Did Organized Labor Induce Labor? Unionization and the American Baby Boom

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Abstract

Conventional economic theories cannot fully explain the timing, duration, and size of the American Baby Boom. I propose a new explanation: the rise of the labor movement. Union density more than tripled following the passage of the 1935 National Labor Relations Act (NLRA). To test unionization's contribution to fertility increases, I construct novel county-level estimates of union membership and exploit local variation in exposure to the NLRA shock. Union growth has positive impacts on both birth rates and completed fertility. Effects are driven primarily by wage growth, protection against adverse labor market shocks, and impacts on female labor force participation.

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1 Introduction

The Baby Boom was a demographic event unlike any other in modern history, with economic and social aftershocks that are still felt today. The U.S. Baby Boom was especially large, and is typically dated as occurring between 1946 and 1964 (Colby and Ortman, 2014). This convention reflects the commonly-held belief that fertility increases resulted from the end of World War II and the subsequent postwar economic expansion. However, such classic accounts of the Baby Boom fail to predict the timing of fertility increases, which began well before the end of the war.[1] Unforeseen by demographers at the time and still elusive today, the fundamental causes of Baby Boom remain "one of the 20th century's great puzzles" (Bailey and Collins, 2011).

In this paper, I contribute a key missing piece to this puzzle by proposing a novel explanation for the Baby Boom: the rise of organized labor. Through the late 19th and early 20th centuries, labor unions did not have federally-recognized rights to represent workers or bargain collectively on their behalf. Nearly moribund by the end of the 1920s, the labor movement was unexpectedly revived during the New Deal era, as a series of federal labor laws reshaped the role of unions in the American economy. The 1935 National Labor Relations Act (NLRA, or "Wagner Act") was the centerpiece of this new legal regime. Under the NLRA, private sector workers gained legal rights to organize into trade unions, bargain collectively for rights and benefits, and engage in work stoppages and strikes. Union membership increased sharply in the years following the NLRA, and surged again during World War II. At mid-century, organized labor was the most influential labor market institution in the American economy, as landmark collective bargaining agreements defined wages, benefits, and working conditions for millions of unionized workers and set powerful precedents for many more unorganized workers. Figure 1 plots national union membership rates and birth rates during the 20th century, and provides *prima facie* evidence for a positive relationship between unionization and lagged birth rates beginning around the time of the NLRA's enactment.

An extensive literature documents the benefits of union membership for workers and their families and suggests several plausible mechanisms through which unionization may have influenced fertility. Union workers earned 10-20% more than comparable non-union workers throughout the mid-20th century (Farber et al., 2021). And, despite high inflation, real wages more than doubled in this period (Williamson, 1995) in part due to automatic cost-of-living adjustments negotiated through collective bargaining agreements (Barnard and Handlin, 1983). Union membership may have therefore increased fertility through an income channel, especially since union members of this era were predominantly male breadwinners for whom income effects likely dominated (Becker, 1960).[2] In addition to income effects, union membership may have promoted marital and family formation by insuring workers and their families against labor market risks and work-related uncertainty,[3] or by reducing the risks of childbearing through access to

[1] The earliest recorded reference to a "baby boom" in the U.S. is from a *Life* magazine article published December 1, 1941 – six days before Pearl Harbor and the U.S.'s official entry into World War II (Hogan et al., 2008).

[2] Women historically made up large proportions of certain unions, including those in the communications and garment industries. However, women made up less than 10% of overall union membership prior to 1940. Female union membership as a percentage of overall union membership peaked during WWII (22%), but decreased in the postwar years (17% in 1954). (Cobble, 1960).

[3] Prior work finds that union membership reduces the likelihood of job separations (Polsky, 1999; Braakmann and Hirsch, 2023), improves access to unemployment insurance (Budd and McCall, 1997) and workers' compensation (Hirsch et al., 1997), and increases occupational safety and worker protections (Boal, 2009; Morantz, 2013; Sojourner and Yang, 2022). Some collective bargaining agreements in the postwar era also guaranteed workers an income that approximated their normal

collectively-bargained healthcare.[4]

My first set of results draws on cross-sectional data from a variety of opinion polls and economic surveys administered between 1941 and 1956. I find that union households had 10-20% larger families than non-union households, and this result is not driven by observable differences in age, race, urbanicity, state of residence, or occupation. These results provide basic descriptive evidence for a positive relationship between unionization and fertility. However, such comparisons may fail to capture spillover effects across households. On the one hand, if union growth negatively impacts economic outcomes in the non-union sector, the aggregate effect of unionization on fertility could be null, or even negative. On the other hand, if the threat of unionization induces unorganized firms to proactively offer increased benefits to retain their workers (Lewis, 1963; Farber, 2005; Zuberi, 2019), household-level comparisons may understate the equilibrium impact of unionization.

Moving beyond the cross-sectional results, an ideal empirical design would estimate the causal impacts of unionization on fertility, net of spillover effects within local labor markets. There are two primary obstacles associated with this empirical approach. First, place-level data on historic union membership rates are not available from any existing source until after the enactment of the NLRA.[5] Second, the mid-20$^{\text{th}}$ century was an eventful period in American economic history, and it is challenging to disentangle the causal effect of unionization from many confounding factors that may have influenced fertility. For example, during periods of economic expansion, unionization tends to increase due to tight labor markets (Ashenfelter and Pencavel, 1969). But economic growth may positively impact fertility independent of union growth.

To overcome limitations of existing data, I introduce novel place-level data on local union membership in several large states from 1920-1961. I draw primarily on data digitized from annual convention proceedings of state chapters of the American Federation of Labor (AFL), the Congress of Industrial Organizations (CIO), and the merged AFL-CIO, in addition to national proceedings of several large independent unions. In cases where these documents include disaggregated information on financial receipts and union voting strength, it is possible to infer union membership by location, and then aggregate to form county-level estimates. My main analysis sample is comprised of over 400 counties in five states – California, Illinois, Missouri, Pennsylvania, and Wisconsin – for which such disaggregated measures of membership levels are consistently available. These data represent the first estimates of county-level union membership during the peak of the American labor movement, and the first series of sub-national estimates spanning the years before and after the NLRA.

To isolate plausibly exogenous variation in local exposure to the NLRA shock, I construct a Bartik-style shift-share instrument (SSIV) that combines temporal variation in national union membership rates at the industry level with cross-sectional variation in pre-determined local industry shares (Bartik, 1991).

wage throughout the year, which was particularly important in industries like mining and auto manufacturing that were subject to large cyclical and seasonal fluctuations (Barnard and Handlin, 1983).

[4]The large postwar expansion of employer-based healthcare was concentrated in unionized workplaces (Thomasson, 2002). A series of policy and Supreme Court decisions in the late 1940s made it possible for unions to negotiate for health benefits as a part of collective bargaining agreements. The number of workers covered by health insurance negotiated by unions increased from 600,000 in 1946 to 12 million workers and 17 million dependents in 1954, which accounted for approximately 25% of all health insurance in the U.S. (Starr, 1978)

[5]Farber et al. (2021) introduce state-level estimates of historic union membership using Gallup survey data. However, Gallup did not begin asking about union membership status until 1937, so it is not possible to assess pre-NLRA trends using these data.

The relevance of the instrument stems from the empirical fact that some industries have historically been more amenable to unionization than others, in part due to differences in cost structures and the substitutability of capital and labor (Grout, 1984). Intuitively, the instrument therefore captures variation in local union growth that (1) is related to an area's long-standing, latent demand for unionization, but (2) would not have been realized absent the national policy shock of the NLRA. In this setting, identification follows from the assumption that any unobserved factors that affect changes in unionization are not jointly correlated with lagged local industry shares and changes in fertility. To strengthen the case that shares are (conditionally) exogenous, I control for a battery of local demographic and economic characteristics, measured at pre-period levels and interacted with time fixed effects, in all baseline specifications.

I first measure the impact of unionization on birth rates using a two-period long-difference model that compares outcomes before and after the passage of the NLRA. Instrumental variable (IV) estimates indicate that a 10 percentage point (pp) increase in local union membership rates is associated with 9 more births per 1000 women of childbearing age, a 15% increase over the base period mean. A back-of-the-envelope calculation suggests that union growth can account for approximately 25% of the overall increase in birth rates between 1934 and 1960. I show that the results are robust to varying the start and end years of the long-difference, dropping urban and rural outliers, specifying various lag relationships between treatment and outcomes, and controlling for a variety of period-specific shocks, including exogenous shocks to labor demand, New Deal spending, and impacts of World War II. I also segment counties into treatment groups based on SSIV-predicted exposure to the NLRA shock and estimate reduced-form event studies to test key identifying assumptions. I find that, conditional on the inclusion of baseline controls, birth rates evolved similarly across areas prior to treatment. However, consistent with the timing of the NLRA shock, outcome paths diverged shortly after 1935, as high treatment counties experienced much greater growth in birth rates relative to low treatment counties. Notably, the time path of treatment effects tends to track important developments in organized labor, but not expansions and contractions in the business cycle.

Given that I can only estimate treatment effects for the subset of states with available county-level union membership data, a natural concern is that the results may not generalize beyond the main sample. I extend the analysis in two ways to address this concern. First, I estimate the reduced-form relationship between the SSIV and birth rates for a sample of all U.S. counties; second, I incorporate data from additional sources to measure effects of unionization on birth rates at the state-level. In both cases, I detect effects that are comparable to those in the main sample. The fact that the positive relationship between unionization and fertility is robust across varying levels of geography and unrelated data sources provides strong supporting evidence for the main results.

Birth rates capture flows into childbearing, and thus provide the best measure of the contemporaneous response of fertility to local union shocks. However, the Baby Boom was marked not only by high birth rates, but also by large increases in completed fertility – the stock of children born by the end of a woman's childbearing years.[6] To test for durable impacts on fertility and rule out the possibility that treatment caused families to change only the *timing* of births, I measure the effect of unionization on average completed fertility across birth year cohorts. I construct novel estimates of average completed fertility

[6]A key point raised by Van Bavel and Reher (2013) and other related work is that high birth rates during the Baby Boom did not simply reflect the realization of births postponed from the Depression or World War II. Moreover, high fertility among young women during the Baby Boom was not fully compensated by lower fertility in later childbearing years.

at the county level for synthetic birth year cohorts using restricted-use versions of the U.S. Decennial Censuses. In this case, the long-difference compares outcomes for cohorts of women who reached peak childbearing age before and after the enactment of the NLRA. I find that women with greater exposure to the union shock had more children by the end of their childbearing years, with effects concentrated after age 25. Overall, union growth can account for approximately 20% of the overall increase in completed fertility during this period.

By design, aggregate county-level effects subsume both direct effects (i.e., those on union households in treated areas) and spillovers (effects on non-union households in treated areas, and on union households in non-treated areas). To shed light on the role of spillovers, I decompose the equilibrium effects of unionization on completed fertility using within-county variation in exposure to treatment. Specifically, I test for treatment effect heterogeneity based on the industry of the household head. I find that the county-level effect of unionization on completed fertility is driven primarily by households with heads employed in the industries most likely to be directly affected by the NLRA shock. This treatment effect heterogeneity persists even in areas that experienced little to no actual increase in unionization, which suggests the presence of within-industry spillover effects.

Fertility increases during the Baby Boom were driven in part by a "Marriage Boom", as the rate of new marriages more than doubled between 1932 and 1945 (Schellekens, 2017). I supplement the main fertility results by estimating effects on marriage rates, and find that exposure to unionization is associated with higher marriage rates among women aged 25-35. These results suggest that unionization influenced fertility in part by speeding up the formation of new marriages, and thus extending the window for childbearing within marriages.

I close the empirical analysis by analyzing the contribution of various economic mechanisms to the overall effect of unionization on fertility. The results suggest that unionization influenced fertility through two main channels. First, labor unions secured economic gains for their majority-male members, which increased household resources and provided insurance against labor market risks. Second, the growing influence of unions contributed to the contraction of female labor force participation after WWII, and thus lowered the opportunity cost of childbearing for young women. Indeed, wage gains and reductions in female employment in this period were often complementary pieces of bargains struck between employers and unions to pay workers a "family wage", which ensured that male breadwinners could support their families while women provided unpaid labor in the home (Macunovich, 2010).[7]

This paper is the first to link the economic impacts of widespread unionization to fertility, and the first to examine the contribution of the labor movement to the American Baby Boom. In generating these novel connections, I make several contributions that span multiple areas of research.

First, my findings inform the existing literature on the causes of the Baby Boom. Conventional wisdom based on early work by Easterlin (1968) and Becker and Barro (1988) centers the role of economic growth in driving postwar increases in fertility. In addition, labor economists have long acknowledged that the labor movement fundamentally transformed how the gains from growth were distributed within

[7] From Macunovich (2010): "Men and their unions, as they entered industrial work, negotiated two things: young women would be laid off once they married (the commonly acknowledged 'marriage bar'), and men would be paid a 'family wage' ... these two measures ensured that the vast majority of male wage earners would be supported in the home by unpaid labor, effectively making them more productive in the workplace. Industrialists were simply acknowledging that with each male worker they were in fact obtaining the services of two workers – the man and his wife."

the economy during this period. My findings unite these two literatures, and suggest that the historic singularity of the Baby Boom was attributable in part to the remarkable rise of collective bargaining, which provided a novel technology for translating economic expansion into broad-based prosperity. Other previous work highlights the contributions of period-specific shocks.[8] While unionization has not been previously linked to the Baby Boom, the rise of the labor movement had wide-ranging impacts on the income distribution, the economic security of workers, the healthcare system, and the composition of the labor force. Insofar as unionization interacts with other relevant shocks in this period, my findings tend to complement rather than substitute for existing work on the Baby Boom. Finally, as a particularly long-lasting shock, union growth is able to account for some previously unexplained features of the Baby Boom, including early increases in fertility prior to WWII and persistently high fertility through the late 1950s and early 1960s.

More broadly, this work contributes to the extensive literature that considers how labor market institutions and policies influence fertility. Consistent with standard economic models of fertility (Becker, 1960) and a growing empirical literature that connects income shocks to fertility,[9] I show that place-level income shocks resulting from union growth are an important mechanism in driving treatment effects. Beyond income effects, I find that unions' role in protecting workers from adverse labor market shocks contributed to fertility increases. Previous work has shown that policies that protect against job loss have a pro-natalist effect (Kearney, 2023), and that job insecurity is associated with lower fertility (Tölke and Diewald, 2003; Sobotka et al., 2011; Schneider, 2015; Mansour, 2018; Clark and Lepinteur, 2022). Until now, the connection between collectively-bargained job protections and fertility has not been made. Finally, there is a well-documented negative correlation between female labor force participation and fertility throughout this period (Doepke et al., 2023). I find that changes in labor force participation among women after WWII – and the resulting changes in fertility – were rooted not only in residual effects of the war (Goldin and Olivetti, 2013; Doepke et al., 2015; Shatnawi and Fishback, 2018; Brodeur and Kattan, 2022), but also in the growing influence of male-dominated labor unions. Overall, my results suggest that labor market institutions, including labor unions, play an important but often overlooked role in shaping demographic outcomes, both by impacting wage setting and conditions of employment and by affecting the composition of the workforce.

This research also relates to the enormous literature that asks: "what do unions do?" (Freeman and Medoff, 1984) Most empirical work on labor unions focuses on work-related outcomes; e.g., impacts on wages and income inequality, non-wage compensation, employment, productivity, and occupational safety. More recently, a smaller literature considers the impacts of unions in the broader economy, including the role that unions play in promoting marital formation (Schneider and Reich, 2014), the intergenerational effects of union membership (Freeman et al., 2015; Budd et al., 2022), the influence of organized labor on regional growth and decline (Alder et al., 2023), and fiscal impacts of labor unions (Sojourner and Pacas, 2019). Building on the basic insight that the effects of unions on workers have implications for workers' families, my work adds family formation to the wide-ranging set of outcomes that have been

[8] See: Greenwood et al. (2005); Murphy et al. (2008); Bellou and Cardia (2014); Albanesi and Olivetti (2014); Doepke et al. (2015); Albanesi and Olivetti (2016); Brodeur and Kattan (2022).

[9] See: Lindo (2010); Black et al. (2013); Lovenheim and Mumford (2013); Currie and Schwandt (2014); Bleakley and Ferrie (2016); Kearney and Wilson (2018); Ager et al. (2020); Bratsberg et al. (2021); Cesarini et al. (2023); Yonzan et al. (2024)

previously linked to unionization. Given the historical setting, I also contribute more specifically to a body of empirical work on the effects of the 1935 National Labor Relations Act (Taylor and Neumann, 2013; Collins and Niemesh, 2019; Farber et al., 2021; Holt, 2024).

Finally, I contribute to the methodological literature that seeks to measure geographic variation in the presence and growth of American labor unions during the 20$^{\text{th}}$ century. The Decennial Census has never included a question about union status, and survey-based microdata on union membership was not collected by the Current Population Survey (CPS) until 1973 – more than a decade into the decline of unionism in the U.S. Pioneering work by Troy and Sheflin (1985) relies on union reports and personal correspondence to estimate union density at the state-level for selected years after 1939, and Farber et al. (2021) draw on Gallup opinion polls to estimate state-level union membership from 1937 onward. Other recent work, including contributions by Schmick (2018), Sezer (2023), and Medici (2024), use archival sources to estimate union membership at the local-level; however, these sources cover selected years in the late 19$^{\text{th}}$ and early 20$^{\text{th}}$ centuries, well before the passage of the NLRA. In this challenging data environment, I introduce the first county-level estimates of union membership during the mid-20$^{\text{th}}$ century, a period of labor power that is without parallel in American history. These high-resolution data significantly extend the research frontier for future work on the historic and long-run impacts of labor unions. In addition, as the first series of sub-national estimates spanning the years both before and after the New Deal, these data are uniquely well-positioned to offer insights into the causal effects of watershed moments in labor history, including the original NLRA.

2 Background: The NLRA and the Rise of the Labor Movement

The rise of the labor movement was one of the most remarkable and unexpected developments of the New Deal era.[10] Prior to 1933, the U.S. did not have a unified national labor policy (Stepan-Norris and Kerrissey, 2023). The labor policy that did exist in a patchwork of state laws, industry codes, and Supreme Court rulings tended to favor employers' goals of suppressing union organizing. Unions were not illegal in most cases, but federal law afforded them no official recognition to represent workers or bargain collectively on their behalf. As a result, early union gains were limited to a relatively narrow subset of industries. I plot national union membership over time in Appendix Figure A1. By 1932, fewer than three million workers – less than one-tenth of the U.S. non-farm workforce – were union members, and prospects for future growth appeared dim. In an address delivered to the American Economic Association (AEA) in that same year, AEA president George Harold Barnett commented: "American trade unionism is slowly being limited in influence by changes which destroy the basis on which it is erected... I see no reason to believe that American trade unionism will ... become in the next decade a more potent social influence" (Freeman, 1997).

Just a few months after Barnett's address, the enactment of the 1933 National Industrial Recovery Act (NIRA) represented the first major effort to reshape the federal legal status of labor unions.[11] Among

[10]Throughout this section, I draw heavily from several sources on historical developments in labor relations, including Wolman (1936), Biles (1991), Wachter (2012), Fishback (1998, 2020), Hanes (2020), Farber et al. (2021), and Stepan-Norris and Kerrissey (2023), among others.

[11]The passage of the Norris-LaGuardia Act in 1932 was another important early development. Norris-LaGuardia banned "yellow-dog contracts", which required workers to pledge not to join unions as a condition of employment, and also established

other provisions, the NIRA guaranteed collective bargaining rights to labor unions for the first time. A handful of well-established unions seized on this development with large organizing campaigns in 1933 and 1934.[12] However, despite some early traction, any momentum generated from the NIRA ultimately proved to be a false start. Without any credible enforcement mechanisms in place, many employers simply ignored the newly-granted rights of unions.[13] The Supreme Court dealt a final blow to the NIRA in May 1935 by ruling that the law was unconstitutional.

By the time the NIRA collapsed in 1935, a new effort led by Senator Robert Wagner was already underway to pass a stronger federal labor bill that could remedy the NIRA's shortcomings. The resulting bill, the National Labor Relations Act (NLRA, or "Wagner Act"), retained the collective bargaining provisions of the NIRA for all workers in the private sector, but added several important features. First, the NLRA established a new National Labor Relations Board (NLRB) with the power to define bargaining units, hold and certify union elections, and redress unfair labor practices. Second, the NLRA provided a detailed set of rules to govern both union elections and collective bargaining, and outlawed employer tactics designed to discourage lawful union organizing activities. Third, the NLRA established the legally protected right to engage in peaceful work stoppages and strikes. In buttressing the lofty goals of the NIRA with significant and unprecedented federal enforcement powers, the NLRA marked the beginning of a new era in U.S. labor relations.

The NLRA was signed into law in July 1935 but, like its predecessor the NIRA, its initial implementation was marred by legal challenges and political uncertainty. Emboldened by a December 1935 lower court ruling that found the NLRA to be unconstitutional, many employers continued to openly ignore the provisions of the new law and the authority of the NLRB. The fortunes of the NLRA changed, however, with Roosevelt's landslide victory in the 1936 election.[14] The decisive result not only resolved political questions about the continuation of the New Deal program, but also prompted a reversal of opinion within the Supreme Court, which narrowly upheld the constitutionality of the NLRA on appeal in a 5-4 vote handed down in April 1937.[15] In the months following the 1936 election, a massive wave of recognition strikes boosted union membership to historic levels,[16] and new collective bargaining agreements – including some negotiated by the newly-formed Congress of Industrial Organizations (CIO)

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the right to form labor unions without employer interference.

[12]For example, the Amalgamated Clothing Workers doubled its membership from 60,000 to 120,000 between early 1933 and mid 1934, and the United Mine Workers of America quadrupled its membership from 100,000 to 400,000 within a year of the NIRA's passage (Wolman, 1936).

[13]Historians have noted that Roosevelt likely never intended NIRA's Section 7(a) to inaugurate a new era in U.S. labor relations. Indeed, according to Roosevelt's biographer Frank Freidel: "[Roosevelt] probably agreed with New Dealer Francis Biddle's view of Section 7(a) as an 'innocuous moral shibboleth.'"

[14]The singular efforts of Sen. Wagner to craft and garner support for the bill were also of undeniable importance. Leon Keyserling, Wagner's legislative secretary, observed: "There would never have been a Wagner Act or anything like it at any time if the Senator had not spent himself in this cause to a degree which almost defies description." (Biles, 1991)

[15]On the heels of the 1936 election victory, Roosevelt announced his plans to reorganize the federal judiciary (i.e., the "court packing plan") in February 1937, and brought his case to the American people in a fireside chat in March. The NLRA was upheld in April, with decisive votes coming from Justice Owen Roberts and Chief Justice Charles Evans Hughes, both of whom had been previously hostile to New Deal legislation. Historians have argued that this so-called "switch in time that saved nine" was an act of self-preservation by the centrists on the court in the face of increasing political pressure (Devins, 1996).

[16]In the early years of the NLRB, union recognition was primarily gained through strikes, not NLRB-certified elections. The number of U.S. workers involved in recognition strikes increased from 272,013 in 1936 to 941,802 in 1937 (Freeman, 1997). Over time, the growing authority of the NLRB caused a shift away from strikes and toward certified elections as the primary means of securing union recognition; by December 1939, the NLRB had held 2,500 elections in which 1.2 million workers cast ballots (Biles, 1991).

in the previously unorganized manufacturing industries – drove large wage increases (Hanes, 2020; Holt, 2024). Overall, national union membership nearly doubled in the four years following the passage of the NLRA (see Appendix Figure A1).

The mobilization of the U.S.'s industrial capacity for World War II inaugurated a second surge in union growth. There were several policy changes that linked the ramping up of war production with unionization. First, starting in 1940, only NLRA-compliant firms were eligible to receive defense contracts from the National Defense Advisory Commission. Second, in exchange for pledges not to strike, the National War Labor Board (NWLB) imposed automatic enrollment and maintenance-of-membership for any firm with a war production contract, so that workers had to actively disenroll shortly after being hired to opt out of union membership. The NWLB also permitted unions to implement "dues checkoffs", which automatically deducted union dues from members' paychecks. And, although the NWLB imposed wage ceilings, it tended to allow wage increases in union shops whenever feasible in order to avert strikes. Seizing on these favorable conditions, the CIO achieved stunning success in completing the organization of the mass production industries during the war.[17] National union membership nearly doubled again between 1939-1945, and by the end of the war unions had a seat at the bargaining table in America's most prolific corporations.

The "routinization" of collective bargaining was perhaps the most important legacy of wartime unionism (Biles, 1991), as collective bargaining became the rule, not the exception, in many of the industries at the center of the postwar economic boom. Moreover, with wage controls in place throughout the war, unions began to widen the scope of collective bargaining to include an array of new benefits, including pension plans, health insurance, and other forms of non-wage compensation.[18] A series of Supreme Court decisions in 1948 and 1949 affirmed the rights of unions to bargain over such "fringe benefits", which became increasingly common thereafter. Overall, the percent of total compensation made up by supplements to wages and salaries increased from 4% during the war to over 8% by the early 1960s (Bauman, 1970). Unions played an especially important role in the early formation of the U.S. employer-based health insurance market. By mid-1950, virtually every major union had negotiated some form of a pension or "health and welfare" program (Rowe, 1951). Overall, the number of workers covered by union-negotiated health insurance increased from 600,000 in 1946 to 12 million workers and 17 million dependents by 1954 – approximately one-fourth of the U.S. health insurance market (Helms, 2008). Automatic cost-of-living adjustments to wages, which grew to cover more than 3.5 million workers by 1952 (Johnson, 1957), were another important feature of postwar collective bargaining agreements.

During the postwar peak of the labor movement, more than one in every three non-farm workers belonged to a labor union. Private sector union membership began to steadily decline beginning in the mid-1950s; a large literature reviews the causes of the decline of American unionism (e.g., see Farber and Western (2016) and related work). The NLRA has been amended several times since 1935 but remains a cornerstone of the legal system of labor relations in the U.S. today.[19]

[17] During this period, CIO unions negotiated collective bargaining agreements for the first time with several of the country's largest and most staunchly anti-union firms, including the "Little Steel" corporations, Ford Motor Company, Goodyear, Armour, and Westinghouse, among others. (Biles, 1991)

[18] In addition, recent work by Vickers and Ziebarth (2022) suggests that collective bargaining agreements were an important mechanism in perpetuating wage structures imposed by the National War Labor Board after the war, with persistent effects on inequality through the 1960s.

