during periods of positively selected union members and low density, such as 1936 and today, unions are still an equalizing force, although nowhere as quantitatively important as during the period of peak union density, where union density was high and union members considerably less educated than non-union members.

5.2 Time-series Regressions

While the distributional decompositions capture the effect of union density on household income inequality, they require a strong assumption that there are no spillovers, threat effects, or political economy mechanisms that alter wages for non-union workers. The plausibility of these more macro mechanisms warrants an aggregate analysis, complementing the individual household regressions estimated above. Further, our household survey data is binned and misses inequality across individuals, as well as inequality at the bottom and the top of the distribution, which can be addressed with more standard inequality measures constructed from other sources.

Our aggregate analysis of the effect of unions on inequality is motivated by the literature on the college wage premium. Following Katz and Murphy (1992) as well as Goldin and Margo (1992) and using a mix of data from the Decennial Census, the CPS and a 1915 survey from Iowa, Goldin and Katz (2008) show that the evolution of the college premium

[30] Details on our detailed decomposition into unionization and union wage effects is provided in Appendix F.

between 1915 and 2005 is well-explained by the relative supply of college workers, controlling for flexible functions of time. Autor, Katz, and Kearney, 2008 confirm this analysis using data from the CPS in the 1963-2005 period and adding more covariates.

We begin by simply adding union density to the specifications estimated in these papers:

$$\log\left(\frac{wage_t^{Col}}{wage_t^{HS}}\right) = \beta UnionDensity_t + \gamma log\left(\frac{N_t^{Col}}{N_t^{HS}}\right) + f(t) + \lambda X_t + \epsilon_t. \tag{5}$$

The dependent variable is the log college wage premium, which we specify as a function of the supply of skilled workers, $log(N_t^{Col}/N_t^{HS})$, a polynomial in time, $f(t)$, other time-series controls X_t, which we vary to probe robustness, and, importantly, $UnionDensity_t$.[31]

We choose time-series controls X_t both to follow past literature as well as to capture the most obvious confounds in estimating the effect of unions on inequality. Specifically, following Autor, Katz, and Kearney (2008) we include the real value of the federal minimum wage and the civilian unemployment rate and following Piketty, Saez, and Stantcheva (2014) we include the top marginal tax rate in the federal individual income tax schedule. As unions historically push for full employment, higher minimum wages and higher top tax rates, these might be "bad controls" and their inclusion would understate the full effect of union density on inequality. We adjust for heteroskedasticity and AR(1) serial correlation in the error ϵ_t using Newey-West standard errors.[32]

The first two columns of Table 2 show the results from this exercise. Col. (1) does not include additional controls X_t, whereas col. (2) does. The coefficient on union density is negative and highly significant (and very similar to each other in magnitude), and we discuss specific magnitudes below.

We also find a significant and negative coefficient on skill shares and in fact (despite somewhat different sample periods) recover a coefficient very close to those in Goldin and Katz (2008) and Autor, Katz, and Kearney (2008). Interestingly, as we show in Appendix Table A.6, union density and the skill-shares measure negatively co-vary at both the annual and state-year level (though this negative covariance is small and insignificant once we condition on our usual regression controls). Thus, controlling for skill shares tends to increase the sig-

[31]As we do not have a strong view regarding whether, at the aggregate level, our Gallup-based estimate of early union density is better than the traditional BLS estimate, we take a simple average of the two.

[32]These regressions can be seen as following Katz and Autor (1999), who decompose group-level wages into their "latent competitive wage" (i.e., relative skill shares and technological trends, augmented with measures of institutions, such as union density). However, we do not model group level density as having group-level effects, as in Card and Lemieux (2001), who put relative union shares (college union density divided by HS grad union density) as a regressor in the relative wage equation; rather, we consider overall density as affecting the relative wage.

nificance of union density, and vice versa. This point is important because going forward we will sometimes use noisy measures of skill share (e.g., interpolations between Census years), but as skill shares and density both tend to decrease inequality and negatively covary, noisy measurement of this control variable should generally yield conservative coefficient estimates on density.

Panel A of Appendix Table A.7 shows that these results are robust to many variants of our main specifications (e.g., using the Gallup series alone or the BLS series alone to calculate $UnionDensity_t$ instead of averaging the two together, and substituting either a quartic or a quadratic for the cubic time polynomial) and also reports more of the coefficients, which we suppress in the main tables in the interest of space.

While the canonical analysis in Goldin and Katz (2008) and related work focuses on the college premium, we extend our analysis in Table 2 by using the same specifications as in cols. (1) and (2) but using other measures of inequality as outcomes. Cols. (3) - (4) of Table 2 are identical to Cols. (1) - (2) except that the 90/10 log wage ratio for men (also taken from the IPUMS Census and CPS) is used as the outcome variable. The results are quite similar, with union density again having a negative and significant association with inequality that is robust to adding our vector of controls. Cols. (5) - (8) examine the 90/50 and the 10/50 ratios, showing that the effect we find on the 90/10 comes from the *bottom* half of the distribution, as the coefficients on density, while negative, are insignificant for the 90/50. For these outcomes, the same robustness checks documented in the top panel of Appendix Table A.7 are reported in panels B, C and D.

The rest of Table 2 examines annual data.[33] These additional years not only give us more observations, but also allow us to use inter-Census variation (e.g., during World War II). Cols. (9) and (10) use the Gini coefficient constructed by Kopczuk, Saez, and Song (2010) from Social Security data. The next two columns use the top-ten-percent income share from Piketty and Saez (2003).[34] The final two columns use the labor share of national income from Piketty, Saez, and Zucman (2018). For all three of these outcomes, the union density coefficient suggests a significant decrease in inequality (a negative coefficient for the Gini

[33]As noted earlier, a small complication in using these annual outcomes is that our pre-CPS estimates of the skill shares $log(N_t^{Col}/N_t^{HS})$ in equation (5) come from the Census and thus in principle are only available every ten years. To circumvent this issue, we include two separate education controls: (*i*) skill shares as measured (annually) in our Gallup data and an annual measure of skill shares equal to that from the CPS when it is available; and (*ii*) interpolating between Census years in the earlier period. In this sense, we treat education as a nuisance variable and simply try to control flexibly for it, allowing us to continue to estimate the conditional effect of union density.

[34]Results are qualitatively similar, with larger coefficients, if we instead use the top 10 share from Piketty, Saez, and Zucman (2018). We present results on top ten share using the Piketty and Saez (2003) data for consistency with the state-year measure we use below (Frank, 2015).

and top-ten share, and a positive one for labor share), robust to controls. The three panels of Appendix Table A.8 provide the usual robustness checks.

Our estimate magnitudes are generally sensible yet economically significant. Table 2 implies that a ten percentage point increase in union density results in a 12-15 percent fall in the college premium, 2-1.7 percent falls in 90-10 wage ratios for men, small and insignificant effects on 90-50 male wage ratio, and 1.5 to 1.8 percent increase in the 50-10 wage ratio. We further find that the same size increase leads to a 0.016 to 0.014 decrease in the Gini, roughly 3% of the mean, and 2.3 to 3.5 percentage points in the top ten share and 4.5-4.8 percentage points in the labor share.

The magnitudes implied by the time-series analysis are clearly larger than those implied by the micro-effect of unions on union members, even including the spillover effects. There are clear limitations to the time-series analysis—perhaps most obviously, concerns about endogeneity of union density and suspect inference due to small samples. Moreover, unlike the analysis of skill shares in Goldin and Katz (2008) and similar papers, the inclusion of union density is not theoretically motivated.

To examine the role of spillovers more rigorously, we draw on the counterfactual distributions we estimated in the previous section. In Appendix F we use the difference between the actual Gini (constructed here from our survey data, not the SSA data) and the DFL counterfactual Gini coefficient from Section 5.1 as an outcome in the time-series regression, again controlling for skill shares and time polynomials. The coefficient on union density in this regression isolates the effect of union density on inequality that is solely due to the effect of unions on the incomes of union households. This could be called the pure "micro" effect of unions. The effect here is roughly between -0.04 and -.06, so that a 10 percentage point increase in union density reduces the Gini via the micro effect by roughly 0.005 points. But the effect of union density on the overall Gini itself is -0.3, where a 10 percentage point increase in density reduces the Gini by 0.03. This table suggests much of the effect of unions on inequality would be through the effects on non-union workers, but there are good reasons to think our selection equation is mis-specified (no controls for industry or occupation, for example, which Figure A.12 suggests increases the union premium) and use of binned income data implies we are underestimating the micro-effect of unions on inequality.

In the next section, we take an intermediate position on the scope of spillovers and the endogeneity of union density by estimating similar aggregate regressions at the state-year level, which allows a much richer set of controls, including state and year fixed effects.

5.3 State-Year Panel Regressions

While the time-series analysis generates summary accounts of the aggregate association of unions on the U.S. economy, a major limitation are the many unobserved factors (e.g., technology, macroeconomic policy, trade, outsourcing, industry structure) potentially correlated with both inequality and union density and not necessarily absorbed by our controls. In this section we replicate the analysis at the state-year level, controlling for state and year fixed effects, which can absorb a considerable amount of unobserved heterogeneity.

The Gallup data always contain state identifiers, so we can construct continuous state-year measures of union density throughout the pre-CPS period, something that was not possible with previous data.[35] Although we do not attempt to isolate exogenous variation in union density in this section, we can determine whether the inverse inequality-density relationship in the aggregate time series also holds at the state-year level, conditional on year and state fixed effects.[36] Importantly, as all states have access to the same national technology, the vector of year fixed effects in this design controls for simple variants of SBTC that affect all states the same way.

We combine our Gallup state-year measures with household state-year measures calculated from the CPS. We take a weighted average of Gallup-generated state-year union densities and CPS-generated state-year union densities, with weights proportional to the number of observations in each sample (so the CPS gets a much larger weight). This procedure results in a panel of annual state-year union density measures going back to 1937. Note that such a high-frequency panel was impossible to construct before the Gallup data, as in most years the BLS and Troy series did not break down their aggregate counts geographically, and when they did, it was generally only for a few years (Troy) or by coarse regions (BLS).

To examine the effect of unions on inequality, we closely follow equation (5) and estimate specifications of the form:

$$y_{st} = \beta UnionDensity_{st} + \gamma log\Big(\frac{N_{st}^{Col}}{N_{st}^{HS}}\Big) + \lambda X_{st} + \mu_{t,r(s)} + \delta_s + \epsilon_{st} \tag{6}$$

where y_{st} is a measure of inequality, for example the college-HS wage gap or the percent of total income accruing to the top ten percent, in state s and year t. A contribution of our paper that we use in this analysis (as well as in the next subsection) is the construction of

[35] Troy (1965) presents state breakdowns for 1939 and 1956, and Hirsch, Macpherson, and Vroman (2001) use BLS reports to construct state-year measures of density from 1964 onwards.

[36] Similar regressions are estimated at the cross-country level by Jaumotte and Osorio Buitron (2020), though their sample period of 1980-2010 is far shorter than ours.

historical state-year measures of the labor share of net income, following Piketty, Saez, and Zucman (2018). We present details and validation in Appendix H.

As before, we control for skill-shares $\log(N_{st}^{Col}/N_{st}^{HS})$ in all specifications.[37] We include state fixed effects (δ_s) and a vector of year fixed effects that allow each year to have a different effect for the South ($\mu_{t,r(s)}$). Note that we include South-by-year fixed effects because, as discussed earlier, Gallup's sampling of the South improves over time and we want to flexibly control for this evolution. We cluster the standard errors at the state level.

As before, we will show results with and without X_{st}, a vector of additional state-year controls. We try our best to capture the same covariates as in equation (5), though in some cases controls that are available at the annual level in the historical period are not available at the state-year level. To control for economic expansions and contractions, we include in X_{st} state-year log income per capita and state-year measures of the share of households subject to the federal income tax. We include these measures as proxies for relative local economic prosperity, as annual state-level unemployment rates are not consistently available until the 1963 CPS. We include top marginal income tax rates by state, and to more fully capture the political-economy climate, we also control for a state-year level "policy liberalism" index developed by Caughey and Warshaw (2016).[38] Manufacturing moving from the unionized Northeast and Midwest to the South and West is often cited as a reason for the decline in density, so we include in X_{st} the one-digit industry employment shares at the state-year level. Finally, to deal with possible unobserved but smooth state-specific changes in technology or other unobservables that may be confounding the estimated relationship, X_{st} also includes state-specific linear and quadratic trends.

Because our Gallup sample size will become small for less populous states, our coefficients may be attenuated due to finite-sample bias in our state-year level union density measures. To address this concern, we use a "split-sample" IV strategy.[39] For each state-year, we split the Gallup observations into two random samples s_0 and s_1, and use the union density calculated from s_1 to instrument the union density calculated from s_0. This procedures yields the following first-stage equation:

[37]The top-ten-percent and labor shares of income are available at the *annual* level, so just as in the time-series regressions we include the interpolated IPUMS-CPS education measure (at the state-year level) as well as the Gallup measure of education for these outcomes (at the state-year level).

[38]We are indebted to Jon Bakija, Stefanie Stantcheva and John Grigsby for facilitating our access to the state-level income tax data.

[39]See Angrist and Krueger (1995) for an early description of the methodology. Inoue and Solon (2010) and Aydemir and Borjas (2011) provide further exposition and applications, respectively.

$$UnionDensity^0_{st} = \eta UnionDensity^1_{st} - \iota \log\left(\frac{N^{Col}_{st}}{N^{HS}_{st}}\right) + \lambda^f X_{st} + \mu_{t\times South} + \delta_s + \nu_{st}. \quad (7)$$

The second-stage equation in the split-sample IV is merely equation (6) with $UnionDensity_{st}$ replaced by $\widehat{UnionDensity}^0_{st}$, the prediction generated from the first stage. Since $UnionDensity^1$ and $UnionDensity^0$ are calculated from a *random* split of the data, the sampling errors in the two measures will be orthogonal. Omitted-variable bias aside, if the only issue is measurement error, the IV estimator β^{IV} will yield a consistent, unattenuated estimate of β. We repeat this procedure 200 times and report bootstrapped estimates and standard errors.

Table 3 shows results from the specification in equation (6) across the state-year analogues of the inequality outcomes used in Table 2. As in the previous subsection, the odd-numbered columns do not include the additional controls X_{st}, while the even-numbered columns do.

Cols. (1) and (2) show results when the college premium is the outcome variable. The coefficient on state-year union density is negative and significant, and the magnitude barely changes whether or not additional controls are included. Indeed, across the male percentile ratios and the Gini coefficient (cols. 3 to 10), the coefficient on state-year density is consistently signed, significant and quite robust to adding additional controls.

We now turn to regressions where state-year measures of top-ten and labor share of income are the outcomes. The first two columns for the top-ten share (cols. 11 and 12) and labor share (cols. 14 and 15) are analogous to all of the earlier outcomes and show a significant, robust negative (positive) coefficient when top-ten (labor) share is the outcome (though the point-estimate for the labor-share regressions is somewhat more sensitive to controls than our other outcomes). Unlike the earlier outcomes, which rely on Census income data and thus cannot extend earlier than 1940, these outcomes allow us to go back further in time, which we do in the third column for each outcome (cols. 13 and 16). Not only can we extend back to 1937 using Gallup density data, but we can also use the 1929 Handbook of American Trade Unions to develop a measure of state-level union density for 1929.[40] While we require microdata for much of the previous analysis, in this section, we need only a state-level measure. Adding 1929 is especially useful because it pre-dates the New Deal and Great Depression, two events potentially linked to both inequality and union density. Cols. (13)

[40]This measure is based on the distribution of union locals across states in 1929. Cohen, Malloy, and Nguyen (2016) construct a similar measure and validate it for a number of states. We provide more details on its construction in Appendix C. The next time the Handbook is available is 1937. We already have our Gallup data from that year, so the Handbook only provides one additional year of data (i.e., 1929).

and (16) replicate, respectively, cols. (11) and (14) and if anything adding this additional year slightly increases the magnitudes on the density variable.[41]

In Appendix Tables A.9 and A.10 we show a variety of specifications that add intermediate sets of controls between the odd and even columns reported in Table 3. These tables also contain a set of estimates (column 1) that do not use the split-sample IV for state-year union density. These estimates verify the presence of attenuation bias, with the split-sample IV coefficients roughly fifty percent larger than the OLS coefficients.

A natural concern is that unions' compression of state-level income distributions comes at the cost of slowed economic growth (e.g., lowered net business entry or capital flight). In fact, union density shows consistently positive, but sometimes insignificant, effects on log state income per capita, and we can rule out even small negative effects of unions on state-level economic activity (see Appendix Table A.11).

The magnitudes in these regressions are substantially smaller than those in the time-series regression, but generally larger than those implied by the distributional decompositions. While we do not have enough microdata to do the decomposition at the state-year level, we think these regressions reveal a lower bound on the extent of spillovers. For the Gini, the state-year effect is about 50% larger than the aggregate effect via the "micro-channel" reported in Appendix F and similar to the estimated size of spillovers at the state-year-industry level in 1968 reported in Section 5.1. We find it reassuring that the ecological regressions and the Fortin, Lemieux, and Lloyd (2018) decompositions yield similar results as to the extent of spillovers, despite having very different underlying samples and methodologies.

5.4 Isolating exogenous policy variation

While quite robust, our state panel analysis so far makes no attempt to isolate plausibly exogenous variation in union density. In this final exercise, we attempt to isolate exogenous components of this variation, focusing on a period highlighted by Goldin and Katz (2008). They note that in the years around World War II, particularly in the 1940s, the decline in inequality "went far beyond what can be accounted for by market forces alone," and they suggest that unions played a role. While Goldin and Katz use aggregate time-series data to identify the 1940s as an anomaly that unions might help explain, our state-year data further support their insight. In Appendix Figure G.1, we show that states that exhibit

[41] We do not replicate the columns with additional controls because not all of our controls go back to 1929. To control for skill shares in 1929, we use the 1940 Census for older ages and project them back to 1929. While migration in between 1929 and 1940 would create error, we use each individual's state of residence in 1935 to construct these projected measures. See Appendix C for more information and validation.

larger increases in union density from 1940-1950 also experience larger declines in inequality.

We progress beyond this correlation in two important ways. First, unlike most existing examinations of the Great Compression phenomenon (e.g., Collins and Niemesh, 2019), which for reasons of data availability have largely focused on inequality as measured in Census years, our use of annual data allows us to precisely examine the timing of state-specific changes in inequality. This higher-frequency analysis enables us to rule out a variety of transitory effects of the mid- and late-1930s and World War II and focus on the rise in union density as a persistent institutional change. We show that unions play an especially important role in compressing inequality from the mid 1930s until the mid 1940s.

Second, we isolate plausibly exogenous components of the increase in density at the state level. In fact, two events account for almost all the mid-twentieth-century rise in private-sector union density in the US: first, the legalization of labor organizing itself (via the 1935 Wagner Act and the 5-4 Supreme Court decision upholding it in 1937) and the spurt of unionization that immediately followed; and, second, the start of the Second World War in Europe, which, as we noted earlier, facilitated union growth because of the massive increase in demand for U.S. industrial production and the federal government's enforcement of pro-union policies at firms receiving defense contracts. We construct two measures that capture the incidence of these two policy shocks across states. First, we define our $\textit{Wagner shock}_s$ as the number of new members added via NLRB elections and large recognition strikes between 1935 and 1938 in state s. This measure isolates the increase in union density driven by worker take-up of the new federal procedures created by the Wagner act, rather than changes due to, say, local variation in the 1938 recession, union friendly state governments, or worker organizing occurring outside the NLRA process. Second, we define our $\textit{War-spending shock}_s$ as the value of defense production contracts from 1940-1945 received by state s. Both terms are defined per capita and then standardized (mean subtracted out and then divided by standard deviation).[42]

These two events provide hope for identification because they both have the following three characteristics: (1) the source of the shock was a *national* policy and thus was not driven by, say, technological changes that could be endogenous to local labor market conditions or pro-union local politicians; (2) despite being driven by the federal government, these two shocks had differential effects across states, providing geographic variation; (3) these differential effects across states do not appear to stem from endogenous variation, as

[42]Gillezeau (2017) looks at state-year persistence in union density over time, also using Gallup data to measure union density in 1939 and 1945 along with data from Troy, and uses state-level war contracts as a cross-sectional instrument. He does not look at inequality nor does he consider a panel specification as we do.

outside of the period of these two national policy shocks, more heavily treated states do not trend differently with respect to union density or inequality measures. Put differently, while we do not claim that these shocks hit a *random* set of states, the pre-existing differences across states do not correlate with differential changes in density or inequality outside of the treatment period. For example, in Appendix G we show that states with larger IV values had greater strike activity since at least 1914 (when the BLS began recording strike data by state), suggesting they indeed may have had greater *latent* demand for unions long before the Wagner Act, and we use pre-1929 strikes interacted with post-Wagner Act as an alternative instrument in the Appendix. However, we show that these strikes were generally *unsuccessful*, and only during about a ten-year window beginning in 1935 (when the federal government briefly takes a pro-union stance) does this latent demand for unions translate into actual growth of union density. We show many more results and robustness checks as well as provide additional historical context in Appendix G; the remainder of this subsection summarizes the key points.[43]

We show results in both changes and levels. For results in changes, our first-stage specification is:

$$\begin{aligned} Union_{st} - Union_{s,t-9} = & \beta_1 \, Wagner\ shock_s \times \mathbb{I}_t^{t=1938} + \beta_2 \, War\text{-}spending\ shock_s \times \mathbb{I}_t^{t=1947} \\ & + \gamma_1 \, Wagner\ shock_s + \gamma_2 \, War\text{-}spending\ shock_s \\ & + \lambda_{r(s)t} + \gamma \left(log\Big(\frac{N_{st}^{Col}}{N_{st}^{HS}}\Big) - log\Big(\frac{N_{st-9}^{Col}}{N_{st-9}^{HS}}\Big) \right) + \eta \mathbb{X}_{st} + e_{st}, \end{aligned} \tag{8}$$

where the outcome variable is a *nine-year* change in union density in state s (as 1929 is our first year of union data, the intervals in our regression sample are 1929-1938, 1938-1947, 1947-1956, etc.). The *Wagner* and *War-spending* shocks are defined above, their interactions with $\mathbb{I}_t^{t=1938}$ and $\mathbb{I}_t^{t=1947}$, respectively, are the two instruments, $\lambda_{r(s)t}$ are Census-region-by-year fixed effects, and $\mathbb{X}_{st}$ are other controls that we vary to probe robustness. We also include the (interpolated) state-year skill shares, with coefficient γ for all specifications as in our state-year regressions above. Note that we generate estimates for 1929 state skill shares based on the 1940 Census, adjusting for age and migration (details in Appendix C). Our identifying assumptions are that the passage and unexpected upholding of the Wagner Act pushes union density upward from 1929 to 1938 but not otherwise, and that the war-spending shock pushes density upward from 1938 to 1947 but not otherwise.

Using nine-year intervals may seem odd, but it is done intentionally, as it allows us to

[43]We also experimented with Right-to-Work laws as an alternative instrumental variable, but found no sufficiently robust effect of Right-to-Work on union density. We provide an extensive discussion of these results and the Right-to-Work literature in Appendix I.

avoid shocks related to the Great Depression and our period of missing data for state-year density (1930-1936), as well as avoid any year with war-related wage controls (1942-1946). Beyond the union-friendly policies that we use as identification, defense production may have also increased demand for low-skilled workers, which might itself temporarily lower inequality and is another reason to avoid those years. In the IV analysis it is especially important to include 1929, as it gives us a pre-Wagner Act datapoint, so the intervals 1929-1938 and 1938-1947 present the natural starting point to our sample period.

The first two columns of Table 4 show the results of estimating equation (8). Col. (1) is our preferred specification and shows that the two interaction terms substantially shift upward union density in the appropriate window (i.e., the *Wagner-Act shock* during the 1929-1937 window and the *War-spending shock* during the 1938-1947 window). Importantly, the *main effects* of the *Wagner* and *War-spending* variables are not significant and in fact wrong-signed, meaning that outside of the specific windows captured by the interaction terms, *Wagner* and *War-spending* states are not predisposed to union-density growth. Note that the two IVs are highly correlated—states that saw larger density gains immediately after Wagner also received more defense contracts, as we show in a scatter plot in Appendix Figure G.4—so showing that the two IVs have an effect in the appropriate window is not a trivial test. Col. (2) shows that the first-stage F-statistic is even larger when the clear outlier in Figure G.4, Michigan, is excluded from the sample. We do further checks of the first-stage in Appendix G.

The top-ten share is the outcome in cols. (3) to (5). In col. (3), we show as a baseline the OLS results for the treatment-period intervals only. Despite the fact that measurement error in the un-instrumented changes in density surely result in attenuation bias, the OLS effect of changes on density on changes in top-ten share is large, negative and significant.[44] The IV analogue is larger in magnitude, and reported in col. (4), with col. (5) showing robustness to dropping Michigan. As we saw in cols. (1) and (2) with union density, we again see that Wagner and war-spending states are not *generally* disposed to reducing the top-ten income share, as the main effects of *Wagner* and *War-spending* are small and insignificant. But the (instrumented) effect of union density is large, negative, and significant, suggesting that a ten percentage-point increase in a state's union density leads to a six percentage-point decline in top-ten share of income.

Why would the IV be larger than the OLS in our context? First, the IV addresses measurement error, especially important in a panel setting. Second and more interesting, the

[44] The OLS results for the full period remain negative and significant, but smaller, as one would guess from the state-year OLS results in Table 3. The same pattern holds true for the labor-share results in col. (6) of this table.

endogeneity of union density in this context likely masks some of the effect of unions on inequality. As we show below, in the 1920s, states that would later be hit with our IV shocks had *greater* top-ten shares; and as we show in Appendix Figure G.6, they also had more strikes in the 1920s (though almost all were unsuccessful in this pre-Wagner era). These results suggest that, absent the federal intervention embodied in our IVs, inequality and worker agitation *positively* correlated, leading OLS estimates to understate the effect of union density on inequality, at least in this critical period.

Finally, cols. (6) through (8) shows the analogous results for labor-share. We find the same pattern (OLS and IV agreeing in sign and all highly significant, with the IV results larger in magnitude).

We also show higher-frequency results in levels. As noted, our two policy shocks are highly correlated, so in this higher-frequency analysis we pool them. (They are already standardized, so we merely sum them.) We then regress our union density measure on the pooled IV along with region fixed effects, separately by year. We repeat the same exercise with the top-ten and labor share of income. As we are now examining outcomes in levels, not changes, our hypothesis is that the only period in which the relationship between union density and the pooled IV should exhibit a sustained *increase* is during the treatment period (i.e., from the 1935 passage of Wagner until the end of the war). Similarly, the only period in which we should observe a sustained decrease (increase) in the relationship between the top-ten (labor) share and the pooled IV is during the treatment period.

The first series in Figure 10 shows the relationship between union density and the pooled policy shock IV. While we have limited pre-period data on union density, there is little correlation with our IV and our proxied union density in 1929; thus, states that would soon be hit by these shocks were not, to the extent we can measure, historically union friendly. However, beginning in the late 1930s and continuing through the war years, the IV is associated with a massive increase in union density. By 1947, most of the increase is complete. There is little backsliding (and minimal further growth), so states that enjoyed these one-time shocks remain (relatively) union-heavy states. Of course, we might discount any effect *during* the war, given the command economy during this time, but the rise in union density appears sticky. It remains high despite the fact that the default enrollment and other war-time policies end in 1945. In Panel A of Appendix Figure G.3 we use our microdata to show that this relationship is not driven by an ecological bias: households in treated states select into unions at a higher level in the treatment period, even conditional on the household covariates included in our selection regressions above.

The next series presents the same reduced-form results for the top-ten share—showing the coefficient separately by year when it is regressed on the pooled IV and region fixed effects. A nice feature of this reduced-form analysis as opposed to the IV analysis is that we can show the relationship between outcome variables and the IVs even in years when we do not observe union density. Indeed, as we noted while discussing Table 4, Figure 10 shows that our IV predicts *higher* top-ten-share inequality from 1917 until the mid-1930s. However, over the treatment period, the coefficient on the IV dramatically declines and recovers slowly over the rest of our sample period. The evolution of labor share tells a similar story, though due to data limitations we have a shorter pre-period. Over the treatment period, the relationship between state labor share and the pooled IV climbs sharply. Again, in our analysis in changes in Table 4 we avoided years directly touched by the war, but this annual analysis clearly shows that wartime declines in inequality *endure*, even though all formal war-time labor market policies were abolished by 1946.

The primary threat to the analysis in Table 4 and Figure 10 is that states that are differentially unionizing in response to the Wagner Act and defense production are also experiencing other state-specific shocks that reduce inequality over this period. In Appendix G we rule out a large number of such confounds by allowing a variety of state characteristics to have time-varying effects and by controlling for contemporaneous policies (e.g., state-level minimum wages and state-level top marginal tax rates) and politics. In particular, as state manufacturing is an obvious potential confound, we show results allowing states' pre-treatment-period or contemporaneous manufacturing employment to have a different effect in each year. Nor is manufacturing employment a *mediator* of the relationship between inequality and union density: the relationship of the IV variables with manufacturing employment is stable from 1910 to 1950 (see Appendix Figure G.10), with a very short-lived increase during the war. This long-run stability is in sharp contrast to the dramatic and *persistent* increase (decrease) in the pooled IV variable's relationship with union density and labor share (top-ten share). In Appendix Figure G.11 we also show that the pooled instrument is uncorrelated with probability of Democratic governor. Appendix Table G.4 further shows that in the reduced form regressions, each instrument affects the outcome variable *only* in the relevant time period, and both instruments independently yield qualitatively similar results. We also provide evidence from our Gallup opinion data that egalitarian or pro-worker norms or beliefs are not significantly affected by our instruments or by the war years more generally. Public attitudes toward union (business) grew more negative (positive) during the war, if anything more so in states hit by our IV shocks (see the discussion in Appendix G.5.4 and the regression results in Appendix Table G.8).

In terms of magnitudes, the IV estimates are quite large. A ten-percentage-point increase in union density leads to a roughly 5-6 percentage-point decrease in the top ten share of income and a 2-3 point increase in the labor share. These magnitudes would imply that the

16 percentage point increase in union density between 1940 and 1970 could explain 80-90% of the fall in the top-ten percent share in the same period, and over 100 percent of the increase in the labor share. Applied to the modern period, the same magnitudes would imply that the 14 percentage-point de-unionization between 1970 and 2014 can account for 65-70% of the increase in the top ten percent share and 60-85% of the fall in labor share.

We note two complications in interpreting these state-year regressions (which also apply to the state-year OLS regressions in Section 5.3) and in particular whether they lead to overstatement of the effects of density. First, migration of individuals with high capital income from more to less unionized states in response to these policy shocks would lower inequality in more unionized states, but do nothing to lower national inequality. If, as seems likely, most such moves are within Census region, then our region x year fixed effects in fact exacerbate the problem. To probe this issue, we re-run the regression without the region x year fixed effects, and in fact the estimates are larger, suggesting that within-region negative spillovers are small or are offset by, say, unskilled, low-wage laborers moving to newly unionized states. Second, our state-year results do not necessarily "aggregate up" to the aggregate effect on inequality (e.g., unions' reducing the top-ten share in the richest states will have a much larger effect on the national top-ten share than their reducing the top-ten share in a poor state like Mississippi (whose 90th percentile during much of our sample period is not much higher than the national median). However, the IV estimates are even larger when weighted by state-population or income (see Appendix Table G.3), suggesting that this aggregation bias is not generating an overestimate of the effect of unions.

How could unions reduce inequality so drastically in this period? The LATE identified by these two shocks likely differ from that induced by other variation in union density. During our treatment period, unions organized the "superstar" firms (Autor et al., 2020) of their day. Appendix Figure G.2 shows the number of the four largest companies with major union contracts, both by employment and market capitalization. The increase in union coverage among the largest firms over the treatment period is far more dramatic than the overall rise in union density (as displayed in Figure 2). In our treatment period, unions won recognition at (and negotiated national contracts with) the largest companies in America—General Motors, Ford, US Steel, and AT&T. The resulting decrease in inequality (as measured by top ten share) could well be disproportionate: for example, large firms may exercise standard-setting influence in their sectors or have, by dint of their scale, low non-supervisory labor share and high payments to CEOs and shareholders. This latter explanation is consistent with Frydman and Molloy (2012), who argue unionization was the primary restraint on CEO pay in this period. This explanation is also consistent with the smaller effects when Michigan is dropped, as the large auto companies based in that state were the largest employers in the country and became unionized in our treatment period. While the absence of detailed microdata—and in particular firm identifiers—makes it difficult to distinguish precise mechanisms, we find these results intriguing and worthy of future work.

Further, unions during our treatment period were organizing sectors experiencing an influx of less educated workers, including Black workers from the Great Migration. Panel B of Appendix Figure G.3 shows that the increase in union density driven by the instruments was disproportionately among non-white households.[45] The expansion of unions induced by the Wagner Act and National War Labor Board brought unusually high numbers of Black workers into unions, and the larger effects of unions on Black (and other less educated) workers incomes documented in Section 4 would imply a particularly large LATE on labor share and top incomes.

Mechanically, if the increase in union density driven by these policies had a larger effects on wages than the average increase in density, then the large magnitudes, at least on the labor share measure, could be readily explained. The effect of a one percentage point increase in union density on the labor share of income (assuming no other effects on income per capita or employment) would be close to the vicinity of our IV coefficients, even without spillovers.[46] While we lack the data to examine the effects separately on non-union vs union members, it is clear that the IV estimate magnitudes are not implausibly large.

6 Conclusion

We leverage historical polling data, allowing us to provide a systematic, representative study of unions' effects on the income distribution over a much longer period than existing work. A combination of low-skill composition, compression, and a large union income premium made mid-century unions a powerful force for equalizing the income distribution. We show that unions were a major force in the Great Compression, above and beyond what can

[45]We would have liked to repeat this exercise for education as well as race, but we do not have education information in Gallup until 1942, more than halfway through our treatment period.

[46]Labor share is given by $(w^u u + w(1-u))\frac{L}{Y}$ where u is union density, w (w^u) is the non-union (union) wage, L is employment, and Y is total income. If union premium is 25% as in our main estimate from 1942, then the mechanical effect of a ten percentage point increase in union density on labor share, starting from a base (union density $u = 0$) labor share of 0.7, is $0.1 \times 0.25 \times \frac{wL}{Y} = 0.017$, over half the effect in Table 4. Suppose that unions are organizing firms and workers that pay much higher premia in levels. Quantitatively, this combination would imply a large increase in the level of union wages, putting a 30% union premium due to negative selection (Table A.2 Column 5) on top of a 20% largest-firm premium, as reported in Bloom et al. (2018). The firm-size premium is well-documented for even non-union firms (Brown and Medoff, 1989), and a constant union premium applied to a higher wage would result in a larger increase in labor share. Together these two effects would increase the change in the labor share to 0.025, close to the estimate in column 8 of Table 4.

be accounted for by the direct effect of unions on union members. We leverage cross-state instruments from the two policy shocks that explain almost all the increase in 20th century union density, and find that they have large effects on inequality as measured by the labor share or the top income share, further providing evidence that unions affect moments of the income distribution beyond what can explained by their effects on union members alone.

The famous U-shape in U.S. economic inequality over the twentieth century has been the object of a large and distinguished literature adjudicating the roles of supply-and-demand of skilled labor versus changes in labor-market institutions such as unions. Our results push the body of evidence towards the conclusion that institutions can have substantial and lasting effects on the income distribution, while also confirming a significant role for relative skill supplies. We believe the large and immediate effects of the Wagner Act and War Labor Board we find are hard to attribute to more secular and slower-moving changes like skill shares, but an important question would be how the subsequent rise in education triggered by the GI Bill helped sustain these low levels of inequality.

Looking forward, recent events suggest a spurt of grass-roots organizing activity, from the Covid-related mass walkouts at Amazon distribution centers and wildcat strikes at Tyson and other meat-processing plants to the wave of teachers strikes in 2018 and 2019. The configuration of crisis and mobilization targeting the country's largest firms recalls the 1930s, though our results suggest that without legal or other institutional changes at the federal level, translating this activity into growth in union density or coverage will be difficult.

We welcome future work that develops theoretical models explaining the joint evolution of union density, skill composition, premia, and overall inequality that we have documented. More work on the effect of unions, perhaps in light of the recent literature documenting pervasive labor market power (Manning, 2020), would inform whether unions could be an important part of a feasible policy package to lower inequality.

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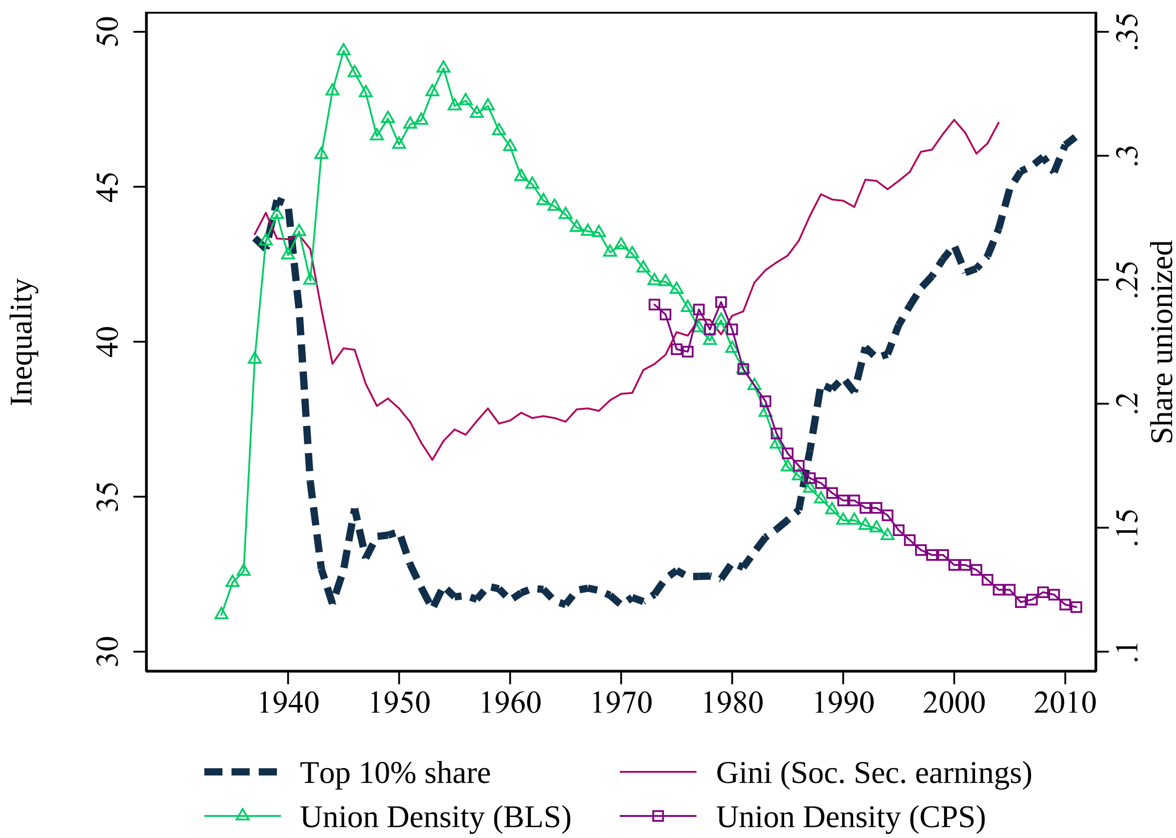
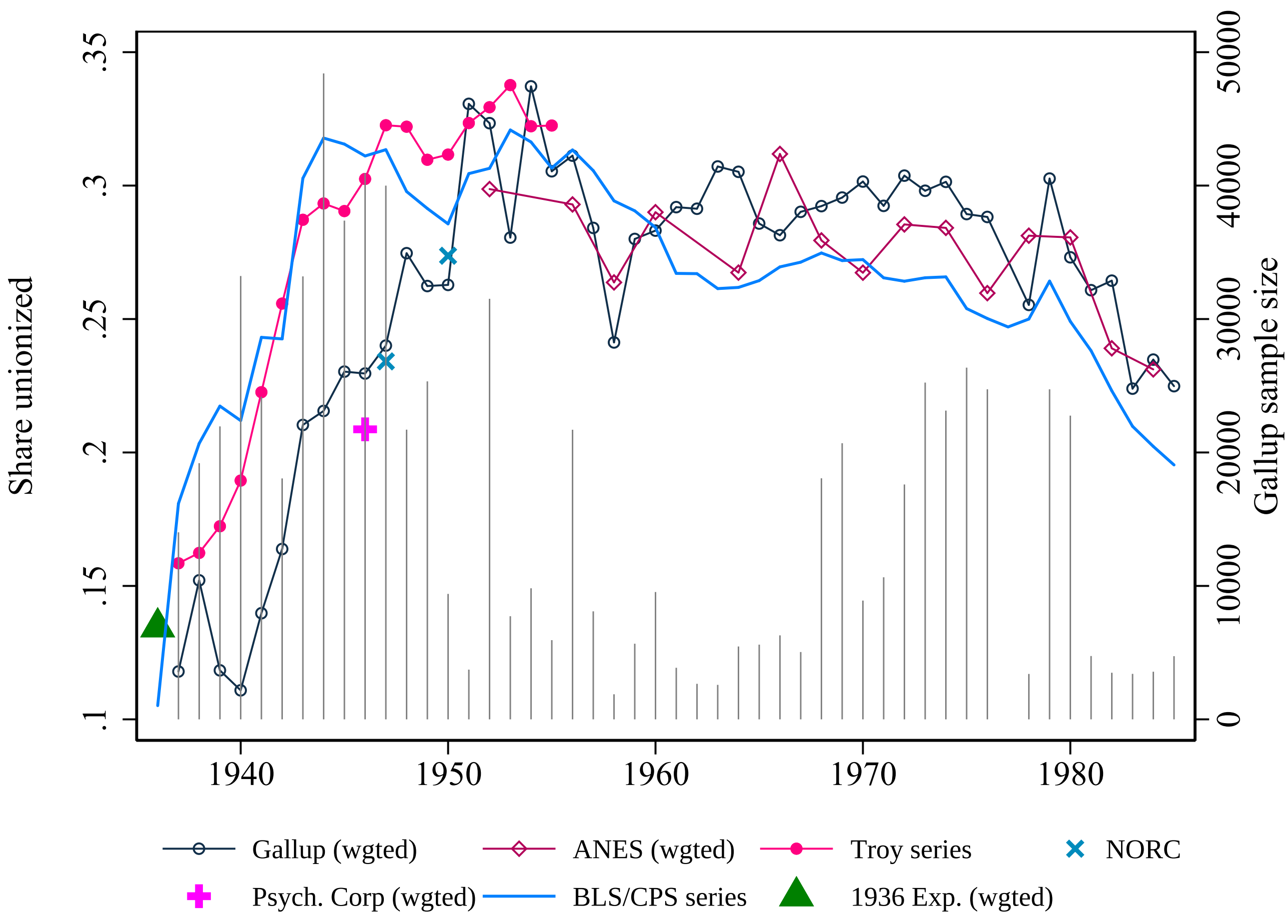
Figure 1: Union density and inequality measures, 1935-2011



Sources: Top share inequality from Piketty and Saez (2003, updated 2016). Union density data from Historical Statistics of the United States and the Current Population Survey. We discuss these data sources in detail in Section 2.2 and Appendix E.

Notes: In this figure, the top-ten and Gini statistics go from zero to 100 and the union density from zero to one.

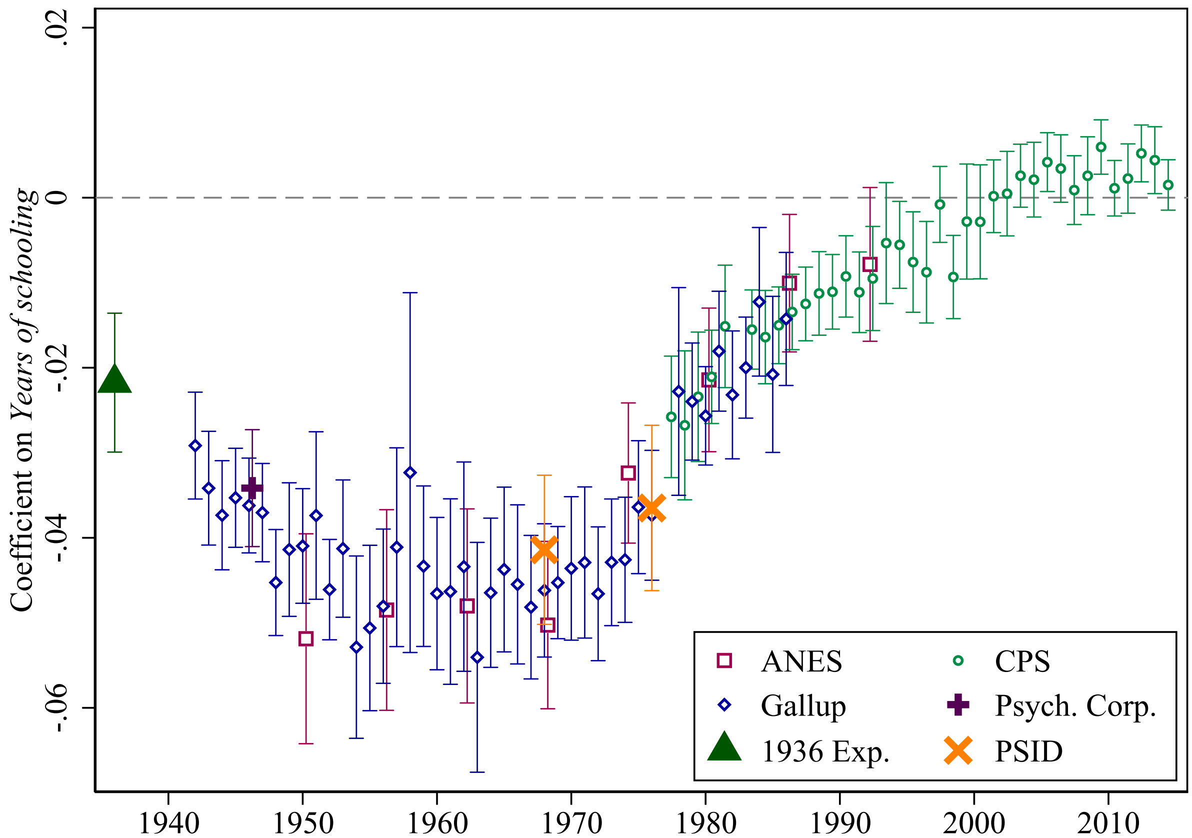
Figure 2: The share of households with a union member, comparing our survey-based measures to existing time series, 1936-1985



Data sources: See Sections 2.2 and Appendices B and E.

Notes: No sample restrictions are imposed (so farmers and those over age 65 *are* included in this graph). The vertical spikes indicate the number of Gallup observations per year *that include the union variable* (plotted on the right-hand-side axis). The existing time series (the BLS and Troy measures) are *counts* of union members, so we divide them by Census estimates of the number of households (geometrically interpolated between Census years) to make them as comparable as possible to our household membership series. The Gallup, ANES, 1936 Expenditure and Psychological Corporation are all weighted, either with survey-provided weights or to match Census demographics as described in Section 2.2 and Appendix B.

