Unions and Inequality Over the Twentieth Century: New Evidence from Survey Data*

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Abstract

Despite a large literature on unions and inequality, virtually no representative microdata on union membership is available prior to the 1973 CPS. We bring a new source of data, opinion polls, primarily from Gallup ($N \approx 900,000$), to look at the effects of unions on inequality from 1936 to the present. First, we present a new time series of household union membership from this period. Second, we estimate union household income premiums over this same period, finding that despite large changes in union density, the premium holds steady, at roughly 15 log points. For most of this period, it is larger for non-whites and the less-educated. Third, we show that throughout this period, selection into unions with respect to predicted non-union wages was negative and $u$-shaped, with selection on predicted non-union wages reaching its most negative point in the 1950s and 1960s. Finally, we present a number of results that, across a variety of identifying assumptions, suggest unions have had a significant, equalizing effect on the income distribution over the twentieth century: unconditional-quantile regressions using repeated cross-sectional variation across households, time-series regressions using variation over time in national union density and panel regressions using variation over time within states all point to unions reducing income inequality.

JEL Classification Numbers:

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1 Introduction

The recent rise in inequality since the 1970s has coincided with a decline in union density, as can be seen in Figure 1. However, scholars debate the causal direction of this relationship and the quantitative importance of the decline of the labor movement in explaining the rise of inequality, relative to other forces such as technological change, trade, taxation, and education.

A substantial literature in labor economics and sociology emphasizes the decline of unions as an important contributor to rising wage inequality (Card 2001; DiNardo et al. 1996; Western and Rosenfeld 2011). Since Freeman and Medoff (1984), scholars have largely used the individual level data in the Current Population Survey. However, as Figure 1 shows, the period for which the CPS is available only begins in 1973. Before this time, almost all information on union density came from union reports on aggregate counts, which precludes answering such important questions as who joined unions and whether union members were better paid relative to what their demographics and human capital would predict. Moreover, from 1973 onward, union membership has exhibited monotonic decline, while inequality has been increasing monotonically, making it hard to disentangle any relationship between the two trends.

In this paper we bring a new source of 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 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 1937. We use these data to make four contributions to the literature on unions and in particular their effect on inequality.

Our first contribution is to document a new time series that tracks the share of households with at least one union member. The two most widely used time-series of union density from the pre-CPS period come from the Bureau of Labor Statistics (BLS) and Troy (1965), working under the auspices of the National Bureau of Economic Research. The BLS sought to use unions’ reports of their membership, whereas Troy sought to derive membership numbers from unions’ revenue estimates. Both series are aggregate time series and are thus not broken down by demographics or even geography.

Contemporaneous labor economists bemoaned lack of data. Troy (1965, pp) writes: “Accurate determination of the industrial affiliation of membership nationally and by state will require the addition of questions relating to union membership and representation in the industrial and business censuses and the decennial Population Census. Indeed, if this were done in the Population Census, cross-tabulations might be obtained on the characteristics of union members, such as occupation, sex, and age, as well as industry and location. If these steps were taken, a substantial improvement in the quality and coverage of union statistics would result.”
Generally, our series closely tracks both of these aggregate time series (and the CPS, once it becomes available), in changes if not always levels. The Gallup, BLS and Troy data show a large increase in union share during World War II. The share of households with a union member peaks in the mid 1950s in all three series. In the more modern era, the Gallup and CPS both decline monotonically in the period when we can observe both data sources. While we rely mostly on Gallup given its large sample size and the fact that it consistently asked about union status, when we are able to compare Gallup to other survey companies, the household union share measures are very close to each other.

Our second contribution is to make use of the fact that, unlike the BLS and Troy time-series data, these opinion surveys provide microdata and thus allow us to estimate household union income premia. Until the CPS, such an exercise (or the related exercise of estimating a wage premium using workers’ union status and individual earnings) was nearly impossible without very strong assumptions. For example, one of the few pre-CPS attempts was Lewis (1963), who used variation across industries in average wages and union density, concluding that more heavily unionized industries paid higher wages. An exception is the recent work by Callaway and Collins (2016), who find significant union wage premia, especially for those in the bottom of the wage distribution, using data from a survey of men living in Philadelphia, New Haven, Chicago, St. Paul, San Francisco, and Los Angeles in 1951.

We can combine our many sources of survey data to estimate a union household income premium in 1936, 1942, 1946, and then nearly every year from 1952 onward. Across these surveys, we can typically estimate this effect controlling for the age, education, race and gender of the respondent, the occupation of the household head, and state fixed effects. Remarkably, despite important changes to union density during this period, we find a relatively steady effect: union households have income roughly fifteen to twenty log points higher than do other households with similar demographics and human capital proxies. We can show that the household union premium is not driven by union households having more workers. While researchers have shown using the CPS that union workers are more likely to enjoy non-wage benefits (Buchmueller et al., 2004), we confirm in this earlier period that union families also appear to enjoy more non-wage benefits such as paid vacation.

Our third contribution also makes use of the micro-data, which allows us to determine who joined unions in the days of their rise and peak influence. We find robust evidence that union members are negatively selected with respect to proxies for estimated non-union wages, with this pattern exhibiting a u-shape, with peak negative selection occurring roughly in the 1950s and 1960s. We examine three proxies for non-union wages: education, occupational status and race. Selection into unions with respect to education shows a u-shape, reminiscent of the u-shape in inequality over the 20th century. In 1936, union households were only
slightly negative with respect to education, though selection becomes more negative overtime, peaking around 1960. After that point, negative selection begins to dissipate, and by 2000 in the CPS essentially goes to zero. As more evidence in support of this pattern of negative selection, we find a very similar u-shape for selection by occupation score.

Historians disagree on the degree to which unions discriminated against black workers during the pre-World War II and Great Compression period, with some arguing that they exacerbated white-black wage gaps and others arguing that, at the very least, they were less discriminatory than the non-covered labor market more generally during this period (Northrup 1971; Foner 1976; King Jr 1986; Katznelson 2013). We find evidence of exclusion before World War II. But the war appears to have a profound and lasting impact on black unionization. Non-whites in fact become more likely to be unionized (conditional on other demographics and state fixed effects) during the war, and their unionization advantage peaks around 1960. Since then, this advantage monotonically declines and today we find that white households are no less likely to be unionized than non-white households.

There are many ways by which unions might affect the income distribution. The most studied mechanism is via the union premium: so long as workers are negatively selected into unions with respect to predicted wages, the union premium results in condensing the wage distribution (Card 2001). Residual wage inequality also appears lower among union workers, suggesting that unions reduce inequality with respect to unobservable traits as well (Card 2001). Scholars have also argued that unions can affect the wages of non-union workers as well, in a positive direction via union “threat” effects (Farber 2005; Taschereau-Dumouchel 2015) or by the setting of wage fairness norms throughout an industry (Western and Rosenfeld 2011), or in a negative direction by creating surplus labor supply for uncovered firms (Lewis 1963). Unions might also affect the compensation of management (Pischke et al. 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. 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). Given these diverse mechanisms, the effect of union membership on the income distribution might be larger or smaller than that implied by micro-data analysis of the union wage premium and selection into unions.

Our fourth and final contribution explores this question, using three empirical methods and identifying assumptions to address the obvious challenge that neither individual union status nor trends in union density are exogenously determined. First, we use repeated cross-sectional variation in household union status by year to estimate the effect of changes in union status on the shape of the income distribution. We use recentered influence functions,
following [Firpo et al. (2009)], as the outcome in OLS regressions to estimate the unconditional quantile partial effect of unions in each annual cross-section of data, as well as the effect on the Gini coefficient. These estimates recover the effect of a marginal change in union density on inequality. We find that unions consistently increase family incomes at the bottom 10th percentile of the unconditional distribution, and do so much more than at the median, and generally have weak negative effect on the 90th percentile. In terms of overall inequality, a marginal change in union density reduces the Gini coefficient by roughly .075 in every year we have data from 1936 to 1970, a remarkably stable effect.

