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

Henry Farber, Dan Herbst, Ilyana Kuziemko, Suresh Naidu

December 4, 2017

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. The variance of residual incomes is also more compressed in the union than the non-union sector throughout our sample period. Third, we show that throughout this period, selection into unions with respect to proxies for predicted non-union wages (e.g., education, race, occupational status) was negative and u-shaped, with selection 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 our long sample period: 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:
Key words: Unions, Inequality, Economic History.

*All remaining errors are our own. We thank our research assistants Obaid Haque, Chitra Marti, Brendan Moore, Tamsin Kantor, Amy Wickett and especially Fabiola Alba, Divyansh Devnani, Elisa Jacome, Amitis Oskoui, and Maryam Rostoum. We have benefited from comments by seminar participants at Columbia, Harvard, INSEAD, the NBER Development of the American Economy meetings, McGill University, Princeton, Rutgers, Sciences Po, Universitat Pompeu Fabra, and Vanderbilt. Farber: Princeton University and NBER, `farber@princeton.edu`. Herbst: Princeton University, `dherbst@princeton.edu`. Kuziemko: Princeton University and NBER, `kuziemko@princeton.edu`. Naidu: Columbia University and NBER, `sn2430@columbia.edu`.

1 Introduction

The rise in U.S. income 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.[1] 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 (NBER). 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.

[1] 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 give virtually identical measures of the union share of households in the years we can observe both data sources. While we rely mostly on Gallup given its large sample size and the fact that it consistently asks 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 1951 survey of men living in Philadelphia, New Haven, Chicago, St. Paul, San Francisco, and Los Angeles.

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. The premium tends to be larger for blacks and the less-educated. 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. Similarly, work from the CPS era shows that *residual* variance in individual earnings is lower in the union sector (Card, 2001), a result we replicate for household income in the historical period.

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 in most of the post-war period whites are in fact less likely to be unionized than non-whites (conditional on other demographics and state and year fixed effects). Like education, this pattern exhibits a u-shape, with the white "disadvantage" peaking in the early 1960s. Since then, minorities' union advantage monotonically declines and today we find that non-white households are no more likely to be unionized than 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). As already noted, residual wage inequality also appears lower among union workers, suggesting that unions reduce inequality with respect to unobservable traits as well. 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 corresponding sets of 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 tenth percentile of the unconditional distribution, do so somewhat more than at the median, and generally have weak negative effect on the ninetieth percentile.

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 and Murphy (1992), Autor *et al.* (2008) 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 male wage ratio, the individual 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 conditional on year and state fixed effects. We find a significant, negative relationship between state-year union density and state-year measures of inequality. The result remains robust after controlling extensively for standard covariates, including log skills shares at the state-year level, industry employment shares, and log state per capita GDP.

While each of three approaches involves strong identification assumptions, we take heart in the fact that the assumptions and data sources vary across applications and yet results all point to negative effects of unions on inequality. For our summary measure of inequality, the Gini coefficient, results are also generally consistent with each other. The distributional regressions at the individual level imply that a 12 percentage point increase in union density (the decline between 1970 and 2004) reduces the household Gini coefficient by roughly 0.9 (out of 100) in every year we have data from 1936 to 1970, a remarkably stable effect, and roughly 13% of the 7 point increase in the household income Gini between 1970 and 2004. The time-series results imply, for example, that a 12 percentage point increase in national union density would imply a 1.2 to 3.2 point reduction in the individual Gini coefficient, between 10 and 30 percent of the 9 point increase. And the coefficients from the state-year regressions imply that the same 12 percentage point increase in union density leads to a 1.2 to 0.6 point reduction in the individual Gini coefficient, between 5 and 10 percent of the total increase.

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 concensus 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.[2] 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) argues that the supply-demand framework relies on inferring changes in demand, which are unobservable, and argues instead for a larger role for institutions. In political science, Hacker and Pierson (2011) emphasizes changes in laws and regulation over supply-demand channels in explaining inequality, and Ahlquist (2017) provides a recent survey on the effects of unions on inequality, emphasizing both economic and political channels.

We also contribute to a venerable literature on the economics of trade unions. Economists' interest in the effects of unions on workers goes back to Adam Smith, who noted asymmetries between combinations of employers versus workers, and John Stuart Mill's claim that in the presence of imperfections "trade unions...are the necessary instrumentality of [the] free market" Mill (2008) [p. 319 (original publication year 1848)]. 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

[2] They build 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.

determinants of union membership and the union premium with individual level data, while Lewis (1986) provides a survey of the union-nonunion differences over the twentieth century, generally relying on industry-level data for the pre-CPS period. Recent identification of the effect of unions on firms and workers has used discontinuities in union recognition elections in the United States (DiNardo and Lee, 2004; Lee and Mas, 2012; Frandsen, 2013).[3] 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 7.2 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 7 presents our evidence on the effect of unions on the shape of the income distribution. Section 8 offers concluding thoughts and directions for future work.

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.[4]

[3] Although see Frandsen (2017), who cautions against validity of this IV by showing that unions disproportionately lose (win) "close elections" in years when Republicans (Democrats) have a majority on the National Labor Relations Board.

[4] 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.

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 1945 Monthly Labor Report notes as its sources: "This study is based on an analysis of approximately 15,000 employer-union agreements as well as employment, union membership, and *other data available to the Bureau of Labor Statistics* [emphasis ours]" (Bureau of Labor Statistics, 1945) [5]

It is obviously hard to verify information from unspecified "sources available to the BLS" but even in instances where the BLS can rely on union membership reports, concerns arise. A key issue is that unions had important incentives to over-state their membership and until the late 1950s faced no penalty for doing so. In the early and mid-1930s, the main umbrella organization for local unions was the American Federation of Labor (AFL). They were often charged with over-stating their membership, presumably to inflate their political influence. For example, a 1934 *New York Times* story casts doubt on the AFL's claim to represent over six million workers, noting that "complete and authoritative data are lacking" and that the figures provided by the AFL "are not regarded as accurate."[6] 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

[5]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.

[6]See, *"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]."

of government officials, tasks that were aided by a public perception that the federation was large and growing. Based on our read of *New York Times* articles on unions in the late 1930s and early 1940s, one of the most common if not the most common topic is the conflict between the two federations.[7] Individual unions still had incentives to compete for influence within their given federation, and thus inflate membership.

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

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* 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.[8] 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.[9]

In addition, Troy imputes the membership of many CIO unions in the late 1930s and

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

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

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

1940s by assigning them the membership of their *AFL counterpart in the same sector*.[10] 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).

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.[11]

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.

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

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

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 Berinsky (2006) 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.[12]

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.

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 the next 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

[12] Berinsky (2006) provides great detail on Gallup's quota-based sampling procedures, from which we draw much of the information in this subsection.

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, outside of the South, Gallup appears to sample blacks in proportion to their population, even in the very early years of its existence. Also, outside the South, Gallup appears to accurately sample the remaining six regions of the US.[13]

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 $White \times South$ cells, given that the summary statistics show that Gallup sampling along these dimensions appears suspect in the early years. Beginning in 1942 (the first year in which Gallup surveys ask the union and education questions in the same survey) we weight by $White \times Education \times South$, where $Education \in$ {No high school degree, HS degree, Some college, College graduate}, thus giving us $2 \times 4 \times 2 = 16$ cells on which to match. In practice, however, our results are very similar with and without weights.

3.3 Additional checks on pre-1950 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.

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

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. The existing CPS-based measures of union density and union premia are all based on individual workers. We use CPS micro data from 1977 onward to produce *household* union density and union premia estimates comparable to the those derived from Gallup, which we describe in Appendix A.3. Note that we end the Gallup series in 1986 to have roughy ten years of overlap with the CPS to check consistency.

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.[14] The second is a 1946 survey performed by the U.S. Psychological Corporation. In 1947 and 1950 we use data from National Opinion Research Corporation (NORC) as a check on our union density estimates from Gallup, but as these data do not have state identifiers, we do not use them in micro-data regression analysis. Summary statistics for the CPS, ANES as well as these additional data sources appear in Appendix Table A.1. 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.

4 A New Time-series Measure of 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 1959, "husband" changes to "husband/wife" 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

[14] The consumption survey asked union dues as an expenditure category, and thus can be used to measure household union membership.

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.

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. Finally, from 1976 onward, we plot the share of CPS households with at least one union member. Reassuringly, in the years when Gallup and the CPS overlap, they are highly consistent. Appendix Figure A.3 shows the CPS-Gallup data zoomed in on the period of their overlap.

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).[15]

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. One noteworthy difference is in

[15]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, Freeman *et al.* (1998) instead uses civilian non-agricultural *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.

patterns of AFL vs CIO composition, shown in Appendix Figure A.4. While both the BLS and Troy series show relative share of CIO union membership declining in the 1937-1946 period, Gallup shows a relative increase. The magnitude and duration of the post-Wagner act surge in CIO unionization rates is a source of persistent disagreement among scholars of the New Deal Skocpol *et al.* (1990); Goldfield (1989), and we do not pursue this further in this paper.

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 *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, but potentially inaccurately) 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 (Lichtenstein, 2003). 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 unemployed and retired respondents in these surveys 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 (though we drop any year in which the top-coded category contains more than 30 percent of observations). 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 in the ANES in 1952, and then every other year after 1956.[16]

We supplement these surveys with any other survey we are able to find that asks union membership, household income and education.[17] These supplemental surveys are the 1936 Expenditure Survey and the 1946 U.S. Psychological Corporation survey.[18]

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

[17] 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.

[18] The 1936 Expenditure Survey is the only one in which income is not binned. To make it comparable to our other data sources, we winsorize at 95% and 5% percent and then create 15 equally sized income bins.

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 and survey source (e.g., Gallup, ANES, CPS):

$$\begin{aligned} ln(y_{hst}) =& \beta Union_h + \gamma_1 Female_h^R + \gamma_2 Race_h^R + f(age_h^R) + \\ & g(Employed_h) + \lambda_h^{eduR} + \nu_t + \mu_s + e_{hst}. \end{aligned} \tag{1}$$

While we are estimating a *household income* function, we do our best to mimic classic Mincerian controls. In the above equation, y_{hst} is household income of household h from survey date t in state s; $Union_h$ is an indicator for whether anyone in the household is a 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; λ_h^{educR} is a vector of fixed effects for the educational attainment of the respondent; and μ_s and ν_t are vectors of state and survey-date fixed effects, respectively.[19] Note that for the 1946 U.S. Psychological Corporation and for the Gallup surveys from 1961 onward, we cannot control for the number of workers per household, but below we show that this bias should be small.

Figure 3 shows results from estimating equation (1) for each of our surveys (or, in the case of Gallup where we have many surveys per year, pooled 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, for Gallup, groups of surveyed aggregated to year) does the confidence interval intersect zero. To our eye, the family union premium appears quite stable over this nine-decade period, echoing the finding in Card (2001) that the union wage premium was surprisingly stable between 1973 and 1993, even as private-sector union density declined by 50%. 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

[19] Many of our surveys come from a single year, so a survey (and thus year) fixed effect ν_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.

worker premium, as estimated in Blanchflower and Bryson (2002)), though the standard errors are very large given the small ANES sample sizes.

5.2 Robustness

While we do not focus on them, Appendix Table A.2 shows the coefficients on the Mincer equation covariates in equation (1). For ANES (since it covers a long time period), we split the sample in two, so that coefficients can be compared across time. In all cases, the coefficients on the covariates have the same signs as we typically see from an individual earnings regression.

In Appendix Figure A.5, 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. As we show in the next section, union households typically have household heads with lower-status occupations than non-union households, so we would expect controlling for occupation to increase the estimated union premia. Indeed, the Appendix Figure confirms this expectation, depicting coefficients that are somewhat larger than in the main Figure 3. With occupation controls, not a single point estimate falls below ten log points.

As noted earlier, we cannot control for the employment status of household members in the Gallup and the Psychological Corporation data. Appendix Figure A.6 shows that any bias is likely very small: in the ANES, *not* controlling for employment status increases the estimated union premium only slightly, relative to the baseline results where these controls are included. Union households are more likely to have at least one person employed (likely the union member himself), which explains why controlling for household employment has a (slight) negative effect on the estimated union household premia. However, *living with a union member* is a negative predictor of own employment (see Appendix A.7), which might help explain why the correction is relatively small.

5.3 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 A.3, Gallup respondents are significantly more likely (twenty percentage points, with respect to a baseline average of 0.52) 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 A.4, union households are far more likely to say it would be hard for them to find a job just as good. We interpret this variable 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.

5.4 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 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 nine decades of our sample period, this differential effect appears relatively stable. For each additional year of education, the household union premium declines by roughly four log points.

The 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 very early period, however, with low union density and (as we show in the next section) even lower union density among non-whites, these point estimates, unsurprisingly, have very large standard errors, given that our data is also sparse during this period. However, by the 1950s, non-whites in fact enjoy a significantly larger union premium. In the most recent years of the CPS, however, the additional premium for non-whites is still present, but no longer statistically significant.

Our conclusion from the heterogeneity analysis is that, at least for most of our sample period, disadvantaged households (i.e., those with respondents who are non-white or less educated) are those most benefited (in terms of family income) by having a household member in a union. Ignoring this differential effect would tend to underestimate the effect of unions on inequality.

5.5 Effects on residual income dispersion

An important characteristic of unions is that, by attaching wages to jobs instead of to workers, they lower the influence of both observed skill, as documented above, but also unobserved skill on wages. A well-documented resulting fact is that the union wage distribution is compressed even after conditioning on observable measures of human capital (e.g., Freeman and Medoff, 1984 and Card, 2001).

We implement an analogous analysis at the household level. Separately for union and non-union households, we regress log family income on all the covariates (except union) in equation 1. As before, we perform this analysis separately by survey-source/year. We then calculate residuals for each sector and compute the ratio of variances between the union and non-union residuals (which has an F-distribution with degrees of freedom given by the two sample sizes). If unions compress the distribution of unobserved skill, then this ratio should be less than 1.

Figure 6 shows, separately by survey-source/year, the ratio of variance of residual log family income between the union and non-union sector, together with 95% confidence intervals. The ratio is uniformly below 1, and often below 0.5, with confidence intervals that always exclude equality of the variances. While there does not seem to be a strong pattern over time in the union-nonunion difference in residual income inequality, it does appear that the CPS-era pattern of unions compressing residual inequality in fact holds throughout the post-1936 period.

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.

6.1 Selection by education

We estimate selection into unions via the following specification, separately by survey-source/year:

$$Union_{hst} = \beta Educ_h^R + \gamma_1 Female_h^R + \gamma_2 age_h^R + \gamma_3 (age_h^R)^2 + \mu_s + \nu_t + e_{hst}. \tag{2}$$

$Educ_{hst}^R$ is a single measure of the respondent's education (e.g., years of schooling, a high school dummy). All other notation follows that in equation (1). The vector of estimated β 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 (though we return to selection on education conditional on race momentarily).

Figure 7 shows these results across our key datasets. A marked u-shape emerges. The pattern is remarkably consistent across all data sources. Appendix Figures A.10 and A.11 show similar patterns when a high-school dummy or college dummy serve as the education variables, though selection on high school graduation flattens out during the CPS era.

At the nadir of the u (around the mid 1960s), the coefficients suggest that an additional year of education reduced the likelihood of living in a union household by roughly 3.5 percentage points. The Appendix Figures suggest similar magnitudes: a high school degree (relative to not having one) reduced union household membership by roughly fifteen percentages points during this period, and a college degree by roughly twenty-five percentage points.

6.2 Selection by race

We next examine selection into unions by race, via identical analysis as in equation (2) except that a dummy variable for $White$ replaces $Educ_h^R$.[20] Again, a u-shape emerges. In the beginning of our sample periods, whites are (conditional on our covariates) more likely to be in union households than non-whites. This advantage disappears and in fact reverses roughly during World War II and continues to grow more negative until about the 1960s. Since then, whites gain on non-white households until today there is essentially no differential.

While not quite as consistent as for education, selection by race again agrees for the most part across data sources. There is some disagreement between the ANES and Gallup in the 1950s and 1960s, though the large standard errors around the ANES estimates certainly

[20]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.

don't allow us to reject equality. There is some disagreement between the Gallup and CPS, whereby the Gallup shows miniminal negative selection with respect to education by the early 1980s, whereas CPS shows that whites are still somewhat less likely to live in union households. However, by the end of the sample period, there is no remaining selection by race in the CPS either.

Appendix Figure A.9 replicates this analysis, but excludes 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. In fact, the results are very similar.

A natural question is whether the u-shapes with respect to years of education and race are simply picking up the same variation. Table 3 explores whether they can be separately identified. To be conservative, we use only Gallup data in these regressions, to ensure that none of the u-shape is driven by differences across data sources. We specify the u shape as a simple quadratic function of the *year* variable. Col. (1) shows that the u shape is indeed significant with respect to race, and that significance endures even after we drop observations with missing *education* values (col. 2), and control flexibly for education (col. 3). Similarly, the u shape in years of educaiton is also significant after controlling for *white* (col. 4). When we include both the *white* and *education* u-shapes simultaneously, we can no longer identify the *white* u-shape at conventional levels of significance (p-values on both the linear and quadratic terms are roughly 0.19) but the significant u shape for years of education remain.

The final five columns of this table repeat the analysis after dropping the South, given the issues with Gallup's sampling of blacks in the region until the 1950s. For this sample, both u shapes can be identified simultaneously. We take the evidence from this table as suggesting that unions in the 1950s and 1960s were indeed drawing from parts of the distribution that would otherwise have low wages—less educated and non-white workers—and that these results are based on at last somewhat independent variation.

6.3 Selection by occupation

Finally, we examine selection by occupational status. We view these results more cautiously—given the substantial differences in the way our survey sources code occupation, we do not feel comfortable combining data sources so for this exercise consider only Gallup. But even restricting ourselves to Gallup does not fully address the issue of changing occupational categories, as Gallup's categories change modestly over time. As such, we view these results more as confirmation of the education and race results that unions were indeed drawing from the most negatively-selected parts of the income distribution during the 1950s and 1960s.

Gallup occupational categories in 1937 and 1938 are substantially more coarse than in later years, so we begin this analysis in 1939. We start by showing how a broad "labor" occupation predicts living in a union household.[21] During our sample period, laborer have family income 16 log points lower then other occupations (conditional on state and year fixed effects), so it is indeed a low-status occupational category. As Figure 9 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.[22] 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.

7 The effect of unions on the income distribution

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. We also showed that residual variance in lower in the union sector, suggesting that unions also lower the premium to unobserved attributes as well. Moreover, in Section 6 we showed that was disadvantaged households—non-white and less-educated—who were most likely to have a union member during most of our sample period, especially in the two decades after World War II.