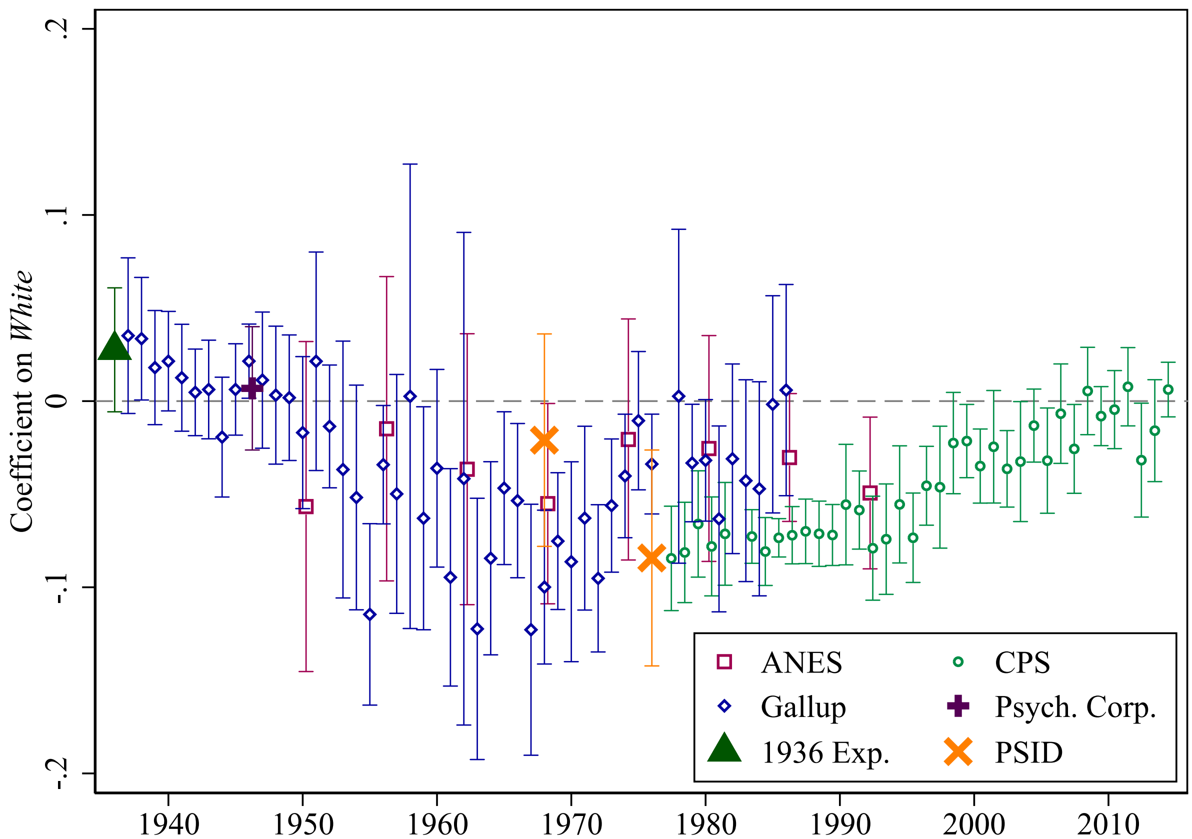
Figure 3: How does years of schooling predict union household status?



Data sources: Gallup data, 1937–1986; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 2.2 for a description of each data source.

Notes: We regress household union status on $Years\,of\,education$, state s and survey-date t fixed effects, age and its square, and gender (all demographics refer to the survey *respondent*). We estimate this equation separately by survey source and by year. Some survey sources give actual years of schooling. For those that do not, we impute in the following manner: six years for "less than middle school;" eight years for "middle school;" ten years for "some high school;" twelve years for "high school;" fourteen years for "some college" or "vocational training;" sixteen years for "college," eighteen years for "more than college." The figure plots the coefficient on $Years\,of\,education$. For the ANES, because the samples are smaller, we group surveys into six-year bins. The plotted confidence intervals are based on standard errors clustered by state.

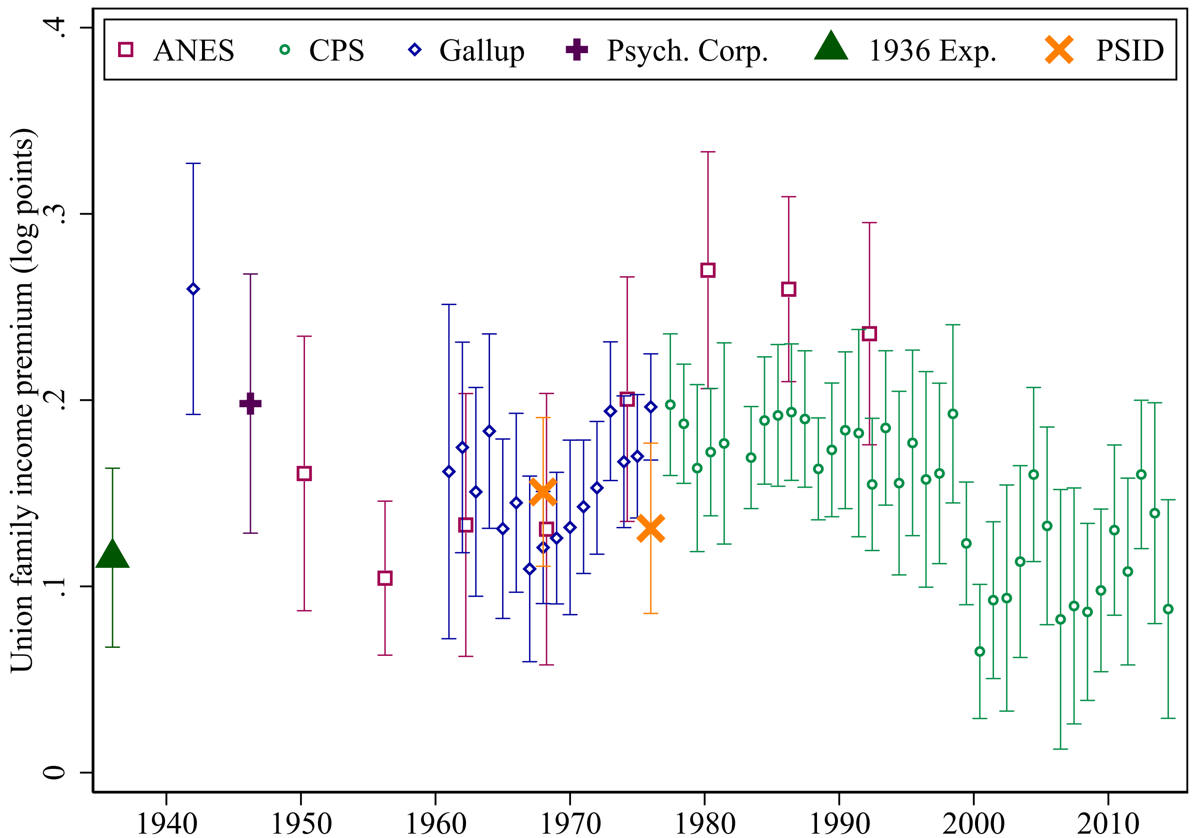
Figure 4: How does race predict union household status?



Data sources: Gallup data, 1937–1986; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 2.2 for a description of each data source.

Notes: For each data source, we estimate (separately by year if a data source has multiply years), household union status on a *White* dummy variable, state s and survey-date t fixed effects, age and its square, and gender (all demographics refer to the survey *respondent*). We plot in this graph the coefficients on $White$ from each of these estimations. For the ANES, because the samples are smaller, we group surveys into six-year bins. Confidence intervals are based on standard errors clustered by state.

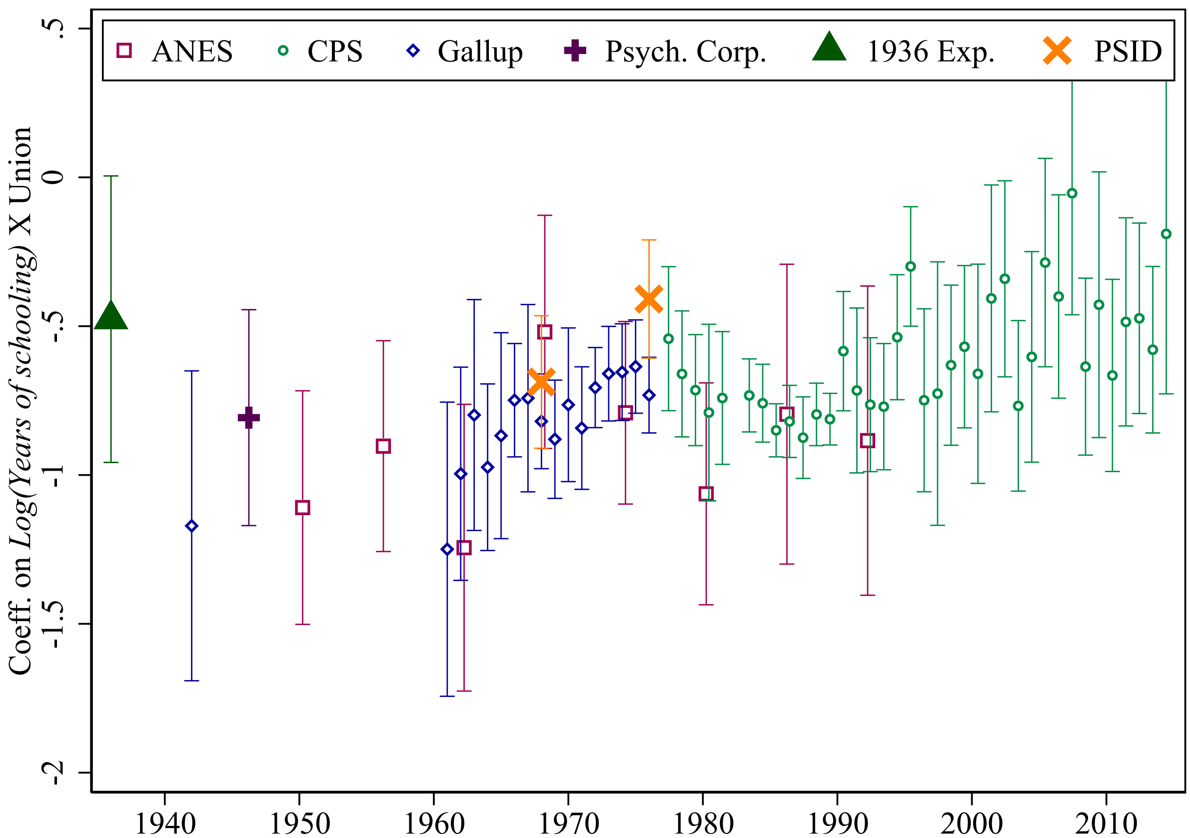
Figure 5: Estimates of the union family income premium



Data sources: Gallup data, 1942, 1961–1976; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 2.2 for a description of each data source. See Appendix C for details on CPS family income variable construction.

Notes: Each plotted point comes from estimating equation (2), which regresses log family income on *household union status*, with controls for age, gender, race, state and survey-date fixed effects. Occupation controls are not included as they are not consistent across data sources or within data sources across time. We estimate a separate regression for each survey source and year. For the ANES, because the samples are smaller, we group surveys into six-year bins. The plotted confidence intervals are based on standard errors clustered by state.

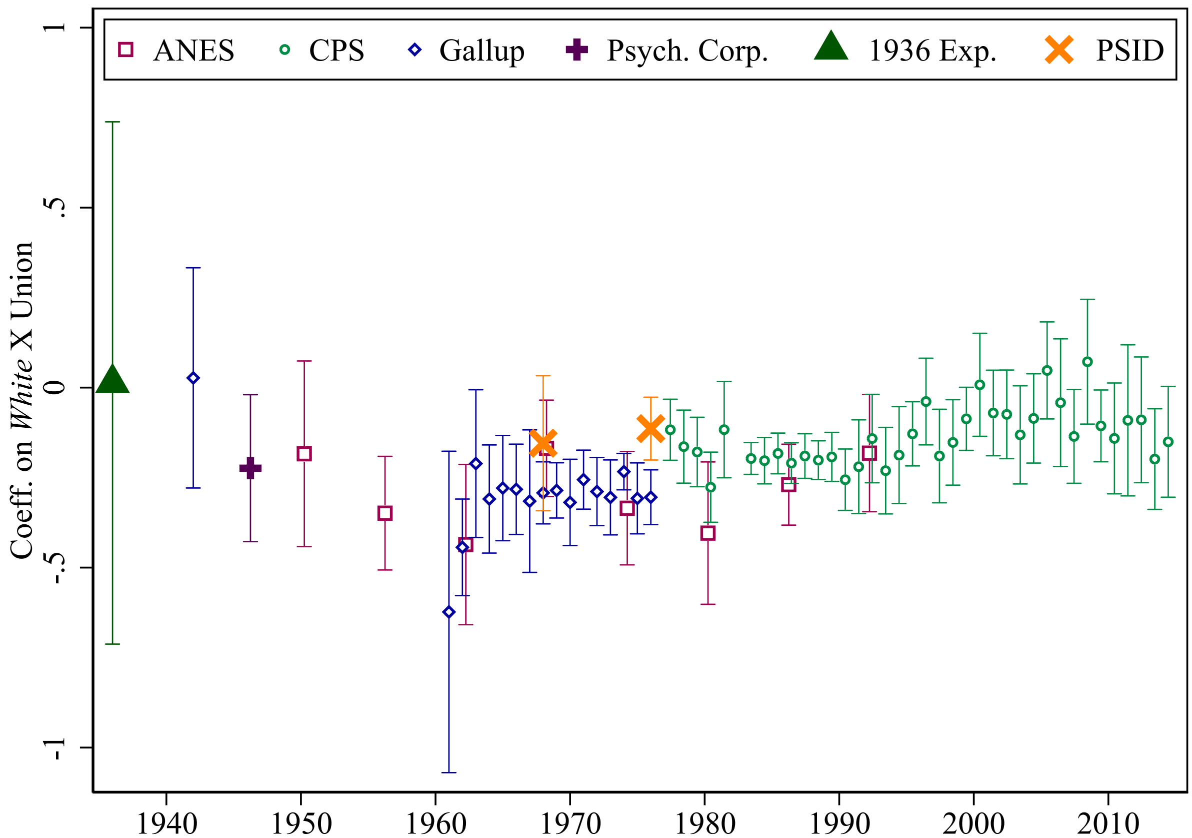
Figure 6: Differential family union premium by respondent's log years of schooling



Data sources: Gallup data, 1942, 1961–1976; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 2.2 for a description of each data source. See Appendix C for details on CPS family income variable construction.

Notes: Each plotted point comes from estimating an equation regressing log family income on household union status, its interaction with respondents' log years of schooling, and all other controls in equation (2). We estimate this equation separately by survey source and by year. (The notes to Figure 3 describe how we impute years of schooling if the survey source only gives us categories of educational attainment.) The figure plots the coefficient on the interaction $Years\,of\,schooling \times Union$. For the ANES, because the samples are smaller, we group surveys into six-year bins. The plotted confidence intervals are based on standard errors clustered by state.

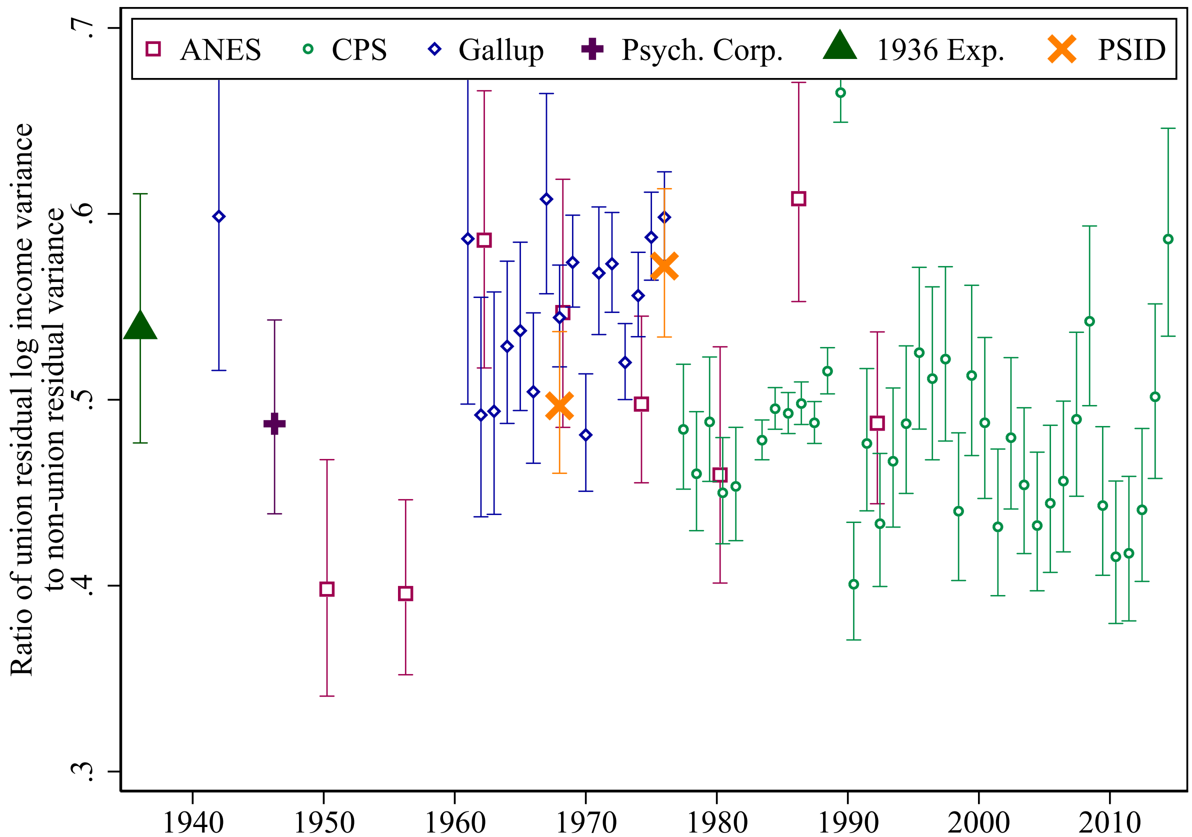
Figure 7: Differential family union premium for whites relative to minorities



Data sources: Gallup data, 1942, 1961–1976; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 2.2 for a description of each data source. See Appendix C for details on CPS family income variable construction.

Notes: Each plotted point comes from estimating an equation regressing log family income on household union status, its interaction with a White dummy variable, and all other controls in equation (2). We estimate this equation separately by survey source and by year. The figure plots the coefficient on the interaction $White \times Union$. For the ANES, because the samples are smaller, we group surveys into six-year bins. The plotted confidence intervals are based on standard errors clustered by state.

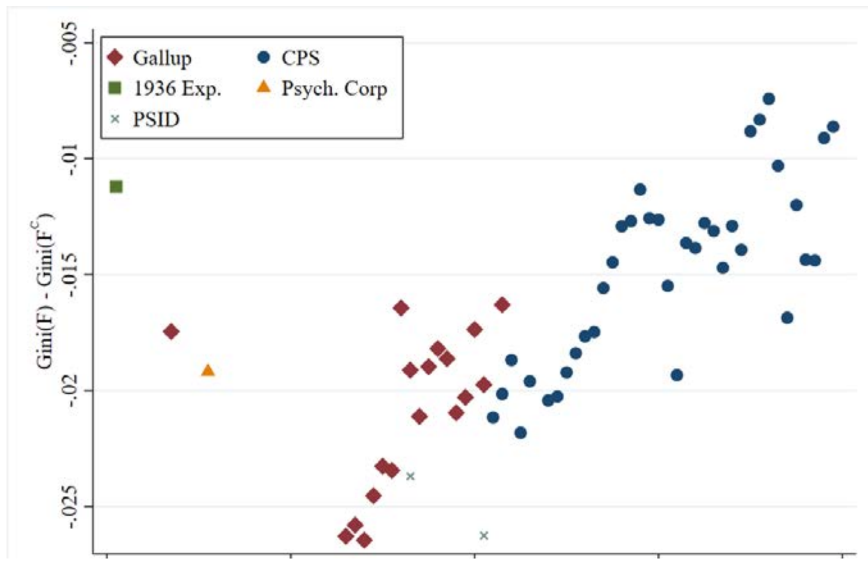
Figure 8: Ratio of residual variance between union and non-union sectors



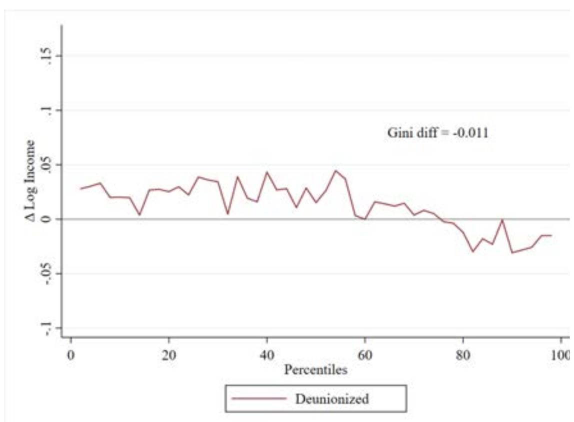
Data sources: Gallup data, 1942, 1961–1976; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 2.2 for a description of each data source. See Appendix C for details on CPS family income variable construction.

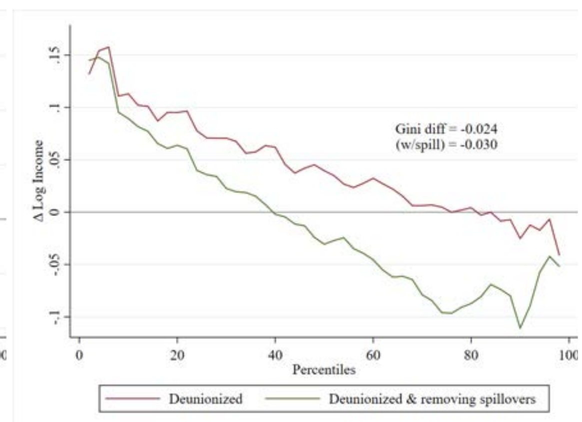
Notes: Each plotted point is the ratio of variance of residuals from regressing log family income on the controls in equation (2) separately for union and non-union households. As usual, we perform this analysis separately by survey source and year. See Section 4.4 for more detail. The figure plots the ratio of the variance of residuals in the union sector to that of the non-union sector (so ratios less than one suggest that residual variance in the union sector is more compressed than in the non-union). The plotted confidence intervals are based on inverting the F-statistic testing the null that the ratio is equal to 1. For the ANES, because the samples are smaller, we group surveys into six-year bins.

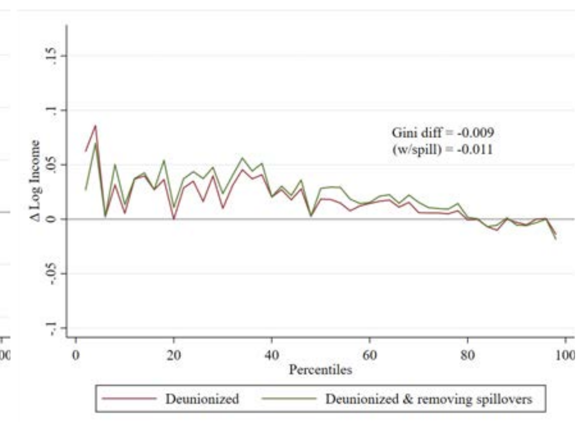
Figure 9: Actual vs. “no-unions” counterfactual income distributions



(a) Yearly union impact (assuming no spillovers to non-union households)







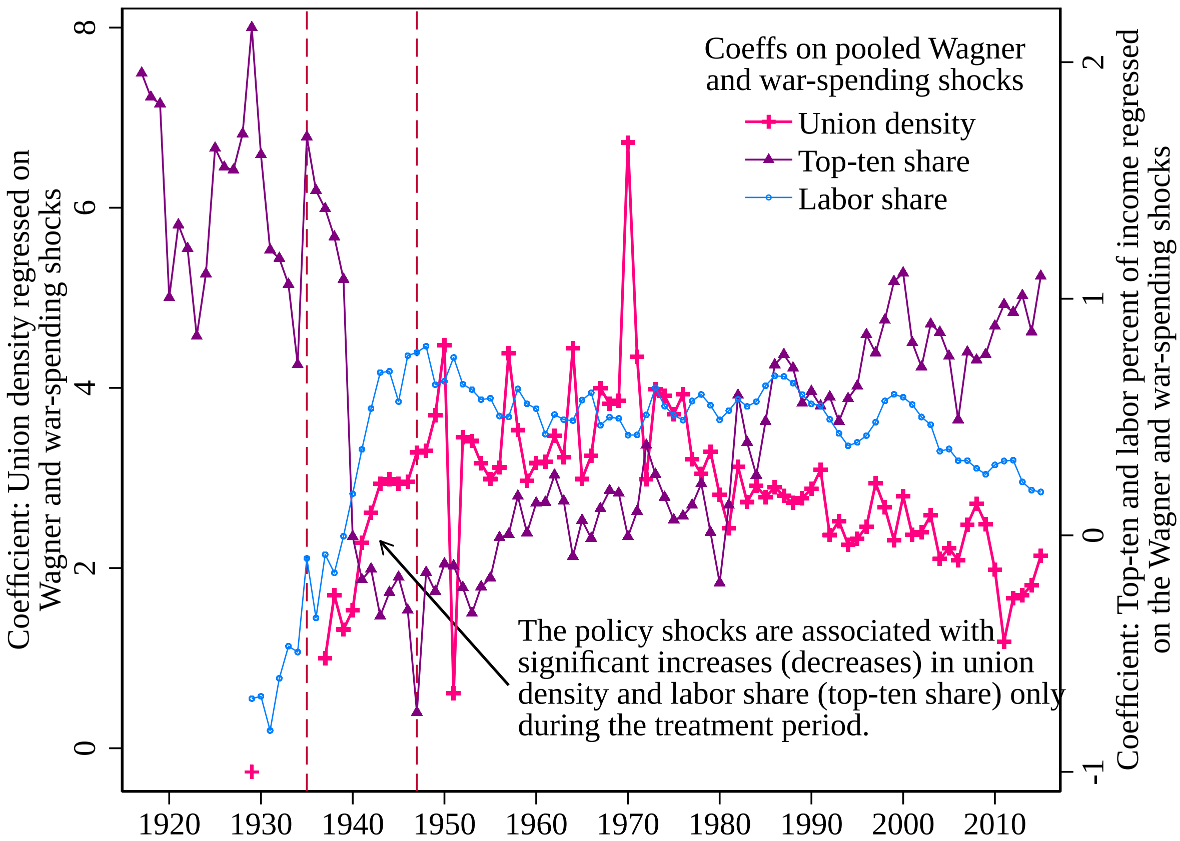
(b) 1936 (Exp. survey)

(c) 1968 (PSID)

(d) 2014 (CPS)

Note: This figure compares the observed population (F_Y) and the counterfactual population without unions (F_{Y_n}) in selected years. Panel F.1a plots yearly differences in true and counterfactual Gini coefficients. Panels F.1b, F.1c, and F.1d plot differences in true and counterfactual log-income percentiles for selected years. Income is denominated in 2014 dollars using CPI.

Figure 10: Regressing density and inequality outcomes on the pooled policy shock variable



Sources: Union density data from Gallup and CPS, except for 1929 (see Section 5.3 and Appendix B for construction of 1929 density, which follows Cohen, Malloy, and Nguyen (2016)). Top-ten income data are from Frank (2015). See Appendix H for construction of state-level labor share of net income.

Notes: Each point on this graph is the estimated coefficient β_y from the following regression:

$$\sum_{y \leq 2014} \beta_y IV_s \mathbb{I}_t^{t=y} + \lambda_{r(s)t} + e_{st}$$

where Y_{st} is the outcome variable (state-year union density, top-ten income share, or labor share of income); IV_s is the pooled policy shock variable (our "Wagner Act shock" and our "war-spending shock" both standardized, then summed); $\mathbb{I}_t^{t=y}$ are year fixed effects; the summation runs over all years y in the sample period (1929 and 1937-2014 for union density; 1929-2014 for labor share; 1917-2014 for top-ten income share); and $\lambda_{r(s)t}$ is a vector of Census *region* $\times$ *year* fixed effects. Note that these regressions are equivalent to regressing, separately for each year, the outcome variable on the IV and region fixed effects. We multiply union density by 100 to be on the same scale as labor share. However, in most tables (e.g., Tables 2 through 4) density is between zero and one to conserve table space by avoiding coefficients with multiple zeros after the decimal point.

Table 1: Decomposition of Change in Gini

	Time Period (1)	*Total Change in Statistic* (2)	*Change Attributable to:* Δ Union Wages (3)	Δ Unionization (4)	Total Union Effect (5)
Panel A: Gini	1936 to 1968	-0.0526	0.00331	-0.0158	-0.0125
			(-6.290)	(30.06)	(23.77)
	1968 to 2014	0.144	0.00904	0.00603	0.0151
			(6.278)	(4.188)	(10.47)
Panel B: 90/10	1936 to 1968	-0.188	0.0115	-0.0986	-0.0871
			(-6.127)	(52.47)	(46.34)
	1968 to 2014	0.817	0.0931	0.0366	0.130
			(11.39)	(4.474)	(15.87)
Panel C: 90/50	1936 to 1968	-0.102	0.0254	-0.0443	-0.0188
			(-24.83)	(43.21)	(18.38)
	1968 to 2014	0.360	0.0226	0.0207	0.0434
			(6.297)	(5.760)	(12.06)
Panel D: 10/50	1936 to 1968	-0.0855	-0.0139	-0.0544	-0.0683
			(16.27)	(63.57)	(79.84)
	1968 to 2014	0.458	0.0705	0.0159	0.0863
			(15.40)	(3.464)	(18.86)

Note: This table reports the union-related components of decompositions of changes in Gini coefficient over time. Each row represents a separate decomposition. Column 1 specifies the beginning and end years of the decomposition. Column 2 reports the total change in computed Gini coefficient. Column 3 reports the change in Gini attributable to changes in union versus non-union incomes. Column 4 reports the change in Gini attributable to changes in the conditional unionization rate. Column 5 reports the total effect of both union wage changes and unionization (Column 3 + Column 4). Numbers in parentheses report components as a percentage of total change in Gini coefficient.

Table 2: Aggregate inequality as a function of union density

	Dependent variable:													
	Coll. premium		90/10 ratio		90/50 ratio		10/50 ratio		Gini coeff.		Top 10 share		Labor share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Union density	-1.258***	-1.585***	-2.203***	-1.736***	-0.346	-0.281	1.857***	1.455***	-0.162***	-0.141***	-35.90***	-23.34**	48.33***	45.21***
	[0.402]	[0.494]	[0.360]	[0.628]	[0.350]	[0.362]	[0.365]	[0.536]	[0.0387]	[0.0373]	[13.06]	[11.40]	[11.59]	[13.15]
Skill share	-0.535***	-0.585***	-0.0443	0.156	-0.270***	-0.177**	-0.226**	-0.332**						
	[0.0768]	[0.106]	[0.0756]	[0.124]	[0.0862]	[0.0718]	[0.112]	[0.135]						
Mean, dept. var	0.482	0.482	1.434	1.434	0.669	0.669	-0.765	-0.765	0.410	0.410	36.589	36.589	73.104	73.104
Annual edu. controls?	No	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Addit. controls?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Cubic polynomial?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Min. Year	1940	1940	1940	1940	1940	1940	1940	1940	1940	1940	1940	1940	1940	1940
Max. Year	2016	2016	2016	2016	2016	2016	2016	2016	2004	2004	2014	2014	2015	2015
Observations	56	56	56	56	56	56	56	56	65	65	75	75	76	76

Sources: For cols. (1) - (8), outcome variables generated from Census IPUMS and CPS; for cols (9) and (10) from Kopczuk, Saez, and Song (2010); for cols (11) and (12) from Piketty and Saez (2003, updated 2016) and for cols (13) and (14) from Piketty, Saez, and Zucman (2018). The union density explanatory variable is the simple average between the Gallup- and BLS-based density measures (see Section 5.2 for detail).

Notes: Note that union density is out of one (not 100) to conserve table space by avoiding coefficients with zeros after the decimal. All regressions include controls for the log share of college versus high-school educated workers, calculated in the early years from Census IPUMS and for later years from the CPS. The first four columns use outcome variables calculated from the source (so are only available in Census years until the CPS), but the last four columns use *annual* measures as outcomes , calculated from administrative data. For these measures, we have to control *annually* for skill shares. We include two annual controls: annual skills shares as measured in Gallup and annual skills shares as measured in the Census IPUMS and the CPS (interpolated between Census years in the pre-CPS years). As these two measures are correlated, we do not report their coefficients because they are hard to interpret (and are not the variables of interest). For each outcome variable, the first specification has *parsimonious* controls (only a time cubic and the skill shares controls) and the second has *additional* controls (federal minimum wage, the national unemployment rate, and the top marginal tax rate in the federal income tax schedule). Appendix Tables A.7 and A.8 provides additional specifications using the skill premium, the log percentile ratios, the Gini coefficient, the top-ten share and the labor share as outcomes. Note that the log 90/10, 90/50, and 10/50 ratios are for men only, but all other inequality measures pool both men and women. Standard errors are robust to heteroskedasticity and AR(1) serial correlation. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table 3: State-year inequality as a function of union density

	Dependent Variable:															
	Coll. prem.		90/10 ratio		90/50 ratio		10/50 ratio		Gini coeff.		Top 10			Labor share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Household	-0.180	-0.205	-0.349**	-0.312**	-0.142	-0.125	0.206*	0.187*	-0.054**	-0.053**	-4.207**	-3.422**	-4.716**	5.543***	3.882**	5.834***
union share	[0.134]	[0.126]	[0.167]	[0.148]	[0.088]	[0.086]	[0.112]	[0.100]	[0.027]	[0.022]	[1.911]	[1.659]	[1.985]	[1.874]	[1.770]	[1.889]
Mean	0.817	0.817	1.985	1.985	1.045	1.045	-0.382	-0.382	0.530	0.530	64.810	64.810	65.860	86.374	86.374	86.374
Controls?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	No	Yes	No
Min Year	1940	1940	1940	1940	1940	1940	1940	1940	1940	1940	1937	1937	1929	1937	1937	1929
Max. Year	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014	2014
Observations	1,962	1,962	1,962	1,962	1,962	1,962	1,962	1,962	1,962	1,962	3,544	3,544	3,591	3,544	3,544	3,591

Sources: For cols. (1) through (10), dependent variables created using Census and CPS data. Note that the Gini coefficient used in Table 2 is not available at the state level, so in cols. (9) and (10) we calculate a state-level annual Gini from the Census and CPS. For cols. (11) through (13) outcome variables are taken from Frank, 2015; for cols (14) through (16) we construct a state-level labor share of net income (see Appendix H for details and validation). The key explanatory variable comes from state-year average household union share generated from Gallup in the earlier years and the CPS in later years. Cols. (13) and (16) add a 1929 measure of state-year density based on data from the Handbook of American Trade Unions (see Appendix C and Cohen, Malloy, and Nguyen (2016) for details and validation) and a 1929 measure of skill shares based on the 1940 Census with age and migration adjustment (see Appendix C for details and validation).

Notes: Note that union density is out of one (not 100) to conserve table space by avoiding coefficients with zeros after the decimal. All estimates are from split-sample-IV regressions (see Section 5.3 for estimating equations), repeated 200 times (bootstrapped estimates reported). All regressions include state and year fixed effects; $South \times Year$ fixed effects; and state-year education controls (both from Gallup and CPS at the annual level, and interpolated from the IPUMS Census at the decade level). "Controls" include state-year share of employment in all one-digit industry categories, state-spec. quadratic time trend, state-year log income, state-year share of households filing taxes, state-year minimum wage, state top marginal income tax rate, a "policy liberalism" index (from Caughey and Warshaw, 2016), and state-year top marginal tax rates. Sample size is larger for the top 10 and labor share outcomes because they are available at the annual level and go back further in time; for the other outcomes, until the CPS in the 1970s, we only have data from the decadal Census beginning in 1940. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Table 4: IV estimation of changes in state union density and changes in state inequality

	Dept. variable: Change in state-level outcomes							
	Density (first stage)		Top-ten share			Labor share		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Change in union density			-24.33***	-62.32***	-56.38***	10.41***	33.88***	25.20***
			[7.208]	[10.96]	[12.74]	[3.753]	[6.034]	[5.302]
Wagner shock x (1929-1938)	0.0465***	0.0672***						
	[0.0127]	[0.0136]						
War shock x (1938-1947)	0.0378***	0.0347**						
	[0.0130]	[0.0143]						
Wagner shock	-0.00143	-0.00151		0.345	0.405		0.0712	0.142
	[0.00196]	[0.00346]		[0.232]	[0.310]		[0.0931]	[0.146]
War shock	-0.00346	-0.00356		-0.311	-0.337		-0.0261	-0.0262
	[0.00323]	[0.00346]		[0.346]	[0.355]		[0.138]	[0.135]
Dept. var. mean	-0.000763	-0.00109	-2.631	0.643	0.643	2.514	0.0320	0.0206
F-stat	17.21	26.17		17.21	26.17		17.21	26.17
IV or OLS?	OLS	OLS	OLS	IV	IV	OLS	IV	IV
Excl. control period	No	No	Yes	No	No	Yes	No	No
Excl. MI?	No	Yes	No	No	Yes	No	No	Yes
Observations	409	400	94	409	400	94	409	400

Sources: Union density data from Gallup and CPS, except for 1929 (see Section 5.3 and Appendix B for construction of 1929 density, which follows Cohen, Malloy, and Nguyen (2016)). Top-ten income data are from Frank, 2015. See Appendix H for construction of state-level labor share of net income.

Notes: Note that union density is out of one (not 100) to conserve table space by avoiding coefficients with zeros after the decimal (whereas both top-ten and labor-share outcomes are out of 100). Each observation is a state × nine-year interval (1929-1938, 1938-1947, 1947-1958, etc). We standardize (subtract the mean and divide by the standard deviation) each policy shock variable to ease interpretation of coefficients. The Wagner shock is the (per capita) state-level gain in union membership via NLRB elections and recognition strikes between 1935 and 1938. The war shock is the (per capita) value of 1940-1945 defense contracts to each state. See Section 5.4 and Appendix G for more detail. Dependent variables means are means of the the 9-year changes. We exclude Michigan in some columns for robustness, because of its outlier status on both IV measures (see Appendix Figure G.4). See Appendix G for more robustness checks of the first-stage and IV results. $^{*}p = 0.1, ^{**}p = 0.05, ^{***}p = 0.01$.

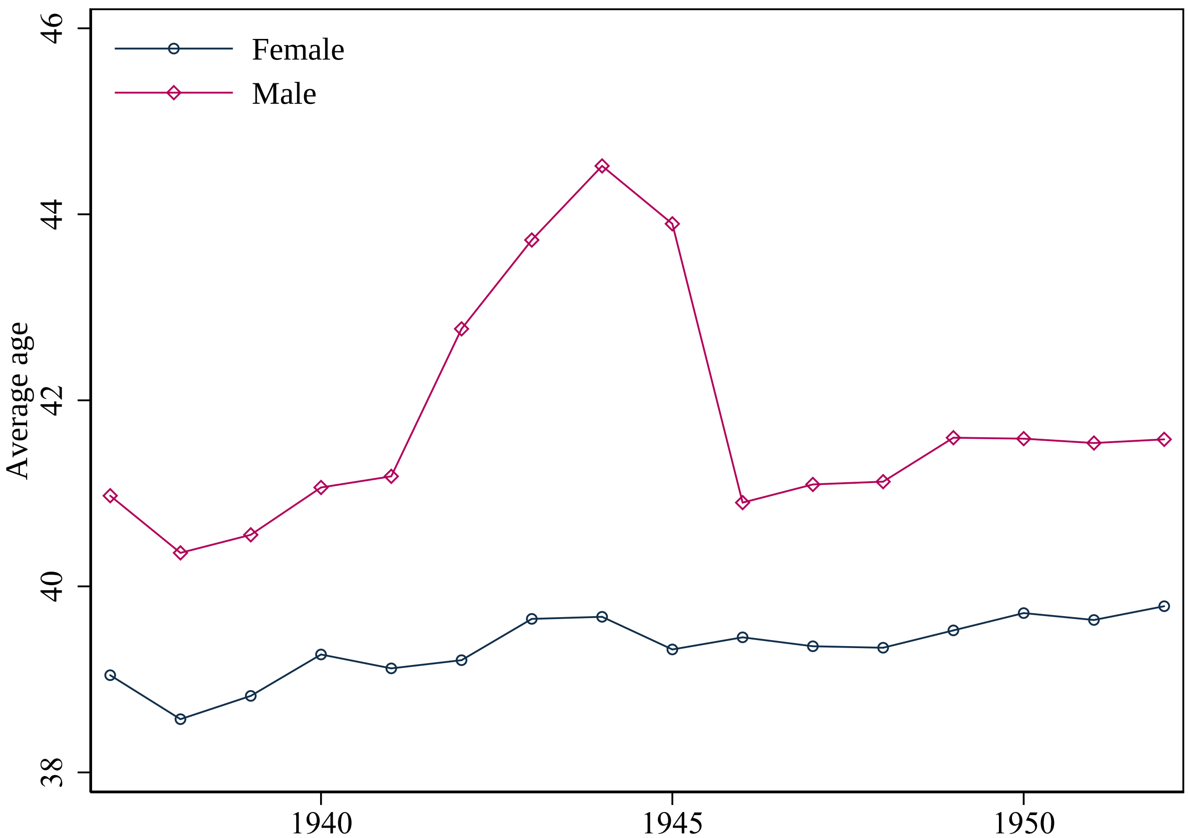
Appendix A. Supplementary Figures and Tables Noted in theText

Appendix Figure A.1: Comparing unemployment rates in Gallup and the HSUS

Data sources: Gallup and Historical Statistics of the United States (HSUS)

Notes: Sample in Gallup includes farmers

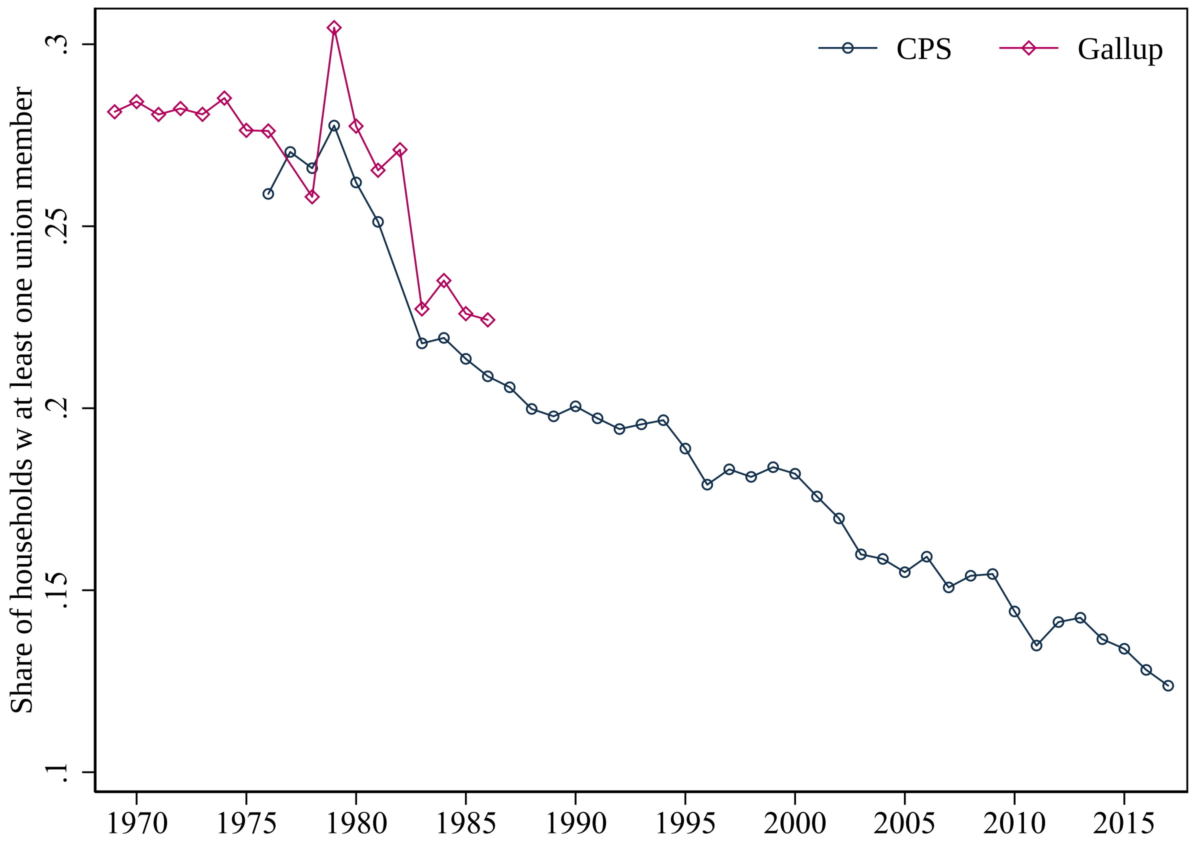
Appendix Figure A.2: Age distribution in Gallup, by gender, 1937-1952



Data sources: Gallup microdata.