The implicit identifying assumptions in this exercise are that (a) conditional on our demographic and human capital controls, selection into unions is as good as random (b) the effect of unions on the distribution are fully captured by their effect on the household incomes of their members (i.e., that there are no spillover effects to non-union households).

Our second approach borrows from the skill-biased technological change literature, in particular [Katz et al. (1998) and Goldin and Katz (2009)], who use time-series regressions to show that the supply of skilled workers is a strong, negative predictor of the college-wage premium. We extend their analysis by using additional outcome variables (e.g., the 90/10 log wage ratio, the Gini coefficient, and the top-ten-percent income share) and adding annual union density as an explanatory variable. The identifying assumption in this analysis no longer rules out spillovers, but instead relies on variation in union density being as good as random conditional on other time-series controls. For each of these outcomes, union density emerges as a significant, negative predictor of income inequality, even when controlling for variation in the supply of skilled workers and other covariates (e.g., the minimum wage, the top marginal rate of the income tax schedule) posited to reduce inequality.

Finally, we extend the time-series analysis to state-year panel analysis, treating each state as a separate labor market that we can study over time. This part of the paper does not make use of the Gallup microdata per se, but makes use of the fact that the Gallup data are plentiful (we have over 900,000 observations that include the union measure) and always include state identifiers. We create state-year measures of household union density going back to 1937 and examine whether the inverse relationship depicted in Figure 1 holds at the state-year level. We find a significant, negative relationship between state-year union density and state-year top ten percent income shares. The result remains robust after controlling for standard covariates, including log skills shares at the state-year level and log state per capita GDP.

While each of three approaches involves strong identification assumptions, we take heart in the fact that the assumptions vary across applications and yet results all point to negative effects of unions on inequality.
Our paper relates to the large literature on the evolution of wage and income inequality over the twentieth and early twenty-first century (see Goldin and Margo (1992) for an early documentation of the “u-shape” over the twentieth century). No clear consensus has emerged on the main determinants of this pattern. Goldin and Katz (2009) acknowledge the role of institutions such as unions, but their analysis focuses on the supply and demand of skilled labor in explaining the time-series variation. Other scholars have emphasized the role of institutions (such as unions, regulation, social norms, and tax policy) over supply-demand factors. For example, Card and DiNardo (2002) argue that the supply-demand framework relies on inferring changes in demand, which are unobservable, and argues instead for a larger role for institutions. Hacker and Pierson (2011) emphasize changes in laws and regulation over supply-demand channels.

We also contribute to a venerable literature on the economics of trade unions. Interest in the effects of unions on workers goes back to Adam Smith’s musings about combinations of employers versus workers and John Stuart Mill’s claim that “trade unions...are the necessary instrumentality of [the] free market” (?, pp.XX), and many early American economists made the study of U.S. unions part of their work. The modern neoclassical approach to labor unions was pioneered by Lewis (1963), who focused on the union-nonunion differential. 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, while provides a survey of the union-nonunion differences over the twentieth century, generally relying on industry-level data. Recent identification of the effect of unions on firms and workers has been drastically improved by the use of discontinuities in union recognition elections in the United States (???), although see (?) for caveats on the discontinuity design. Virtually all the microdata-based literature on the union premium has used only post-1973 data, a limitation our data lets us overcome.

The rest of the paper is organized as follows. Section 2 discusses the existing historical time-series on union density. Section 3 introduces the Gallup data, and includes a detailed discussion on sampling and re-weighting. Section 4 presents our new time-series on household union membership. Section 5 estimates household union income premiums over much of the 20th century and Section 6 estimate selection into unions by education and race. Section ?? presents our state-year regressions and also explores potential mechanisms for why unions would condense the income distribution. Section 8 offers concluding thoughts and directions for future work.

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2They building on earlier work by Katz and Murphy (1992) and Katz et al. (1998) in developing the supply-demand framework using data from the 1940s through 1990s.

3For example, Richard Ely, John Commons, and Edward Seligman all made extensive qualitative use of union records in writing their early institutionalist economics.
2 Existing measures of union density pre-dating the Current Population Survey

The CPS first asks respondents their union status in 1973. 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 collects mandatory financial reports from unions as a condition of the Landrum-Griffin Act, and Troy and Sheflin (1985) incorporate them into their estimates of union density. Beginning in 1964, the BLS disaggregates union membership counts by state, and Hirsch et al. (2001) splice these reports together with the CPS to form state-year union density panel beginning in 1964 and continuing through today.

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.

2.1 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 1944 Monthly Labor Report says [pp. XX]: “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].”

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 no penalty for doing so. In the early and mid-1930s, the main umbrella

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, Troy and Sheflin, Wolman, and Galenson) in the Appendix.

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. Inverting this formula and using the convention records, rough estimates of union membership could be formed.
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.”

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, the modal article described the conflicts between the two federations.

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 report concluded that actual CIO membership was less than fifty percent of the official number the federation was reporting (Galenson, 1960).

### 2.2 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 member” and proceeds to estimate union membership using the financial reports

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6See, “Organized Labor is Put at 6,700,0000”, which cast considerable doubt on the AFL’s reports of membership, writing “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 [emph. added].”

7As 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 ALF head William Green and CIO head John Lewis, respectively.
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 7). 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. 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.

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. 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.

3 Background on Gallup Polls

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).

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8“Other” is down to 10% by 1960 in Troy (1965).

9Troy (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.

10From Troy (1965) [pp. A53]: "The average membership per local industrial union is arbitrarily estimated to be 300, end this figure is multiplied each year by the number of such unions reported by the CIO. 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.”
But 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.

Gallup was scrupulously non-partisan, never running polls on behalf of either party. AIPO also devoted considerable efforts to neutral, easy to understand question wording. A major coup for Gallup was his correct prediction of the 1936 presidential election outcome, whereas *Literary Digest* predicted Alf Landon as the winner (Franklin Roosevelt in fact beat Landon in a landslide). Following the election, Gallup, along with his fellow pollsters, quickly became household names. Gallup began surveying citizens on a wider range of social and political issues, beyond electoral races. By 1940, about eight million people had encountered Gallups tri-weekly polling report, *America Speaks!*. Gallup and other pollsters made money by selling their results to businesses for consumer research and newspapers for public opinion.

3.1 Gallup Sampling and Surveying Methodology before 1950

Gallup used “quota-based” sampling during this early period. 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 notes, “interviewers preferred to work in safer areas and tended to question approachable respondents,” which likely led to Gallup over-sampling, within each quote strata, more prosperous and well-off respondents.

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 husband’s opinions on Election Day.

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11Similar 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.

12Berinsky (2006) 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. From 1950 onward, Gallup uses modern-day probabilistic sampling procedures. Weights are often provided, but their documentation is not consistent. Throughout the paper, we will use weights we generate from the Census, detailed in Section ??.

3.2 Comparing Gallup to Census microdata

We begin with Gallup data from 1950 onward, returning momentarily to earlier data. Table 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 still exhibits some under-sampling of the South and in particular under-sampling of the blacks in the South, but by 1960 both of these biases have disappeared. It is obvious that Gallup is no longer only targeting voters, given that in 1960 the large majority of blacks in the South are still disenfranchised but Gallup nonetheless samples them according to their actual population. Throughout this period, blacks outside the South are sampled at a rate that roughly matches their population share. 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 ten percentage points in 1950. 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, not surprising as it was still under-sampling Southern blacks in that year. Later in the paper we will show results weighted and unweighted, but Table 1 gives some sense of how much “work” the weights must do.

Table 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 human capital. 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.

Regarding human capital proxies, Gallup respondents are slightly less educated than their 1940 Census counterparts. We strongly suspect, however, that this discrepancy may be noise due to the small number of survey respondents who were asked about their education in 1940, and in fact education shares for years in the mid and late 1940s look higher than those predicted from merely interpolating the 1940 and 1950 Census values (available upon request). 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. For example, “professionals” and “proprietors, managers, officials” appear more numerous in Gallup (these categories are especially useful because IPUMS categories fully nest these Gallup occupations). Reassuringly, farmers and farm laborers are similarly represented in both samples (these two Gallup categories are also fully nested in IPUMS categories, again easing comparisons across data sources).