[21]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.

[22]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.

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 10^{th}, 50^{th}, and 90^{th} 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, under the assumption of no spillovers, we can decompose the family income distribution into the $union = 1$ and $union = 0$ distributions so that

$$F(y, X) = p \cdot F(y, X \mid union = 1) + (1 - p) \cdot F(y, X \mid union = 0). \tag{3}$$

Note that the assumption of no spillovers is incorporated into this expression by making the union and non-union joint distribution of income and characteristics independent of the share of the population that is unionized.

Consider some distributional statistic $\nu(F)$, such as the τ^{th} percentile, $Q_\tau = F^{-1}(\frac{\tau}{100})$ or the Gini coefficient $Gini(F)$. 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 ν on the independent variable of interest. As with 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 ν instead of family income as the dependent variable:

$$\begin{aligned}RIF(y_{hst}, \nu(F)) =& \beta Union_h + \gamma_1 Female_h^R + \gamma_2 Race_h^R + f(age_h^R) + \\ & g(Employed_h) + \lambda_h^{eduR} + \nu_t + \mu_s + e_{hst}.\end{aligned} \tag{4}$$

In our setting, the RIF of a given statistic is, roughly speaking, the derivative of ν 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.[23]

Figure 10 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, though, relative to the 90/10 ratio, it is especially sensitive to changes in the middle part of the distribution. Figure 11 shows the effect of union density on the Gini coefficient in each year. For the bulk of the sample period, the effect is quite stable, roughly -0.075, and falling as we approach the present.

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 adapting regressions from the time-series literature on the college wage premium, for example the analysis

[23]For Q_τ, this expression is given by $(Q_\tau + \frac{\tau}{Q_\tau}) - \frac{\mathbf{1}(y_h < Q_\tau)}{f_y(Q_\tau)}$. The intuition is that the marginal effect of an increase in union density to a given statistic is given by the average effect of each union individual on that statistic (each observation's "influence"). The estimate for β 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 τ, i.e., $\frac{dF(Q_\tau)}{dUnion}$, divided by the density of household income at Q_τ ($\frac{dF(Q_\tau)}{dQ_\tau}$). The resulting coefficient thus measures $\frac{dQ_\tau}{dUnion}$ the marginal change in the value of the quantile at τ in response to a small change in $Union$. The RIF of the Gini is not particularly illustrative and we omit it here, see Firpo *et al.* (2009) for details.

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. Autor *et al.* (2008) confirm this analysis using data from the CPS in the 1963-2005 period and adding more 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:

$$\log(wage_t^{Col}/wage_t^{HS}) = \beta UnionDensity_t + \gamma log\left(\frac{N_t^{Col}}{N_t^{HS}}\right) + \sum_{k=1}^{3} \lambda_k t^k + \epsilon_t. \tag{5}$$

The cubic polynomial in time captures changes in relative demand, although it could also be picking up unmeasured institutional factors driving the college premium. Our primary coefficient of interest is β, the effect of union density on the log skill premium.

Because Gallup union density may be mis-measured 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 information from both measures, effectively up-weighting 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 Piketty *et al.* (2014) 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 γ only in this tables (all coefficients are shown in the Appendix). Col. (1) largely follows 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. In col. (2), we instrument the Gallup measure with the BLS measure, and include our additional covariates, which somewhat increases the coefficient magnitude. Appendix Table A.5 shows a variety of alternative specifications (using the BLS union series instead of Gallup, instrumenting but excluding controls, and using a quartic instead of a cubic time polynomial).

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 specifications but using other measures of inequality as outcomes. Cols. (3) - (4) of Table 4 are identical to Cols. (1) - (2) except that the 90/10 log wage ratio for men is used as the outcome variable. The results are quite similar, with union density again having a negative and significant association with inequality. Appendix Table A.6 shows this result holds across alternative specifications.

Cols. (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 from the Census in the pre-CPS era. These additional years not only give us more observations, but also allow us to use inter-Census variation (e.g., during World War II). Cols. (5) - (6) use the Gini coefficient constructed by Kopczuk *et al.* (2010) from Social Security data, while cols. (7) - (8) use the top-ten-percent income share from Piketty and Saez (2003). The Gini series only extends through 2004. As before, we present alternative specifications in Appendix Table A.7 and A.8.

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

Col. (5) shows that the effects of union density on the Gini is large and robust, and grows considerably after instrumenting and adding controls (col. 6). Cols. (7) and (8) repeat the analysis in cols. (5) and (6) with the top 10 percent share of income as the outcome. The coefficients are large, negative and highly significant. We defer discussion of magnitudes to 7.4, in order to compare them with results from our alternative estimation strategies.

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 cubic in time (and quartic in the Appendix Tables), ensuring that the effect of unions is not simply mirroring the *u*-shape present in many time-series over the 20th century.

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.[24] One limitation of our survey-based data is that small states get small samples, resulting in noisy estimates for annual variation. We use both winsorized measures as well as a split-sample instrumental variables strategy to mitigate this problem. Although we do not have exogenous variation in union density, 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.[25]

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 Frank (2015), downloaded from the World Wealth and Income Database Alvaredo *et al.* (2016). This data is calculated using internal IRS data, but is not adjusted for capital gains. Frank (2015) show that when the same methodology is applied to national US data the results are still quite close to 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.[26]

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

[25] Similar regressions estimated at the cross-country level by Jaumotte and Osorio (2015).

[26] The Gallup measure is quite highly correlated (correlation = .724) with the existing Hirsch-Macpherson measures (individual union density as a fraction of non-farm employment) for the 1964-1986 years, which are where there is overlap. This correlation increases to .75 when we restriction attention to the CPS years with state identifiers (1978-1986). However, our sample extends further back, to 1937, where there has only been the Troy estimates of state-level union density in 1939 and 1953. Even though these are constructed from even more fragmentary records than the annual series we discuss above (many union reports did not disaggregate by state), we are also correlated with these data in both cross-sections and changes (1939 correlation = 0.78, 1953 correlation =

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

$$y_{st} = \beta UnionDensity_{st} - \frac{1}{\sigma} log\left(\frac{N_{st}^{Col}}{N_t^{HS}}\right) + \gamma X_{st} + \mu_{t\times South} + \delta_s + \epsilon_{st} \tag{6}$$

where y_{st} is a measure of inequality, for example the college-HS wage gap or the percent of total income accruing to the top %10, in state s and year t. We include South-by-year and state fixed effects in all regressions, $\mu_{t\times South}$ and δ_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.

We also control for skill-shares $\log\left(\frac{N_{st}^{Col}}{N_t^{HS}}\right)$ in all specifications. The Top %10 share of income is available at the annual level, so just as in the time-series regressions we include both the interpolated IPUMS-CPS education measure as well as the Gallup measure of education for that outcome.

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. This results in a system of equations given by:

$$UnionDensity_{st}^0 = \eta UnionDensity_{st}^1 - \iota \log\left(\frac{N_{st}^{Col}}{N_t^{HS}}\right) + \gamma^f X_{st} + \mu_{t\times South} + \delta_s + \nu_{st} \tag{7}$$

$$y_{st} = \beta UnionDensity_{st}^0 - \frac{1}{\sigma} \log\left(\frac{N_{st}^{Col}}{N_t^{HS}}\right) + \gamma X_{st} + \mu_{t\times South} + \delta_s + \epsilon_{st} \tag{8}$$

Since $UnionDensity^1$ and $UnionDensity^0$ are calculated from a random split of the data, the differences between the two measures (both observable covariates and unobserved ν_{st} are going to be orthogonal in 7. Omitted variable issues aside, if the only issue is measurement error, the IV estimator β^{IV} will yield a consistent, unattenuated estimate of β.

Table 5 shows results from the specification in 6 across our 4 main inequality measures. Column 1 estimates the IV specification following 6, with just education controls, state and

0.75, correlation in changes =0.5).

South-by-year fixed effects. We then show a much more saturated specification in Column 2, with Tables showing other combinations of covariates in the Appendix. The Appendix also 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.

There are of course numerous possible omitted variables in estimating the relationship between inequality and union density, even controlling for skill-shares and state and year fixed effects. Endogenous migration of workers of differing skills, endogenous selection of union organizing efforts and endogenous industry location decisions all may confound our simple regression specification. We deal with these by using an extensive battery of controls, paralleling the covariates in our time-series regressions, in the even numbered columns. The additional controls in column 2 include "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. As the business cycle is possibly an important determinant of both union density and inequality it is important to control for this, just as we controlled for civilian unemployment in the time-series regressions above. In addition, given that the top 10% share is derived from tax data the robustness of our coefficient to these controls is reassuring.

Col. (2) also includes industry controls, captured here as industry employment shares from the BLS data on state-year employment by industry. As unionization rates are highly heterogenous across industries, controlling for industry employment shares deals with possible confounds due to technological and sectoral changes across states over time. Further, to proxy for the overall policy environment, we include controls for the state-level minimum wage as well as a state-year index of overall "policy liberalism" constructed by Caughey and Warshaw (2016). Finally, to deal with possible smooth but unobserved state-specific changes in technology or other unobservables that may be confounding the estimated relationship, column 2 also includes state-specific linear and quadratic trends.

We repeat this sequence of estimates for each of our outcomes. Across outcomes, adding this extensive set of additional controls does not alter the statistical significance of the IV coefficients, although they are roughly 50% smaller. The negative correlation of unions and top income shares remains negative and significant even when we control for a wide variety of potential omitted variables, even allowing each state to be on a separate "U"-shaped latent pattern of inequality.

In Appendix A.2 we show a variety of specifications that add intermediate sets of controls between the odd and even columns reported in Table 5. Perhaps unsurprisingly, the state-specific quadratic trends are the controls that lower the magnitude of the union variable, and these in fact may be absorbing some of the effect of unions.

Further, in the Appendix we report results on state-year log GDP per capita (Appendix Table A.13), as well as a variety of policy outcomes (Appendix Table A.14). Union density shows consistently positive, but sometimes insignificant, effects on state GDP per capita, and we can rule out large negative effects of unions on state-level economic activity. Finally, it does not appear that unions drive state-level policy towards redistribution. While signs are consistent with unions pushing in a more pro-redistribution direction at the state level, we find no robust statistically significant effects on the presence of a state minimum wage, the level of the minimum wage, the tax-income ratio, or the overall measure of policy liberalism used above.

7.4 Discussion of Estimate Magnitudes

While each of three approaches involves strong identification assumptions, all regressions point to negative effects of unions on inequality. The state-year effect magnitudes vary across outcomes, but are generally smaller than the time-series results and are comparable with the magnitudes of the state-year regressions. As discussed in the introduction, our results on the Gini are similar across the RIF, OLS time-series, and state-year specifications, with the 12 percentage point decline in unions between 1970 and 2004 implying between .6 and 1.2 point increase in the Gini, which increased 7 points at the household level and 9 points at the individual level.

Across the other outcomes, differences between specifications is more pronounced. Our smaller wage premium for an additional 4 years of schooling for the respondent implies that a 12 percentage point increase in union density would have lowered the college income premium by 2%. While the individual college wage premium has increased 26% between 1970 and 2004, this is not exactly comparable to the household number estimated by the RIF. At the more aggregate level, 12 percentage point higher national union density implies roughly a 12 percent smaller skill premium if we take the annual coefficient, and a 3.4-3.9 percent smaller college premium if we take the state-year specification results instead. The RIF results, while noisy, imply at most a -0.7 effect on the household 90-10 ratio. The aggregate effect of unions on 90-10 male gap is much larger, with a coefficient of roughly -3, while the state-year 90-10 male earnings ratio coefficients are less than a third of that, between -1 and -0.5. The state-year and RIF results imply that the 12 percentage point union reduction increased the 90-10 ratio (of men and households, respectively) by between 5 and 12 percent, while the increase in the individual male 90-10 ratio over the 1970-2004 period was almost 50 percent. Thus, the share of the increase explained is comparable to Card (2001), who finds that unions explain 20% of the increase in male wage inequality (measured by variance of log wages).

The annual time-series regression implies quite large impacts of union density on the top 10 percent share of income. The same 12 percentage-point decline in density between 1970 and 2004 would predict a 4 to 8 percentage-point increase in the top ten share. The rise in top 10 percent share between 1974 and 2000 is 12.13, and the time-series coefficients imply that deunionization could explain between 30 and 60 percent of this.

However, while our state-year effects on the top 10% share are robust and significant, they are not large, being an order of magnitude smaller than the aggregate time-series results, and imply that deunionization explains between 2.5% and 5% of the increase. The descrepancy in these two estimands could be due to endogenity, but it could also be that the income of the national top decile is coming from very different sources than the income of state-level top earners.

To sum up, across a variety of measures of inequality that we can estimate using individual variation, aggregate time-series variation, and state-year panel variation, we find that unions have a negative effect on inequality. The state-year and RIF results are broadly comparable, and imply that deunionization can explain up to a third of the increase in inequality, while the aggregate time-series effects of union density are much larger for tail-driven measures of inequality, but also much more likely to be contaminated by omitted variables bias. 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

Despite over six decades of union decline, the labor movement remains an often discussed remedy to increased income inequality, and is a major institutional candidate for explaining the post-WW2 Great Compression in the income distribution. We leverage historical Gallup polling data to provide the first, systematic, representative, study of union effects using microdata prior to the CPS. Our aggregate series match the existing series quite well, particularly in recent periods where the existing series are based on CPS individual survey data rather than union reports. The availability of microdata enables us to examine the union family income premium over a long stretch of time, which we find to be remarkably constant, and larger for less-skilled and minority workers. We also examine selection into unions based on education, race, occupation, and predicted income, and find that, on all dimensions, selection into unions exhibited a "U"-shape, with the most negatively selected workers entering unions roughly during the period of peak union density in the 1950s.

We use this data to shed light on the relationship between union density and U.S. inequality, which previously had been limited to the relative recent CPS era. Using distributional regressions, aggregate time-series analysis, and a state-year panel with fixed effects, we find that union density lowers inequality, although the precise quantitative contribution varies across measures of inequality. We do not pretend to have the last word on union effects in the pre-CPS period. While all correlations we have presented are extremely robust, the absence of identifying variation in all of our union specifications makes these results more tentative.

More broadly, we have shown the value of historical polling data for answering outstanding questions in economic history. Besides measuring variables such as union membership not present in official datasets such as the census, we believe the data assembled for this project will be valuable for documenting economic changes at higher frequencies than available with existing datasets. In particular, the month-to-month turmoil of the war years is captured by Gallup's surveys with more regularity than most government surveys, and may prove an interesting area for future research.

References

Abowd, J. M. (1989). The effect of wage bargains on the stock market value of the firm. *The American Economic Review*, pp. 774–800.

Acemoglu, D. and Robinson, J. A. (2013). Economics versus politics: Pitfalls of policy advice. *The Journal of Economic Perspectives*, **27** (2), 173–192.

Ahlquist, J. S. (2017). Labor unions, political representation, and economic inequality. *Annual Review of Political Science*, **20** (1).

Alvaredo, F., Atkinson, A. B., Piketty, T., Saez, E. and Zucman, G. (2016). The world wealth and income database. *Website: http://www. wid. world*.

Autor, D. H., Katz, L. F. and Kearney, M. S. (2008). Trends in us wage inequality: Revising the revisionists. *The Review of economics and statistics*, **90** (2), 300–323.

Berinsky, A. J. (2006). American public opinion in the 1930s and 1940s: The analysis of quota-controlled sample survey data. *International Journal of Public Opinion Quarterly*, **70** (4), 499–529.

Blanchflower, D. and Bryson, A. (2002). *Changes over time in union relative wage effects in the UK and the US revisited*. Tech. rep., National Bureau of Economic Research.

Buchmueller, T. C., DiNardo, J. and Valletta, R. (2004). A submerging labor market institution? unions and the nonwage aspects of work. In *Emerging labor market institutions for the twenty-first century*, University of Chicago Press, pp. 231–264.

Bureau of Labor Statistics (1945). *Extent of Collective Bargaining and Union Status, January 1945*. Tech. rep., bulletin # 829.

Callaway, B. and Collins, W. J. (2016). Unions, workers, and wages at the peak of the american labor movement.

Card, D. (2001). The effect of unions on wage inequality in the us labor market. *ILR Review*, **54** (2), 296–315.

— and DiNardo, J. E. (2002). Skill-biased technological change and rising wage inequality: some problems and puzzles. *Journal of Labor Economics*, **20** (4), 733–783.

Caughey, D. and Warshaw, C. (2016). The dynamics of state policy liberalism, 1936–2014. *American Journal of Political Science*, **60** (4), 899–913.

DiNardo, J., Fortin, N. M. and Lemieux, T. (1996). Labor market institutions and the distribution of wages, 1973-1992: A semiparametric approach. *Econometrica*, **64** (5), 1001–1044.

Dinardo, J. and Hallock, K. F. (2002). When unions mattered: the impact of strikes on financial markets, 1925–1937. *ILR Review*, **55** (2), 219–233.

DiNardo, J. and Lee, D. S. (2004). Economic impacts of new unionization on private sector employers: 1984–2001. *The Quarterly Journal of Economics*, **119** (4), 1383–1441.

Farber, H. (2005). Nonunion wage rates and the threat of unionization. *ILR Review*, **58** (3), 335–352.

Firpo, S., Fortin, N. M. and Lemieux, T. (2009). Unconditional quantile regressions. *Econometrica*, **77** (3), 953–973.

Foner, P. S. (1976). *Organized labor and the black worker: 1619-1973*, vol. 475. International publishers.

Frandsen, B. (2013). The surprising impacts of unionization on establishments: Accounting for selection in close union representation elections. *Manuscript. Department of Economics, Brigham Young University*.

Frandsen, B. R. (2017). Party bias in union representation elections: Testing for manipulation in the regression discontinuity design when the running variable is discrete. In *Regression Discontinuity Designs: Theory and Applications*, Emerald Publishing Limited, pp. 281–315.

Frank, M. (2015). Frank-sommeiller-price series for top income shares by us states since 1917. *WTID Methodological Notes*.

Freeman, R. and Medoff, J. (1984). Medoff. what do unions do.

Freeman, R. B. *et al.* (1998). Spurts in union growth: Defining moments and social processes. *NBER Chapters*, pp. 265–296.

Frydman, C. and Saks, R. E. (2010). Executive compensation: A new view from a long-term perspective, 1936–2005. *Review of Financial Studies*, p. 120.

Galenson, W. (1960). *The CIO challenge to the AFL: a history of the American labor movement, 1935-1941*. Harvard University Press.