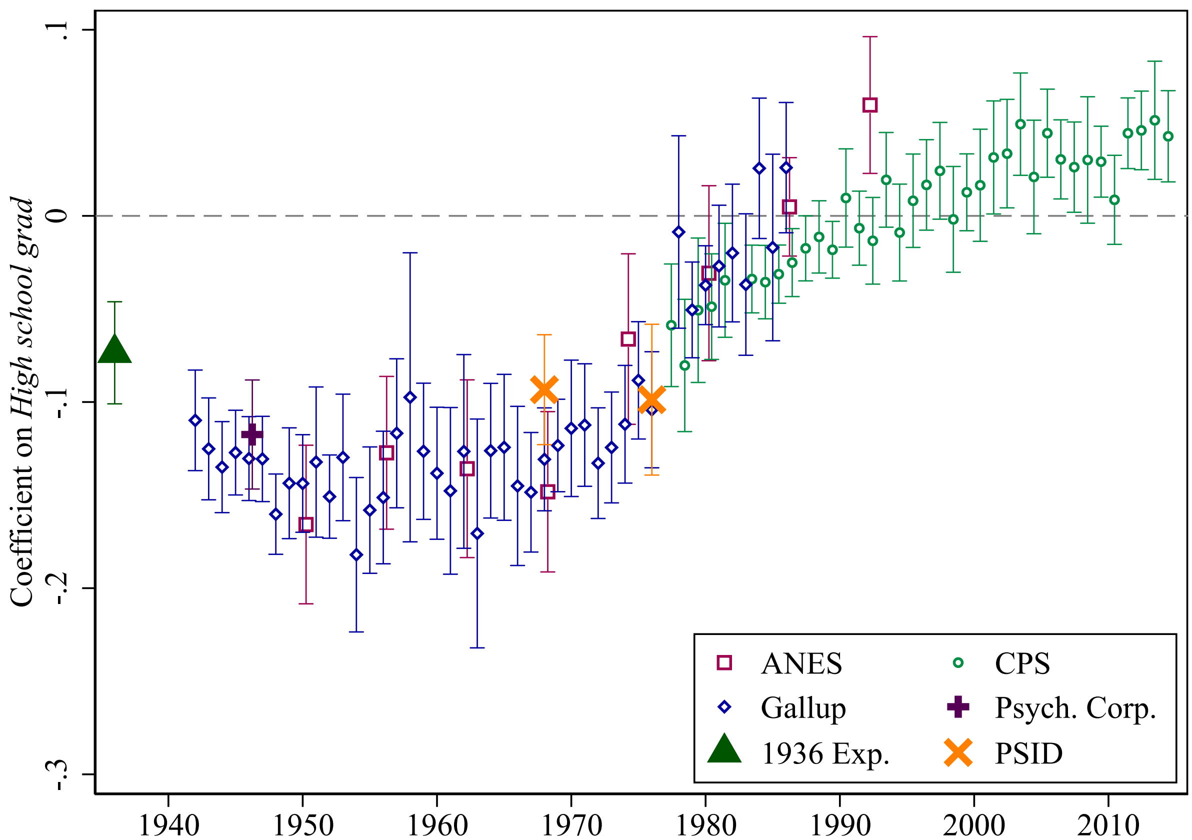
Notes: We show the large increase in the male average age in our Gallup dataset from 1942-1945 to demonstrate its ability to pick up high-frequency demographic changes (such as the deployment of young men overseas during World War II).

Appendix Figure A.3: Comparing household union density in Gallup and CPS, 1970–present



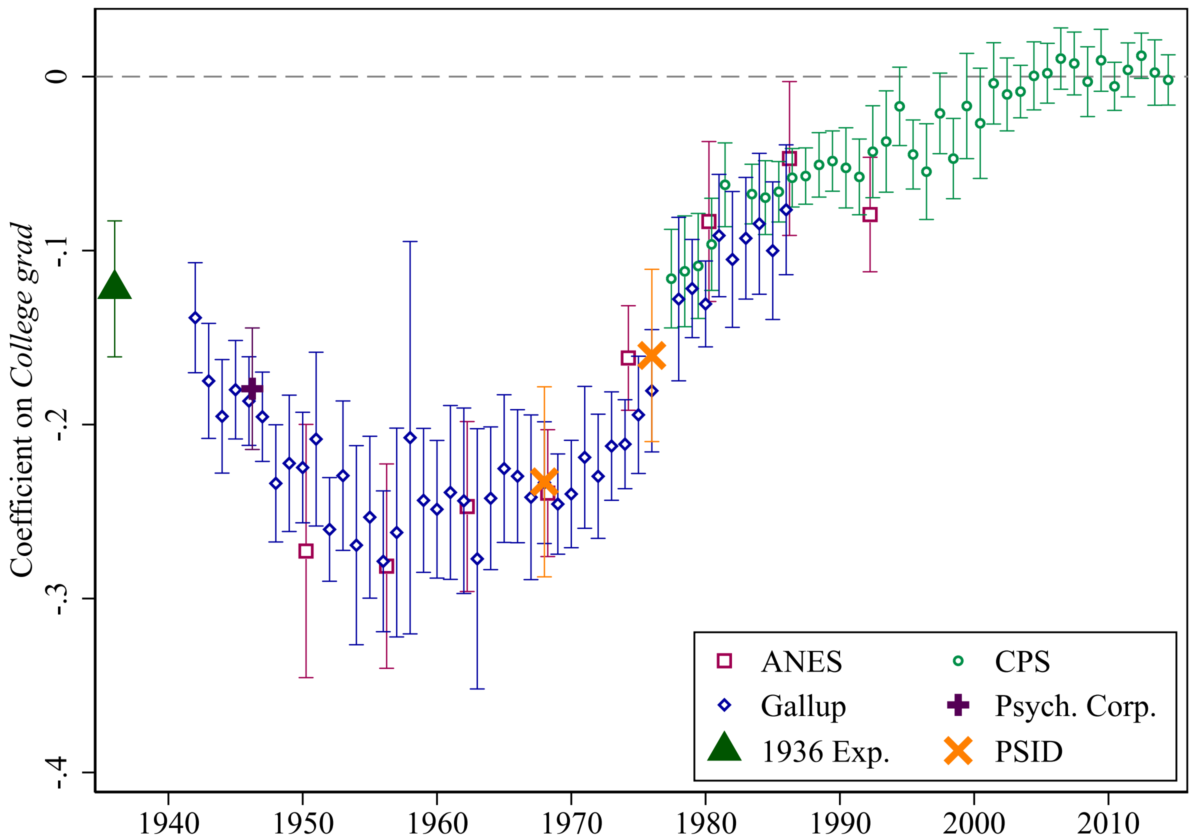
Data sources: Gallup and Current Population Survey

Appendix Figure A.4: Selection of union households by high-school graduation



Sources: Gallup, 1937–1986. CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996, U.S. Psych. Corporation, 1946.
Notes: For each data source, we estimate, separately by year, household union status on a *High School Grad* dummy variable, state s and survey-date t fixed effects, age and its square, and gender. We plot in this graph the coefficients on *High School Grad* from each of these estimations. For the ANES, because the samples are smaller, we group surveys into six-year bins. Standard errors are clustered by state.

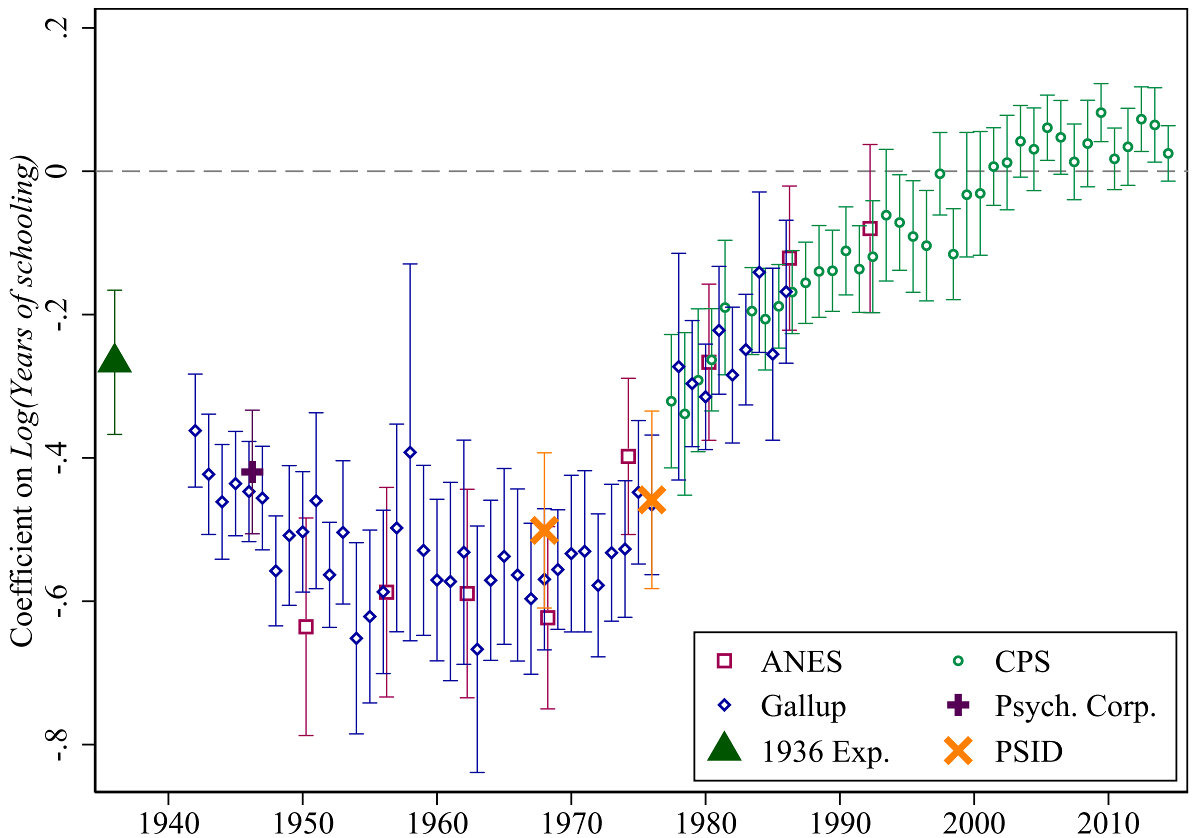
Appendix Figure A.5: Selection of union households by college graduation



Sources: Gallup, 1937–1986. CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996, U.S. Psych. Corporation, 1946.

Notes: For each data source, we estimate, separately by year, household union status on a *College Grad* dummy variable, state s and survey-date t fixed effects, age and its square, and gender. We plot in this graph the coefficients on *College Grad* from each of these estimations. For the ANES, because the samples are smaller, we group surveys into six-year bins. Standard errors are clustered by state.

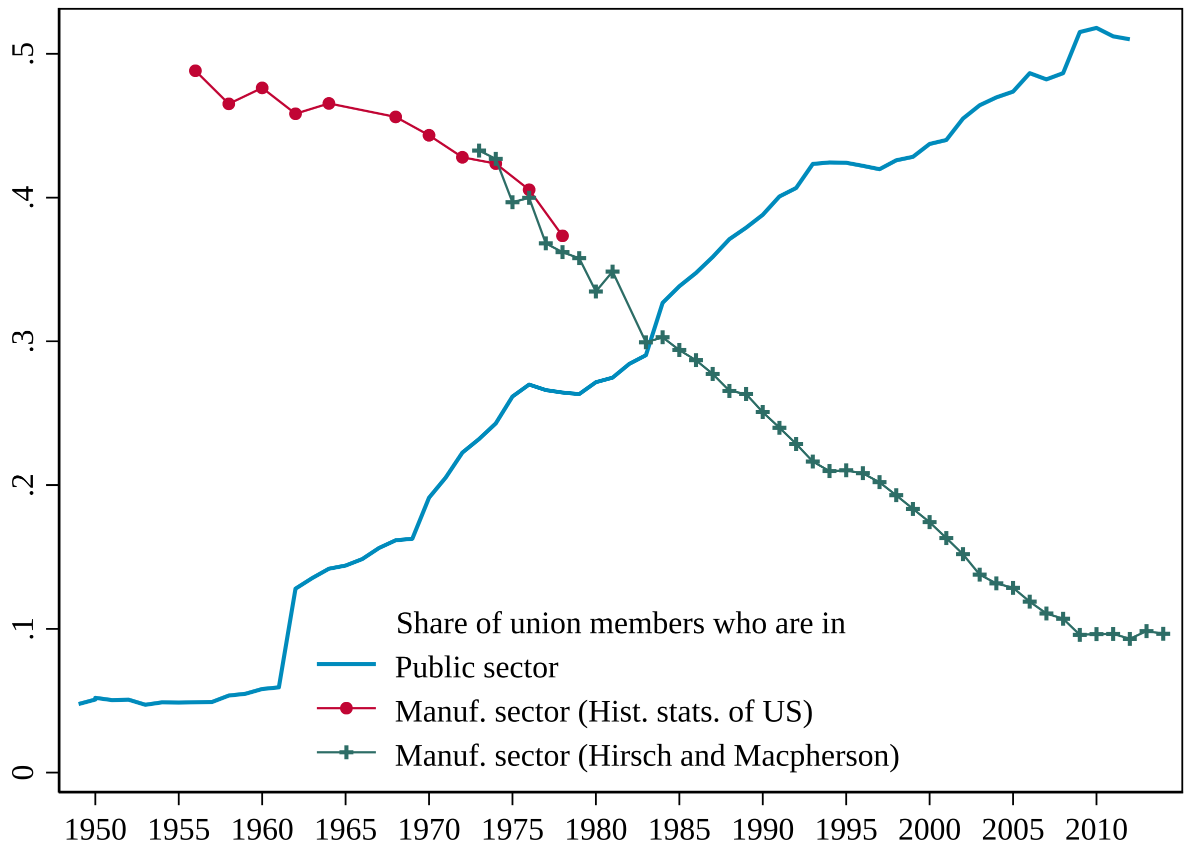
Appendix Figure A.6: Selection of union households by log years schooling



Sources: Gallup, 1937–1986. CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996, U.S. Psych. Corporation, 1946.

Notes: For each data source, we estimate, separately by year, household union status on *Log Years Education*, state s and survey-date t fixed effects, age and its square, and gender. We plot in this graph the coefficients on *Log Years Education* from each of these estimations. For the ANES, because the samples are smaller, we group surveys into six-year bins. Standard errors are clustered by state.

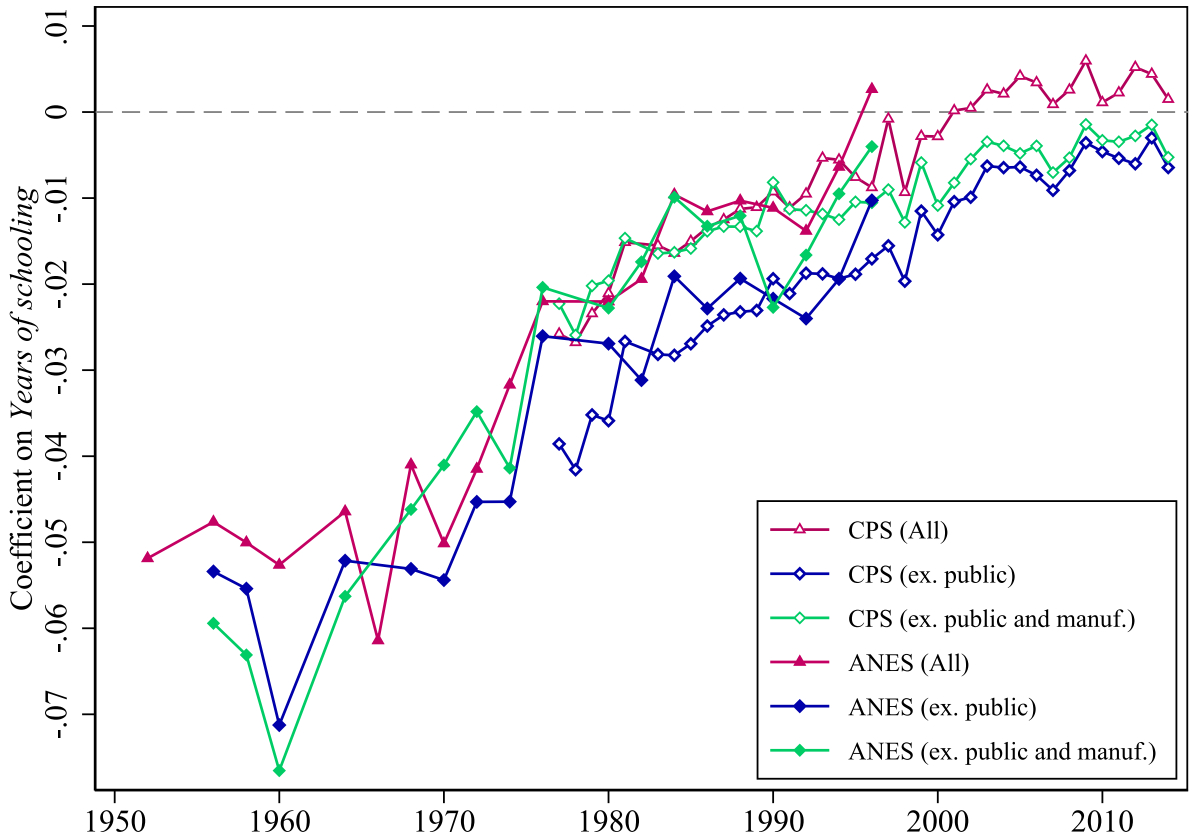
Appendix Figure A.7: Share of union members in public sector and manufacturing



Data sources: For the public-sector series, we thank John Schmitt at EPI. The early manufacturing series is from the Historical Statistics of the United States. The later manufacturing series is from the CPS, calculated by Hirsch and Macpherson and posted on `unionstats.com`.

Notes: These series refer to union *members*, not households, as in much of the paper.

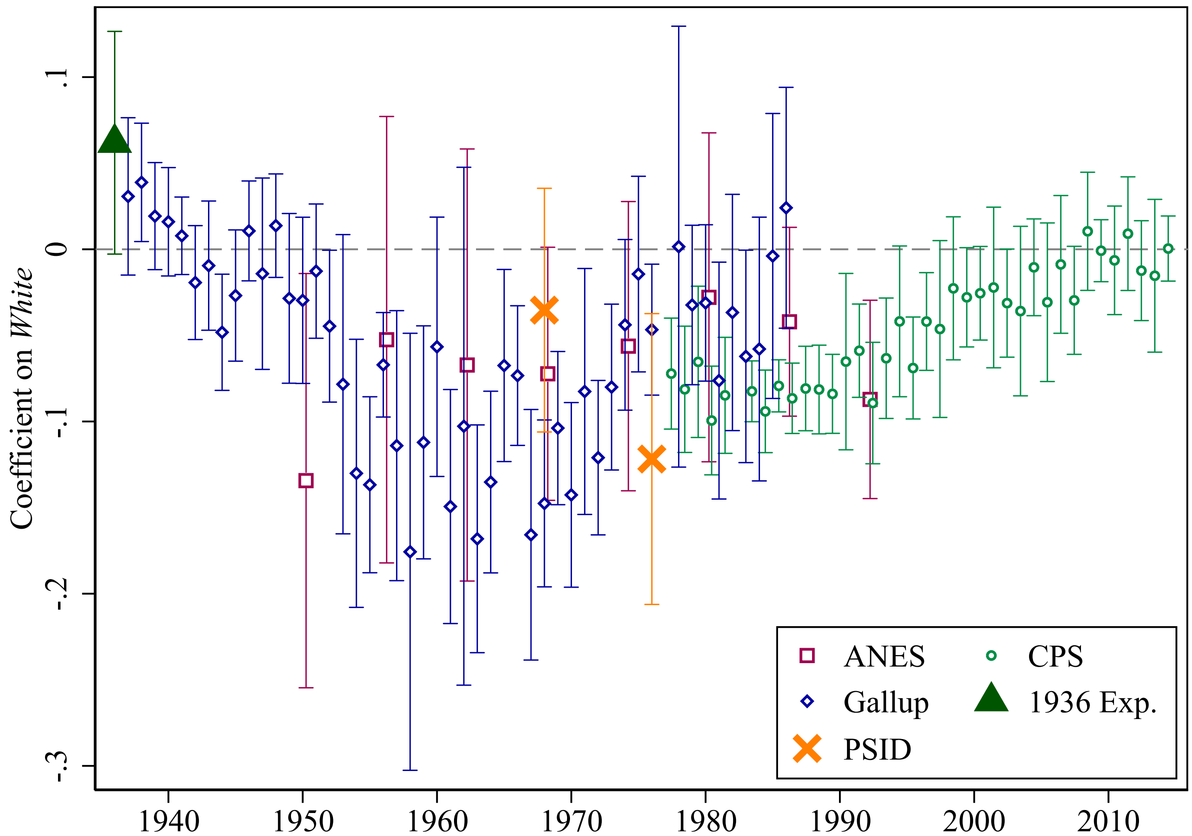
Appendix Figure A.8: Selection of union households by education in the ANES and CPS (dropping households with a public- or manufacturing-sector worker)



Sources: CPS, 1978–2016; ANES, 1952–1996.

Notes: For each data source, we estimate, separately by year, household union status on a *Years of education* variable, state s and survey-date t fixed effects, age and its square, and gender. We plot in this graph the coefficients on *Years of education* from each of these estimations. For the ANES, because the samples are smaller, we group surveys into six-year bins. Note that we only include ANES and CPS in this graph, because other data sources do not allow us to identify industrial sectors of workers in the household.

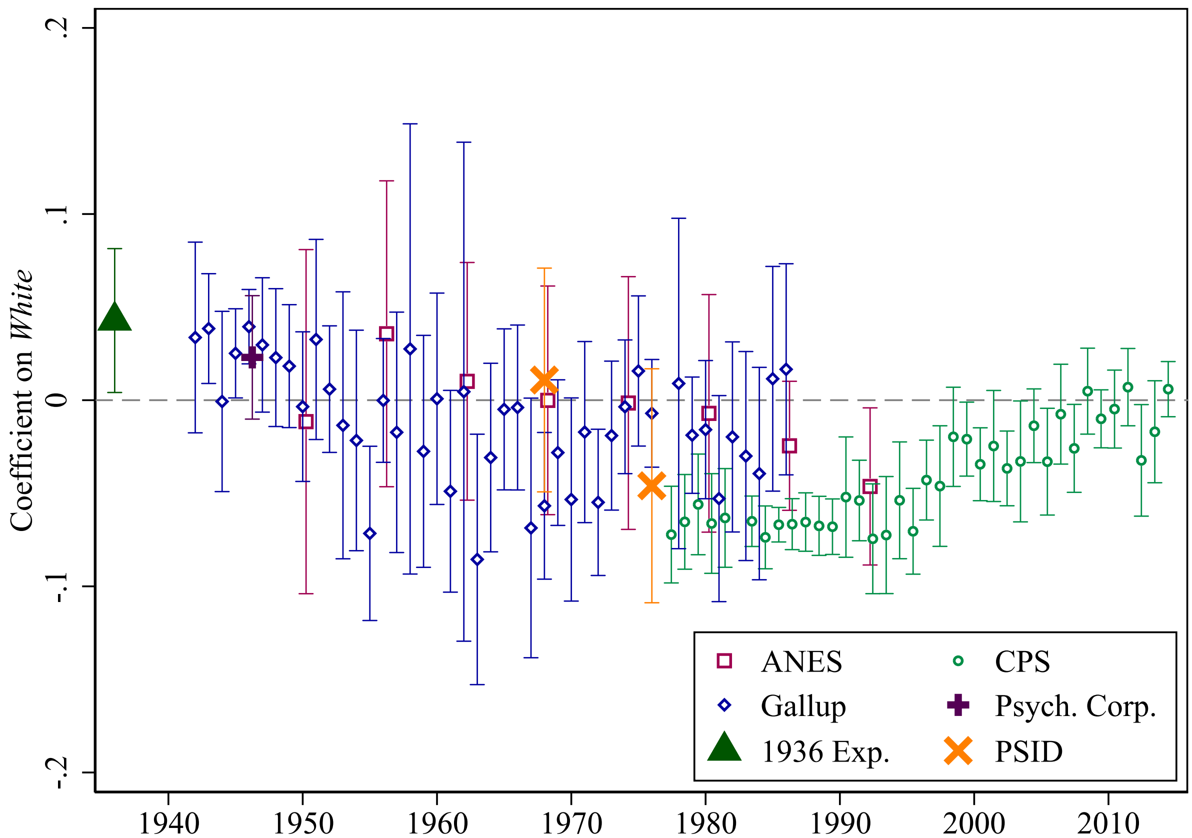
Appendix Figure A.9: Selection of union households by race (dropping Southern states)



Data sources: Gallup data, 1937–1986; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996. See Section 2.2 for a description of each data source.

Notes: For each data source, we estimate, separately by year, household union status on a *White* dummy variable, state s and survey-date t fixed effects, age and its square, and gender. We plot in this graph the coefficients on $White$ from each of these estimations. For the ANES, because the samples are smaller, we group surveys into six-year bins. Note that we cannot use the U.S. Psychological Corporation survey in this figure because, while it has state identifiers (thus we can thus control for state fixed effects), the codebook does *not* provide the state names that correspond to the codes (so we cannot drop the South). Confidence intervals are based on standard errors clustered by state.

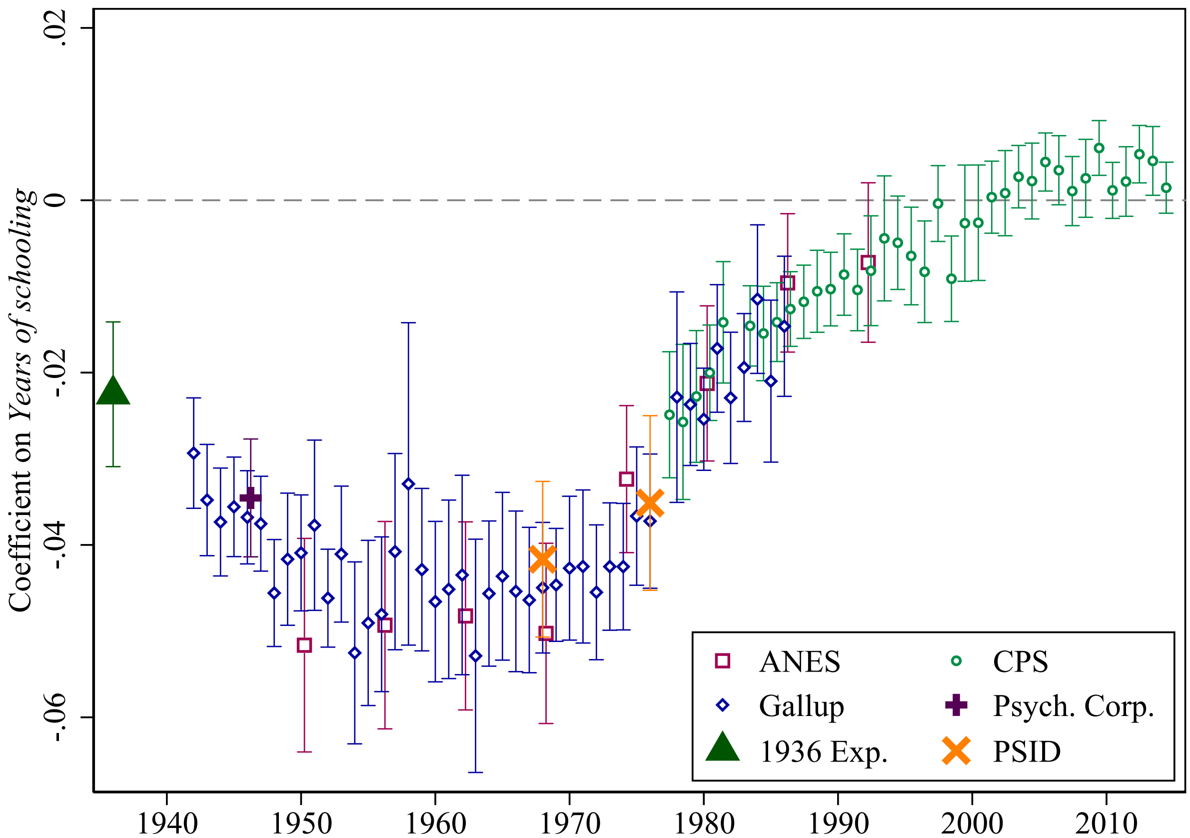
Appendix Figure A.10: Selection of union households by race (conditional on education)



Data sources: Gallup data, 1937–1986; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996. See Section 2.2 for a description of each data source.

Notes: For each data source, we estimate, separately by year, household union status on a *White* dummy variable, state s and survey-date t fixed effects, age and its square, gender, *and* years of schooling. Otherwise, the analysis is identical to that in Figure 4. Note that conditioning on education means we lose data from 1937-1941, as the Gallup education question is not included in these surveys. Confidence intervals are based on standard errors clustered by state.

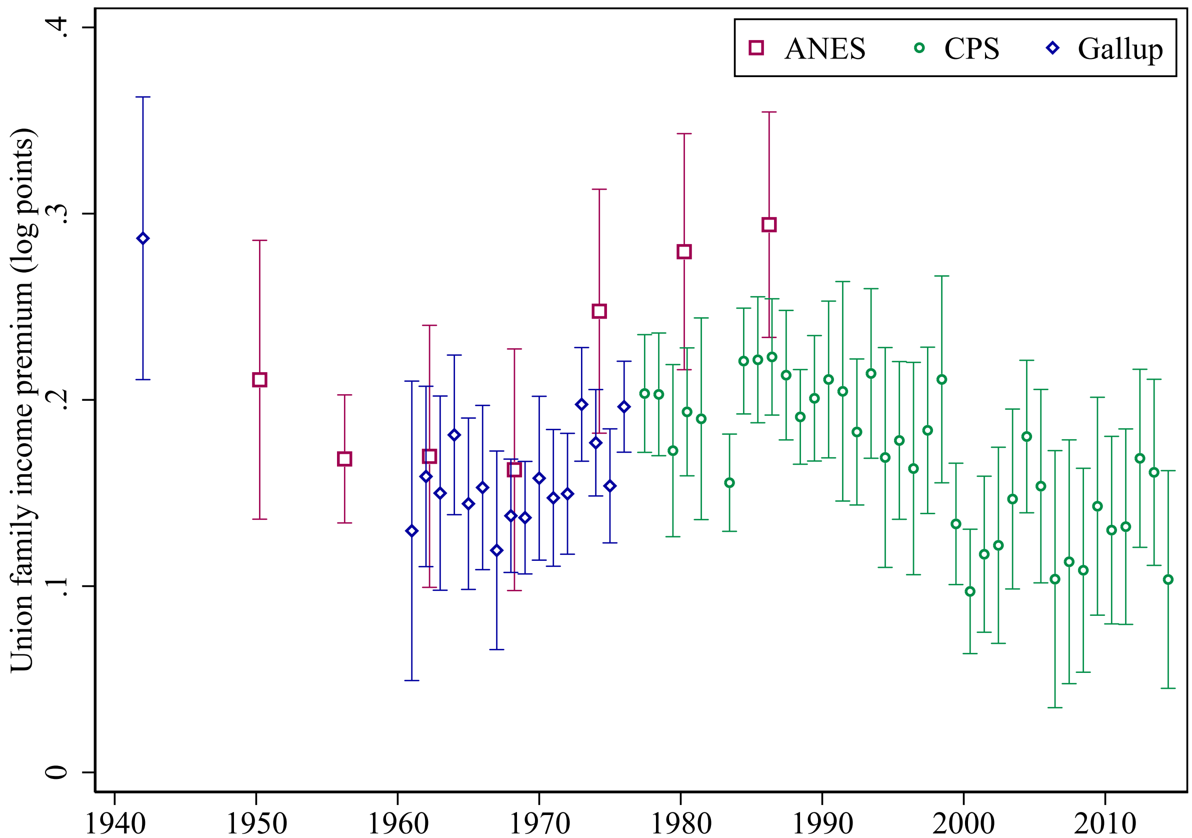
Appendix Figure A.11: Selection of union households by education (conditional on race)



Data sources: Gallup data, 1937–1986; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996. See Section 2.2 for a description of each data source.

Notes: For each data source, we estimate, separately by year, household union status on years of schooling, state s and survey-date t fixed effects, age and its square, gender, *and* a *White* dummy variable. Otherwise, the analysis is identical to that in Figure 3. Note that conditioning on education means we lose data from 1937-1941, as the Gallup education question is not included in these surveys. Confidence intervals are based on standard errors clustered by state.

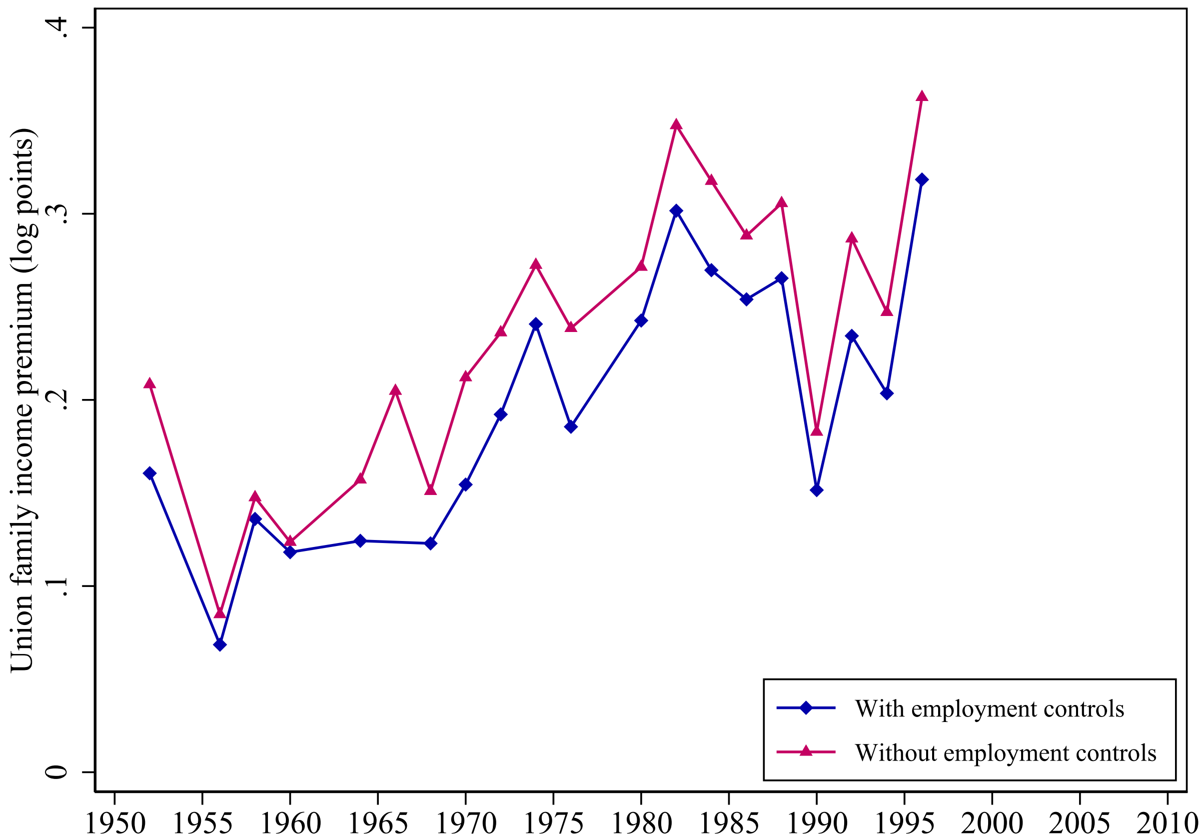
Appendix Figure A.12: Estimates of the union family income premium (including occupation controls when available)



Data sources: Gallup data, 1942, 1961–1976; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 2.2 for a description of each data source. See Appendix C for details on family income variable construction.

Notes: Each plotted point comes from estimating equation (2), which regresses log family income on household union status and controls for age, gender, race, state and survey-date fixed effects and (in most cases) fixed effects for the occupation of the head. We cannot perfectly match occupation categories across regressions, which is why we relegate this graph to the appendix. For the ANES, because the samples are smaller, we group surveys into six-year bins. The plotted confidence intervals are based on standard errors clustered by state.

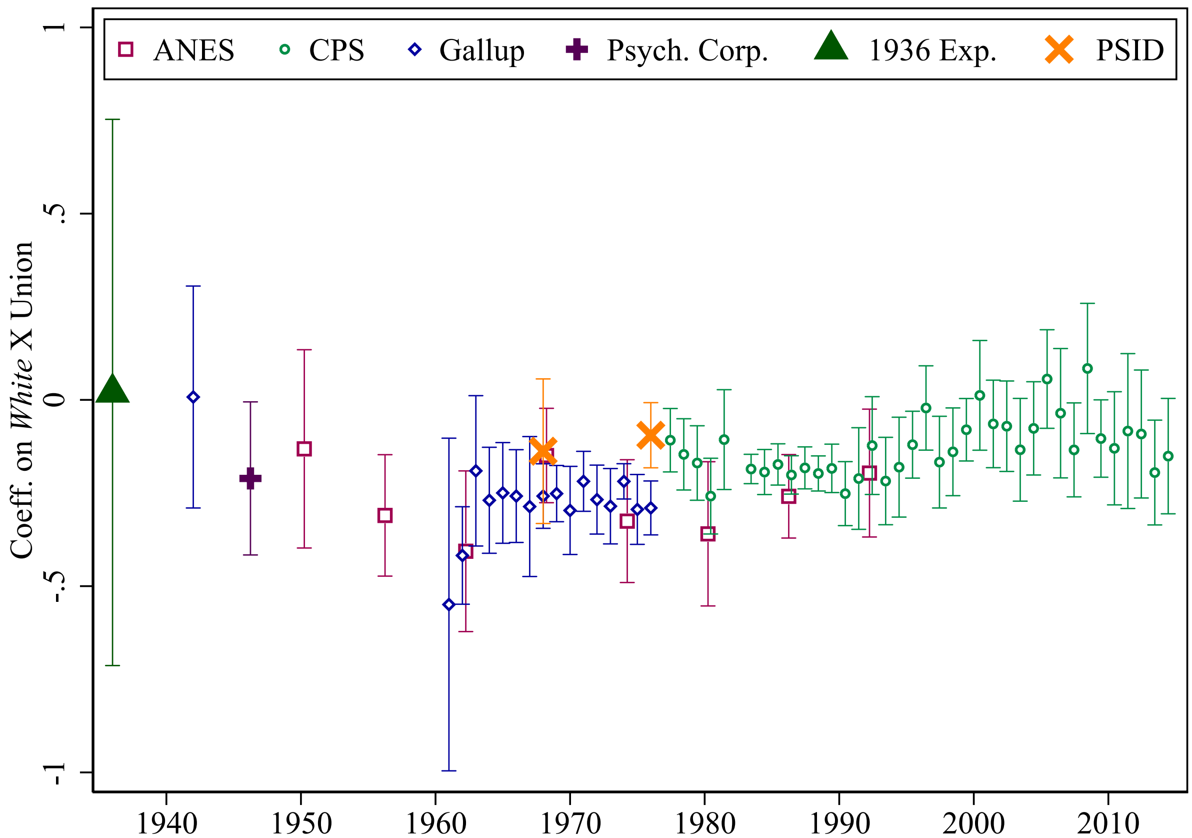
Appendix Figure A.13: Estimates of the union family income premium from ANES (with and without employment status controls)



Data sources: See Section 2.2 for a description of ANES data.

Notes: Each plotted point comes from estimating equation (2), which regresses log family income on the household union dummy and controls for age, gender, race, state and survey-date fixed effects. In addition, the first series includes an indicator for the household head being employed and a separate indicator for the respondent being employed. See Section 4.1 for more detail.

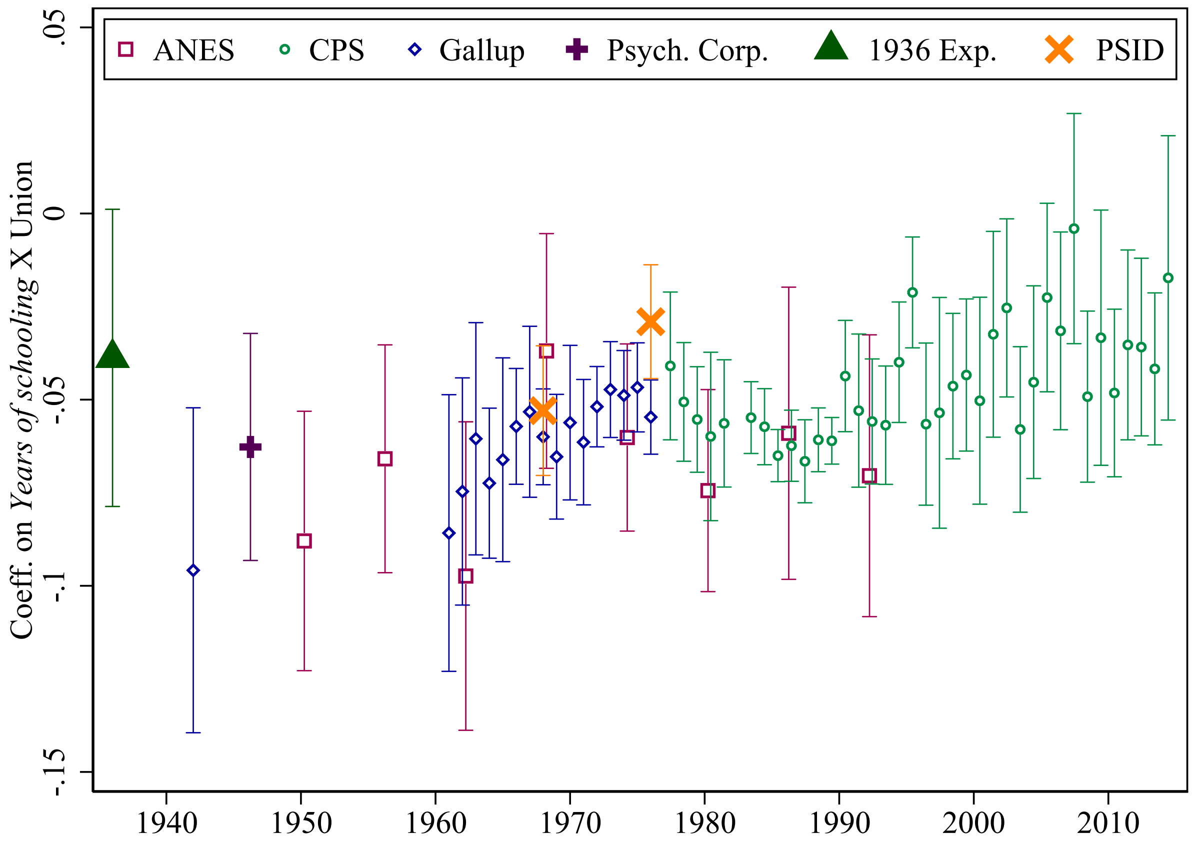
Appendix Figure A.14: Union family income premium by race (conditional on *Yrs. schooling*$\times$*Union*)



Data sources: Gallup data, 1942, 1961–1976; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996; U.S. Psych. Corporation, 1946. See Section 2.2 for a description of each data source. See Appendix C for details on family income variable construction.

Notes: In this figure we estimate the differential union premium paid to white households, conditional on any differential premium by education of the respondent. This figure is identical to the union-premium-by-race analysis in Figure 7, except that we add *Years of schooling*$_h^R \times Union_h$ to each estimating equation, where *Years of schooling*$_h^R$ is the years of schooling for the respondent from household h, and $Union_h$ is our usual household union measure. The plotted confidence intervals are based on standard errors clustered by state.

Appendix Figure A.15: Union family income premium by education (conditional on $White \times Union$)



Data sources: Gallup data, 1942, 1961–1976; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996; U.S. Psych. Corporation, 1946. See Section 2.2 for a description of each data source. See Appendix C for details on family income variable construction.

Notes: In this figure we estimate the differential union premium paid to more-educated households, conditional on any differential premium by race of the respondent. This figure is identical to the union-premium-by-education analysis in Figure 6, except that we add $White^R_h \times Union_h$ to each estimating equation, where $White^R_h$ is a dummy for the respondent from household h, and $Union_h$ is our usual household union measure. The plotted confidence intervals are based on standard errors clustered by state.

Appendix Table A.1: Estimating family union income premium and reporting coefficients on additional covariates, by data source and time period

	Dep't var: Logged family income					
	(1)	(2)	(3)	(4)	(5)	(6)
Union household	0.116***	0.259***	0.196***	0.160***	0.129***	0.246***
	[0.0239]	[0.0332]	[0.0337]	[0.0151]	[0.0212]	[0.0179]
Years of education		0.175***	0.146***	0.115***	0.122***	0.122***
		[0.00672]	[0.00681]	[0.00371]	[0.00624]	[0.00653]
Years of educ., household head	0.125***					
	[0.00770]					
White dummy	0.880***	0.461***	0.410***	0.443***	0.517***	0.326***
	[0.0477]	[0.0883]	[0.0317]	[0.0299]	[0.0656]	[0.0408]
Female		-0.109***	-0.203***	-0.0903***	-0.121***	-0.126***
		[0.0312]	[0.0195]	[0.00386]	[0.0200]	[0.0154]
Household head is female	0.0955***					
	[0.0261]					
Age	0.0749***	0.0521***	0.0682***	0.0698***	0.0640***	0.0740***
	[0.00824]	[0.0134]	[0.00515]	[0.00227]	[0.00407]	[0.00407]
Age squared, divided by 1000	-0.842***	-0.614***	-0.884***	-0.817***	-0.744***	-0.753***
	[0.0999]	[0.165]	[0.0625]	[0.0261]	[0.0518]	[0.0454]
Data source	Exp. survey	Gallup	U.S. Psych.	Gallup	ANES	ANES
Year(s) in sample	1936	1942-1942	1946	1961-1975	1952-1970	1972-1990
Observations	4976	2538	5415	171973	9212	12925

Data sources: Gallup data, 1942, 1961–1975; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 2.2 for a description of each data source. See Appendix C for details on family income variable construction.