For the most part, these patterns hold when we drop Southern states from both samples (the final two columns of Table 2). Importantly, Gallup appears to sample blacks in proportion to their population outside of the South, 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.

In general, we will show results with Gallup data using weights to match (interpolated) Census IPUMS summary statistics, even though the need for weights is not obvious after 1950 or 1960. From 1937 until 1941, we will weight so that Gallup matched the IPUMS in terms of White × 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 × Education × South, where Education ∈ {No high school degree, HS degree, Some college, College graduate}, thus giving us 2 × 4 × 2 = 16 cells on which to match. In practice, however, our results are very similar with and without weights.

\[^{13}\text{We use Gallup-defined geographic regions in this table.}\]
3.3 Additional checks on the Gallup data

While our focus is on union density, Gallup has also asked employment status since the 1930s. 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). The concordance is reassuring, given that a concern voiced by pollsters at the time was that the surveys were prone to sample the relatively well-off (both due to enumerator discretion and quotas tilted towards voters). We in fact see large shares of Gallup respondents during the so-called “Roosevelt recession” report that the household head is unemployed (note that levels are not directly comparable as Gallup and HSUS use different definitions of unemployment in the 1930s).

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.

3.4 Additional data sources

While we rely mostly on the Gallup data, we can supplement Gallup with a number of additional survey data sources from the 1930s through 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 often we will we augment our Gallup data with the ANES. We will also make use of CPS micro data from 1973 onward, which we do not describe in detail given its widespread use.

We were also able to find a handful of additional survey datasets that ask union status as well as the other variables we need to estimate a family income premium (i.e., education, family income, state of residence and basic demographics). The first is an expenditure survey that the BLS conducted in 1936. The second is a 1946 survey performed by the U.S. Psychological Corporation, followed in 1947 and 1950 by surveys from the National Opinion Research Corporation (NORC). Summary statistics for the CPS, ANES as well as these additional data sources appear in Appendix Table A.3. In general, at least along the dimensions Gallup appears most suspect in its early years (share residing in the South and share black), these data sources appear more representative. The table shows all data sources unweighted, though we will use ANES weights in years they are provided, to follow past literature. We do not weight the other additional surveys.

14The consumption survey asked union dues as an expenditure category, and thus can be used to measure household union membership.
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 19xx, “husband” changes to “spouse.” In some years, however, the question does not ask which member or members of the household is 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, 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. Many Gallup surveys also asks to which union federation (AFL, CIO, other) the respondent or spouse belongs, and we harmonize this information as well.

The first two series in Figure 2 show, respectively, unweighted and weighted household union membership in Gallup. Union membership appears mostly flat from 1937 through 1940. Recall that our weights can do very little “work” in these years as we do not have any surveys where education and union membership are asked together until 1942 and as such adjustments are made only for race and region. Union membership roughly doubles in both series from 1940 until the end of World War II in 1945. Union membership 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, union membership in the Gallup data slowly but steadily declines. Note that in most years, the weighted and unweighted series look very similar. In general, the biases in the Gallup data are likely to somewhat cancel out in terms of estimating union density: while the under-representation of the South (traditionally very hostile to unions) biases the Gallup measure upwards, its sampling of higher-status occupations biases it downward.

The next four series add our supplemental survey-based data. Note that each of these series generally has fewer observations per year than Gallup. The ANES sits very close to Gallup, though appears 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) sit somewhat higher than Gallup estimates for those years.

Next, we plot the widely-used historical data series described in Section 2, the BLS and the Troy series. Recall that 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).\footnote{Note that our dividing by U.S. households makes this time series slightly different than those readers may be more accustomed to seeing. For example, \footnote{instead uses civilian non-agricultural \textit{workers} as his denominator. If non-union workers exit the civilian labor force for military service in World War II, this would increase measured union density. Similarly, agricultural union members, while always a small share of union membership, are excluded from this traditional measure. Finally, the restriction to employed workers rather than the labor force imparts a cyclicality to union density that may make it difficult to perceive the secular trend.}} From 197x onward, we track the share of CPS households with at least one union member. Finally, we plot household union density based on CPS data from 197x to 2016. Reassuringly, in the years when Gallup and the CPS overlap, they are highly consistent.

However, the Gallup measures do not always agree with the BLS and Troy series in levels, though for the most part are consistent in changes.

4.1 Differences among the time series

There are multiple reasons why Gallup and the BLS/Troy series diverge prior to 1950, though it is heartening to see they agree much more in the 1950s, when both union reporting and Gallup sampling improve. As noted in the previous section, we have reason to believe that the Troy and BLS series over-stated membership. While unions had incentives to overstate membership, respondents themselves had no incentive to tell Gallup survey-takers they were union members when they were not, so this bias is unlikely to affect the Gallup numbers. Below, however, we focus on reasons that Gallup and other opinion surveys may \textit{under-state} union membership relative to these historical series and later the CPS.

First, there is a legitimate possibility that individuals are union members without knowing it, especially during certain historical moments, meaning union reports would accurately classify them as members but they would (truthfully) tell Gallup that they were not. During World War II, some unions default-enrolled all new workers and automatically collected dues from workers’ paychecks (workers would have to actively take steps to un-do this default process). Workers could thus be members of unions without knowing it. During the war, a period of rapid union growth, the National War Labor Board (NWLB) gave unions this privilege—default enrollment of new workers—in war-related plants, in exchange for a no-strike pledge \cite{Lichtenstein}. It is thus perhaps not surprising that the increase in membership during the war, while large in the Gallup data, is even larger in the union-membership-reports-based series.

Second, and related, is that 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 (who is no longer paying dues) might well consider himself no longer a
member (though, as already 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 unem-
ployed and retired respondents nonetheless identify as union members). A similar ambiguity
arises for retirees. Indeed, Figure 2 shows that the Gallup estimate diverges from the BLS
and Troy estimates the most in 1937–1939, the “Roosevelt recession” period. Gallup shows
essentially no growth during this period, whereas the BLS and Troy show robust growth.
Indeed, it is well documented that at least anecdotally dues payments plummeted for CIO
unions during this period, as millions of workers were laid off (Lichtenstein, 2003).

Third, as noted, Gallup over-samples the well-to-do, especially before 1950, which likely
biased union membership toward zero. But in fact, additional, offsetting sampling biases—
most notably the under-representation of the South, a region historically hostile to unions—
likely over-states density estimates. While we can never fully discount the possibility that
non-representative sampling is causing Gallup to understate density, given that we only find
a marginal increase in density after applying weights suggests it is not a major factor.

In summary, we are not surprised that the union density measures based on opinion
surveys differ slightly in levels from the more widely used measures in the literature, given
non-trivial differences in methodology. We are heartened that in almost all cases they firmly
agree in changes.

5 The union family income premium over the twentieth century

We now use the survey microdata to estimate union premia. As we did in creating our union
time-series figure, we draw from a variety of data sources. From Gallup, we can estimate a
premium in 1942 and then each year beginning in 1961. Gallup occasionally asked household
income before 1961, but only in 1942 do we have a survey that also includes the union
question and education. Our other major data source is the ANES. We can estimate a
household union income premium every year in the ANES beginning in 1952.

We supplement these surveys with any other survey we are able to find that asks union
membership, household income and education. These supplemental surveys are the 1936
Expenditure Survey and the 1946 U.S. Psychological Corporation survey.

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15 As seen in Figure 2, we can estimate union density in the ANES in 1948, but that survey does
not include household income.