Goldfield, M. (1989). Worker insurgency, radical organization, and new deal labor legislation. *American Political Science Review*, **83** (4), 1257–1282.

Goldin, C. and Margo, R. (1992). The great compression: The wage structure in the united states at mid-century. *The Quarterly Journal of Economics*, **107**, 1–34.

Goldin, C. D. and Katz, L. F. (2009). *The race between education and technology*. Harvard University Press.

Hacker, J. S. and Pierson, P. (2011). *Winner-take-all politics: How Washington made the rich richer–and turned its back on the middle class*. Simon and Schuster.

Hirsch, B. T., Macpherson, D. A. and Vroman, W. G. (2001). Estimates of union density by state. *Monthly Labor Review*, **124** (7), 51–55.

Igo, S. E. (2007). *The averaged American: Surveys, citizens, and the making of a mass public*. Harvard University Press.

Jaumotte, F. and Osorio, C. (2015). *Inequality and labor market institutions*. International Monetary Fund.

Katz, L. F., Krueger, A. B. *et al.* (1998). Computing inequality: have computers changed the labor market? *The Quarterly Journal of Economics*, **113** (4), 1169–1213.

— and Murphy, K. M. (1992). Changes in relative wages, 1963–1987: supply and demand factors. *The Quarterly Journal of Economics*, **107** (1), 35–78.

Katznelson, I. (2013). *Fear itself: The new deal and the origins of our time*. WW Norton & Company.

King Jr, M. L. (1986). If the negro wins, labor wins. *A Testament of Hope: The Essential Writings of Martin Luther King, Jr*, pp. 201–207.

Kopczuk, W., Saez, E. and Song, J. (2010). Earnings inequality and mobility in the united states: evidence from social security data since 1937. *The Quarterly Journal of Economics*, **125** (1), 91–128.

Lee, D. S. (1999). Wage inequality in the united states during the 1980s: Rising dispersion or falling minimum wage? *The Quarterly Journal of Economics*, **114** (3), 977–1023.

— and Mas, A. (2012). Long-run impacts of unions on firms: New evidence from financial markets, 1961–1999. *The Quarterly Journal of Economics*, **127** (1), 333–378.

Leighley, J. E. and Nagler, J. (2007). Unions, voter turnout, and class bias in the us electorate, 1964–2004. *Journal of Politics*, **69** (2), 430–441.

Lemieux, T. (2006). Increasing residual wage inequality: Composition effects, noisy data, or rising demand for skill? *The American Economic Review*, **96** (3), 461–498.

Lewis, H. G. (1963). *Relative employment effects of unionism*. University of Chicago.

— (1986). Union relative wage effects. *Handbook of labor economics*, **2**, 1139–1181.

Lichtenstein, N. (2003). *Labor's War at Home: The CIO in World War II*. Temple University Press.

Mill, J. S. (2008). *Principles of Political Economy*. Oxford University Press.

Northrup, H. R. (1971). *Organized labor and the Negro*. Periodicals Service Co.

Piketty, T. and Saez, E. (2003). Income inequality in the united states, 1913–1998. *The Quarterly journal of economics*, **118** (1), 1–41.

—, — and Stantcheva, S. (2014). Optimal taxation of top labor incomes: A tale of three elasticities. *American economic journal: economic policy*, **6** (1), 230–271.

Pischke, J.-S., DiNardo, J. E. and Hallock, K. F. (2000). Unions and the labor market for managers.

Skocpol, T., Finegold, K. and Goldfield, M. (1990). Explaining new deal labor policy. *American Political Science Review*, **84** (4), 1297–1315.

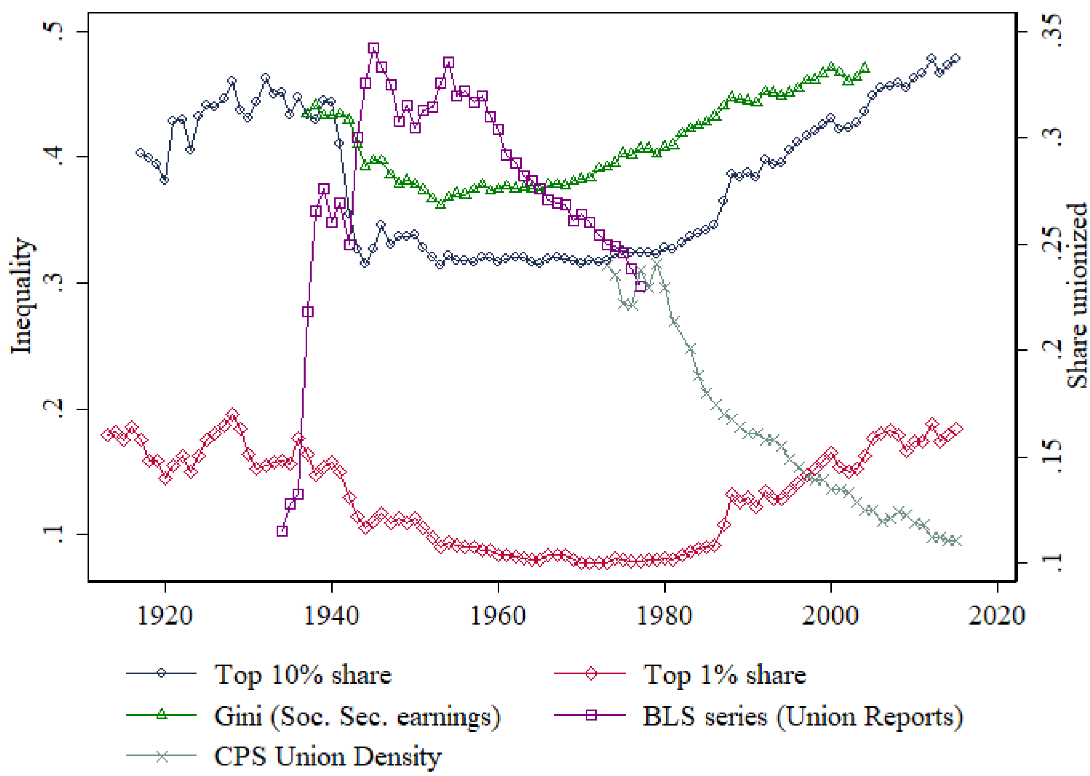
Taschereau-Dumouchel, M. (2015). The union threat.

Troy, L. (1965). *Trade Union Membership, 1897–1962*. NBER.

— and Sheflin, N. (1985). Union sourcebook: Membership, finances, structure, directory. *West Orange, NJ: Industrial Relations Data and Information Services*.

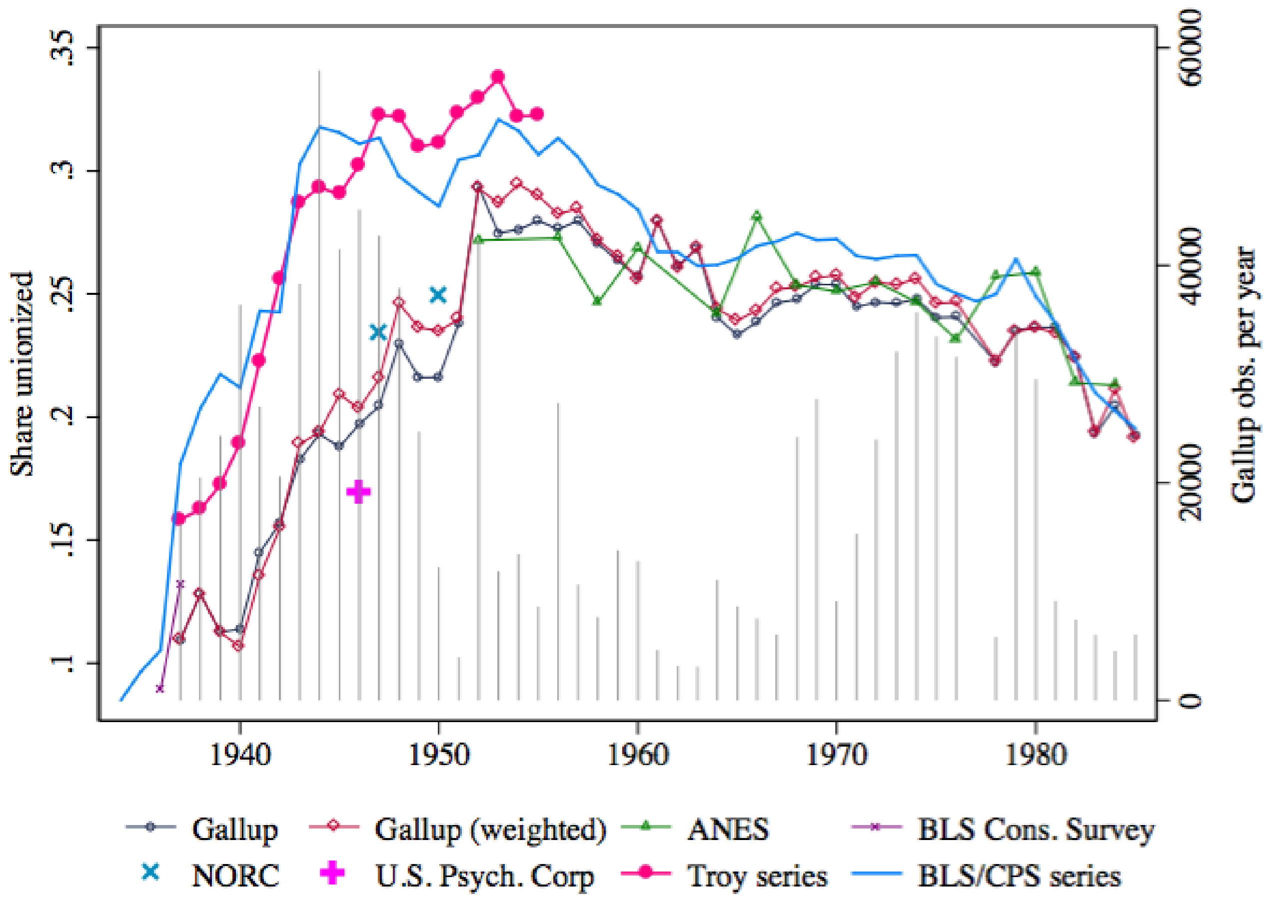
Western, B. and Rosenfeld, J. (2011). Unions, norms, and the rise in us wage inequality. *American Sociological Review*, **76** (4), 513–537.

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



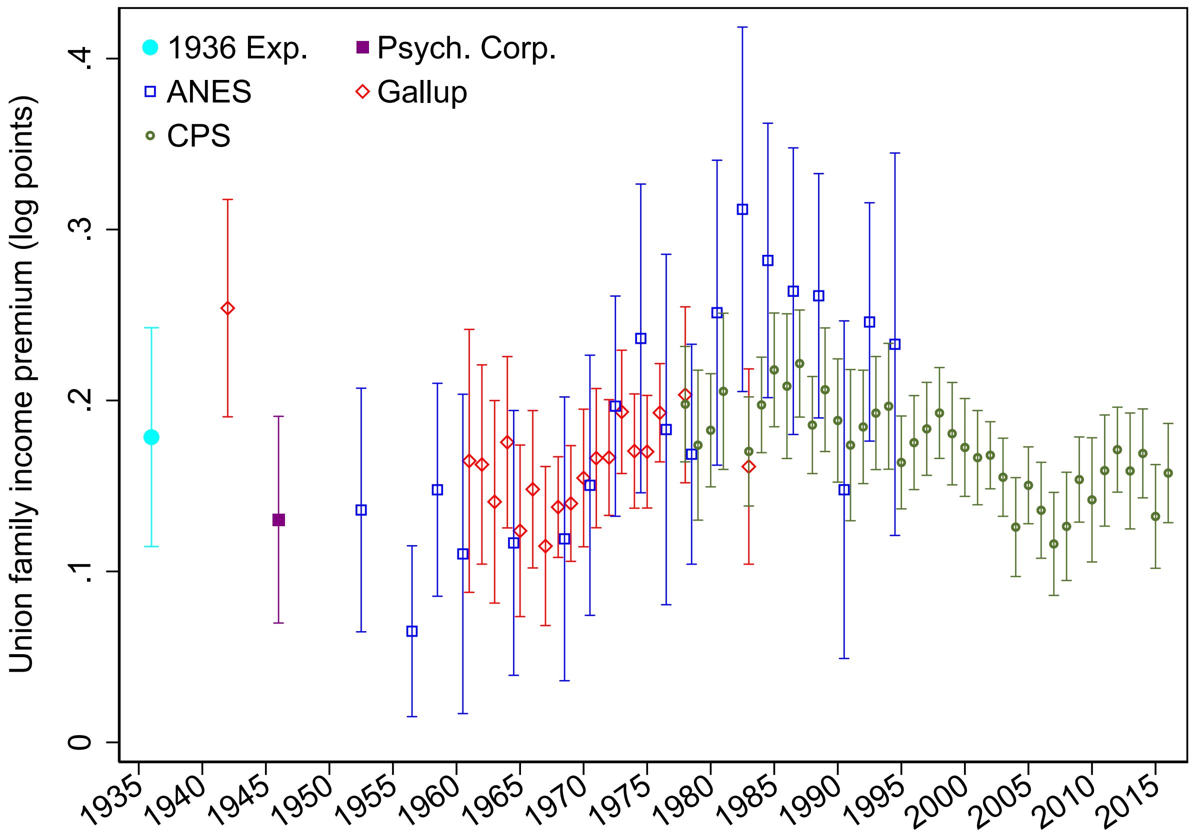
Data sources: Top share inequality from Piketty and Saez (2003, updated 2016). Union density data from Historical Statistics of the United States and the Current Population Survey. We discuss these data sources in detail in Section 2 and 3.4.

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



Data sources: See Sections 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).

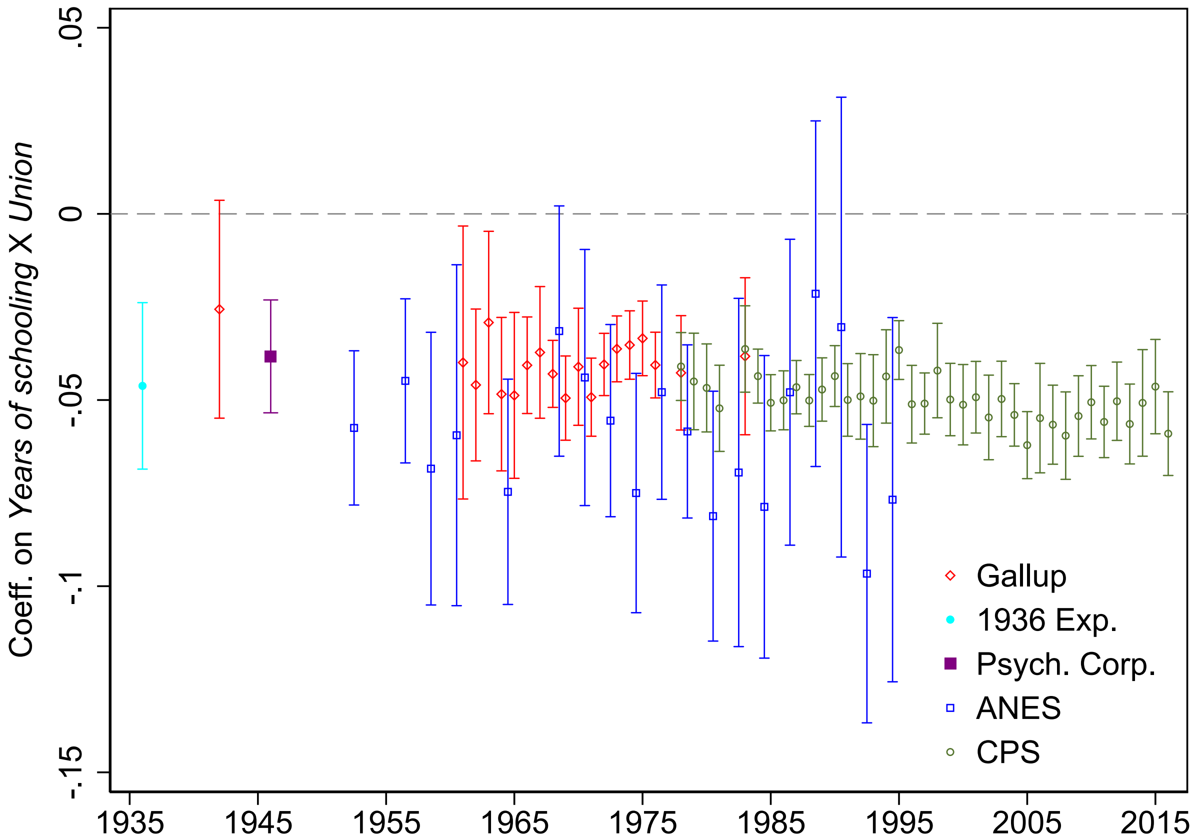
Figure 3: Estimates of the union family income premium



Data source: Gallup data, 1942, 1961–1974; CPS, 1976–2016; BLS Expenditure Survey, 1937; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 3 for a description of each data source.

Notes: Each plotted point comes from estimating equation (1), which regresses 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.