Notes: All regressions include state fixed effects and survey date fixed effects. We control for number of employed individuals in the household, except in the Gallup and U.S. Psych. data where this control is not available. Otherwise, all other samples include ages 21–64. Since the goal of the table is to show the coefficients from regressions run on the datasets least likely to be familiar to readers, we do not include the CPS. Standard errors in brackets, clustered by state.
$^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Appendix Table A.2: Estimating family union income premium using 1956-1960 ANES panel

	Dept. variable: Log family income				
	(1)	(2)	(3)	(4)	(5)
Union household	0.134***	0.114***	0.103*	0.0635	0.0692
	[0.0309]	[0.0283]	[0.0559]	[0.0809]	[0.0544]
Union household x Low-educ. respondent				0.0486	
				[0.106]	
Union household x Non-white respondent					0.249
					[0.209]
Added controls?	No	Yes	No	No	No
Respondent FE?	No	No	Yes	Yes	Yes
Observations	3303	3303	3303	3303	3303

Notes: All regressions include year fixed effects and a quadratic in age. Sample restricted to ages 18 to 65. Controls include race, sex, education and occupation fixed effects. "Low education" is high school degree or less. Standard errors in brackets, clustered by individual. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Appendix Table A.3: Heterogeneity of the union premium

	Dept. variable: Log family income						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Union household	0.194***	0.202***	0.198***	0.191***	0.185***	0.170***	0.148***
	[0.00404]	[0.0233]	[0.0236]	[0.00749]	[0.00389]	[0.00354]	[0.00852]
Union x National unemp. rate		-0.117					
		[0.352]					
Union x National union density			-0.0152				
			[0.103]				
Union x Δ Ln(CPI)				0.0775			
				[0.148]			
Union x South					0.0562***	-0.0118*	-0.00312
					[0.00417]	[0.00641]	[0.00717]
Union x State ever RTW						0.0832***	0.0959***
						[0.00751]	[0.0100]
Union x State currently RTW							0.0214**
							[0.00879]
Observations	1,153,757	1,153,757	1,148,781	1,153,757	1,153,757	1,153,757	1,153,757

Notes: All regressions include state and survey-date fixed effects and number of employed individuals in household whenever available. *State ever RTW* is a state-level dummy indicating that a state passed a right-to-work law at some point during our sample period. *State currently RTW* is coded as one for any year after a state passes its first RTW law. Standard errors in brackets, clustered by year. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Appendix Table A.4: Paid vacation as a function of union status (Gallup, 1949)

	Dep't var: Do you (or husband) get paid vacation?				
	(1)	(2)	(3)	(4)	(5)
Union household	0.223***	0.188***	0.323**	0.288	0.130***
	[0.0319]	[0.0292]	[0.129]	[0.222]	[0.0291]
White x Union household			-0.144		
			[0.130]		
Years educ. x Union household				-0.00904	
				[0.0194]	
Low-skill labor x Union					0.137***
					[0.0487]
Dept. var. mean	0.517	0.524	0.524	0.524	0.524
State FE?	Yes	Yes	Yes	Yes	Yes
Demographic controls?	Yes	Yes	Yes	Yes	Yes
Occupation FE?	No	Yes	Yes	Yes	Yes
Observations	1969	1911	1911	1911	1911

Notes: Data from Gallup, May 1949. Demographic controls include respondent's age and square, education (four fixed effects), gender, and race. When occupation controls are added, they refer to the head of the household. Low-skill occupation dummy denotes "unskilled and semi-skilled labor." Standard errors in brackets, clustered by state. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

Appendix Table A.5: Ease of finding a job as good as the one you have

	Dept. var: Would be easy to find a job as good as current one				
	Gallup (1939)			GSS (1977-2018)	
	(1)	(2)	(3)	(4)	(5)
Union household	-0.124***	-0.121***	-0.0943***	-0.0863***	-0.0766***
	[0.0275]	[0.0272]	[0.0310]	[0.00953]	[0.00960]
Mean, dept. var.	0.497	0.497	0.497	0.254	0.255
State FE	Yes	Yes	Yes	Reg.	Reg.
Demogr. controls	No	Yes	Yes	No	Yes
Educ. controls	No	No	Yes	No	Yes
Occup. controls	No	No	Yes	No	No
Observations	1978	1978	1978	12039	12019

Notes: The Gallup question reads: "If you lost your present job (business, farm), how hard do you think it would be for you to get another job (business, farm) just as good?" We code "impossible" and "quite hard" ("fairly hard" and "easy") as zero (one). Demographic controls include respondent's age and its square, education (four fixed effects), gender and race. Occupation controls refer to household head; low-skill occupation to "unskilled, semi-skilled labor." The GSS question reads: "About how easy would it be for you to find a job with another employer with approximately the same income and fringe benefits you now have?" We code "very easy" ("somewhat easy" and "not easy at all") as one (zero). All GSS regressions include year fixed effects. Demographic and education controls are as in Gallup. Standard errors are in brackets and clustered by state (region). $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.6: Covariance between union density and skill shares

	Annual regressions				State-year panel regs.	
	(1)	(2)	(3)	(4)	(5)	(6)
Skill share measure	-0.0820***	-0.0485	-0.217***	-0.0211	-0.0208**	-0.00312
	[0.0196]	[0.0353]	[0.0506]	[0.0444]	[0.00917]	[0.0131]
Time polynomial?	None	Cubic	Quad.	Quartic	None	None
State FE?	N/A	N/A	N/A	N/A	No	Yes
Year FE?	No	No	No	No	No	Yes
Observations	56	56	56	56	1968	1968

Notes: This table shows how our union density variable and the skill-share measure (both used extensively in Section 5) co-vary at different levels of aggregation as well as conditionally and unconditionally. See Section 2 for more information on the construction of the density variable. We follow Autor, Katz, and Kearney (2008) and Goldin and Katz (2008) in constructing skill-shares measures (see Appendix C for more information). $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.7: Aggregate coll. premium, 90/10, 90/50 ratios as functions of density

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: College premium						
Skill share	-0.474***	-0.547***	-0.482***	-0.535***	-0.585***	-0.560***	-0.717***
	(0.085)	(0.087)	(0.073)	(0.077)	(0.106)	(0.103)	(0.109)
Gallup union density		-0.800**					
		(0.353)					
BLS union density			-1.176***				
			(0.434)				
Density (avg. of Gallup, BLS)				-1.258***	-1.585***	-2.316***	-1.354***
				(0.402)	(0.494)	(0.339)	(0.403)
	Panel B: Log 90/10 ratio						
Skill share	0.063	-0.069	0.050	-0.044	0.156	0.196	0.108
	(0.110)	(0.081)	(0.093)	(0.076)	(0.124)	(0.132)	(0.144)
Gallup union density		-1.453***					
		(0.377)					
BLS union density			-1.997***				
			(0.324)				
Density (avg. of Gallup, BLS)				-2.203***	-1.736***	-2.932***	-1.653***
				(0.360)	(0.628)	(0.443)	(0.568)
	Panel C: Log 90/50 ratio						
Skill share	-0.254***	-0.256***	-0.257***	-0.270***	-0.177**	-0.132	-0.191*
	(0.082)	(0.092)	(0.077)	(0.086)	(0.072)	(0.094)	(0.095)
Gallup union density		-0.026					
		(0.285)					
BLS union density			-0.554*				
			(0.277)				
Density (avg. of Gallup, BLS)				-0.346	-0.281	-1.604***	-0.257
				(0.350)	(0.362)	(0.307)	(0.359)
	Panel D: Log 10/50 ratio						
Skill share	-0.316**	-0.187*	-0.307**	-0.226**	-0.332**	-0.328**	-0.298**
	(0.131)	(0.097)	(0.131)	(0.112)	(0.135)	(0.130)	(0.137)
Gallup union density		1.427***					
		(0.298)					
BLS union density			1.443***				
			(0.395)				
Density (avg. of Gallup, BLS)				1.857***	1.455***	1.327***	1.396**
				(0.365)	(0.536)	(0.419)	(0.553)
Controls?	No	No	No	No	Yes	Yes	Yes
Time Polynomial	Cubic	Cubic	Cubic	Cubic	Cubic	Quadratic	Quartic
Observations	56	56	56	56	56	56	56

Notes: This table shows variants of the specifications estimated in cols. 1 and 2 (Panel A), cols. 3 and 4 (Panel B), cols. 5 and 6 (Panel C), and cols. 7 and 8 (Panel D) of Table 2.
$^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

Appendix Table A.8: Aggregate Gini, top-ten, labor share of income as functions of density

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Gini coefficient						
Skill share	-0.006	-0.001	0.009	0.008	0.001	0.073***	-0.007
(interpolated)	(0.013)	(0.014)	(0.014)	(0.014)	(0.019)	(0.018)	(0.018)
Educ. share ratio	0.014***	0.009**	0.009**	0.007**	0.008**	0.018***	0.010**
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Gallup union density		-0.115***					
		(0.032)					
BLS union density			-0.120***				
			(0.035)				
Density (avg. of				-0.162***	-0.141***	-0.192***	-0.147***
Gallup, BLS)				(0.039)	(0.037)	(0.042)	(0.036)
Observations	65	65	65	65	65	65	65
	Panel B: Top-ten income share						
Skill share	-13.066***	-13.587***	-10.731***	-12.470***	-21.353***	-11.592**	-18.667***
(interpolated)	(2.644)	(2.675)	(2.884)	(2.764)	(5.718)	(4.487)	(6.227)
Educ. share ratio	1.576	0.611	-0.333	-0.308	-0.310	-0.112	0.981
	(2.099)	(2.154)	(1.505)	(1.853)	(1.349)	(1.476)	(1.238)
Gallup union density		-17.865**					
		(7.906)					
BLS union density			-37.472***				
			(12.328)				
Density (avg. of				-35.898***	-23.340**	-38.599***	-25.913***
Gallup, BLS)				(13.055)	(11.401)	(11.999)	(9.693)
Observations	75	75	75	75	75	75	75
	Panel C: Labor share of income						
Skill share	-6.910*	-6.104*	-9.991***	-7.481***	-3.891	8.489*	-4.545
(interpolated)	(3.509)	(3.092)	(2.532)	(2.509)	(3.153)	(4.760)	(3.489)
Educ. share ratio	-3.182**	-1.933	-0.499	-0.618	-0.723	-0.617	-0.981
	(1.444)	(1.367)	(0.686)	(1.070)	(1.226)	(1.327)	(1.577)
Gallup union density		22.812***					
		(8.359)					
BLS union density			52.244***				
			(7.502)				
Density (avg. of				48.329***	45.213***	22.826*	45.770***
Gallup, BLS)				(11.595)	(13.150)	(11.936)	(12.753)
Observations	76	76	76	76	76	76	76
Controls?	No	No	No	No	Yes	Yes	Yes
Time Polynomial	Cubic	Cubic	Cubic	Cubic	Cubic	Quadratic	Quartic

Notes: This table shows variants of the specifications estimated in cols. 9 and 10 (Panel A), cols. 11 and 12 (Panel B), and cols. 13 and 14 (Panel C) of Table 2. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.9: Skill premium, percentile ratios, and Gini coefficient as a function of state-year union density

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: College premium						
Household union share	-0.122**	-0.187	-0.169	-0.205	-0.214*	-0.195	-0.182
	[0.052]	[0.136]	[0.141]	[0.130]	[0.128]	[0.124]	[0.113]
	Panel B: Log 90/10 ratio						
Household union share	-0.227**	-0.345**	-0.291*	-0.293*	-0.307**	-0.251*	-0.197
	[0.098]	[0.168]	[0.160]	[0.155]	[0.149]	[0.136]	[0.125]
	Panel C: Log 90/50 ratio						
Household union share	-0.091*	-0.140	-0.118	-0.112	-0.122	-0.122	-0.097
	[0.048]	[0.088]	[0.088]	[0.088]	[0.086]	[0.079]	[0.084]
	Panel D: Log 10/50 ratio						
Household union share	0.135**	0.205*	0.173	0.181*	0.184*	0.129	0.100
	[0.063]	[0.113]	[0.106]	[0.104]	[0.102]	[0.105]	[0.100]
	Panel E: Gini coefficient						
Household union share	-0.035**	-0.055**	-0.041	-0.052**	-0.054**	-0.046**	-0.050**
	[0.016]	[0.027]	[0.027]	[0.023]	[0.022]	[0.022]	[0.025]
Observations	1,960	1,960	1,960	1,960	1,960	1,960	1,960
Min Year	1940	1940	1940	1940	1940	1940	1940
Max. Year	2014	2014	2014	2014	2014	2014	2014
SouthXyear FE	Yes	Yes	Yes	Yes	Yes	No	Yes
Split-Sample IV	No	Yes	Yes	Yes	Yes	Yes	Yes
Income covars.	No	No	Yes	Yes	Yes	Yes	Yes
Industry Shares	No	No	No	Yes	Yes	Yes	Yes
Policy covars.	No	No	No	No	Yes	Yes	Yes
RegionXyear FE	No	No	No	No	No	Yes	No
State-spec. quad.	No	No	No	No	No	No	Yes

Sources: See notes to Table 3.

Notes: IV estimates are from split-sample-IV regressions (see Section 5.3 for estimating equations). All regressions include state and year fixed effects; $South \times Year$ fixed effects; and state-year education controls (both from Gallup and CPS at the annual level, and interpolated from the IPUMS Census at the decade level). “Industry shares” controls for state-year share of employment in all one-digit industry categories. “State-spec. quad.” indicates that state-specific quadratic time trends are included. “Policy covars.” indicate that state-year minimum wage and a “policy liberalism” index (from Caughey and Warshaw, 2016) are included. Standard errors are clustered at the state level. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.10: State Year top-ten income share, Labor share as a function of union density

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Top 10p. Income						
Household union share	-2.739**	-4.192**	-4.340**	-3.732**	-3.479**	-3.248**	-2.403**
	[1.125]	[1.917]	[1.704]	[1.788]	[1.693]	[1.614]	[1.075]
	Panel B: Labor share						
Household union share	3.656***	5.567***	6.018***	4.037**	3.972**	3.442*	1.090
	[1.198]	[1.870]	[2.010]	[1.906]	[1.789]	[1.857]	[1.029]
Observations	3,537	3,537	3,537	3,537	3,537	3,537	3,537
Min Year	1937	1937	1937	1937	1937	1937	1937
Max. Year	2014	2014	2014	2014	2014	2014	2014
SouthXyear FE	Yes	Yes	Yes	Yes	Yes	No	Yes
Split-Sample IV	No	Yes	Yes	Yes	Yes	Yes	Yes
Income covars.	No	No	Yes	Yes	Yes	Yes	Yes
Industry Shares	No	No	No	Yes	Yes	Yes	Yes
Policy covars.	No	No	No	No	Yes	Yes	Yes
RegionXyear FE	No	No	No	No	No	Yes	No
State-spec. quad.	No	No	No	No	No	No	Yes

Sources: See notes to Table 3.

Notes: IV estimates are from split-sample-IV regressions (see Section 5.3 for estimating equations). All regressions include state and year fixed effects; $South \times Year$ fixed effects; and state-year education controls (both from Gallup and CPS at the annual level, and interpolated from the IPUMS Census at the decade level). "Industry shares" controls for state-year share of employment in all one-digit industry categories. "State-spec. quad." indicates that state-specific quadratic time trends are included. "Policy covars." indicate that state-year minimum wage and a "policy liberalism" index (from Caughey and Warshaw, 2016) are included. Standard errors are clustered at the state level. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.11: Log state-year income per capita as a function of union density

Household union	0.112***	0.170***	0.138**	0.141**	0.032	-0.010
share	[0.034]	[0.059]	[0.066]	[0.064]	[0.059]	[0.038]
Observations	3,537	3,537	3,537	3,537	3,537	3,537
Min Year	1937	1937	1937	1937	1937	1937
Max. Year	2014	2014	2014	2014	2014	2014
SouthXyear FE	Yes	Yes	Yes	Yes	No	Yes
Split-Sample IV	No	Yes	Yes	Yes	Yes	Yes
Industry Shares	No	No	Yes	Yes	Yes	Yes
Policy covars.	No	No	No	Yes	Yes	Yes
RegionXyear FE	No	No	No	No	Yes	No
State-spec. quad.	No	No	No	No	No	Yes

Sources: Details on Log State Net Income/Capita data construction are in Appendix H
Notes: IV estimates are from split-sample-IV regressions (see Section 5.3 for estimating equations). All regressions include state and year fixed effects; $South \times Year$ fixed effects; and state-year education controls (both from Gallup and CPS at the annual level, and interpolated from the IPUMS Census at the decade level). "Industry shares" controls for state-year share of employment in all one-digit industry categories. "State-spec. quad." indicates that state-specific quadratic time trends are included. "Policy covars." indicate that state-year minimum wage and a "policy liberalism" index (from Caughey and Warshaw, 2016) are included. Standard errors are clustered at the state level. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix B. Background on Gallup and other historical data sources

B.1. Brief history of Gallup and other historical polling data

One of the main contributions of the paper is the introduction of newly available household-level data that include information on union membership. We draw much of these data from public opinion polls, which have recently been posted online by the Roper Center at Cornell.[47]

Polling has a long history in American life. The earliest systematic polls were conducted by magazines, in particular *Literary Digest*, which would include a returnable postcard with opinion questions to conduct "straw polls" on the issues of the day (Igo, 2007).[48] Beginning in the late 1930s, George Gallup, Elmo Roper, and Archibald Crossley began importing techniques from market research into the domain of public opinion polling.

Gallup established the American Institute of Public Opinion (AIPO) and set out to conduct nationwide surveys of American opinions on a range of social and political issues.[49] Gallup was scrupulously non-partisan, never running polls on behalf of a particular party. AIPO also devoted considerable efforts to develop neutral, easy to understand question wording. By 1940, about eight million people had read Gallup's tri-weekly polling report, *America Speaks!* which was syndicated in newspapers. Gallup and other pollsters made money by selling their results to businesses for consumer research and newspapers for public opinion.

B.2. Evolution of Gallup's sampling methodology

B.2.1. Gallup methodology before 1950

Before 1950, Gallup used so-called "quota-based" sampling. Survey-takers had to fill quotas for each pre-determined strata thought to capture distinct political views. Enumerators were given both hard (e.g., gender, must have one-third female) and soft (e.g., age, "get a good spread") quotas, but within each quota, interviewers had a lot of discretion. As Berinsky (2006a) notes, "interviewers preferred to work in safer areas and tended to question approachable respondents," which likely led to Gallup over-sampling, within each quota strata,

[47] See `https://ropercenter.cornell.edu/ipoll-database/`.

[48] The Earliest *Literary Digest* poll we could find referenced was a poll to predict the outcome of the 1916 presidential election.

[49] Similar organizations were formed at roughly the same time: Roper's company was steadily employed by Fortune magazine starting in 1935, Henry Cantril started the Organization of Public Opinion Research (OPOR) in 1940, and the University of Chicago's National Opinion Research Center (NORC) was founded in 1941.

more prosperous and well-off respondents.[50]

Gallup once noted that the "the voting public...is the universe of the opinion researcher," suggesting his aim was to be representative of *voters*, which implies substantial underrepresentation of certain segments of the population. Presumably because the South had low turnout (given many of its elections during this time did not even manage a Republican challenger), it was under-sampled. Southern blacks were differentially underrepresented among Southerners, consistent with their near total disenfranchisement during this period. Gallup purposely over-sampled men because of a belief that women merely adopted their husbands' opinions on Election Day.[51]

Documentation for Gallup surveys prior to 1950 describe the sampling procedure as follows:

> Prior to 1950, the samples for all Gallup surveys, excluding special surveys, were a combination of what is known as a purposive design for the selection of cities, towns, and rural areas, and the quota method for the selection of individuals within such selected areas. The first step in obtaining the sample was to draw a national sample of places (cities, towns, and rural areas). These were distributed by six regions and five or six city size, urban rural groups or strata in proportion to the distribution of the population of voting age by these regional-city size strata. The distribution of cases between the non-south and south, however, was on the basis of the vote in presidential elections. Within each region the sample of such places was drawn separately for each of the larger states and for groups of smaller states. The places were selected to provide broad geographic distribution within states and at the same time in combination to be politically representative of the state or group of states in terms of three previous elections. Specifically they were selected so that in combination they matched the state vote for three previous elections within small tolerances. Great emphasis was placed on election data as a control in the era from 1935 to 1950. Within the civil divisions in the sample, respondents were selected on the basis of age, sex and socioeconomic

[50]Berinsky, 2006a provides great detail on Gallup's quota-based sampling procedures, from which we draw much of the information in this subsection. Consistent with discretion within the quota-based sampling leading to oversampling of the well-to-do, Gallup over-predicts the Republican vote share in 1940 and 1944, though in both cases he still correctly predicts Roosevelt victories. In 1948, this over-sampling of Republican voters leads him to incorrectly call the election.

[51]It is worth noting that any oversampling of men is not a substantial problem for our purposes since we are interested in measures of union status and income at the *household* level. Since most men and women are combined in households, particularly in the earlier years, reports of "any union members in the household" and "household income" should not be affected by whether the surveyed individual in the household was male or female.

quotas. Otherwise, interviewers were given considerable latitude within the sample areas, being permitted to draw their cases from households and from persons on the street anywhere in the community.

B.2.2. Gallup methodology after 1950

From 1950 onward, Gallup uses modern-day probabilistic sampling procedures. Weights are often provided, but their documentation is not consistent. As a result, in our analyses of the Gallup data we use weights that we generate from the Census, as detailed in Appendix B.5.

The following excerpt is taken from post-1950 Gallup survey documentation on sampling:

> All Gallup polls since 1950, excluding certain special surveys, have been based on a national probability sample of interviewing areas. Refinements in the sample design have been introduced at various points in time since then. However, over this period the design in its essentials has conformed to the current procedure, as follows:
>
> 1. The United States is divided into seven size-of-community strata: cities of population 1,000,000 and over; 250,000 to 999,999; and 50,000 to 249,999; with the urbanized areas of all these cities forming a single stratum; cities of 2,500 to 49,999; rural villages; and farm or open country rural areas.
> 2. Within each of these strata, the population is further divided into seven regions: New England, Middle Atlantic, East Central, West Central, South, Mountain, and Pacific Coast.
> 3. Within each size-of-community and regional stratum the population is arrayed in geographic order and zoned into equal-sized groups of sampling units.
> 4. In each zone, pairs of localities are selected with probability of selection proportional to the size of each locality's population–producing two replicated samples of localities.
> 5. Within selected cities for which population data are reported by census tracts or enumeration districts, these sample subdivisions are drawn with probability of selection proportional to the size of the population.
> 6. For other cities, minor civil divisions, and rural areas in the sample for which population data are not reported by census tracts or enumeration districts, small, definable geographic areas are drawn, with the probability

of selection proportional to size where available data permit; otherwise with equal probability.

7. Within each subdivision selected for which block statistics are available, a block or block cluster is drawn with probability of selection proportional to the number of dwelling units.

8. In cities and towns for which block statistics are not available, blocks are drawn at random, that is, with equal probability.

9. In subdivisions that are rural or open country in character, segments approximately equal in size of population are delineated and drawn with equal probability.

10. In each cluster of blocks and each segment so selected, a randomly selected starting point is designated on the interviewer's map of the area. Starting at this point, interviewers are required to follow a given direction in the selection of households, taking households in sequence, until their assigned number of interviews has been completed.

11. Within each occupied dwelling unit or household reached, the interviewer asks to speak to the youngest man 18 or older at home, or if no man is at home, the oldest woman 18 or older. This method of selection within the household has been developed empirically to produce an age distribution by men and women separately which compares closely with the age distribution of the population. It increases the probability of selecting younger men, who are at home relatively infrequently, and the probability of reaching older women in the household who tend to be under-represented unless given a disproportionate chance of being drawn from among those at home. The method of selection among those at home within the household is not strictly random, but it is systematic and objective and eliminates interviewer judgement in the selection process.

12. Interviewing is conducted at times when adults are most likely to be at home, which means on weekends or if on weekdays, after 4:00 p.m. for women and after 6:00 p.m. for men.

13. Allowance for persons not at home is made by a "times-at-home" weighting procedure rather than by "call-backs." this procedure is a standard method for reducing the sample bias that would otherwise result from underrepresentation of persons who are difficult to find at home.

14. The pre-stratification by regions is routinely supplemented by fitting each obtained sample to the latest available census bureau estimates of the regional distribution of the population. Also, minor adjustments of the sample are made by educational attainment (by men and women separately), based on the annual estimates of the census bureau derived from their current population survey. The sampling procedure described is designed to produce an approximation of the adult civilian population living in the United States, except for those persons in institutions such as hospitals.

Note that not until the 1980s does Gallup switch from face-to-face interviews to phone interviews. For this period we make use of the much larger CPS data instead of Gallup, so the vast majority of our Gallup data comes from face-to-face interviews.

B.3. The Gallup union question

The typical Gallup union question is "Are you (or is your husband) a member of a labor union?", with the choices most often being: "neither," "yes, I am," "yes, he is," "yes, both are." In 1959, "husband" changes to "husband/wife." In some years, however, the question does *not* ask which member or members of the household is or are in a union, so we cannot, for example, always measure individual union status. We harmonize these questions to form a measure of *household* union status, where we code a household as union if either household head or spouse is a union member. While technically the implied unit of observation is *couple* (or individual if the respondent is not part of a couple), we will generally refer to this measure as *household union status*. Importantly, Gallup asks this question of *all* respondents, not skipping those in, say, agricultural occupations or who are unemployed.

B.4. Weighting the Gallup data

To construct weights, we use post-stratification methods (i.e., cell-weighting). Specifically, we weight observations in the Gallup data so that the annual proportions of education-race-region cells in Gallup match the corresponding proportions in U.S. Census data. The process involves several steps: First, we construct comparable measures of education (less than high school, high school graduate, some college, college graduate), race (white, non-white), and region (South, non-South) in both Gallup and Census data. Second, we construct annual proportions of each education-race-region cell for each dataset. In the Census data, we apply representative household weights and linearly interpolate values for intercensal years to best approximate the "true" annual proportions of each cell. Third, we generate cell-specific weights w_{ct} by applying the following formula:

$$w_{ct} = \frac{\pi_{ct}^C}{\pi_{ct}^G} \tag{9}$$

where c denotes a particular education-race-region cell (e.g., white Southerners with a college degree), and π_{ct}^C and π_{ct}^G denote annual cell proportions for Census and Gallup, respectively. Finally, we let $w_{it} = w_{ct}$ for each respondent i in year t corresponding to cell c in the Gallup data and re-normalize so $\sum_i^{N_t} \frac{w_{it}}{N_t} = 1$ for each year t.

We repeat the procedure above for several alternative cell definitions (e.g., education-race-age-state, age-gender-region). Our preferred weights use education-race-region cells because we find this definition makes our sample as representative as possible without compromising comparability across surveys or creating excessively small or "empty" cells.[52] For surveys without education data, we use race-region weights.

B.5. Comparing Gallup to Census Microdata

We begin with Gallup data from 1950 onward, returning shortly to earlier data. Table B.1 compares Gallup data to 1950–1980 Census data. To summarize how the *actual* (unweighted) Gallup observations compare to the full U.S. adult population, we compare unweighted Gallup data to Census IPUMS tabulations. Given Gallup's well-documented under-sampling of the South, we show results separately for Southern and non-Southern states.

In 1950, Gallup exhibits some under-sampling of the South, but, by 1960, this bias had disappeared. From 1950 to 1960, Gallup under-sampled blacks in both the South and the Non-South. This bias continued in the South through 1970, to a smaller degree. These biases reflect the substantial disenfranchisement of blacks, particularly in the South during this period. Age and gender appear representative in Gallup in both regions in each decade.

Gallup respondents outside the South are more educated than their Census counterparts, with the largest gap being a high school completion difference of around 8.5 percentage points in 1950 and 1960. In the South, except for 1950, Gallup and IPUMS show similar levels of education. Gallup Southern respondents have higher high school completion rates than those in the Census in 1950, as Gallup was still under-sampling Southern blacks in that year. In Appendix D we show some of our key results with the Gallup data both unweighted and weighted to match Census characteristics, but Appendix Table B.1 gives some sense of how much "work" the weights must do.

[52]For a more thorough discussion of post-stratification weighting, including optimal cell "fineness," see Berinsky, 2006b

Appendix Table B.2 looks separately at 1940, given that Gallup's sampling procedures were quite different during its earlier years. In fact, in 1940, very few Gallup surveys ask about education (the summary statistics we present for that variable are based on only 5,767 observations), so in this table we include occupation categories as supplemental proxies for socio-economic status. The first column shows, again, unweighted Gallup data. Col. (2) presents summary statistics for all adults in the 1940 IPUMS. Perhaps the most striking discrepancy is gender: consistent with their stated methodology at the time, Gallup over-samples men. Col. (3) adjusts the Census sampling so that men are sampled at the Gallup frequencies and also down-weights large households (since Gallup only interviews one person per household). Comparing col. (1) versus (3) shows, as expected, that Gallup significantly under-samples the South.

Consistent with concerns about Gallup over-sampling the affluent, Gallup respondents in 1940 are substantially more educated than their Census counterparts. Unfortunately, given that only in 1942 does Gallup begin to regularly include an education question, the Gallup sample for which we have an education measure in 1940 is quite small (about 5,700 individuals, relative to over 150,000 for the other Gallup variables in 1940). Given the small education sample in 1940, we use occupational categories to further explore socio-economic status in Gallup versus the 1940 Census. Gallup and IPUMS use different occupation categories—Gallup's are much coarser and unfortunately IPUMS categories do not completely nest Gallup categories—so comparisons are not straightforward. Consistent with the concerns cited earlier that Gallup over-sampled the well-to-do, Gallup respondents appear to have slightly higher-status occupations relative to their Census counterparts, with "white-collar" workers significantly overrepresented.

For the most part, these patterns hold when we drop Southern states from both samples (the final two columns of Table B.2). Importantly, outside of the South, Gallup appears to sample blacks in proportion to their population, even in the very early years of its existence. Also, outside the South, Gallup appears to accurately sample the remaining six regions of the US.[53]

In general, we show results with Gallup data using weights to match (interpolated) Census IPUMS summary statistics, even though the need for weights is not obvious after 1960. From 1937 until 1941, we weight so that Gallup matched the IPUMS in terms of $White \times South$ cells, given that the summary statistics show that Gallup sampling along these dimensions appears suspect in the early years. Beginning in 1942 (the first year in which Gallup surveys ask the union and education questions in the same survey) we weight by $White \times Education \times South$, where $Education \in$

[53] We use Gallup-defined geographic regions in this table.

{No high school degree, HS degree, Some college, College graduate}, thus giving us $2 \times 4 \times 2 = 16$ cells on which to match. In practice, however, our results are very similar with and without weights.

References

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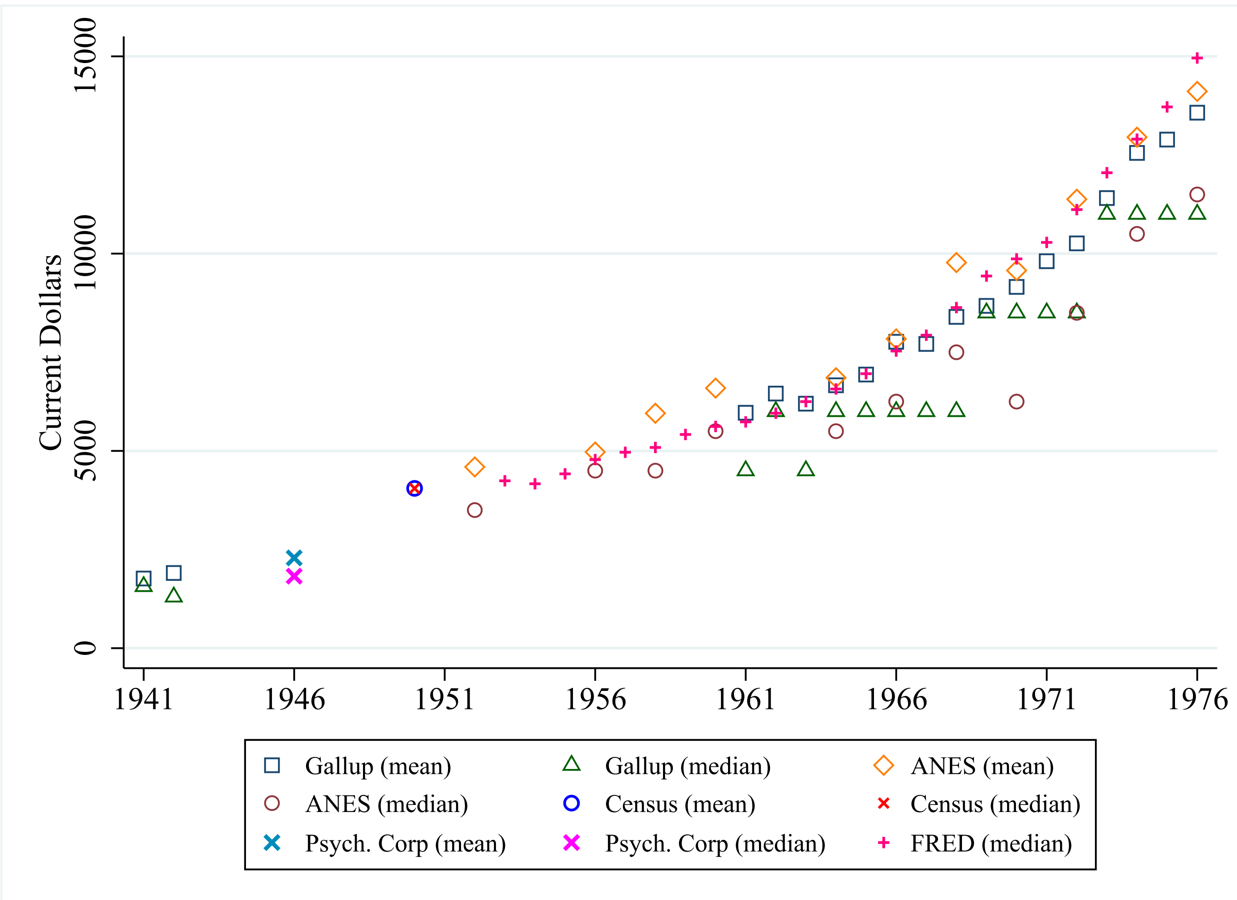
Appendix Table B.1: Comparing Gallup and IPUMS, 1950–1980

	1950		1960		1970		1980	
	Census	Gallup	Census	Gallup	Census	Gallup	Census	Gallup
South Share	0.258	0.133	0.260	0.254	0.270	0.262	0.295	0.264
—*South*								
Female	0.530	0.516	0.532	0.539	0.528	0.515	0.521	0.508
Age	39.48	40.90	41.18	42.99	41.12	41.97	39.84	41.45
Black	0.205	0.0759	0.183	0.137	0.159	0.124	0.159	0.157
HS grad.	0.280	0.405	0.387	0.376	0.513	0.565	0.674	0.703
—*Non-South*								
Female	0.523	0.508	0.520	0.527	0.523	0.514	0.517	0.510
Age	40.64	40.44	41.68	41.71	41.33	41.47	39.97	40.63
Black	0.0506	0.0479	0.0638	0.0577	0.0742	0.0616	0.0816	0.0880
HS grad.	0.418	0.502	0.491	0.578	0.634	0.712	0.768	0.814
Observ.	250519	91682	4488254	23620	2023944	75911	6186033	59138

Sources: Gallup surveys and 1950–1980 IPUMS.

Notes: We use the Gallup definition of the "South": all eleven states of the former Confederacy plus Oklahoma. All Census results use IPUMS person weights.

Appendix Figure B.1: Household income measures in our historical survey data compared to official statistics



Sources: See Section 2 for a description of each of our historical data sources. The 1950 data points come from the Census and from 1953 onward from Federal Reserve Economic Data (FRED).

Notes: As our historical data sources are unfamiliar and non-standard sources of household income, we compare them to official government statistics. Beginning in the 1970s, we use the more standard CPS and thus do not show comparisons.

Appendix Table B.2: Comparing Gallup and IPUMS in 1940

	Gallup	Census	Census	Gallup	Census
—*Demographics*					
Black	0.0291	0.0895	0.0906	0.0325	0.0357
Female	0.338	0.505	0.344	0.341	0.343
Age	40.46	39.61	40.06	40.41	40.55
HS Graduate	0.493	0.278	0.266	0.494	0.290
College Graduate	0.0720	0.0472	0.0499	0.0709	0.0543
—*Geography*					
Northeast	0.0836	0.0660	0.0629	0.0947	0.0854
Mid Atlantic	0.261	0.253	0.241	0.295	0.327
East Central	0.208	0.187	0.186	0.236	0.252
West Central	0.177	0.127	0.129	0.200	0.175
South	0.117	0.258	0.263	0	0
Rocky Mountain	0.0752	0.0284	0.0308	0.0851	0.0418
Pacific Coast	0.0783	0.0754	0.0818	0.0887	0.111
—*Occupation*					
Farmer	0.213	0.156	0.159	0.188	0.109
Professional	0.0792	0.113	0.122	0.0808	0.129
Propietors, managers, officials	0.0105	0.0928	0.0875	0.0108	0.0933
Clerks (white collar)	0.299	0.0535	0.0539	0.306	0.0609
Skilled workmen and foremen	0.0926	.	.	0.0970	.
Unskilled or semi-skilled labor	0.194	.	.	0.204	.
Sales workers	.	0.0462	0.0457	.	0.0499
Craftsmen	.	0.142	0.139	.	0.153
Operatives	.	0.146	0.147	.	0.159
Service workers (priv. HH)	.	0.0103	0.0105	.	0.00626
Other service workers	.	0.0477	0.0468	.	0.0508
Laborers	.	0.0932	0.0973	.	0.0944
No answer, N/A, etc.	0.111	0.0999	0.0920	0.113	0.0949
Gender/HH adj?	No	No	Yes	No	No
Ex. S/SW?	No	No	No	Yes	Yes
Observations	144996	736832	736832	127995	544375

Sources: Gallup surveys and 1940 IPUMS.
Notes: The Gallup sample size varies substantially by variable during this period. For the col. (1) sample, all demographics except for education and all geographic variables have a sample size around 159,000 (with small variations due to missing observations). The occupation codes have a sample size of roughly 21,000. The high school completion indicator has a sample size of 5,700. In col. (4) each sample size is roughly twelve percent smaller. "HH / gender adjustment" underweights women and people in large households in the IPUMS to better match Gallup sampling (which only sampled one person per household and had a target female share of one-third). "Ex S/SW" excludes Southern and Southwestern states (all eleven states of the former Confederacy plus Oklahoma). Note that occupation categories are coarser in Gallup than in the Census (but unfortunately, Gallup categories do not nest Census categories). We do our best to match occupation across these different categorizations. All Census results use IPUMS person weights.

Appendix Table B.3: Summary statistics from supplementary data sets

	(1) ANES	(2) BLS exp. dataset	(3) U.S. Psych. Corp.	(4) NORC	(5) ANES panel
Union household	0.240	0.116	0.172	0.274	0.284
Female	0.596	0.507	0.496	0.514	0.538
White	0.848	0.819	0.890	0.903	0.906
Age	41.35	40.98	42.13	39.84	41.72
HS graduate	0.738	0.363	0.442	0.403	0.532
South	0.288	0.271	0.208	.	0.239
Log fam. inc.	10.73	10.07	10.11	7.913	8.511
Sample period	1952-2012	1936	1946	1950	1956-1960
Observations	32475	5517	5665	1106	3783

Notes: See Section 2.2 and Appendix B for details on the data sources.

Appendix C. Sample selection and construction of key variables

C.1. Sample Selection

To construct our main Gallup sample, we apply the following selection criteria to the population of recorded Gallup survey respondents from years 1937 through 1987. First, we eliminate respondents to surveys in which the union membership question was not asked. Second, we remove any respondents younger than 21 or older than 64 (we cap at 65 to focus on the working-age population, and only halfway through our sample period did Gallup begin to include 18-20 year olds and we wish to have a consistent sampling rule throughout the entire period). Third, we remove respondents who live in Alaska, Hawaii, or Washington DC (again, Gallup did not include these respondents at the beginning of our sample period). For the state-year analyses we also exclude Idaho because the state identifiers are often miscoded as Hawaii.

Our CPS sample is taken from the May supplements in years 1976 to 1981, the Merged Outgoing Rotation Groups in years 1983 to 1989, and the Annual Social and Economic Supplement in years 1990 to 2015. Note that the CPS did not ask about union status in 1982. Since the CPS contains information for all individuals within a household, to make the CPS comparable with Gallup, we restrict our CPS sample to one randomly selected observation from each household, which we refer to as the "designated" respondent.[54] For state-year measures our CPS-based series begins in 1977, as individual state-of-residence identifiers are not available before that time. We exclude designated respondents in armed forces. Additionally, we exclude Alaska, DC and Hawaii from all analyses, and Idaho from the state-year analysis to make it comparable with the Gallup sample.

C.2. Variable Construction

Union Density In both Gallup and CPS, union density is calculated as the number of households with at least one reported union member divided by the total number of households. The Gallup sample is limited to respondents aged 21-65 whereas the CPS sample is limited to "designated" respondents aged 18-65.

Family Income Our Gallup measure of family income covers years 1942 and 1961 through 1976. Gallup family income is derived from the responses to survey questions of the following form: "Which best represents the total annual income, before taxes, of all the members of

[54] The exception to this is Appendix Figure D.5, which examine the robustness of our premium estimates to using all observations within a household.

your immediate family living in your household?" Responses are coded into income bins which vary across surveys. We construct a harmonized income measure by calculating the midpoint of each interior binned response. For top and bottom bins, we estimate implied midpoints from a fitted Pareto distribution as in Von Hippel, Hunter, and Drown (2017). Our CPS measure of family income is taken from the May and March supplements in years 1978 through 2015. This measures combines all reported income from household members 15 years and older. To construct this variable in early CPS years (May and March before 1990), we use the family income variable, which is binned into 12 categories. For the following years (CPS March only) we use the continuous family income variable, which reports the total income for the respondent's family. To make the continuous variable comparable with the binned variable of earlier years, we recode it into bins matching those of the ANES income variable in the corresponding year.

College premium, college high school share ratio, wage ratios The college wage premium, college high school share ratio, and the 90-10, 90-50, and 10-50 wage ratios are calculated using a sample of 18 to 65 year-old full-time, full-year wage and salary workers who make at least one-half of the minimum wage and who have 0-48 years of potential experience in the March CPS (1964-2019 for the time series analysis and 1977-2019 for the state-year analysis) and the 1940-1970 Census.[55] Unemployed and NILF respondents are excluded from the analysis.

In the time-series analysis, we calculate changes in each measure between 1940-50, 1950-60, and 1960-70 in the Census data and append these changes to the measure from 1964-2019 (or 1977-2019 in the state-year analysis) calculated from the March CPS.

The **college-high school share ratio** is calculated in terms of efficiency units following the methodology outlined in Autor, Katz, and Kearney, 2008. Workers are divided into cells based on two sexes, five education categories (high school drop outs, high school graduates, some college, college graduates, greater than college), and years of experience (ten-year bins for the state-year analysis).

For each cell in each year we calculate the weighted sum of weeks worked by all individuals in the cell using the individual weights from the data. This comprises the "quantity" of labor supplied. To translate this into efficiency units of labor supply we also calculate the "price" of each week of labor in a particular cell. The "price" of labor corresponds to weighted average of log real weekly income in each cell, normalized by a reference wage (the wage of male high school graduates with the highest category of experience cell in our data, which is

[55] We follow Autor, Katz, and Kearney, 2008 and calculate years of potential experience as age minus assigned years of schooling minus six, rounded down to the nearest integer value.

40-48 in the main sample but 30-40 in the backwards projection to 1930 described below), and averaged over the entire period. The efficiency units of labor supplied by each cell is the product of the "quantity" and "price" of labor.

The total efficiency units of labor supplied in a given year is calculated by summing across cells. We calculate aggregate college-equivalent labor supply as the share of total efficiency units of labor supplied by college or college-plus workers plus half of the share of labor supplied by workers with some college. The college-high school share ratio is the natural logarithm of the ratio of college-equivalent to non-college-equivalent labor supply shares in each year.

As the 1930 census does not ask years of schooling, we construct the 1929 college-high school share ratios by projecting backwards from cohorts in 1940, using their state of residence in 1935. We use the efficiency units in 1940 aggregated across 34-64 age groups, which are the cohorts that would be 24-54 in 1930. The correlation between these age groups in 1940 is 0.885 and 0.883 updated by migration, which validates the backward projection for that year.

The **college wage premium** is calculated following the methodology outlined in Autor, Katz, and Kearney, 2008 and Goldin and Katz, 2008. The premium is the fixed weighted average of the premium earned by college graduates vs high school graduates and more than college educated workers vs high school graduates. These premiums are estimated by regressing the log real hourly earnings on a set of five education dummies, a full-time dummy, a female dummy, a non-white dummy, a set of three geographic division dummies, a quartic in experience and the interaction of female with both non-white and the quartic in experience. The weights are the relative employment shares of college and more than college educated workers in 1980.

Weights are calculated as follows:

$$CollegeShare = \frac{\text{Number of workers with exactly college education}}{\text{Number of workers with exactly college or more than college education}}$$

$$MoreThanCollegeShare = \frac{\text{Number of workers with more than college education}}{\text{Number of workers with exactly college or more than college education}}$$

The **90-10, 90-50, and 10-50 wage ratios** are calculated as the difference in the Xth and Yth percentile of log real weekly earnings among men in our sample.

Gini coefficient For the aggregate time-series analysis, the Gini coefficient is taken from Kopczuk, Saez, and Song, 2010. For the state-year analysis, we estimate the Gini coefficient from a sample of 18 to 65 year-old workers who are not self-employed, have non-allocated income, and have 0-48 years of potential experience in the March CPS (1977-2019) and the

1940-1970 Census. We append changes in the Gini coefficient between 1940-50, 1950-60, and 1960-70 in the Census data to the coefficient in 1977-2019 calculated from the March CPS.

Manufacturing Employment We estimate major industry employment shares from 1910 to 2015 by combining data from the Census, BLS State and Area Employment, Hours and Earnings series, and ACS. Although the BLS is our preferred data source, it is only available between 1939 and 2001. Furthermore, not every state-industry pair has data beginning in 1939, and for some pairs data starts as late as 1982. We therefore supplement the BLS series with Census data from 1910 to 1980 and ACS data from 2001 to 2015.

For each dataset, state, and year we group calculate the share of employed individuals that work within each major industry: mining, construction, manufacturing, transportation, trade, finance, services, and government. We group 1950 census industry codes in the Census and ACS to match these BLS industries.[56] To combine the Census and ACS with the BLS, we append changes in the Census and ACS to the BLS series in its first and last year, respectively.

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[56]Mining corresponds to 1950 census industry codes 206-239, construction to 246, manufacturing to 306-499, transportation to 506-598, trade to 606-699, finance to 716-756, services to 806-899, and government to 906-976.

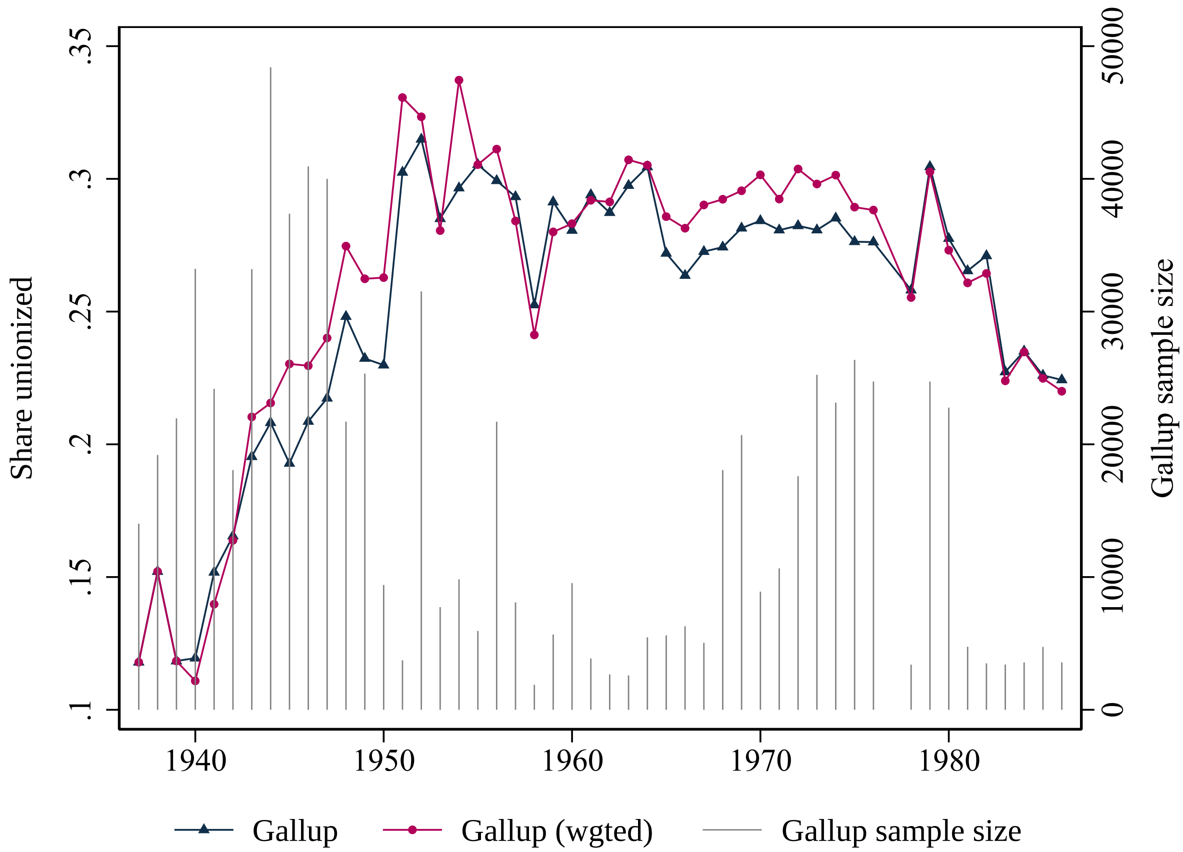
Appendix D. Main results using various weighting schemes and individual- instead of household-level union membership

As described in Section 2 and Appendix B, two issues in the Gallup data complicate comparisons with the CPS and other standard data sources. First, especially in its first few decades, Gallup polls over-sampled the well-off and under-sampled all Southerners but particularly black Southerners. Second, we cannot always infer individual-level union membership in the Gallup and other historical survey data, so instead we mostly use a household-level measure (i.e., is anyone in the household a union member).