[19] One such amendment was the Labor Management Relations Act of 1947, also known as the Taft-Hartley Act, which

3 Cross-Sectional Survey Evidence

In this section, I present evidence of the cross-sectional relationship between union membership and family size using survey microdata from multiple years and sources during the Baby Boom. In particular, I test whether union households tend to have more children than comparable non-union households. This descriptive evidence provides a first test of whether the hypothesized mechanisms linking union membership to increased fertility are plausible.

3.1 Data

The first major source of historic survey data comes from Gallup opinion polling. Since 1937, Gallup has periodically asked respondents whether any member of the household belongs to a labor union. These microdata were first introduced to the literature by Farber et al. (2021). I identify over 120 such surveys that were administered from 1937-1964 and contain a version of the union membership question. Of these 120, 7 contain both the union membership question and a question related to family size.[20] The Gallup data are somewhat limited by methodological issues,[21] but offer relatively large sample sizes, cross-sections for multiple years, and several key covariates.

A second key source of cross-sectional data is the Census Occupational Mobility Survey, directed by Gladys Palmer in 1951 (Palmer and Brainerd, 1954). The survey was implemented to study labor force mobility in the 1940s and was administered using modern stratification techniques.[22] Census enumerators conducted the survey through a series of in-person interviews in six Northern cities: Chicago, New Haven, Los Angeles, Philadelphia, San Francisco, and St. Paul. The sampling frame includes men in the labor force aged 25 and older. The Palmer survey explicitly asks about labor union membership as well as the "number of children under 18 in [the respondent's] own family", and offers the largest sample size of any survey from this period (N = 6,936).

Finally, I incorporate data from the 1956 American National Election Studies (ANES), a series of surveys that were designed to gauge public opinion before and after political elections. The 1956 survey was the first wave of a 3-part panel survey, with follow-up engagements in 1958 and 1960. I focus on the 1956 wave, since it is recorded before any attrition can take place and so provides the largest sample size. Like the Palmer survey, ANES specifically records the number of own children in the respondent's household and the union status of the household head. However, sample size is an issue, as the data includes only 664 household heads.

permitted states to enact so-called "right-to-work" laws.

[20]Family size questions take two forms in these surveys. The first version, included in surveys from October 1941, November 1941, and December 1943, asks: "How many people live in your home with you, including yourself?" The second version, included in surveys from March 1942, August 1951, October 1952, and January 1953, asks: "How many are there living with you in your immediate family, including children and yourself?" In either case, I subtract two from the recorded response to proxy for the number of children in a household, since the vast majority of children lived in two-parent households during this time. In cases where subtracting two from the recorded household size results in a negative value, I impute the number of children to equal zero.

[21]First, Gallup's sampling frame is the universe of voters, which implies substantial under-representation of certain demographics. Second, Gallup did not implement modern probabilistic sampling procedures until 1950. Taking these two points together, Southerners, minorities, and low-income households were especially under-represented in Gallup surveys prior to 1950. For a comprehensive review of Gallup's sampling procedures and related issues from this time, see Appendix B of Farber et al. (2021).

[22]See Callaway and Collins (2018) for a detailed review of Palmer's sampling procedures.

3.2 Results

I synthesize baseline results for a sample of household heads aged 25-54 across all surveys in Figure 2. Each "unadjusted" estimate is equal to the simple difference in the mean fertility outcome between union and non-union households, scaled by the non-union mean. Each "adjusted" estimate is equal to the coefficient of a regression of the fertility outcome on union membership and a set of controls,[23] scaled by the non-union mean. In all cases, a positive value indicates that fertility is higher among union households, and whiskers represent 95% confidence intervals. I sort point estimates in descending order by sample size.

The combined cross-sectional results suggest that union households had larger family sizes during the Baby Boom. All unadjusted point estimates are positive, and the majority are statistically significant at the 5% level (including the top four ranked by sample size). Most estimates imply that union membership is associated with about 10-20% more children per household.[24] The regression-adjusted results are broadly consistent with the simple differences, which implies that most of the cross-sectional variation in family size which can be explained by union membership is not attributable to observed possible confounders like race, age, and occupation.

4 Data

In my main analysis, I link novel estimates of union membership to fertility outcomes at the county-level. In this section, I describe how I construct county-level union membership estimates from archival sources, and present a series of descriptive results and validation exercises. I also describe the data sources and methods used to construct fertility and marriage outcomes.

4.1 Union Membership

Despite the central role that labor unions played in the American economy during the 20$^{\text{th}}$ century, there is surprisingly little systematic measurement of union membership at sub-national levels throughout this period. The U.S. Decennial Census has never included a question about union membership, and consistent microdata on union membership was not collected by the CPS until 1973. As a result, most previous work on the historical impacts of unions relies on national aggregates.[25] In this challenging data environment, some recent work has made progress in measuring historic union membership for sub-national geographies. Most notably, Farber et al. (2021) construct state-level estimates of union membership from 1937 onward, drawing primarily on Gallup opinion polls. However, since this series is not available until after the passage of the 1935 NLRA, it is not well-suited to assess key identifying assumptions based on pre-period trends. In addition, most of the variation in union growth during this

[23] The baseline controls available in each survey are: a dummy for region = South, race, age, a quadratic in age, urban/rural residence, occupation, sex, state fixed effects and a vector of dummy variables which indicate missing values for each of the above. Several of these covariates may be "bad controls"; e.g., unionization may influence fertility in part through selection into certain occupations, so within-occupation comparisons will tend to understate the impact of unionization. Still, adding these controls helps to isolate the variation which drives the difference between unionized and non-unionized workers.

[24] E.g., in the Palmer survey data, the average difference (union − non-union) in own children in the household = +0.15, and the average non-union household in this sample has 1.1 children; 0.15 / 1.1 = 0.14.

[25] National measures of union membership are available as early as 1880 from various sources (Freeman, 1997).

period was within, rather than across, states.[26] While several other contributions – including those by Schmick (2018), Sezer (2023), and Medici (2024) – use archival sources to estimate union presence and membership at the county-level, these sources only cover selected years in the late 19$^{\text{th}}$ and early 20$^{\text{th}}$ centuries, well before the passage of the NLRA.

Given the limitations of existing sources, I construct new estimates of union membership at the county-year level in several large states from 1920-1961. I draw primarily on convention proceedings of state-level chapters of the American Federation of Labor (AFL), the Congress of Industrial Organizations (CIO), and the merged AFL-CIO.[27] Figure 3 plots the share of national union membership by affiliation. Throughout this period, more than 80% of all union members belonged to local branches (or "locals") affiliated with the AFL, CIO, or AFL-CIO. State-level chapters of each federation held regular conventions that drew representatives from locals across the state. Conventions provided opportunities to legislate new policy pertaining to affiliated members, compile updates from across the region, organize new initiatives, and voice support for political proposals.

Occasionally, proceedings documents contain direct measures of union membership disaggregated to the local-level. More often, proceedings contain other measures that serve as indirect proxies for local union membership. State federations levied "per capita taxes" on affiliated members, typically paid at a per member-per month rate, to fund organizational expenses. When such per capita tax receipts are itemized at the local level, and the per capita tax rate is known, it is possible to use financial reports to infer local union membership.[28] If per capita tax receipts are unavailable, I instead rely on the number and voting strength of delegates elected to represent locals. Voting strength was generally allocated to locals in proportion to their "paid up membership" – the number of members current on per capita tax payments. I therefore use vote counts in combination with representation schemes (as defined in convention rules) to estimate local membership. Since the representation schemes were often defined in terms of ranges (e.g., "1 delegate for the first 50 members, and 1 delegate for every 100 members thereafter"), I estimate membership based on the midpoint of the range implied by each local's observed voting strength. I present examples of the source material used to construct measures of each type in Appendix B.

My main sample for the county-level analyses includes approximately 400 counties in five states – California, Illinois, Missouri, Pennsylvania, and Wisconsin – for which high quality, locally-disaggregated measures (or proxies) of union membership are consistently available. These states accounted for about 25% of the U.S. population and 35% of all union members in 1950, and so provide a reasonable approximation of the national environment. My panel includes union membership estimates for three periods: 1920-1925, 1932-1934, and 1956-1961. The first and second periods capture union density in the pre-NLRA era, while the third period measures union density at the peak of the labor movement.[29]

[26]County-level union membership rates are available for five states in my sample (as I discuss in detail later in this section). Within this sample of states, differences in state-level means account for approximately 14% of the total variation in county-level growth in union membership rates (1934-1960).

[27]Most of these proceedings were available as digitized scans from ProQuest History Vault (see the "State Labor Proceedings: AFL, CIO, and AFL-CIO Conventions, 1885-1974" module). If proceedings were not accessible through ProQuest, I obtained hard copies from various archival collections.

[28]This method has been adopted by others in the literature, including: Boal (2006), Cohen et al. (2016), and Medici (2024).

[29]I construct estimates for selected years only, for two main reasons. First, even for the five states in the main sample, measures are not always available at an annual-level. Second, I cannot construct estimates between 1937-1955 due to

While independent unions account for a relatively minor proportion of national membership throughout this period, there are several unions that were independent at some point between 1920-1961 and claimed large memberships in certain regions. For example, the United Mine Workers of America (UMWA) was affiliated with the AFL until 1937, the CIO from 1937-1942, independent from 1942-1947, briefly re-affiliated with the AFL in 1948, and then independent again until its affiliation with the AFL-CIO in the 1980s. Failing to capture UMWA membership would differentially impact some areas more than others, (e.g., in the coal mining regions of Pennsylvania), and so could result in non-classical measurement error. Therefore, I supplement the data from state-level convention proceedings with estimates constructed from national convention proceedings of several large independent unions including the UMWA, the Teamsters (IBT),[30] and the United Electrical, Radio and Machine Workers (UE).[31] These three unions accounted for 64% of national membership in independent unions in 1960. In total, 85-90% of all union members in the U.S. were affiliated with one of these organizations or with the AFL, CIO, or AFL-CIO throughout my measurement period (Troy, 1965).

To construct the final membership estimates at the county-level, I first map the local-level data to counties using the Geonames database.[32] I use additional online and printed references to conduct manual lookups for any places that fail to map to a county and to disambiguate places that map to multiple counties. If a local is attributed to multiple cities (e.g., Janesville and Beloit, WI), I split estimated membership out evenly across all cities. I drop any entities that cannot be attributed at the county level (e.g., statewide groups), as well as union councils, leagues, departments, and central bodies, for which membership cannot be inferred using per capita receipt- or vote-based approaches. I aggregate membership estimates in each county across all sources, and collapse to constant county-equivalent geographies according to 1910 boundaries. If data are missing for any sample year from a given source, I linearly interpolate values using the first available adjacent years from that source. For simplicity, I attribute data from convention proceedings to the calendar year that immediately precedes the convention year, but this mapping should be understood to be an approximation.[33] Table 1 provides a detailed breakdown of all sources and methods used to construct union membership estimates in each state and year.

I construct union membership rates by dividing total membership by county-level employment from U.S. Decennial Censuses, including the 1920-1950 full count Decennials from IPUMS (Ruggles et al., 2024) and samples of the long form 1960-1970 Decennials from the Federal Statistical Research Data

measurement issues with CIO membership. Disaggregated data is only sporadically available from state CIO conventions and, since CIO membership made up about one-third of total union membership in this period, AFL-only estimates would fail to reliably capture membership dynamics. The CIO did not exist until November 1935, so AFL proceedings are sufficient to measure membership levels in the first two periods. By 1956, the AFL and CIO had merged to create the AFL-CIO, so AFL-CIO proceedings are sufficient to capture membership levels in the third period.

[30]The Teamsters were expelled from the AFL and became independent in 1957.

[31]The UE was a charter union of the CIO in 1938, but was expelled in 1949 and independent thereafter.

[32]The Geonames geographical database is available for download free of charge under a creative commons attribution license. Geonames compiles information from a variety of sources, including the National Geospatial-Intelligence Agency, the U.S. Board on Geographic Names, the U.S. Geological Survey Geographic Names Information System, and the U.S. Census Bureau.

[33]It is not straightforward to map measurement periods defined in the proceedings data to calendar years, as reporting standards vary widely both across and within conventions over time. For example: the 1958 CA CIO proceedings present per capita receipts from June 1, 1958 - November 30, 1958, the 1959 CA AFLCIO proceedings present receipts from December 10, 1958 - June 30, 1959, and the 1960 CA ALFCIO proceedings present receipts from July 1, 1959 - June 30, 1960. I show that the main results are not sensitive to adjusting the sample to include various years of data.

Centers (FSRDC) internal-use files. I linearly interpolate employment for all intercensal years.

While these novel county-level estimates have distinct advantages over existing sources, estimates from convention proceedings may underestimate true union membership in a local area for several reasons. First, unemployed and striking workers were often excluded from dues, and thus may not have contributed to membership counts based on paid-up membership, even if they technically remained union members. Second, locals may have felt incentivized to underestimate their membership levels to reduce their per capita tax burden (Boal, 2006).[34] Third, some locals may have lacked the capacity to send a delegation to statewide conventions, leading to an undercount in vote-based estimates. Finally, any union members affiliated with independent or company unions (besides UMWA, IBT, and UE) do not figure into estimates. In Section 5.1, I describe how I instrument for local union membership rates to mitigate possible sources of measurement error.

In a first set of descriptive results, Figure 4 plots county-level union membership rates in California and Wisconsin, where I observe consistent measures of membership in unions affiliated with the AFL or AFL-CIO for *all years* in the measurement period. Panels (a) and (c) depict time series for all counties in each state, while panels (b) and (d) highlight dynamics in the five largest counties by employment in each state. Several facts are worth noting. First, union membership rates rarely exceed 5% in the pre-NLRA era, a period in which organized labor had no official legal status. Second, there is a clear secular increase in membership rates after the passage of the NLRA in 1935, and another surge as war production ramped up in the early 1940s. However, there is a great deal of variation in the magnitude of union growth across areas. Third, membership levels are clearly understated when CIO membership is unobserved (grey-shaded areas), but measurement becomes more complete with the incorporation of AFL-CIO data following the merger in the mid-1950s. Overall, these descriptive results are consistent with the historical narrative set out in Section 2, which centers the NLRA as a watershed moment in touching off an unprecedented wave of unionization that continued into the mid 20$^{\text{th}}$ century.

In Figure 5, I depict variation in county-level union membership growth from 1934-1960 in each of the five sample states. While it is generally the case that urban, industrialized areas exhibit the greatest union growth (e.g., San Francisco, CA; St. Louis and Kansas City, MO; Pittsburgh and Philadelphia, PA; Milwaukee, WI) and rural, sparsely populated areas exhibit the least union growth, there are a number of notable exceptions to this pattern. In particular, the UMWA drove large unionization efforts in Southern Illinois, Western Pennsylvania, and parts of Central Missouri, while lumber and woodworking unions contributed to extensive gains in North Central California. Overall, there is substantial variation in local responses to the NLRA shock across counties.

Finally, I aggregate the county-level data to the state-level and compare my estimates to those of Farber et al. (2021), who construct state-level union membership rates from Gallup opinion polls from 1937 onward, and Troy and Sheflin (1985), who derive state-level estimates from union membership reports and other primary sources in 1939 and 1960.[35] I present these comparisons in Appendix Figure A2. I find that while my estimates are positively correlated with Troy and Sheflin (1985)'s estimates in both levels (b) and changes (d), they are negatively correlated with Farber et al. (2021)'s estimates in

[34] On the other hand, a local seeking to compete for influence within their organization may have *overinflated* paid-up membership (Farber et al., 2021).

[35] Unfortunately, since county-level measures do not exist from any other source in this period, I cannot validate my data at the county-level.

levels (a) and changes (c). My methodology is most closely related to that employed by Troy and Sheflin (1985), who draw in part from financial reports of union conventions, which may account for this result. I also note that some discrepancies may stem from the fact that sources do not overlap in time, which complicates comparisons: my series includes data from the pre-NLRA period, while the Farber et al. (2021) and Troy and Sheflin (1985) series do not begin until 1937 and 1939, respectively. Still, given the issues raised from these exercises, I show in Section 6.3 that the main results at the state-level are robust to using estimates from either Farber et al. (2021) or Troy and Sheflin (1985).

4.2 Birth Rates

Data on birth rates are primarily from Bailey et al. (2016). Drawing from original government sources, they document vital events at the county-year level from 1915-2007, including the number of live births by place of occurrence, live births by place of residence, and the population of females of childbearing age (15-44). The outcome of interest is the birth rate (also known as the "general fertility rate", or GFR), which is equal to the number of live births per 1000 women of childbearing age.

Births by residence provide the best measurement of fertility in this context, as changes in births by occurrence over time may be driven by confounding factors such as access to hospitals. However, since Bailey et al. (2016) do not provide county-level births by residence until 1937, I supplement the Bailey et al. (2016) series using hand-collected data on births by residence from Special Reports of the Vital Statistics of the U.S. in 1935 and 1936.[36] Unfortunately, official sources with data on births by residence for all counties do not exist prior to 1935, so I use births by occurrence to construct birth rates from 1915-1934. I show in Appendix Figure A3 that births by residence and births by occurrence are very highly correlated at the county-level until World War II (when the supply and demand of healthcare grew dramatically, driving up the share of births in hospitals). In particular, the weighted average of county-level absolute differences between the two measures is $< 5\%$ in the mid- to late-1930s, and correlation coefficients exceed 0.998. Therefore, it is unlikely that using births by occurrence for the pre-1935 period is an important source of measurement error.

4.3 Completed Fertility

From 1940-1990, U.S. Decennial Censuses record the number of children ever born to ever-married women aged 15 and older.[37] These data can be used to estimate completed fertility by measuring the number of children ever born toward the end of a woman's childbearing years.[38]

There are several challenges associated with using Census data on children ever born at the county level. Most importantly, children ever born is a "sample-line" variable in the 1940 and 1950 Decennials. In these years, respondents whose names were recorded on specific lines on the census form were asked a supplemental battery of questions, including several on marital and childbearing history.[39] Sample-line

[36]County-level data for 1935 are from State Summaries contained in Volume II of the Vital Statistics Special Reports (1936); county-level data for 1936 are from State Summaries contained in Volume 6 of the Vital Statistics Special Reports (1938).

[37]The Census specifically instructs respondents to include births by all fathers, whether or not the children survive, and to exclude stillbirths, adopted children, and stepchildren. The IPUMS variable is *CHBORN*.

[38]For example, Albanesi and Olivetti (2014) construct completed fertility estimates at the state-birth year cohort level by measuring children ever born for women aged 35-44 in various Censuses.

[39]See Ruggles (1995) for more information on sample-line procedures.

respondents constituted 5% of all observations in 1940 and 3.33% of all observations in 1950.[40] As a result, county-birth year cell sizes are typically very small, even when using the 1940 full count data: 7% of county-birth year cells contain zero observations, and the median cell contains only 5 women. In 1950, when the sample-line is even more limited, 18% of county-birth year cells contain zero observations, and the median cell contains only 2 women. Moreover, the IPUMS' preliminary release of the 1950 full count data does not include sample-line weights, which are necessary to construct representative estimates.[41] Unfortunately, these issues impact some of the most important birth year cohorts in my measurement period, who experienced peak childbearing during the 1920s-1940s.

The timing of measurement presents additional issues when using Census data on children ever born, even when sample sizes are sufficiently large. By definition, completed fertility must be measured toward the end of a woman's childbearing years. Measurement is therefore conditional on surviving to the mid-30s, at minimum. If mortality risk before the end of childbearing is differential across populations, this may introduce bias.[42] Also, in the absence of detailed migration histories, completed fertility must be attributed to the location observed at measurement. This amounts to an implicit assumption that women did not migrate during the course of their childbearing years, which may be a source of bias if migration is correlated with treatment and outcomes.[43] Measurement error associated with migration is likely to be decreasing in the size of the geographic units of analysis, so small geographies like counties are especially problematic. Finally, since data from each Decennial Census must be used to construct estimates for multiple birth year cohorts, completed fertility cannot be measured at the same age across cohorts. E.g., Children ever born can be measured at ages 35-44 for women born 1906-1915 in the 1950 Census. Given that age-specific birth rates do not approach zero until after age 40 in this period,[44] estimates using this method will mechanically understate completed fertility for cohorts who are younger at measurement, and bias may result if childbirth timing is differential across groups. Increasing the age range at measurement (e.g., to 45-54) would address this form of error but exacerbate issues related to mortality and migration.

To overcome these limitations, I use microdata from the 1910-1990 Decennial Censuses to construct an alternative estimate of county-level completed fertility for synthetic cohorts born 1886-1945.[45] In each Decennial Census, I restrict to a sample of household heads that do not reside in group quarters and are headed by either (1) a female or (2) a male with a female spouse. I then use information on within-household relationships and the observed ages of children to attribute children born since the last Census to the female head or spouse. I compute the average number of children born since the last

[40] However, since the sampling frame for children ever born was limited to ever-married women of a certain age, the actual proportion of respondents for whom children ever born is non-missing is even lower: 1.4% of total respondents have non-missing data in 1940, and 0.66% of total respondents have non-missing data in 1950.

[41] The 1950 1% sample contains sample-line weights, but county-level estimates in most places are not viable due to a lack of observations.

[42] Based on 1940 age-adjusted death rates: 91.2% of white females would be expected to survive to age 35, while only 79.9% of black females would be expected to survive to age 35. (Grove and Hetzel, 1968).

[43] Thompson (2019) finds evidence of differential completed fertility between women who migrated from the South vs. those who stayed in the South during the Civil Rights Era.

[44] In 1940, the birth rate was 46.4/1000 for women aged 35-39, 15.6 for women aged 40-44, and 1.9/1000 for women aged 45-49 (Grove and Hetzel, 1968).

[45] Full count versions of the 1910-1950 Censuses are publicly available from IPUMS (Ruggles et al., 2024). Since counties are not generally identified in the public use files after 1950, I also draw on samples of the long form 1960-1990 Censuses from the Federal Statistical Research Data Centers (FSRDC) internal-use files.

Census at the county-birth year cohort level, and move sequentially across Decennial Censuses to cover the full range of childbearing years (ages 15-44) for all cohorts. Finally, I sum the averages for each county-birth-year cell across Census years to derive the total cohort fertility rate, or "TCFR".[46]

I depict TCFR construction for a subset of birth year cohorts in Figure 6. Years within the body of the diagram refer to the Decennial Census in which births since last Census are measured at the specified age for each birth year cohort. For example, I construct the TCFR for the cohort born in 1905 using data from four Censuses: births at age 15 are measured in 1920, births at ages 16-25 are measured in 1930, births at ages 26-35 are measured in 1940, and births at ages 36-44 are measured in 1950. The TCFR estimate for the 1905 cohort is equal to the sum of the average number of children born since the last Census across all four measurements.

As an alternative estimate of completed fertility, TCFRs address several key limitations of estimates derived from the children ever born variable. First, the underlying data used to construct TCFRs are available for all individuals in all Census years – not only those on the sample-line. As a result, county birth year cells are much larger: the median county-birth year cell size is 94 women in 1940 and 103 women in 1950. In addition, TCFRs are less sensitive to measurement error due to migration and mortality: women start contributing to the county-level TCFR once they are observed in any Census above the age of 15, and any births since the last Census are attributed to their county of residence at measurement which, crucially, can change over time.[47] Finally, TCFRs provide a consistent measure of completed fertility across the full range of childbearing years for all cohorts.

Despite their many advantages, TCFRs are not as straightforward to interpret as estimates based on children ever born. While children ever born describes completed fertility for a clearly defined cohort of women, TCFRs capture completed fertility for "synthetic cohorts" at the place-level. Since the composition of women from any given cohort in a county could change over time (i.e., due to migration or mortality), TCFR is not a pure stock measure like children ever born. Without individually-linked records across all Censuses, it is difficult to assess the importance of these distinctions.[48] Also, unlike children ever born, TCFRs fail to capture any children who did not survive long enough to be directly observed in any Census, which could cause TCFRs to underestimate true completed fertility. Finally, the ages of infants are known to be measured with some error in the preliminary release of the 1950 full count Census.[49]

I compare the TCFR and children ever born measures in Appendix C. I find show that the series track each other closely in levels and in changes. However, the measures are less correlated in the aggregate and at the county-level when the cells used to estimate children ever born are small, and for cohorts for whom children ever born is measured at younger ages. Overall, the county-level TCFR distribution is much smoother and less skewed than that of children ever born.

[46]This approach is similar to that of Black et al. (2013), who construct "synthetic cohort" estimates of completed fertility by combining the children ever born measure from the 1990 Census and the number of children in the household aged 0-10 from the 2000 Census.

[47]Of course, there could still be mortality and migration since the last Census, which would impact TCFR estimates in ways similar to those described above for children ever born. However, the implicit "no migration" and "no mortality" assumptions are weaker for the TCFR estimates, since they only apply to the 10 years since the last Census and not to all childbearing years.

[48]Even with individually-linked records, however, it would be necessary to take a stand on how to attribute births of migrating women at the place-level.

[49]See: IPUMS guidance on the January 2024 - 1950 Full Count Preliminary Release.

I use TCFRs as the primary measure of completed fertility throughout the main analyses. However, I also present results using children ever born as an alternative outcome in Appendix Tables A2 and A3.

4.4 Marriage Rates

I construct age-specific marriage rates on a decennial basis using microdata from the 1920-1950 full count Census files (Ruggles et al., 2024) and from the 1960 FSRDC long form Census file. In each Census, I restrict to a sample of women aged 18-40 who do not reside in group quarters. The county-level marriage rate at age k is equal to the fraction of women aged k in county i who are currently married (i.e., not widowed or divorced), regardless of whether the spouse is present or absent. Marriage rates therefore reflect the stock of existing marriages at each age. Throughout the marriage rate analysis, I interpret differences in marriage rates as stemming primarily from differences in flows into marriage (new marriages) rather than flows out of marriage (divorces), but these channels cannot be separately decomposed using the Census data. Divorce rates increased slightly during this period, but remained very low by contemporary standards.[50]

5 Empirical Strategy

To measure the impacts of unionization on fertility and marriage outcomes, I estimate the following long-difference model using OLS:

$$\mathrm{y}_{it} = \alpha_0 + \beta_{OLS}\mathrm{UnionRate}_{it} + \mathbf{X}'_{it}\Pi + \mu_i + \delta_t + \varepsilon_{it} \tag{1}$$

where i indexes counties and t indexes time periods. The variable y_{it} is an outcome, such as the birth rate or completed fertility, and $UnionRate_{it}$ is the union membership rate. The variables μ_i and δ_t represent county and time fixed effects, respectively, so β_1 is identified by changes in unionization within counties over time. The vector $\mathbf{X}_{it}$ contains time-varying controls, including state-year fixed effects. In the baseline specification, I weight by female population in each county-time cell and cluster standard errors at the county level.