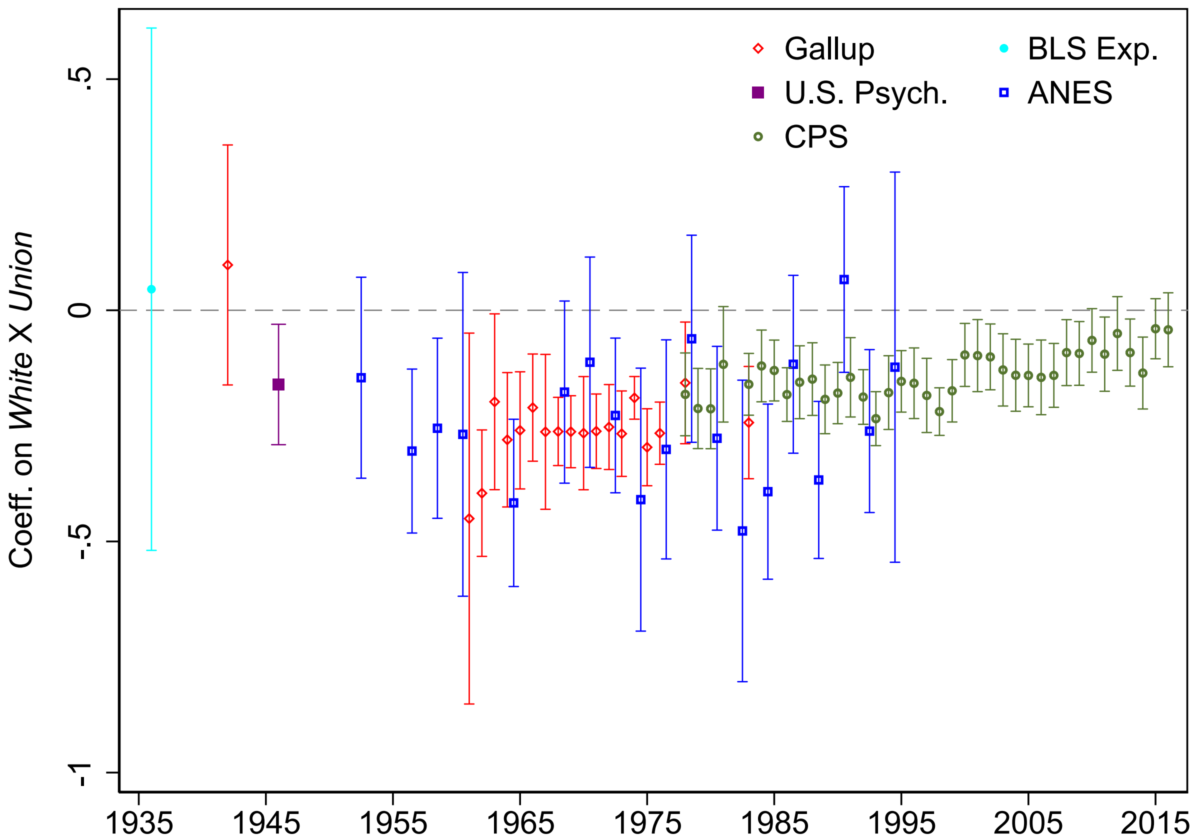
Figure 4: Differential family union premium by respondent's years of schooling



Data source: Gallup data, 1942, 1961–1974; CPS, 1976–2016; BLS Expenditure Survey, 1937; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 3 for a description of each data source.

Notes: Each plotted point comes from estimating equation regressing log 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. Some survey sources give actual years of schooling. For those that do not, we impute in the following manner: six years for "less than middle school;" eight years for "middle school;" ten years for "some high school;" twelve years for "high school;" fourteen years for "some college" or "vocational training;" sixteen years for "college," eighteen years for "more than college." The figure plots the coefficient on the interaction $Years\, of\, schooling \times Union$. The plotted confidence intervals are based on standard errors clustered by state.

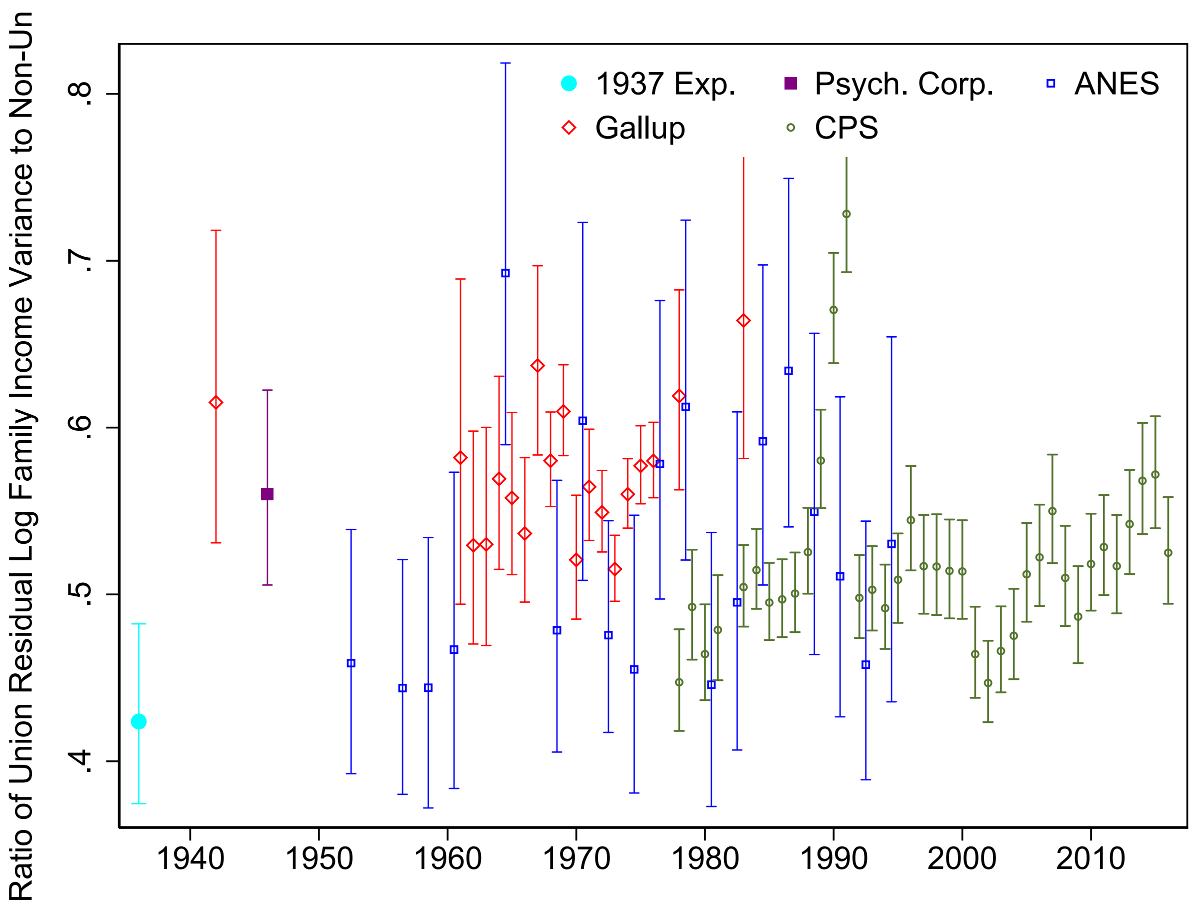
Figure 5: Differential family union premium for whites relative to minorities



Data source: Gallup data, 1942, 1961–1974; CPS, 1976–2016; BLS Expenditure Survey, 1937; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 3 for a description of each data source.

Notes: Each plotted point comes from estimating equation regressing log 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.

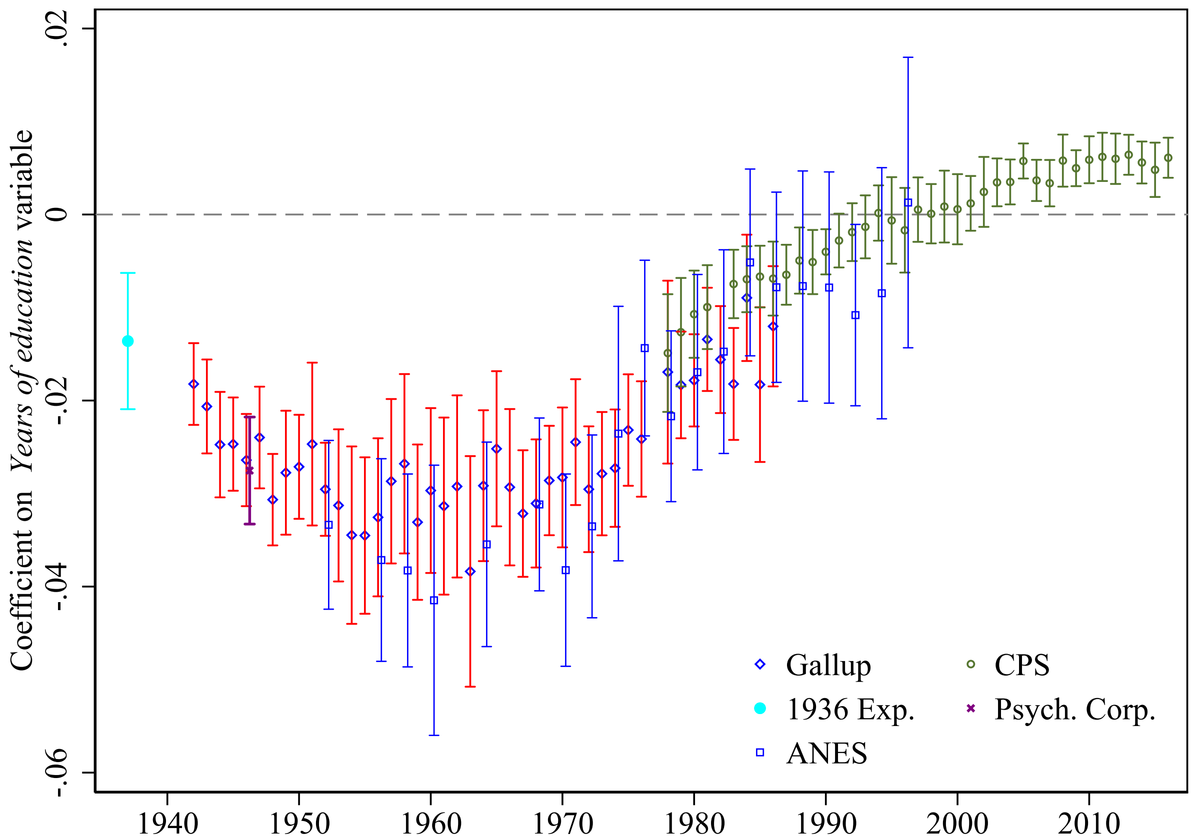
Figure 6: Ratio of residual variance between union and non-union sectors



Data source: Gallup data, 1942, 1961–1974; CPS, 1976–2016; BLS Expenditure Survey, 1937; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 3 for a description of each data source.

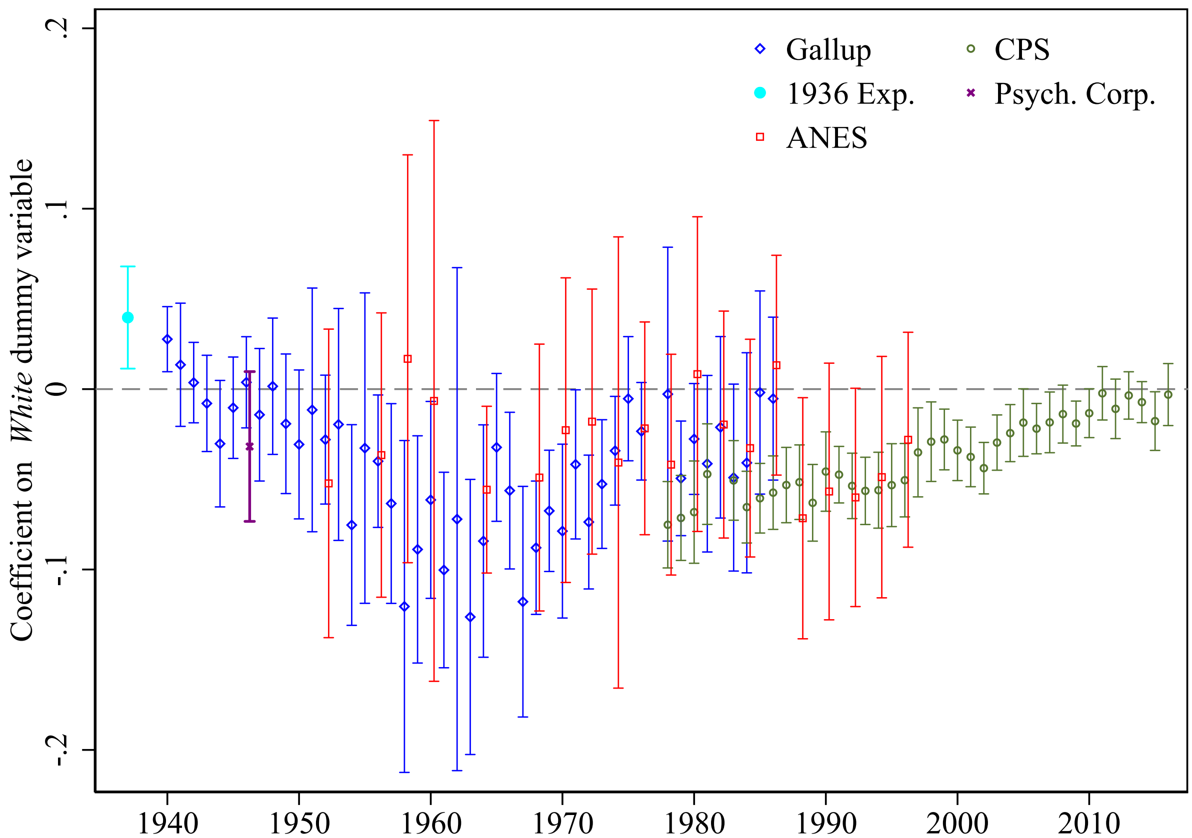
Notes: Each plotted point is the ratio of variance of residuals from regressing log family income on the controls in equation (1) separately for union and non-union households, separately by survey source and by year. The figure plots the ratio of the variance of residuals in the union sector to that of the non-union sector (so ratios less than one suggest that residual variance in the union sector is more compressed than in the non-union). The plotted confidence intervals are based on inverting the F-statistic testing the null that the ratio is equal to 1.

Figure 7: How does educational attainment predict union household status?



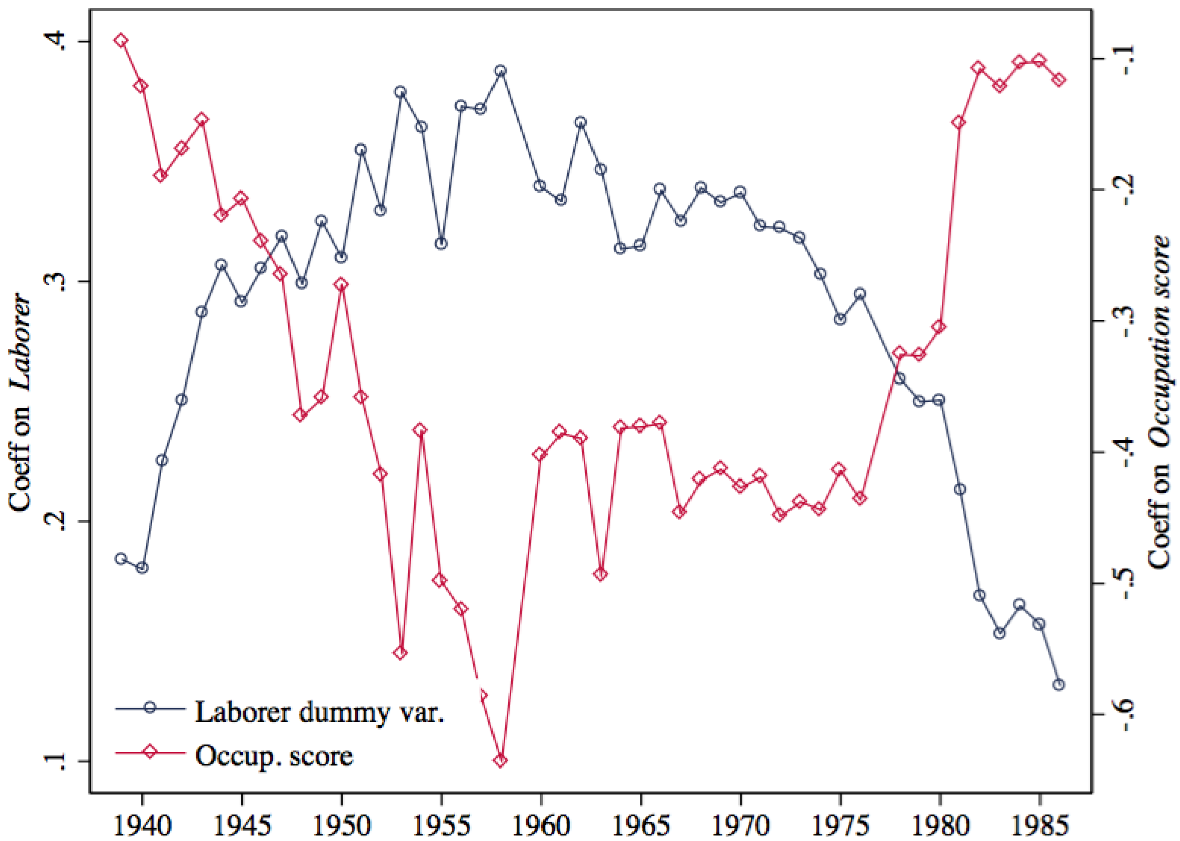
Data sources: Gallup data, 1937–1986; CPS, 1976–2016; BLS Expenditure Survey, 1937; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 3 for a description of each data source. *Notes*: We regress household union status on $Year\,of\,education$, state s and survey-date t fixed effects, age and its square, and gender. (The notes to Figure 4 describe how we impute years of education if the survey source only gives us categories of educational attainment.) We estimate this equation separately by survey source and by year. The figure plots the coefficient on $Year\,of\,education$. The plotted confidence intervals are based on standard errors clustered by state.

Figure 8: How does race predict union household status?



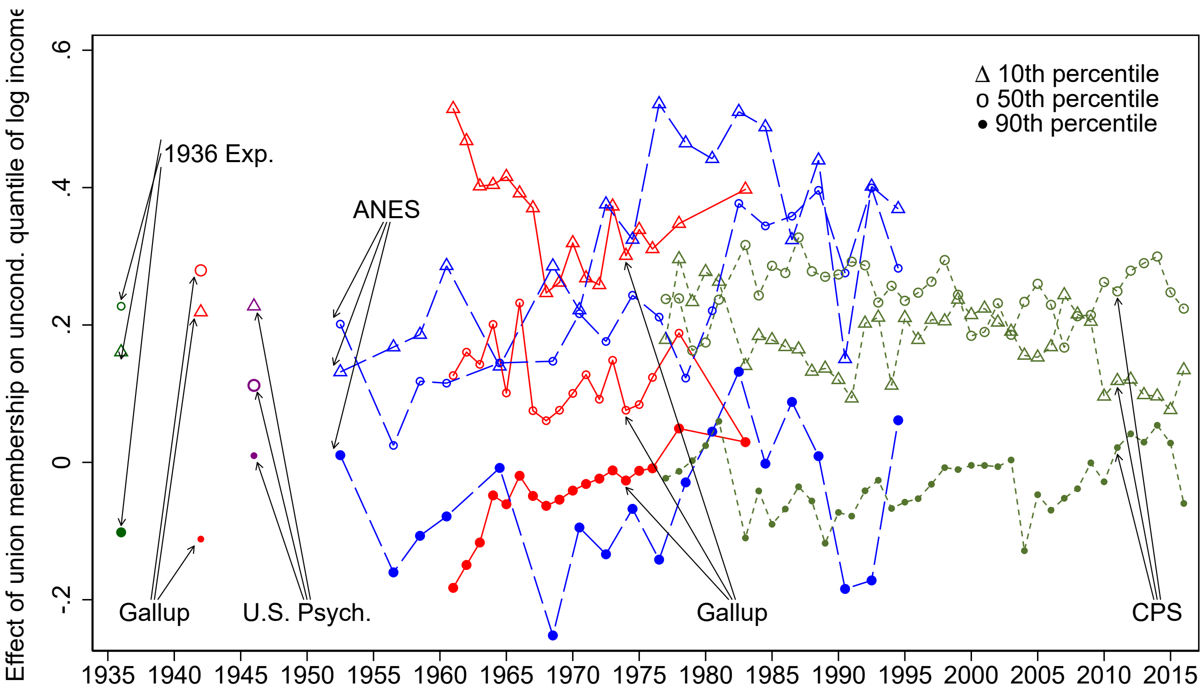
Data sources: Gallup data, 1937–1986; CPS, 1976–2016; BLS Expenditure Survey, 1937; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 3 for a description of each data source. *Notes*: For each data source, we estimate, separately by data source and year, household union status on a *White* 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.