An obvious concern is that some of the trends in the size of the union premium or selection into union that we document over our long sample period are in fact artifacts of these aspects of Gallup's data. For example, changes in selection into union *households* might reflect changes in assortative mating and not union membership *per se*.

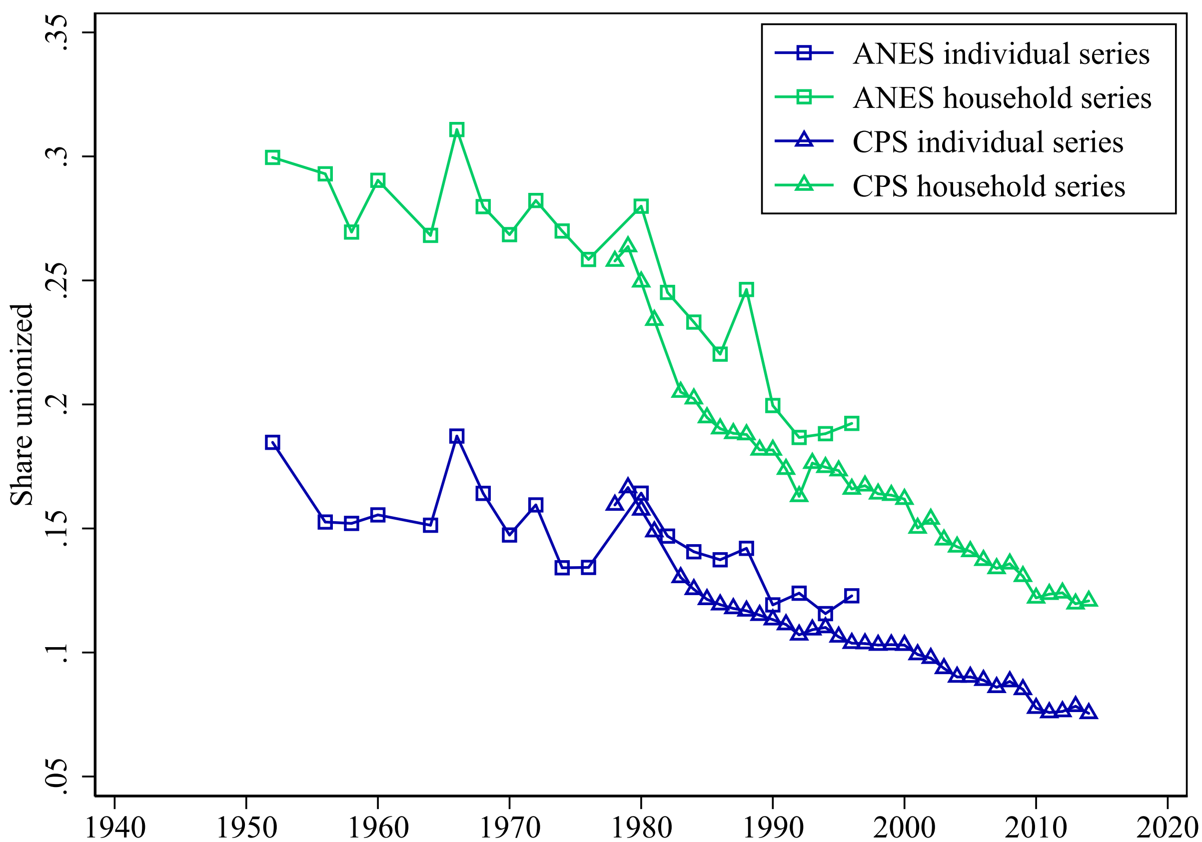
In this appendix, we reproduce, when possible, some of our main results (a) under various weighting schemes and (b) using an individual- instead of household-based measure of union membership. We also show some results for men only, as in the early years union membership was almost entirely male. Thus, for this subsample the household membership will closely proxy individual membership.

Appendix Figure D.1: Union share of households in the Gallup data (weighted vs. unweighted)



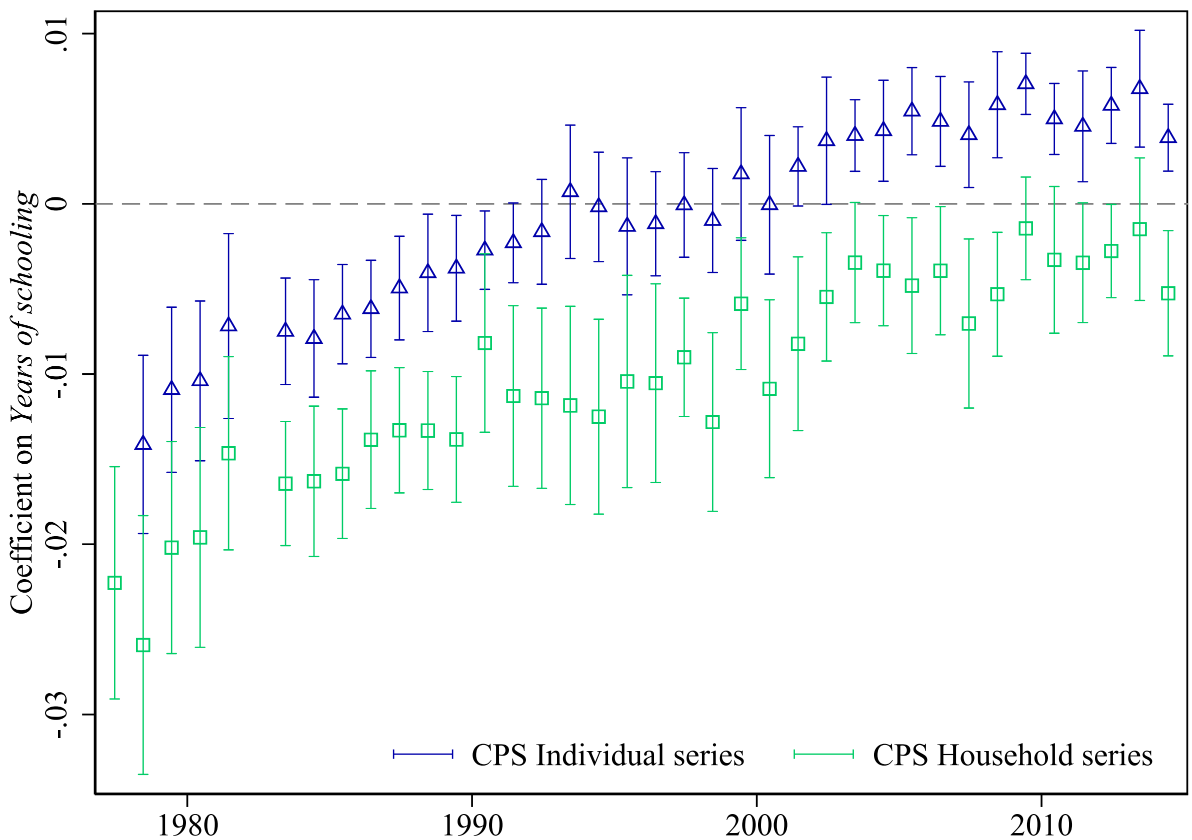
Data sources: Gallup. See Section 2.2 and Appendix B for more details on data and weight construction.

Appendix Figure D.2: Comparing individual versus household union density in CPS and ANES, 1952–present



Data sources: Current Population Survey and American National Election Survey

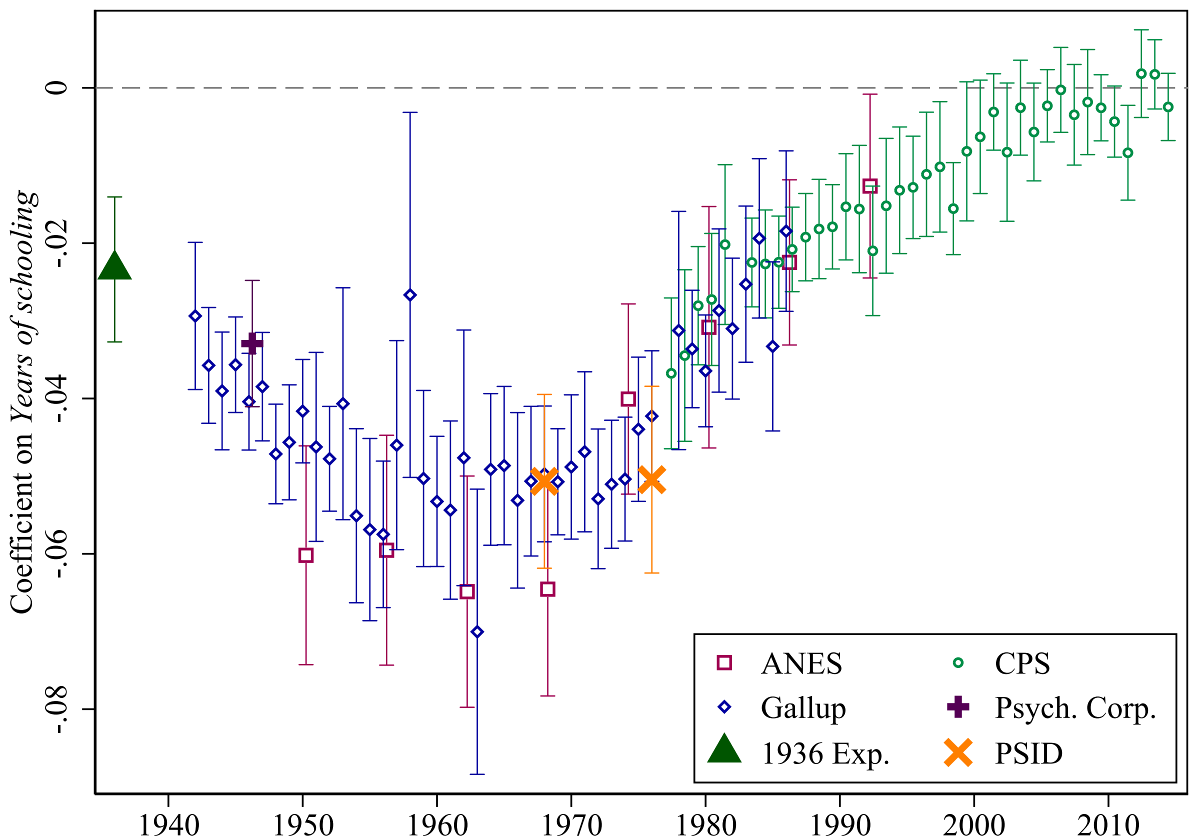
Appendix Figure D.3: Selection into unions by years of scholling in the CPS, individual and household measures



Data sources: Current Population Survey.

Notes: The "household series" replicates the CPS analysis in Figure 3 (i.e., regresses, separately by year, a household union dummy on years of schooling, gender and state fixed effects, plotting the coefficient on years of schooling. The "individual series" substitutes *individual union membership* as the outcome variable instead of the household union dummy.

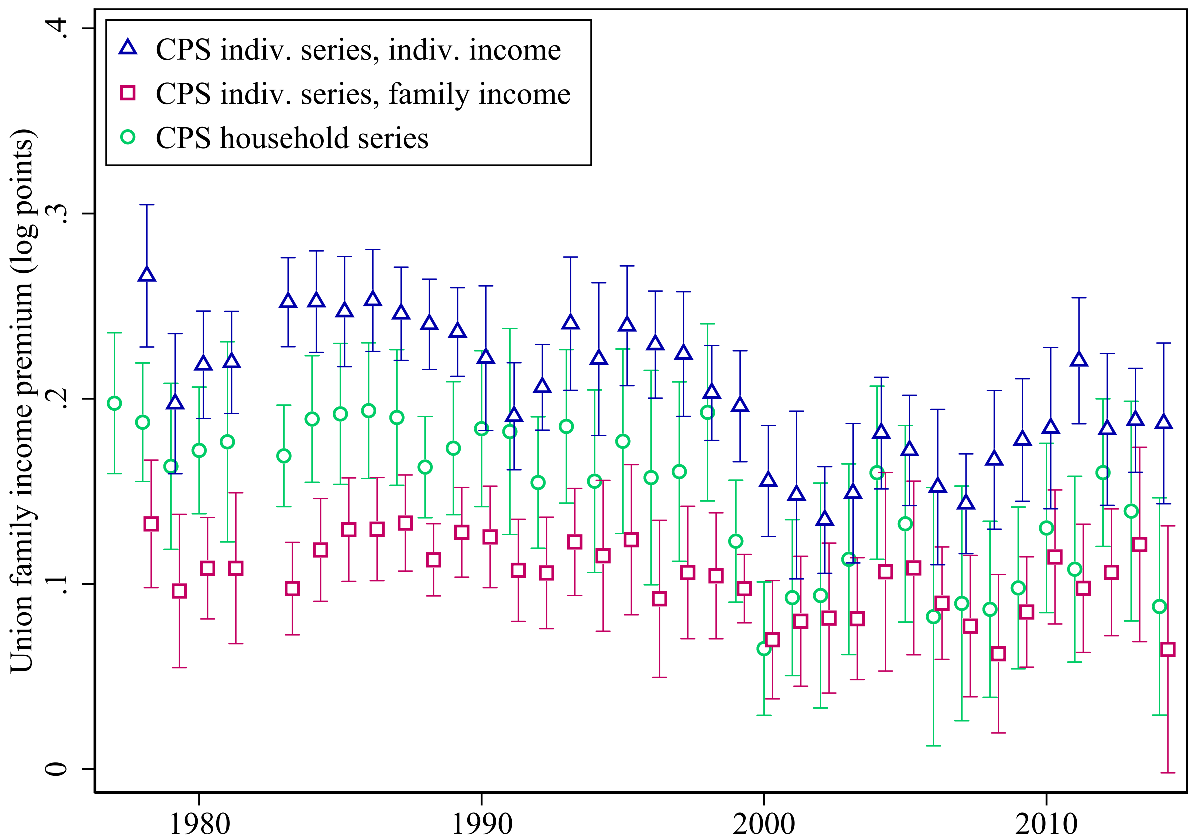
Appendix Figure D.4: Selection into unions by education, male survey respondents only



Data sources: Gallup data, 1937–1986; CPS, 1978–2016; BLS Expenditure Survey, 1936; ANES; 1952–1996; U.S. Psych. Corporation, 1946. See Section 2.2 for a description of each data source.

Notes: We regress household union status for male respondents only on $Years\,of\,education$, state s and survey-date t fixed effects, age and its square, and gender. (The notes to Figure 6 describe how we impute years of schooling if the survey source only gives us categories of educational attainment.) We estimate this equation separately by survey source and by year. The figure plots the coefficient on $Years\,of\,education$. For the ANES, because the samples are smaller, we group surveys into six-year bins. The plotted confidence intervals are based on standard errors clustered by state.

Appendix Figure D.5: Comparing union family and individual premium in the CPS



Data sources: CPS, 1978–2016. See Appendix C for details on CPS individual and family income variable construction.

Notes: Each plotted point comes from estimating equation (2), which regresses log family income on a union dummy and controls for age, gender, race, and state fixed effects. Occupation controls are not included. For each series, we estimate a separate regression for each year. The first series regresses log *individual* earnings on *individual*-level union membership. The second series regresses log *family* income on *individual*-union membership. The third series regresses log *family* income on whether the individual has a union member in the *household* (whether or not the individual himself is in a union) and is the concept we use in most of the paper. The plotted confidence intervals are based on standard errors clustered by state.

Appendix Table D.1: Gallup selection results through 1950, robustness to weights

	Dependent variable: Union household			
	(1)	(2)	(3)	(4)
yrsed	-0.0394***	-0.0386***	-0.0369***	-0.0307***
	[0.00309]	[0.00274]	[0.00299]	[0.00266]
Dept. var. mean	0.257	0.258	0.258	0.195
Weighting scheme	Baseline	None	White x Sth	Schickler
Observations	600744	610126	610126	62085

Sources: See Section 3 and Appendix B for details.

Notes: All regressions include state and survey-date fixed effects. Respondents are include ages 21–64. Baseline weights are those we use throughout the paper (weights to make Gallup match interpolated Census cells for $White \times South \times Education\,categories$ (16 cells)). $White \times Sth$ are analogous, but match only on those four cells. Raking weights are constructed by matching yearly marginal mean population shares by *Black*, *Female*, and *Region* to interpolated census shares. See Deville, Särndal, and Sautory, 1993 for more details. "Schickler weights" are taken from Schickler and Caughey, 2011 and match on *Black* and whether a residence has as phone. They are only available through 1945. Standard errors in brackets, clustered by state.
$^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix E. Existing Measures of Union Density Pre-Dating the Current Population Survey

The CPS first asks respondents their union status in 1973, and then only in selected months until 1983 from which time information on union status was collected each month in the CPS as part of the outgoing rotation group supplement. Before this survey, the primary sources for union density are the BLS and Troy/NBER historical time series mentioned in the introduction. The data underlying these calculations are union reports of membership and dues revenue when available, and a variety of other sources when not available. Neither of these data sources ever used representative samples of individual workers to calculate union density.

In general, the data derived from union reports likely become more accurate by the 1960s. Post-1959 the BLS collected mandatory financial reports from unions as a condition of the Labor-Management Reporting and Disclosure (Landrum-Griffin) Act, and Troy and Sheflin (1985) incorporate these data into their estimates of union density. Beginning in 1964, the BLS disaggregates union membership counts by state, and Hirsch, Macpherson, and Vroman (2001) splice these reports together with the CPS to form state-year union density panel beginning in 1964 and continuing through today.[57]

Before the 1960s, however, union data were far less standardized. In the remainder of this section, we detail the methodology of the two most widely used data sources on aggregate union density: the BLS and Troy series.

E.1. The BLS Estimate of Early Union Density

The BLS series is based on union-reported membership figures starting in the late 1940s. Prior to 1948, the methodology for calculating union membership does not appear standardized. For example, the 1945 Monthly Labor Report notes as its sources: "This study is based on an analysis of approximately 15,000 employer-union agreements as well as employment, union membership, and *other data available to the Bureau of Labor Statistics* [emphasis ours]" (Bureau of Labor Statistics, 1945)[58]

[57]Freeman et al. (1998) constructs a time-series of union density from 1880 to 1995, splicing together the official series from the BLS with series constructed from the CPS. Freeman reports alternative series constructed by other scholars (Troy (1965), Troy and Sheflin (1985),Wolman (1924), and Galenson (1960)) in the Appendix to his paper.

[58]For example, one alternative source the BLS used was convention representation formulas. "Convention formulas" specified the number of seats, as a function of membership, each union would have at the umbrella organization convention. By inverting this formula and using the convention records, rough estimates of union membership could be formed.

It is obviously hard to verify information from unspecified "sources available to the BLS" but even in instances where the BLS can rely on union membership reports, concerns arise. A key issue is that unions had important incentives to over-state their membership and until the late 1950s faced no penalty for doing so. In the early and mid-1930s, the main umbrella organization for local unions was the American Federation of Labor (AFL). They were often charged with over-stating their membership, presumably to inflate their political influence. For example, a 1934 *New York Times* story casts doubt on the AFL's claim to represent over six million workers, noting that "complete and authoritative data are lacking" and that the figures provided by the AFL "are not regarded as accurate."[59] Individual unions also had an incentive to inflate the numbers they reported to the AFL. For example, the number of seats each union would receive at the annual convention was based on a formula to which membership was the main input.

If anything, these incentives to over-report likely grew after 1937, when the Committee on Industrial Organization broke away from the AFL to form a rival umbrella organization, the Congress of Industrial Organizations (CIO). Both federations of labor, the AFL and CIO, now competed for local unions to join their umbrella organizations, as well as for sympathies of government officials, tasks that were aided by a public perception that the federation was large and growing. Based on our read of *New York Times* articles on unions in the late 1930s and early 1940s, one of the most common if not the most common topic is the conflict between the two federations.[60] Individual unions still had incentives to compete for influence within their given federation, and thus inflate membership.

Membership inflation became such an issue that the federations themselves may not have known how many actual members they had. In fact, the CIO commissioned an *internal* investigation into membership inflation, conducted by then-United Steelworkers of America president Philip Murray. Murray's 1942 report concluded that actual CIO membership was less than fifty percent of the official number the federation was reporting (Galenson, 1960).

[59]See, *"Organized Labor is Put at 6,700,000"*, New York Times, May 1935. reporting that "For one thing, complete and authoritative data are lacking, and this is especially true during times of depression, when some unions drop unemployed workers from the rolls and exempt them from paying dues. . . . . The [AFL] reported an average membership of 2,609,011 for the year ended Aug. 31, 1934. These official figures, which are not regarded as an accurate measure of the movement, are far below the peak figure of 4,078,740 for 1920."

[60]As just one example, a 1938 *NYT* headline and subtitles read: "Green Says Lewis Falsified Report; A.F.L. Head Alleges Statement on C.I.O. membership is an 'Amazing Inflation; Questions Income Data," referring to AFL head William Green and CIO head John Lewis, respectively.

E.2. The Troy Estimates of Early Union Density

In his NBER volumes estimating union density, Troy is well aware of the problems documented above with the BLS estimates. For this reason, he defines membership as "dues-paying members" and proceeds to estimate union membership using unions' financial reports where available, presumably under the assumption that financial reports were less biased than membership reports. For each union, he divides aggregate union dues revenue by average full-time member dues to recover an estimate of union membership. While Troy is cognizant of the limitations of his data and methodology, he believes the biases are largely *understating* union membership (e.g. some groups, such as veterans, pay lower than average or no dues).

But union financial reports, like membership reports, are also not verified until the late 1950s. Nor is it obvious that union revenue data are not similarly inflated (in fact, the AFL accused the CIO of lying about their income data, as we mention in footnote 60). Moreover, revenue data are largely incomplete for the 1930s and 1940s. For example, in his 1940 estimates, Troy (1965) notes that the sources for 54.4% of his total is *not* in fact from financial reports, but instead an "Other" category, which includes personal correspondence with unions, asking their membership.[61] As such, for these early years, the Troy data in fact appears to face the same issue with membership-inflation as does the BLS data.[62]

In addition, Troy imputes the membership of many CIO unions in the late 1930s and 1940s by assigning them the membership of their AFL counterpart in the same sector.[63] This procedure likely over-states CIO membership, given that the AFL was believed to be twice as large as the CIO during this period (we also find this 2:1 ratio in our Gallup data), though obviously that average ratio may vary by sector.

In summary, while a likely improvement over the BLS series, it is difficult to believe that Troy's estimates (or Troy and Sheflin (1985)) are without extensive mismeasurement. Given the limitations of the existing pre-CPS data on union density, in the next section we introduce a new source: Gallup and other opinion surveys.

[61] "Other" is down to 10% by 1960 (Troy (1965)).

[62] Troy (1965) also only presents validation exercises for his post-1950 data, comparing reported measurement with that inferred from dues receipts for the Chemical and Rubber Workers in 1953, leaving it open whether the BLS or Troy (or neither) is correct for the pre-1950 series.

[63] From Troy (1965) [pp. A53]: "The average membership per local industrial union is arbitrarily estimated to be 300, and this figure is multiplied each year by the number of such unions reported by the ClO. The estimate of an average membership of 300 is deemed a fair one since the average membership of the local trade and federal labor unions of the AFL, a class of unions similar to the local industrial unions of the CIO, varies from a low of 82 in 1937 to a high of 193 in 1948."

E.3. Other pre-CPS state-year measures of union density

The only sources of state-year data on union density prior to the CPS we are aware of are measures created by Hirsch, Macpherson, and Vroman (2001) from BLS reports (which begin disaggregating union membership regionally, often by state, in 1964) from 1964-1977, and measures created by Troy and Sheflin (1985) for the years 1939 and 1956. Our Gallup measure is quite highly correlated (correlation = .724) with the existing Hirsch-Macpherson measures (individual union density as a fraction of non-farm employment) for the 1964-1986 years, which are where there is overlap. This correlation increases to .75 when we restriction attention to the CPS years with state identifiers (1978-1986).

The historical Troy measures for 1939 and 1953 are constructed from even more fragmentary records than the annual series we discuss above (as many union reports did not disaggregate either revenue or membership by state). Nevertheless our Gallup measures are also correlated with these data in both cross-sections and changes (1939 correlation = 0.78, 1953 correlation = 0.75, correlation in changes =0.5).

Finally, to test for pre-trends in our IV design, we make use of the 1929 Handbook of American Trade Unions, which reports the number of locals for each union by state. We then take the national membership of each union and apportion it to states in 1929 based on the share of locals in that state to form a proxy for the number of members of a given union in a given state, and then sum across unions to get a state-level measure of union membership in 1929. Cohen, Malloy, and Nguyen (2016) construct a similar measure and validate it for a number of states.

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Appendix F. Distributional Decomposition Appendix

Re-weighting Let households' selection into unions be given by $u(X, \epsilon)$ in reality and $u^C(X, \epsilon)$ under some counterfactual, C. The true income distribution, F_Y, is observed, but the counterfactual, F_Y^C, must be estimated. Using Bayes rule, we find that

$$\begin{aligned} F_Y^C &= \int\int F_{Y|X,u} dF_{u^C|X} dF_X \\ &= \int\int F_{Y|X,u} dF_{u|X} \Psi(u, X) dF_X \\ &= \int\int F_{Y|X,u} \Psi(u, X) dF_{u,X}, \end{aligned} \tag{10}$$

where $\Psi(u, X)$ is reweighting factor given by

$$\Psi(u, X) \equiv u * \frac{\Pr(u^C = 1|X)}{\Pr(u = 1|X)} + (1 - u)\frac{\Pr(u^C = 0|X)}{\Pr(u = 0|X)}. \tag{11}$$

Equation 10 illustrates how the counterfactual income distribution relates to the observed income distribution, allowing us to simulate the former by reweighting on observables in the latter. As Equation 11 shows, the nature of this reweighting depends not only on $\Pr(u = 1|X)$, which we estimate using predicted values from logistic regressions of observed union status, but also on $\Pr(u^C = 1|X)$, which depends on the counterfactual in question. In our case we will consider setting a within-year counterfactual where $\hat{\Pr}(u^C = 1|X) = 0$, effectively deunionizing the income distribution by reweighting union members to have the same income distribution as the non-union members with the same X. We will also consider an over-time counterfactual where $\hat{\Pr}(u^C = 1|X) = \hat{\Pr}(u^{t_B} = 1|X)$, where u^B indicates union membership in a base year t_B.

Decomposing the Total Union Effect Unions can contribute to changes in inequality through two channels: first, changes in union membership over time; and second, changes to the union-non-union wage structure. For each time period, we further decompose the total union component into these respective "unionization" and "union wage" effects by considering an alternative counterfactual. For each time period t_B to t, we reweight year-t

households to unionize as they would in year t_B:

$$\Pr(u^{C_B} = 1|X, t) \equiv \Pr(u = 1|X, t_B). \tag{12}$$

Applying this counterfactual to Equation 11 allows us to generate weights by predicting year-t households' union status with year-t_B estimates of union-selection (we use logistic regression). Applying these weights to year-t households allows us to separate Equation 4 into its respective subcomponents:

$$\begin{aligned}\Delta^U = &\underbrace{\left[\text{Gini}(F_{Y_t}) - \text{Gini}(F_{Y_t}^{C_B})\right]}_{\text{Unionization Effect}} \\ &+ \underbrace{\left(\left[\text{Gini}\left(F_{Y_t}^{C_B}\right) - \text{Gini}\left(F_{Y_t}^{C_0}\right)\right] - \left[\text{Gini}(F_{Y_{t_B}}) - \text{Gini}\left(F_{Y_{t_B}}^{C_0}\right)\right]\right)}_{\text{Union Wage Effect}},\end{aligned} \tag{13}$$

Ideally, we could compare the results of our decomposition to a similar exercise conducted using 1951 Palmer survey data by Callaway and Collins (2018), but they report all of their effects in percentile ratios. We are limited by only having binned income data in the years closest to 1951, so our percentile ratios are unstable. Therefore, we elect to use the Gini coefficient instead. Nonetheless, our results are qualitatively consistent with theirs: union members are negatively selected, and the union premium is larger for otherwise lower-wage workers. We can infer from these results that unions exercised a considerable compressing effect. In Callaway and Collins (2018), the reduction in inequality amounts to 16-24 percent across percentile ratios in their 1951 urban wage-earners sample, while we observe a 5 and 7.6 percent decrease in the Gini coefficient in household income in 1947 and 1960, respectively.

Incorporating spillover effects One limitation of the standard DFL reweighting procedure is that it uses observed non-union wages to simulate de-unionization, assuming that changes in unionization have no spillover effects. To relax this assumption, we adopt the distributional-regression strategy developed by Fortin, Lemieux, and Lloyd (2018). Specifically, we model the year-t likelihood of household income falling between quantiles k and $k + 1$ for each of twenty-five income quantiles:

$$p_k\left(X_{it}, U_{sjt}, y_k\right) \equiv \Pr\left(y_k \leq Y_{it} < y_{k+1} |\, X_{it}, U_{sjt}\right) \text{ for k = 1,...,K}, \tag{14}$$

where Y_{it} denotes realized household income, y_k denotes income at the kth quantile, X_{it} denotes household demographics (including union status), and U_{sjt} denotes the share of unionized workers in state s and industry j at year t.[64] $p_k(\cdot)$ is estimated separately for union and non-union households using a heteroskedastic-robust ordered probit model:

$$\Pr\left(Y_{it} \geq y_k | X_{it}, U_{sjt}\right) = \Phi\left(X_{it}\beta + y_k X_{it}\lambda + \sum_{m=0}^{4}\left[y_k^m U_{sjt}\phi_m\right] - c_k\right). \tag{15}$$

We then construct a spillover reweighting factor, ξ_{ik}, which captures the change in the likelihood of falling into income bin k one would experience if their state(-industry) union share were at some counterfactual level U_{sjt}^C:

$$\xi_{ik} = \frac{\hat{\Pr}\left(y_k \leq Y_{it} < y_{k+1} | \, X_{it}, U_{sjt}^C\right)}{\hat{\Pr}\left(y_k \leq Y_{it} < y_{k+1} | \, X_{it}, U_{sjt}\right)} = \frac{\hat{p}_k\left(X_{it}, U_{sjt}^C, y_k\right)}{\hat{p}_k\left(X_{it}, U_{sjt}, y_k\right)} \tag{16}$$

We then generate predicted probabilities for each household in year-t using true and counterfactual union densities in their state or state-industry. For the "within-year" impact of spillovers shown in the dashed lines of Figures F.1b, F.1c, and F.1d, these counterfactual union shares are simply set zero, $U_{sjt}^{C_0} = 0$. For the spillover-adjusted unionization components of the decompositions reported in Table F.2, we generate predictions using state- or state-industry-level unionization rates from the *base* year, $U_{sjt}^{C_0} = U_{sjt_B}$.[65] Finally, we adjust the counterfactual income distributions from Section 5.1 by simply multiplying a given household's union-selection weighting factor, Ψ_i, by the spillover weight $\hat{\xi}_{ik}$ corresponding to the income bin k_i in which it falls. The result is an income distribution that looks as though individuals unionized as they did in year t_B *and* received the spillover benefits of year-t_B unions.

Appendix Table F.2 shows the results of the decomposition, with and without spillovers. The effects of unions are again large for the 1936-1968 period, but are small for the recent period. This result, as well as the relatively small effect of unions on household income inequality in the recent period is in contrast with DiNardo, Fortin, and Lemieux (1996) and Fortin, Lemieux, and Lloyd (2018) who find both larger effects of unionization and larger effects of spillovers in the recent period. As Appendix Table F.3 shows, the difference is pri-

[64] Because we lack panel data on households' industries prior to 1977, we use state union shares rather than state-industry union shares in earlier years. Similarly, incomplete state coverage and absence of year variation prevents us from estimating any spillover effects prior to the 1960s.

[65] For year-t households in states or state-industry pairs not represented in the base year, we predict their counterfactual union shares using predictions from a regression of union shares against a quadratic time trend and state-specific linear time trends. When year-t includes industry information, we include industry-specific time trends in the regression and interpolate early state-industry shares using industry-level density estimates from Troy (1965) reweighted by employment shares from IPUMS, following Collins and Niemesh (2019).

marily due to the inequality concept and population being used, rather than the differences in the selection equation. We use household income inequality, while DiNardo, Fortin, and Lemieux (1996) use individual earnings inequality, and often focus on men. Changing household composition, female labor force participation, and wealth inequality are just some of the forces affecting household income inquality that would be missed in simply looking at individual male earnings. The divergence between household and individual inequality changes is smaller in the early part of our sample than the latter part: the top 10% measured by individual income in Piketty, Saez, and Zucman (2018) between 1936 and 1968 is 13.2, while it is 12.4 when measured in tax units (which are closer to our notion of households), while the change in top 10% between 1968 and 2014 is 8.6 when measured at the individual level while it is 12.4 when measured at the tax unit level.

Appendix Table F.1: Yearly Union Impact and Union Density: $\theta_{Gini} \equiv$ Gini - CF Gini

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	θ_{Gini}	θ_{Gini}	θ_{Gini}	θ_{Gini}	Gini	Gini	Gini
Union Density	-0.0622***	-0.0422***	-0.0550***	-0.0481**	-0.304***	-0.0790***	-0.0716***
	(0.00798)	(0.00860)	(0.0150)	(0.0145)	(0.0678)	(0.0127)	(0.0125)
College Share				0.0224			0.0254
				(0.0170)			(0.0144)
CF Gini						0.903***	0.902***
						(0.0253)	(0.0273)
Linear Time Trend?	No	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic Time Trend?	No	No	Yes	Yes	Yes	Yes	Yes
R-squared	0.697	0.739	0.746	0.754	0.948	0.998	0.998
N	57	57	57	57	57	57	57

Note: This table reports OLS estimates of the marginal response of the Gini coefficient to historical changes in union density, adjusting for observable changes in the population via the counterfactual-weighting procedure described in Section 5.1. Columns 1-2 report coefficients from an OLS regression of yearly union impact, $\nu\left(F_{Yt}\right) - \nu\left(\hat{F}_{Y_{nt}}\right)$, against the yearly unionization rate. Columns 4 and 5 report coefficients from alternative specifications, which put $\nu\left(F^{C}_{Y^n_t}\right)$ on the right-hand side. Robust standard errors are shown in parentheses. $^{*}p = 0.1,^{**}p = 0.05,^{***}p = 0.01$.

Appendix Table F.2: The Impact of Unionization with and without Spillovers

	Time Period	*Total Change in Statistic*	*Unionization Component* no spillovers	*Unionization Component* w/spillovers
	(1)	(2)	(3)	(4)
Panel A: Gini	1936 to 1968	-0.0526	-0.0158	-0.0197
			(30.06)	(37.49)
	1968 to 2014	0.144	0.00603	0.00775
			(4.188)	(5.376)
Panel B: 90/10	1936 to 1968	-0.188	-0.0986	-0.145
			(52.47)	(77.40)
	1968 to 2014	0.817	0.0366	0.0362
			(4.474)	(4.431)
Panel C: 90/50	1936 to 1968	-0.102	-0.0443	-0.0566
			(43.21)	(55.30)
	1968 to 2014	0.360	0.0207	0.0209
			(5.760)	(5.819)
Panel D: 10/50	1936 to 1968	-0.0855	-0.0544	-0.0888
			(63.57)	(103.9)
	1968 to 2014	0.458	0.0159	0.0153
			(3.464)	(3.340)

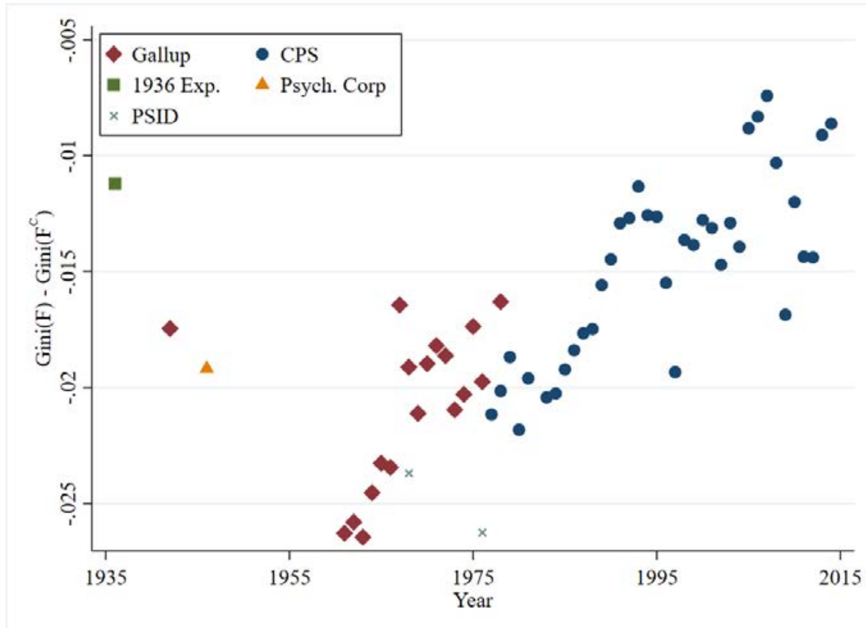
Note: This table reports the union-related components of decompositions of changes in Gini coefficient over time with and without spillovers estimated as described in Appendix F. Each row represents a separate decomposition. Column 1 specifies the beginning and end years of the decomposition. Column 2 reports the total change in computed Gini coefficient. Column 3 reports the change in Gini attributable to changes in union versus non-union incomes. Column 4 reports the change in Gini attributable to changes in the conditional unionization rate. Column 5 reports the total effect of both union wage changes and unionization (Column 3 + Column 4). Numbers in parentheses report components as a percentage of total change in Gini coefficient.

Appendix Table F.3: Decomposition of Change in Gini (CPS) from Individual to Household Measure

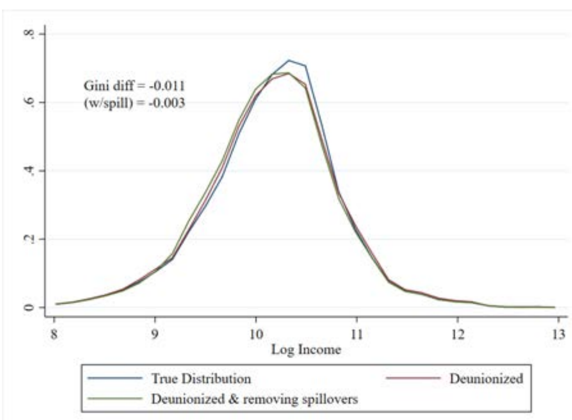
	Time Period	*Total Change in Statistic*	*Change Attributable to:* Δ Union Wages	Δ Unionization	Total Union Component
	(1)	(2)	(3)	(4)	(5)
Individual Union Status and Earnings, Men Only	1979 to 2014	0.0893	0.00536	0.00979	0.0151
			(6.004)	(10.96)	(16.96)
	1981 to 1988	0.0194	-0.00121	0.00619	0.00498
			(-6.241)	(31.84)	(25.60)
	1990 to 2014	0.0268	0.00311	0.00467	0.00778
			(11.60)	(17.41)	(29.01)
Individual Union Status and Earnings, Men and Women	1979 to 2014	0.0590	0.00536	0.00533	0.0107
			(9.091)	(9.032)	(18.12)
	1981 to 1988	0.00890	0.000462	0.00359	0.00405
			(5.191)	(40.37)	(45.56)
	1990 to 2014	0.0209	0.00361	0.00313	0.00673
			(17.23)	(14.95)	(32.18)
HH Union Status and Individual Earnings, Men and Women	1979 to 2014	0.0590	0.00141	0.00482	0.00623
			(2.391)	(8.174)	(10.57)
	1981 to 1988	0.00890	-0.000507	0.00264	0.00213
			(-5.698)	(29.67)	(23.97)
	1990 to 2014	0.0209	0.00410	0.00302	0.00711
			(19.58)	(14.41)	(33.99)
Household Union Status and Income	1979 to 2014	0.104	0.00189	0.00883	0.0107
			(1.813)	(8.459)	(10.27)
	1981 to 1988	0.0480	-0.00281	0.00474	0.00193
			(-5.844)	(9.874)	(4.029)
	1990 to 2014	0.0730	0.00372	0.00264	0.00636
			(5.090)	(3.612)	(8.703)

Note: This table reports the contribution of unions to inequality in different CPS samples, showing how the population, income, and union measure affect the decomposition. The top row shows the results for just individual male workers, with unionization and earnings measured at the individual level. Row 2 adds women. Row 3 changes the definition of union to be the household measure we use in the main text, but keeps earnings measured at the individual level. Row 4 then changes the measure to be household income, and changes the population to be households rather than individuals.

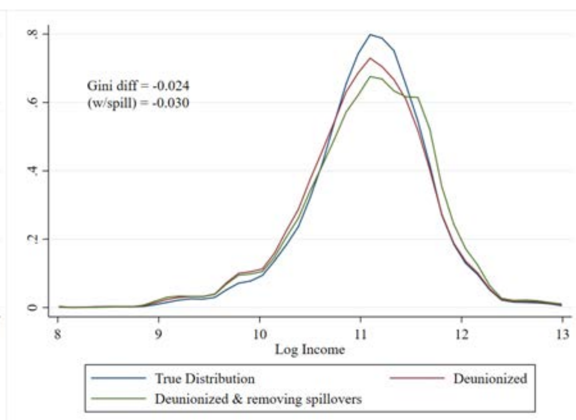
Appendix Figure F.1: Income Distributions: True vs. No-Unions Counterfactual



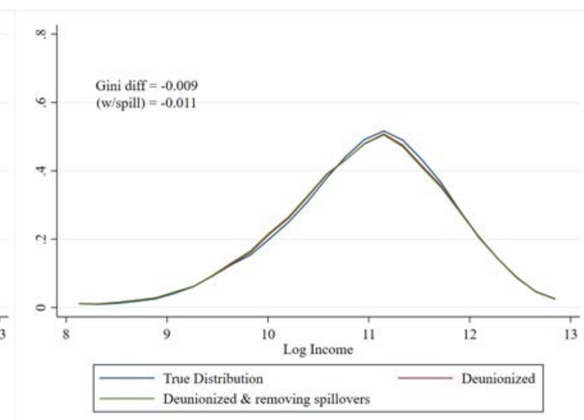
(a) Yearly Union Impact (assuming no spillovers)



(b) 1936



(c) 1968 (PSID)



(d) 2014 (CPS)

Note: This figure compares the observed population (F_Y) and the counterfactual population without unions (F_{Y_n}) in selected years. Panel F.1a plots yearly differences in true and counterfactual Gini coefficients. Panels F.1b, F.1c, and F.1d plot kernel-density estimates of true and counterfactual log-income distributions for selected years. Spillovers are estimated using state-year-industry level union density in the CPS and state-year union density in the other samples, imputed where necessary. Income is denominated in 2014 dollars using CPI.

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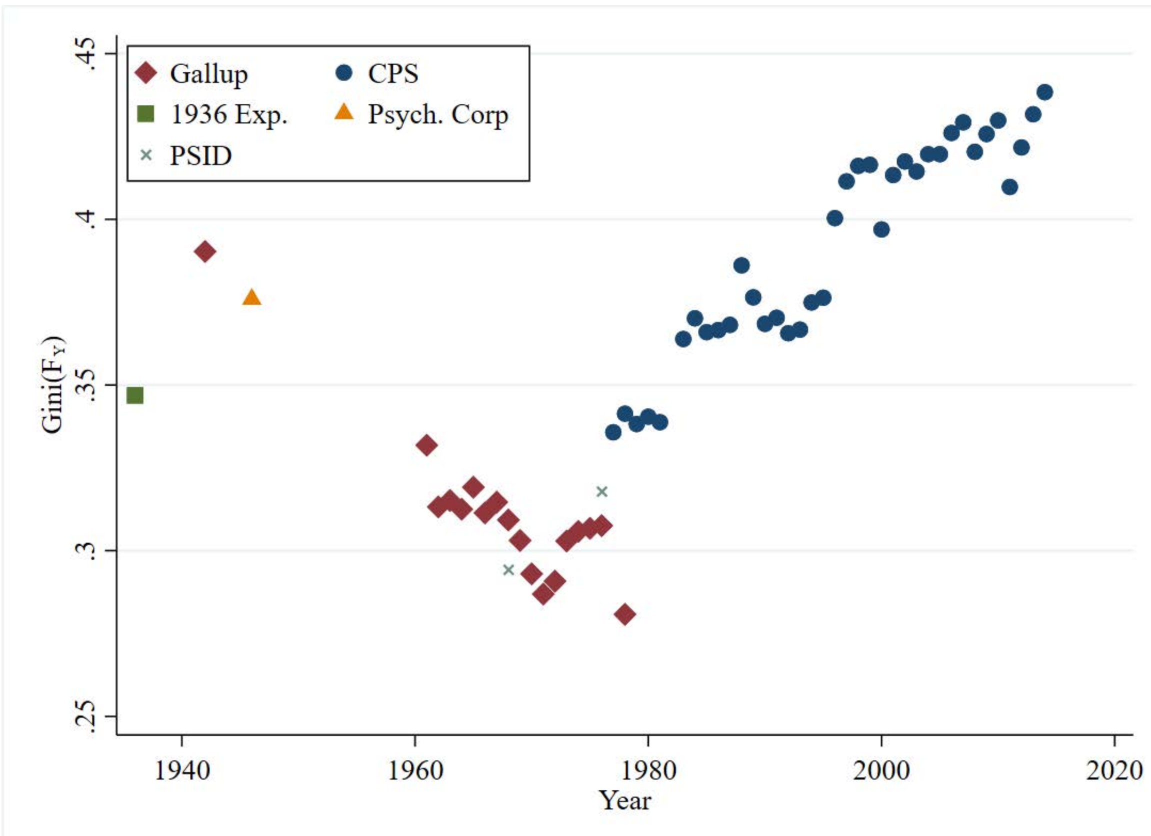
Appendix Figure F.2: Gini Coefficient In Survey Data Over Time



Note: This figure reports the Gini coefficient in each year, computed using Gallup, ANES, and CPS data.