When the outcome is a flow measure (e.g., birth rates), time periods correspond to years, and I estimate the long-difference model using union membership rates from 1934 (pre-NLRA) and 1960 (post-NLRA). I also impose a two-year lag structure between union membership rates and outcomes to take into account gestational lags and the time involved in family planning decisions.[51]

When the outcome is a stock measure (e.g, completed fertility), time periods refer to birth year cohorts. In addition, I re-specify the treatment variable in cohort-level regressions to be "union exposure" – equal to the average union membership rate in county i during birth year cohort t's peak childbearing years.[52] Women born in 1901 form the pre-period cohort, as they had mostly aged out of childbearing by the time the NLRA took effect in the late 1930s; I measure their union exposure from 1920-1925. Women

[50] The rate of divorces to new marriages was 0.17 in 1930, 0.26 in 1960, and has remained above 0.45 since 1975. (Carter et al., 2006)

[51] Schultz (1981) finds that the biological average to produce a birth is 24-31 months. In alternative specifications, I show that the main results are robust to varying to the lag structure.

[52] Women aged 19-24 had the highest age-specific birth rates during this period (Kirmeyer and Hamilton, 2011).

born in 1937, who experienced peak childbearing from 1956-1961, form the post-period cohort for the long-difference.

5.1 Instrumenting for Union Membership Rates

The parameter of interest β_1 is subject to bias if unobserved factors that vary within counties over time are correlated with both union membership rates and fertility. Ex ante, the direction of bias to expect from OLS estimates is not clear. For example, economic growth may have direct effects on fertility, but may also promote union growth through tight labor markets (Ashenfelter and Pencavel, 1969), resulting in a spurious positive relationship. On the other hand, if the pro-natalist effects of New Deal policies (Fishback et al., 2007) or household technology adoption (Greenwood et al., 2005) were concentrated in rural areas with relatively little union presence, OLS estimates may be biased downward.

I address the threats posed by unobserved confounders by constructing a shift-share instrument that isolates plausibly exogenous variation in local union membership rates. Following Collins and Niemesh (2019), this instrument combines temporal variation in national union membership rates at the industry-level with cross-sectional variation in lagged local industry shares:

$$\text{SSIV}_{it} = \sum_{j=1}^{N} \text{Natl Union Membership Rate}_{jt} \times \text{IndShare}_{ij}^{1910} \tag{2}$$

where i indexes counties, j indexes industries, and t indexes time periods. SSIV_{it} therefore predicts local union membership rates in year t[53] as the weighted average of national industry-level union membership rates, where the weights correspond to the local industry shares measured in 1910.[54] Appendix D provides details on the sources and methods used to construct the shift-share instrument.

The relevance of the instrument stems from the empirical fact that some industries have historically been more amenable to unionization than others. This heterogeneity is attributable in part to differences in cost structures: unions have greater bargaining power, and thus are more attractive to workers, in industries with long-lived capital investments and high profit margins (Grout, 1984). For example, the railroad and coal mining industries were some of the first to be organized, and remained union strongholds throughout the 20th century. Other features that have historically characterized union-friendly industries include a demand for scarce skilled labor, production that is highly time-sensitive, spatial isolation, and low substitutability between capital and labor (Stepan-Norris and Kerrissey, 2023).

I plot union membership levels and rates by industry group in Figure 7. Panels (a) and (b) plot series for major industry groups, and panels (c) and (d) plot data for several sub-industries within manufacturing. While most industries experienced union gains after the passage of the NLRA, there is considerable variation in the degree of unionization across industries. Union growth was particularly strong in the manufacturing, construction, and transportation/communications industries, but weak in the agriculture, government,[55] trade, services, and finance/real estate/insurance sectors. The mining

[53] In the same way that I aggregate and then average year-level union membership rates to construct union exposure, I transform SSIV_{it} to predict union exposure during peak childbearing years for cohort-level regressions.

[54] I measure industry shares in 1910 to ensure that variation comes from durable features of the local economy, not transient components related to World War I (1920) or the Great Depression (1930).

[55] Public employees were not covered by the provisions of the 1935 NLRA, and did not have the explicit right to collectively bargain in any local, state, or Federal jurisdiction until the late 1950s.

industry presents a more complicated case – union density among miners was relatively high prior to the NLRA, grew substantially after 1935 and during WWII, and then declined to pre-NLRA levels by 1960. Panels (c) and (d) show that even within manufacturing, some sub-industries were more amenable to union growth (e.g., chemical, rubber, and plastic products) than others (e.g., stone, clay, and glass products).

Intuitively, the shift-share instrument captures differential exposure to the NLRA shock as a function of long-standing local industrial composition. For example, consider two comparable counties without any union presence prior to the NLRA: Cameron County (1930 population = 5,307) and Forest County (1930 population = 5,180) in northern Pennsylvania. Appendix Figure A5 compares 1910 industry shares in each county. In panel (a), each point represents a different industry group, and the size of each point is scaled by the national change in unionization for that group. i.e., Transportation experienced the largest union shock, while Government experienced the smallest shock. Points above the 45 degree line correspond to industries for which Cameron County had a relatively larger share, while points below correspond to industries for which Forest County had a relatively larger share. There are several important differences between the two counties. First, Cameron County had a higher share of employment in Transportation, and a lower share in Agriculture. Second, although both counties had similar overall shares of manufacturing at baseline (30.1% in Cameron, 25% in Forest), the manufacturing subindustries characterized by stronger post-NLRA union growth (e.g., Metal, Machinery, and Equipment) were more prominent in Cameron County, while the manufacturing subindustries characterized by weaker union growth (e.g., Lumber, Wood, and Furniture Products) were more prominent in Forest County. Similarly, both counties had a mining presence, but Cameron County had a higher share in coal mining (relatively high union growth) and Forest County had a higher share in crude petroleum and natural gas extraction (relatively low union growth). Based on these characteristics, the shift-share instrument predicts that union membership rates will increase by more in Cameron County in the post-period – and in fact, by 1960, the union membership rate in Cameron County was 26.3%, but only 7.7% in Forest County.

Given that my research design emphasizes differential local exposure to relatively few industry-level shocks, I argue that identification follows from the exogeneity of the shares, rather than the shifts (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022).[56] Therefore, in a difference-in-difference framework, the key identifying assumption is that – conditional on controls – any unobserved factors that affect changes in unionization must not be jointly correlated with local industry shares measured in 1910 and changes in fertility. Since identifying variation comes from changes within counties over time, exogeneity in this setting does not require that high treatment areas are comparable to low treatment areas in level terms. And, importantly, this assumption is about exogeneity *conditional on observables*. To strengthen the case that shares are conditionally exogenous, the preferred specification includes an extensive set of economic and demographic characteristics, measured at baseline and interacted with time period fixed effects. Specifically, the set of baseline measures include: log population, the percent white, the percent male, and the percent foreign-born from the 1930 Census; the change in retail sales per capita from 1929-1933 from Fishback et al. (2003); and the change in birth rates from 1929-1933 from Bailey et al. (2016), which captures otherwise unobserved factors that may influence fertility and are related to broader economic trends. In augmented specifications, I additionally control for post-period shocks,

[56] I provide additional evidence on the validity of share-based identification in this setting in Appendix Table E.1.

including aid received through New Deal programs and variables that capture the impacts of World War II (war contract spending, registration rates, and casualty rates).[57]

While it is not possible to directly test the exogeneity assumption using potential outcomes in the post-period, a common indirect test of this assumption is to compare trends in observed outcomes in the pre-period. I supply evidence on pre-trends from reduced-form event studies in Sections 6.2 and 7.2.

The exclusion restriction requires that any effect of the shift-share instrument on fertility outcomes operates exclusively through the unionization channel. Though this assumption is also not directly testable, several points are worth noting. First, I use industry shares from several decades prior to the passage of the NLRA, which emphasizes long-standing features of local areas and purges the instrument of any direct impacts of unionization on industrial structures. Second, as I show in Sections 6.1 and 6.2, the timing of treatment effects is consistent with expected dynamics from the union shocks, but not with dynamics from other possible confounders. Third, in order to confound identification, alternative channels must be correlated with changes in both unionization and fertility, but orthogonal to the vector of controls. While it is not possible to rule out all such cases, the exclusion restriction is a much weaker assumption to satisfy after conditioning on observables.

Besides addressing omitted variable bias, the shift-share instrument may also reduce bias resulting from measurement error in the union membership data. Assuming the shift-share instrument is uncorrelated with classical measurement error in union membership (e.g., from noisy estimates based on voting strength), it will correct for attenuation bias present in OLS specifications. Moreover, the IV may correct for non-classical measurement error (e.g., from systematic under- or over-statement of paid-up membership) if the correlation between the instrument and the error term is sufficiently lower[58] than that between union membership estimates and the error term – though the direction of the bias correction is *ex ante* unknown.

6 Effect of Unionization on Birth Rates

Annual birth rates nearly doubled during the Baby Boom, from 69 live births per 1000 women of childbearing age in 1936 to 120/1000 in 1957. As a contemporaneous measure, birth rates may capture transitory impacts on fertility if treatment causes families to change the timing but not the ultimate number of births. However, birth rates are less sensitive to the possibly confounding impacts of migration, mortality, and other factors that may intervene between the time of treatment and the time that completed fertility is measured. Birth rates also shed light on how fertility dynamics evolve in real-time, which helps pin down the time path of treatment effects and allows for more direct tests of key identifying assumptions.

6.1 Long-Difference Results

The long-difference compares changes in birth rates that result from changes in union membership rates from 1934 to 1960. Since the birth rate analysis is at the county-year level, the independent variable

[57] I provide detailed descriptions of the methods and sources used to construct each of these variables in Appendix G.

[58] The degree of correlation between the instrument and the error term must be some fraction of the correlation between the endogenous variable and the error term for IV to outperform OLS in this respect. The strength of the first-stage determines the size of that fraction.

of interest is the union membership rate. I instrument for union membership rates using the shift-share instrument defined in Section 5.1. To take into account gestational lags plus the time involved in family planning decisions, I assume that birth rates lag union membership rates by two years. In alternative specifications (see Appendix Figure A6), I find that the main results in this section are not sensitive to this particular lag structure. Birth rates are by place of occurrence prior to 1935, and by place of residence from 1935 onward; Section 4.2 provides a detailed description of these measures.

The first-stage is given by:

$$\text{UnionRate}_{it} = \gamma_0 + \gamma_1 \text{SSIV}_{it} + \mathbf{X}'_{it}\Pi + \mu_i + \delta_t + \nu_{it} \tag{3}$$

where γ_1 represents the percentage point increase in actual union membership rates associated with a 1pp increase in SSIV-predicted union membership rates.

The reduced-form equation is:

$$\text{Birth Rate}_{it} = \beta_0 + \beta_1 \text{SSIV}_{it} + \mathbf{X}'_{it}\Pi + \mu_i + \delta_t + \varepsilon_{it} \tag{4}$$

where β_1 represents the increase in the birth rate associated with a 1pp increase in predicted union membership rates. The 2SLS coefficient of interest is therefore $\beta_{2SLS} = \frac{\beta_1}{\gamma_1}$, which estimates a local average treatment effect (LATE).

I present the first-stage results in Table 2, and the OLS, Reduced-Form, and IV/2SLS results in Table 3. The sample includes all counties in the five states with county-level union membership data. Following the baseline specification, I weight county-year cells by female population and cluster standard errors at the county-level. In each table, column 1 includes county, year and state $\times$ year fixed effects, column 2 includes the baseline set of controls interacted with year fixed effects, and column 3 additionally includes the post-period controls.

As expected, the first-stage between SSIV-predicted and actual union membership rates is strong. The point estimates suggest that there is near a one-to-one correspondence between SSIV-predicted and actual union membership rates. In Panel C of Table 3, $\hat{\beta}_{2SLS}$ from the specification with baseline controls (column 2) implies that a 10pp increase in the local union membership rate is associated with about 9 additional births per thousand women of childbearing age, a 14% increase over the base period mean. Since average union membership rates increased by 14pp from 1934-1960 and average birth rates increased by 46/1000, $\hat{\beta}_{2SLS} = 0.87$ suggests that unionization can account for about 25% of the overall growth in birth rates in this sample.

The difference between OLS and 2SLS estimates indicates that OLS is biased downward. This may be because the shift-share instrument purges additional omitted variable bias present in OLS estimates. However, given that the IV-OLS gap persists even after conditioning on the large set of controls, a likely explanation is that OLS estimates are attenuated by classical measurement error stemming from noise in the union membership data. Non-classical measurement error may also play a role. For example, the source data may under-estimate true union membership in highly treated areas, perhaps due to the non-uniform distribution of members affiliated with unobserved independent unions.[59] Finally,

[59]The data on observed independent unions (Teamsters, UE, UMWA) in my sample suggest that this is plausible. There is a positive county-level correlation between 1934-1960 growth rates in AFL/CIO/AFLCIO union membership and 1934-1960

treatment effect heterogeneity could account for differences between the ATE estimated by OLS and the LATE estimated by 2SLS. Compliers in this setting are, roughly speaking, areas that were amenable to unionization based on their underlying industrial composition and were treated as a result of the national NLRA shock, but would have remained untreated absent the NLRA shock. If such areas had a lower base of fertility than comparison areas in the pre-NLRA period,[60] their relative response to the NLRA shock may have been greater due to ceiling effects.

As an indirect rest of the exclusion restriction, I re-specify the long-difference to estimate the effect of changes in union membership rates between 1920 and 1934 on outcomes. Intuitively, if underlying industrial composition affects changes in fertility only through changes in unionization, it should be the case that there is no reduced-form relationship between changes in SSIV-predicted union membership rates and changes in birth rates in the pre-NLRA period. I present the results in Appendix Table A1.[61] Although the OLS point estimate with the baseline controls (Panel A, column 2) and the reduced-form estimate with fixed effects only (Panel B, column 1) are positive and significant at the 5% level, there is a null reduced-form effect after including the full set of controls. Taken together, these results indicate that while neither the shift-share instrument nor the controls may be sufficient to satisfy key identifying assumptions on their own, using the instrument in combination with the controls likely yields a valid empirical design. Moreover, the first-stage is weak. This is important because it suggests that the instrument captures plausibly exogenous variation in union membership rates that arises specifically from the combination of longstanding local latent demand for unionization and the national NLRA policy shock, and not other potentially endogenous local factors.

I conduct a series of other robustness checks and summarize the results in Appendix Figures A6 and A7. In Appendix Figure A6, each plotted point (in descending order by first-stage F-statistic) corresponds to an estimate of β_{2SLS} from a different regression, and whiskers represent 95% confidence intervals. I include the baseline set of controls and fixed effects in all specifications. First, to address the concern that treatment effects are driven by industry-level shocks to labor demand, I additionally control for an index meant to capture local shifts in the relative demand for skilled and unskilled workers. Following Goldin and Margo (1992), this index combines variation in the local skill mix within each industry (measured in 1940) with national shifts in industry employment shares.[62] In additional checks, I test for sensitivity to removing population weights, controlling for changes in the local share of the labor force in manufacturing,[63] dropping urban and rural outliers from the sample, alternative standard error estimation procedures, and variations in the lag structure between treatment and outcomes. Though there is some variation, all point estimates are positive and statistically significant at the 5% level, and tend close to the baseline estimate. In Appendix Figure A7, I show that the results are not substantively impacted by using alternative specifications of the start and end years in the long-difference.

growth rates in independent union membership.

[60] I find evidence for this in Figure 8.

[61] County-level birth rates are not available in Missouri until 1927 (see Appendix Table A6), so the sample includes only CA, IL, PA, and WI.

[62] I describe the construction of this index in detail in Appendix G. Following Collins and Niemesh (2019), I define "skilled" workers to be those with a high school degree. Such workers made up roughly 30% of the labor force in 1940. Unfortunately, educational attainment is not available from the U.S. Decennial Census until 1940, so I cannot measure local skill mixes based on educational attainment using 1930 (pre-NLRA) data.

[63] Note that I construct the shift-share instrument using data at the sub-industry level within the manufacturing sector – see Appendix D. Therefore, it is possible to control for the manufacturing share at a coarser level.

6.2 Event Study Results

The key identifying assumption is that, absent the NLRA shock, outcomes would have evolved on similar paths across areas over time. I provide supporting evidence for this assumption by estimating the reduced-form relationship between predicted exposure to the NLRA shock and birth rates using an event study design, which resembles Equation 4:

$$\text{Birth Rate}_{itq} = \beta_0 + (\mathbf{I}_{t\neq 1935} \times \text{D}_{iq})'\boldsymbol{\beta} + \mathbf{X}'_{itq}\Pi + \mu_i + \delta_t + \varepsilon_{itq} \tag{5}$$

where $\mathbf{I}_{t\neq 1935}$ is a vector of indicator variables for each birth year, omitting 1935 – the year the NLRA was passed – as the base year.

Since the treatment dose is continuous across areas, I bin sample counties in quintiles q according to the SSIV-predicted change in union membership rates between 1934-1960. Callaway et al. (2024) show that in difference-in-differences settings with continuous treatment, disaggregating by discrete treatment groups allows for a straightforward assessment of the parallel trends assumption without regard to within-group variations in treatment intensity. In order for group-specific treatment effects to be uncontaminated by selection bias, however, it is necessary to assume that there is no treatment effect heterogeneity across groups. I find that this "strong" parallel trends assumption may be difficult to satisfy in this context (see Appendix E.3), and so I interpret the results with some caution.

Historically, county-level data on birth rates were only available for states that were part of the U.S. Birth Registration Area (BRA). The BRA initially covered 10 states in 1915, and achieved complete coverage of all 48 states in 1933 (see Appendix Table A6). Throughout this section, I restrict to counties that were in BRA-covered states as of 1927. This sample includes counties in 40 states (including all five states in the main analysis sample) that together accounted for 87.6% of the total U.S. population in 1930.

I plot (population-weighted) average birth rates by year and treatment quintile from 1927 onward in Figure 8. Pre-trends are similar across treatment groups: birth rates decline everywhere through the trough of the Great Depression in the early 1930s, then level off by 1935. Beginning in the late 1930s, birth rates in high treatment counties converge toward those in low treatment counties. Convergence accelerates during World War II, such that by 1945 the gap between high and low treatment areas is noticeably smaller. There is a clear level shift in fertility across all areas immediately after WWII, which likely captures births that were postponed during the war and are unrelated to unionization. Convergence resumes in the 1950s, and birth rates are nearly identical in all but the highest treatment areas by 1960. Overall, the basic profile of the Baby Boom is reflected in the high treatment counties, but – with the exception of the immediate postwar years – there is relatively little increase in birth rates in low treatment counties from 1935-1960.

I present results from the reduced-form event studies for counties in the main analysis sample in Figure 9. Since the NLRA was a national shock to which all counties had some exposure, there are no "never-treated" units. However, counties in quintile 1 provide the best approximation for never-treated units, so I first estimate the model separately for quintiles 2, 3, 4, and 5, with quintile 1 serving as the reference group in each case (panel A). In panel B, I re-estimate the reduced-form event study using quintile 3 – counties that received an "average dose" of treatment – as the omitted reference group. In each case, the base year is 1935, and specifications include fixed effects and the baseline controls. There is no evidence of differential pre-trends across treatment groups. After 1935, effects tend to increase monotonically with treatment intensity. The largest year-over-year effects occur during World War II, when unionism among industrial workers surged for the first time, and in the early 1950s, in the wake of landmark collective bargaining agreements that increased wages, expanded healthcare coverage, and guaranteed pension benefits.[64] Notably, treatment effect dynamics do not closely track expansions and contractions in the American business cycle; for example, differences between high and low treatment groups steadily increased during WWII as the economy expanded, but remained large in the late 1940s during the postwar contraction. This suggests that economic growth plays at most a limited role in confounding identification of the main effects.

6.3 Generalizing the Results

I have shown that unionization had robust effects on birth rates in the five states in the main analysis sample. To test whether these results generalize to a national sample, I extend the analysis from Section 6.1 in two ways. First, I estimate the reduced-form relationship between SSIV-predicted union membership rates and birth rates for a sample of all U.S. counties. Second, I incorporate additional data sources to measure the effect of unionization on birth rates at the state-level.

I present reduced-form results at the county-level in Table 4. I separately estimate β_1 from Equation 4 for the main analysis sample of counties – i.e., those in the five states with county-level union membership data – in columns 1 and 2, and for a national sample of counties in columns 3 and 4. The reduced-form point estimates are always positive and are comparable across samples, both with and without added controls, suggesting that the main effects from Section 6.1 are not driven by idiosyncratic features of counties in the main analysis sample.

In Table 5, I re-estimate β_{OLS} from Equation 1 using state-level data from alternative sources. In column 1, I draw on Farber et al. (2021)'s survey-based estimates of union membership rates; in column 2, I use Troy and Sheflin (1985)'s union membership estimates, which are based on union reports, archival sources, and personal correspondence. Since neither source produces estimates for the pre-NLRA period, the long-difference is slightly modified from Section 6.1. The Farber et al. (2021) series begins in 1937, with annual estimates thereafter. Therefore, the long-difference in column 1 compares changes in outcomes resulting from changes in union membership rates measured in 1937-1960. Troy and Sheflin (1985) only report state-level estimates for selected years, including 1939 and 1960. As a result, the long-difference in column 2 compares changes in outcomes resulting from changes in union membership rates measured in 1939-1960. I include state and year fixed effects in all specifications. Overall, the results provide strong evidence for an economic relationship between unionization and fertility that generalizes across samples and data sources. IV point estimates (panel C) are comparable across specifications, and imply

[64] One such agreement was the "Treaty of Detroit", negotiated by UAW leader Walter Reuther with Ford, Chrysler, and General Motors in 1949-1950. The contract included new provisions for healthcare coverage, unemployment benefits, pension plans, and wage increases that became a template for collective bargaining agreements in the automotive industry, as well as other industries, for decades. The annual wage in the auto industry increased from $2,998 in 1947 to $5,409 in 1958 (Barnard and Handlin, 1983). Another important development in this period was the rollout of the UMWA's Welfare and Retirement Fund in the early 1950s. Figinski and Troland (2018) find that the effect of the UMWA's expansion of health insurance to Appalachian miners was "larger than that associated with the initial rollout and subsequent expansions of Medicaid."

that a 10pp increase in union membership rates is associated with 8-10 more births per 1000 women of childbearing age, about a 12% increase over the base period mean.

The state-level estimates tend to be smaller than the corresponding county-level estimate of β_{OLS} from column 1 of Table 3 (=1.708). However, it is difficult to know whether these differences arise from attenuation bias in the state-level analysis, the presence of within-state spillovers, or discrepancies in the sources and methods used to measure union membership. I revisit the role of spillovers in Section 7.4.

7 Effect of Unionization on Completed Fertility

Completed fertility increased by more than 35% during the Baby Boom – from a trough of 2.7 births per woman for the cohort born in 1906 to a peak of 3.65 births per woman for the cohort born in 1931. In this section, I extend the analyses from Section 6 by testing whether unionization drove increases in completed fertility, and examine the age profile of treatment effects. I also shed light on the role of spillovers by decomposing the effects on completed fertility using within-county variation in exposure to treatment.[65]

7.1 Long-Difference Results

The long-difference analysis follows Section 6.1. However, since completed fertility varies by birth year cohort, the independent variable of interest is union exposure – the average union membership rate experienced during peak childbearing years in county i by cohort t. I compare outcomes for cohorts born in 1901 and 1937, for whom union exposure is measured in 1920-1925 and 1956-1961, respectively.

I present the first-stage results in Table 6. There is a strong and positive first-stage relationship: F-statistics[66] are well above conventional thresholds, and the point estimates imply that there is approximately a one-to-one correspondence between union exposure as predicted by the SSIV and actual union exposure.

I report the OLS, reduced-form, and 2SLS results in Table 7. The sample includes counties in the five states with union membership data. To construct a balanced panel with sufficiently large cell sizes, I drop any county with fewer than five underlying observations in any county-cohort cell.[67] Estimation and inference follows the baseline specification from Section 5: I weight by the female population in each county-cohort cell, and I cluster standard errors at the county level.

OLS results (panel A) provide evidence for a positive relationship between union exposure and completed fertility. The magnitude of β_{OLS} decreases after adding the full set of controls (column 2). Such upward bias is consistent with greater post-NLRA union growth in areas with strong economic growth, and with selection of high-fertility immigrants into highly unionized areas, for example.

In panel C, the estimate of β_{2SLS} from the fully-saturated model (column 2) is statistically significant at the 5% level, and implies that a 10pp increase in union exposure is associated with a 0.08 increase in TCFR, a 3% increase over the base period mean. A back-of-the-envelope calculation suggests that this effect size is moderately large, but plausible. Within this sample, average union exposure increased by

[65] Throughout this section, I use total cohort fertility rates (TCFRs, as described in Section 4.3), as the primary outcome. I show supplementary results using the alternative children ever born outcome in Appendix Tables A2 and A3.

[66] Since I do not assume i.i.d. standard errors, I report the Kleibergen and Paap (2006) rk F statistic.

[67] I show in Appendix Figure A8 that the results are unchanged when this restriction is not applied.

12pp from the 1901 to the 1937 birth year cohorts, while average completed fertility increased by about 0.5 children per woman. Therefore, $\hat{\beta}_{2SLS} = 0.008$ implies that union growth accounts for approximately 20% of the overall increase in completed fertility during this period. Reassuringly, this effect size is comparable to the implied magnitude of the treatment effect from Section 6.1, which could account for approximately 25-30% of the overall increase in birth rates. The fact that birth rate effects are relatively larger than completed fertility effects may be because birth rates include a transient component resulting from the intertemporal substitution of births across the cycle of childbearing years, in addition to a more permanent component. As in Section 6.1, IV estimates are larger than OLS estimates, which is consistent with attenuation bias from measurement error in the union membership data.