Figure 9: How does household head occupation predict union household status?



Data sources: Gallup data, 1939–1986.
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:
$Log(income)_{hst} = \lambda_h^{OccHH} + \gamma_1 Year_t + \gamma_2 Year_t^2 + \mu_s + e_{hst}$ 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, age and its square, gender, as well as $\sum_{y(t)} Occupation_h$, where in the first series *Occupation* is proxied by the laborer dummy variable and in the second by our estimated occupation score. The regression is estimated separately by year and survey source. Confidence intervals are based on standard errors clustered by state.

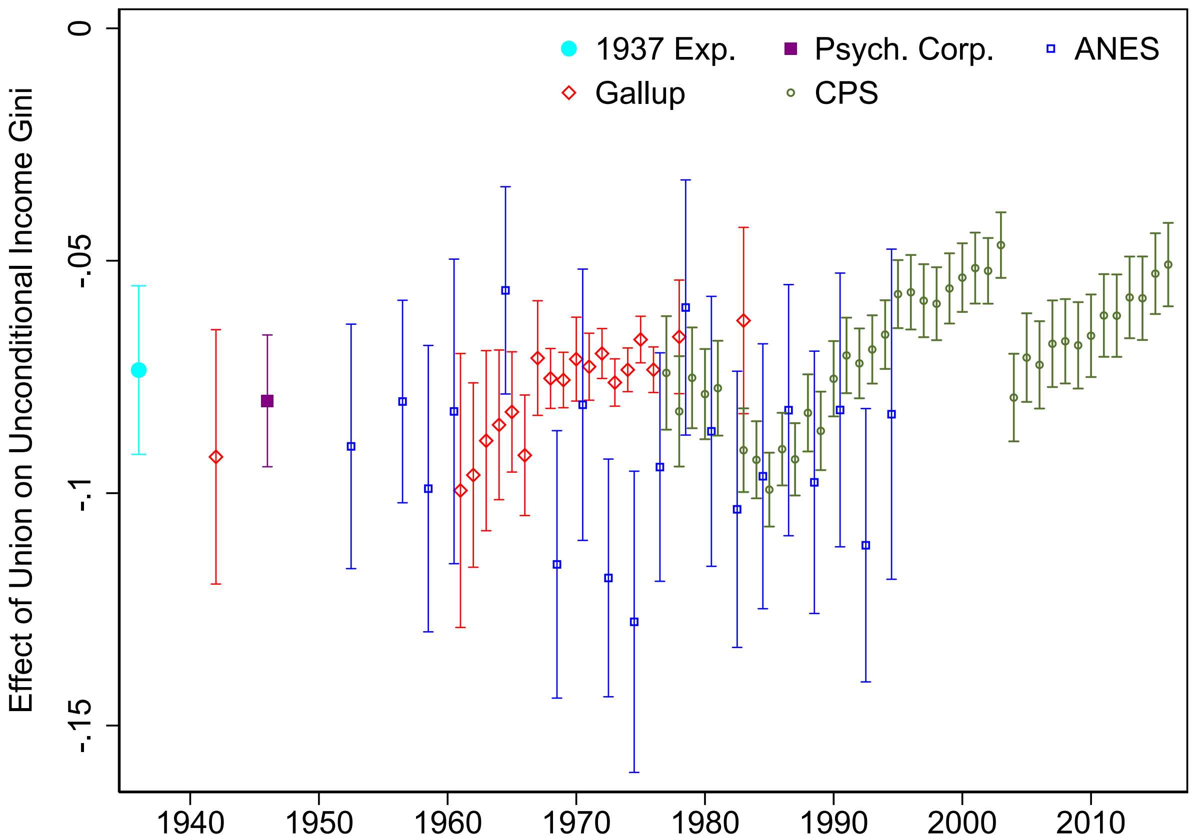
Figure 10: Effects of Union Density on Family Income Quantiles



Data source: Gallup data, 1942, 1961–1974; CPS, 1976–2016; BLS Expenditure Survey, 1937; ANES, 1952–1996, U.S. Psych. Corporation, 1946. See Section 3 for a description of each data source.

Notes: Each plotted point comes from estimating equation (4), which regresses the recentered influence function (RIF) for the specified quantile on controls for age, gender, race, educational attainment fixed effects, household employment status controls, state and survey-date fixed effects. Occupation controls are not included. The plotted confidence intervals are robust to heteroskedasticity.

Figure 11: 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 (RIF) for the Gini coefficient on controls for age, gender, race, educational attainment fixed effects, household employment status controls, 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

	1950		1960		1970		1980	
	Census	Gallup	Census	Gallup	Census	Gallup	Census	Gallup
South Share	0.242	0.161	0.260	0.215	0.271	0.277	0.290	0.296
—*South*								
Female	0.516	0.516	0.520	0.515	0.529	0.505	0.529	0.503
Age	44.61	43.86	45.07	47.67	45.94	47.27	45.19	46.47
Black	0.200	0.113	0.183	0.124	0.160	0.113	0.159	0.152
HS grad.	0.294	0.347	0.364	0.312	0.473	0.529	0.620	0.648
—*Non-South*								
Female	0.515	0.503	0.517	0.523	0.528	0.509	0.528	0.502
Age	46.67	43.05	45.94	45.98	46.27	45.98	45.27	44.07
Black	0.0530	0.0424	0.0611	0.0244	0.0709	0.0676	0.0785	0.0930
HS grad.	0.385	0.489	0.451	0.514	0.579	0.661	0.709	0.765
Observ.	296223	25328	1081562	22145	2444218	29095	1494469	31473

Sources: Gallup surveys and 1950–1980 IPUMS.
Notes: We use the Gallup definition of the "South": all eleven states of the former Confederacy plus Oklahoma. All Census results use IPUMS person weights.

Table 2: Comparing Gallup and IPUMS in 1940

	Gallup	Census	Census	Gallup	Census
–Demographics					
Black	0.0285	0.0873	0.0895	0.0319	0.0349
Female	0.335	0.505	0.341	0.338	0.341
Age	42.67	42.75	43.04	42.66	43.67
HS graduate	0.241	0.264	0.253	0.246	0.274
–Geography					
Northeast	0.0836	0.0672	0.0633	0.0947	0.0855
Mid Atlantic	0.257	0.251	0.239	0.291	0.323
East Central	0.208	0.188	0.187	0.235	0.252
West Central	0.179	0.130	0.131	0.202	0.178
South	0.117	0.255	0.260	- -	- -
Rocky Mountain	0.0760	0.0283	0.0312	0.0860	0.0422
Pacific Coast	0.0801	0.0761	0.0834	0.0907	0.113
–Occupation					
Professional	0.0772	0.0493	0.0500	0.0773	0.0549
Farmer	0.193	0.140	0.139	0.167	0.0970
Farm laborers	0.00474	0.0247	0.0293	0.00478	0.0191
Propietors, managers, officials	0.126	0.0927	0.0884	0.130	0.0941
Clerks (white collar)	0.167	- -	- -	0.171	- -
Clerical and kindred	- -	0.0519	0.0525	- -	0.0591
Sales workers	- -	0.0452	0.0451	- -	0.0492
Skilled workmen and foremen	0.0918	- -	- -	0.0956	- -
Craftsmen	- -	0.138	0.137	- -	0.151
Semi-skilled workers	0.120	- -	- -	0.124	- -
Operatives	- -	0.138	0.143	- -	0.154
Non-farm unsk. laborers	0.0733	- -	- -	0.0767	- -
Laborers	- -	0.0893	0.0952	- -	0.0922
Servant classes	0.0290	- -	- -	0.0315	- -
Service workers (priv. HH)	- -	0.0101	0.0106	- -	0.00633
Other service workers	- -	0.0478	0.0473	- -	0.0514
No answer, N/A, etc.	0.118	0.173	0.164	0.122	0.172
HH/gender adjustment	N/A	N/A	Yes	N/A	Yes
Ex. S/SW?	No	No	No	Yes	Yes
Observations	See notes	812729	812729	See notes	603250

Sources: Gallup surveys and 1940 IPUMS.

Notes: The Gallup sample size varies substantially by variable during this period. For the col. (1) sample, all demographics except for education and all geographic variables have a sample size around 159,000 (with small variations due to missing observations). The occupation codes have a sample size of roughly 21,000. The high school completion indicator has a sample size of 5,700. In col. (4) each sample size is roughly twelve percent smaller. "HH / gender adjustment" underweights women and people in large households in the IPUMS, to better match Gallup sampling (which only sampled one person per household and had a target female share of one-third). "Ex S/SW" excludes Southern and Southwestern states (all eleven states of the former Confederacy plus Oklahoma). All Census results use IPUMS person weights.

Table 3: Formally testing for u-shaped selection into unions by race and education

	Dep't variable: Respondent in a union household									
	Including South					Excluding South				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
White	328.5**	421.1***	301.4**	-0.00844	195.0	794.9***	857.1***	697.8***	-0.0283**	607.8***
	[140.5]	[153.5]	[147.9]	[0.00751]	[148.0]	[161.2]	[162.2]	[167.7]	[0.0118]	[164.1]
White x year	-0.335**	-0.429***	-0.307**		-0.198	-0.811***	-0.874***	-0.711***		-0.620***
	[0.143]	[0.157]	[0.151]		[0.151]	[0.165]	[0.166]	[0.171]		[0.168]
White x year squared,	0.0851**	0.109***	0.0781**		0.0503	0.207***	0.223***	0.181***		0.158***
divided by 1,000	[0.0366]	[0.0399]	[0.0385]		[0.0385]	[0.0421]	[0.0423]	[0.0437]		[0.0428]
Years of schooling				105.5***	101.8***				164.5***	158.7***
				[23.41]	[22.45]				[21.14]	[20.94]
Years edu. x year				-0.108***	-0.104***				-0.168***	-0.162***
				[0.0239]	[0.0229]				[0.0216]	[0.0214]
Years edu. x year				0.0274***	0.0264***				0.0427***	0.0412***
squared, divided by 1,000				[0.00609]	[0.00584]				[0.00552]	[0.00547]
Dept. var. mean	0.247	0.267	0.267	0.267	0.267	0.292	0.315	0.315	0.315	0.315
Ex. if educ missing?	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Educ. FE?	No	No	Yes	No	No	No	No	Yes	No	No
Implied white min.	1964.4	1965.1	1964.4	N/A	1968.9	1961.6	1962.2	1961.6	N/A	1962.3
Implied educ. min.	N/A	N/A	N/A	1962.7	1962.5	N/A	N/A	N/A	1963.1	1963.2
Observations	684911	569683	569683	569683	569683	546121	445980	445980	445980	445980

Sources: Gallup, 1937–1986. See Section 3 for a detailed description.
Notes: In this table we test for whether interacting $White$ and $Years\,of\,schooling$ with a linear and quadratic terms of (calendar) year yields a statistically significant u shape. We use these coefficients to calculate the implied "minimum" year of positive selection (or "maximum" year of negative selection), as both $White$ and $Years\,of\,schooling$ are positive predictors of family income. Besides the coefficients reported in the table, we also control for survey-date and state fixed effects, age and its square, and gender. In the second panel, we exclude all observations from the South ("South" is defined as in Gallup: all eleven states of the former Confederacy plus Oklahoma) because Gallup does not sample blacks in proportion to their population in the South until the 1950s. Standard errors are clustered by state. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 4: Aggregate inequality statistics as a function of union density

	Dependent variable:							
	Coll. premium		90/10 ratio		Gini coeff.		Top 10 share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Union density	-0.959**	-1.294***	-2.935***	-3.097***	-0.100**	-0.273**	-35.21***	-69.75***
	[0.377]	[0.438]	[0.502]	[0.669]	[0.0447]	[0.114]	[11.13]	[24.61]
Mean, dept. var	0.555	0.555	1.620	1.620	0.410	0.410	36.319	36.319
R-squared	0.976	0.978	0.976	0.983	0.985	0.981	0.950	0.948
Gallup edu. control?	No	No	No	No	Yes	Yes	Yes	Yes
Addit. controls?	No	Yes	No	Yes	No	Yes	No	Yes
BLS IV?	No	Yes	No	Yes	No	Yes	No	Yes
Cubic polynomial?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	37	37	52	52	65	65	73	73

Sources: For cols. (1) - (4), outcome variables generated from Census IPUMS and CPS; for cols (5) and (6) from Kopczuk *et al.* (2010) and for cols (7) and (8) from Piketty and Saez (2003, updated 2016). Explanatory variables are union share of households, averaged by year using Gallup data (see Section 3 for detail); annual union density from BLS (see Section 2).
Notes: All regressions include controls for the log share of college versus high-school educated workers, calculated in the early years from Census IPUMS and for later years from the CPS. The first four columns use outcome variables calculated from the source (so are only available in Census years until the CPS), but the last four columns use as outcomes *annual* measures, calculated from administrative data. For these measures, we have to control *annually* for skill shares. We include two annual controls: annual skills shares as measured in Gallup and annual skills shares as measured in the Census IPUMS and the CPS (interpolated between Census years in the pre-CPS years). For each outcome variable, the first specification has parsimonious controls (only a time cubic and the skill shares controls) and the second has additional controls (federal minimum wage, the national unemployment rate, and the top marginal tax rate in the federal income tax schedule). The second specification also instruments with Gallup union density measure with the BLS measure (see Section 7.2 for estimating equations). Appendix Tables A.5, A.6, A.7, A.8 provide additional specifications using the skill premium, the log 90/10 ratio, the Gini coefficient and the top-ten share, respectively, as outcomes. Note that to the log 90/10 is for men only, but all other inequality measures pool both men and women. Standard errors are robust to heteroskedasticity. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Table 5: Inequality and Union Density 1940-2012: State-year panel regressions

	Dependent variable:							
	Coll. premium		90/10 ratio		Gini coeff.		Top 10 share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Household union share	-0.328** [0.139]	-0.245** [0.119]	-1.095** [0.440]	-0.495*** [0.189]	-0.100*** [0.0329]	-0.0589** [0.0259]	-5.309*** [1.699]	-2.756** [1.104]
Mean, dept. var.	0.416	0.416	1.548	1.548	0.351	0.351	36.99	36.99
Industry shares	No	Yes	No	Yes	No	Yes	No	Yes
State-spec. quad.	No	Yes	No	Yes	No	Yes	No	Yes
Income covars.	No	Yes	No	Yes	No	Yes	No	Yes
Policy covars.	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1781	1781	1781	1781	1781	1781	3251	3251

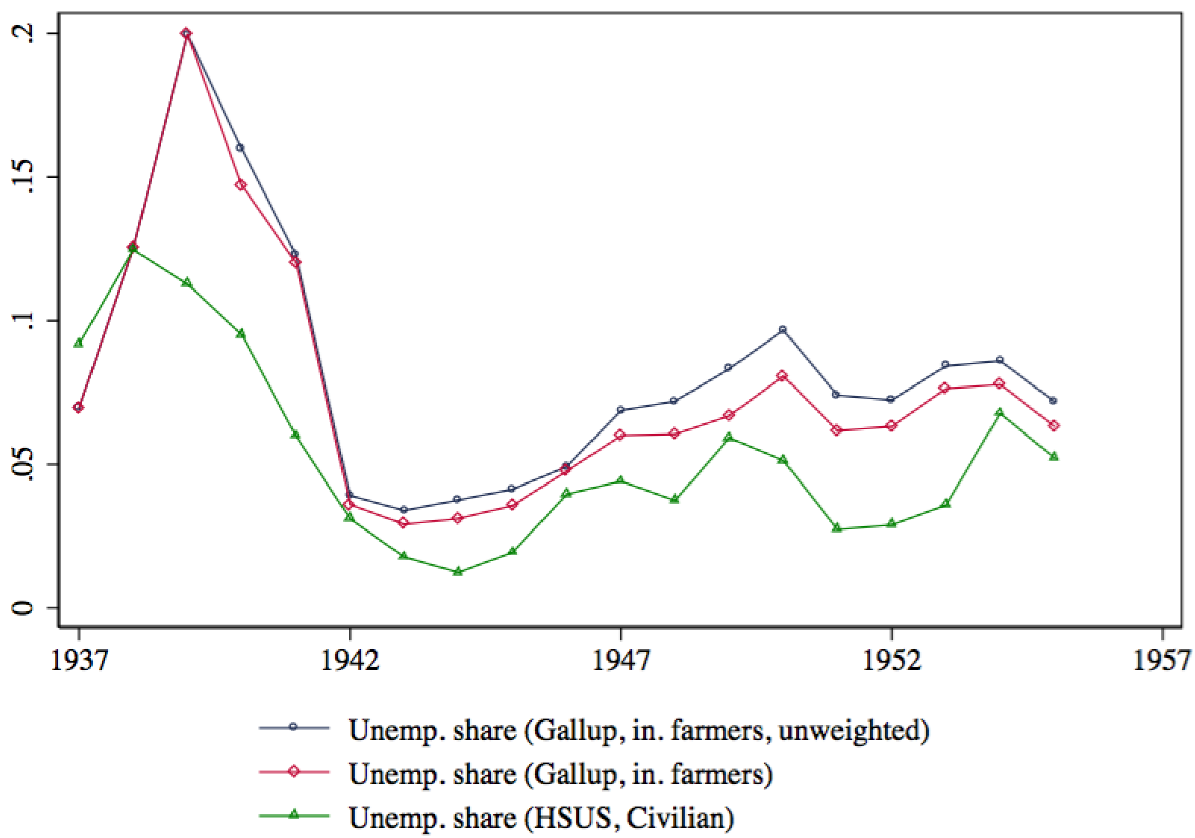
Sources: For cols. (1) through (6), dependent variables created using Census and CPS data; for (5) and (6) they are taken from Frank (2015). See Appendix for variable construction. The key explanatory variable comes from state-year average household union share generated from Gallup in the earlier years and the CPS in later years.