16 In fact, the real constraint are surveys that ask both household income and union status. Almost
all surveys we have found that ask for household income also ask respondents’ education.
5.1 Estimating the union family income premium over time

Across all these surveys, we are able to estimate the following regression equation, separately by year $y$ and survey source (e.g., Gallup, ANES, CPS).

$$
\ln(y_{hst}) = \beta_{\text{Union}_h} + \gamma_1 FEMALE_{h}^{R} + \gamma_2 RACE_{h}^{R} + f(AGE_{h}^{R}) + g(Employed_{h}) + \lambda_{h}^{\text{eduR}} + \nu_t + \mu_s + \epsilon_{hst}.
$$

(1)

In the above equation, $y_{hst}$ is household income of household $h$ from survey date $t$ in state $s$; $\text{Union}_h$ is an indicator for whether anyone in the household is a 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 function controlling for the number of workers in the household; $\lambda_{h}^{\text{eduR}}$ is a function of fixed effects for the educational attainment of the respondent; and $\mu_s$ and $\nu_t$ are vectors of state and survey-date fixed effects, respectively.\textsuperscript{18}

Note that for the 1946 U.S. Psychological Corporation and for the Gallup surveys from 1961–197x, we cannot control for the number of workers per household, but in Section ?? we show this bias should be small. We also impose the condition to exclude any survey-year in which the top-coded family income category contains more than twenty percent of observations, though again we show robustness to including these observations.

Before showing how our estimates of $\beta$ vary across the twentieth century, we first show in Table 3 how the union income premium changes as controls are added. For this illustration we use data from the 1956-1958 ANES. These years include very detailed covariates, so we can see how our coefficient of interest changes when we add covariates beyond those we include in our standard specification, equation (1). It is also instructive to see how the union premium changes as sets of covariates are added.

Col. (1) shows the simple bivariate relationship between household union status and income. While positive and significant, it appears explained by union households being located in high-income states and is wiped out when state fixed effects are added in col. (2). Controlling for gender, race and age (cols. 3 and 4) have little effect, at least in this particular

\textsuperscript{18}Many of our surveys come from a single year, so a survey (and thus year) fixed effect $\nu_t$ is picked up by the constant term. For Gallup, as we have multiple surveys per year, we include survey fixed effects and estimate the Gallup coefficients separately by year. For ANES, we also estimate the coefficients separately by year and as there is only one survey per year there is no need to add survey fixed effects.
time period. Controlling for respondent’s education (col. 5) yields a positive and significant union effect: union households have income 13 log points higher than we would otherwise expect. Note that this result suggests some degree of assortative mating: that the respondent’s education (regardless of whether she is a household head) has so much power to explain household income suggests it proxies for the household head’s education as well. It is this specification (col. 5) that we will estimate for each of our surveys across time.

The ANES surveys tend to be more detailed than the rest of our surveys, so we can see what happens to the coefficient on Union household when we add covariates beyond those in equation (1). Like respondent’s education, controlling for the head’s occupation category (roughly 100 categories for these years in the ANES) substantially increases the estimate union premium, pushing it up to 19 log points. While we have occupation categories in most of our other data sources, they vary considerably, as as such we do not include occupation in our main estimation but will show results including it in the Appendix. Similarly, in the 1956-1958 ANES, we can also control for the household head’s industry, which we cannot in our other data sources. The point-estimate declines slightly (from 19.0 to 16.8 log points).

Figure A.4 shows results from estimating equation (1), or the col. (5) specification from Table 3, for each of our surveys (or, in the case of Gallup, separately by year). We show 95-percent confidence intervals calculated from standard errors that are clustered by state. Remarkably, the household income premium appears relatively stable from 1936 to 1986, at roughly 15 log points. Of the 17 point estimates, only three are greater than 0.20 and only two are less than 0.10. In no survey (or groups of surveyed aggregated to year) does confidence interval intersect zero. The ANES results from the early 1980s suggest that perhaps the union premium is larger during this period (roughly echoing the same pattern in the individual worker premium, as estimated in ?), though the standard errors are very large given the small ANES sample sizes.

5.2 Robustness

In Appendix Figure ??, we show results after controlling for occupation of the household head. As noted, occupation categories vary considerably across survey sources, which is why we relegate this figure to the Appendix. Not surprising, given the results from Table 3 that adding occupation controls increases somewhat the estimate union premium, the point estimates increase. With occupation controls, not a single point estimate falls below ten log points.

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19Note that respondent’s gender has little effect in our specification because, in contrast to wage regressions, here we are measuring effects on household income.
5.3 Household versus individual union premium

As noted earlier, most papers on union wage effects have used the CPS to estimate individual union earnings premia. The Gallup data does not split household income into individual earnings. Nor does it typically ask for the employment status of individual household members, so we cannot determine whether, say, part of the household union premium is simply due to union households having more members in the workforce.

The ANES does ask more granular information on employment. We can use it to estimate whether living with a union member predicts higher levels of own employment. If so, then the union household income premium we estimated earlier might be difficult to interpret as an improvement in wellbeing, given that it might be driven in part by decreased household leisure.

In Appendix Figure A.5 we show living with a union member decreases the likelihood you are employed. This effect somewhat attenuates over time, but remains negative throughout our sample period. Given the union household premium we estimated earlier, our results suggest that, at least in our sample period, a positive income effect reduces the likelihood of a second earner entering the labor force. As such, the household income premia we estimate earlier might in fact understate the effect of unions on household wellbeing, because union households would appear to enjoy more leisure.

5.4 Non-income measures of compensation

Our measure of family income does not include other, possibly important dimensions of compensation (whether pecuniary or not). For example, in the more modern era, research has found that union workers enjoy better non-wage benefits (Buchmueller et al., 2004). Unfortunately, Gallup and are other sources do not consistently ask about benefits such as health insurance. One except is from a 1949 Gallup survey that asked about paid vacation. As we show in Appendix Table ??, Gallup respondents are significantly more likely (twenty percentage points, with respect to a baseline average of XXX) to say that they or their husband received paid vacation as a benefit. While not significant, the union household vacation benefit is substantially larger for non-whites and those with less education and is significantly larger for the low-status occupation of laborers.

We have another Gallup survey from 1939 that asks respondents how easily they could find a job as good as their current one. As we show in Appendix Table ??, union households are far more likely to say it would be hard for them to find a job just as good. We interpret this as a proxy for worker surplus—union workers appear less likely to be bargained down toward their reservation conditions. While workers may simply be considering wages when
thinking about whether a job is “as good as” another, to the extent they consider a wider set of job characteristics (safety, working conditions, benefits, etc.) this result is an additional piece of evidence that union members felt their jobs were better—in a broad sense—than non-union members.

5.5 Heterogeneous treatment 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 race (whites versus non-whites) and estimated years of education. As we are now effectively comparing union and non-union households across different cuts of the data, standard errors are larger and thus it is harder to conclude much from a single survey. Nonetheless, the overall trends aggregate across all of our surveys suggest that union premiums were larger for non-whites and the less-educated.

Figure 4 shows that throughout the period, less-educated household enjoyed a larger union family income premium. In fact, over the six 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 results by race show a somewhat different pattern (Figure 5). In the late 1930s and early 1940s, the point estimates from the union family income premium regressions suggest an advantage for whites. During this period, however, with low union density and even lower union density among blacks, these point estimates, unsurprisingly, have very large standard errors, given that our data is also sparse during this period. However, by the 1950s, blacks in fact enjoy a larger union premium.

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 black or less educated) are those most benefited (in terms of family income) by having a household member in a union. Ignoring this differential effect would tend to underestimate the effect of unions on inequality, an issue we return to in Section 7.

6 Who joined unions during their heyday?

While the analysis in Section 5 shows that union households enjoyed greater family income than their education, demographics or occupation would predict, the effect on the shape of the wage distribution is ambiguous. If union members are better off than other workers in the latent distribution of non-union wages, then a substantial union wage premium would increase wage dispersion. In fact, some contemporary scholars argued unions increased wage
inequality, as it pulled the wages of the covered sector away from the non-covered sector. We now analyze who joined unions over our sample period.

While we resorted to non-Gallup surveys in the previous section—as Gallup does not ask income between 1942 and 1960, we would otherwise not be able to study the premium during this period—in this section we do not need income data and thus to facilitate comparisons across different time periods we focus on Gallup.

6.1 Selection by education

Not until 1942 does Gallup ask both educational attainment and household union status in the same survey, so we begin our analysis in that year. We estimate selection in the following specification, separately by year $y$:

$$
Union_{ht} = \beta E_{ht} + \gamma_1 Female_{ht} + \gamma_2 age_{ht} + \gamma_3 (age_{ht})^2 + \mu_h + \nu_t + e_{ht}.
$$

$E_{ht}$ is a single measure of the respondent’s education (we show results varying this measure, using a dummy for high school completion, a dummy for college completion, and estimated years of schooling). All other notation follows that in equation (1). The vector of estimated $\beta$ values tells us, in each year, how own education predicts whether you or your spouse is in a union, conditional on basic demographics and state of residence. Note here that we are not controlling for occupational categories or race.