I perform several robustness checks, summarized in Appendix Figure A8. Each plotted point (in descending order by first-stage F-statistic) represents an estimate of β_{2SLS} from a different regression, and whiskers correspond to 95% confidence intervals. I include the full set of controls and fixed effects in all specifications. To address the concern that areas with large initial shares of employment in construction may experience differentially high growth in the supply of single family dwellings which may directly impact fertility, I reconstruct the shift-share instrument after leaving out Construction employment. I also test whether the main results are sensitive to alternative specifications of the union exposure variable, and to varying the birth year cohorts included in the long-difference analysis. In additional checks, I show that the results are not driven by a few highly populated cities or rural outliers, are robust to various standard error estimation procedures, and are not sensitive to the removal of regression weights. While not all of these estimates are precise enough to be statistically significant, they are uniformly positive and are centered around $\hat{\beta}_{2SLS}$ from column 2 of Table 7.

I also decompose the shift-share instrument and conduct a series of empirical tests to shed light on the identifying variation underlying the IV estimates. Goldsmith-Pinkham et al. (2020) show that shift-share instruments can be interpreted as over-identified estimators that aggregate a set of individual, just-identified instruments – the predetermined local industry shares – using a set of "Rotemberg" weights. I present detailed results in Appendix E, and note several key points here. First, variation in the national industry-level shocks explains only about 25% of the variation in the Rotemberg weights, which signals that identifying variation in the aggregate shift-share instrument is driven primarily by the plausibly exogenous shares. Second, negative weights account for a relatively small share of total Rotemberg weights, so the shift-share estimator permits a LATE-like interpretation. Third, reduced-form event studies that use the top industries by Rotemberg weight as separate just-identified instruments provide support for key identifying assumptions. Finally, there is evidence of considerable treatment effect heterogeneity across industries, which is a likely explanation for the rejection of the null in a Hansen test of the over-identified model.

7.2 Event Study Results

I adapt the reduced-form event study from Equation 5 to assess pre-period trends in completed fertility and the timing of treatment effects. Completed fertility event studies are at the county-cohort level, and treatment groups are defined by changes in SSIV-predicted union exposure for cohorts born 1901-1937. Otherwise, the model specification follows Section 6.2.

I preview the event study results in Figure 10, which plots average TCFRs over time by treatment quintile for all counties in the U.S. For cohorts born prior to 1905, trends are similar across groups as completed fertility is generally in decline. Starting with cohorts born around 1905, the trend breaks and completed fertility increases in all groups except the lowest treatment quintile. Since women born around this time were in their early 30s when the effects of the NLRA began to diffuse, this is consistent with the timing of the union shock. From the 1915 birth year cohort onward, convergence accelerates across groups, and relative fertility increases are especially large in the highest treatment quintile. These cohorts would have been the first to experience the impacts of broad-based union growth during their peak childbearing years. For cohorts born in the 1930s and later, completed fertility differentials across groups are largely eliminated. There is a secular decline in completed fertility toward the end of the measurement period, which is likely due to factors unrelated to unionization; e.g., the proliferation of the first easy-to-use and reliable contraceptives, which impacted fertility through both direct and indirect (e.g., marital formation, investment in human capital) channels (Bailey, 2006).

I present the reduced-form event study results in Figure 11. I plot coefficients and 95% confidence intervals for each quintile. In panel A, I estimate results using quintile 1 as the omitted reference group; in panel B, I estimate results using quintile 3 as the omitted reference group. The sample includes counties in the five states with union membership data, and I include a parsimonious set of controls including cohort-, county-, and state-cohort fixed effects. I select 1902 as the base birth year.[68] Pre-period trends suggest that groups were trending in similar ways until around the 1905 birth year, but begin to diverge thereafter. Relative increases in completed fertility are largest in the highest treatment quintile, and relative decreases in completed fertility are largest in the lowest treatment quintile. Effects on completed fertility accumulate over time, consistent with increasing exposure to treatment for cohorts who were younger at the time the NLRA was passed. Differences across groups stabilize for cohorts born in the mid-1930s and later, which corresponds to the levelling off and eventual decline of union growth in the decades after WWII.

7.3 Decomposing Effects by Age

The growth in completed fertility during the Baby Boom was driven by increases in childbearing across the full range of reproductive years. The most dramatic fertility increases were among young women: from 1940 to 1960, birth rates nearly doubled among women aged 15-24. However, during the same period birth rates also grew by 61% among women aged 25-29, by 35% among women aged 30-34, and by 21% among women aged 35-39 (Grove and Hetzel, 1968). In this section, I decompose the aggregate relationship between unionization and completed fertility by estimating treatment effects by age. The analysis sheds light on the features of union growth that are most likely to have played a role in promoting fertility, and helps situate the main results among existing explanations of the Baby Boom.

I estimate the average stock of births at each age using a variation of the method used to construct TCFRs (see Section 4.3). Whereas the TCFR measure captures births during all childbearing years (ages

[68] At the cohort-level, it is not clear how the base year should be defined, since treatment tends to increase continuously across a range of birth year cohorts. I select 1902 as the base year for essentially the same reason that I select women born in 1901 as the pre-period cohort for the long-difference: women born in 1902 would have been 33 at the passage of the NLRA, 35 when the NLRA was upheld by the Supreme Court, and 39 at the beginning of World War II. So while it is not implausible that this cohort could have received some exposure to treatment at the end of their childbearing years, the magnitudes of any such impacts must have been relatively small.

15-44), here I construct separate outcomes for each age k by measuring the cumulative sum of births born to mothers aged 15 to k. As with TCFRs, I estimate average outcomes by county and birth year cohort.

Figure 12 traces out the age profile of cumulative births by treatment group and birth year cohort for a national sample of counties. As in Section 7.2, I bin counties into treatment group quintiles based on the SSIV-predicted change in union exposure between the pre- (1901) and post-period (1937) cohorts, and estimate group-level averages for each cohort. For ease of interpretation, I plot estimates only for the lowest (quintile 1) and highest (quintile 5) treatment groups.

Until about age 20, outcomes are comparable across both treatment groups and birth year cohorts.[69] Starting in the early 20s, outcomes begin to diverge. There is a general increase in childbearing from the 1901 to the 1937 cohorts, but increases relative to 1901 are much larger for high treatment areas. Through the late 20s and into the early 30s, post-period outcomes for the lowest treatment group converge back to – and ultimately exceed – pre-period levels. These dynamics suggest that women in counties that received a low treatment dose experienced some changes in the timing of births, but the average stock of births actually decreased over time. By contrast, the gap between pre- and post-period outcomes in high treatment areas widens through the late 20s, stabilizes in the early 30s, and remains large through the end of childbearing years. The age profile of births for women in these counties is therefore consistent with well-documented features of the Baby Boom: gains in completed fertility were driven by increases in childbearing that began in the early 20s and persisted through the end of the reproductive years.

I formalize the comparisons made in the descriptive analysis above by re-estimating the IV long-difference model from Section 7.1 using cumulative births at each age as separate outcomes. As in Section 7.1, the long-difference measures the effect of changes in union exposure on changes in outcomes for cohorts born in 1901 and 1907. I follow the baseline specification and include the full set of controls and fixed effects. I present OLS and IV results for the main analysis sample, as well as reduced-form results for a national sample of counties, in Figure 13. Though IV estimates (panel B) are often not statistically significant, treatment effects appear to steadily accumulate with age, with an upward trend that begins in the early 20s. The OLS estimates (panel A) follow the same general pattern but are smaller than IV estimates, possibly due to attenuation from measurement error. The reduced-form estimates (panel C) from the national sample of counties are more precise and also suggest that treatment effects accumulate with age.

The results suggest that the effects of unionization are concentrated on births to women who were already beyond peak childbearing years.[70] Through age 25, I can reject (at the 5% level) that a 10pp increase in union exposure leads to changes in cumulative fertility of greater than $\pm$ 0.07 births per woman. Since some union benefits accumulate over time due to seniority rules, one interpretation of these results is that treatment effects grow with age because treatment intensity increases with age. An alternative explanation is that the impacts of treatment are not immediately felt by younger couples; for example, collectively-bargained job protections might generate relative benefits for union households only upon the realization of a future labor market shock. Unfortunately, I cannot separately identify

[69]There is a kink in the birth profile around age 18 for cohorts born in 1901. This is likely due to the effects (lagged by 9 months) of the U.S.'s involvement in WWI and the influenza pandemic of 1918-1919.

[70]Consistent with these results, I show in Appendix F that unionization is not associated with significant changes in the average age of mothers at first birth.

contemporaneous and dynamic effects of treatment from this decompositional exercise. In addition, given that this analysis only includes women born in 1901 and 1937, the results may not be representative of the age profile of treatment effects for all post-period cohorts. Despite these limitations, the descriptive fact that unionization appears to explain a relatively greater share of the increase in later childbearing is a notable point of divergence from other prominent explanations of the Baby Boom that emphasize effects on early childbearing (e.g., Doepke et al. (2015)).

7.4 Spillovers

A large literature considers the ways in which developments in the union sector may influence conditions in the non-union sector (e.g., Simons (1948); Lewis (1963); Kahn (1978); Neumark and Wachter (1995)). On the one hand, if gains won through collective bargaining causes unionized shops to reduce payroll, workers in the non-union sector may face increased competition, leading to lower wages or disemployment through "crowding effects" (Simons, 1948). On the other hand, the threat of unionization may induce unorganized firms to proactively offer increased benefits to retain their workers (Rosen, 1969). Union strength may also generate local spillovers through political channels.[71]

The county-level analysis captures the equilibrium impact of unionization, net of spillovers that occur at the level of local labor markets.[72] This is a necessary feature for explaining aggregate phenomena such as increasing fertility during the Baby Boom. However, since aggregate effects subsume both both direct effects (i.e., those on union households in treated areas) and spillovers (effects on non-union households in treated areas, and on union households in non-treated areas), they may obscure the economic mechanisms that connect treatment to outcomes.

In this section, I decompose the equilibrium effects of unionization on completed fertility using within-county variation in exposure to treatment. Specifically, I construct TCFRs separately by the major industry group of the household head[73] and estimate variants of the long-difference model at the county-cohort-industry level. The OLS estimating equation is:

$$\text{TCFR}_{ijt} = \beta_0 + \beta_1 \text{UnionExp}_{it} + \beta_2 \text{UnionSec}_j + \beta_3 \text{UnionExp}_{it} \times \text{UnionSec}_j + \mathbf{X}'_{it}\Pi + \mu_i + \delta_t + \varepsilon_{ijt} \quad (6)$$

where i and t index counties and birth year cohorts, respectively, and j indexes major industry groups. $UnionSec_j$ is an indicator for the union sector, and is equal to one for households attributed to the construction, manufacturing, mining, or transportation/communications industries.[74] The coefficient on

[71]For example, Zuberi (2019) studies the efforts of the UNITE HERE! union to organize hotel workers in the U.S. and Canada in the 2000s. The author argues that one result of the UNITE HERE! campaign was the passage of new labor code protections that covered all workers in British Columbia, including the right not to be fired without just cause, two weeks paid vacation after one year of employment, and one year of paid maternity leave.

[72]Union strength may also generate spillovers that extend beyond county boundaries. For example: "The success of the UAW and other unions in securing pensions led to increases in Social Security payments that benefitted [*sic*] even unorganized workers. Social Security was funded by a payroll tax levied equally on employer and employee; negotiated pensions, however, as in the auto industry, were usually funded solely by the employer. Thus the greater the share of pension costs the employer could shift to Social Security, the less his burden" (Barnard and Handlin, 1983). If such spillovers promoted fertility in the broader non-union sector, county-level estimates may understate the true equilibrium effect of union gains.

[73]About 10-15% of household heads in each Census do not have an attributed industry. These individuals may be unemployed or out of the labor force, or may work in non-classifiable industries. Since the analysis in this section requires assignment to a major industry group, I drop all unattributed households from the sample.

[74]Since union density in the mining industry was near pre-NLRA levels by 1960, I estimate alternative specifications that

the interaction term, β_3, identifies the effects of place-based union shocks on households with heads attributed to the union sector. Since these households are most likely to be directly impacted by union shocks, I interpret β_3 as an estimate of direct effects. β_1 is therefore interpretable as an estimate of within-county spillover effects (those on households attributed to the non-union sector) and β_2 is interpretable as an estimate of within-industry spillover effects (those on union sector households in places that did not receive a union shock). As before, I also estimate an IV specification using SSIV-predicted union exposure as an instrument for union exposure.

I present OLS and IV results for the main analysis sample in Table 8. I weight by the number of households in each county-cohort-industry cell and include the full set of controls and fixed effects. OLS and IV coefficients on the interaction term (β_3) are positive and statistically significant at the 1% level, signifying that union shocks had positive effects on completed fertility for households in the union sector. Coefficients on the union exposure term (β_1) are positive but not statistically significant, which suggests that the aggregate effect of unionization is driven primarily by direct effects. However, it is possible to rule out large negative within-county spillover effects. Based on IV estimates, I can reject (at the 5% level) that a 10pp increase in local union exposure causes completed fertility to decrease by more than 0.04 children per woman (1.5% of the base period mean) in the non-union sector. Coefficients on the union sector indicator (β_2) are positive, large, and significant at the 1% level. This suggests that, conditional on residing in an area with little to no union presence, households in the union sector received substantial positive spillovers from secular increases in unionization in their industries. This result is consistent with the historic role that landmark collective bargaining agreements played in setting industry-wide wage and benefit patterns (e.g., in the automotive industry).

In the absence of variables capturing union membership status in the Census microdata, industry-level variation provides a useful approximation of household-level exposure. However, given that (1) not all households in the union sector will actually be directly affected by treatment and (2) some households in the non-union sector will be directly treated, the sector-level approach may yield underestimates of direct effects and overestimates of spillover effects. In addition, this analysis cannot identify within-sector spillovers; e.g., if union gains in the construction industry contributed to pay increases among unorganized laborers employed in manufacturing, effects on both groups will be aggregated under direct effects. Finally, inclusion in the sample is conditional on employment. Estimates of direct and indirect effects may therefore be biased by endogenous compositional changes resulting from the impacts of local unionization on the labor force.

8 Effects of Unionization on Marital Formation

In this section, I supplement the main fertility results by estimating the effect of union membership on marriage rates. Since nearly all children in this period were born to married mothers,[75] it is difficult to make sense of the relationship between unionization and fertility without also considering the impacts of unionization on marital formation. Moreover, to the extent that marriage rates reflect broader economic conditions and determine how resources are allocated across households, they represent a key outcome of

do not classify mining in the union sector. The results (available on request) are qualitatively similar.

[75] The share of children born to unmarried mothers was 4% in 1940 and 5.3% in 1960 (Ventura, 1995).

interest, independent of implications for fertility.

I depict the evolution of national age-specific marriage rates over time in Figure 14. I plot marriage rates separately at age 20 (panel A), 25 (panel B), and 30 (panel C). As in Section 6.2, I construct treatment group quintiles based on changes in the SSIV-predicted union membership rate from 1934-1960, and estimate group-level averages in each year. Marriage rates were stable prior to WWII, implying that pre-war increases in fertility were likely due to an increase in births within existing marriages, not additional births resulting from a surge in new marriages. In addition, trends are comparable across areas through 1940, which suggests that unionization played at most a limited role in influencing marital formation prior to WWII. In the post-war period, marriage rates increase everywhere, but increases are especially large in high treatment areas. As a result, there is significant convergence in outcomes across groups by 1960.

To measure the causal effects of union membership rates on marital formation, I re-estimate the IV long-difference model from Sections 6.1 and 7.1 using marriage rates at each age as separate outcomes. The long-difference measures the effect of changes in union membership rates from 1934-1960 on changes in outcomes measured in 1930 and 1960. I follow the baseline specification and include the full set of controls and fixed effects. I plot the OLS, IV, and reduced-form estimates in Figure 15. From the IV estimates in panel B, I find that unionization is associated with an increased propensity of marriage for women aged 25-35. In particular, IV estimates imply that a 10pp increase in union membership rates is associated with a 2-4pp increase in marriage rates, which accounts for about 20-30% of the overall increase in marriage rates for these age groups. As in the case of the main fertility results, OLS estimates (panel A) are positive but smaller than IV estimates, likely due to attenuation from measurement error.

Without retrospective data on marriage histories and exposure to treatment, I cannot separately identify the effect of unionization on the timing of marriages from differential effects of unionization on marriage propensities across cohorts. For example, the IV results from Figure 15 suggest that there are no treatment effects for women aged 36 and older. This is consistent with unionization speeding up the formation, but not affecting the ultimate stock, of marriages. However, an alternative interpretation is that older cohorts in 1960 – who reached marrying age in the late 1930s – received a lesser treatment dose than younger cohorts measured in the same year. Since I attribute treatment based on union membership rates in 1960 to all cohorts whose marriage rates were measured in 1960, it is possible that I overstate exposure to treatment for older cohorts, resulting in downwardly biased estimates.[76] In addition, if migration is correlated with treatment and marriage outcomes, the results may be upwardly biased by selection effects; e.g., if married households seek out areas with many available unionized jobs, or if unmarried women out-migrate from highly unionized areas in search of job opportunities.

Despite issues of interpretation, the results in this section indicate that unionization likely played some role in driving the "Marriage Boom" that accompanied the Baby Boom. Just as marriage and childbearing are inextricably linked, the marriage and fertility analyses are mutually self-reinforcing: many of the same mechanisms that plausibly connect unionization to fertility are also related to marriage decisions, and increases in marriage rates could mechanically lead to increases in fertility. These results underscore how the labor movement's impact extended beyond a narrow set of economic outcomes to

[76] The fact that reduced-form estimates from the national sample (panel C of Figure 15) remain positive and statistically significant through age 40 suggests that measurement error may in fact attenuate OLS and IV estimates.

influence the broader social and demographic landscape in the U.S. during the mid-20$^{\text{th}}$ century.

9 Mechanisms

Union membership was associated with an extensive bundle of benefits for workers in this era. In particular, previous work on the economic determinants of fertility suggests that union membership may influence family formation through wage increases, job protections, and improved benefit packages, among other channels. Union membership may also impact fertility through spillover effects that operate through marriage and labor markets. In this section, I identify the features of union membership that are most strongly associated with fertility increases. Though not causal, these results shed light on the particular economic mechanisms that underlie the main results in preceding sections.

9.1 Individual-Level Results

I return to the Palmer Survey (1951) from Section 3 to explore mechanisms at the individual-level. The Palmer Survey is unique among surveys of this era in that it provides retrospective data on employment histories, along with union status and a measure of household size, for a relatively large sample of workers (Callaway and Collins, 2018). Retrospective data is particularly important in this setting; for example, the impact of stronger job protections on fertility may only be detectable over an extended period of time. Relative to point-in-time measures from other cross-sectional sources, the Palmer data are well-suited to capture such dynamic effects.

The analysis broadly follows that of Section 3.2. I use the number of own children under age 18 in the household as a stock measure of fertility, and I capture union status using a dummy variable for union membership at time of measurement. As in Section 3.2, the sample includes male household heads in five Northern labor markets (Los Angeles, New Haven, Philadelphia, San Francisco, St. Paul). However, unlike Section 3.2, here I further restrict to individuals aged 25-39, for whom measurements of the economic variables of interest more closely correspond to peak years of family formation.

I present the results in Table 9. Each column reports estimates from separate regressions of the number of own children in the household on union status and the mechanism variables specified in each row. Following Section 3.2, all specifications additionally include city fixed effects and controls for race, age, a quadratic in age, and occupation. I first replicate the baseline cross-sectional result, which suggests that union members have 0.13 more children, on average. I then progressively add covariates that capture various channels through which union status might influence fertility, including weekly earnings from the longest job held in 1950[77] and several variables relating to job security over the past decade. I also conduct an indirect test of the role of expectations and "relative income" (Easterlin, 1968) by including a measure that captures the change in occupational status of sample respondents relative to their fathers. I construct this measure by linking each respondent's own occupation and their father's occupation (as

[77]Unfortunately, information on earnings from previous jobs was considered to be less reliable than employment histories, and so was not extensively used by survey analysts (see pg. 11 of Palmer and Brainerd (1954)). Therefore, earnings from the longest-held job in 1950 is the best available measure of earnings. Earnings are topcoded at $100, which affects approximately 12% of observations in the sample. Following Callaway and Collins (2018), I assign a value of $125 to these observations.

recorded in the survey) to Census-based occupational income scores,[78] and subtracting the father's score from the son's.

The results indicate that union membership influenced fertility through both earnings and economic security channels. Adding weekly earnings and the various measures of job security decreases the magnitude of the coefficient on union status, such that the point estimate is no longer statistically significant at the 5% level. As expected, earnings, average job duration, and months spent in the labor force are all positively and significantly associated with fertility. Additionally, years spent living in the local area[79] is positively related to fertility, which is consistent with a protective effect of union membership against adverse labor market shocks that could force workers to migrate. Relative gains in occupational standing relative to one's father does not appear to be an important mechanism in this setting. I show in Appendix Table A4 that the results are noisier, but qualitatively similar, when analyzing effects on the extensive margin (i.e., using "any children" as the outcome).

9.2 County-Level Results

The analysis in Section 9.1 is motivated by a robust literature that demonstrates the effects of union membership on outcomes for individual workers. However, since county-level data on union membership in this era has not been available until now, it is not clear whether individual-level mechanisms persist at the level of local labor markets, net of possible spillover and equilibrium effects. For example, unions in this period occasionally conceded the right to collectively bargain over job protections in order to secure health benefits and wage increases.[80] To the extent that such compromises created winners and losers within local labor markets, the aggregate effect of growing union strength is *ex ante* ambiguous. Moreover, some candidate mechanisms can only be detected at the area-level, either because data is unavailable at the individual-level (e.g., access to healthcare) or because the mechanisms operate as within-market spillover effects (e.g., impacts on female labor force participation).

To shed light on the first order impacts of unionization on local economies, I re-estimate the county-level long-difference model from Section 6.1 using a set of alternative outcomes that plausibly connect unionization to fertility. In particular, I measure the effects of union membership rates on median household income, the income distribution, unemployment rates, female labor force participation, the supply of hospital beds, maternal and infant mortality rates, and home ownership.[81] I follow the baseline specification from Section 6.1 and include the full set of controls. I present the results for the main analysis sample of counties in Appendix Table A5.

There is a large, positive effect of unionization on median household incomes, which suggests that, at minimum, wage premia earned by organized workers are not compensated by losses among unorganized workers – and in fact may generate positive spillovers to non-union households. To measure impacts

[78] The IPUMS variable is *OCCSCORE*. Occupational earnings scores are based on occupational classifications and income data from the 1950 Decennial Census.

[79] Areas refer to "standard metropolitan areas as defined by the Bureau of the Census" (Palmer and Brainerd, 1954).

[80] The UMWA's Welfare and Retirement Fund is one prominent example of such a compromise: "Faced with rising competition from alternate fuels and nonunion coal, union president John L. Lewis agreed not to oppose mechanization in the mines in exchange for the ability to provide quality cradle-to-grave services for union members through the Fund... Mechanization resulted in layoffs and also reduced the number of new hires, thereby initiating an aging of the mining workforce" (Krajcinovic, 1997).

[81] I provide detailed descriptions of the sources and methods used to construct each outcome in Appendix G.

on the within-county distribution of income, I separately estimate the effects of unionization on the share of low- and middle-income households, conditional on the median household income. Low-income households are those in the bottom quintile of the national income distribution, while middle-income households are between the 20^{th} and 85^{th} percentiles of the national income distribution.[82] Consistent with the well-established finding that unionization played a central role in reducing income inequality during this period (Collins and Niemesh, 2019; Farber et al., 2021), I find that union membership rates reduced the share of low-income households and increased the share of middle-income households. In providing a ladder up to the middle class, unions may also have promoted homeownership.[83] The results in column 9 provide some support for positive impacts on homeownership, though the estimated effects are only significant at the 10% level.

The negative and statistically significant effect on local unemployment rates indicates that the impacts of collectively-bargained job protections were not wholly eroded by endogenous firm relocations or union compromises in the long run. However, union growth is also negatively and significantly associated with female labor force participation. This is consistent with a large historical literature that documents the labor movement's resistance to the inclusion of women within their ranks, and to the integration of women in the economy more broadly (e.g., see Milkman (2013)).[84]

The estimated effects on the supply of healthcare (as proxied by the bed-rated capacity of general hospitals) and maternal mortality have the expected signs but are not precise. There is a positive effect on infant mortality which, given the main fertility results, may be due to tighter birth spacing, greater average birth parity, and/or increased fertility in later childbearing years.

I test the degree to which each mechanism mediates the effect of unionization on birth rates in Table 10. Each column reports estimates from separate regressions of birth rates on union membership rates and the various candidate mechanisms. All regressions follow the parsimonious baseline specification from Section 6.1, which includes county, year, and state $\times$ year fixed effects. The union membership rate coefficient is most sensitive to the inclusion of median family income (column 2), the (conditional) share of low-income households (column 3), the unemployment rate (column 5), and the female share of the labor force (column 6). In each case, coefficients on mechanisms have the expected signs: income is positively related to birth rates, while point estimates for the conditional low-income share, unemployment rates, and female shares of the labor force are negative. Once all mechanisms enter in a "horserace" regression (column 11), the union membership rate coefficient is small and not statistically different from zero, signalling that the channels through which unionization influences fertility are well-captured by this set

[82] These are approximations. See Appendix G.

[83] Labor unions have a long history of involvement in housing policy and advocacy. As early as the 1920s, several unions established banks and credit unions to offer low-interest mortgages to members. Notably, Detroit and Flint, Michigan – the two major hubs of UAW representation – had the highest proportion of owner-occupied homes of any major American city in the mid-20^{th} century (Barnard and Handlin, 1983).

[84] Many unions sought to serve their majority-male constituents by excluding female members, who they feared would depress wages and undermine bargaining power. Moreover, though female labor force participation surged during World War II, women were often the first to be laid off after 1945, as union seniority rules favored male workers who sought to reclaim their jobs after returning home. The end of the war also precipitated the return of written agreements that re-codified discriminatory practices in union shops: "Some contracts forbade the hiring of married women and required the resignation of single female employees who married. Other agreements provided that if a married women could show cause for employment – for example, if her husband was incapacitated or in the service – she might be allowed to work, but only under certain onerous constraints. ...[UAW] Local 391 had a particularly insulting way of insuring a married woman's gratitude; she had to pay the local one dollar per week for permission to work." (Gabin, 2013)

of candidate mechanisms. Among the mechanism variables, the income distribution measures absorb a relatively large share of the variation.