Notes: All estimates are from split-sample-IV regressions (see Section 7.3 for estimating equations). All regressions include state and year fixed effects; $South \times Year$ fixed effects; and state-year education controls (both from Gallup and CPS at the annual level, and interpolated from the IPUMS Census at the decade level). "Industry shares" controls for state-year share of employment in all one-digit industry categories. "State-spec. quad." indicates that state-specific quadratic time trends are included. "Income covars." indicate that state-year GDP and state-year share of households filing taxes are included. "Policy covars." indicate that state-year minimum wage and a "policy liberalism" index (from Caughey and Warshaw, 2016) are included. Sample size is larger for the top 10 outcome because it is available at the annual level in all years; for the other outcomes, until the CPS in the 1970s, we only have data from the decadal Census.

$^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

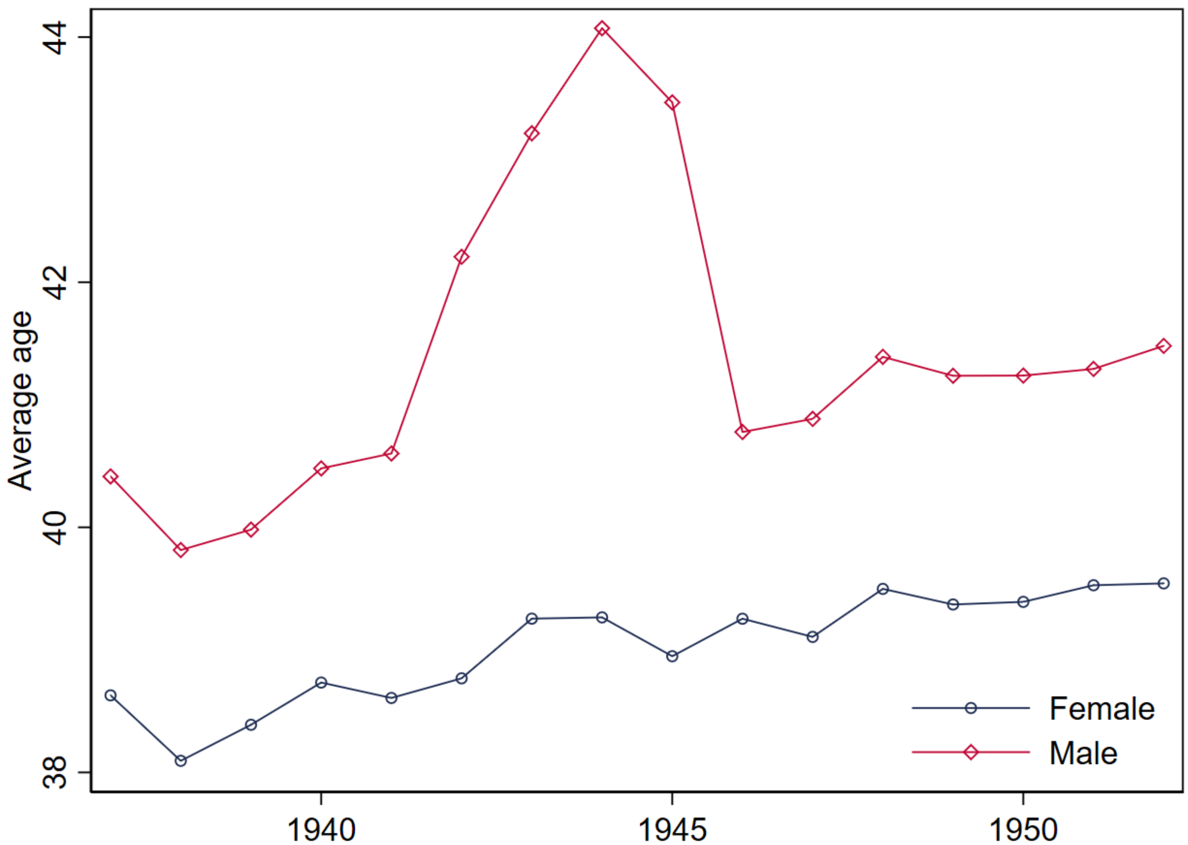
Appendix A. Supplementary figures and tables noted in the text

Appendix Figure A.1: Gallup Unemployment Rate



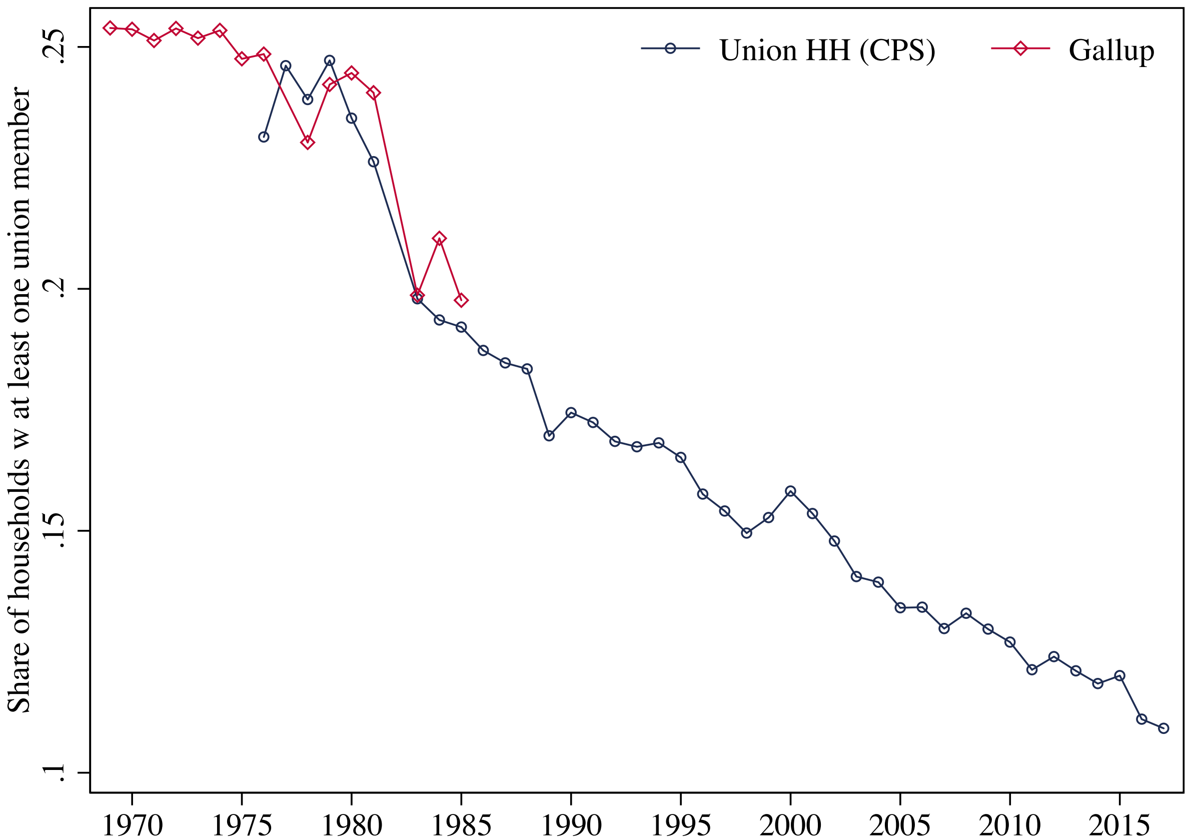
Data sources: Gallup and Historical Statistics of the United States

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



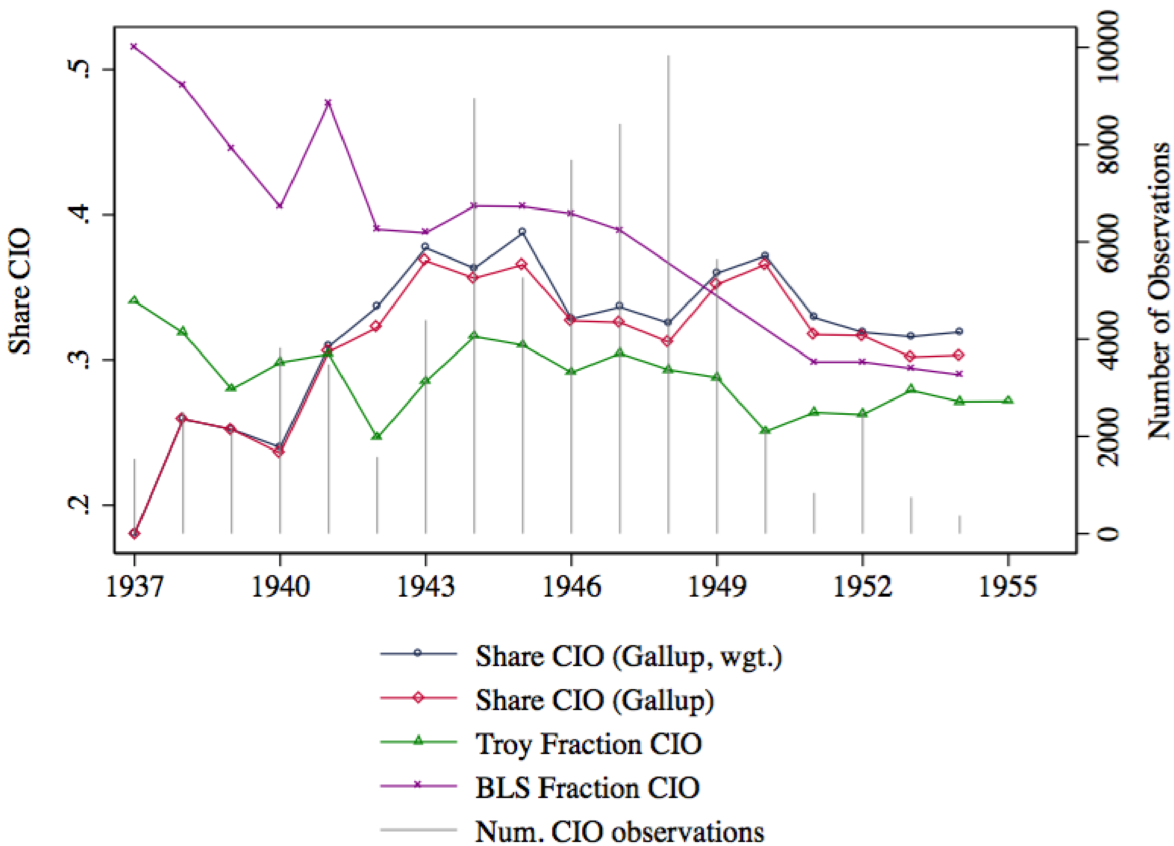
Data sources: Gallup microdata.

Appendix Figure A.3: CPS-Gallup Household Measure Comparison



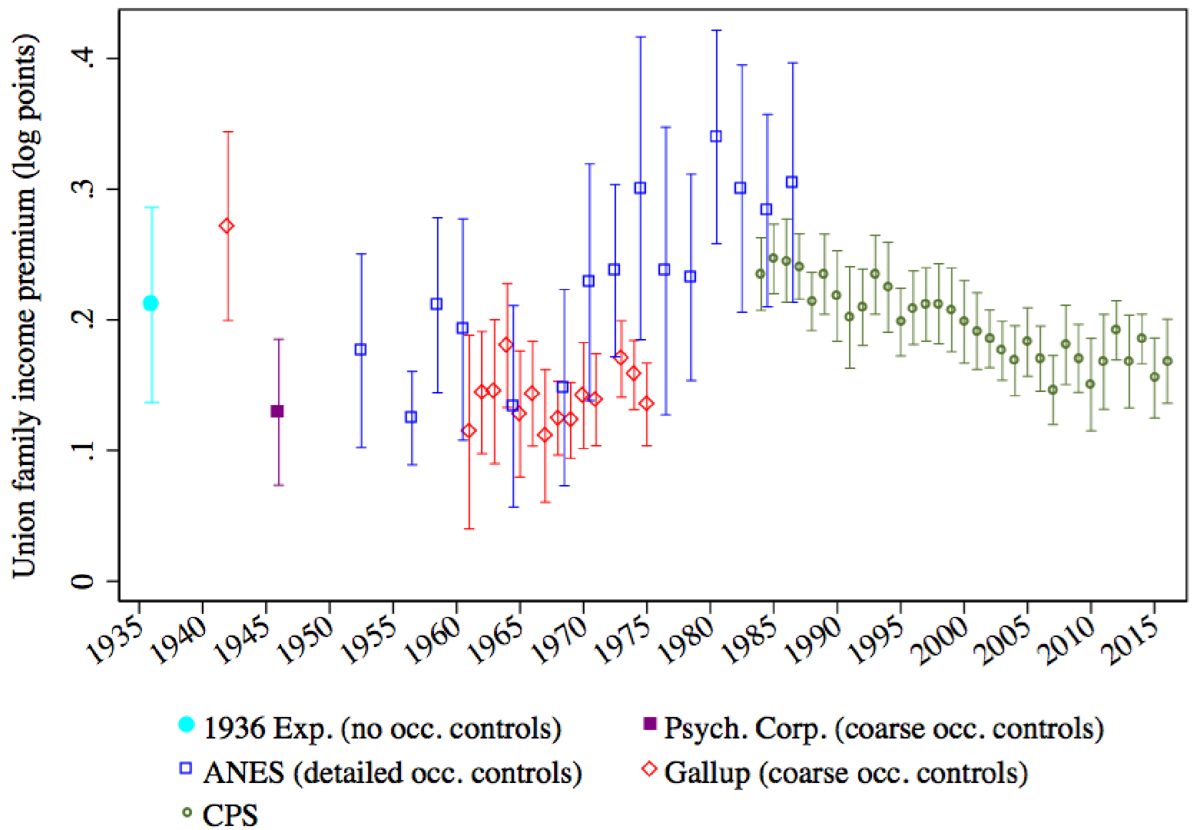
Data sources: Gallup and Current Population Survey

Appendix Figure A.4: Share CIO



Data sources: Gallup and Historical Statistics of the United States

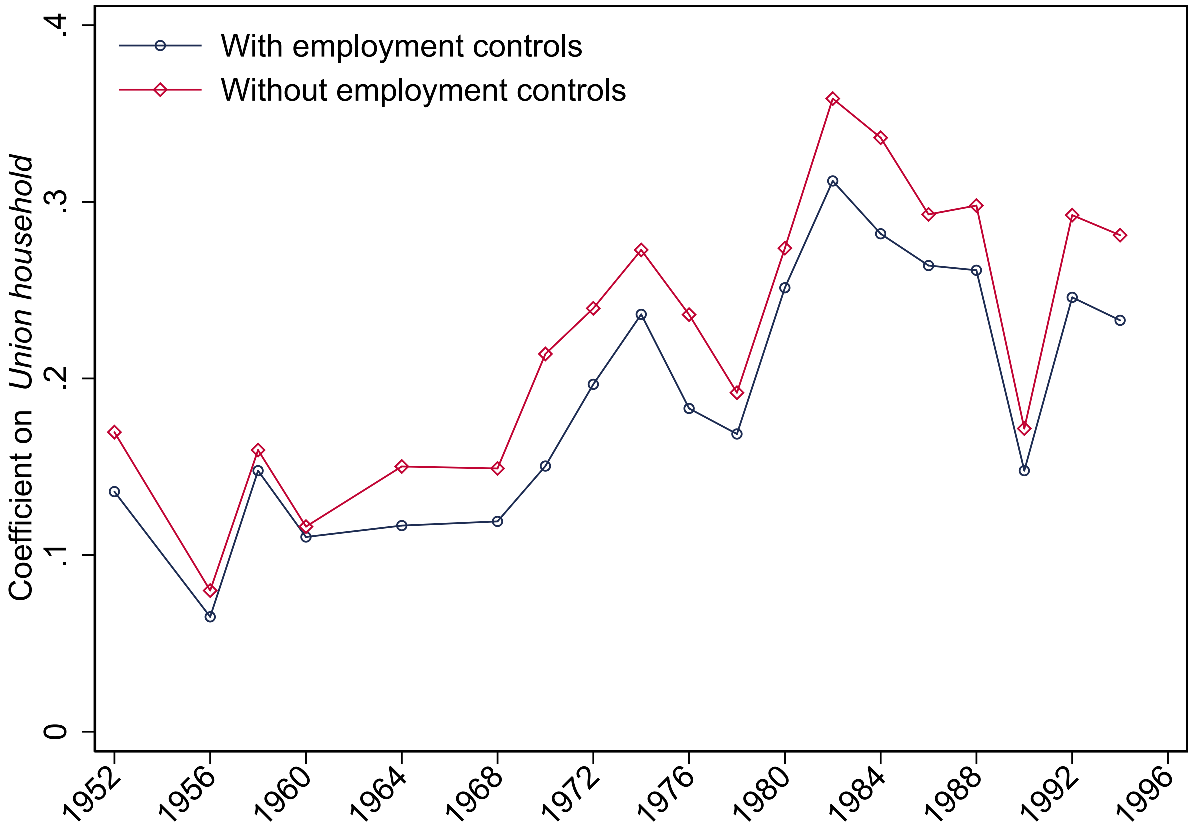
Appendix Figure A.5: Estimates of the union family income premium (including occupation controls when available)



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

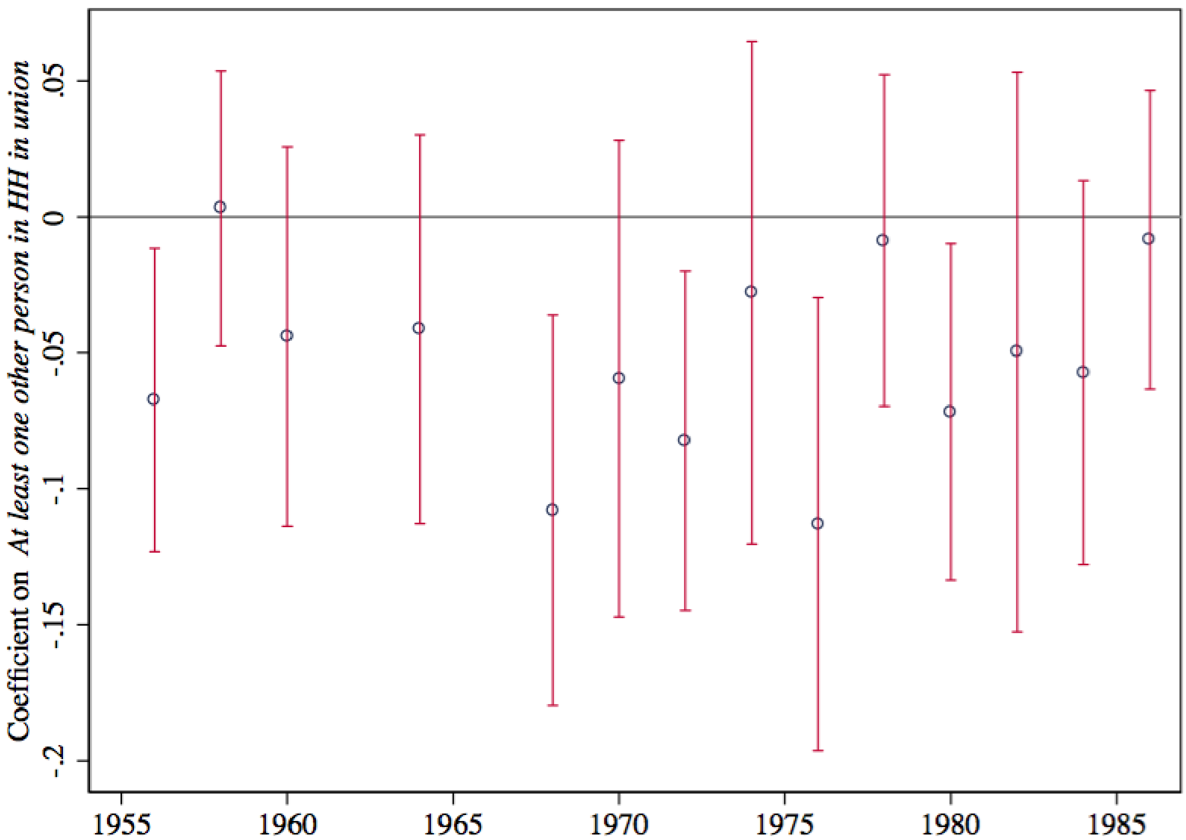
Notes: Each plotted point comes from estimating equation (1), which regressed log family income on controls for age, gender, race, state and survey-date fixed effects and (in most cases) fixed effects for the occupation of the head. The plotted confidence intervals are based on standard errors clustered by state.