Figure 6 shows these results for our three education measures. For all three measures, a $u$-shape emerges, though most weakly for the high school dummy measure. In 1955 (roughly the trough of the $u$ in all three series) a high school degree reduced the probability of living in a union household by roughly 15 percentage points, a college degree by over 25 percentage points, and each additional year of schooling by roughly 3.5 percentage points. Appendix Figure ?? shows that the $u$-shape is confirmed when we add in our other data sources (the 1936 Expenditure Survey, the 1946 U.S. Psych Corp survey, ANES, CPS, etc.).

6.2 Selection by occupation

Gallup occupational categories in 1937 and 1938 are substantially more coarse than in later years, so we begin this analysis in 1939. For this analysis, we do not include the CPS, since differences in occupational categories between the two data sources make comparisons difficult.

We begin by showing how a broad “labor” occupation predicts living in a union house-
During our sample period, laborer have family income 16 log points lower than other occupations (conditional on state and year fixed effects), so it is a low-status occupational category. As Figure 7 shows, having a laborer occupation strongly predicts living in a union household over the entire sample period, but especially in the 1950s. The shape of the relationship over time is a distinct, inverse-u. In the late 1950s, laborers were over 35 percentage points more likely to be in a union than other occupations, up from just under twenty percent in 1940. By the end of our sample period in 1986, laborers are barely ten percent more likely to be unionized than other occupations.

We also calculate a rough non-union occupation score. Occupation score displays a clear u-shape over our sample period and, not surprisingly given that laborers is a low-status occupation, is the rough inverse of the Laborer coefficient pattern. In particular, both series show that union households exhibited rapid occupational upgrading from 1975 onward.

6.3 Selection by race

Finally, we examine selection into unions by race. To generate the first series in Figure 8, we follow equation (2) and regress our household union dummy on the interactions between White and year, gender, age (and its square), state and survey fixed effects. Interestingly, race is not a strong predictor of household union status over our sample period: the white coefficients are positive but not statistically distinguishable from zero in the pre-World War II period and for the most part are in fact otherwise negative, though small. Again, a u-shape emerges. Beginning in the 1970s, the non-white advantage with respect to union membership begins to decline, thou

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20 While Gallup uses slightly different occupation categories from year to year, in each year, we can create a “laborers” category, in some cases just grouping skilled and unskilled labor together and in other cases also grouping a “semi-skilled” category in the surveys when Gallup offers it. This aggregate category does not seem very affected when other categories are added.

21 We follow these steps to create occupation score. First, we regress log income (in the years we have it) on occupation fixed effects and state and year and year squared for non-union households. We include year and year squared instead of year fixed effects so that we can project this prediction onto all Gallup years after 1938 (instead of merely the years where we have the income variable). We project this prediction on all households with valid occupation, state and year values and then use it to predict household union status as in the other analysis in this subsection. We emphasize that this procedure is “rough” in that occupational scores do occasionally change from survey to survey.

22 We do not replicate this occupation-category-based figure with our other data sources, given that occupation categories vary widely by data source.

23 Results are essentially exactly the inverse when instead of White we use a black dummy. We use White instead because sometimes Gallup will use “negro” and sometimes “non-white” and thus White would appear, in principle, a more stable marker, though in practice it makes no difference.
Appendix Figure ?? replicates this analysis but drops the South, given that Gallup significantly under-sampled blacks in the region at least through 1950 and its correction of this bias over time might make comparisons of union selection by race across time difficult. However, the results are very similar.

7 The effect of unions on the income distribution [incomplete]

In Section 5, we showed that union households enjoy family income roughly fifteen to twenty log points higher than their demographics, state of residence, education and occupation would predict. We then showed, in fact, that for most of our sample period, this premium has always been larger for the less educated and, by 1950, for minorities as well. Moreover, in Section 6 we showed that it is exactly these groups that have, at least since World War II, been over-represented within unions.

These pieces of evidence would suggest that unions were a powerful force pushing to lower income inequality during the heyday of the labor movement. In this section, we quantify the effect of unions on the income distribution in three ways. First, we simulate counterfactual income distributions in the CPS from 1973 onward assuming the 1950s union environment—both in terms of the the level of union density and selection into unions. As noted in the introduction, however, unions may have larger or smaller effects on income variance than that predicted by this exercise, depending on which general equilibrium effects prevail. To that end, we directly estimate the effect of state-year union density on state-year measures of inequality, treating each state as a separate labor market.

7.1 Unions and inequality: Cross-sectional variation in household union status

In this section we present within-year effects of union membership on inequality using recentered influence functions (RIF) as in Firpo et al. (2009). In our case, the RIF is a transformation of the dependent variable that allows the coefficient on the household union dummy to recover the effect of union density on some aggregate statistic of the family income distribution. We present effects of union density on the 10th, 50th, and 90th percentiles (denoted $Q_{10}, Q_{50},$ and $Q_{90}$), as well as on the Gini.

The virtue of RIF regressions is that they provide a tractable method to estimate the marginal effect of a variable on a statistic of the whole distribution. Let the joint distribution of family income $y$ and covariates $X$ be $F(y, X)$, and let the union density be given by $p \in [0, 1]$, so that $\mathbb{E}(union) = p$, where union is our usual household union dummy. Then we can
decompose the family income distribution into the union = 1 and union = 0 distributions so that

\[ F(y, X) = p \cdot F(y, X | \text{union} = 1) + (1 - p) \cdot F(y, X | \text{union} = 0). \]  \hspace{1cm} (3)

Consider some distributional statistic \( \nu(F) \), such as the \( \tau \)th percentile, \( Q_\tau = F^{-1}(\frac{\tau}{100}) \) or the Gini coefficient \( \text{Gini}(F) \). We can write the effect of a small change in union density on this statistic as \( \frac{d\nu(F)}{dp} \). [Firpo et al. (2009)] show that the value of this derivative is obtained from a regression of the RIF of the statistic \( \nu \) on the independent variable of interest. As for the union premium results, we run this estimation separately by survey source and year \( y \), using a specification parallel to equation (1) but with the RIF of a given inequality statistic \( \nu \) instead of family income as the dependent variable:

\[
\text{RIF}(y_{\text{hst}}, \nu(F)) = \beta \text{Union}_h + \gamma_1 \text{Female}_h + \gamma_2 \text{Race}_h + f(\text{age}_h) + g(\text{Employed}_h) + \lambda_{\text{edu}} + \nu_t + \mu_s + \epsilon_{\text{hst}}.
\]  \hspace{1cm} (4)

In our setting, the RIF of a given statistic is, roughly speaking, the derivative of \( \nu \) as the distribution in expression (3) is slightly perturbed toward the union = 1 distribution, an expression that must be derived for each statistic of interest.\( ^{24} \)

Figure ?? shows the effect of moving the family income distribution toward that of union families on the 90th, 50th, and 10th percentile of family income. While estimates fluctuate over time, the effect of unions is large and positive on the 10th percentile, and negative or zero for the 90th percentile, with the effect on the median falling in between. The implied effect on the 90-10 ratio varies from -0.4 to -0.2.

We conduct a parallel analysis for the Gini coefficient, which summarizes changes in inequality coming from all parts of the distribution. Figure ?? shows the effect of union density on the Gini coefficient in each year.