Overall, the county-level results suggest that the labor movement shaped fertility not only by securing economic gains for its majority-male membership, but also by impacting opportunity costs for women in local marriage and labor markets. In reducing wage inequality, union growth may have diminished the option value associated with marital search (Loughran, 2002), leading to higher marriage rates and, subsequently, to higher fertility. At the same time, organized labor's efforts to resist gender integration likely reduced the opportunity costs of childbearing for women. Indeed, previous work finds that the exit of young women from the labor force after WWII was an important proximate cause of the Baby Boom (Doepke et al., 2015). To the extent that unions played an active role in driving these dynamics, unionization can therefore be viewed as a complement to, rather than a substitute for, such explanations.

10 Summary and Discussion

This paper provides the first evidence that the rise of the labor movement following the enactment of the 1935 National Labor Relations Act (NLRA) contributed to fertility increases during the American Baby Boom. Drawing on novel estimates of union membership at the county-level, I find that areas with greater exposure to the NLRA shock experienced larger increases in marriage and birth rates and, ultimately, in completed fertility. Unionization influenced fertility through two main channels. First, labor unions secured a range of economic gains for their majority-male membership through collective bargaining – including higher wages, stronger job protections, and more generous non-wage benefits – which increased household resources and provided insurance against labor market risks. Second, the growing influence of unions, especially in the industrial sector, led to declining female labor force participation after World War II, and thus lowered the opportunity cost of childbearing for young women.

These results cast the Baby Boom in a new light. Conventional explanations of the Baby Boom link technological progress and postwar economic growth to fertility increases. In practice, the unusual magnitude and duration of the U.S. Baby Boom was likely due to the confluence of multiple, mutually-reinforcing factors. For example, economic growth is typically not sufficient for producing baby booms on a historic scale; however, in securing a larger share of the gains from growth for workers and families, collective bargaining provided a novel technology for translating economic expansion into broad-based prosperity, which likely amplified effects on fertility. In centering the far-reaching impacts of the NLRA, this work is also consistent with institutional perspectives that emphasize how the design of economic policies and institutions shaped demographic changes during the Baby Boom.

Beyond the Baby Boom, this work offers a shift in perspective from conventional economic treatments of labor unions, which tend to consider only the most proximate impacts of unionization on firms and workers; for example, effects on wages, employment, and productivity. This focus fails to provide a full accounting of the ways in which organized labor transformed virtually all aspects of workers' lives in the mid-20$^{\text{th}}$ century. A basic insight of this paper is that in setting wages and defining conditions of employment, labor market institutions – including labor unions – cannot be thought of as separate from household decisions about marriage and family formation.

These findings have important implications for policies that seek to address contemporary demographic challenges. Birth rates have fallen precipitously in the U.S. and other developed countries since the Baby Boom, and at least some of this decline is attributable to a rise in economic precariousness among young workers (Sobotka et al., 2011).[85] Pro-natalist policies that aim to ease economic constraints of childbearing (e.g., child tax credits) are expensive and have proven to be largely ineffective in boosting long-run fertility (Brainerd, 2014; Lopoo et al., 2018; Sobotka et al., 2019). My results suggest that such re-distributive policies do not succeed in part because they address some symptoms, but not the root causes, of precarity in the labor market. A more promising approach may be to take steps to strengthen labor market institutions that determine pre-distributive outcomes. An important caveat is that the historic effects of unionization on fertility were driven not only by effects on economic security but also by discriminatory practices that depressed female labor force participation. A challenge for contemporary policymakers will therefore be to adapt the desirable features of strong labor unions from the Baby Boom era to new realities in the labor market.

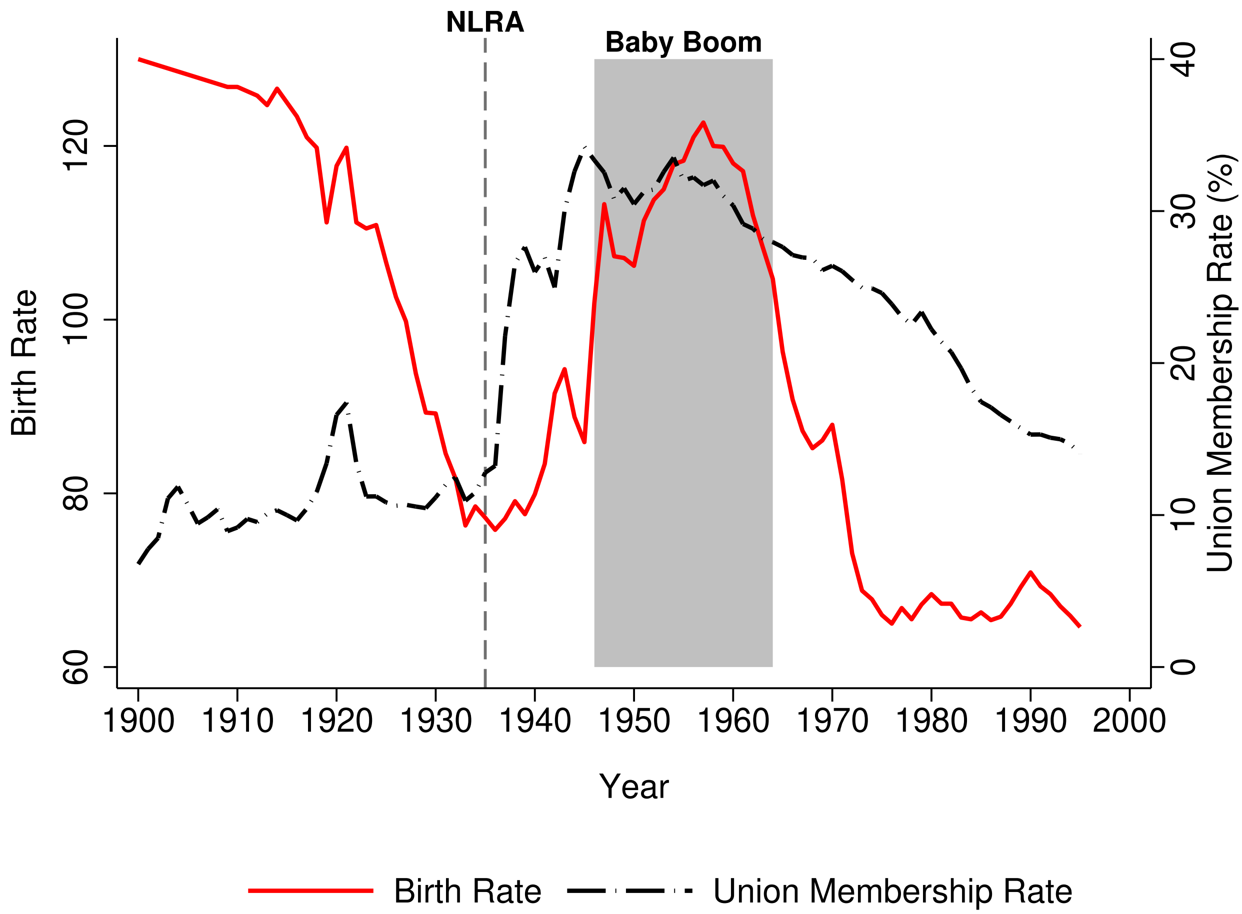
More broadly, my results suggest that changes in labor market institutions can act as place-based shocks, with far-reaching impacts on regional growth and decline. I show that fertility increases during the Baby Boom were not uniform across areas, but were in fact highly localized and tied to underlying industrial structures. Moreover, many of the same areas that experienced high growth in unionization during the Baby Boom also experienced the negative effects of deindustrialization in the latter part of the 20$^{\text{th}}$ century (Alder et al., 2023). Many such places, including former coal mining areas in Appalachia and industrial cities in the "Rust Belt", now face an array of social and economic difficulties related to sustaining an aging population and workforce with a diminished tax base. Contemporary efforts to implement industrial policies and other labor market reforms should therefore consider both short- and long-run equilibrium effects for local economies.

This paper has several limitations that may be addressed in future research. First, I limit the scope of the main analysis to the Baby Boom. Existing work suggests that much of the decline in fertility after 1960 is attributable to technological and cultural changes, including the introduction of "the pill" (Bailey, 2006, 2010). However, the time series in Figure 1 suggest that union density and fertility exhibited similar boom-bust dynamics throughout the 20$^{\text{th}}$ century, so the erosion of union strength may have also played a role in the "Baby Bust". Second, this analysis focuses on developments in the United States. The labor movement was, however, an international movement, and Appendix Figure A9 shows that many other Western countries that experienced Baby Booms also experienced increases in unionization around the same time. Cross-country comparisons would allow researchers to explore the generalizability of these results to other settings. Finally, the role of labor unions in the U.S. economy has changed considerably since the Baby Boom. However, nearly a century after the National Labor Relations Act touched off the first major wave of unionization in the U.S., the economic dislocations wrought by the COVID-19 pandemic have contributed to a new surge in interest and support for labor unions.[86] An important unresolved question posed by this work is whether the historical connection between unionization and fertility persists today.

[85] While some of the recent fertility decline may be due to changing preferences and cultural norms, survey evidence suggests that the number of children desired by American families has remained stable since the 1970s (Saad, 2018).

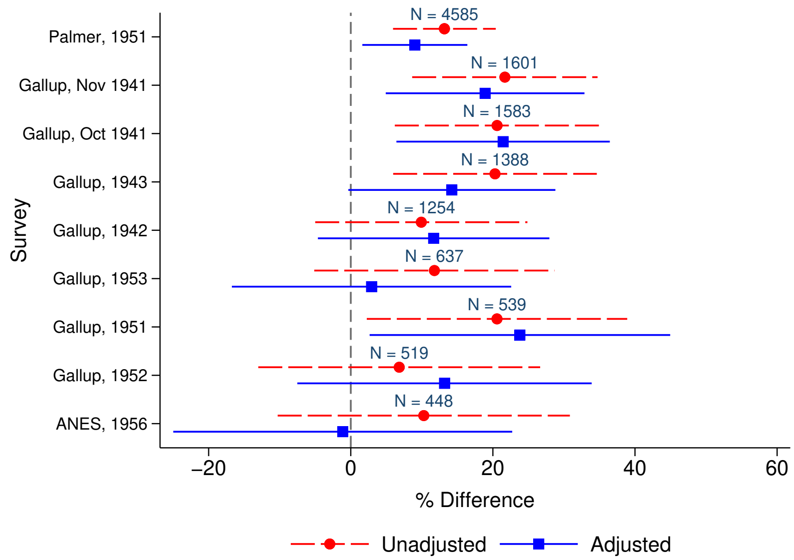
[86] In 2022, Gallup estimated that 71% of Americans approved of labor unions, the highest approval rating since 1965 (McCarthy, 2022). Unions also won NLRB elections at a 76.6% rate in 2022, the highest win rate in the 21$^{\text{st}}$ century (Molla, 2022). In 2023, high profile strikes by the UAW and SAG-AFTRA and organizing campaigns among Stabucks and Amazon workers brought additional attention to the labor movement.

Figure 1. Birth Rates and Unionization in the United States during the 20th Century



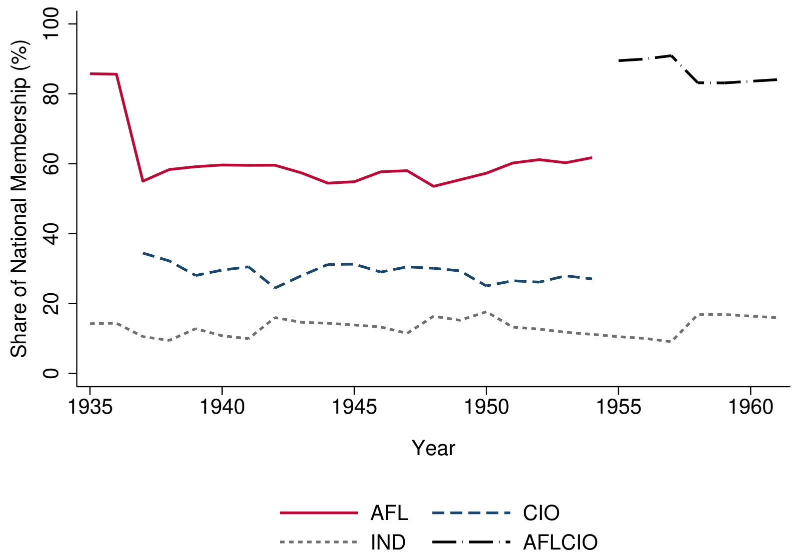
Notes: Birth rates, from Bailey et al. (2016), are defined to be live births per 1,000 women aged 15-44 years. The union membership rate, from Freeman (1997)'s Appendix A, is the number of total union members divided by U.S. non-farm employment. The shaded area corresponds to the Baby Boom, per the U.S. Census's definition (1946-1964). The NLRA was enacted in 1935.

Figure 2. Cross-Sectional Estimates of the Effect of Unionization on Family Size



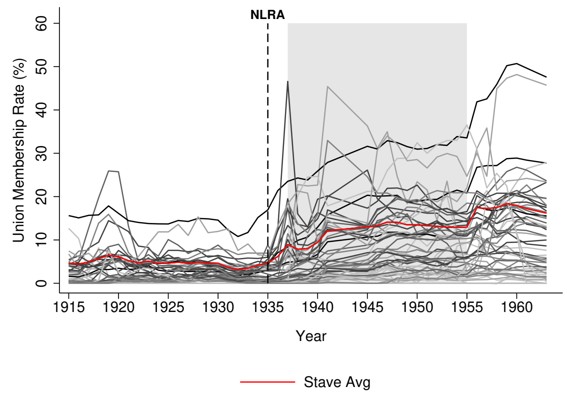
Notes: Results are for a sample of household heads between the aged 25-54. The Palmer Survey further restricts to men in the labor force in 5 Northern cities. In all cases, the independent variable is a dummy for whether the head of household is a member of a labor union. The dependent variable is a proxy for the number children in the household, which varies across surveys. The unadjusted estimate is equal to the simple difference in mean outcomes between union and non-union HHs, scaled by the non-union mean. The adjusted estimate is equal to the coefficient of a regression of the outcome on union membership and a set of controls, scaled by the non-union mean. The following baseline controls enter in all specifications: a dummy for region = South, race, age, a quadratic in age, urban/rural residence, occupation, sex, state fixed effects and a vector of dummy variables which indicate missing values for each of the above. Surveys are listed in descending order by sample size. I derive standard errors using a non-linear Wald test. In all cases whiskers depict 95% confidence intervals.

Figure 3. Share of National Union Membership, by Federation

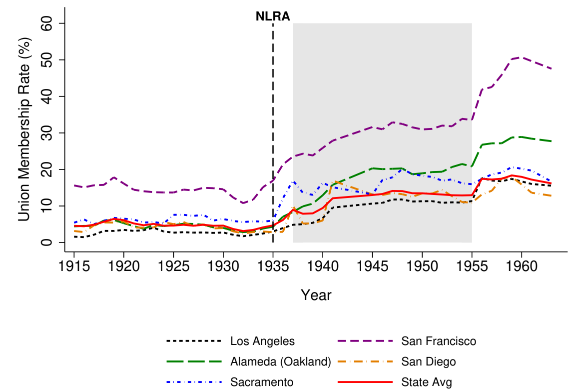


Notes: Data are from Troy (1965). The CIO was formed in 1937. The AFL and CIO merged at the national level to form the AFL-CIO in 1955.

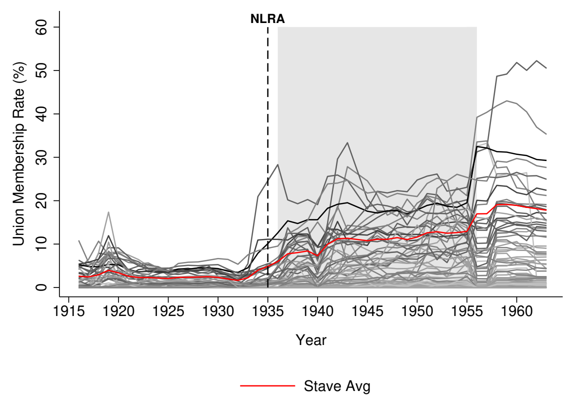
Figure 4. County-Level Union Membership Rates
AFL and AFL-CIO Members



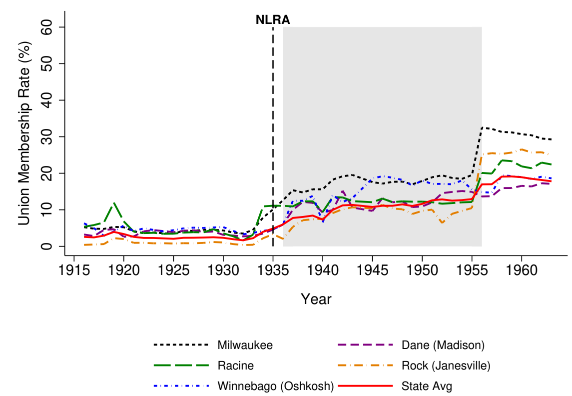
(a) California, All Counties



(b) California, Top 5 Counties



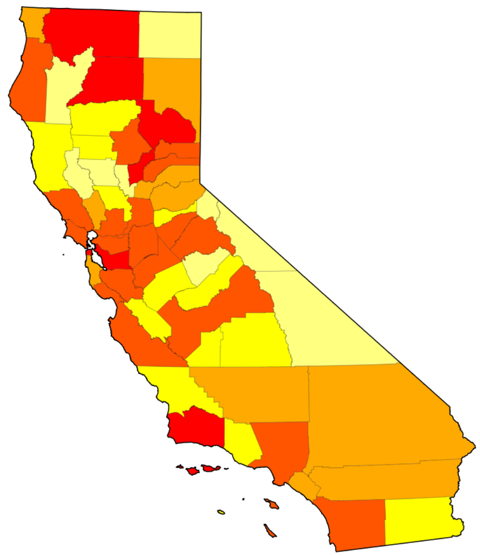
(c) Wisconsin, All Counties



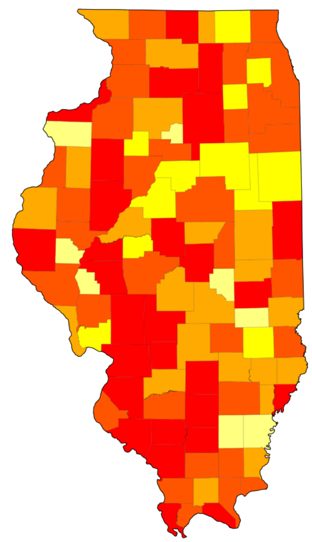
(d) Wisconsin, Top 5 Counties

Notes: Union membership rates are equal to total members in unions affiliated with the AFL or AFL-CIO, divided by employment. Grey-shaded areas correspond to years for which the CIO existed but CIO membership is not observed (1937-1955 in California, 1936-1956 in Wisconsin). In panels (a) and (c), county-level series are shaded according to 1930 employment levels (darker = greater employment). In panels (b) and (d), counties are ranked in the top 5 based on 1930 employment levels.

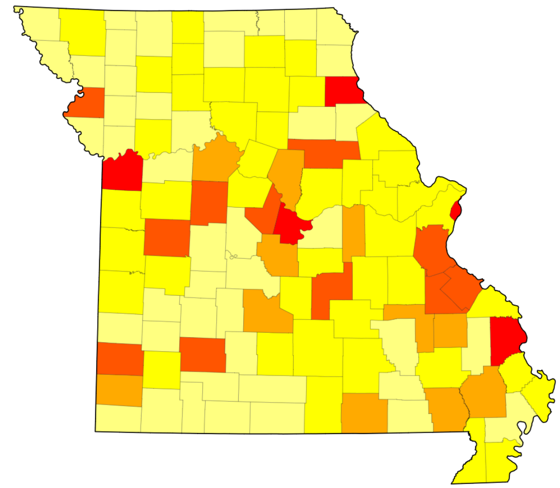
Figure 5. Changes in Union Membership Rates, 1934-1960



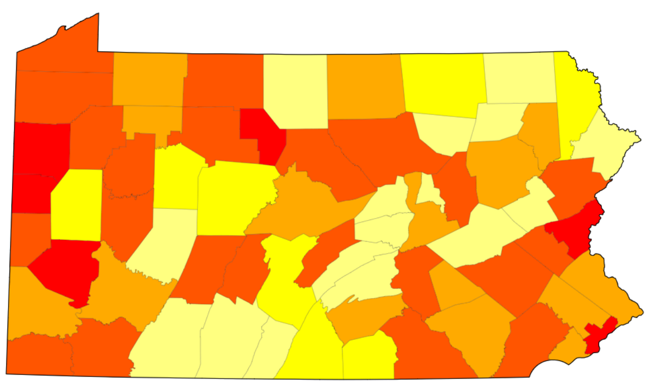
(a) California



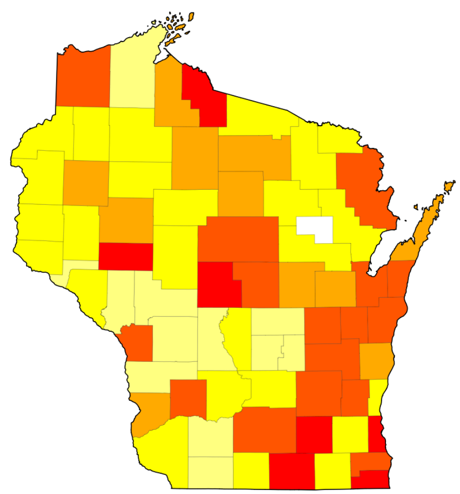
(b) Illinois



(c) Missouri

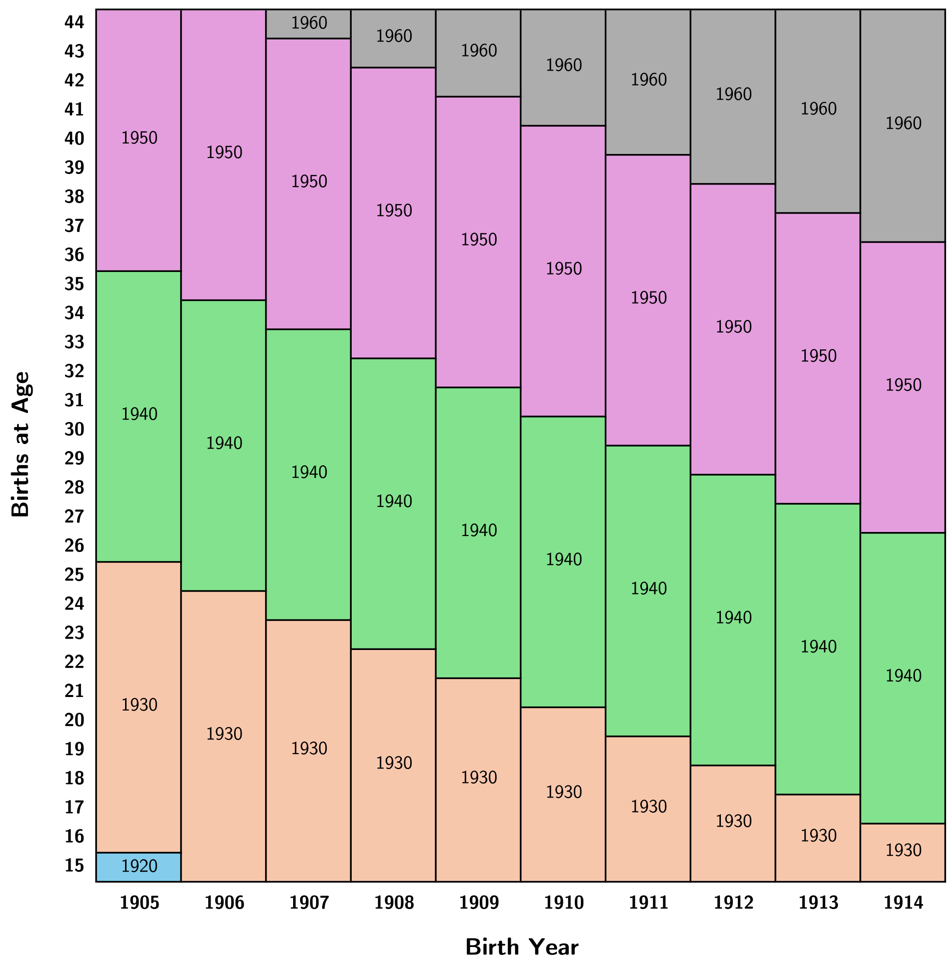


(d) Pennsylvania



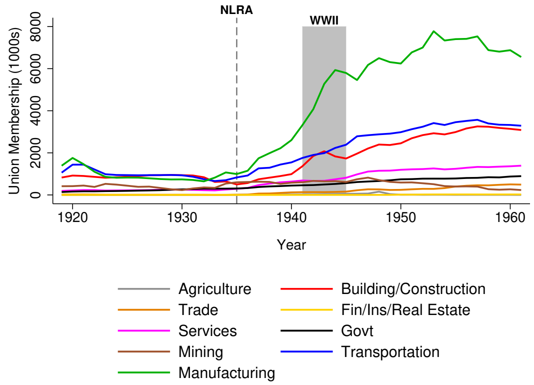
(e) Wisconsin

Figure 6. Example of TCFR Construction:
Cohorts Born 1905-1914

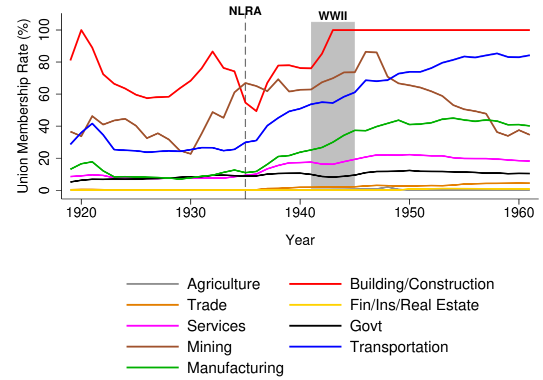


Notes: Years within the body of the diagram refer to the Decennial Census in which births since last Census are measured at the specified age for each birth year cohort.