Appendix G. Detailed IV analysis

As we demonstrate in Section 5 of the main text, there is a robust, negative relationship between union density and a variety of inequality measures, both at the aggregate time-series level (Section 5.2) and at the state-year level (Section 5.3). In this Appendix, we provide a more detailed treatment of the IV analysis summarized in Section 5.4 of the main paper. We focus on two key policy shocks that take place in the 1930s and 1940s, both of which had large but differential effects across states, allowing for identification of the effects of changes in state-level union density on changes in state-level measures of inequality. We begin by presenting historical details on the relevance of the two policy changes and qualitative evidence on the plausibility of the exclusion restriction. We then present our first-stage and 2SLS estimates, followed by a variety of econometric checks on our identification assumptions.

G.1. Two policy shocks that increased union density

We make use of two historical policies that together spurred a substantial increase in union density over a short, roughly ten-year period of time. First, we use the legalization of union organizing itself via the 1935 National Labor Relations Act (the NLRA, or "Wagner Act") and the 5-4 Supreme Court decision that upheld its constitutionality in 1937. As we will show, these events are associated with a modest increase in strike activity but a much larger increase in the probability of a strike's success, as well as a large increase in union members via the Act's establishment of a union recognition process via the National Labor Relations Board (NLRB). *We construct our "Wagner Act" shock as follows: new union members, by state, added from 1935 to 1938 via NLRB elections and successful recognition strikes, divided by 1930 state population.*[66]

In the midst of this new legal opportunity for union organizing, Germany invaded Poland in September of 1939, marking the start of World War II in Europe. By the spring of 1940, the war created enormous U.S. government demand for military equipment to aid the Allied cause. Between 1940 and 1945, the federal government mobilized much of the country's

[66]For the NLRB elections data, we thank Ethan Kaplan. The strikes data come from The Labor Fact Book, a publication of *Labor Research Associates (LRA)*, which was a labor journal that operated from the 1930s through the 1960s. *The Labor Fact Books* only record *large* strikes, but unlike BLS strike measures they allow us to tabulate successful union recognition strikes by *state*, obviously crucial to our state-year analysis. Where multiple states are listed we assign them equally, but have also experimented with allocation based on share manufacturing. Note that BLS reports also records much of this information (whether a strike is for union recognition or some other goal, the strike's outcome, the state, etc.), but all in *separate* tables, and thus constructing cross-tabulations by state is not possible.

industrial capacity for war production, spending \$340 billion on national defense (or over three times the nominal GDP in 1940).[67] Because the war coincided with unprecedented union power, important concessions were made to labor in exchange for its cooperation. First, Roosevelt announced in 1940 that only firms that were NLRA-compliant would receive defense contracts from the National Defense Advisory Commission.[68] Second, when the US enters the war after Pearl Harbor as a military combatant, the newly established National War Labor Board (NWLB) imposes automatic enrollment and maintenance-of-membership at any firm receiving war-related production orders: if the firm was unionized, then any new worker would be default-enrolled into the union upon starting a job and would be maintained as a union member. He would only have a 15-day window to dis-enroll, but "few workers took the initiative to withdraw from the union in their first hectic weeks on the job."[69] Third, the NWLB allowed unions to have dues automatically deducted from members' paychecks ("dues checkoff"), eliminating the onerous practice of nagging members in-person for late dues and creating for the first time a steady source of revenue for unions. As we show in Appendix Figure G.2, unions managed to organize other "superstar" firms of the time during the war. Further, the new union members brought in under these policies were disproportionately low education and, as we show in Appendix Figure G.3, Black, who have larger union premia than the average union member.

Given the thumb the government put on the scale in unions' favor in war-related industries, we posit that the more defense contracts a state received during the war, the more union density grew. *We construct our "war spending shock" as follows: total 1940-1945 military spending by state, divided by state population.*[70]

G.2. First-stage relationship between the policy shocks and union density

G.2.1. Results in changes

We take two approaches to documenting the first-stage relationship and other results. First, we examine results in changes within state. We take care to avoid years during the Second

[67]See Brunet (2018).

[68]This convinced even the staunchly anti-union Henry Ford to recognize the United Auto Workers (UAW) in 1941, lest he lose out on these enormous defense contracts. See chapter six of Loomis (2018)

[69]See Lichtenstein (2003), Kindle Location 1415.

[70]We use newly digitized war-era military supply contract data to construct per capita 1940-1945 war spending for each state. This measure is in 1942 dollars. We are very grateful to Gillian Brunet for sharing these data.

World War itself because war-specific institutions (most obviously wage controls, which were not fully lifted until 1946) could have a direct effect on inequality. We are also limited by data availability, particularly that of the endogenous variable, state-year union density, which we only have in 1929 and then from 1937 onward. These constraints lead us to estimate the following first-stage equation:

$$\begin{aligned}
Union_{st} - Union_{s,t-9} = & \beta_1 \, Wagner\ shock_s \times \mathbb{I}_t^{t=1938} + \beta_2 \, War\text{-}spending\ shock_s \times \mathbb{I}_t^{t=1947} \\
& + \gamma_1 \, Wagner\ shock_s + \gamma_2 \, War\text{-}spending\ shock_s \\
& + \lambda_{r(s)t} + \gamma \left(log\Big(\frac{N_{st}^{Col}}{N_{st}^{HS}}\Big) - log\Big(\frac{N_{s,t-9}^{Col}}{N_{s,t-9}^{HS}}\Big) \right) \\
& + \eta \mathbb{X}_{st} + e_{st},
\end{aligned} \tag{17}$$

where the outcome variable is a *nine-year* change in union density in state s, $Wagner\, shock_s$ is the per capita number of new members added via NLRB elections and recognition strikes from 1935-1938 in state s, $\mathbb{I}_t^{t=1938}$ is an indicator variable for year $t = 1938$ (so, an interaction term that turns on for the 1929-1938 interval), $\mathbb{I}_t^{t=1947}$ is an indicator for year $t = 1947$ (so, for the 1938-1947 interval), $\lambda_{r(s)t}$ are Census region-by-year fixed effects, and $\mathbb{X}_{st}$ are other controls that we vary to probe robustness. Using nine-year intervals may seem odd, but it is done intentionally. Our data constraints (i.e. missing state-level union density from 1930-1936) plus our desire to avoid any year with war-related wage controls means that intervals included in this regression are 1929-1938, 1938-1947, 1947-1956, and so on until the end of our sample in 2014. The nine-year intervals allow us to skirt the wage-control period (which ends in 1946) and make use of our only year of pre-Wagner state density data, 1929.

Appendix Table G.1 shows the results of estimating equation (8). Col. (1) is our preferred specification and shows that the two interaction terms substantially shift upward union density in the appropriate window (i.e., the Wagner-Act shock during the 1929-1937 window and the war-spending shock during the 1938-1947 window). Importantly, the main effects of the Wagner and war-spending variables are not significant, meaning that outside of the specific windows captured by the interaction terms, Wagner and war-spending states are not predisposed to union-density growth. The associated F-statistic is also well above the rule-of-thumb cut-off value.

The rest of the table examines robustness. Col. (2) adds state fixed effects. Since the regression is in changes, adding state fixed effects is analogous to adding state-specific trends in an in-levels regression. Col. (3) weights the state-year observations by 1930 state population, and col. (4) drops Michigan (the outlier for both policy shocks), all with minimal effect on the coefficients of interest. Col. (5) adds interactions of each policy shock with the "wrong" window to the col. 1 regression—a demanding specification check given the high correlation between the two variables, as shown earlier in Appendix Figure G.4. While the standard errors on the variables of interest increase somewhat, the point-estimates are quite stable. Moreover, the coefficients on the "wrong" interactions are insignificant: the effect of the Wagner shock is only significant in the earlier window and that of the war shock only significant in the later window.

Nonetheless, it is clear from the changes in the coefficients and the fall in the F-statistic between columns (3) and (5) that the two shocks are highly correlated. Appendix Figure G.4 shows a scatter plot of our two policy shock variables. The figure shows, as expected, that Michigan (the birthplace of the modern U.S. labor movement in the 1930s and the "Arsenal of Democracy" during the war) is an outlier for both of the shocks. More generally, the two shocks have a correlation of 0.7, and so we pool the two shocks into a single state-level shock variable. Using this single instrument and interacting it with the two treatment windows gives similar results, as shown in column (6) of Table G.1.

G.2.2. Results in levels

The second approach we take is more graphical and non-parametric: we simply regress state-year union density (in levels) on the pooled policy shock variable, separately in each year. Instead of using nine-year intervals to avoid the war and specifying in which windows we expect the see effects, we plot the relationship in each year and observe whether the changes emerge in the periods we predict.

In particular, we estimate:

$$Union_{st} = \sum_{y \in 1929,1937...2014} \beta_y IV_s \mathbb{I}_t^{t=y} + \lambda_{r(s)t} + \mathbb{X}_{st}\gamma + e_{st}, \tag{18}$$

where $Union_{st}$ is state-year density, IV_s is the time-invariant pooled policy shock variable for state s, $\mathbb{I}_t^{t=y}$ is an indicator variable for when year t is equal to year y, $\lambda_{r(s)t}$ is a vector of $region \times year$ fixed effects, and $\mathbb{X}_{st}$ is a vector of covariates that we vary to probe robustness, but always includes log skill shares $log\left(\frac{N_{st}^{Col}}{N_{st}^{HS}}\right)$. y is summed over all years for which we have a state-year union density estimate (i.e. 1929 and then 1937 onward). In our baseline estimation, $\mathbb{X}_{st}$ is omitted, and thus equation (18) is equivalent to regressing union density on the pooled IV and region fixed effects separately by each year of the sample period, and then plotting the resulting β_y values.

As the results are in levels, our hypothesis makes predictions about the *changes* in the relationship between union density and the pooled IV variable. We argue that the only time union density should exhibit a sustained change in its relationship with the IV is during the treatment period (1935 to the end of the war), and that the relationship should *increase*. We are agnostic as to the sign of the density-IV correlation before the treatment period, but we expect that it should increase from this level on during the treatment period.

The results from the baseline estimation are shown in the first series of Figure G.5. We only have pre-period data for 1929, but we see a large increase from 1929 to 1937. Unfortunately we cannot show the precise timing due to lack of data. The coefficient in 1929 is close to zero, showing that before the treatment period, states about to be hit by our policy shock variables were not historically union friendly. From 1937 onward we have annual data, and the relationship between the IV and union density increases steadily during the remainder of the treatment period. Afterwards, we see no sustained increase but also no back-sliding, suggesting that the states hit by the policy shock variables retain (relative to other states) greater density levels even after the war ends.

G.3. Are the policy shocks plausibly exogenous?

Appendix Table G.1 and Figure G.5 show that our shocks appear to have a strong, first-stage effect on union density, but of course they do not speak to whether the shocks provide a valid experiment. In arguing that these policy shocks provide quasi-exogenous variation in union density, we never claim that they hit a random subset of states. Indeed, states with larger IV values (i.e., those that gained more union members via strikes and elections in the mid and late 1930s as well as received more dollars per capita of government war contracts) were different in important ways from other states. Table G.2 uses the 1920 Census to examine what state-level characteristics predict the pooled IV variable. By far the strongest predictor is the manufacturing share of employment in the state. Not only is pre-period manufacturing a key predictor of the IV, but the manufacturing sector is key to the first-stage of the IV as well, as we are arguing that the government taking over manufacturing production during World War II was the driving mechanism for why war spending increased union density in a state. For these reasons, we will give special attention to the potential confound of manufacturing in Section G.5.1.

The rest of this section provides historical context for the two policy shocks, which helps establish their validity as sources of identification.

G.3.1. The "Wagner shock"

The historical consensus, both from contemporaneous accounts as well as more modern assessments, argues that the decision of the federal government to no longer intervene on the side of employers—*not* a sudden increase in union demand among workers—led to the historic gains in density immediately after the Wagner Act's passage. Employers had considerable latitude, both legal and extra-legal, in combating unions before Wagner. Firms put down strikes and other organizing activity with an array of raw paramilitary power and espionage, and if needed, military assistance from the state. White, 2016 describes the weapons the major steel companies stockpiled to deter or put down organizing activity: "[T]he major steel companies had evolved potent systems of labor repression that included political and legal resources as well as extensive police forces and stockpiles of armaments....massive arsenal[s] of firearms and gas weapons." Henry Ford not only commanded a "brutal private army", but also paid an espionage force of over 1,000 employees to spy on fellow workers and report back any hints of organizing activity.[71]

A final recourse for firms was the power of the state. Prior to the NLRA, the coercive powers of the American government, at all levels, were regularly used against organized labor, with military deployments and judicial repression commonplace (Naidu and Yuchtman, 2018). Riker (1979) documents that the most frequent domestic use of the national military in the nineteenth century was to put down labor unrest. As late as summer 1934 the national guard was called in to put down major strikes in Toledo and Minneapolis, as well as a general strike of West-coast dockworkers lead by the Teamsters. In all cases the national guard succeeded after pitched street battles.

The Wagner Act legally protected collective actions such as picketing and strikes, bypassed judicial injunctions, and mandated resources for independent enforcement of organizing rights. It was this policy shift, not an increase in union organizing, that led to the sudden gains in the second half of the 1930s. Writing about the 1937 Flint sit-down strike (which led to GM's official recognition of the UAW), Lichtenstein, 1995 notes that: "The UAW victory was possible not so much because of the vast outpouring of union sentiment among autoworkers, but because General Motors was temporarily denied recourse to the police power of the state." Taking a more modern perspective, Loomis (2018) agrees: "[T]he government played a critical role in determining Flint's outcome. Ten years earlier, with the stridently anti-union Calvin Coolidge as president, the outcome would likely have turned out very different, no matter what the Flint strikers did."

We provide two pieces of evidence on strikes in support of historians' contention that organizing successes immediately after Wagner's passage did not stem from an increase in grass-roots organizing activity, but rather a top-down change in the rules government used to referee management-labor relations. We treat strikes as a proxy for labor activism and mobilization. First, zooming in on the period immediately before and after the Wagner

[71] The "private army" quote is from Loomis (2018, p. 122), and Lichtenstein (1995) discusses anti-union espionage at Ford.

Act passes, we show in Appendix Figure G.6 that strike activity increases only modestly upon passage of the Wagner Act. We also show in Appendix Figure G.5 that strike activity increases only modestly upon passage of the Wagner Act. Although leaders in the CIO urged their colleagues to "seize the once-in-a-lifetime organizing opportunities so evident in the mid-1930s" (Lichtenstein, 2003)[72], strike activity only rises by twenty percent. Nor do their goals change remarkably, as there is only a modest uptick (15 percent) in the share of strikes for which union recognition is a key goal.

The most dramatic change is the share of strikes that are *successful*, which increases from just over twenty percent to forty percent. This time-series evidence supports the conclusion of White (2016) that "poverty and resentments alone did not undermine the open shop. The surge of unionization was influenced by the arrival from above of a new political economy premised on greater regulation of industrial production by the federal government."

Appendix Figure G.6 only speaks to national time-series evidence; it is possible that organizing activity shifted toward union-friendly states after Wagner, in violation of our identification story. By constrast, Appendix Figure G.7 shows that the relationship between our Wagner variable and state strike activity is roughly constant since 1914 (the first year of state-level strikes data). Essentially, the same states were striking before and after the Wagner Act, but only met with success after its passage.

This steady relationship supports the reading that the geographic variation in post-NLRA density gains can be modeled as arising from (a) *constant* differences in latent union demand at the state level interacted with (b) a national policy shock in 1935 that allowed that demand to translate into density gains. Latent union demand likely comes from industrial structure (such as high fixed-cost capital investments and product market power enabling workers to capture rents) or cultural and ideological differences across states. Political scientists and sociologists (Davis, 1999; Eidlin, 2018; Goldfield, 1989) who study the period emphasize the role of persistent communities and networks of highly ideological labor activists pushing for strikes and other forms of collective action even when success was impossible. Appendix Figure G.7 supports these arguments. If, as we claim, the geographic variation in post-Wagner gains in density are explained by the interaction between long-standing differences in demand for unions in certain localities and a shift in the federal government's position on the legality of organizing, then it should be possible to construct an alternative IV using *earlier* episodes of union demand interacted with the treatment period. We perform this exercise in Section G.5.2.

[72] As further evidence that the modest increase in organizing was likely endogenous to the NLRA, the CIO, with its unprecedented focus on organizing industrial workers, was only formed as a committee within the AFL six months after the NLRA's passage.

G.3.2. The "war shock"

While we will perform extensive robustness tests later in this Appendix, here we provide evidence from existing work that per capita war spending is plausibly exogenous to other factors that could shape inequality.

Brunet (2018), whose war-spending data we in fact use to construct our war-shock measure, shows that war spending had only a modest state-level fiscal multiplier (0.25 to 0.3).[73] She conducts a battery of tests showing that war spending was independent of a variety of other state-level changes during World War II. For example, she shows that war spending was not correlated with increases in government employment, nor was it targeted to places with more available labor (e.g., those states with lower pre-war employment levels). These results foreshadow the success of our robustness checks in Section G.5, in that flexibly controlling for a variety of state-level characteristics typically has little effect on our main results.

Furthermore, the war contracts did not radically change the geography of American industry; contracts favored existing manufacturing firms and their subcontractors. As we will show in Section G.5.1, any differential increase in manufacturing employment correlated with the IV was extremely short-lived (disappearing by 1946), and states that received more war contracts do not subsequently show faster growth in manufacturing employment after the war ends. Much of war production involved *conversion* of existing factories, and as such not substantially the expanding overall manufacturing share of employment. Yet, even in states that built new factories to accommodate the demands of war production, such as those in the South, manufacturing employment rapidly returned to baseline and did not gain a solid foothold until decades later (Jaworski, 2017).

Finally, Rhode, Snyder Jr, and Strumpf (2017) show that during the war, defense contracts were free of the usual political considerations. They find that the electoral importance of a state did not predict the volume of its war contracts, perhaps because contracts were drawn up directly by military, not Congressional or White House, agencies.[74]

[73]This result echoes Fishback and Cullen (2013), who find that war spending at the county-level led to some modest population growth, but limited if any sustained per capita economic growth.

[74]In his memoirs, Donald Nelson, the chairman of the War Production Board, frequently emphasizes the importance of ensuring that production orders came directly from the military and were free of interference from civilian authorities. See Nelson (1946).

G.4. Main IV results

G.4.1. Results in changes

We begin with the two-stage-least-squares (2SLS) analogue of our first-stage results in Table G.1, with $Wagner\ shock_s \times \mathbb{I}_t^{t=1938}$ and $War\ shock_s \times \mathbb{I}_t^{t=1947}$ as the two excluded instruments.

The first six columns of Appendix Table G.3 show results when the top-ten income share is the outcome, following the same specifications as in Appendix Table G.1. Our preferred estimate in col. (1) suggests that a ten-percentage-point increase in state union density decreases the top-ten share by roughly 6.2 percentage points, with the point-estimates from other specifications ranging from 3.6 to 8.1 percentage points.

The remaining six columns of Appendix Table G.3 show analogous results using the state labor-share as the outcome, with our preferred estimate indicating a 3.6 percentage point increase from a ten percentage-point increase in density. The remaining specifications cluster quite tightly around this baseline result.

For completeness, Appendix Table G.4 shows the corresponding reduced form specifications. Reassuringly, both instruments have independently significant effects on both labor share and top ten share in most specifications that include the two together, with the war shock having a larger reduced form effect than the Wagner Act shock.

G.4.2. Results in levels

As we did with the first-stage results, we also show annual results in levels. Again, predictions in this setting map to *changes* in the relationship between the pooled IV variable and the inequality outcomes. The only time when the relationship between the IV and our inequality outcomes should change is during the treatment period. One advantage of this approach over the 2SLS regressions is that we do not need to observe union density to plot the reduced-form relationship between our inequality outcomes and the pooled IV variable. We can thus look further back in time in the reduced form than we can in the first-stage.

The first series of Appendix Figure G.8 shows the relationship between the pooled IV and the top-ten income share from 1917 onward, using the same specification as we showed for the first-stage relationship in Appendix Figure G.5. The figure shows that in the pre-period, the pooled IV is associated with a *higher* share of income going to the richest ten percent, meaning states that would soon be hit by our pro-union policy shocks were not historically more egalitarian (in fact, the opposite), at least by this measure. While noisy, this positive pre-period relationship can generally be distinguished from zero each year and is largely unchanged until the mid- to late-1930s. It then begins a dramatic and sustained decline. By the start of the war in Europe, the sign of the relationship has flipped. The relationship slowly recovers some of its magnitude over the rest of the sample period, but the changes cannot be distinguished from zero in any of these years. The shape of the relationship between the pooled IV and the top-ten share echoes the results from Appendix Table G.3: the only period of sustained decrease in the relationship between top-ten inequality and the IV is during the treatment period.

The first series of Appendix Figure G.9 is the labor-share analogue of this analysis. It tells a very similar story, though data limitations shorten the pre-period relative to that of state top-ten inequality. In the early 1930s, our IV predicts a *lower* state-level labor share, again highlighting that states that would soon receive pro-union policy shocks were not historically worker-friendly. Over the treatment period, the sign of this relationship flips and then remains positive over the rest of the sample period. Again, the only period of sustained increase in the relationship between the state-year top ten and labor shares to the IV is during the treatment period.

G.5. Robustness checks

In this section, we rule out a number of potential violations of our exclusion restriction, which says that any other determinants of inequality are independent of the change in union density induced by our policy variables. Potential confounding variables include the change in manufacturing employment, omitted determinants of new unionization following the Wagner act, other policies such as taxes and minimum wages, and finally, any independent role of egalitarian norms or beliefs. In the subsections below we present evidence ruling out these alternative mechanisms.

G.5.1. Controlling for contemporaneous and pre-period difference in manufacturing

We start with the role of manufacturing, which we view as the most important potential confound. As we showed in Appendix Table G.2, states that have a larger manufacturing share of employment in the pre-treatment period have larger values for our IV variable, so we have reason for concern.

The first three columns of Appendix Table G.5 show how our top-ten 2SLS results vary as we add manufacturing controls. The first column of this table reproduces the baseline result, col. 1 of Appendix Table G.3, for ease of comparison. In col. 2, including contemporaneous state manufacturing share of employment and its interaction with the two treatment windows reduces the first-stage F statistic somewhat and increases the coefficient on union density from 0.62 to 0.7. In col. 3, controlling for 1920-era manufacturing share of employment also reduces the first-stage F (to just below ten), with little effect on the second-stage point-estimate. Interestingly, while adding these controls for manufacturing employment weakens the first stage given its high correlation with the policy shock variables, contemporaneous or historical manufacturing employment does *not* appear to be an alternative mechanism for reducing top-ten-share inequality during our treatment periods. The coefficients on the interactions of both manufacturing variables with the two treatment windows are positive (significantly so for the first window), suggesting manufacturing-heavy states (all else, including the policy shock variables, equal) predicts *higher* inequality during our treatment period.

The first three columns of Appendix Table G.6 perform the parallel analysis when labor-share is the outcome. As expected, the effects on the first-stage are identical, though in the case of labor share the second-stage point-estimates are more stable, and the manufacturing controls and interactions have coefficients close to zero.

We perform similar robustness tests in Appendix Figures G.5, G.8 and G.9. These test demonstrate robustness of our estimated relationship between the pooled IV and union density, top-ten share, and labor share, respectively, to including the same controls for manufacturing employment. Echoing the results in the tables, the first stage is somewhat noisier, but follows the same general shape. While the reduced-form relationships between the inequality outcomes and the pooled IV sometimes shift *in levels*, the large changes that occur during our treatment period remain. We also control for pre-treatment agricultural share of employment, as it is a potential confound noted in Brunet (2018), with little effect on the estimates.

A final concern related to manufacturing is that the massive shift to producing the tanks, planes, and artillery needed for the war effort may have permanently transformed some states' manufacturing sectors, making it impossible to partial out any effect of the coincident rise of unions. Appendix Figure G.10 puts the manufacturing share of employment on the *left-hand side* of the analysis, exploring whether the shocks embedded in our pooled IV variable are associated with *permanent* changes in a state's manufacturing share of employment. While a positive blip can be observed for the few years of direct American combat involvement, the effect of the IV on state's manufacturing share completely disappears by 1946, whereas the effects on union density and inequality remain sticky. In fact, from 1910 to 1955 there is no sustained change in the relationship between a state's manufacturing employment and our IV variable: states with greater values for the IV are clearly more reliant on manufacturing employment, but the relationship is steady for over forty years. Beginning in the late 1950s, which is well after our treatment period, the relationship begins a slow and steady decline.

To summarize, our key findings are robust to controlling flexibly for contemporaneous manufacturing employment, as well as allowing pre-period differences in manufacturing employment to have a different effect in each year. These checks are important because of the strong positive relationship between the IV and state-level manufacturing employment. Moreover, the policy shocks we use as identification appear to have no lasting effect on states' manufacturing employment, consistent with the papers cited in Section G.3.2. States with large values for the IV are more manufacturing intensive before, during, and after our treatment period. It thus appears that manufacturing employment neither confounds nor mediates the relationship between the IV and union density or that between the IV and our inequality measures.

G.5.2. Using pre-treatment-period strikes as an alternative instrument

We view the Wagner Shock (i.e., the number of union members gained in a state from 1935 to 1038 via recognition strikes and NLRB elections) as the second most serious threat to the IV analysis, considering that/given that it may be driven by local factors (e.g., friendly state governments, unobserved increases in local labor demand, or other local economic conditions) that might have their own independent effect on inequality. We do not observe coincident changes in the relationship between the pooled IV and Democrats in the governor's mansion. Appendix Figure G.11 shows that in fact there is no systematic relationships between the two variables over the course of our long sample period—it is possible that even within party, IV states during the treatment period enjoyed more worker-friendly political environments (or other local factors conducive to union organizing) in a manner difficult to observe.

We thus turn to a more comprehensive check on this possible endogeneity concern. As we showed in section G.3 and Appendix Figure G.7, states that gained the most union members immediately after the Wagner Act passed had long harbored the greatest latent demand for unions (at least as proxied by strike activity). Yet until the mid 1930s, this demand did not translate to greater density because the government consistently sided with management, with no formal protection of the right to organize.

Based on this logic, we substitute the Wagner shock in our IV with a measure of *pre-period* demand for unions: the (per capita) number of strikes in a state from 1921-1928, the years immediately before our first year of union density data in 1929. Whatever economic or political factors that might have contaminated the Wagner Act variable as an IV are unlikely to exist in this earlier period. While FDR was neutral if not friendly toward unions, Warren G. Harding's inauguration in 1921 ushered in an intense anti-union period at the federal level. Conversely, we might worry that union-friendly Democratic governors such as Michigan's Frank Murphy or Pennsylvania's George Howard Earle III played a role in the organization of industrial giants GM and U.S. Steel in the late 1930s, these states were controlled by Republicans in the 1920s. Finally, whatever local economic conditions prevailed in these states in the mid and late 1930s (specifically, the end of the Great Depression and the start of the Roosevelt Recession) are unlikely to reflect conditions during this pre-crash Roaring Twenties period. In summary, this measure reflects state-level demand for unions among workers (which we argue is long-standing and slow-moving), but is purged of any local effects specific to the mid- and late-1930s that may affect our outcomes of interest.

In Appendix Table G.7, we replicate the first-stage and 2SLS results using this measure of latent union demand instead of the Wagner shock. The war-spending shock remains unchanged. While the first-stage is less precise, the point-estimates are comparable to those in Appendix Table G.1, and the resulting 2SLS point estimates are also similar to their baseline estimates in Appendix Tables G.3.

G.5.3. Other robustness checks

The remaining rows of Appendix Tables G.5 and G.6 focus on robustness to other policies that might reduce inequality. Of course, these could be "bad controls" in that, say, greater union density might lead to states to increase the minimum wage or pass other worker-friendly policies. Nonetheless, robustness to these controls would help show the centrality of union density in moving our inequality measures during our treatment period. Furthermore, the 1930s and 1940s is a moment of historically active policy-making at the federal and state level, so it is important to show robustness to controlling flexibly for these policies.

Col. (4) of both tables adds as a control the share of tax units filing a federal income tax return in each state-year (and, as always, its interaction with the two treatment windows), as this share increases substantially during the war years and as such could have its own effect on the income distribution (a large public-finance literature shows that even pre-tax measures of inequality can be shaped by taxes). As we have alluded to already, local politics could be a confound, and col. (5) thus controls in the same manner for whether the state has a Democratic governor. The next two columns focus on state-level economic policy, in particular the minimum wage (which states can raise above the federal minimum) and a state-year "policy liberalism index" developed by Caughey and Warshaw (2016).

The next two columns refer to the local effects of major federal interventions. While our IV makes use of America's industrial support of the Allies, from December 1941 onward, the U.S. was also an active *military* partner, and the loss of so many working-age men to the armed forced may have had effects on labor markets during our key period. We thus control for mobilization rates by state from 1942 to 1945, and as usual its interactions with the treatment periods. In column (10), we control for per capita New Deal spending in each state in the same manner.

The final two columns adds additional state-year level covariates. Column (11) adds state top marginal tax rates on income, as described above, and Column (12) allows the state-year level measure of skill shares to have a separate effect in each treatment period, rather than a constant effect as in our main specification.

None of these controls meaningfully change the 2SLS coefficient for the labor-share outcome. The one outcome sensitive to these controls for the top-ten outcome is the IRS share, which is not surprising as the top-ten and the IRS share are drawn from the same data source and thus some mechanical correlation is likely present. Even so, it remains negative and significant. Moreover, none of these additional robustness checks reduce the first-stage F statistic below ten.

G.5.4. Did World War II create egalitarian norms?

Finally, we consider a widely held view that the massive economic and military mobilization during World War II created lasting, egalitarian social norms that helped keep inequality in check for several decades.[75] If such sentiment came in part from actual war-related production, then it is a factor both correlated with our policy shock and related to inequality and thus threatens our identification.

We respond to this claim in three ways. First, we look at Gallup questions asking people how the war changed their views, in an attempt to see if aggregate changes in sentiment support the "egalitarian social norms" hypothesis. Our results are surprising (at least to us). We find no evidence that the war created the pro-labor or pro-worker sentiment that we would expect if egalitarian norms were an important constraint on inequality in the immediate post-war period. For example, in 1945, 56 percent of Gallup respondents tell pollsters that their view of labor unions is *worse* than before the war, while only 19 percent say the same of business owners and managers.[76]

The Gallup question that is most directly related to how the war shaped respondents' views about fairness, deservingness, and income is from a June 1945 survey asking respondents both who they think has done the best financially during the war and who *should* be doing better?[77] There is an overwhelming consensus that workers have made out well, as 62

[75]Goldin and Margo (1992), Piketty and Saez (2003), and Goldin and Katz (2008) are among highly-cited works in economics that speculate as to the war creating egalitarian social norms.

[76]In a March 1945 poll, Gallup asked: "Is your attitude toward labor unions today more or less favorable than it was before the war?" to which 56 percent answered "less favorable," 24 percent "the same," and 20 percent "more favorable." Gallup asked in the same survey the analogous question, with "owners and managers of business concerns" in place of "labor unions." In response to this question, only 19 percent answered "less favorable," 49 percent "the same" and 32 percent "more favorable."

[77]These are questions 10a and 10b from the June 1-5, 1945 survey. The wording of question 10a

percent choose workers as the group that has done best, compared to only 19 percent that chooses white-color professionals and managers/owners of businesses. Moreover, 38 percent of Gallup subjects say that these well-off occupation groups *should* have done better during the war, compared to only nine percent saying the same about workers.

While these aggregate sentiments cast some *a priori* doubt on the egalitarian-social-norms hypothesis, our second response to the argument is to check if respondents in states hit with the two policy shocks are more likely to say that the war changed their views in a worker-friendly manner. In Table G.8 we regress a dummy variable coded as one if the respondent said they think workers and the poor should be doing better than they are against the pooled IV (col. 1), only the Wagner shock (col. 2), only the war-spending shock (col. 3), and both variables entered in the same regression (col. 4). In all cases, the coefficients of interest are very close to zero and insignificant. The remaining four columns perform the same exercise, but for the respondent saying that business owners/managers and professionals should be doing better. We again find small coefficients, with the only marginally significant results suggesting that respondents in Wagner-shock states are *more* sympathetic to business and professional interests.

Our third response considers a related "norms" argument: even if the war did not change Americans' stated views on what constitutes a fair income distribution, war-time wage structures altered worker reference points, and this process constrained post-war inequality (see, e.g., Kahneman, Knetsch, and Thaler (1986) on how respect for reference points constrains labor-market equilibria). The wages set by the NWLB and the 1942 Stabilization Act were more egalitarian than those that prevailed in the pre-war economy. While the government officially lifted them in 1946, workers and managers may have simply grown accustomed to this new, more compressed wage structure.

Yet, the immediate post-war years seem an unlikely moment for reference points or expectations to have much bite. First, inflation spiked briefly after the war, which should have quickly eroded any nominal wage stickiness.[78] Second, labor churn reached an all-time high after the war. U.S. military personnel shrunk from over 12 million in 1945 to only 1.5 million by 1947, meaning that over ten million Americans suddenly entered the potential labor

is "What class or group of people in this country has done best financially during the war compared to what they made before the war?" The follow-up question (10b) is: "Do you think any class or group of people in this country is NOT making as much money as it should? [capitalization in the original]."

[78]Annual inflation during the war years averaged 5.1 percent, and was even lower at 3.3 percent between 1943-1945, whereas it averaged over 11 percent in 1946-1947. See `https://www.minneapolisfed.org/about-us/monetary-policy/inflation-calculator/consumer-price-index-1913-`.

supply.[79] Similarly, non-farm payroll contracted by two million (or by 4.9 percent) in the single month of September 1945, a record that would stand in both absolute and percentage terms until the Covid-19-related layoffs in April 2020.[80] Thus, even if workers had formed strong reference points concerning wages during the war, those workers may not have been in the same job or even still in the labor force a few years or even months later. Finally, the War Industries Board during World War I *also* imposed wage controls in war production, though without any of the pro-union policies that accompanied the World War II effort. If norms born from wage controls limit post-war inequality growth, we should have expected a similar, though muted, dampening of inequality in the years after the war, as U.S. involvement lasted only 19 months, compared to 44 in World War II. Instead, the 1920s ushered in historic growth of top-share income inequality.[81]

We thus conclude that in the immediate post-World-War-II era, unions were not particularly popular, and if anything war-era defense production had burnished the reputation of business over that of workers. Nevertheless, war-era policy made unions powerful (both in terms of millions of new members and solid revenue streams via automatic maintenance-of-membership and dues check-off), and over the next few decades they played an important role in maintaining historically low levels of inequality.

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[79] See Acemoglu, Autor, and Lyle (2004).

[80] See `https://www.bls.gov/cps/employment-situation-covid19-faq-april-2020.pdf`.

[81] Goldin and Margo (1992) note that skill premia appear to briefly compress during the First World War in the US but then quickly bounce back, and they also highlight the difference with the Second World War.

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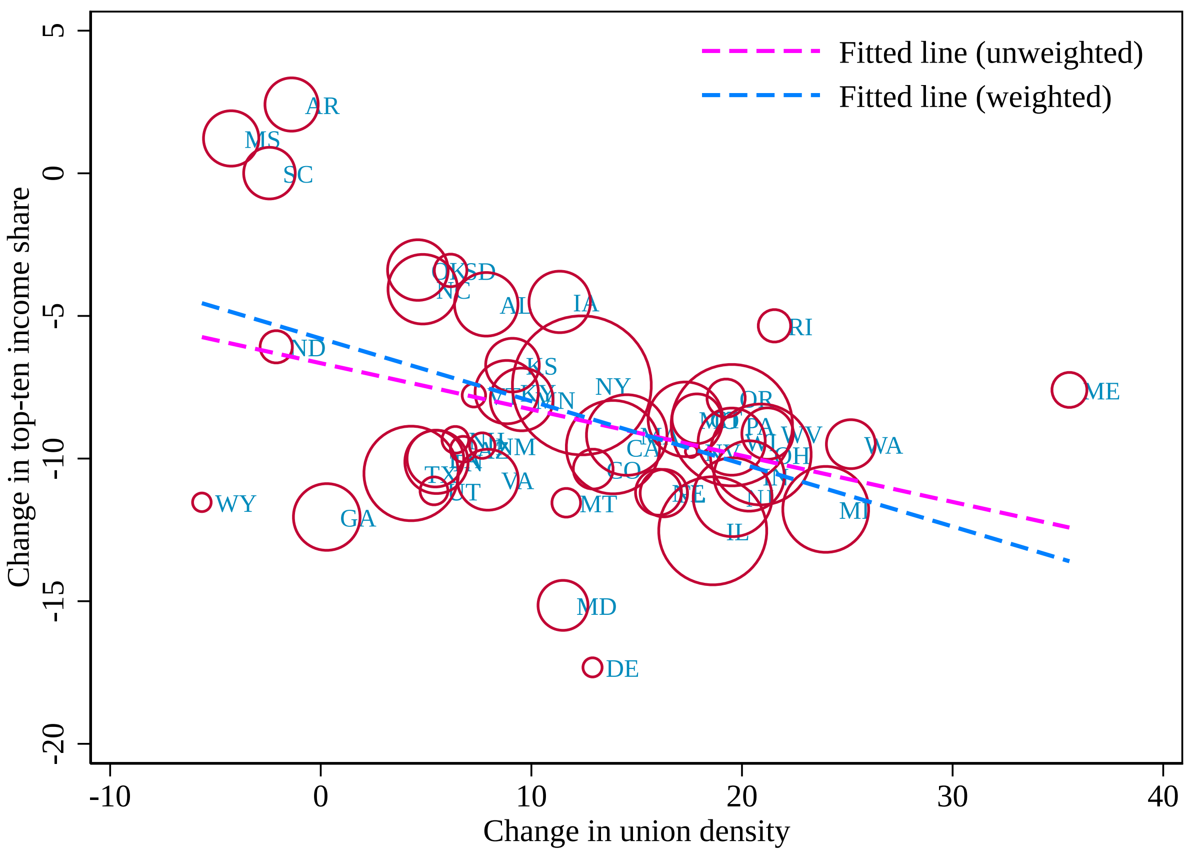
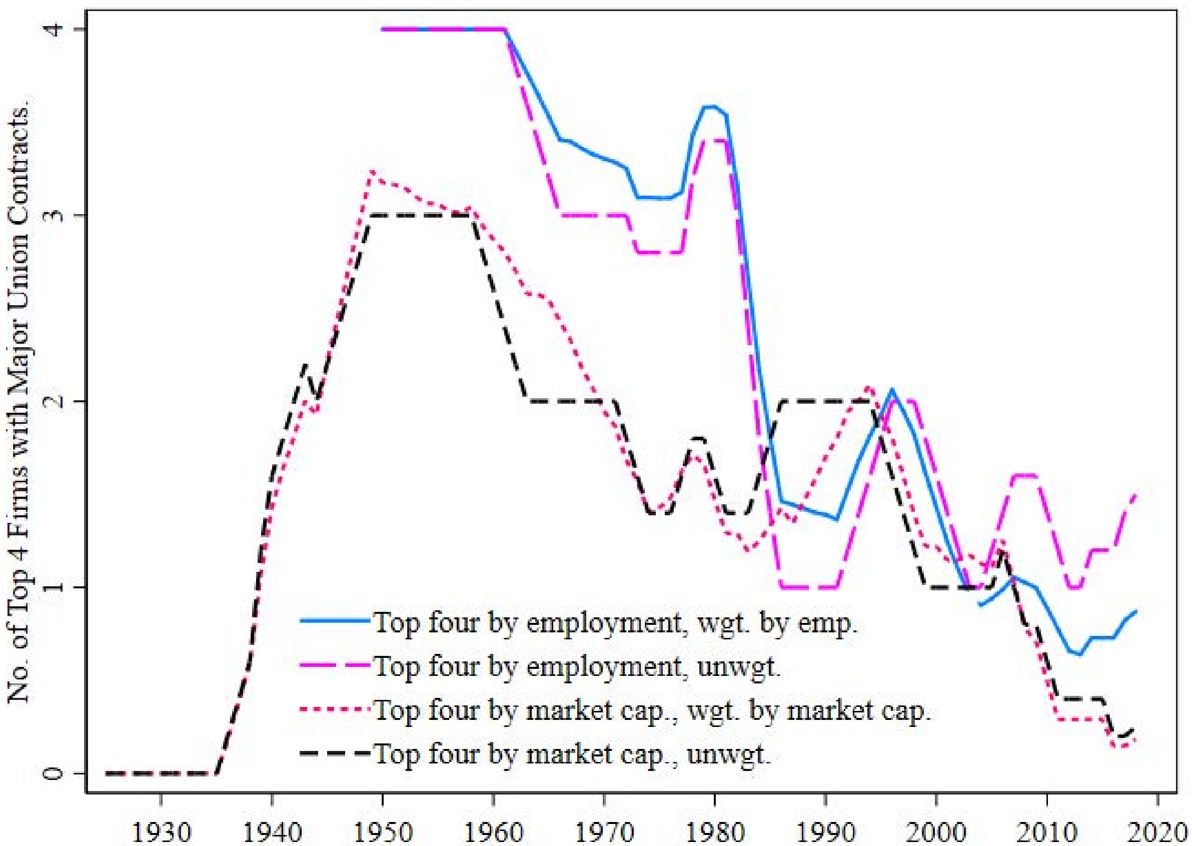
Appendix Figure G.1: Negative correlation between changes in union density and changes in inequality, 1940-1950



Sources: Union density data are from Gallup; top-ten income data are from Frank, 2015.

Notes: On the x-axis, we plot state-level change in union density over the 1940s. On the y-axis, we plot state-level changes in the top-ten income share. To reduce noise, we calculate the change by subtracting the average of 1939 and 1940 from the average of 1949 and 1950. For the "weighted" fitted line, we weight by state population in 1930.

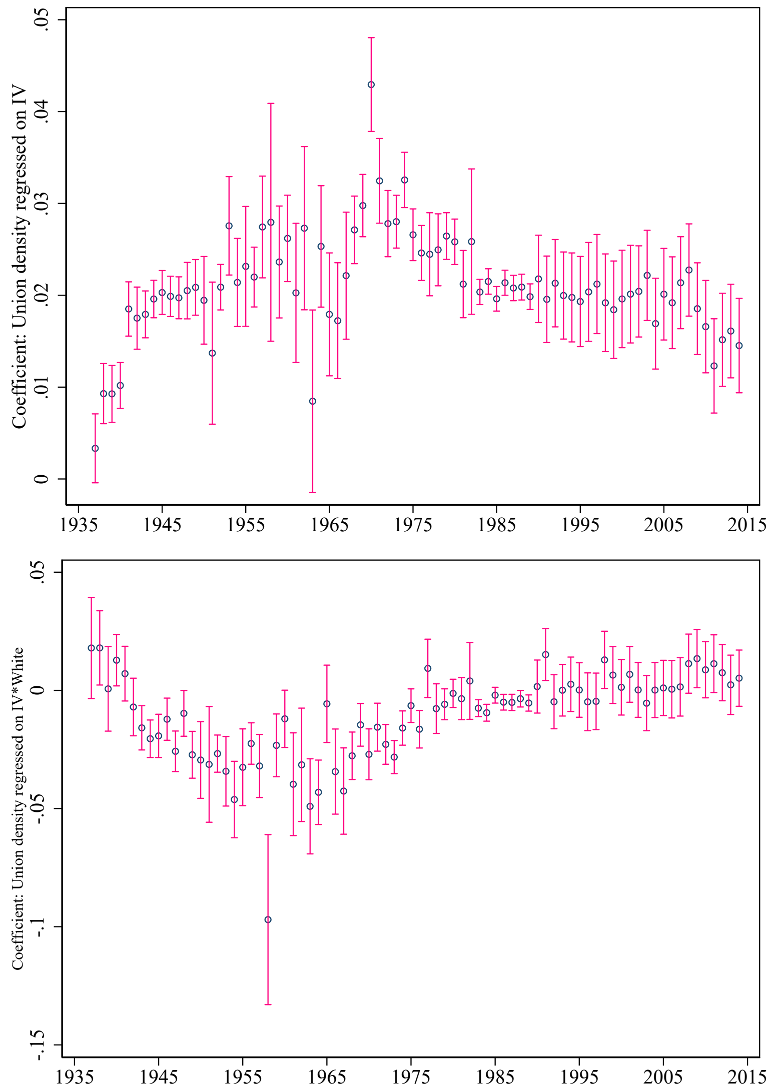
Appendix Figure G.2: Share of "superstar" firms that are unionized



Sources: Market capitalization data from CRSP; employment data from Compustat. Firms are identified by PERMNO in CRSP and GVKEY in Compustat.

Notes: This Figure shows the number of the top-four firms with major union contracts by market capitalization and employment. We identify the union contract status of each of these firms by looking up each of the top four firms on the OLMS collective bargaining agreement website at `https://www.dol.gov/agencies/olms/cba` and the Catherwood library at Cornell `https://digitalcommons.ilr.cornell.edu/blscontracts/`, which together have copies of many major collective bargaining agreements filed with the Department of Labor. If they have any contract listed, we then use historical sources to identify when the first independent union contract (excluding company unions) covered the bulk of their core business. For top four firms with no entry in these two Department of Labor databases, we consult a variety of historical sources to confirm that they were never unionized. More details available on request.

Appendix Figure G.3: IV effect on household unionization and selection into unions by race



Sources: Household data from Gallup and CPS, as described in Section 2.3, Appendix C and B.