\( ^{24} \)For \( Q_\tau \), this expression is given by \( (Q_\tau + \frac{\tau}{Q_\tau}) - \frac{1(y_n < Q_\tau)}{f_y(Q_\tau)} \). Roughly, the intuition is XXXX. The estimate for \( \beta \) in equation (4) is therefore the effect of a change in union density on the probability that a household’s income is less than the value of the quantile \( \tau \), i.e., \( \frac{dF(Q_\tau)}{d\text{Union}} \), divided by the density of household income at \( Q_\tau \) (\( \frac{d\nu(Q_\tau)}{d\text{Union}} \)). The resulting coefficient thus measures \( \frac{d\nu(Q_\tau)}{d\text{Union}} \) the marginal change in the value of the quantile at \( \tau \) in response to a small change in \( \text{Union} \). The RIF of the Gini is not particularly illustrative and we omit it here.
7.2 Unions and inequality: Time-series variation in national union density

While the distributional regressions capture the effect of union density on inequality, they require a strong assumption that there are no spillovers, threat effects, or political economy mechanisms that alter wages for non-union workers. Given the plausibility of these more macro mechanisms, an aggregate analysis is warranted, complementing the individual household regressions estimated above.

We begin our aggregate analysis of the effect of unions on inequality by replicating specifications from the time-series literature on the college wage premium, for example the analysis in [Goldin and Katz (2009)], which spans the whole 20th century. Following [Katz and Murphy (1992) (and 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 (2009) show that the evolution of the college premium between 1915 and 2005 is well-explained by the relative supply of college workers, controlling for flexible functions of time. Confirm this analysis using data from the CPS in the 1963-2005 period, adding covariates, and argue that the non-market factors stressed by Card and DiNardo (2002), Lee (1999), and Lemieux (2006) have limited explanatory power in explaining the rise of inequality, measured as the 90-50 or 50-10 ratios. However, they do not consider unions as a potential non-market factor in their analysis.

We begin by estimating the Katz-Murphy specification extended to the whole 1939-2012 period. We augment this specification by including union density as measured in our Gallup surveys, estimating:

$$\log(\frac{\text{wage}_{Col}^t}{\text{wage}_{HS}^t}) = -\frac{1}{\sigma} \log \frac{N_{Col}^t}{N_{HS}^t} + \gamma \text{UnionDensity}_t + \sum_{k=1}^{3} \beta_k t^k + \epsilon_t. \quad (5)$$

In this specification, $\sigma$ yields an estimate of the elasticity of substitution between college and high-school. The cubic polynomial in time captures changes in relative demand, although it could also be unmeasured institutional factors driving the college premium. Our primary coefficient of interest is $\gamma$.

Because Gallup union density may be mismeasured due to sampling biases, we instrument Gallup union density with the BLS measure of union density. While both contain errors, they are likely to be orthogonal: unions misreporting membership (the source of BLS errors) is not likely to be correlated with Gallup’s changes in sampling patterns and with sampling noise in the years that Gallup did not frequently ask the union question. Hence, the IV combines

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25In particular, we use the richest specification in ?, which is col. (6) of Table 2, albeit applied to a slightly longer time period.
information from both measures, effectively upweighting the years where both measures are close to each other.

Finally we include a set of covariates, which we take from key papers in the determinants-of-inequality literature. Specifically, following Autor et al. (2008) we include the real value of the federal minimum wage and the civilian unemployment rate and following ? we include the top marginal tax rate in the federal individual income tax schedule.

The first two columns of Table 4 shows the results. We present the coefficient $\gamma$ only in this tables (all coefficients are shown in the Appendix). Col. (1) largely replicates Goldin and Katz (2009), estimated on our slightly different sample period (1939 to 2012 instead of 1915 to 2005), but including our Gallup measure of annual union density, which yields a negative and significant coefficient. While not reported to save space, the implied elasticity of substitution between college and high-school workers is 1.43, quite close to the findings in the literature, suggesting that union density is largely independent of skill-shares. In col. (2), we instrument the Gallup measure with the BLS measure, and include additional covariates, which increases the coefficient magnitude considerably, consistent with the presence of measurement error in the union density measure.

While the canonical analysis in Goldin and Katz (2009) and related work focuses on the college premium, we extend our analysis by using the same specification but using other measures of inequality as outcomes. Columns 3-4 of Table 4 are identical to Columns 1-2 except that the 90/10 log wage ratio is used as the outcome variable. The results are quite similar (with both skill shares (not shown) and union density exerting a negative effect on the 90/10), although the effect of unions appears more robust than the effect of skill shares. Appendix Table ?? shows this remains true across a variety of specifications.

Columns 5-8 of Table 4 extends this analysis to inequality measures constructed from administrative data (rather than surveys). These have the advantage of being available annually, instead of just every ten years in the pre-CPS era. These additional years not only give us more observations, but also allow us to use variation from between Census years in the estimation. Columns 5-6 use the Gini coefficient constructed by ? from Social Security data, while columns 7-8 use the top-ten-percent income share from ?, with the small difference in that we can extend the series to 2012 as these data have been more recently updated than the Gini.

A small complication is that our estimates of the skill shares come from survey data, and thus in principle are only available every ten years in the early period. To circumvent this issue, we include two separate education controls: skill shares are measured (annually) in our Gallup data and an annual measure of skill shares equal to that from the CPS when it is available and interpolating between Census years in the earlier period (we only report
the latter variable in the table, as “Relative college supply, interpolated,” though note this measure includes the annual CPS measures once they become available). In this case, 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. Appendix Tables ??? and ?? show results using only the non-interpolated years, as well as results omitting the Gallup controls.

Columns 5-6 show that the effects of union density on the Gini are large and robust. The point estimates suggest that the XX percentage point decline in union density between 1975 and 2004 predicts a 0.058 to 0.066 increase in the Gini. The actual rise in the Gini over this time period is XX. Again, the instrumented measure of union density is roughly double the OLS estimate.

Columns 7-8 repeat the analysis with the Top ten percent share of income. The same twenty percentage-point decline in density between xx and xx would predict a 14.0 to 19.3 percentage-point increase in the top ten share. The actual rise over the time period is XX.

While there are clear limitations to the time-series analysis (e.g. no exogenous variation in union density, and small samples with auto-correlated errors make inference suspect), all specifications in this section control for a quartic in time, ensuring that the effect of unions is not simply mirroring the u-shape present in many time-series over the 20th century. We include the quartic to follow the literature, but we show in Appendix Table ?? that our results are robust to using lower-order polynomials in time.

Overall, the annual time-series analysis suggest that skill shares perform better than union density in explaining the college wage premium. However, across a broader range of inequality outcomes, unions tend to exert a more stable and significant negative effect than do skills shares. We take these results as suggesting that past work may have overlooked the powerful role of union density in explaining the shape of inequality time-series.

7.3 Unions and inequality: Variation in union density over time within state

While the time-series analysis generates summary accounts of the aggregate effect of unions on the US economy, a major limitation is that there are many unobserved factors (e.g. macro-policy, trade, outsourcing, industry structure) that are likely correlated with both inequality and union density that are not absorbed by our controls. In this section we replicate the analysis at the state-year level, controlling for state and year fixed effects that will 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 1937-1986 period, something that was not possible with previous data.\footnote{\citet{Troy(1965)} presents state breakdowns for 1939 and 1956, and \citet{?} use BLS reports to construct state-year measures of density from 1964 onwards.} One limitation of our survey-based data is that small states get small samples, resulting in noisy estimates for annual variation. We both winsorized measures as well as a split-sample instrumental variables strategy to mitigate this problem. Although we do not have an identification strategy, we can see if the inverse inequality-density relationship that holds in the aggregate time series hold at the state-year level, conditional on year and state fixed effects.\footnote{Similar regressions estimated at the cross-country level by \citet{Jaumotte and Osorio-Buitron (IMF 2015)}.}

While the census allows geographically disaggregated measures of inequality, in order to exploit the higher-frequency nature of our independent variable, we use recently constructed top income shares from \footnote{\citet{Alvaredo et al (2016)}}, downloaded from the World Wealth and Income Database. This data is calculated using internal IRS data, but is not adjusted for capital gains. \footnote{?} show that when the same methodology is applied to national US data the results are still quite close to \citet{Piketty and Saez (2003)}.