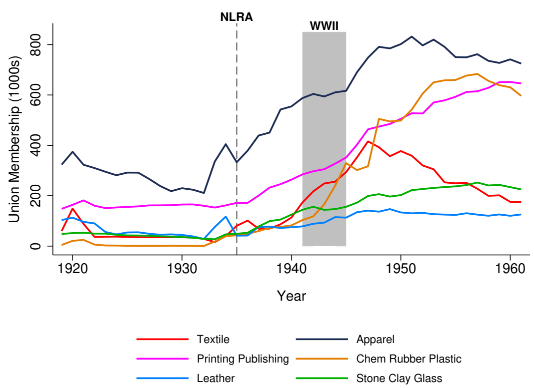
Figure 7. Union Membership by Industry and Sub-Industry



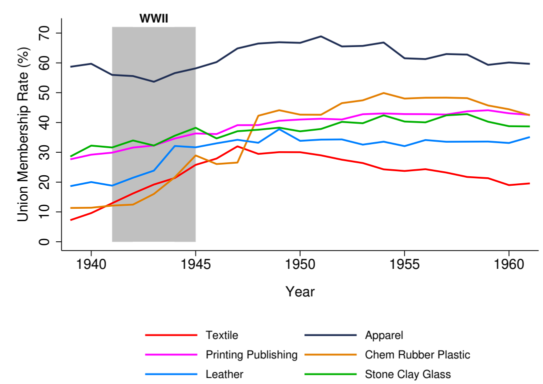
(a) Membership, by Major Industry Group



(b) Membership Rate, by Major Industry Group



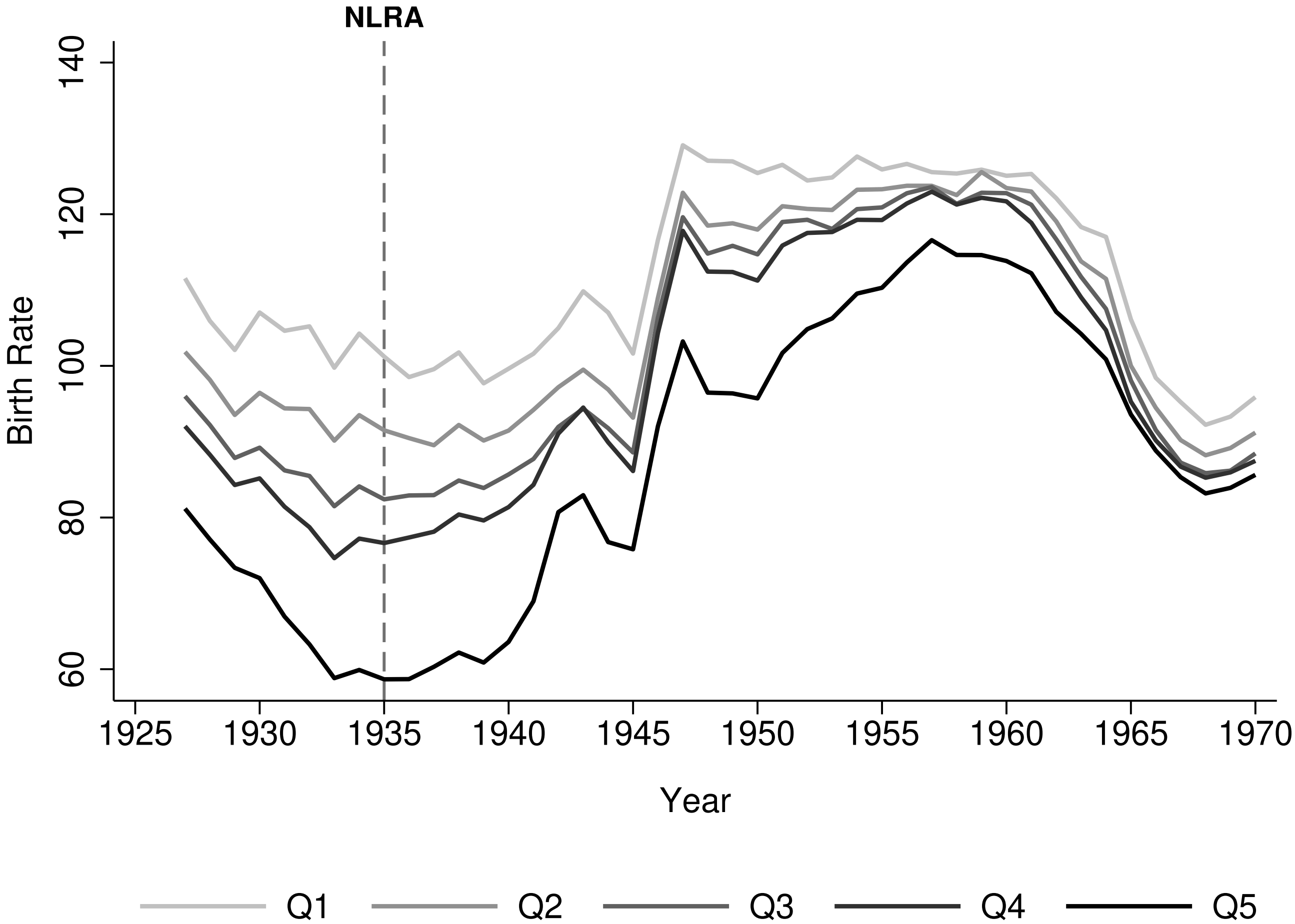
(c) Membership, by Sub Industry Group



(d) Membership Rate, by Sub Industry Group

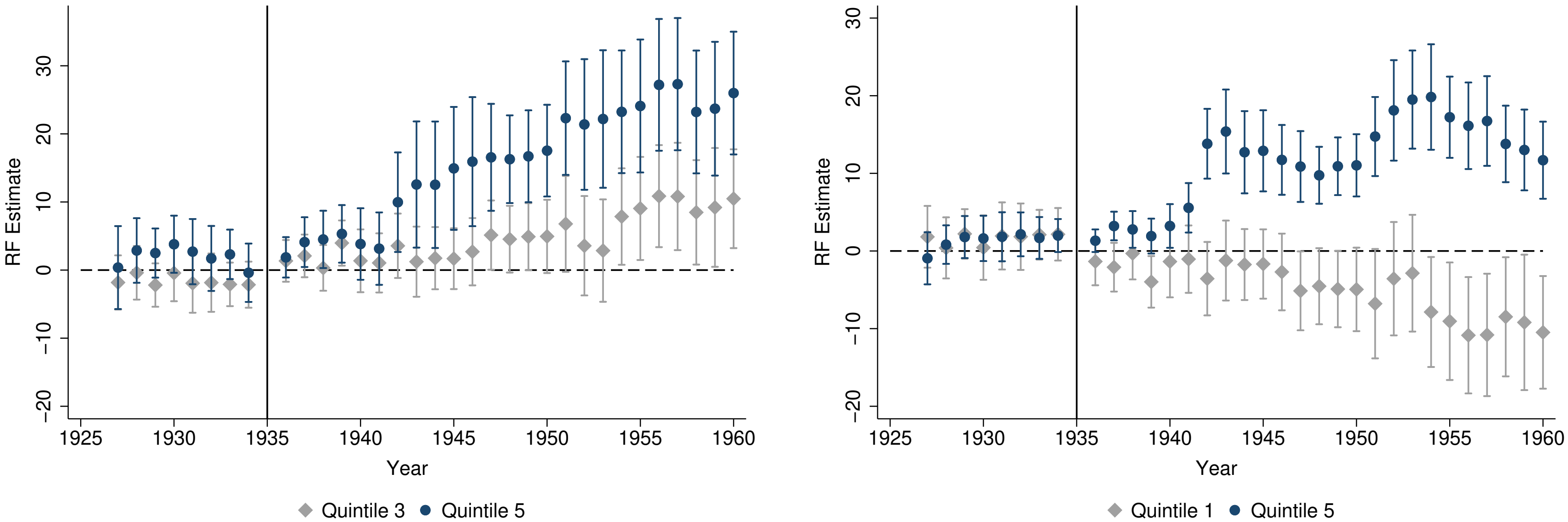
Notes: Union membership by industry is available from Wolman (1936) before 1935, and from Troy (1965) from 1935 onward. In each year, the denominator for the union membership rate includes all employed workers aged 14 and older. Panels (c) and (d) present data for a subset of manufacturing industries for which industry-level employment is available beginning in 1939. In panel (d), I cannot construct denominators prior to 1939, so rates are plotted from 1939 onward. For mappings of industry groups to IPUMS and SIC codes, see Table 11. For details on the construction of industry-level union membership and union membership rates, see Appendix D.

Figure 8. Birth Rates: Means by Year and Treatment Quintile



Notes: The sample includes all counties in states that were part of the U.S. Birth Registration Area in 1927 (excludes CO, GA, NV, NM, OK, SC, SD, and TX). I bin counties in quintiles according to SSIV-predicted changes in union membership rates from 1934-1960, where Q1 = the lowest treatment group and Q5 = the highest treatment group. Group-level averages are weighted by the female population in each year.

Figure 9. Birth Rates: Reduced-Form Event Study

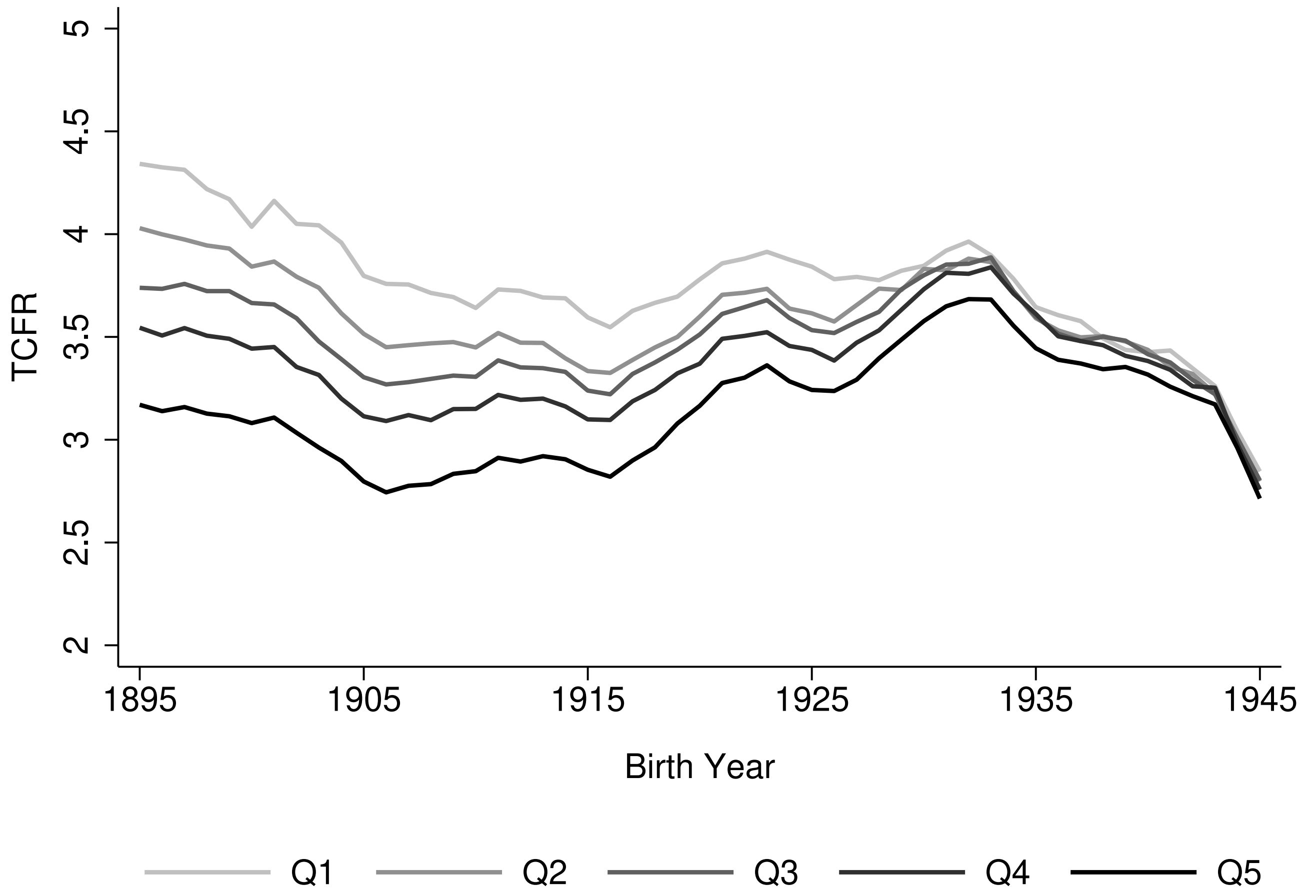


(a) Ref. Group = Q1

(b) Ref. Grop = Q3

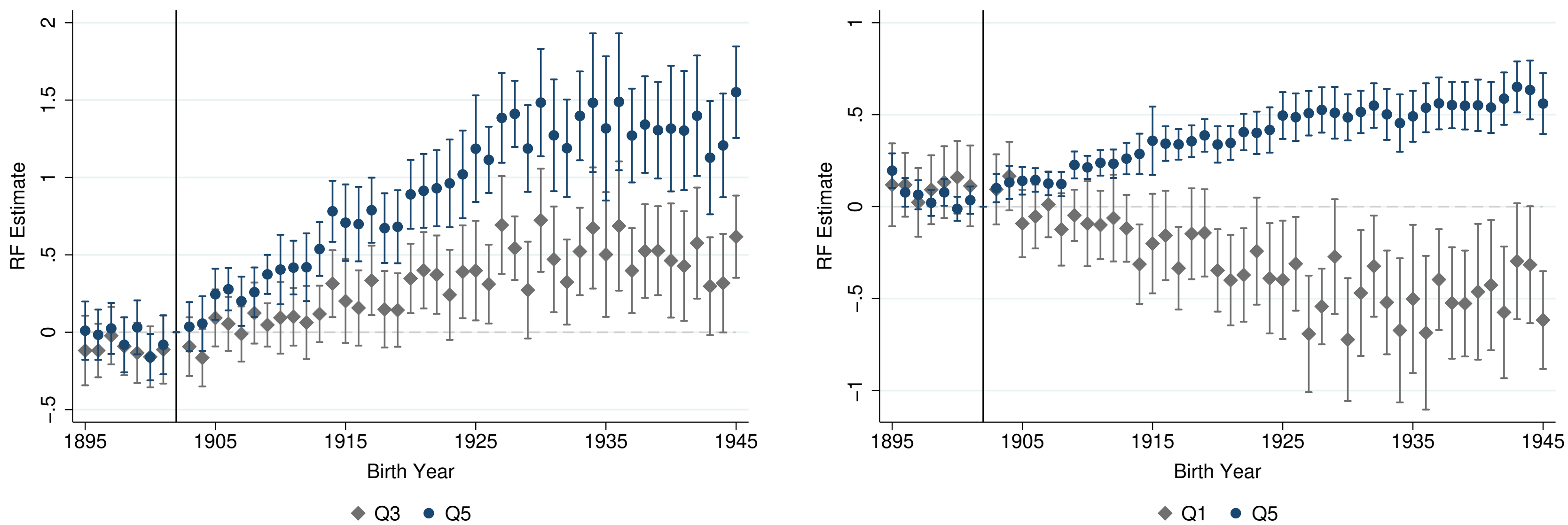
Notes: I plot point estimates and 95% confidence intervals from the reduced-form event study model with birth rates as the outcome (see Section 6.2). The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). I weight by the female population in each county-year cell. I bin counties in quintiles according to SSIV-predicted changes in union membership rates from 1934-1960, where Q1 = the first (i.e., lowest) quintile by predicted treatment and Q5 = the fifth (i.e., highest) quintile. Birth rates are by place of occurrence prior to 1935, and by place of residence from 1935 onward. The specification in Panel A uses Q1 as the reference group, and plots estimates for Q3 and Q5; the specification in Panel B uses Q3 as the reference group, and plots estimates for Q1 and Q5. In each case, regressions include fixed effects and baseline controls, and the base year is 1935.

Figure 10. TCFRs: Means by Birth Year and Treatment Quintile



Notes: The sample includes all counties in the 48 contiguous states. I bin counties in quintiles according to SSIV-predicted changes in union exposure for cohorts born 1901-1937, where Q1 = the lowest treatment group and Q5 = the highest treatment group. Group-level averages are weighted by the female population in each birth year.

Figure 11. TCFRs: Reduced-Form Event Study

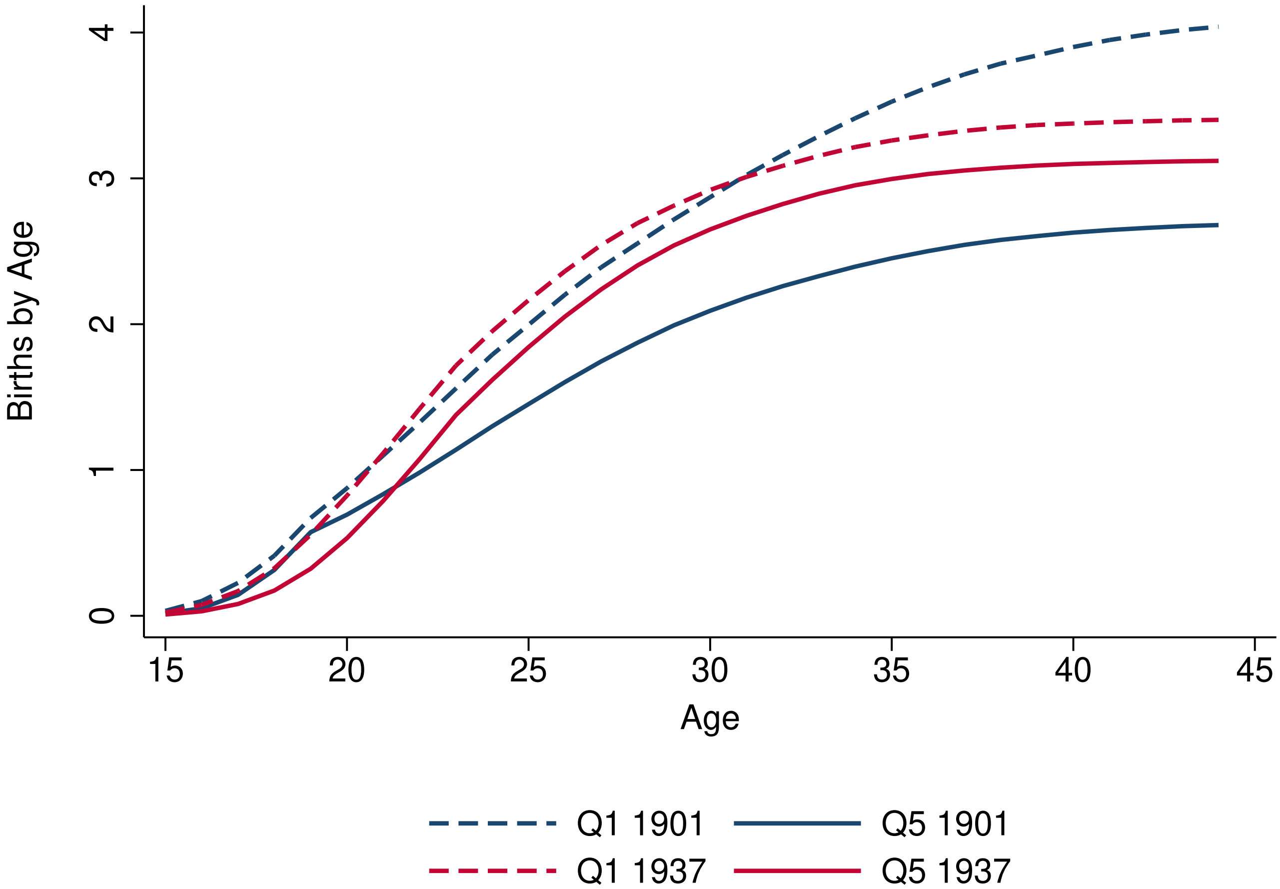


(a) Ref. Group = Q1

(b) Ref. Grop = Q3

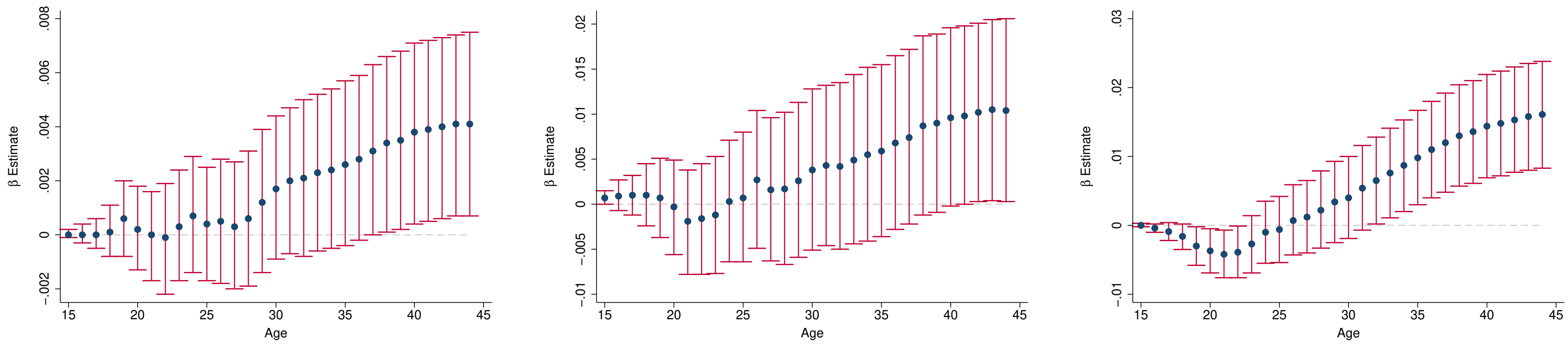
Notes: I plot point estimates and 95% confidence intervals from the reduced-form event study model with TCFRs as the outcome (see Section 7.2). The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). I weight by the female population in each county-birth year cell. I bin counties in quintiles according to SSIV-predicted changes in union exposure for the 1901-1937 cohorts, where Q1 = the first (i.e., lowest) quintile by predicted treatment, and Q5 = the fifth (i.e., highest) quintile by predicted treatment. The specification in Panel A uses Q1 as the reference group, and plots estimates for Q3 and Q5; the specification in Panel B uses Q3 as the reference group, and plots estimates for Q1 and Q5. In each case, regressions include county, year, and state × year fixed effects. The base birth year is 1902.

Figure 12. Births by Age: Means by Treatment Quintile
Cohorts Born 1901 and 1937



Notes: The sample includes all counties in the 48 contiguous states. I bin counties in quintiles according to SSIV-predicted changes in union exposure for cohorts born 1901-1937, where Q1 = the lowest treatment group and Q5 = the highest treatment group. Group-level averages are weighted by the female population in each birth year.

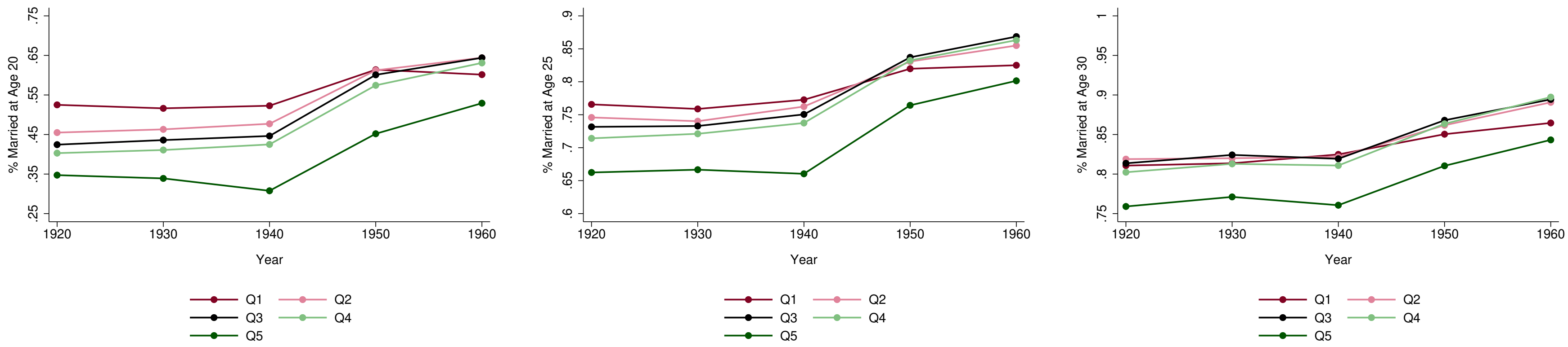
Figure 13. Births by Age LD: Main Effects



(a) OLS **(b)** IV/2SLS **(c)** Reduced-Form

Notes: Each set of estimates (plotted points correspond to point estimates, and whiskers correspond to 95% CIs) reports results from separate long-difference regressions of cumulative births at the specified age on union exposure. The long-difference measures changes in outcomes that result from changes in union exposure between the cohorts born in 1901 (pre-NLRA) and 1937 (post-NLRA). Panel A reports OLS estimates, panel B reports IV/2SLS estimates using the aggregate shift-share IV as an instrument for union exposure, and panel C reports reduced-form estimates using the aggregate shift-share IV. In panels A and B, the sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI); in panel C, the sample includes all counties in the contiguous 48 states. All regressions include include county, birth year, and state $\times$ birth year fixed effects, as well as the following time-varying measures: log(population), the share of the labor force in manufacturing, pct white, pct male, pct urban, pct aged 15-44, pct foreign-born, the change in retail sales from 1929-1933, New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, WWII casualty rates per capita, and pct of households with a modern washing machine. I weight by female population in each county-birth year cell. I estimate HAC-corrected standard errors based on clusters defined by 100km radii around county centroids (Colella et al., 2019).

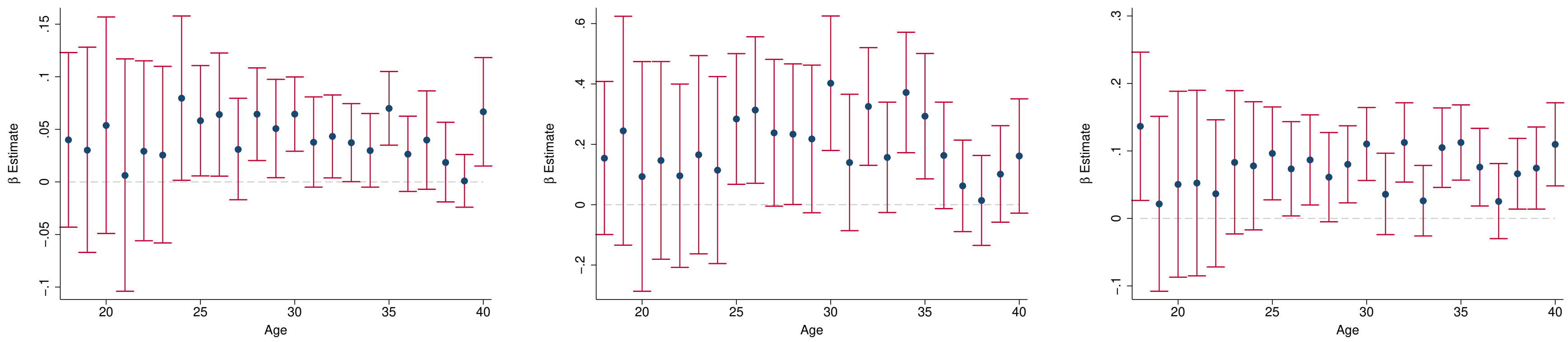
Figure 14. Age-Specific Marriage Rates: Means by Year and Treatment Quintile



(a) % Married by Age 20 **(b)** % Married by Age 25 **(c)** % Married by Age 30

Notes: The sample includes all counties in the 48 contiguous states. I bin counties in quintiles according to SSIV-predicted changes in union membership rates between 1934 and 1960, where Q1 = the lowest treatment group and Q5 = the highest treatment group. I construct age-specific marriage rates as described in Section 4.4. Group-level averages are weighted by the female population in each year.