Appendix Figure A.6: Estimates of the union family income premium from ANES (with and without employment status controls)



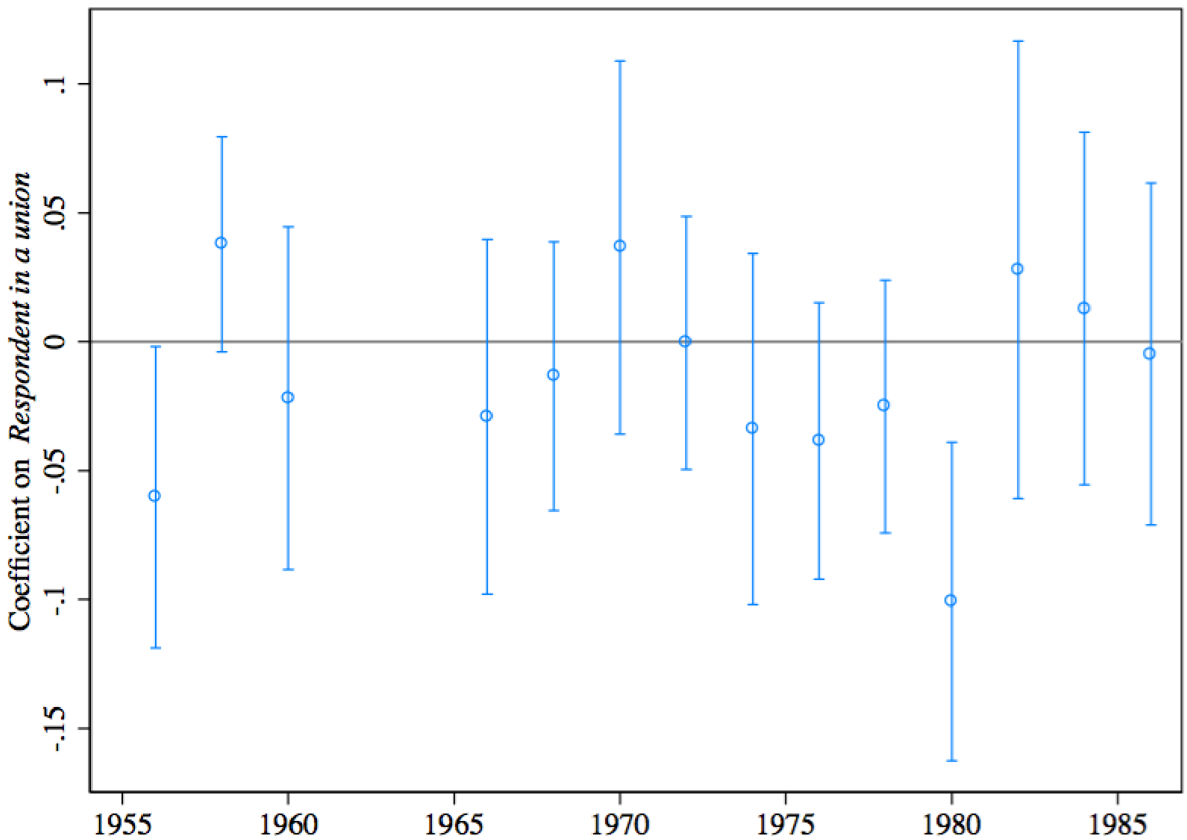
Data source: See Section 3 for a description of each data source.
Notes: Each plotted point comes from estimating equation (1), which regressed log family income on controls for age, gender, race, state and survey-date fixed effects and (in most cases) fixed effects for the occupation of the head. The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

Appendix Figure A.7: Own employment as a function of someone *else* in the household being in a union



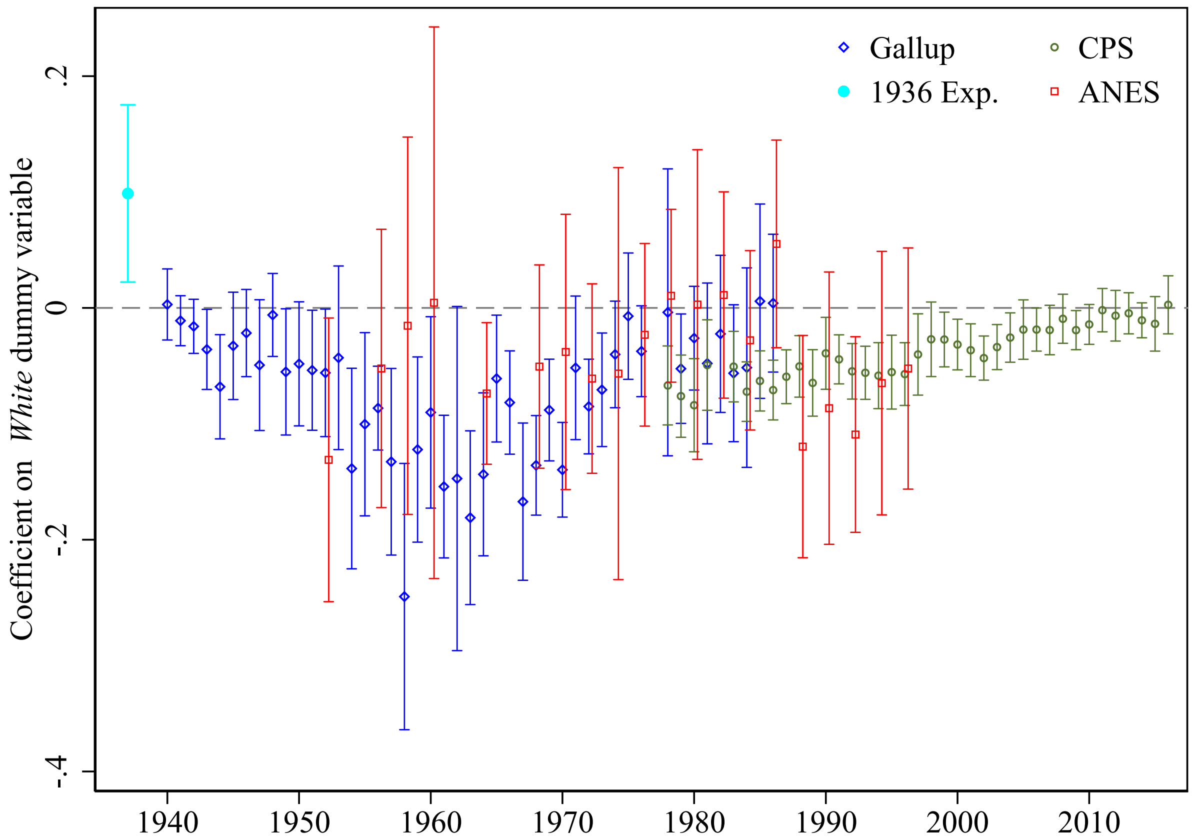
Data sources: ANES.
Notes: Each point plotted in this graph is the coefficient from regressing "am employed" on a dummy variable for "someone *else* in the household is in a union." This variable is coded as one if (a) the respondent herself is not in a union but someone else is; (b) the respondent *and* 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.8: Probability of being married as a function of individual union status



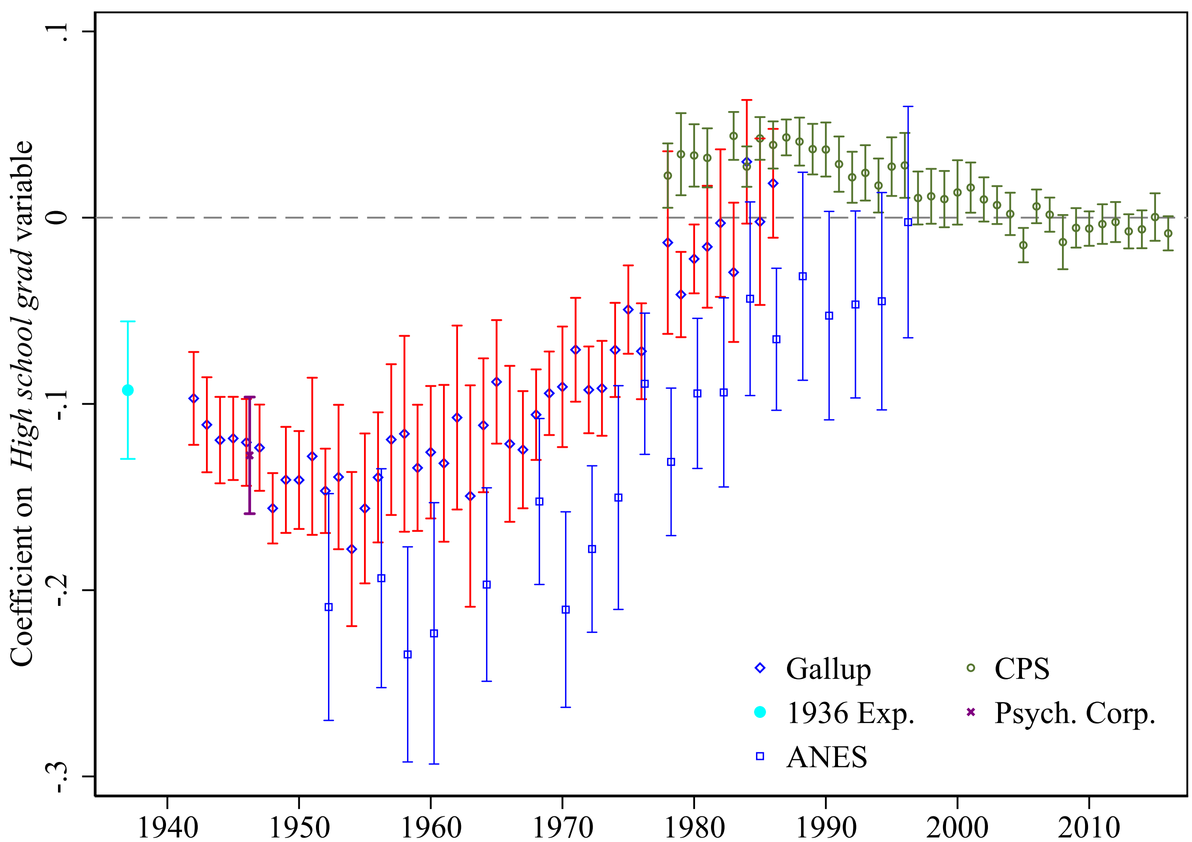
Data sources: ANES.
Notes: Each point plotted in this graph is the coefficient from regressing "am married" on a dummy variable for the *individual* being in a union. This regression pools all years and interacts "in a union" with year dummy variables. It includes controls for education categories, age 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.9: Selection of union households by race (dropping Southern states)



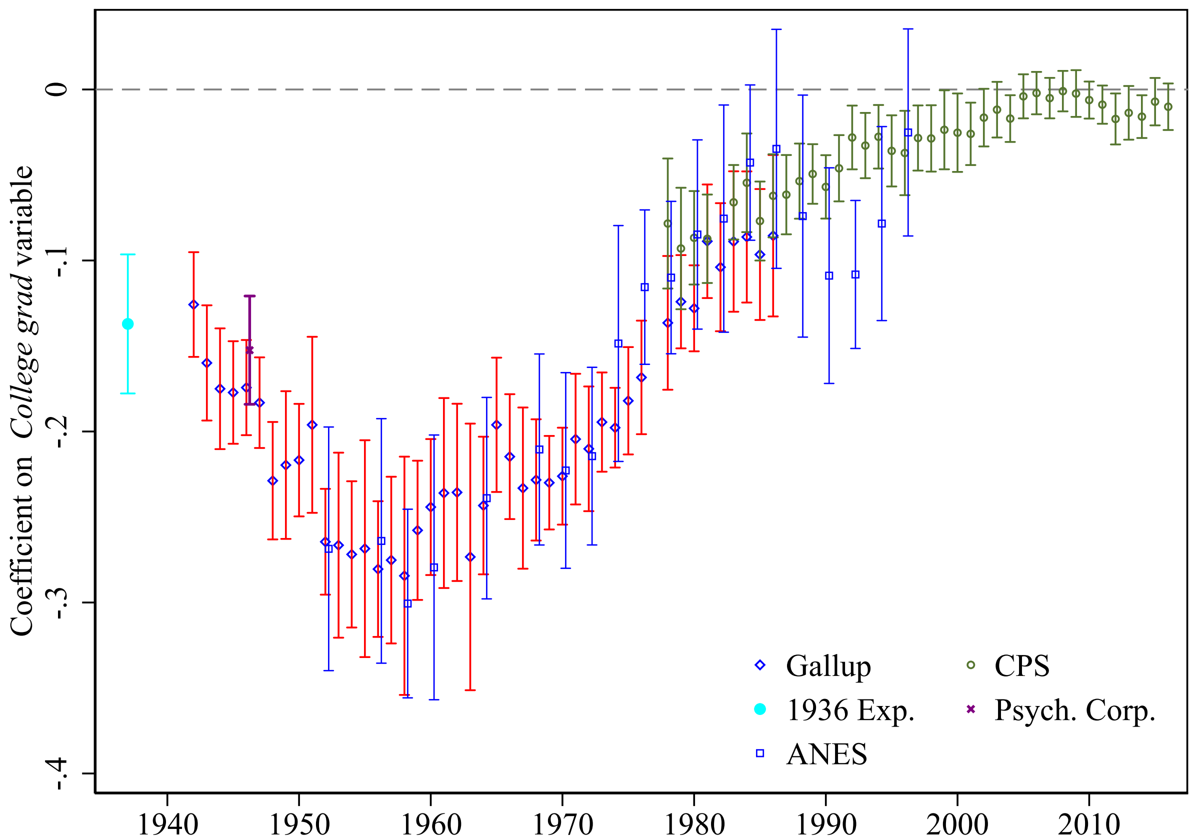
Data sources: Gallup
Notes:

Appendix Figure A.10: Selection of union households by high-school graduation



Data sources: Gallup
Notes:

Appendix Figure A.11: Selection of union households by college graduation



Data sources: Gallup
Notes:

Appendix Table A.1: Summary statistics from supplementary data sets

	(1) ANES	(2) BLS exp. dataset	(3) U.S. Psych. Corp.	(4) NORC
Union household	0.254	0.141	0.250	0.250
Female	0.548	0.0346	0.502	0.502
White	0.858	0.920	0.908	0.908
Age	39.67	40.85	3.387	3.387
HS graduate	0.360	0.405	0.375	0.375
South	0.277	0.232		
Log fam. inc.	9.380	7.121	7.823	7.823
Sample period	1952-1988	1937	1950	1946
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. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

A.1. Appendix Tables For Time Series Regressions

Appendix Table A.2: Estimating family union income premium, by data source and time period

	Dep't var: Logged family income					
	(1)	(2)	(3)	(4)	(5)	(6)
Union household	0.212***	0.255***	0.0836***	0.163***	0.150***	0.234***
	[0.0372]	[0.0348]	[0.0229]	[0.00289]	[0.0360]	[0.0189]
Years of educ., respondent		0.132***	0.108***	0.0974***	0.107***	0.111***
		[0.00549]	[0.00587]	[0.000612]	[0.00700]	[0.00465]
Years of educ., household head	0.0915***					
	[0.00766]					
White	0.632***	0.406***	0.255***	0.385***	0.394***	0.327***
	[0.0720]	[0.0618]	[0.0469]	[0.00501]	[0.0626]	[0.0381]
Respondent is female		-0.112***	-0.155***	-0.113***	-0.163***	-0.122***
		[0.0300]	[0.0254]	[0.00271]	[0.0329]	[0.0149]
Household head is female	-0.892***					
	[0.168]					
Age	0.0834***	0.0589***		0.0619***	0.0597***	0.0579***
	[0.00820]	[0.00968]		[0.000796]	[0.00765]	[0.00396]
Age squared, divided by 1,000	-0.899***	-0.671***		-0.741***	-0.679***	-0.587***
	[0.0917]	[0.120]		[0.00972]	[0.0927]	[0.0463]
Age 30-39			0.202***			
			[0.0412]			
Age 40-49			0.193***			
			[0.0295]			
Age 50-59			0.199***			
			[0.0426]			
Data source	Exp. survey	Gallup	U.S. Psych.	Gallup	ANES	ANES
Year(s) in sample	1937	1942	1946	1961-1975	1952-1970	1972-1990
Observations	4157	2524	2373	177099	2628	11777

Sources: See Section 3 for details.