We combine our Gallup state-year measures with household state year measures calculated from the CPS. We regress the Gallup measures on the CPS measures for the sample in which they overlap, and then predict the Gallup measure from the coefficient on the regression for the years in which we only have the CPS measure. This results in a panel of state-year union density measures. We are quite highly correlated (correlation = .724) with the existing Hirsch-Macpherson measures (individual union density as a fraction of non-farm employment) for the post-1964 years, which are where there is overlap. However, our sample extends further back, to 1937, where there has only been the Troy estimates of union density in 1939 and 1953, which we are also highly correlated with in the cross-sections and changes (1939 correlation = XX, 1953 correlation = XX, correlation in changes =XX).

To examine the effect of unions on inequality, we estimate specifications of the form:

$$y^p_{st} = \beta \text{UnionShare}_{st} + \gamma X_{st} + \mu_t + \delta_s + \epsilon_{st}$$

(6)

where $y^p_{st}$ is the percent of total income accruing to the $p^{th}$ percentile and above in state $s$, year $t$. We consider consider $p = 1$ and $p = 10$, that is top one and top ten percent shares. We include year and state fixed effects in all regressions, $\mu_t$ and $\delta_s$, respectively, and $X_{st}$ is a vector of state-year controls that we vary to probe robustness. We cluster the standard errors at state level.

As mentioned above, 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. For every 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 strategy should reduce attenuation bias in our OLS estimates.

Table 5 shows results from the specification in 6. Column 1 estimates a simple bivariate regression following 6, with just state and year fixed effects. We show the analogous IV specification in Column 2, which shows that indeed, the split-sample IV results in coefficients are roughly 50% larger than the OLS coefficients. While this does not solve any omitted variables problems, it does remove the influence of measurement error, a problem given some of the small samples in state-year observations.

Column 3 is our preferred specification, and includes South X year fixed effects as well as “income controls”, which are annual log GDP per capita as well as the share of tax units filing returns. These controls proxy for the overall level of economic activity as well as the extent of taxation. Given that our outcome variable is derived from tax data the robustness of our coefficient to these controls is reassuring. Figure ?? shows the basic pattern in the data that underly our col. (3) coefficient. The results seem not to be driven by outliers or limited to a particular part of the support of the union density variable. The basic magnitudes of the OLS specification can also be read off this graph as well: moving from to 25% union density to 10% reduces top 10% share by 1.5 points, yielding a coefficient around one.

Column 4 includes industry controls, captured here as industry employment shares calculated at the 1-digit level from IPUMS, and interpolated across census years. While our coefficient falls somewhat, it remains strongly significant. Column 5 includes region X year fixed effects, limiting the variation to within the 7 regions coded in Gallup. Again, our coefficient falls somewhat, but remains quite strongly significant. Finally, column 6 is our most demanding specification, and includes state-specific linear and quadratic trends, as well as South X year fixed effects, income controls, and industry controls. The negative correlation of unions and top income shares remains negative and significant even when we allow each state to be on a separate “U”-shaped latent pattern of inequality.

While our effects are robust and significant, they are not large. In terms of interpreting these effects, the union decline from 1980-2015 = 11 percentage points. Our preferred coefficient (column 3 from table 5) implies roughly a 1 percentage point increase in top 10% share. The actual increase in top 10% share from 1980-2015 = 15 percentage points, so we explain over 7% of the increase. Of course, without an identification strategy, it is difficult to know what the true effect is, and it may be larger or smaller than the one implied by our correlational analysis in this section.
8 Conclusions

To be added.
References


Figure 1: Union density estimates and “top share” inequality measures, 1917-2011

Figure 2: Union density over the twentieth century: Comparing our survey-based measures to existing time-series

Data sources: See discussion in Section 2 and 3.

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).
Figure 3: Estimates of the union family income premium

Data source: See Section 3 for a description of each data source.

Notes: Each plotted point comes from estimating equation (1), which regressed log family income on controls for age, gender, race, state and survey-date fixed effects. Occupation controls are not included. We estimate a separate regression for each survey source and year. The plotted confidence intervals are based on standard errors clustered by state.
Figure 4: Differential family union premium by respondent’s years of schooling

Data source: See Section 3 for a description of each data source.

Notes: Each plotted point comes from estimating equation regressing family income on household union status, its interaction with respondents’ year of schooling, and all other controls in equation (1). We estimate this equation separately by survey source and by year. The figure plots the coefficient on the interaction Years of schooling × Union. The plotted confidence intervals are based on standard errors clustered by state.
Figure 5: Differential family union premium for whites relative to minorities

Data source: See Section 3 for a description of each data source.
Notes: Each plotted point comes from estimating equation regressing family income on household union status, its interaction with a White dummy variable, and all other controls in equation (1). We estimate this equation separately by survey source and by year. The figure plots the coefficient on the interaction White $\times$ Union. The plotted confidence intervals are based on standard errors clustered by state.
Figure 6: How does educational attainment predict union household status?


Notes: We regress household union status on state $s$ and survey $t$ fixed effects (which absorb year $y$ fixed effects), age and its square, gender, as well as $\sum_{y(t)} Education_h \times Year(t)$, where Education is proxied by high school completion in the first series, by college completion in the second series, and by (estimated) years of schooling in the third series. Note that each series is based on a separate regression. “Years of schooling” is based on the Gallup education categories 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.”
Figure 7: How does household head occupation predict union household status?

Notes: “Laborer” is defined as anyone who reports “skilled,” “semi-skilled” or “unskilled” labor as head’s occupation. Occupation score is based on the regression: 
\[ \log(\text{income})_{\text{HH}} = \lambda_{\text{OccHH}} + \gamma_1 \text{Year}_t + \gamma_2 \text{Year}_t^2 + \mu_s + e_{\text{HH}} \] for non-union households. We then project the estimated coefficients onto all (union and non-union) households. Note that we include continuous measures of year so as to be able to include years in which Gallup surveys did not ask income. The plotted coefficients are generated by regressing household union status on state \( s \) and survey \( t \) fixed effects (which absorb year \( y \) fixed effects), age and its square, gender, as well as \( \sum_{y(t)} \text{Occupation}_h \times \text{Year}(t) \), where in the first series \( \text{Occupation} \) is proxied by the laborer dummy variable and in the second by our estimated occupation score.
Figure 8: How does race predict union household status?


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. Confidence intervals are based on standard errors clustered by state (we suppress them for the Gallup and CPS surveys to avoid clutter).
Figure 9: Effects of Union Density on Family Income Quantiles

Data source: See Section 3 for a description of each data source.

Notes: Each plotted point comes from estimating equation (4), with regressed the recentered influence function for the specified quantile on controls for age, gender, race, state and survey-date fixed effects. Occupation controls are not included. The plotted confidence intervals are robust to heteroskedasticity.
Figure 10: Effects of Union Density on Family Income Gini

Data source: See Section 3 for a description of each data source.

Notes: Each plotted point comes from estimating equation (4), with regressed the recentered influence function for the Gini coefficient on controls for age, gender, race, state and survey-date fixed effects. Occupation controls are not included. The plotted confidence intervals are robust to heteroskedasticity.
Table 1: Comparing Gallup and IPUMS, 1950–1980

<table>
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Observ. 296223 25328 1081562 22145 2444218 29095 1494469 31473

Sources: Gallup surveys and 1950–1980 IPUMS. “South” refers to all eleven states of the former Confederacy plus Oklahoma. All Census results use IPUMS person weights.
Table 2: Comparing Gallup and IPUMS in 1940

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<th>Gallup</th>
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Sources: Gallup surveys and 1940 IPUMS. 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). All Census results use IPUMS person weights.
### Table 3: Estimating family union income premium, ANES 1956–1958

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<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
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<td>0.0251</td>
<td>0.0193</td>
<td>0.00683</td>
<td>0.129***</td>
<td>0.190***</td>
<td>0.168***</td>
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<td>[0.0446]</td>
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<td>[0.0281]</td>
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<td>Yes</td>
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</tr>
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<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<td>No</td>
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<td>Yes</td>
</tr>
<tr>
<td>Age (H)</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>2621</td>
<td>2621</td>
<td>2621</td>
<td>2621</td>
<td>2621</td>
<td>2595</td>
<td>2595</td>
</tr>
</tbody>
</table>

Sources: ANES surveys from 1956 and 1958.