Figure 15. Age-Specific Marriage Rates LD: Main Effects



(a) OLS

(b) IV/2SLS

(c) Reduced-Form

Notes: Each set of estimates (plotted points correspond to point estimates, and whiskers correspond to 95% CIs) reports results from separate long-difference regressions of marriage rates at the specified age on union membership rates. The long-difference measures changes in outcomes, measured in 1930 and 1960, that result from changes in union membership rates between 1934 (pre-NLRA) and 1960 (post-NLRA). Panel A reports OLS estimates, panel B reports IV/2SLS estimates using the aggregate shift-share IV as an instrument for union membership rates, and panel C reports reduced-form estimates using the aggregate shift-share IV. In panels A and B, the sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI); in panel C, the sample includes all counties in the contiguous 48 states. All regressions include include county, year, and state $\times$ year fixed effects, as well as the following time-varying measures: log(population), the share of the labor force in manufacturing, pct white, pct male, pct urban, pct aged 15-44, pct foreign-born, the change in retail sales from 1929-1933, New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, WWII casualty rates per capita, and pct of households with a modern washing machine. I weight by female population in each county-year cell. I estimate HAC-corrected standard errors based on clusters defined by 100km radii around county centroids (Colella et al., 2019).

Table 1. Union Data Sources

Year	California AFL	California CIO	California AFLCIO	Illinois AFL	Illinois CIO	Illinois AFLCIO	Missouri AFL	Missouri CIO	Missouri AFLCIO	Pennsylvania AFL	Pennsylvania CIO	Pennsylvania AFLCIO	Wisconsin AFL	Wisconsin CIO	Wisconsin AFLCIO	Independent UE	Independent UMWA	Independent IBT
1920	1921^{r}			1921^{v}			1921^{r}			1921^{v}			1921^{r}				1921^{v}	AFL
1921	1922^{r}			1922^{v}			1922^{r}			1922^{v}			1922^{r}				Int	AFL
1922	1923^{r}			1923^{v}			1923^{r}			Int			1923^{r}				Int	AFL
1923	1924^{r}			1924^{r}			1924^{r}			1924^{m}			1924^{r}				1924^{v}	AFL
1924	1925^{r}			1925^{r}			1925^{r}			1925^{m}			1925^{r}				Int	AFL
1925	1926^{r}			1926^{r}			1926^{r}			1926^{m}			1926^{r}				Int	AFL
1932	1933^{r}			1933^{r}			1933^{r}			1933^{v}			1933^{r}				Int	AFL
1933	1934^{r}			1934^{r}			1934^{r}			1934^{v}			1934^{r}				1934^{v}	AFL
1934	1935^{r}			1935^{r}			1935^{r}			1935^{v}			1935^{r}				Int	AFL
1956	1957^{r}	1957^{r}		1957^{r}	Int				1957^{m}	Int	1957^{r}				1958^{m}	1957^{v}	Int	1957^{v}
1957	1958^{r}	1958^{r}		1958^{r}	Int				1958^{m}	Int	Int				1958^{m}	1958^{v}	Int	Int
1958			1959^{r}	1959^{r}	Int				1959^{m}	Int	1959^{r}				1960^{r}	1959^{v}	Int	Int
1959			1960^{r}			1960^{r}			1960^{m}	Int	1960^{r}				1960^{r}	1960^{v}	Int	Int
1960			Int			1961^{r}			Int			1961^{r}			1962^{r}	1961^{v}	Int	1961^{v}
1961			1962^{r}			1962^{r}			1962^{m}			1962^{r}			1962^{r}	1962^{v}	Int	Int

Notes: Years in the body of the table correspond to the year the specified convention was held. The CIO was formed in November 1935, and the first state-level CIO conventions took place in 1936. The AFL and CIO merged at the national level in 1955 to form the AFL-CIO, but the timing of state convention mergers varied. The United Electrical, Radio and Machine Workers of America (UE) union was founded in 1936 and became independent in 1949 after disaffiliating from the CIO. The United Mine Workers of America (UMWA) union was founded in 1890 and was affiliated at various points with the AFL and with the CIO until 1948, when it became the largest independent union in the U.S. The UMWA re-joined the merged AFL-CIO in 1989. The International Brotherhood of Teamsters (IBT) was affiliated with the AFL (and with its successor organization, the AFL-CIO) since its inception in 1903, but was expelled in December 1957 and functioned independently until 1985 when it re-affiliated with the AFL-CIO.

CA AFLCIO 1960 linearly interpolated using CA AFLCIO 1960 and 1962. IL CIO 1956-1958 linearly interpolated using IL CIO 1956 and 1960. MO AFLCIO 1960 linearly interpolated using MO AFLCIO 1960 and 1962. PA AFL 1922 linearly interpolated using PA AFL 1922 and 1924. PA AFL 1956-1959 linearly interpolated using PA AFL 1955 and 1961. PA CIO 1957 linearly interpolated using PA CIO 1957 and 1959. WI AFLCIO proceedings in 1958, 1960 and 1962 provide data covering the two preceding fiscal years. UMWA 1921-1922 linearly interpolated using UMWA 1921 and 1924. UMWA 1924-1925 linearly interpolated using UMWA 1924 and 1927. UMWA 1932 linearly interpolated using UMWA 1932 and 1934. UMWA 1934 linearly interpolated using UMWA 1934 and 1936. UMWA 1956-1961 linearly interpolated using UMWA 1956 and 1964. IBT 1957-1959 linearly interpolated using IBT 1957 and 1961. IBT 1961 linearly interpolated using IBT 1961 and 1966.

^{m} = membership directly observed
^{r} = per capita receipt-based membership estimate
^{v} = vote-based membership estimate

Table 2. Birth Rate LD: First-Stage
County-Level

	Union Membership Rate		
	(1)	(2)	(3)
Pred. Union Membership Rate	0.878***	0.768***	0.724***
	(0.180)	(0.164)	(0.161)
	[0.000]	[0.000]	[0.000]
F	23.70	21.91	20.39
Fixed Effects?	X	X	X
Baseline Controls?		X	X
Post Period Controls?			X
Base Dep. Var. Mean	4.42	4.42	4.42
N	826	826	826
N (counties)	413	413	413

Notes: Each column reports estimates from separate regressions of union membership rates on SSIV-predicted union membership rates. The long-difference compares outcomes in 1934 (pre-NLRA) and 1960 (post-NLRA). The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). "Fixed Effects" include county, year, and state $\times$ year fixed effects. "Baseline Controls" include: log(population), pct white, pct male, and pct foreign-born, measured in the 1930 Census and interacted with time fixed effects; the change in retail sales from 1929-1933, interacted with time fixed effects; and the change in birth rates from 1929-1933, interacted with time fixed effects. "Post Period Controls" include: New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, and WWII casualty rates per capita. I weight by female population in each county-year cell. Standard errors, clustered at the county level, are in parentheses, p-values are in brackets. I report the Kleibergen-Paap rk Wald (first-stage) F statistic.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table 3. Birth Rate LD: Main Effects
County-Level

	Dependent variable: Birth Rate		
	(1)	(2)	(3)
Panel A: OLS			
Union Membership Rate	0.568***	0.132*	0.109*
	(0.117)	(0.069)	(0.062)
	[0.000]	[0.058]	[0.081]
Panel B: Reduced Form			
Pred. Union Membership Rate	1.499***	0.666***	0.704***
	(0.137)	(0.147)	(0.127)
	[0.000]	[0.000]	[0.000]
Panel C: IV/2SLS			
Union Membership Rate	1.708***	0.866***	0.972***
	(0.357)	(0.271)	(0.302)
	[0.000]	[0.002]	[0.001]
F	23.70	21.91	20.39
Fixed Effects?	X	X	X
Baseline Controls?		X	X
Post Period Controls?			X
Base Dep. Var. Mean	62.51	62.51	62.51
N	826	826	826
N (counties)	413	413	413

Notes: Each column reports estimates from separate regressions of birth rates by residence on the specified independent variable. The long-difference measures changes in outcomes that result from changes in union membership rates between 1934 (pre-NLRA) and 1960 (post-NLRA). I impose a 2-year lag relationship between treatment and outcomes. The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). "Fixed Effects" include county, year, and state × year fixed effects. "Baseline Controls" include: log(population), pct white, pct male, and pct foreign-born, measured in the 1930 Census and interacted with time fixed effects; the change in retail sales from 1929-1933, interacted with time fixed effects; and the change in birth rates from 1929-1933, interacted with time fixed effects. "Post Period Controls" include: New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, and WWII casualty rates per capita. I weight by female population in each county-year cell. Standard errors, clustered at the county level, are in parentheses, p-values are in brackets. I report the Kleibergen-Paap rk Wald (first-stage) F statistic.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table 4. Birth Rate LD: Reduced-Form Estimates
County-Level

	Dependent variable: Birth Rate			
	(1)	(2)	(3)	(4)
	Main Sample		National Sample	
Pred. Union Membership Rate	1.499***	0.666***	1.316***	1.028***
	(0.137)	(0.147)	(0.081)	(0.099)
	[0.000]	[0.000]	[0.000]	[0.000]
Fixed Effects?	X	X	X	X
Baseline Controls?		X		X
Base Dep. Var. Mean	62.51	62.51	69.02	69.02
N	826	826	4504	4504
N (counties)	413	413	2252	2252

Notes: Each column corresponds to a different specification of Equation 4. The outcome is the birth rate. The main sample includes counties in the five states for which county-level union membership data is available: CA, IL, MO, PA, and WI; the national sample includes all counties in the 48 contiguous states. "Fixed Effects" include county, year, and state × year fixed effects. "Baseline Controls" include: log(population), pct white, pct male, and pct foreign-born, measured in the 1930 Census and interacted with time fixed effects; the change in retail sales from 1929-1933, interacted with time fixed effects; and the change in birth rates from 1929-1933, interacted with time fixed effects. I weight by female population in each county-year cell. Standard errors, clustered at the county level, are in parentheses, p-values are in brackets.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table 5. Birth Rate LD: OLS Estimates
State-Level

	Dependent variable: Birth Rate	
	(1)	(2)
	Farber et al. (2021)	Troy and Sheflin (1985)
Panel A: OLS		
Union Membership Rate	0.271**	0.521**
	(0.118)	(0.235)
	[0.026]	[0.032]
Panel B: Reduced Form		
Pred. Union Membership Rate	1.037***	0.845**
	(0.310)	(0.333)
	[0.002]	[0.015]
Panel C: IV/2SLS		
Union Membership Rate	0.800***	1.027**
	(0.276)	(0.458)
	[0.006]	[0.030]
F	17.41	11.64
Fixed Effects?	X	X
Base Dep. Var. Mean	71.04	77.65
N	90	94
N (states)	45	47

Notes: Each column corresponds to a different specification of Equation 1. The outcome is the birth rate. The union sample includes the five states for which county-level union membership data is available: CA, IL, MO, PA, and WI. In column 1, I use union membership rates from Farber et al. (2021), and the long-difference compares changes in outcomes resulting from changes in union membership rates measured in 1937-1960. In column 2, I use union membership rates from Troy and Sheflin (1985), and the long-difference compares changes in outcomes resulting from changes in union membership rates measured in 1939-1960. I drop DE, ID, and NV from the sample in column 1 due to small cell sizes in the Farber et al. (2021) data; AK and HI did not gain statehood until 1959, and are excluded. In both specifications, I weight by the female population in each state-year cell, specify a 2-year lag between treatment and outcomes, and include state and year fixed effects. Standard errors, clustered at the state-level, are in parentheses; p-values are in brackets.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table 6. TCFR LD: First-Stage
County-Level

	Dependent variable: Union Exposure	
	(1)	(2)
Pred. Union Exposure	1.105***	0.899***
	(0.133)	(0.127)
	[0.000]	[0.000]
F	457.8	134.2
Fixed Effects?	X	X
Controls?		X
Base Dep. Var. Mean	4.76	4.76
N	800	800
N (counties)	400	400

Notes: Each column reports estimates from separate regressions of union exposure on SSIV-predicted union exposure. The long-difference compares outcomes for cohorts born in 1901 (pre-NLRA) and 1937 (post-NLRA). The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). "Fixed Effects" include county, birth year, and state × birth year fixed effects. "Controls" include the following time-varying measures: log(population), the share of the labor force in manufacturing, pct white, pct male, pct urban, pct aged 15-44, pct foreign-born, the change in retail sales from 1929-1933, New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, WWII casualty rates per capita, and pct of households with a modern washing machine. I weight by female population in each county-birth year cell, and specify a 2-year lag between treatment and outcomes. I estimate HAC-corrected standard errors based on clusters defined by 100km radii around county centroids (Colella et al., 2019). Standard errors are in parentheses, p-values are in brackets. I report the Kleibergen-Paap rk Wald (first-stage) F statistic. Sample sizes are rounded to comply with FSRDC disclosure avoidance rules.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table 7. TCFR LD: Main Effects
County-Level

	Dependent variable: TCFR	
	(1)	(2)
Panel A: OLS		
Union Exposure	0.0153***	0.0037***
	(0.0022)	(0.0014)
	[0.000]	[0.0096]
Panel B: Reduced Form		
Pred. Union Exposure	0.0467***	0.0072*
	(0.0058)	(0.0037)
	[0.000]	[0.0516]
Panel C: IV/2SLS		
Union Exposure	0.0423***	0.0080**
	(0.0067)	(0.0037)
	[0.000]	[0.0338]
F	457.8	64.58
Fixed Effects?	X	X
Controls?		X
Base Dep. Var. Mean	2.809	2.809
N	800	800
N (counties)	400	400

Notes: Each column reports estimates from separate regressions of TCFRs on the specified independent variable. The long-difference measures changes in outcomes that result from changes in union exposure between the cohorts born in 1901 (pre-NLRA) and 1937 (post-NLRA). The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). "Fixed Effects" include county, birth year, and state × birth year fixed effects. "Controls" include the following time-varying measures: log(population), the share of the labor force in manufacturing, pct white, pct male, pct urban, pct aged 15-44, pct foreign-born, the change in retail sales from 1929-1933, New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, WWII casualty rates per capita, and pct of households with a modern washing machine. I weight by female population in each county-birth year cell. I estimate HAC-corrected standard errors based on clusters defined by 100km radii around county centroids (Colella et al., 2019). Standard errors are in parentheses, p-values are in brackets. I report the Kleibergen-Paap rk Wald (first-stage) F statistic. Sample sizes are rounded to comply with FSRDC disclosure avoidance rules.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table 8. TCFR LD: Spillover Effects
County-Level

	Dependent variable: TCFR	
	(1)	(2)
	OLS	IV/2SLS
Union Exposure	0.002	0.008
	(0.002)	(0.006)
	[0.221]	[0.172]
Union Sector	0.207***	0.143**
	(0.043)	(0.059)
	[0.000]	[0.016]
Union Exposure × Union Sector	0.007***	0.015***
	(0.001)	(0.004)
	[0.000]	[0.000]
F		41.69
Fixed Effects?	X	X
Controls?	X	X
Base Dep. Var. Mean	2.769	2.769
N	5400	5400

Notes: Each column reports estimates from separate county-birth year cohort-industry level regressions of TCFRs on union exposure (see Equation 6). The long-difference measures changes in outcomes that result from changes in union exposure between the cohorts born in 1901 (pre-NLRA) and 1937 (post-NLRA). The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). Union Sector = 1 if the household head is employed in the Construction, Manufacturing, Mining, or Transportation/Communications/Utilities industries, and = 0 otherwise. "Fixed Effects" include county, birth year, and state × birth year fixed effects. "Controls" include the following time-varying measures: log(population), the share of the labor force in manufacturing, pct white, pct male, pct urban, pct aged 15-44, pct foreign-born, the change in retail sales from 1929-1933, New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, WWII casualty rates per capita, and pct of households with a modern washing machine. I weight by female population in each county-birth year cell. I estimate HAC-corrected standard errors based on clusters defined by 100km radii around county centroids (Colella et al., 2019). Standard errors are in parentheses, p-values are in brackets. I report the Kleibergen-Paap rk Wald (first-stage) F statistic. Sample sizes are rounded to comply with FSRDC disclosure avoidance rules.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table 9. Mechanisms: Palmer Survey (1951)
Intensive Margin

	Dependent variable: # Children in HH				
	(1)	(2)	(3)	(4)	(5)
Union member	0.1311**	0.1087*	0.0891	0.1296**	0.0778
	(0.0595)	(0.0593)	(0.0584)	(0.0595)	(0.0583)
	[0.0278]	[0.0670]	[0.1275]	[0.0296]	[0.1825]
Log(Weekly earnings) (1950)		0.3427***			0.2006**
		(0.0883)			(0.0928)
		[0.0001]			[0.0308]
Any unemployment (1940-1951)			-0.1024		-0.0922
			(0.1198)		(0.1207)
			[0.3927]		[0.4450]
Months in labor force (1940s)			0.0066***		0.0063***
			(0.0013)		(0.0013)
			[0.0000]		[0.0000]
Avg job length in mos. (1940-1951)			0.0022*		0.0021
			(0.0013)		(0.0013)
			[0.0782]		[0.1054]
Δ Industry (1949-1951)			-0.0525		-0.0324
			(0.1012)		(0.1019)
			[0.6041]		[0.7504]
Δ Occupation (1949-1951)			-0.0685		-0.0508
			(0.0931)		(0.0941)
			[0.4619]		[0.5888]
Years in area			0.0082***		0.0080***
			(0.0024)		(0.0024)
			[0.0006]		[0.0008]
Δ Son OCCSCORE				0.0036	0.0022
				(0.0026)	(0.0026)
				[0.1754]	[0.3854]
R^2	.069	.076	.107	.07	.11
N	1973	1973	1973	1973	1973

Notes: Results are for a sample of male household heads aged 25-39 in five Northern labor markets (see Section 3.1). Each column reports estimates from a separate regression of the number of children in the household on a dummy variable for union status and the specified mechanism variable(s) in each row. All specifications additionally include controls for race, age, a quadratic in age, occupation, and city fixed effects. Heteroskedasticity-robust errors are in parentheses, and p-values are in brackets.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table 10. Mechanisms: Birth Rate LD
County-Level

	Dependent variable: Birth Rate										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Union Membership Rate	1.7079***	1.4013***	1.2687**	1.6699***	1.4583***	0.9437***	1.6367***	1.6917***	1.6270***	1.5884***	0.1443
	(0.3066)	(0.2132)	(0.5026)	(0.2434)	(0.2703)	(0.2272)	(0.2945)	(0.2971)	(0.2364)	(0.2815)	(0.4046)
Median Family Income		0.0150***	0.0122	0.0193***							-0.0110
		(0.0027)	(0.0113)	(0.0047)							(0.0078)
% Low Inc. HHs | Med. HH Inc.			-0.1910								-1.6074***
			(0.6399)								(0.2334)
% Mid Inc. HHs | Med. HH Inc.				-1.1744***							-0.8419**
				(0.3311)							(0.3608)
Unemployment Rate					-1.2711***						-0.3916
					(0.3481)						(0.2423)
LF Share Female						-2.1617***					-1.1118
						(0.3291)					(0.6852)
ihs(Hospital Beds)							0.7115**				0.1127
							(0.3307)				(0.2369)
Maternal Mortality Rate								0.0045			-0.0005
								(0.0040)			(0.0020)
Infant Mortality Rate									82.7307		-6.6708
									(74.7366)		(60.7953)
Pct. Owner-Occupied Units										0.5470*	-0.1585
										(0.3164)	(0.1541)
Base Dep. Var. Mean	62.51	62.51	62.51	62.51	62.51	62.51	62.51	62.51	62.51	62.51	62.51
N	824	824	824	824	824	824	824	824	824	824	824
N (counties)	412	412	412	412	412	412	412	412	412	412	412

Notes: Each column reports estimates from separate IV long-difference regressions of birth rates (by residence) on union membership rates. The long-difference measures the impact of changes in union membership rates between 1934 (pre-NLRA) and 1960 (post-NLRA) on changes in outcomes. I impose a 2-year lag relationship between treatment and outcomes. The sample includes all counties in the main analysis sample (CA, IL, MO, PA, WI). Each specification includes county, year, and state × year fixed effects, in addition to the variables specified in each row. I provide details on the sources and methods used to construct each variable in Appendix G. I weight by female population in each county-year cell. Standard errors, clustered at the county level, are in parentheses.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table 11. Industry Group Mappings

Industry Group	IND1950 Codes	SIC 1957 Codes
Agriculture	105-126	01-09
Mining: Coal, Metals, Quarrying	206, 216, 236, 239	10-12, 14
Mining: Crude Petroleum, Natural Gas Extraction	226	13
Construction	246	15-17
Manufacturing: Misc	348, 399, 426, 499	19, 39
Manufacturing: Food, Liquor, Tobacco	406-419, 429	20-21
Manufacturing: Textile	436-446	22
Manufacturing: Clothing	448-449	23
Manufacturing: Lumber, Wood, Furniture	306-309	24-25
Manufacturing: Paper, Printing, Publishing	456-459	26-27
Manufacturing: Chemical , Rubber, Plastic	466-478	28-30
Manufacturing: Leather	487-489	31
Manufacturing: Stone, Clay, Glass	316-326	32
Manufacturing: Metals, Machinery, Equipment	336-346, 356-388	33-38
Transportation	506-568	40-47
Communications	578-579, 856	48
Utilities	586-598	49
Trade	606-699	50-59
Finance, Insurance, Real Estate	716-756	60-67
Services	806-849, 857-899	70-89
Government	906-946	91-94

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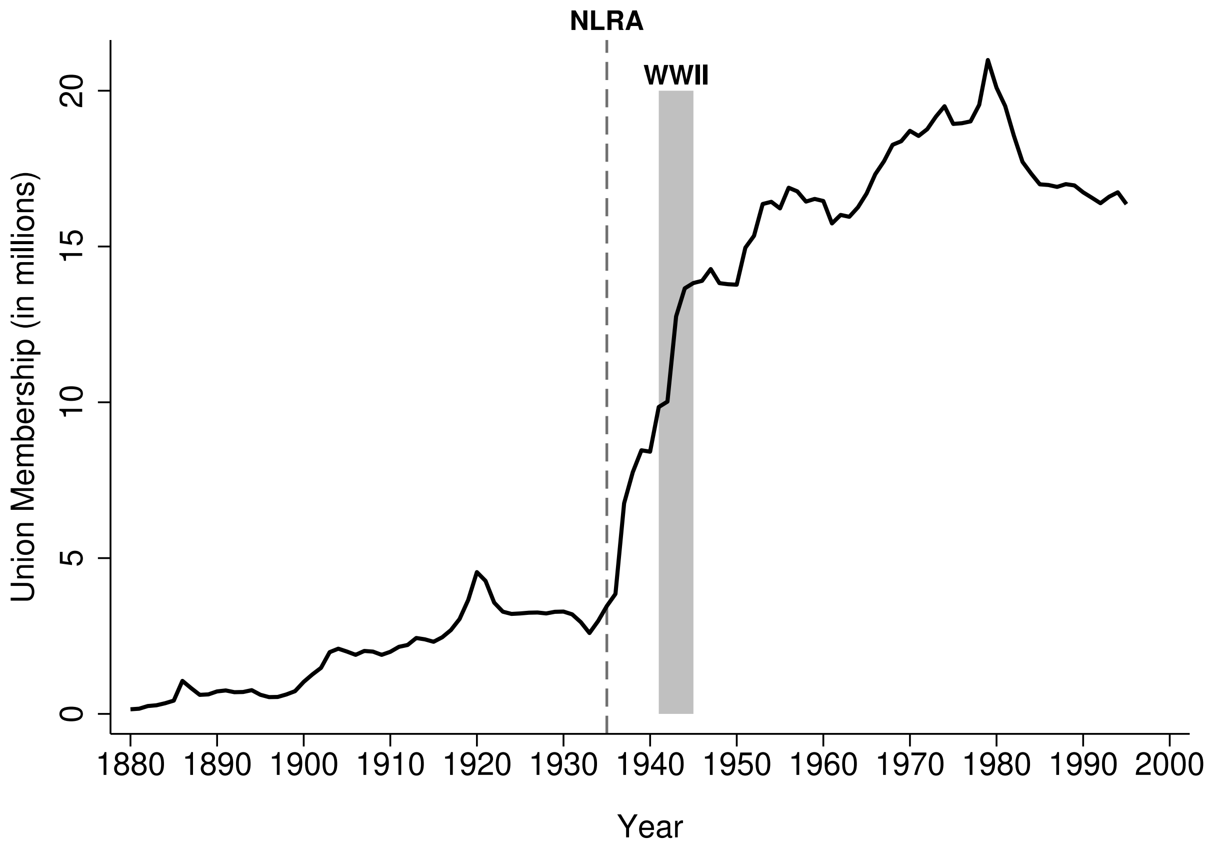
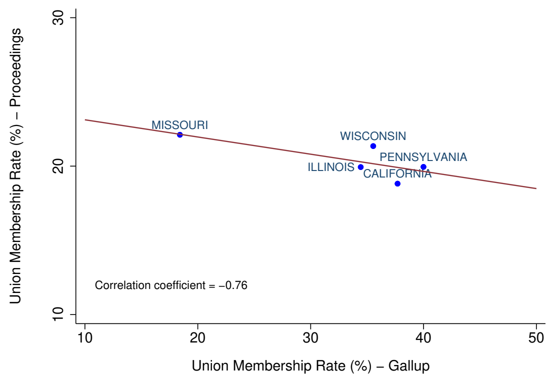
A Additional Tables and Figures

Figure A1. Union Membership in the United States: 1880-1995

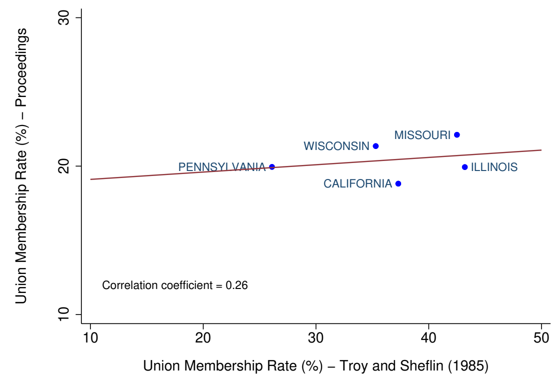


Notes: Source for union membership data: Freeman (1997), Appendix A. The NLRA was enacted in 1935.