Notes: Panel A shows coefficients α_y from the following regression:

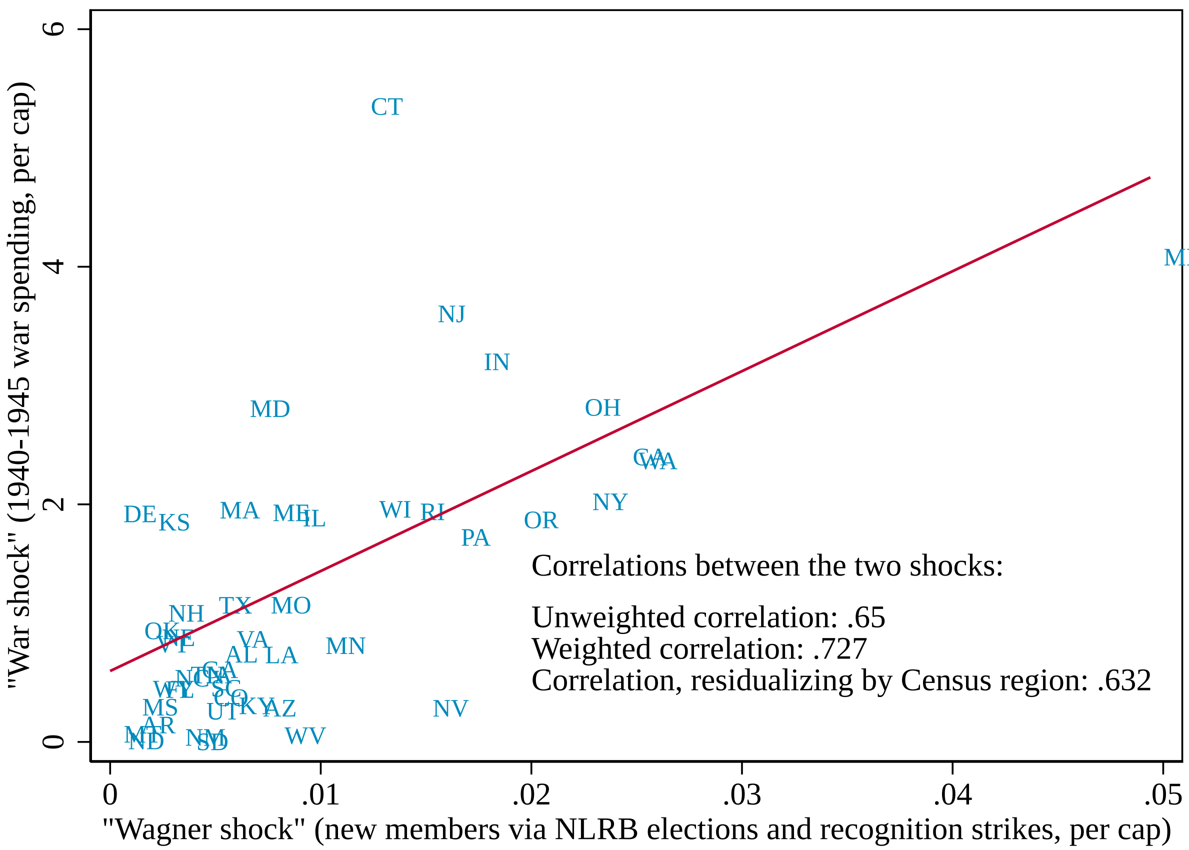
$$Union_{hst} = \sum_{y \leq 2014} \alpha_y \mathbb{I}^{t=y} IV_s + \gamma_1 Female_h^R + f(age_h^R) + \lambda_{r(s)t} + e_{hst}$$

while Panel B shows coefficients β_y from the following regression:

$$Union_{hst} = \sum_{y \leq 2014} \alpha_y \mathbb{I}^{t=y} IV_s + \beta_y White_h^R \times IV_s \times \mathbb{I}^{t=y} + \gamma_1 Female_h^R + f(age_h^R) + \lambda_{r(s)t} + e_{hst}$$

where $Union_{hst}$ is our measure of household union status, IV_s is the pooled policy shock variable; $\mathbb{I}_t^{t=y}$ are year fixed effects; the summation runs over all years y in the sample period for which we have race and state (i.e. 1937); $White_h^R$ denotes white respondents; $Female^R$ denotes femal respondents; $f(age_h^R)$ is a quadratic in respondent age; and $\lambda_{r(s)t}$ is a vector of Census $region \times year$ fixed effects. Panel A shows, by year, the effect of the IV on household union status, while Panel B shows the effect of the IV on differential selection into unions by White households.

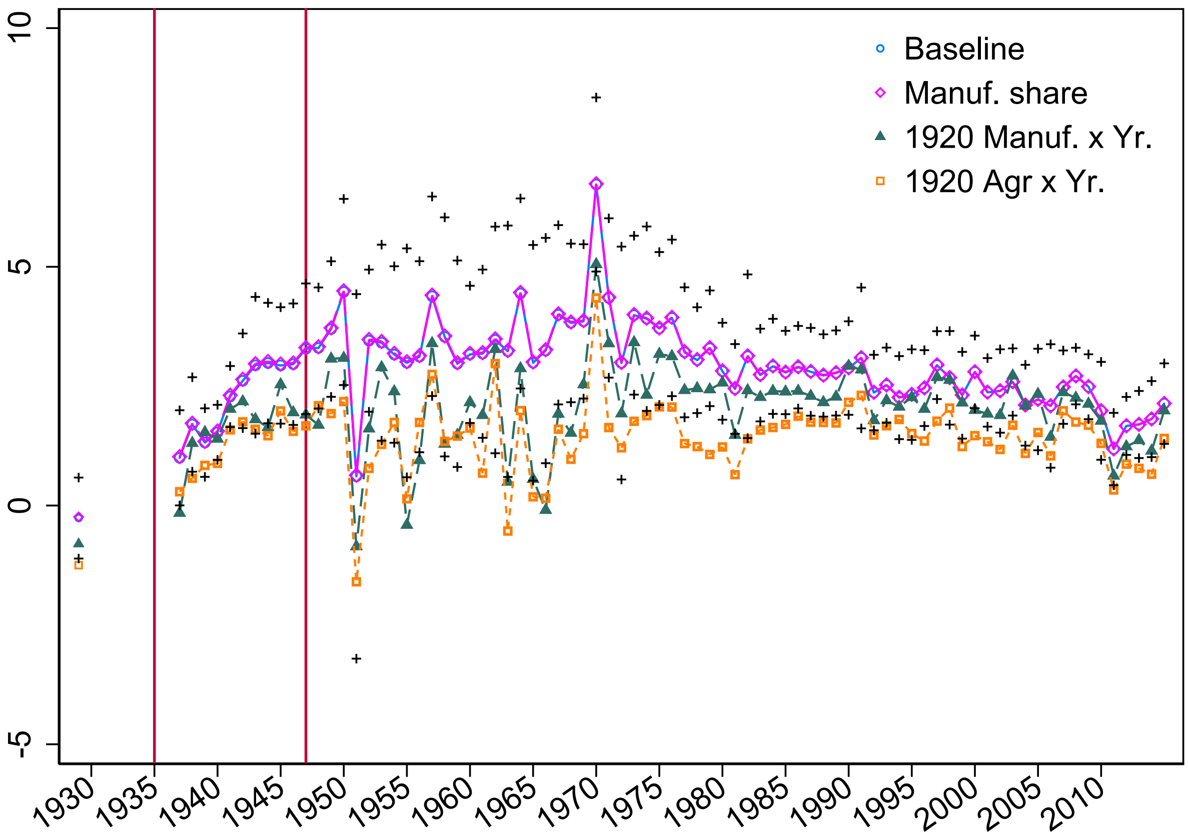
Appendix Figure G.4: Correlation of the two policy shocks used in the IV



Sources: See Appendix Section G.1 for information on the construction of the two policy shock variables.

Notes: On the x-axis is the (per capita) number of new union members by state, in the five years immediately following the passage of the National Labor Relations ("Wagner") Act. On the y-axis is the total value (in 1942 dollars) of military contracts given to firms, by state, from 1940 to 1945. The raw correlation reported is merely the fitted line depicted in the graph. The weighted correlation weights observations by 1930 population, and the residualized correlation is the unweighted correlation after controlling for four Census regions.

Appendix Figure G.5: Regressing union density on the pooled policy shocks IV



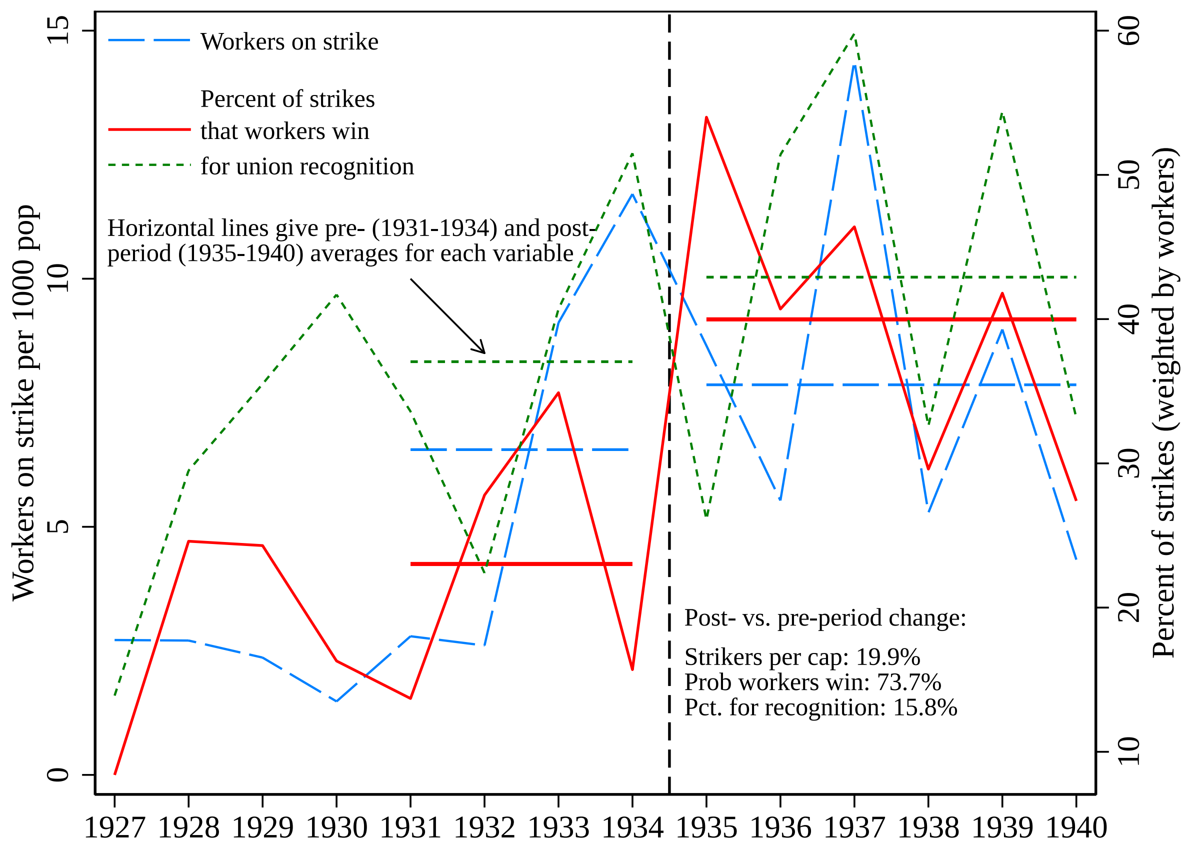
Sources: See notes to Figure 10. For construction of the manufacturing share, see Appendix C.

Notes: The first series reproduces the baseline estimates from Figure 10. That is, it plots the β_t coefficients from the following regression:

$$Union_{st} = \sum_t \beta_t IV_s \mathbb{I}_t^{t=y} + \lambda_{r(s)t} + e_{st},$$

where $Union_{st}$ is state-year union density, $IV_s\mathbb{I}_t^{t=y}$ is the IV interacted with a year-t fixed effect, and $\lambda_{r(s)t}$ is a vector of $region \times year$ fixed effects. The second series adds the contemporaneous manufacturing employment share $Manuf_{st}$ to the baseline equation. The third series adds to the baseline equation the controls $\sum_{y\leq 2014} Manuf_{st}\mathbb{I}^{t=y}$ (i.e., allow contemporaneous manufacturing employment to have a different effect in each year). The fourth series to the baseline equation adds the controls $\sum_y Manuf_s^{1920}\mathbb{I}^{t=y}$,, given the evidence in Appendix Table G.2 that manufacturing share of employment is a key predictor and thus potential a confounder of our IV variable. This control allows the 1920 state-level manufacturing share to have its own effect in each year. The final series adds to the baseline equation the controls $\sum_{y\leq 2014} Agr_s^{1920}\mathbb{I}^{t=y}$, which allows the 1920 state-level agricultural share of employment to have its own effect in each year.

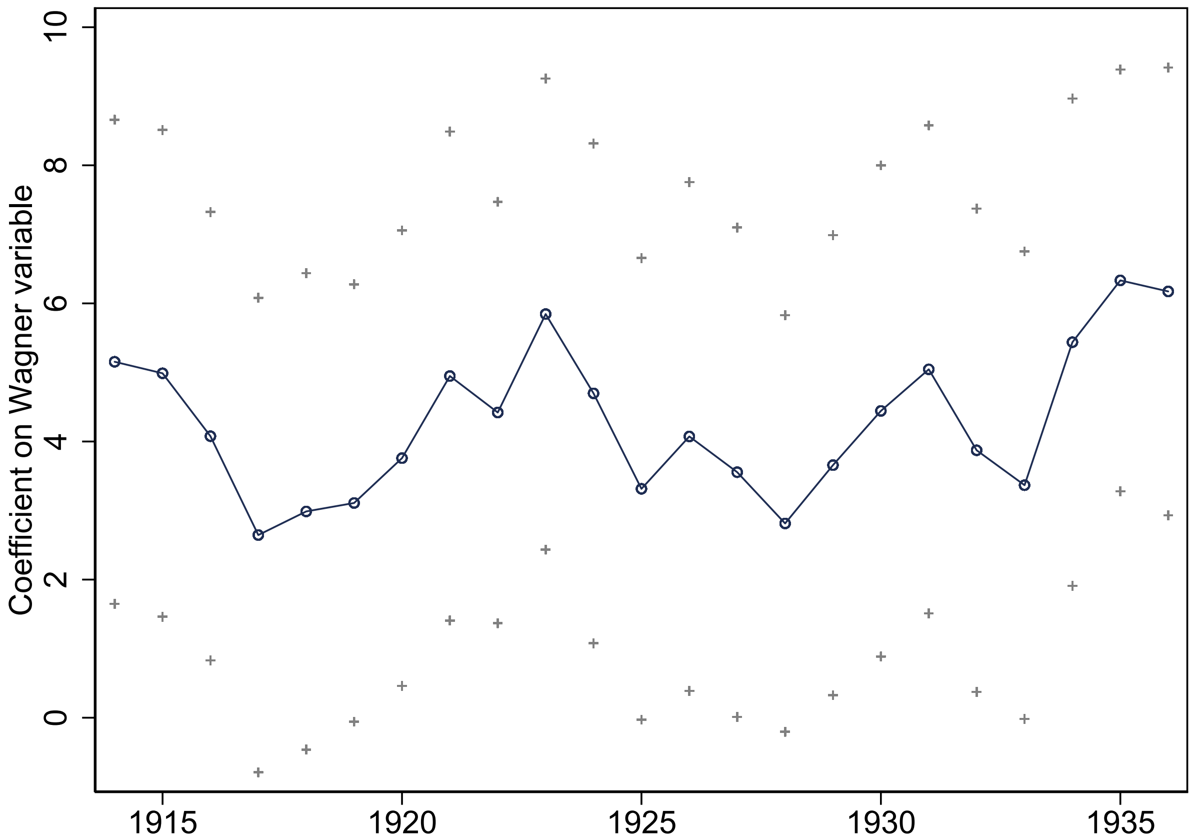
Appendix Figure G.6: Strike activity before and after the 1935 National Labor Relations Act (NLRA)



Sources: All data are taken from BLS publications: Peterson (1937), Peterson (1938), Peterson (1939), Peterson (1940), and Division (1941).

Notes: This figure compares strike activity before and after the passage of the National Labor Relations Act (NLRA, or Wagner Act). The year of passage is marked with the vertical dashed line in the figure.

Appendix Figure G.7: State strike activity regressed on the Wagner policy shock variable by year



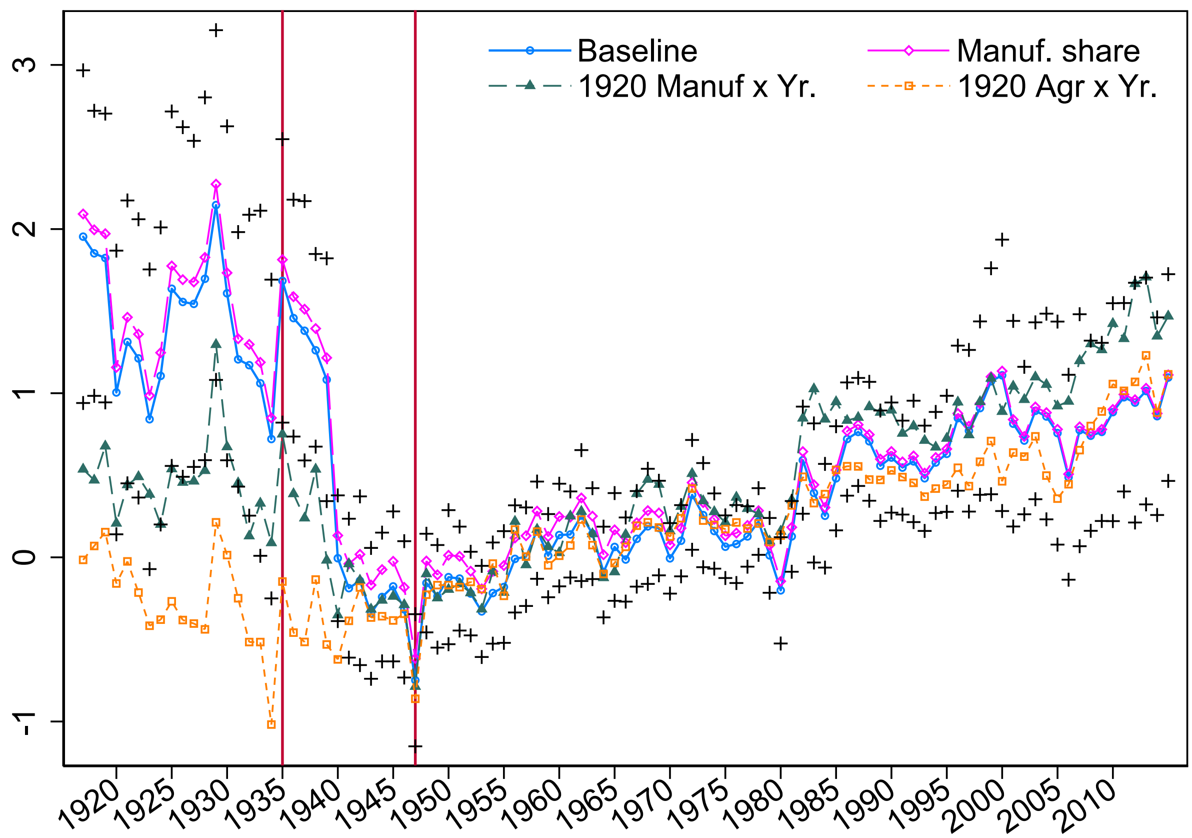
Sources: All data are taken from BLS publication Peterson (1937).

Notes: For each year t of data, we estimate:

$$Rank\, strikes_s = \beta_t Wagner\, shock_s + \lambda_{r(s)} + e_s,$$

where *Rank strikes*$_s$ is the rank of state s in year t with respect to strikes per capita. The measure is increasing in strike activity, so the most strike-prone state in a year would have an outcome value of 47, as we have 47 states each year. $Wagner\, shock_s$ is our usual Wagner-Act policy shock variable, and $\lambda_{r(s)}$ are Census region fixed effects. We plot the estimates for β_t and whiskers mark 95-percent confidence intervals. We use rank instead of strikes per capita to more easily compare coefficients across high- and low-strike years. Note that we analyze strikes per capita, unweighted by the number of workers involved because BLS measures for workers involved are not available for the full period.

Appendix Figure G.8: Regressing top-ten-percent income share on the pooled policy shocks IV



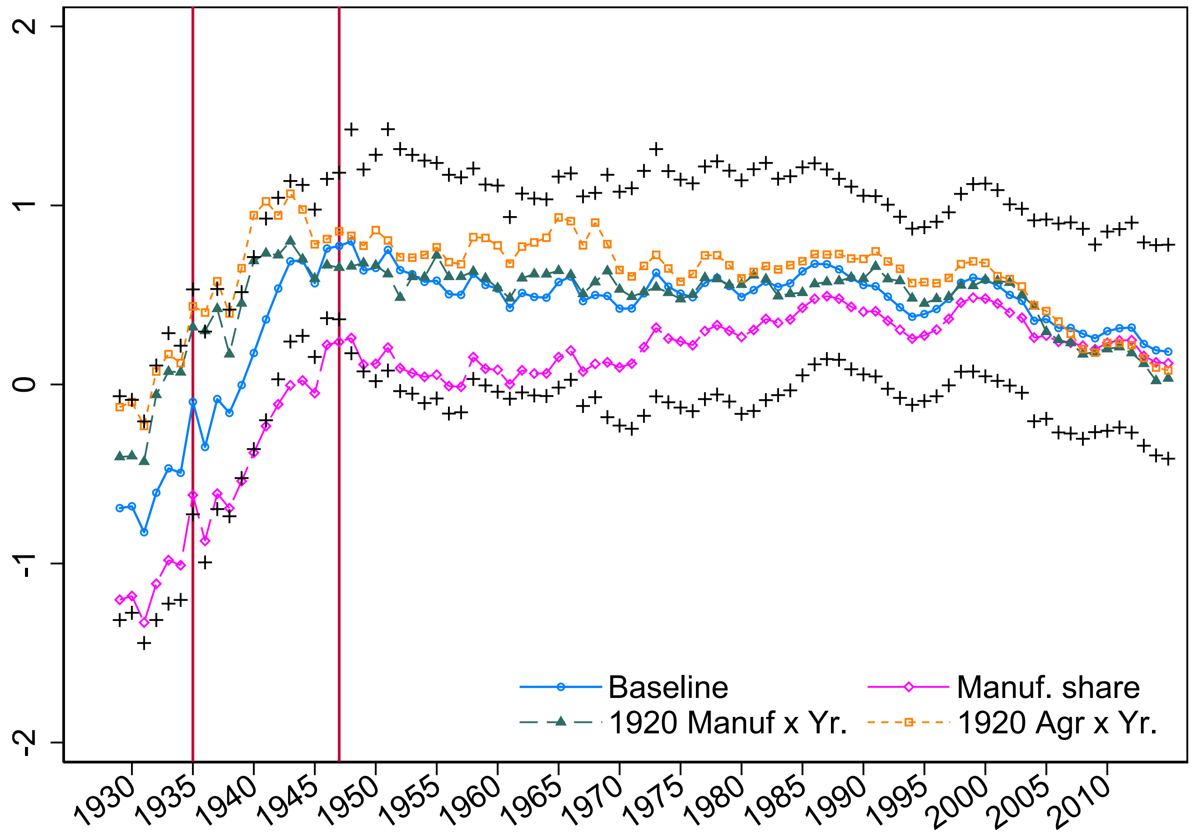
Sources: See notes to Figure 10. For construction of the manufacturing share, see Appendix C.

Notes: The first series reproduces the baseline estimates from Figure 10. That is, it plots the β_t coefficients from the following regression:

$$Top\,ten_{st} = \sum_{y \leq 2014} \beta_y IV_s \mathbb{I}_t^{t=y} + \lambda_{r(s)t} + e_{st}$$

where $Top\,ten_{st}$ is state-year share of income accruing to the richest ten percent of tax units, $IV_s\mathbb{I}_t^{t=y}$ is the IV interacted with a year-t fixed effect, and $\lambda_{r(s)t}$ is a vector of $region \times year$ fixed effects. The second series adds the contemporaneous manufacturing employment share $Manuf_{st}$ to the baseline equation. The third series adds to the baseline equation the controls $\sum_{y \leq 2014} Manuf_{st}\mathbb{I}^{t=y}$ (i.e., allow contemporaneous manufacturing employment to have a different effect in each year). The fourth series adds to the baseline equation the controls $\sum_y Manuf_s^{1920}\mathbb{I}^{t=y}$, which allow the 1920 state-level manufacturing share to have its own effect in each year (given the evidence in Appendix Table G.2 that manufacturing share of employment is a key predictor and thus potential confounder of our IV variable). The final series adds to the baseline equation the controls $\sum_{y \leq 2014} Agr_s^{1920}\mathbb{I}^{t=y}$, which allow the 1920 state-level agricultural share of employment to have its own effect in each year.

Appendix Figure G.9: Regressing labor share on the pooled policy shocks IV



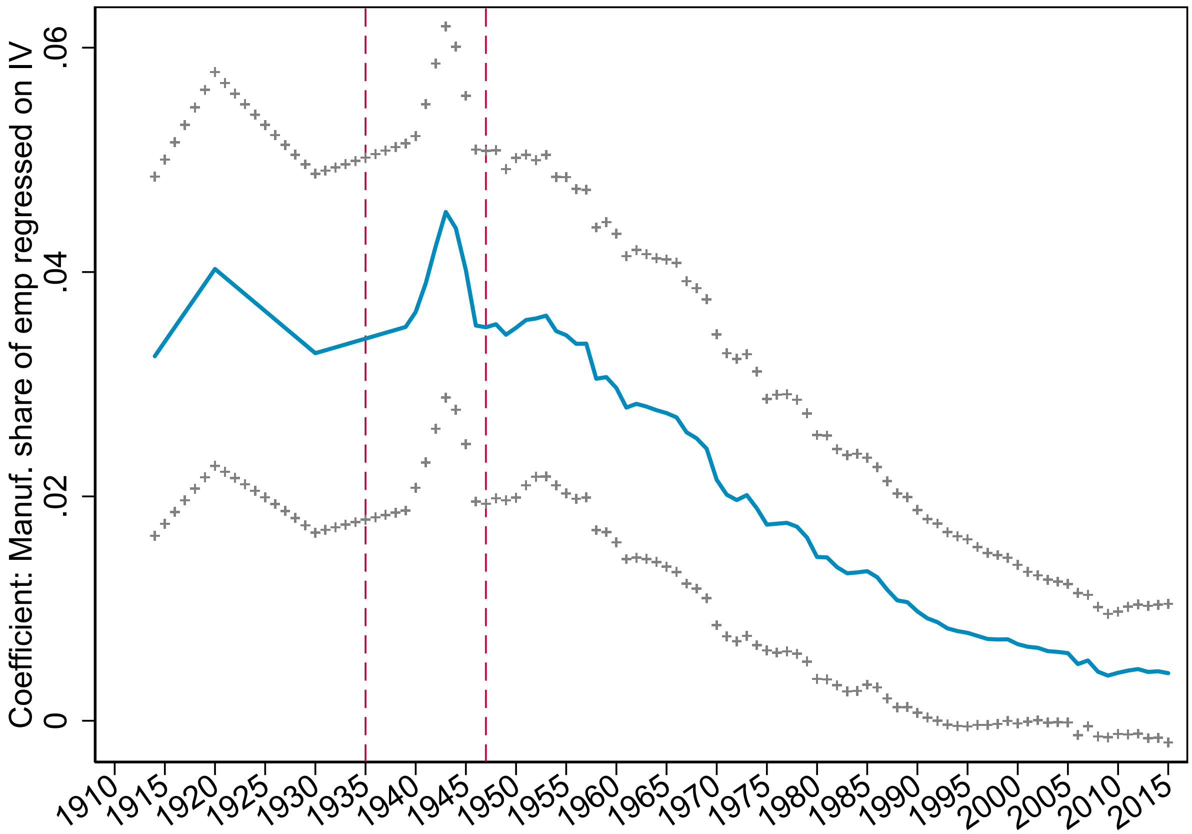
Sources: See notes to Figure 10. For construction of the manufacturing share, see Appendix C.

Notes: The first series reproduces the baseline estimates from Figure 10. That is, it plots the β_t coefficients from the following regression:

$$Labor\,share_{st} = \sum_{y \leq 2014} \beta_y IV_s \mathbb{I}_t^{t=y} + \lambda_{r(s)t} + e_{st}$$

where $Labor\,share_{st}$ is state-year labor share of income, $IV_s \mathbb{I}_t^{t=y}$ is the IV interacted with a year-t fixed effect, and $\lambda_{r(s)t}$ is a vector of *region* $\times$ *year* fixed effects. The second series adds the contemporaneous manufacturing employment share $Manuf_{st}$ to the baseline equation. The third series adds to the baseline equation the controls $\sum_{y \leq 2014} Manuf_{st} \mathbb{I}^{t=y}$ (i.e., allow contemporaneous manufacturing employment to have a different effect in each year). The fourth series adds to the baseline equation the controls $\sum_y Manuf_s^{1920} \mathbb{I}^{t=y}$, which allow the 1920 state-level manufacturing share to have its own effect in each year (given the evidence in Appendix Table G.2 that manufacturing share of employment is a key predictor and thus potential confounder of our IV variable). The final series adds to the baseline equation the controls $\sum_{y \leq 2014} Agr_s^{1920} \mathbb{I}^{t=y}$, which allow the 1920 state-level agricultural share of employment to have its own effect in each year.

Appendix Figure G.10: No sustained effect of the IV on state manufacturing share of employment during the treatment period



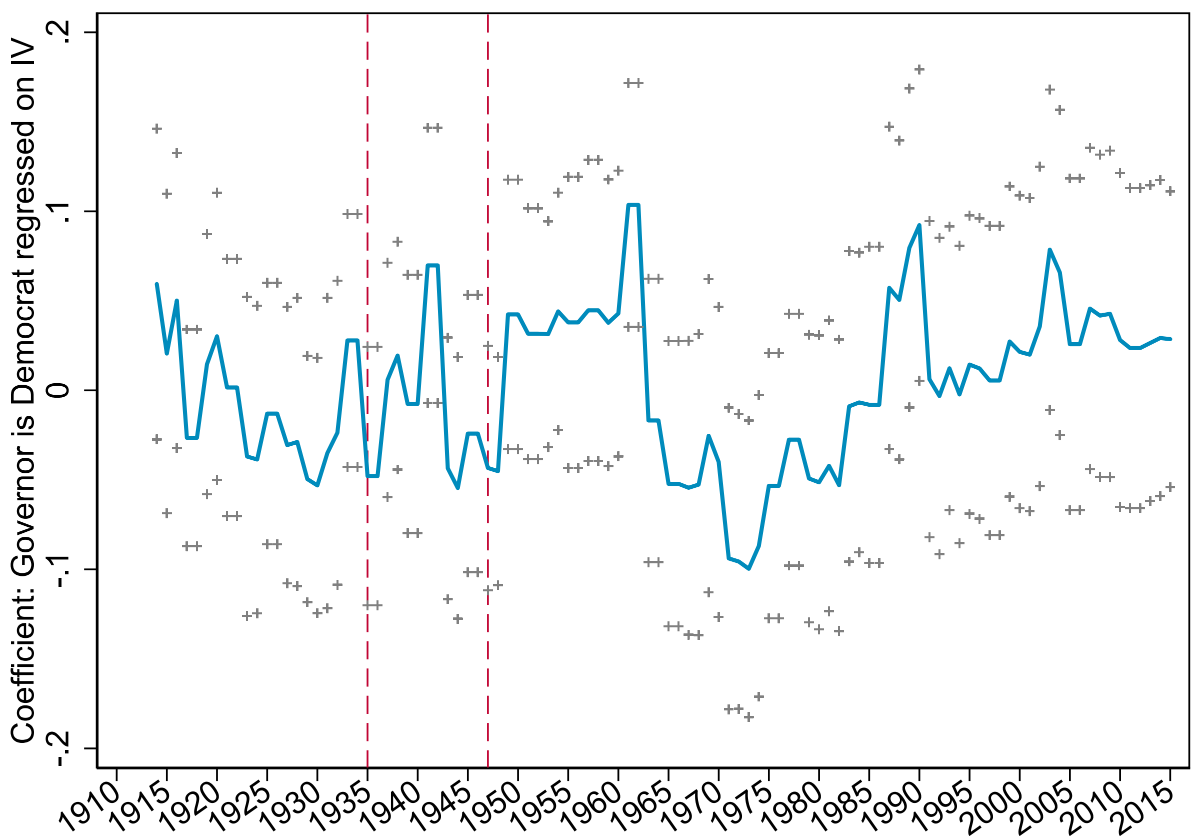
Sources: For construction of the manufacturing share, see Appendix C.

Notes: In this analysis, we follow our baseline specification, but instead consider state manufacturing share of employment as the outcome. That is, the figure plots the β_t coefficients (and their 95-percent confidence intervals) from the following regression:

$$\textit{Manufacturing employment share}_{st} = \sum_{y \leq 2014} \beta_y IV_s \mathbb{I}_t^{t=y} + \lambda_{r(s)t} + e_{st}$$

where all notation is as in the baseline specification. Note that until 1939, we do not have annual data on manufacturing shares and rely on interpolation between Census years. See Appendix C for more details on the construction of the manufacturing employment share data.

Appendix Figure G.11: No systematic relationship between the IV and Democratic governorships



Sources: Democratic governorships data from Besley, Persson, and Sturm (2010).

Notes: In this analysis, we follow our baseline specification in Figure 10, but consider a binary variable coded as one if a Democrat is governor in state s in year t as the outcome. That is, the figure plots the β_t coefficients (and their 95-percent confidence intervals) from the following regression:

$$Democratic\ governor_{st} = \sum_{y \leq 2014} \beta_y IV_s \mathbb{I}_t^{t=y} + \lambda_{r(s)t} + e_{st}$$

where all notation is as in the baseline specification.

Appendix Table G.1: First-stage relationship of the policy shocks and union density

	Dept. variable: Change in state-level union density					
	(1)	(2)	(3)	(4)	(5)	(6)
Wagner shock x (1929-1938)	0.0465*** [0.0127]	0.0468*** [0.0135]	0.0358*** [0.0103]	0.0672*** [0.0136]	0.0567*** [0.0171]	
War shock x (1938-1947)	0.0378*** [0.0130]	0.0376*** [0.0138]	0.0350*** [0.00879]	0.0347** [0.0143]	0.0338** [0.0165]	
Wagner shock	-0.00143 [0.00196]		0.000475 [0.00146]	-0.00151 [0.00346]	-0.00311 [0.00259]	
War shock	-0.00346 [0.00323]		-0.00648** [0.00256]	-0.00356 [0.00346]	-0.00110 [0.00410]	
Change in est. state skill share	-0.0325 [0.0276]	-0.0358 [0.0303]	-0.000852 [0.0322]	-0.0318 [0.0282]	-0.0331 [0.0277]	-0.0336 [0.0274]
War shock x (1929-1938)					-0.0160 [0.0146]	
Wagner shock x (1938-1947)					0.00400 [0.0153]	
Pooled shock x (1929-1938)						0.0217*** [0.00556]
Pooled shock x (1938-1947)						0.0184*** [0.00650]
Pooled Shock						-0.00215 [0.00129]
Dept. var. mean	-0.000763	-0.000763	0.00304	-0.00109	-0.000763	-0.000763
F-stat	17.21	15.44	30.28	26.17	9.664	16.41
Weighted?	No	No	Yes	No	No	No
State FE?	No	Yes	No	No	No	No
Excl. Mich?	No	No	No	Yes	No	No
Observations	409	409	409	400	409	409

Sources: See notes to Table 4.

Notes: Each observation is a state x nine-year interval. We standardize (subtract the mean and divide by the standard deviation) each policy shock variable so their coefficients are easier to compare. The hypothesized treatment period for the Wagner Act shock is the years immediately after its 1935 passage, but due to missing union-density data from 1930-1936, we denote 1929-1938 as its treatment period. The war-spending variable sums state-level war spending from 1940-1945, and so we denote 1938-1947 as its treatment period to use another nine-year interval and to avoid any year of the war when other war-related policies could have direct effects on inequality. Non-treatment intervals are all other non-overlapping nine-year intervals (i.e., 1947-1958, 1958-1967, etc.). In col. (3), weights refer to 1930 state population. In the final column, the pooled IV sums the two (already standardized) policy shock variables. Standard errors are clustered by state. $^{*}p = 0.1,^{**} p = 0.05,^{***} p = 0.01$.

Appendix Table G.2: Correlations of the pooled IV variable with 1920 state characteristics

	Outcome: Pooled Wagner and war shocks IV			
	(1)	(2)	(3)	(4)
Manuf. share of employment	8.263***	3.078	9.484***	6.785
	[1.463]	[2.452]	[3.101]	[4.339]
Agr. share of employment		1.820	0.969	-1.206
		[3.540]	[3.364]	[3.707]
Urban share of pop.		5.572	2.376	0.558
		[3.380]	[3.222]	[3.361]
Black share of pop.		-1.100	-0.391	0.935
		[1.925]	[1.973]	[2.413]
Foreign-born share of pop.		0.772	2.647	3.006
		[3.644]	[3.847]	[3.712]
Log of 1920 state pop		0.313	0.526*	0.0901
		[0.255]	[0.266]	[0.292]
Geographic FE	None	None	Region	Division
Observations	47	47	47	47

Sources: We create state-level averages using 1920 Census micro-data from IPUMS (using person weights).

Notes: By construction, the mean of the dependent variable is zero in all columns, as it is the standardized sum of the two policy shock variables. $^{*}p = 0.1,^{**}p = 0.05,^{***}p = 0.01$.

Appendix Table G.3: Effect of union density on state-level inequality measures, IV results

	Dept var: Top-ten income share						Dept var: Labor-share of state income					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Change in union density	-62.32***	-61.55***	-75.14***	-56.38***	-36.98**	-81.03***	33.88***	33.30***	39.94***	25.20***	26.36***	38.91***
	[10.96]	[10.89]	[17.39]	[12.74]	[14.73]	[16.67]	[6.034]	[6.037]	[5.813]	[5.302]	[6.720]	[7.733]
Wagner shock	0.345	0.0897	0.279*	0.405	0.515**		0.0712	-0.00180	0.127	0.142	0.0186	
	[0.232]	[0.151]	[0.161]	[0.310]	[0.225]		[0.0931]	[0.0964]	[0.108]	[0.146]	[0.102]	
War shock	-0.311	1.602***	-0.275	-0.337	-0.213		-0.0261	-0.192	-0.160	-0.0262	-0.0454	
	[0.346]	[0.286]	[0.268]	[0.355]	[0.361]		[0.138]	[0.159]	[0.143]	[0.135]	[0.146]	
Change in est. state skill share	-5.536***	-5.497***	-3.245	-5.172***	-4.576***	-6.222***	1.394	1.279	0.0402	1.060	1.102	1.567
	[1.853]	[1.924]	[2.273]	[1.831]	[1.518]	[2.166]	[1.096]	[1.159]	[1.299]	[0.928]	[0.958]	[1.167]
Wagner shock x (1938-1947)					-2.369***						0.718	
					[0.703]						[0.669]	
War shock x (1929-1938)					-1.072						0.236	
					[0.723]						[0.311]	
Pooled Shock						0.0750						0.0126
						[0.100]						[0.0499]
Dept. var. mean	0.643	0.643	0.425	0.643	0.643	0.643	0.0320	0.0320	0.367	0.0206	0.0320	0.0320
F-stat	17.21	15.44	30.28	26.17	9.664	16.41	17.21	15.44	30.28	26.17	9.664	16.41
State FE?	No	Yes	No	No	No	No	No	Yes	No	No	No	No
Weighted?	No	No	Yes	No	No	No	No	No	Yes	No	No	No
Excl. Mich?	No	No	No	Yes	No	No	No	No	No	Yes	No	No
Observations	409	409	409	400	409	409	409	409	409	400	409	409

Sources: See notes to Table 4.

Notes: Each observation is a state x nine-year interval. In cols. 1-5 and 6-11 the excluded instruments are the (standardized) Wagner Act shock interacted with its treatment period (1929-1937) and the (standardized) war-spending shock interacted with its treatment period (1938-1947). In cols. 6 and 12 we sum the two policy shocks and interact this pooled IV with the two treatment periods. Non-treatment intervals are all other non-overlapping nine-year intervals (i.e., 1947-1956, 1956-1965, etc.). In cols. (3) and (9), weights refer to 1930 state population. Standard errors are clustered by state. $^{*}p = 0.1,^{**} p = 0.05,^{***} p = 0.01$.

Appendix Table G.4: Effect of union density on state-level inequality measures, reduced form results

	Dept var: Top-ten income share						Dept var: Labor-share of state income					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Wagner shock x	-1.648**	-1.647**	-1.490	-2.472**	-0.747		1.082***	1.082***	1.143***	0.876**	0.978***	
(1929-1938)	[0.747]	[0.790]	[0.934]	[1.122]	[1.175]		[0.222]	[0.235]	[0.224]	[0.384]	[0.343]	
War shock x	-4.155***	-4.155***	-4.835***	-3.832***	-3.579***		1.980***	1.981***	1.921***	2.069***	1.840***	
(1938-1947)	[0.819]	[0.868]	[0.871]	[0.831]	[1.148]		[0.454]	[0.481]	[0.397]	[0.503]	[0.566]	
Wagner shock	0.316	-0.440***	0.110	0.360	0.324**		-0.0123	-0.0383	0.171	0.0681	-0.0252	
	[0.235]	[0.109]	[0.126]	[0.296]	[0.153]		[0.179]	[0.0394]	[0.146]	[0.248]	[0.175]	
War shock	0.0664	0.531***	0.452*	0.0397	0.177		-0.110	0.265***	-0.464**	-0.131	-0.116	
	[0.297]	[0.118]	[0.230]	[0.301]	[0.223]		[0.215]	[0.0556]	[0.185]	[0.225]	[0.215]	
Change in est. state	-3.160**	-3.055**	-3.031**	-3.142**	-3.155**	-3.122**	0.127	0.0882	-0.0522	0.143	0.123	0.0977
skill share	[1.243]	[1.393]	[1.199]	[1.262]	[1.265]	[1.290]	[0.536]	[0.599]	[0.517]	[0.542]	[0.536]	[0.544]
Wagner shock x					-0.978						0.220	
(1938-1947)					[0.852]						[0.431]	
War shock x					-1.568						0.194	
(1929-1938)					[1.022]						[0.398]	
Pooled shock x						-1.142***						0.600***
(1929-1938)						[0.338]						[0.147]
Pooled shock x						-2.230***						1.000***
(1938-1947)						[0.272]						[0.274]
Pooled Shock						0.253***						-0.0686
						[0.0467]						[0.0412]
Dept. var. mean	0.637	0.637	0.426	0.637	0.637	0.637	0.0570	0.0570	0.367	0.0465	0.0570	0.0570
State FE?	No	Yes	No	No	No	No	No	Yes	No	No	No	No
Weighted?	No	No	Yes	No	No	No	No	No	Yes	No	No	No
Excl. Mich?	No	No	No	Yes	No	No	No	No	No	Yes	No	No
Observations	423	423	423	414	423	423	423	423	423	414	423	423

Sources: See notes to Table 4.

Notes: Each observation is a state x nine-year interval. See notes to Table G.3. In cols. 6 and 12, we sum the two policy shocks and interact this pooled IV with the two treatment periods. Non-treatment intervals are all other non-overlapping nine-year intervals (i.e., 1947-1958, 1958-1967, etc.). In cols. (3) and (9), weights refer to 1930 state population. Standard errors are clustered by state.
$^{*}p = 0.1, ^{**}p = 0.05, ^{***}p = 0.01$.

Appendix Table G.5: Effect of union density on top-ten share, robustness checks

	Dept. variable: Change in state top-ten income share											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Change in union	-61.28***	-70.07***	-62.55***	-29.29**	-24.88***	-62.59***	-60.52***	-59.10***	-60.22***	-71.51***	-55.81***	-64.50***
density	[10.71]	[13.79]	[16.54]	[14.18]	[7.983]	[11.95]	[10.50]	[12.62]	[11.09]	[11.36]	[10.66]	[11.12]
Wagner shock	0.319	0.362	0.347	0.311**	0.273	0.348	0.373	0.374	0.321	0.411*	0.328	0.412*
	[0.239]	[0.257]	[0.239]	[0.150]	[0.169]	[0.231]	[0.230]	[0.230]	[0.210]	[0.240]	[0.217]	[0.243]
War shock	-0.360	-0.386	-0.332	-0.00138	0.0591	-0.335	-0.363	-0.267	-0.234	-0.414	-0.258	-0.442
	[0.366]	[0.364]	[0.357]	[0.240]	[0.262]	[0.346]	[0.326]	[0.347]	[0.300]	[0.369]	[0.362]	[0.370]
Control variable		-1.279	-94.07	0.412	2.697	0.179	1.526	0.0725	-11.66	0.00296	0.00891	1.700**
		[3.371]	[209.5]	[1.196]	[2.691]	[0.583]	[1.046]	[0.230]	[7.725]	[0.00352]	[0.0552]	[0.672]
Control var x		13.16*	846.5	9.474**	-64.63***	1.113	42.07	-0.863	4.426	-0.0278**	0.0115	-5.139**
(1929-1938)		[7.903]	[886.1]	[4.675]	[12.36]	[2.213]	[29.75]	[1.289]	[34.74]	[0.0114]	[0.146]	[2.285]
Control var x		4.635	189.9	25.78***	-30.55***	-2.068	-36.62***	-2.085	-12.03	-0.0300**	0.424	-2.118
(1938-1947)		[9.239]	[1004.4]	[5.706]	[4.280]	[2.708]	[9.205]	[1.622]	[32.27]	[0.0129]	[0.296]	[3.412]
Dep. v. mean	0.643	0.643	0.643	0.643	0.643	0.639	0.643	0.643	0.643	0.643	0.643	0.643
F-stat	17.40	11.72	8.876	9.200	11.40	17.68	16.95	10.01	17.73	15.38	17.66	14.91
Control var.	N/A	Manuf. emp. share	1920 Man. share	1920 Ag. share	IRS share	Dem. gov.	Min wage	Liberal index	WWII mob. rate	New Deal aid	State top MTR	State skill share
Observations	409	409	409	409	409	406	409	409	409	409	409	409

Notes: Each observation is a state x nine-year interval. All specifications include the change in skill shares as a control. The first column reproduces col. (1) of Appendix Table G.3. All subsequent columns add explanatory variables $Control_{st}, Control_{st} \times$ (1929-1938), and $Control_{st} \times$ (1938-1947), where $Control_{st}$ is a state-year varying control (listed in the bottom rows of the table), and we include its interactions with the two treatment intervals. The controls are the manufacturing share of employment, the share of tax units that pay federal income tax, whether the governor is a Democrat, the state minimum wage (if it is less than the federal, it is coded as the federal), the state's policy liberalism index (Caughey and Warshaw, 2016), the total New Deal expenditure received by the state in 1933-1939 (taken from Fishback, Kantor, and Wallis, 2003), and the share of the state's young men that were drafted in the Second World War (taken from Acemoglu, Autor, and Lyle, 2004). Standard errors, in brackets, are clustered by state. $^{*}p = 0.1,^{**}p = 0.05,^{***}p = 0.01$.