### Table 4: Union density and Inequality: Aggregate time-series regressions

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Coll. premium</th>
<th>90/10 ratio</th>
<th>Gini coeff.</th>
<th>Top 10 share</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Union density</td>
<td>-0.984***</td>
<td>-1.394***</td>
<td>-1.083***</td>
<td>-1.607***</td>
</tr>
<tr>
<td>[0.344]</td>
<td>[0.395]</td>
<td>[0.401]</td>
<td>[0.567]</td>
<td>[0.0447]</td>
</tr>
<tr>
<td>Mean, dept. var</td>
<td>0.559</td>
<td>0.559</td>
<td>1.462</td>
<td>1.462</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.980</td>
<td>0.982</td>
<td>0.957</td>
<td>0.967</td>
</tr>
<tr>
<td>Gallup edu. control?</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Addit. controls?</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>BLS IV?</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Cubic polynomial?</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>38</td>
<td>38</td>
<td>53</td>
<td>53</td>
</tr>
</tbody>
</table>

Sources: For cols. (1) and (2)
Table 5: Inequality and Union Density 1936-2011

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>HH Union</td>
<td>-5.145 *</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Density-CPS+Gallup (u/w)</td>
<td>[2.586]</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>HH Union</td>
<td>-7.072</td>
<td>-10.34 **</td>
<td>-9.665 ***</td>
<td>-5.681 ***</td>
<td>-6.110 ***</td>
<td></td>
</tr>
<tr>
<td>Density-CPS+Gallup (u/w) (IV 0)</td>
<td>[4.331]</td>
<td>[4.239]</td>
<td>[3.612]</td>
<td>[2.870]</td>
<td>[1.949]</td>
<td></td>
</tr>
<tr>
<td>Share Emp. Mfg.</td>
<td>717.7 ***</td>
<td>508.0 **</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[248.0]</td>
<td>[243.1]</td>
<td>[26.89]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SouthxYr FE?</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>RegxYr FE?</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Inc. Controls</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State-Sp. Quadratics</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Wins. RHS?</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Ind. Controls</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>First-Stage F</td>
<td>324.6</td>
<td>300.6</td>
<td>279.83</td>
<td>194.12</td>
<td>170.87</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Union Density measure is winsorized at 99% thresholds. All regs have state and year fixed effects, SEs clustered by state. *p < 0.1, **p < 0.05, ***p < 0.01
Appendix A. Supplementary figures and tables noted in the text
Appendix Figure A.1: Gallup Unemployment Rate

Data sources: Gallup and Historical Statistics of the United States
Appendix Figure A.2: Age distribution in Gallup, by gender, 1937-1952

Data sources: Gallup microdata.
Appendix Figure A.3: Share CIO

Data sources: Gallup and Historical Statistics of the United States
Appendix Figure A.4: Estimates of the union family income premium (including occupation controls when available)

Data source: See Section 3 for a description of each data source.
Notes: Each plotted point comes from estimating equation (1), which regressed log family income on controls for age, gender, race, state and survey-date fixed effects and (in most cases) fixed effects for the occupation of the head. The plotted confidence intervals are based on standard errors clustered by state.
Appendix Figure A.5: Own employment as a function of someone else in the household being in a union

Data sources: ANES.
Notes: Each point plotted in this graph is the coefficient from regressing “am employed” on a dummy variable for “someone else in the household is in a union.” This variable is coded as one if (a) the respondent herself is not in a union but someone else is; (b) the respondent and someone else is in a union. It is coded as zero if (a) she is the only person in the household in a union; (b) no one in the household is in a union. This regression pools all years and interacts “someone else is in a union” with year dummy variables. It includes controls for education categories, age and age squared, race, gender, year and state fixed effects. Individuals age 65 and older are excluded. Confidence intervals reflect standard errors clustered by state.
Appendix Figure A.6: Probability of being married as a function of individual union status

Data sources: ANES.

Notes: Each point plotted in this graph is the coefficient from regressing “am married” on a dummy variable for the individual being in a union. This regression pools all years and interacts “in a union” with year dummy variables. It includes controls for education categories, age and age squared, race, gender, year and state fixed effects. Individuals age 65 and older are excluded. Confidence intervals reflect standard errors clustered by state.
Appendix Figure A.7: Age distribution in Gallup, by gender, 1937-1952

Data sources: Gallup microdata.
### Appendix Table A.1: Summary statistics from supplementary data sets

<table>
<thead>
<tr>
<th>(1) ANES</th>
<th>(2) BLS exp. survey</th>
<th>(3) U.S. Psych. Corp.</th>
<th>(4) NORC</th>
</tr>
</thead>
<tbody>
<tr>
<td>Union household</td>
<td>0.254</td>
<td>0.141</td>
<td>0.250</td>
</tr>
<tr>
<td>Female</td>
<td>0.548</td>
<td>0.0346</td>
<td>0.502</td>
</tr>
<tr>
<td>White</td>
<td>0.858</td>
<td>0.920</td>
<td>0.908</td>
</tr>
<tr>
<td>Age</td>
<td>39.67</td>
<td>40.85</td>
<td>3.387</td>
</tr>
<tr>
<td>HS graduate</td>
<td>0.360</td>
<td>0.405</td>
<td>0.375</td>
</tr>
<tr>
<td>South</td>
<td>0.277</td>
<td>0.232</td>
<td></td>
</tr>
<tr>
<td>Log fam. inc.</td>
<td>9.380</td>
<td>7.121</td>
<td>7.823</td>
</tr>
</tbody>
</table>

Observations: 30757 4058 1267 1267

Notes: Union Density measure is winsorized at 99% thresholds. All regs have state and year fixed effects, SEs clustered by state. \( ^* p < 0.1, ^{**} p < 0.05, ^{***} p < 0.01 \)

### Appendix Table A.2: Paid vacation as a function of union status (Gallup, 1949)

<table>
<thead>
<tr>
<th>(1) Dep’t var: Do you (or husband) get paid vacation?</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Union household</td>
<td>0.220***</td>
<td>0.183***</td>
<td>0.288**</td>
<td>0.280*</td>
</tr>
<tr>
<td></td>
<td>[0.0332]</td>
<td>[0.0308]</td>
<td>[0.126]</td>
<td>[0.143]</td>
</tr>
<tr>
<td>White x Union household</td>
<td>-0.111</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.127]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Years educ. x Union household</td>
<td>-0.00964</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.0130]</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Low-skill labor x Union</td>
<td>0.149***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.0493]</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

State FE? Yes Yes Yes Yes Yes
Demographic controls? Yes Yes Yes Yes Yes
Occupation FE? No Yes Yes Yes Yes
Observations 1895 1864 1864 1864 1864

Notes: Data from a Gallup survey in May 1949. The dependent variable is a dummy. Demographic controls include respondent’s age and its 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 in the final column refer to the Gallup categories of “unskilled and semi-skilled labor.” Standard errors are in brackets and clustered by state. \( ^* p < 0.1, ^{**} p < 0.05, ^{***} p < 0.01 \)
Appendix Table A.3: Paid vacation as a function of union status (Gallup, 1949)

<table>
<thead>
<tr>
<th>Would be easy to find another job just as good</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Union household</td>
<td>-0.122***</td>
<td>-0.123***</td>
<td>-0.0951***</td>
<td>-0.1000***</td>
</tr>
<tr>
<td></td>
<td>[0.0278]</td>
<td>[0.0256]</td>
<td>[0.0288]</td>
<td>[0.0298]</td>
</tr>
<tr>
<td>Mean, dept. var.</td>
<td>0.499</td>
<td>0.499</td>
<td>0.499</td>
<td>0.495</td>
</tr>
<tr>
<td>State FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Demogr. controls</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Educ. controls</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Occup. controls</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Ex. South</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>1952</td>
<td>1952</td>
<td>1952</td>
<td>1686</td>
</tr>
</tbody>
</table>

Notes: Data from a Gallup survey in May 1949. The dependent variable is a dummy. Demographic controls include respondent’s age and its 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 in the final column refer to the Gallup categories of “unskilled and semi-skilled labor.” Standard errors are in brackets and clustered by state. *p < 0.1, **p < 0.05, ***p < 0.01