Notes: All regressions include state fixed effects. For Gallup, survey date fixed effects are included and for ANES, year fixed effects. We control for number of employed individuals in the household, except in the Gallup and U.S. Psych. data, where this control is not available. For the U.S. Psych. survey, age is given in categories, not in years, and the omitted age category in the regression is "under 30" (and we drop any observation above age 60). Otherwise, all other samples include ages 21–64. Standard errors in brackets, 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)

	Dep't var: Do you (or husband) get paid vacation?				
	(1)	(2)	(3)	(4)	(5)
Union household	0.220***	0.183***	0.288**	0.280*	0.121***
	[0.0332]	[0.0308]	[0.126]	[0.143]	[0.0312]
White x Union household			-0.111		
			[0.127]		
Years educ. x Union household				-0.00964	
				[0.0130]	
Low-skill labor x Union					0.149***
					[0.0493]
Dept. var. mean	0.523	0.526	0.526	0.526	0.526
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 respodent'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.4: Ease of finding equivalent job as function of union status (Gallup, 1939)

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

Notes: Data from a Gallup survey in 1939. The dependent variable is a binary variable asking "Would you find it easy to find a job as good as the one you currently have". 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.5: Time Series Regressions: College High School Premium

	Dep't var: College Premium				
	(1)	(2)	(3)	(4)	(5)
Educ. Share Ratio	-0.369***	-0.393***	-0.386***	-0.280***	-0.351***
	(0.087)	(0.090)	(0.083)	(0.094)	(0.133)
Union Density	-0.999**		-0.686	-1.118*	-0.978
	(0.410)		(0.443)	(0.618)	(0.693)
Union Density BLS		-0.633			
		(0.449)			
Mean, dept. var	0.455	0.455	0.455	0.455	0.455
R-squared	0.949	0.945	0.948	0.955	0.955
Controls?	No	No	No	Yes	Yes
BLS IV	No	No	Yes	Yes	Yes
Time Polynomial?	Cubic	Cubic	Cubic	Cubic	Quartic
Observations	52	52	52	52	52

Sources: The college premium was created using Census and CPS data. See Appendix for variable construction. *Notes*: All regressions include "Time Polynomial" controls, either up to cubic or quartic level. "Controls" include the federal minimum wage, unemployment rate for civilian men, and top marginal tax rates. "IV BLS" uses a BLS Union Density Series as an instrument for the Gallup BLS Union Density Series. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.6: Time Series Regressions: Log 90-10 Percentile for Men

	Dep't var: Log Percentile 90-10 Men				
	(1)	(2)	(3)	(4)	(5)
Educ. Share Ratio	0.414***	0.401***	0.438***	0.182*	0.154
	(0.106)	(0.104)	(0.102)	(0.102)	(0.144)
Union Density	-2.935***		-3.393***	-3.097***	-3.042***
	(0.502)		(0.543)	(0.669)	(0.750)
Union Density BLS		-3.134***			
		(0.524)			
Mean, dept. var	1.620	1.620	1.620	1.620	1.620
R-squared	0.976	0.976	0.976	0.983	0.983
Controls?	No	No	No	Yes	Yes
BLS IV	No	No	Yes	Yes	Yes
Time Polynomial?	Cubic	Cubic	Cubic	Cubic	Quartic
Observations	52	52	52	52	52

Sources: The Log 90-10 Percentile for Men was created using Census and CPS data. See Appendix for further details. *Notes*: All regressions include "Time Polynomial" controls, either up to cubic or quartic level. "Controls" include the federal minimum wage, unemployment rate for civilian men, and top marginal tax rates. "IV BLS" uses a BLS Union Density Series as an instrument for the Gallup BLS Union Density Series. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.7: Time Series Regressions: Gini Coefficient

	Dep't var: Gini Coefficient				
	(1)	(2)	(3)	(4)	(5)
Union Density	-0.085*		-0.234***	-0.188**	-0.235**
	(0.045)		(0.082)	(0.088)	(0.098)
Union Density BLS		-0.121***			
		(0.039)			
Mean, dept. var				0.410	0.410
R-squared	0.984	0.986	0.981	0.984	0.984
Educ. Control	Yes	Yes	Yes	Yes	Yes
Addit. controls?	No	No	No	Yes	Yes
BLS IV?	No	No	Yes	Yes	Yes
Time Polynomial?	Cubic	Cubic	Cubic	Cubic	Quartic
Observations	65	65	65	65	65

Sources: The Gini Coefficient is calculated from social security data by Kopczuk *et al.* (2010). *Notes*: All regressions include “Time Polynomial“ controls, either up to cubic or quartic level. “Controls“ include the federal minimum wage, unemployment rate for civilian men, and top marginal tax rates. “IV BLS“ uses a BLS Union Density Series as an instrument for the Gallup BLS Union Density Series. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$ *Sources*:

Appendix Table A.8: Time Series Regressions: Top 10 Percent Income Share

	Dep't var: Top 10 Income Share				
	(1)	(2)	(3)	(4)	(5)
Union Density	-18.611		-61.910***	-38.252**	-40.609*
	(11.452)		(18.296)	(18.448)	(24.248)
Union Density BLS		-37.903***			
		(9.842)			
Mean, dept. var				36.319	36.319
R-squared	0.945	0.953	0.933	0.959	0.958
Educ. Control	Yes	Yes	Yes	Yes	Yes
Addit. controls?	No	No	No	Yes	Yes
BLS IV?	No	No	Yes	Yes	Yes
Time Polynomial?	Cubic	Cubic	Cubic	Cubic	Quartic
Observations	73	73	73	73	73

Sources: The Top 10 percent income share is calculated from IRS data and updated by Piketty and Saez (2003). *Notes*: All regressions include “Time Polynomial“ controls, either up to cubic or quartic level. “Controls“ include the federal minimum wage, unemployment rate for civilian men, and top marginal tax rates. “IV BLS“ uses a BLS Union Density Series as an instrument for the Gallup BLS Union Density Series. “Educ. Controls“ include College-High School Share Ratio using CPS, Gallup Data and Census (ipolated) data. See Appendix for variable construction. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

A.2. Appendix Tables For State-Year Regressions

Appendix Table A.9: State-Year Regressions: College High School Premium

	Dep't var: College Premium					
	(1)	(2)	(3)	(4)	(5)	(6)
HH Union Density Split A	-0.090	-0.308**	-0.306**	-0.321***	-0.244**	-0.251**
	0.058	0.121	0.122	0.112	0.112	0.112
Mean, dept. var.	0.416	0.416	0.416	0.416	0.416	0.416
R-squared	0.536	0.529	0.531	0.543	0.607	0.608
Education Control	Yes	Yes	Yes	Yes	Yes	Yes
Industry Shares	No	No	No	Yes	Yes	Yes
Income covars.	No	No	Yes	Yes	Yes	Yes
Policy covars.	No	No	No	No	No	Yes
Split-Sample IV	No	Yes	Yes	Yes	Yes	Yes
State-spec. quad.	No	No	No	No	Yes	Yes
Min. Year	1940	1940	1940	1940	1940	1940
Max. Year	2012	2012	2012	2012	2012	2012
Observations	1781	1781	1781	1781	1781	1781

Sources: College High School Premium was created using Census and CPS data. See Appendix for variable construction. *Notes*: IV estimates are from split-sample-IV regressions (see Section 7.3 for estimating equations). All regressions include state and year fixed effects; $South \times Year$ fixed effects; and state-year education controls (both from Gallup and CPS at the annual level, and interpolated from the IPUMS Census at the decade level). "Industry shares" controls for state-year share of employment in all one-digit industry categories. "State-spec. quad." indicates that state-specific quadratic time trends are included. "Income covars." indicate that state-year GDP and state-year share of households filing taxes are included. "Policy covars." indicate that state-year minimum wage and a "policy liberalism" index (from Caughey and Warshaw, 2016) are included. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.10: State-Year Regressions: Log 90-10 Percentile for Men

	Dep't var: Log Percentile 90-10 Men					
	(1)	(2)	(3)	(4)	(5)	(6)
HH Union Density Split A	-0.380***	-0.714**	-0.648**	-0.648***	-0.407**	-0.414**
	0.124	0.307	0.278	0.225	0.192	0.191
Mean, dept. var.	1.543	1.543	1.543	1.543	1.543	1.543
R-squared	0.463	0.457	0.515	0.576	0.703	0.703
Education Control	Yes	Yes	Yes	Yes	Yes	Yes
Industry Shares	No	No	No	Yes	Yes	Yes
Income covars.	No	No	Yes	Yes	Yes	Yes
Policy covars.	No	No	No	No	No	Yes
Split-Sample IV	No	Yes	Yes	Yes	Yes	Yes
State-spec. quad.	No	No	No	No	Yes	Yes
Min. Year	1940	1940	1940	1940	1940	1940
Max. Year	2012	2012	2012	2012	2012	2012
Observations	1781	1781	1781	1781	1781	1781

Sources: Log 90-10 Percentile for Men was created using Census and CPS data. See Appendix for variable construction. *Notes*: IV estimates are from split-sample-IV regressions (see Section 7.3 for estimating equations). All regressions include state and year fixed effects; $South \times Year$ fixed effects; and state-year education controls (both from Gallup and CPS at the annual level, and interpolated from the IPUMS Census at the decade level). "Industry shares" controls for state-year share of employment in all one-digit industry categories. "State-spec. quad." indicates that state-specific quadratic time trends are included. "Income covars." indicate that state-year GDP and state-year share of households filing taxes are included. "Policy covars." indicate that state-year minimum wage and a "policy liberalism" index (from Caughey and Warshaw, 2016) are included. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.11: State-Year Regressions: Gini Coefficient

	Dep't var: Gini Coefficient					
	(1)	(2)	(3)	(4)	(5)	(6)
HH Union Density Split A	-0.033***	-0.091***	-0.080***	-0.079***	-0.048*	-0.049*
	0.011	0.026	0.025	0.024	0.026	0.026
Mean, dept. var.	0.351	0.351	0.351	0.351	0.351	0.351
R-squared	0.609	0.599	0.623	0.657	0.742	0.742
Education Control	Yes	Yes	Yes	Yes	Yes	Yes
Industry Shares	No	No	No	Yes	Yes	Yes
Income covars.	No	No	Yes	Yes	Yes	Yes
Policy covars.	No	No	No	No	No	Yes
Split-Sample IV	No	Yes	Yes	Yes	Yes	Yes
State-spec. quad.	No	No	No	No	Yes	Yes
Min. Year	1940	1940	1940	1940	1940	1940
Max. Year	2012	2012	2012	2012	2012	2012
Observations	1781	1781	1781	1781	1781	1781

Sources: The Gini Coefficient was created using Census and CPS data. See Appendix for variable construction. *Notes*: IV estimates are from split-sample-IV regressions (see Section 7.3 for estimating equations). All regressions include state and year fixed effects; $South \times Year$ fixed effects; and state-year education controls (both from Gallup and CPS at the annual level, and interpolated from the IPUMS Census at the decade level). "Industry shares" controls for state-year share of employment in all one-digit industry categories. "State-spec. quad." indicates that state-specific quadratic time trends are included. "Income covars." indicate that state-year GDP and state-year share of households filing taxes are included. "Policy covars." indicate that state-year minimum wage and a "policy liberalism" index (from Caughey and Warshaw, 2016) are included. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.12: State-Year Regressions: Top 10 Percent Income Share

	Dep't var: Top 10 Income Share					
	(1)	(2)	(3)	(4)	(5)	(6)
HH Union Density Split A	-2.402**	-5.009**	-5.728***	-4.885***	-3.485**	-3.528**
	0.983	2.163	2.168	1.708	1.469	1.466
Mean, dept. var.	36.987	36.989	36.989	36.989	36.989	36.989
R-squared	0.796	0.794	0.818	0.843	0.919	0.919
Education Control	Yes	Yes	Yes	Yes	Yes	Yes
Industry Shares	No	No	No	Yes	Yes	Yes
Income covars.	No	No	Yes	Yes	Yes	Yes
Policy covars.	No	No	No	No	No	Yes
Split-Sample IV	No	Yes	Yes	Yes	Yes	Yes
State-spec. quad.	No	No	No	No	Yes	Yes
Min. Year	1940	1940	1940	1940	1940	1940
Max. Year	2012	2012	2012	2012	2012	2012
Observations	3252	3251	3251	3251	3251	3251

Sources: The Top 10 percent share of income data comes from Frank (2015) and is discussed in the text. *Notes*: IV estimates are from split-sample-IV regressions (see Section 7.3 for estimating equations). All regressions include state and year fixed effects; $South \times Year$ fixed effects; and state-year education controls (both from Gallup and CPS at the annual level, and interpolated from the IPUMS Census at the decade level). "Industry shares" controls for state-year share of employment in all one-digit industry categories. "State-spec. quad." indicates that state-specific quadratic time trends are included. "Income covars." indicate that state-year GDP and state-year share of households filing taxes are included. "Policy covars." indicate that state-year minimum wage and a "policy liberalism" index (from Caughey and Warshaw, 2016) are included.
$^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.13: State-Year Regressions: Log State GDP/Cap

	Dep't var: Log GDP Per Capita					
	(1)	(2)	(3)	(4)	(5)	(6)
HH Union Density Split A	0.097***	0.204***	0.211***	0.166***	0.005	0.005
	0.025	0.050	0.048	0.049	0.036	0.036
Mean, dept. var.	-5.303	-5.303	-5.303	-5.303	-5.303	-5.303
R-squared	0.997	0.997	0.998	0.998	0.999	0.999
Education Control	Yes	Yes	Yes	Yes	Yes	Yes
Industry Shares	No	No	No	Yes	Yes	Yes
Share returns.	No	No	Yes	Yes	Yes	Yes
Other covars.	No	No	No	No	No	Yes
Split-Sample IV	No	Yes	Yes	Yes	Yes	Yes
State-spec. quad.	No	No	Yes	No	Yes	Yes
Min. Year	1940	1940	1940	1940	1940	1940
Max. Year	2012	2012	2012	2012	2012	2012
Observations	3252	3251	3251	3251	3251	3251

Sources: Log State GDP/Cap data comes from Frank (2015). *Notes*: IV estimates are from split-sample-IV regressions (see Section 7.3 for estimating equations). All regressions include state and year fixed effects; $South \times Year$ fixed effects; and state-year education controls (both from Gallup and CPS at the annual level, and interpolated from the IPUMS Census at the decade level). "Industry shares" controls for state-year share of employment in all one-digit industry categories. "State-spec. quad." indicates that state-specific quadratic time trends are included. "Income covars." indicate that state-year GDP and state-year share of households filing taxes are included. "Policy covars." indicate that state-year minimum wage and a "policy liberalism" index (from Caughey and Warshaw, 2016) are included. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Appendix Table A.14: State-Year Regressions: Policy Outcomes

	Coll. premium		90/10 ratio		Gini coeff.		Top 10 share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Household union share	0.168 [0.164]	0.0864 [0.113]	-0.101 [0.207]	0.0132 [0.0631]	0.0710 [0.114]	-0.0208 [0.0743]	0.416 [0.509]	0.184 [0.142]
Mean, dept. var.	0.650	0.650	2.910	2.910	1.694	1.694	-0.00691	-0.00691
Industry shares	No	Yes	No	Yes	No	Yes	No	Yes
State-spec. quad.	No	Yes	No	Yes	No	Yes	No	Yes
Income covars.	No	Yes	No	Yes	No	Yes	No	Yes
Policy covars.	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3157	3157	3251	3251	2915	2915	3251	3251

Sources: Minimum wage indicator, tax/GDP ratio, and policy liberalism index all come from Caughey and Warshaw (2016). *Notes*: IV estimates are from split-sample-IV regressions (see Section 7.3 for estimating equations). All regressions include state and year fixed effects; $South \times Year$ fixed effects; and state-year education controls (both from Gallup and CPS at the annual level, and interpolated from the IPUMS Census at the decade level). "Industry shares" controls for state-year share of employment in all one-digit industry categories. "State-spec. quad." indicates that state-specific quadratic time trends are included. "Income covars." indicate that state-year GDP and state-year share of households filing taxes are included.
$^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

A.3. Variable Construction Details

A.3.1. Union Density

We use the CPS to construct both household union density at the aggregate time-series level as well as the state year level. We use the May CPS from 1976 to 1981 and both the May and March CPS from 1983 to 2017, to calculate union density at the household level. We consider a household "union" when at least one person over 21 years old is an union member. For state year measures, the state identifiers are only available starting in 1979, and so our CPS-based series begins there. We excluded Alaska, DC and Hawaii from time series, additionally we excluded Idaho from the state year analysis in order to match it with the Gallup Data Sample.

A.3.2. Family Income Premium

For the union family income premium we use the family income variable, which is binned into 12 categories. These categories change in 2002, leading to considerable top-coding.

A.3.3. College High School Share Ratio

We use CPS May-March (1964 to 2017) to calculate the college-high school share ratio. For individuals over 21 years old, we calculate the shares for 3 levels of education : up to high school, some college and more than college (more than 16 years of education). The college/high school share ratio is the natural logarithm of the ratio of college share plus half of some college share to high school share plus half of some college share in each year. Using these shares, we calculate the logarithm of this ratio described in following equation:

$$CollegeHSShareRatio_t = \frac{ShareCollege_t + 0.5 * ShareSomeCollege_t}{ShareHighSchool_t + 0.5 * ShareSomeCollege_t} \tag{9}$$

We do a similar exercise using the Census (1940-2010) and then we interpolate them to complete the time series. In the case of the Gallup Data, we calculate the college-high school share ratio using the same procedure.

A.3.4. College High School Premium, 90-10 Wage Ratio and Gini Coefficient

We use CPS May-March (1964 to 2017). We follow Goldin and Katz (2009) and calculate the College High School Premium following for workers of 21-64 years old. The estimated college/high school log wage premium is a fixed weighted average of the college and some college/high school wage gaps. The premium is estimated using the logarithm of weekly earnings for college/some college workers relative to high school graduates workers. The estimation controls include a full-time dummy, a female dummy, a non-white dummy, a quartic in experience and the interaction of female with non-white and the quartic in experience. The estimated log of percentiles 10, 50 and 90 were calculated using the logarithm of weekly earnings from 21-64 years old workers (excluding armed forces). We set unemployed and NILF to zero. We do a similar exercise using the Census (1940-2010) for these variables and we use it to complete the series.