Appendix Table G.6: Effect of union density on labor share, robustness checks

	Dept. variable: Change in state labor share of income											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Change in union density	33.58***	30.35***	27.70***	26.17***	27.95***	37.15***	32.08***	37.28***	33.78***	34.31***	31.02***	36.26***
	[5.989]	[7.245]	[7.701]	[8.411]	[6.422]	[5.593]	[5.570]	[7.230]	[6.103]	[6.377]	[5.898]	[6.418]
Wagner shock	0.0778	0.0846	0.0984	0.0828	-0.0219	0.0618	0.0534	0.0388	0.0754	0.0642	0.0811	0.116
	[0.0948]	[0.109]	[0.109]	[0.0989]	[0.110]	[0.106]	[0.0888]	[0.0839]	[0.0936]	[0.0979]	[0.101]	[0.0970]
War shock	-0.0137	-0.0462	-0.0279	-0.0878	0.0476	0.0175	-0.0293	-0.0815	-0.0496	-0.0559	-0.0564	-0.0986
	[0.142]	[0.123]	[0.121]	[0.134]	[0.157]	[0.160]	[0.130]	[0.134]	[0.126]	[0.123]	[0.158]	[0.152]
Control variable		-0.465	-76.11	0.0146	-5.967**	0.242	-0.265	0.168	5.725	-0.00238	-0.00569	0.651
		[2.154]	[132.6]	[0.970]	[2.928]	[0.358]	[0.498]	[0.145]	[4.476]	[0.00218]	[0.0367]	[0.425]
Control var x (1929-1938)		-1.247	-9.964	0.100	8.359	-2.394***	15.98	-0.768	-13.00	0.00285	-0.0419	1.175
		[3.406]	[318.3]	[2.659]	[8.917]	[0.911]	[13.57]	[0.682]	[14.03]	[0.00512]	[0.0708]	[1.397]
Control var x (1938-1947)		6.324	701.7	-8.149*	11.73**	2.784	30.52***	0.637	-1.771	0.00177	-0.170	-1.618
		[5.072]	[511.2]	[4.633]	[4.646]	[1.754]	[6.146]	[1.037]	[23.34]	[0.0115]	[0.155]	[1.706]
Dep. v. mean	0.0320	0.0320	0.0320	0.0320	0.0320	-0.00161	0.0320	0.0320	0.0320	0.0320	0.0320	0.0320
F-stat	17.40	11.72	8.876	9.200	11.40	17.68	16.95	10.01	17.73	15.38	17.66	14.91
Control var.	N/A	Manuf. emp. share	1920 Man. share	1920 Ag. share	IRS share	Dem. gov.	Min wage	Liberal index	WWII mob. rate	New Deal aid	State top MTR	State skill share
Observations	409	409	409	409	409	406	409	409	409	409	409	409

Notes: Each observation is a state x nine-year interval. The first column reproduces col. (7) of Appendix Table G.3. All subsequent columns add explanatory variables $Control_{st}, Control_{st} \times$ (1929-1938), and $Control_{st} \times$ (1938-1947), where $Control_{st}$ is a state-year varying control (listed in the bottom rows of the table), and we include its interactions with the two treatment intervals. The controls are the manufacturing share of employment, the share of tax units that pay federal income tax, whether the governor is a Democrat, the state minimum wage (if it is less than the federal, it is coded as the federal), the state's policy liberalism index (Caughey and Warshaw, 2016), the total New Deal expenditure received by the state in 1933-1939 (taken from Fishback, Kantor, and Wallis, 2003), and the share of the state's young men that were drafted in the Second World War (taken from Acemoglu, Autor, and Lyle, 2004). Standard errors, in brackets, are clustered by state. $^{*}p = 0.1,^{**} p = 0.05,^{***} p = 0.01$.

Appendix Table G.7: Using 1920s strikes instead of the Wagner Act variable as an IV

	Dept var: Nine-year changes in....		
	(1)	(2)	(3)
	Union density	Top-ten share	Labor share
Change in union density		-114.5***	39.48***
		[32.44]	[14.96]
Strikes 1921-1928 x (1929-1938)	0.0375**		
	[0.0163]		
War shock x (1938-1947)	0.0351***		
	[0.0130]		
Strikes 1921-1928	-0.00215	-0.125	-0.0310
	[0.00228]	[0.284]	[0.111]
War shock	-0.00110	0.164	0.0147
	[0.00260]	[0.275]	[0.122]
Dept. var mean	-0.000763	0.643	0.0320
F-stat		6.635	6.635
Observations	409	409	409

Notes: The regressions in this table are identical to, respectively, col. (1) of Appendix Table G.1 and cols. (1) and (7) of Appendix Table G.3, except that strikes per capita from 1921 to 1928 is used instead of the Wagner shock variable. Standard errors clustered by state.
$^{*}p = 0.1,^{**} p = 0.05,^{***} p = 0.01$.

Appendix Table G.8: Are respondents in states hit with policy shocks more likely to express pro-worker views?

	Dept. var: Subject says these groups should be doing better (x100)							
	Workers, laborers, poor				Biz owners, managers, profs.			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pooled IV	-0.443				1.082			
	[0.335]				[1.081]			
Wagner shock		-0.600		-0.201		0.457		-2.928
		[0.501]		[0.801]		[1.420]		[1.942]
War shock			-0.949	-0.774			4.022^{*}	6.572^{**}
			[0.842]	[1.293]			[2.186]	[3.207]
Mean, dept. var.	8.966	8.966	8.966	8.966	37.58	37.58	37.58	37.58
Observations	2911	2911	2911	2911	2911	2911	2911	2911

Sources: Data taken from Gallup survey fielded June 1-5, 1945.

Notes: Each observation is a respondent. The outcome variable is based on the second part of a two-part question (items 10a and 10b) . The wording of 10a is “What class or group of people in this country has done best financially during the war compared to what they made before the war?”. The follow-up question 10b reads: “Do you think any class or group of people in this country is NOT making as much money as it should? [capitalization in the original].”
$^{*}p = 0.1,^{**} p = 0.05,^{***} p = 0.01.$

Appendix H. Construction of historical state-year labor share of net income

The standard state-year measure of labor share from the Bureau of Economic Analysis (BEA) extends back only to 1963. In this Appendix, we introduce a modified state-year measure of labor share (that is, a state-year measure of the labor share of *net national income*) that we construct beginning in 1929. We describe our methodology (in particular how it builds off of past work) and compare our measure to other measures at the aggregate and state-year level.

The BEA has constructed the state-year labor share of GDP, but only since 1963, and labor shares of personal income since 1929. The former doesn't span the entire time period of our sample; the latter ignores corporate income and so is not consistent with the macroeconomic accounts. In this section we describe how we construct labor shares of *net* income using the information in the components of personal income and allocations of national corporate income available from the BEA, following Piketty-Saez-Zucman 2019 (PSZ). At the end, we show how our measured labor share of net national income compares with the BEA GDP measures during the years they overlap. We use the components of personal income available since 1929, together with allocations of national pre-tax corporate retained earnings, to construct a measure of Net National Income at the state-year level from 1929 onwards. We remind readers of the differences below:

- Gross Domestic Product (GDP) = labor income paid by firms +taxes on production (indirect taxes) + capital income paid by firms. This production-side measure is available at the aggregate level back to 1929 and at the state-year level back to 1963.

- Gross National Income (GNI) = labor income earned by residents + taxes on production (indirect taxes) + capital income earned by residents + out-of-state income transferred to residents. This income-side measure is based on flows to residents of a jurisdiction (state or country). In a closed economy this is identical to GDP, but in an open economy there can be differences.

- Net National Income (NNI) = GNI - depreciation. The definition used by PSZ (p. 561) includes indirect taxes paid to government as income. PSZ then apportion indirect tax income to individuals based on labor and capital incomes minus savings. Other authors exclude indirect taxes from net national income (Rognlie 2015). We will exclude indirect tax income due to data limitations, as we do not observe disaggregated savings in our historical period and so cannot apportion it.

- Personal Income (PI) = NNI - indirect taxes - contributions to government social insurance + transfers from government and business (e.g. insurance payouts) - corporate profits. Personal income is also an income side measure, but deducts income that cannot be spent (indirect taxes, contributions to government insurance, and corporate retained earnings) This measure is available at the state-year level from the BEA back to 1929. See `https://www.bea.gov/system/files/2019-03/SPI2017.pdf` for a more detailed description.

Recall that in a closed economy, income received by individuals is equal to payments to factor owners, so GNI = GDP. Net National Income, however, is theoretically closer to a welfare measure (Weitzman 1976). NNI deducts depreciation from GNI, which is the loss of value to capital holders owing to wear and tear and obsolescence of capital goods. Nobody can consume or save depreciation, so it is deducted from GNI to get measures of income received by capital owners. The capital share of GDP (gross operating surplus in the BEA GDP accounts) reflects capital's importance in production, but net capital income reflects the income accruing to owners of capital. The labor share of net income is the total wage income received by residents plus employer contributions plus labor's share of self-employment income, all divided by total income received by residents and all measured prior to any taxation.

In an open economy, besides depreciation, GDP and NNI can also differ due to differences between the location of production and the location of individuals. GDP records the payments made to workers and owners, regardless of where they live from firms in a jurisdiction. NNI records the payments received by workers and owners living in a jurisdiction, regardless of where the firm paying them is located. The inclusion of income earned from out-of-state is particularly important for measures of inequality because out-of-state income (especially capital income) is potentially large for the rich. While the macroeconomics literature has focused on labor's share of GDP, the literature on distributional accounts and inequality has focused on NNI, and we follow the latter literature here.

Personal income, on the other hand, summarizes all the disposable income received by residents in a state. The labor share of personal income captures labor income as a share of all incomes paid to residents of a state, including transfer payments (but excluding government insurance payments). It is not clear how to handle transfer income: one could either include it in labor income or remove it from total income. We choose the latter in order to focus on a pre-tax measure. Further, personal income is inconsistent with the national accounts, as it includes capital income paid to owners (i.e. interest, rent, and dividends) but excludes retained corporate earnings. As a result the labor share of personal income could be significantly higher than labor share based on the national accounts (indeed the labor share of personal income approaches one during World War II). These difficulties in interpreting the labor share of personal income make it a less-than-ideal measure for estimating the effects of unionization on the distribution of factor income. One benefit of using this measure, however, is that personal income was a focus of economic measurement prior to World War II, so its components are available at the state-year level over a much longer time period. In contrast, state-level GDP components are only available beginning in 1963.

We therefore focus on labor share of NNI, which has three advantages over either GDP or Personal Income, as it is a measure that is a) constructible back to 1929, b) consistent with the national accounts, and c) comparable to the recent literature on inequality.

H.1. Data Availability and Construction of Measures

At the state-year level, the divergence between production and incomes can be considerable, due to much factor income being paid to out-of-state agents and much income being derived from out-of-state asset holdings and transfers. One could imagine unionization having different impacts on NNI vs GDP. While GDP reflects how the organization of production compensates suppliers of capital and labor whereever they are, NNI reflects how residents receive capital and labor income. However, there is no measure of state-year GDP prior to 1963. We can, however, construct a NNI-based measure from the BLS/BEA estimates of **personal income** and its components, which exist back to 1929.

From the definitions above, we can see that NNI = personal income plus corporate net retained earnings plus contributions for government social insurance minus asset income minus transfers. We do not observed any state-level allocations of corporate net retained earnings, which are components of the capital share of GDP and NNI. At the national level, the income from assets held elsewhere + transfers roughly equals the corporate retained earnings plus social insurance contributions, so GDP is nearly equal to NNI + depreciation as shown in: `https://fred.stlouisfed.org/release/tables?rid=53&eid=15274#snid=15293`. Yet, this may not be true at the state level, so the differences between GDP and NNI may be quite large.

Total labor compensation includes wage and salary payments plus employer contributions. The BEA data historically tracked income labelled as "Supplements to wages and salaries" which combines two accounts: "Contribution for government social insurance" and "Employer contributions for employee pension and insurance funds". In measuring Personal Income, the "Contribution for government social insurance" is deducted to get the labor compensation component of personal income because it is not realized as personal income. We have these components separately, so we can add back "contributions for government social insurance" to worker compensation. As discussed above, we deduct government transfers.

The remaining issue for calculating NNI is allocating corporate net retained earnings (before taxes). We assume that national corporate net retained earnings are allocated proportionally to interest/dividend/rental income. PSZ assign corporate net retained earnings across the income distribution. They allocate this income in proportion to corporate equity holdings imputed from dividends and capital gains reported on tax returns and provide auxiliary evidence that this assumption is warranted. Unfortunately, we do not see state-level capital gains income at all nor do we see dividends income by itself in the BEA accounts. Instead we observe the total payments of interest, dividends, and rental income, including imputed rent on housing.

We can assess the plausibility of our allocation rule using the IRS state-level SOI data from 2001-2017, which report capital gains, dividends, interest income, as well as real estate taxes paid. We follow Saez and Zucman (2016) and convert real estate taxes paid into a value of housing stock by assuming a uniform effective tax rate of 1%, and then we convert the implied stock of housing wealth into a rental income flow by multiplying by the PSZ rate of return on gross housing in that year. The average is 7% over this sample period. We can then compare a state's share of dividends plus interest plus rental income with a state's share of equity plus dividends. Figure H.1 shows the year-by-year regression coefficients. In every year between 2001 and 2017, the coefficient from a bivariate regression of state share of dividends + interest + rental income on a state's share of dividends plus capital gain income is greater than 0.9 and generally statistically indistinguishable from 1. This result suggests that the assumption that net retained earnings is allocated across states in proportion to personal capital income (excluding capital gains) is quite reasonable.

Under the assumption that a state's share of corporate retained earnings is proportional to a state's share of dividend, interest, and rental income, our calculation of NNI is given by:

$$Y_{st}^{NNI} = Y_{st}^{PI} + SocIns_{st} - Transfers_{st} + \frac{Y_{st}^{PI,K}}{\sum_{s' \in S} Y_{s't}^{PI,K}} NetCorpRetainedEarn_t \qquad (19)$$

Note that if all corporate profits (minus depreciation) are paid out as dividends, they will be included in the personal income paid as capital income $Y_{st}^{PI,K}$, and net corporate retained earnings is 0. Then NNI will be identical to personal income plus social insurance minus transfers. We calculate pre-tax net corporate retained earnings as the sum of net private saving by domestic business plus corporate taxes paid, both of which are available in the national accounts back to 1929. We also make use of PSZ's estimate of aggregate capital income, $Y_t^{NNI,K}$ as a check and construct an alternative measure of pre-tax net corporate retained earnings as the difference between national capital income and the sum of state-level personal capital income, so $NetCorpRetainedEarn_t = Y_t^{NNI,K} - \sum_{s' \in S} Y_{s't}^{PI,K}$. We find that these two measures of net corporate retained earnings are extremely close, and the differences are likely due to the adjustment for sales taxes in PSZ.

H.2. Construction of the aggregate series

Data sources:

- Annual estimates (1929-2018) of the compensation of employees is obtained from the `https://fred.stlouisfed.org/series/GDICOMPA`FRED.
- Annual estimates (1929-2018) of the GDP are also obtained from the `https://fred.stlouisfed.org/series/GDPA`FRED.
- Annual estimates (1929-2018) of NNI were obtained from PSZ, for comparison.

Calculation

We calculate the aggregate labor share by dividing total compensation of employees by GDP.

H.3. Construction of the state-year series

Data sources

- Data on state-level personal income (1929-2018), as well as wages and salaries, supplements to wages and salaries, and proprietor's income is obtained from the `https://apps.bea.gov/itable/iTable.cfm?ReqID=70&step=1`BEA—Annual State Personal Income and Employment, Personal Income and Employment by Major Component (SAINC4).
- Data on the national `https://fred.stlouisfed.org/series/A065RC1A027NBEA`Personal Income, `https://fred.stlouisfed.org/series/GDPA`GDP, `https://fred.stlouisfed.org/series/A127RC1A027NBEA`net private saving by domestic business, `https://fred.stlouisfed.org/series/FCTAX`federal taxes on corporate income, and `https://fred.`

stlouisfed.org/series/ASLCTAXstate and local taxes on corporate income in each year was obtained from the FRED.

Calculation

We define the labor share of NNI in state s in year t as:

$$Labor\, share_{st} = \frac{wages_{st} + supplements_{st} + 0.66 proprietors\, income_{st}}{Y_{st}^{NNI}},$$

where Y^{NNI} is calculated as in Equation (19). Imputing 2/3 of proprietor's income to labor income is standard for advanced countries (see Krueger, 1999, Gollin, 2002 or Johnson, 1954). We discussed this measure with BEA staff, who confirmed that there is no exact way to get corporate retained earnings at the state-year level, mostly because corporate income is not reported (and hasn't ever been reported) at the state level to the BEA, except for a few highly regulated sectors. We confirm that our measure is highly correlated with the standard BEA measure when they overlap. We further consulted the BEA to see if there was a way to construct a more comprehensive denominator (i.e. including corporate retained earnings) and were told that this would not be feasible with existing data.

H.4. Results

Figure H.2 plots different measures of aggregate labor share over our sample period. The numerator is identical in all measures, but the denominator corresponds to different definitions of income. The figure shows the labor share of Personal Income, Personal Income Without Transfers (which approaches one during World War II), our measure of Net National Income, the measure of labor share of Net National Income from Piketty, Saez, and Zucman (2018) for comparison, and labor share of GDP. Our measure tracks the PSZ measure very closely, with the difference being the deduction of indirect taxes from our NNI measure due to lack of data on how the sales tax incidence (i.e., consumption) is divided between capital and labor income. The labor share of GDP measure is generally the lowest, consistent with depreciation being added to the GDP denominator but not affecting the numerator.

Figure H.3 shows the series for Personal Income, NNI, and GDP (for the post 1963 years) separately for two high union density states and two low union density states. In all cases, while labor share of personal income is greater than labor share of NNI and labor share of NNI is greater than labor share of GDP, all series roughly track each other.

Panel B of Table A.10 shows the same repertoire of state-year panel specifications shown in the paper for other outcomes with the labor share of Net National Income as the outcome. As discussed in the main text, coefficients are uniformly positive and significant, although the inclusion of state-specific quadratic trends lowers the sign and significance somewhat. Table shows the same specifications, with the labor share of Personal Income as the outcome. Coefficients are again uniformly positive and significant, although somewhat less stable and more sensitive to the state-specific time quadratic than the NNI based labor share regressions.

We next compare our results with GDP-based labor share to those with NNI-based labor share, for the years for which we have comparable data. Table H.2 shows two specifications, one without any controls save state and year (and south X year) fixed effects, the other with all the controls in column 6 of Panel B of Table A.10. We show the full sample with our measure of labor share, and then restrict attention to the post-1963 sample where the BEA's labor share of GDP measure is available. While the effect of union density of labor share of GDP is positive and of comparable magnitude (albeit not significant) to the full-sample in the specification without controls, the inclusion of all the controls (particularly the state-specific quadratics) makes the effect close to 0 and insignificant. Our labor share of NNI measure, while smaller in both specifications compared to the full sample, remains statistically significant in both. Much more of the production side of income statistics (as opposed to the income side) at the state-level is interpolated in the 5 years in between Economic Censuses, and this smoothing may be one reason for the difference in precision as well as differential robustness to controls in the two measures.

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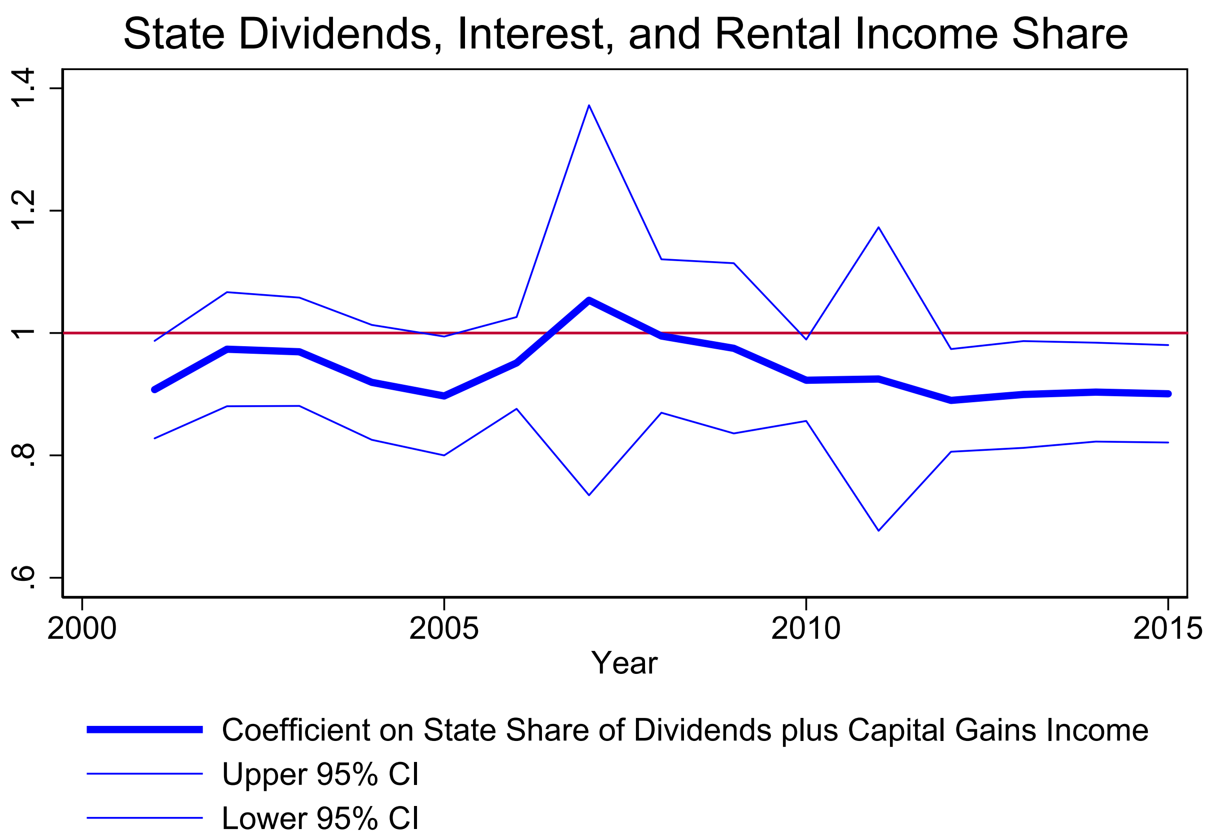
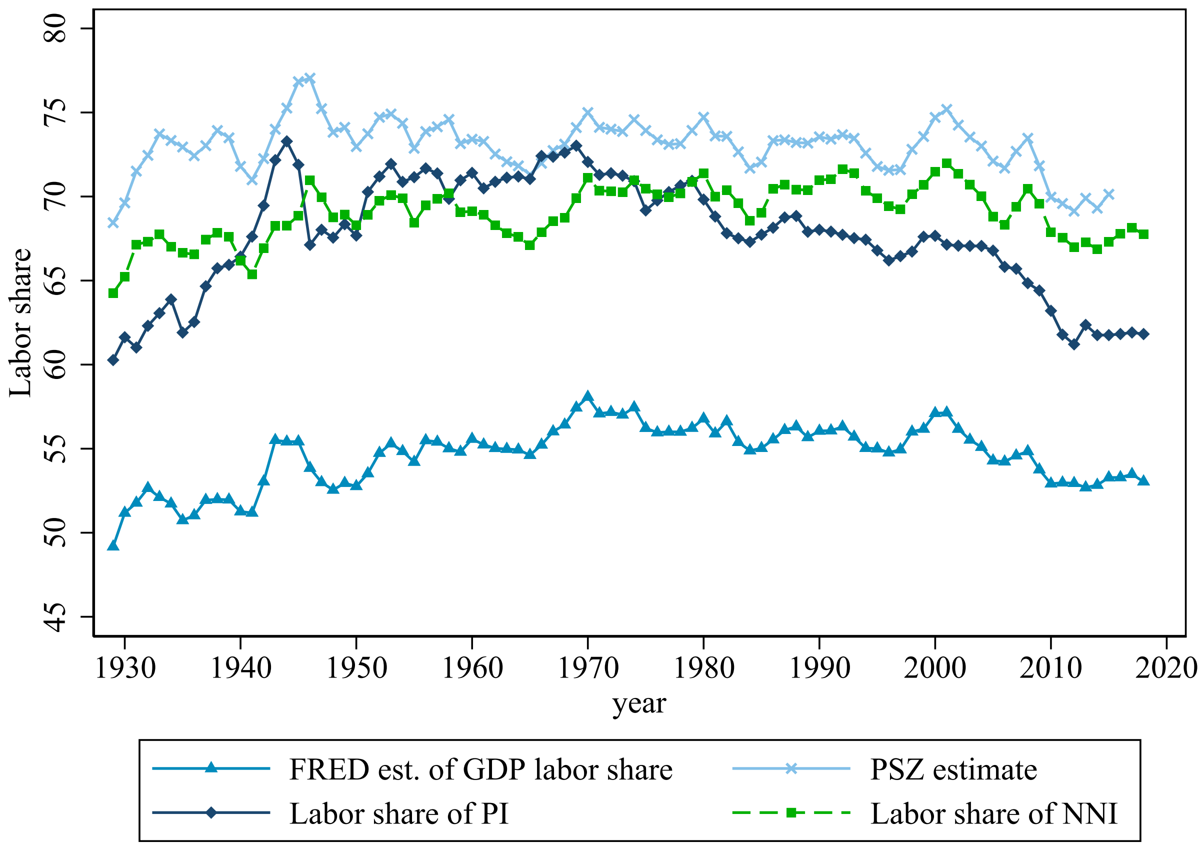
Appendix Figure H.1: Similarity of Shares of Capital Gains Plus Dividends and Shares of Dividends, Interest, and Rental Income.



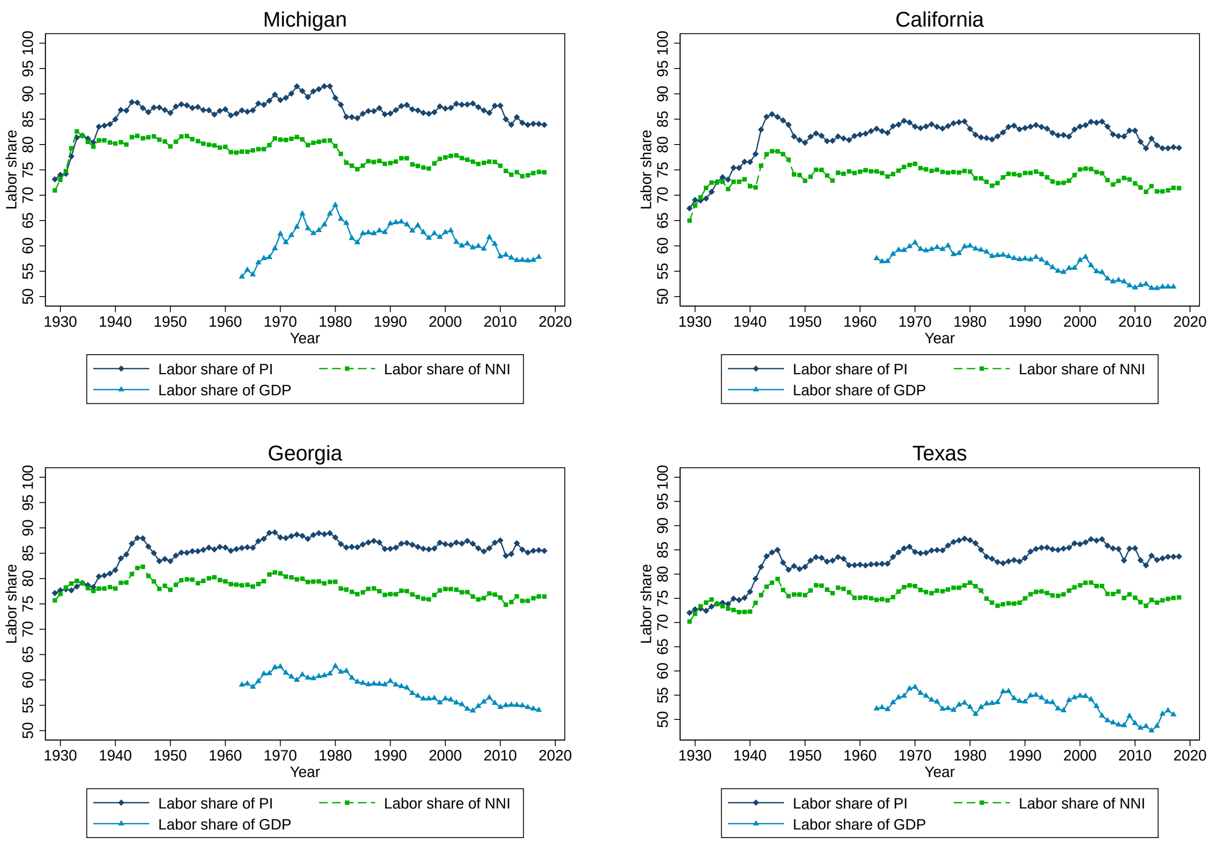
Notes: This graph shows coefficients β^t and confidence intervals from separate regressions of the form $X_{st}^{y_{kgains}+y_{div}} = \alpha_t + \beta^t X_{st}^{y_{div}+y_{int}+y_{rental}} + \epsilon_s$ where X_{st}^{y} denotes the share of taxable income y accruing to residents of state s in year t. Data from IRS Statistics on Income, with rental income y_{rental} calculated from real estate taxes paid as described in the text.

Appendix Figure H.2: Time Series of Aggregate Labor Share Measures



Notes: This graph plots the time-series estimate of our constructed labor share of net income measure (which excludes indirect taxes) against the Piketty, Saez, and Zucman (2018) measure of labor share (which includes indirect taxes) and the GDP and Personal Income based measures of labor share from the BEA.

Appendix Figure H.3: Time Series of Labor Share: High and Low Union Density states



Note: In this figure we plot our state-year estimate of net income based labor share and compare it to the personal income based and GDP based labor income shares for two high union density states (Michigan and California) and two low union density states (Georgia and Texas).

Appendix Table H.1: State-Year labor share of personal income as a function of union density (all years)

	Dep't var: Labor share of personal income					
	(1)	(2)	(3)	(4)	(5)	(6)
Household union share	3.704***	5.235***	6.189***	4.574***	4.311***	0.851
	[1.065]	[1.580]	[1.545]	[1.621]	[1.391]	[0.722]
Mean, dept. var.	82.73	82.73	82.73	82.73	82.73	82.73
R-squared	0.282	0.280	0.304	0.427	0.430	0.825
Education Control	Yes	Yes	Yes	Yes	Yes	Yes
Industry Shares	No	No	No	Yes	Yes	Yes
Income covars.	No	No	Yes	Yes	Yes	Yes
Policy covars.	No	No	No	No	Yes	Yes
Split-Sample IV	No	Yes	Yes	Yes	Yes	Yes
State-spec. quad.	No	No	No	No	No	Yes
Min. Year	1937	1937	1937	1937	1937	1937
Max. Year	2014	2014	2014	2014	2014	2014
Observations	3554	3554	3554	3554	3554	3554

Sources: Labor share of Personal Income constructed from BEA accounts as described in Appendix H text. For specification descriptions and other variables see notes to Table 3.

Appendix Table H.2: State-Year labor share as a function of union density (for 1963+, when we have GDP labor share)

	NNI		GDP (63+)		NNI (63+)	
	(1)	(2)	(3)	(4)	(5)	(6)
HH Union	0.0747***	0.0167**	0.0486	-0.00283	0.0523**	0.0184**
Density-CPS+Gallup (w) (IV 0)	[0.0201]	[0.00737]	[0.0380]	[0.0172]	[0.0248]	[0.00815]
CHSR Census+CPS	-0.0103	0.00267	-0.0180	-0.00392	-0.00363	0.00199
ipolate in efficiency units	[0.0141]	[0.00579]	[0.0148]	[0.00522]	[0.0117]	[0.00282]
Mean, dept. var.	0.761	0.761	0.563	0.563	0.749	0.749
Industry shares	No	Yes	No	Yes	No	Yes
State-spec. quad.	No	Yes	No	Yes	No	Yes
Income covars.	No	Yes	No	Yes	No	Yes
Policy covars.	No	Yes	No	Yes	No	Yes
Min. Year	1937	1937	1963	1963	1963	1963
Max. Year	2014	2014	2014	2014	2014	2014
Observations	3551	3551	2405	2405	2405	2405

Sources: Labor share of GDP from BEA. For specification descriptions and other variables see notes to Table 3.

Appendix I. Right-To-Work Analysis

In this appendix, we consider the potential for the enactment of Right-to-Work (RTW) laws to provide credible exogenous variation in state-level union density. Besides being of independent interest, RIW laws could provide a candidate instrumental variable for our central analyses in the post-1947 period. Unfortunately, the results of this section show that there is at best only weak evidence that RTW laws can serve this function. The great majority of RTW laws, passed in the 1940s and 1950s, were passed in states that, ex ante, had relatively low union density and did show unusual (off-trend) movements in union density subsequent to RTW enactment. This suggests that RTW laws are more an indicator of some combination of pre-existing effective employer and government resistance to unionization as well as lower demand by workers for union representation.

RTW laws are widely considered to be a policy designed to weaken unions by allowing workers to opt out of dues and agency fees. This sets up a classic free-rider problem since unions have the obligation to represent all workers in a bargaining unit, even those who do not pay to support the union. All wages and benefits negotiated by the union as well as other services provided by the union (e.g., handling of workplace grievances, eligibility for promotion, prioritization in the case of layoffs) are enjoyed by all workers, regardless of whether they pay to support the union. The first RTW laws were passed in the mid 1940s, but their legal status was ambiguous until the Taft-Hartley Act of 1947. The Taft-Hartley Act amended the National Labor Relations (Wagner) Act of 1935 and allowed states to enact RTW laws. Over the succeeding decade a substantial number of states enacted RTW laws, a small number of which were subsequently repealed.

Table I.1 contains a list of states that have enacted RTW laws, along with information on the year of enactment. While a handful of states passed RTW laws prior to the enactment of the Taft-Hartley Act in 1947, the act was followed by RTW passage in a substantial number of states (10 states in 1947-48 and another 8 states in the 1950s). These "early adopters" were largely states historically hostile to labor unions.[82] This is in contrast to some of the states with a history of strong labor unions that have adopted RTW laws more recently: Indiana (2012), a re-passage after 1965 repeal), Michigan (2013), West Virginia (2016), and Wisconsin (2016).

There is a substantial older literature based on the idea that right-to-work laws are a natural candidate as exogenous shifters of union density. In fact, this literature largely fails to find substantial negative effects of RTW laws on union density. An early study by Lumsden and Petersen (1975), using state level unionization levels in 1939, 1953, and 1968 confirms that states with RTW laws have lower union density but that changes in union density are unrelated to the presence of a RTW law or how long a RTW law has been in effect. They conclude that the laws have little substantive impact on union density. Using data

[82] Interestingly, 3 northeastern states (Delaware, Maine, and New Hampshire) adopted RTW laws very early (1947-48), but repealed the laws almost immediately. Additionally, Louisiana enacted an RTW law relatively early (1954) and repealed it soon after, before enacting RTW again in 1976.

Appendix Table I.1: State First Passage of Right-to-Work Laws

State	Year	State	Year
Arkansas	1944	Nevada	1951
Florida	1944	Alabama	1953
Nebraska	1946	Mississippi	1954
Arizona	1946	Louisiana	1954
South Dakota	1946	South Carolina	1954
Virginia	1947	Utah	1955
Iowa	1947	Indiana	1957
Tennessee	1947	Kansas	1958
Delaware	1947	Wyoming	1963
North Carolina	1947	Idaho	1985
Texas	1947	Oklahoma	2001
North Dakota	1947	Michigan	2013
New Hampshire	1947	West Virginia	2016
Georgia	1947	Wisconsin	2016
Maine	1948	Kentucky	2017

Note: We obtain the timing of RTW laws from Gall (1988), augmented by the National Right to Work website (`https://www.nrtw.org/`), which provides a comprehensive analysis of the political economy of RTW laws. Five states repealed their RTW laws: DE (1949), IN (1965), LA (1956), ME (1948), NH (1949). Two of these later passed another RTW law: IN (2012), LA (1976).

from 1977 Quality of Employment Survey, Farber (1984) estimates how RTW laws affect 1) worker demand for union representation and 2) the likelihood that a worker who desires union representation is employed on a union job.

He finds that most of the lower probability of unionization in RTW states is driven by lower demand for union representation among workers. Farber (2005) examines the effect of the introduction of RTW laws in Idaho (1985) and Oklahoma (2001). He notes that these states had relatively low union density at the time of RTW enactment and so enactment had at best a small effect on union density. These studies are representative of the earlier literature in that they find little causal impact of RTW laws on union density and that the presence of RTW laws (at least those in effect before 2010), reflects underlying conditions and attitudes unfavorable to unions.

More recently a number of traditionally high-union states have passed right-to-work laws, and some recent work studies the effect of these on economic and political outcomes. Fortin, Lemieux, and Lloyd (2018) use the passage of these laws to investigate the effect of unions on inequality, but they find, only a weak relationship between the introduction of RTW laws and union density even in these high-union-density states. Feigenbaum, Hertel-Fernandez,

Appendix Table I.2: Average State Union Density by When RTW Law Enacted

	Union Density		
RTW Enacted	1937-43	1945-49	2005-09
Never	0.165	0.281	0.188
1944-49	0.072	0.132	0.072
1950-65	0.119	0.169	0.096
2010-17	0.149	0.277	0.178
Overall	0.126	0.215	0.133

Note: Union Density for the indicated states and years based on our combined Gallup and CPS data sources. 41 states are included in the analysis. Not included are Alaska, Hawaii, Idaho (RTW in 1985), Oklahoma (RTW 2001), and the 5 states that repealed RTW laws. See Table I.1 and its note for details.

and Williamson (2018) study the political effects of RTW laws, but do not directly assess the effect of RTW laws on union density.

We directly address the causality question using our historical series on union density by state to verify that adoption of RTW laws in the 1940s and 1950s occurred in states with relatively low levels of unionization even before passage of their RTW laws. Given this fact, it is hard to argue that the adoption of right-to-work laws in these states were an important causal factor in the low union density in these states. Even if there is some causal effect on union density of those RTW laws passed since 2010, this does not help provide an appropriate instrument for union density over the 70+ year period covered by our study.

Table I.2 shows the average union density across states based on when they adopted RTW laws. The first column shows average union density in 1937-43, before any states adopted RTW, by when states adopted RTW. This clearly shows that the early adopters (12 states, 1944-48) were states with substantially lower union presence than states that never adopted RTW laws or adopted later. Union density was 7.2 percent in the period immediately preceding passage of the RTW laws in these early adopter states, compared with 16.5 percent union density in states that never passed RTW laws. In the 9 states that adopted RTW laws between 1951 and 1963, union density averaged 16.9 percent in the late 1940s (column 2) compared with 28.1 percent union density in states that never adopted RTW laws. Subsequent to passage Table I.2 shows that *changes in* union density within each adopting group of states mirrored national trends, increasing in the post-WWII period before declining. There does not appear to be a differentially larger long-run decline (or slower growth) in union density after RTW passage. This selective adoption of RTW laws and lack of differential movement in union density subsequent to RTW passage weakens their appeal as exogenous shifters of union density.

We now turn to a detailed analysis of movements in union density and union organizing activity, as measured by NLRB representation election activity, around the time of passage of state RTW laws. We update the approach in Ellwood and Fine (1987) and conduct event studies around transitions into Right-to-Work status. Ellwood and Fine (1987) examined the effect of Right to Work laws on NLRB elections rather than on union density directly, partly owing to paucity of data: as of their time of writing, there was no annual state-level data on the stock of union membership, something that our harmonized Gallup data remedies. Here we use both union density and NLRB election activity as outcomes. We find that there is no clear evidence of any effect of the enactment of RTW laws on either new union organizing through NLRB elections or on union density.

Specifically, we estimate a linear probability model of of the union status of household i in state s in year t of the form:

$$U_{ist} = \sum_{k=-5}^{10} \gamma^k RTW_{st+k} + X_i\beta^1 + X_{st}\beta^2 + \delta_s + \theta_t + \epsilon_{ist}, \tag{20}$$

where γ^{-1} is normalized to zero and standard errors clustered at the state level. While we include the last period coefficients in the regression, we don't plot them as they are difficult to interpret in an event-study regression. On the graphs, we report the average of the post-RTW dummies minus the average of the pre-RTW dummies, with and without controls. The controls are both individual and state-level. The individual covariates are age, white, and female, while the state-level covariates are share mining, share manufacturing, log of GDP per capita, share of tax units filing returns, minimum wage, policy liberalism, and state-specific quadratics.

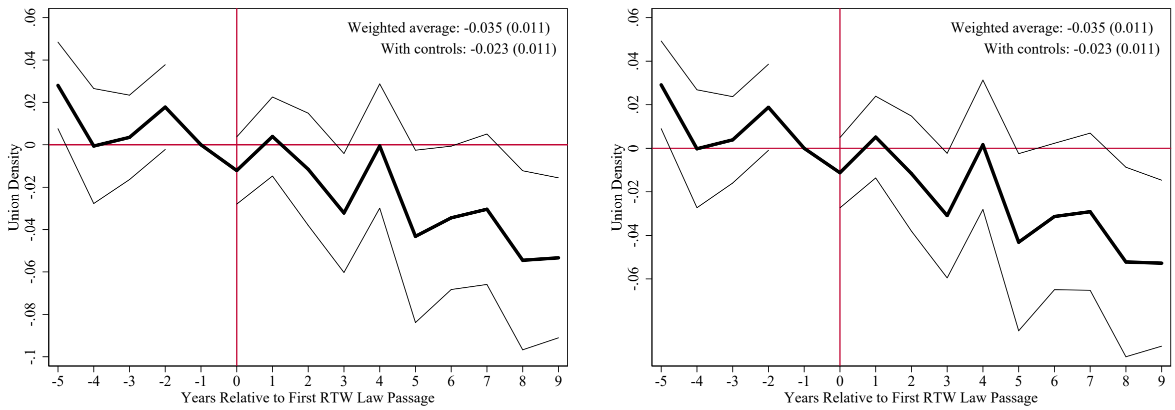
Using a sample that excludes the 5 states that repealed RTW laws at some point, Figure I.1 shows plots of the RTW timing estimates ($\hat{\gamma}^k$) relative to the year prior to RTW enactment, with confidence bounds, from specification 20.[83] The specification in panel (a) does not allow region-specific year effects (i.e. no South $\times$ Year Fixed Effects). Panel (a) of figure I.1 shows that while there appears to be a negative effect of RTW laws on union density, there also appears to be a significant pre-trend.

It is possible that this pre-trend is driven by the widespread adoption of RTW laws in the South either before or immediately after the Taft-Hartley act. The South had lower unionization before RTW, and less growth of unionization after, even as other regions were undergoing considerable increases in union density. We augment specification 20 by adding South X Year fixed effects, to allow for separate time-series movements by region. Panel B of figure I.1 shows the resulting estimates of the RTW timing effects ($\hat{\gamma}^k$), and the pre-trend

[83] The results are virtually unchanged if the five repealing states are included.

Appendix Figure I.1: Union Density-RTW Event Studies, Excluding States that Repealed RTW

(a) W/O South X Year Fixed Effects (b) W/ South X Year Fixed Effects



Effect of RTW laws on household union status measured using Gallup and CPS. The individual covariates are age, white, and female, while the state-level covariates are share mining, share manufacturing, log of GDP per capita, share of tax units filing returns, minimum wage, policy liberalism, and state-specific quadratics.

remains. That said, it may be that our data is simply too noisy to detect the effect, as we are unable to rule out 1-2 percentage point effects at 95% confidence. The 95% confidence interval for the weighted average estimate in Panel B of Figure I.1 is (-.023, .010), which contains the point estimate found at the industry-state-year level by Fortin, Lemieux, and Lloyd (2018).

Beyond what is shown, we have examined a variety of other specifications and samples, including various subsets of covariates, and redefining the pre-Taft Hartley laws to switch on in 1947. In no specification can we find a balanced pre-trend together with a substantial negative effect of RTW laws on union density.

Next we present some evidence on the effect of the enactment of RTW laws on the number of workers newly organized through NLRB elections. As noted by Ellwood and Fine (1987), among others, union density is a function of both the flow of newly organized members as well as the change in the stock of workers in already unionized firms. RTW laws may have an important effect on the ability of unions to organize as well as on the interest of unions in undertaking new organization efforts.

Using data at the state-year level from the NLRB on the number of workers in units where unions won elections (O_{st}), we estimate a specification of number of workers in units where unions won elections in state s in year t of the form:

$$O_{st} = \sum_{k=-5}^{10} \gamma^k RTW_{st+k} + X_{st}\beta + \delta_s + \theta_t + \epsilon_{st}, \tag{21}$$

where γ^{-1} is normalized to zero and standard errors clustered at the state level. As before, while we include the last period coefficients in the regression, we don't plot them as they are difficult to interpret in an event-study regression. On the graphs, we report the average of the post-RTW dummies minus the average of the pre-RTW dummies, with and without controls. The state-level covariates include share mining, share manufacturing, log of GDP per capita, share of tax units filing returns, minimum wage, policy liberalism, and state-specific quadratics. Complete state-year NLRB election data are not available until 1945, so we begin this analysis in 1950.

Analogously to Figure I.1, Figure I.2 shows plots of the RTW timing estimates ($\hat{\gamma}^k$) relative to the year prior to RTW enactment and confidence bounds from specification 21, using a sample that excludes the 5 states that repealed RTW laws at some point.[84] Figure I.2, shows that there is little detectable effect of RTW laws on new members organized through NLRB elections, with or without South X Year FE. This result is not consistent with Ellwood and Fine (1987) but is consistent with a large amount of work documenting little direct effect of RTW on union membership. For example, Farber (1984) shows that RTW does not even affect the stated willingness of workers to join unions.

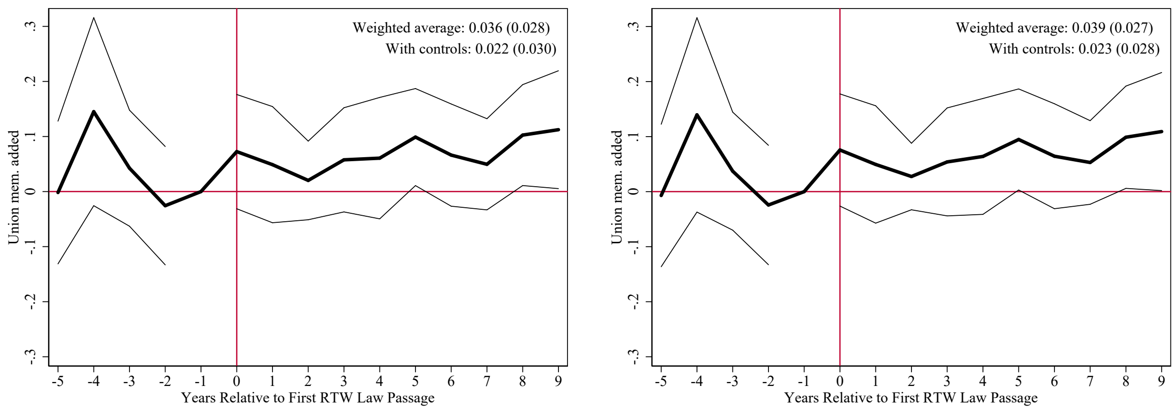
We also examined a variety of political outcomes in addition to union density and new organization. We did find small negative effects of RTW on share of Democrats in state legislatures and senates, with no pre-trend. This is consistent with Feigenbaum, Hertel-Fernandez, and Williamson (2018), who do find large effects of RTW on Democratic vote share (along with small effects on density), along with evidence that unions respond to RTW by reallocating resources from political mobilization towards maintenance of membership.

The weak and ambiguous results related to the effect of the introduction of RTW laws on both the stock of union members and the flow of new members is consistent with our analysis earlier in this appendix that highlighted the heavy negative selection in the passage of RTW laws. RTW laws, particularly those passed in the 1940s and 1950s, tend to be enacted where there is relatively less support for unions. While enactment of Right-To-Work laws may have some effect on unions that we are unable to detect, it is not a plausible candidate for an instrumental variable for union density in our data.

[84]The results are virtually unchanged if the five repealing states are included.

Appendix Figure I.2: New Members from NLRB Elections–RTW Event Studies, Excluding States that Repealed RTW

(a) W/O South X Year Controls (b) W/ South X Year Controls



Effect of RTW laws on new members added via NLRB elections. Data on NLRB elections from NLRB reports, beginning in 1950. The the state-level covariates include share mining, share manufacturing, log of GDP per capita, share of tax units filing returns, minimum wage, policy liberalism, and state-specific quadratics.

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