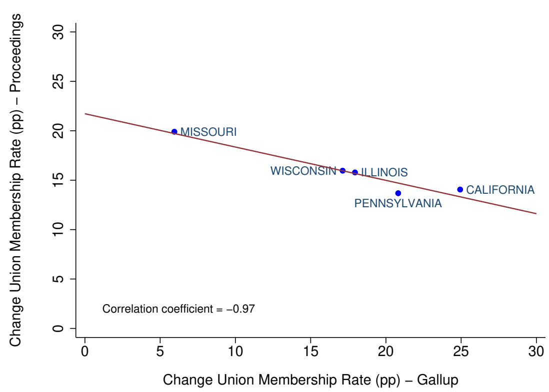
Figure A2. County-Level Union Membership Data Validation



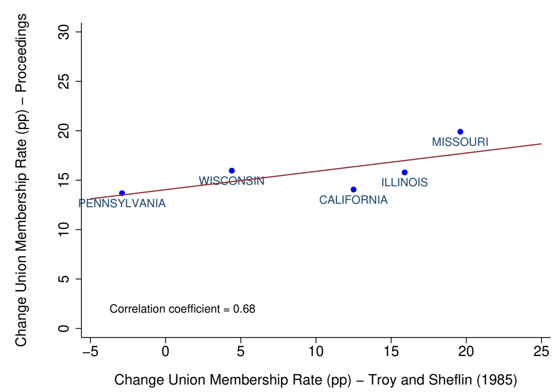
(a) 1960 Levels: Proceedings vs. Gallup



(b) 1960 Levels: Proceedings vs. Troy and Sheflin (1985)



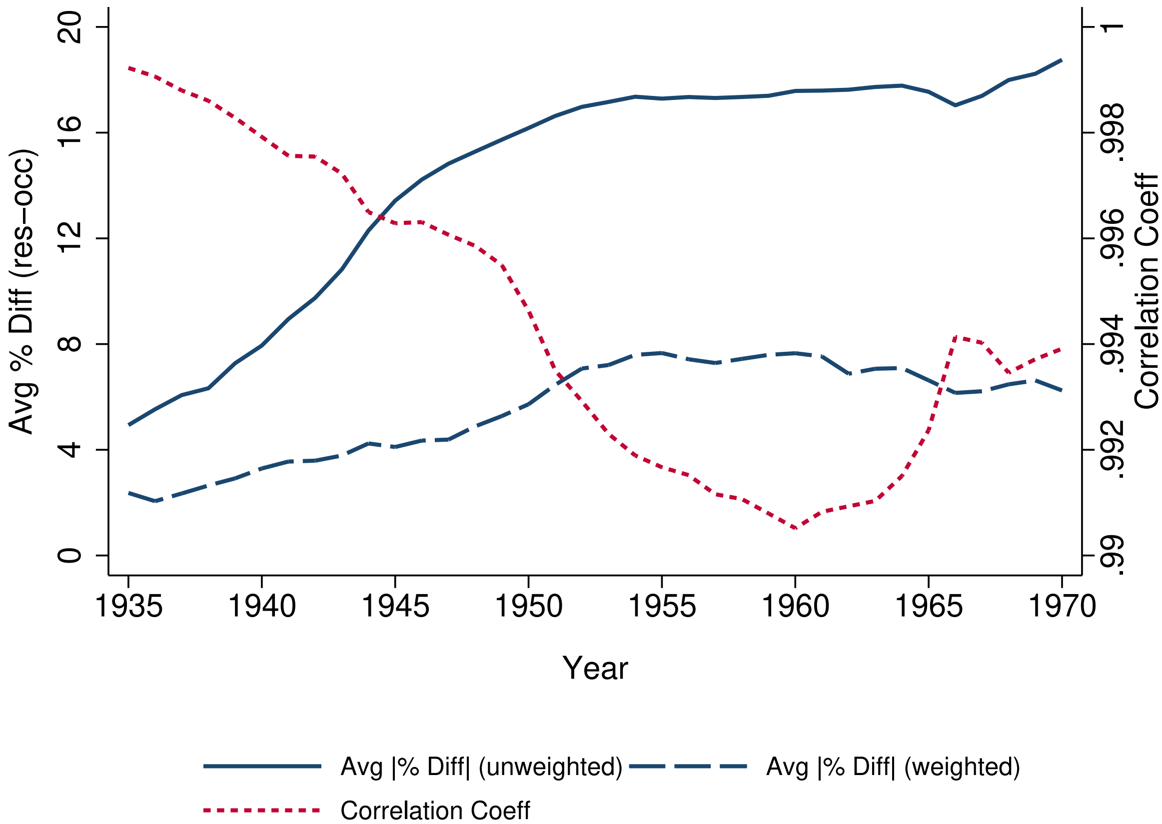
(c) Changes: Proceedings (1934-1960) vs. Gallup (1937-1960)



(d) Changes: Proceedings (1934-1960) vs. Troy and Sheflin (1985) (1939-1960)

Notes: "Proceedings" data refer to novel estimates of union membership from this paper, constructed from union convention proceedings as described in Section 4.1, and aggregated to the state-level. "Gallup" data refer to state-level series constructed by Farber et al. (2021) from Gallup opinion polls. "Troy and Sheflin (1985)" data refer to state-level estimates constructed by those authors from archival materials and personal correspondence. In all cases, the denominator for union membership rates is equal to total non-farm employment. For Proceedings data, changes are measured from 1934-1960; for Gallup data, changes are measured from 1937-1960; for Troy and Sheflin (1985) data, changes are measured from 1939-1960.

Figure A3. Births by Residence vs. Births by Occurrence



Notes: The sample includes all counties in the U.S. The blue solid (long-dashed) line plots the unweighted (weighted) average of the absolute percent difference between births by residence and births by occurrence in each year. The red short-dashed line plots the average county-level correlation coefficient in each year.

Figure A4. Cameron County and Forest County, PA Map

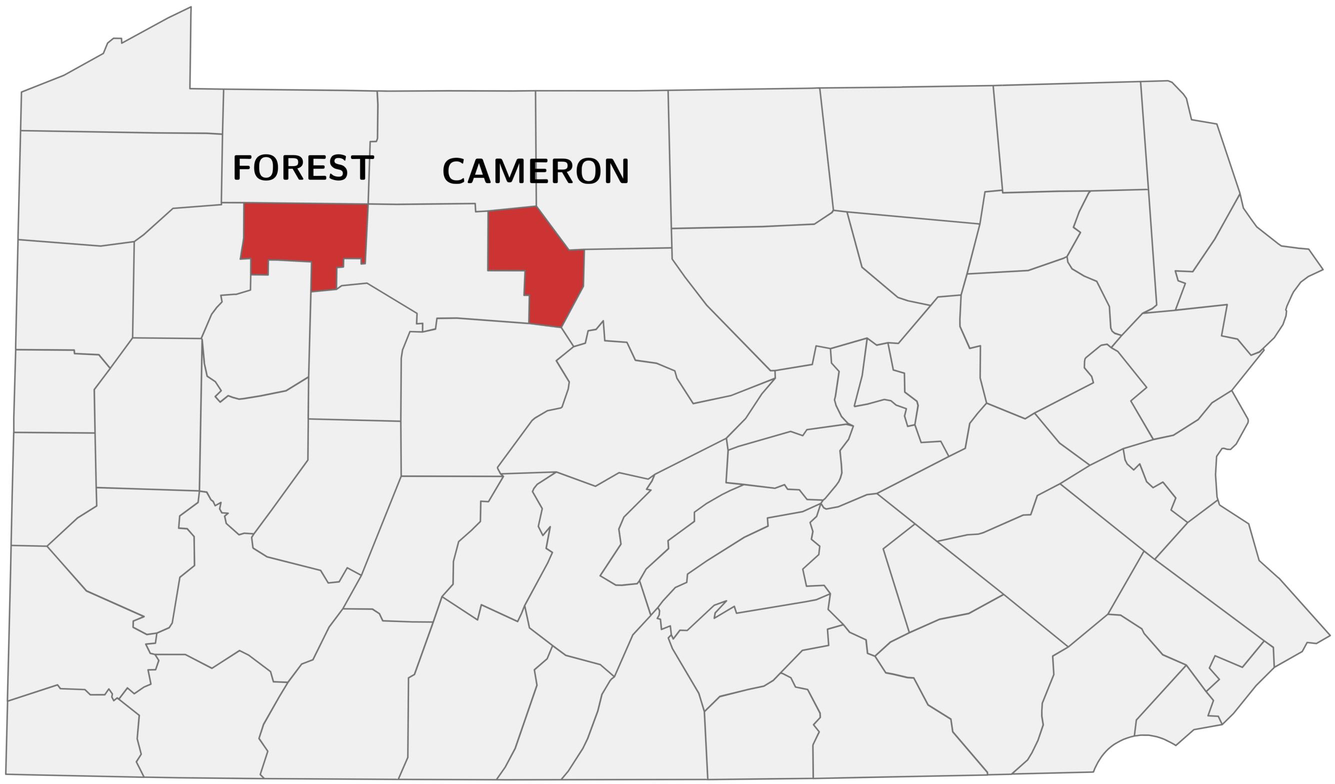
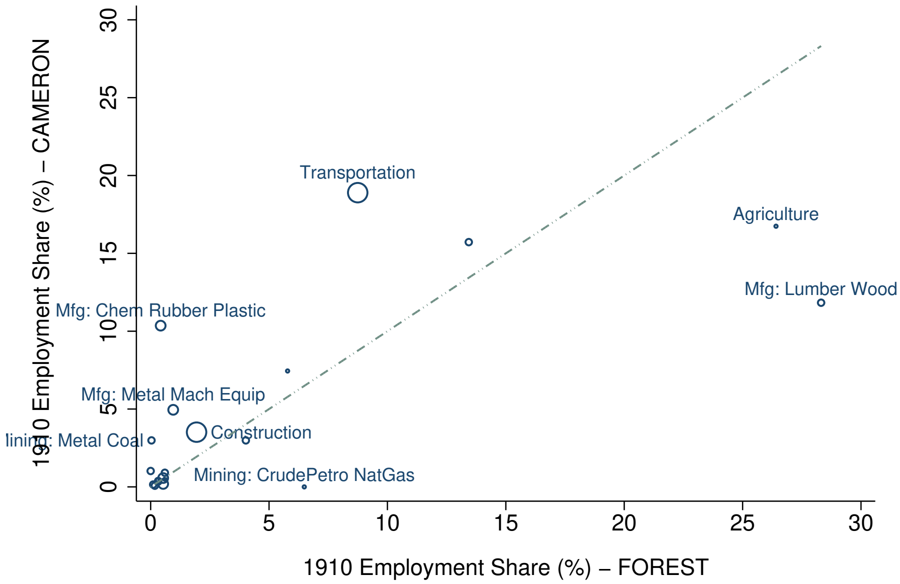
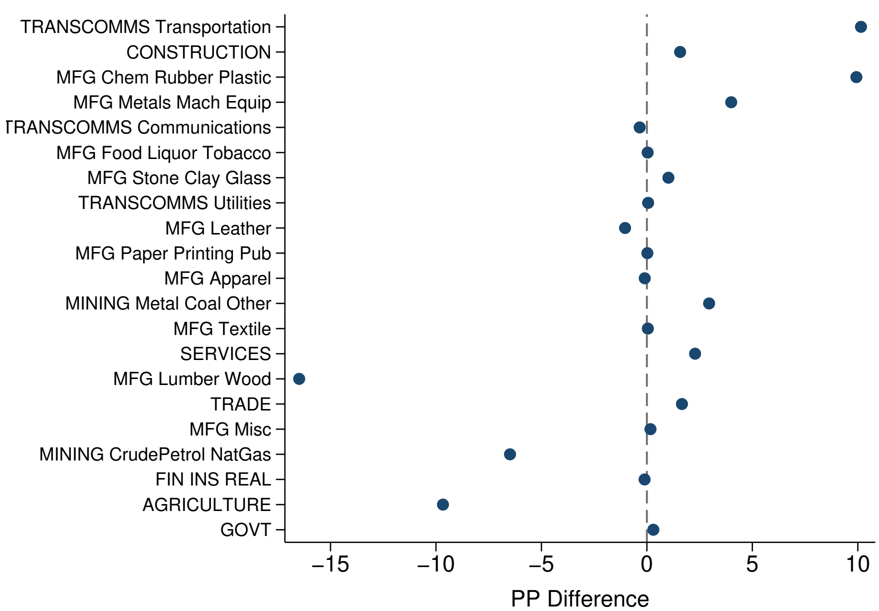


Figure A5. Cameron County and Forest County, PA
1910 Industry Share Comparison



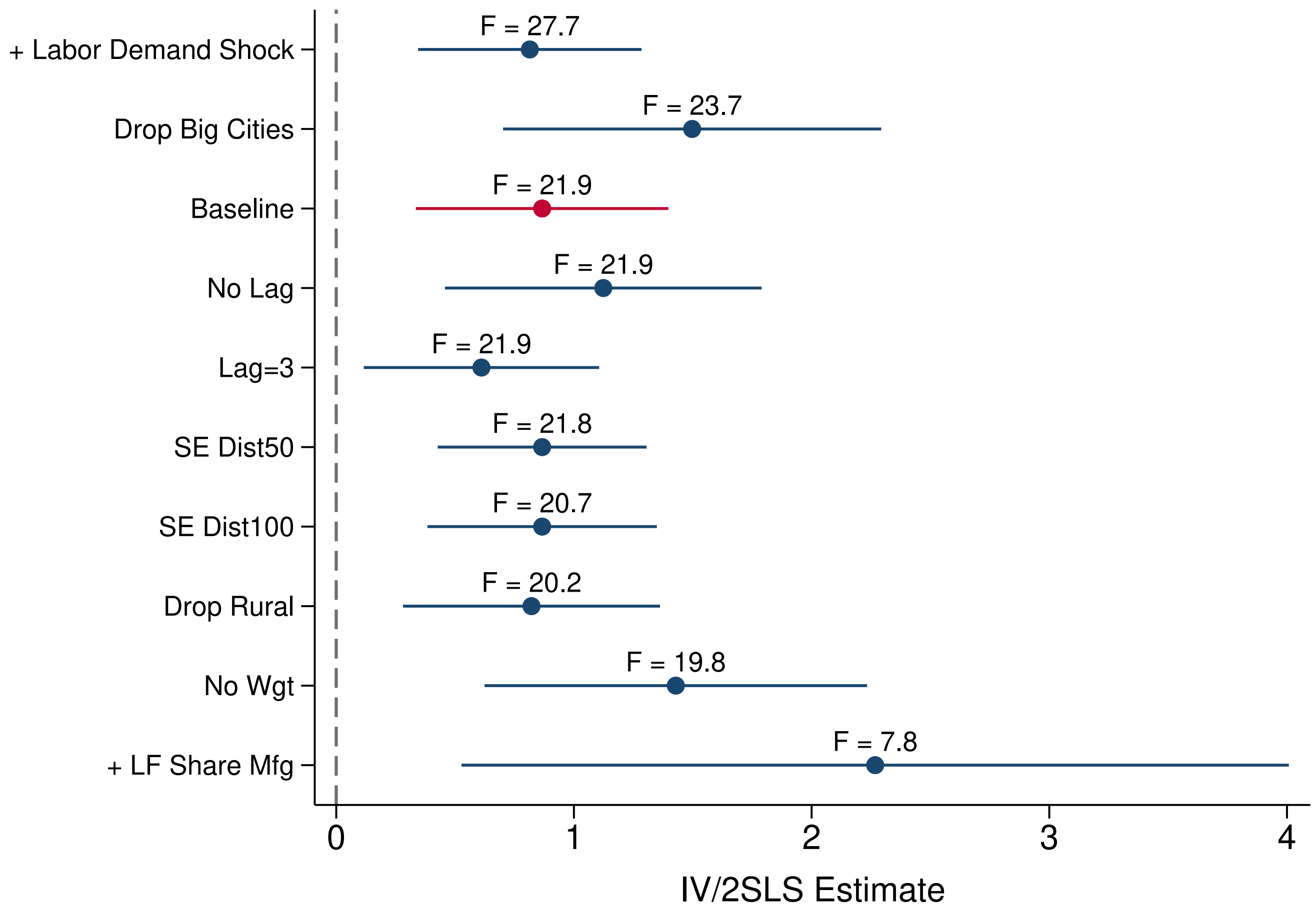
(a) 1910 Industry Shares: Levels



(b) 1910 Industry Shares: Cross-County Differences

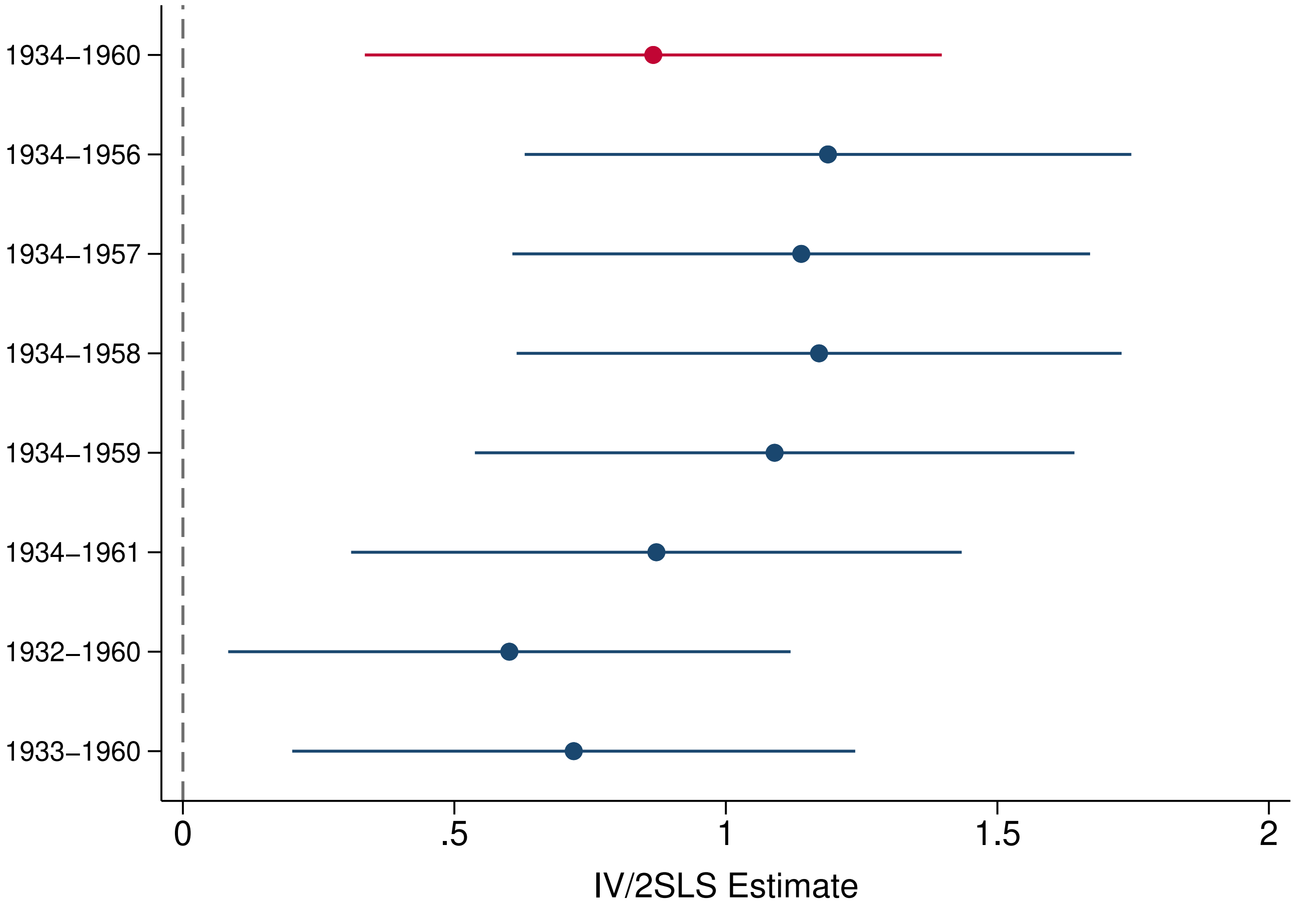
Notes: Panel A plots 1910 industry shares of employment in each county. The marker size represents each industry's growth in national union membership rates from 1934-1960 (larger marker = higher growth). Panel B plots the percentage point difference in 1910 industry share of employment (Cameron minus Forest) for each industry group. Industry groups are presented in descending order by national growth in union membership rates, 1934-1960.

Figure A6. Birth Rate LD: Robustness
County-Level



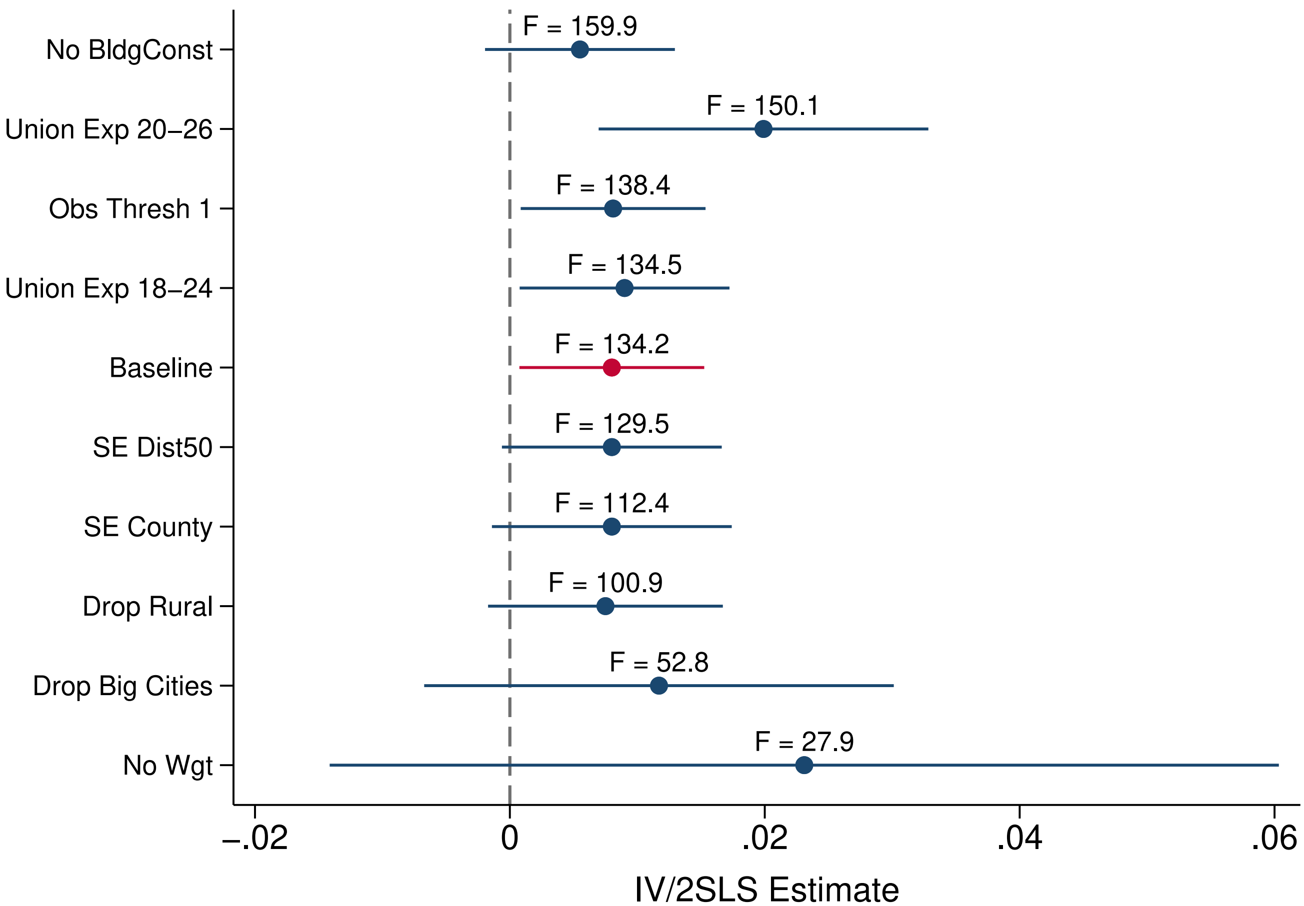
Notes: Each plotted point corresponds to a point estimate of β_{2SLS} from separate regressions of birth rates on union membership rates, where SSIV-predicted union membership rates instrument for actual union membership rates. Whiskers represent 95% confidence intervals. Estimates are sorted in descending order by the Kleibergen-Paap rk Wald F (first-stage) statistic. The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, and WI). The baseline specification corresponds to the 1934-1960 long-difference IV/2SLS model with baseline controls (Table 3, Panel C, column 2). All other specifications differ from the baseline model only in the manner specified by the row labels: "SE County" clusters standard errors at the county-level; "Drop Big Cities" excludes counties with population $\geq$ 500,000 in 1960; "No Lag" does not impose any lag relationship between treatment and outcome variables; "Lag=3" imposes a 3-year lag relationship between treatment and outcome variables; "SE Dist50" clusters standard errors based on units defined by 50km radii around county centroids; "No Wgt" removes population weights; "Drop Rural" excludes counties with 1960 % Urban = 0; "+ Labor Demand Shock" additionally controls for an index that captures local shifts in the relative demand for skilled versus unskilled workers (Goldin and Margo, 1992); "+ LF Share Mfg" additionally controls for the share of the labor force in manufacturing from 1930-1960.

Figure A7. Birth Rate LD: Alternative Start/End Years
County-Level



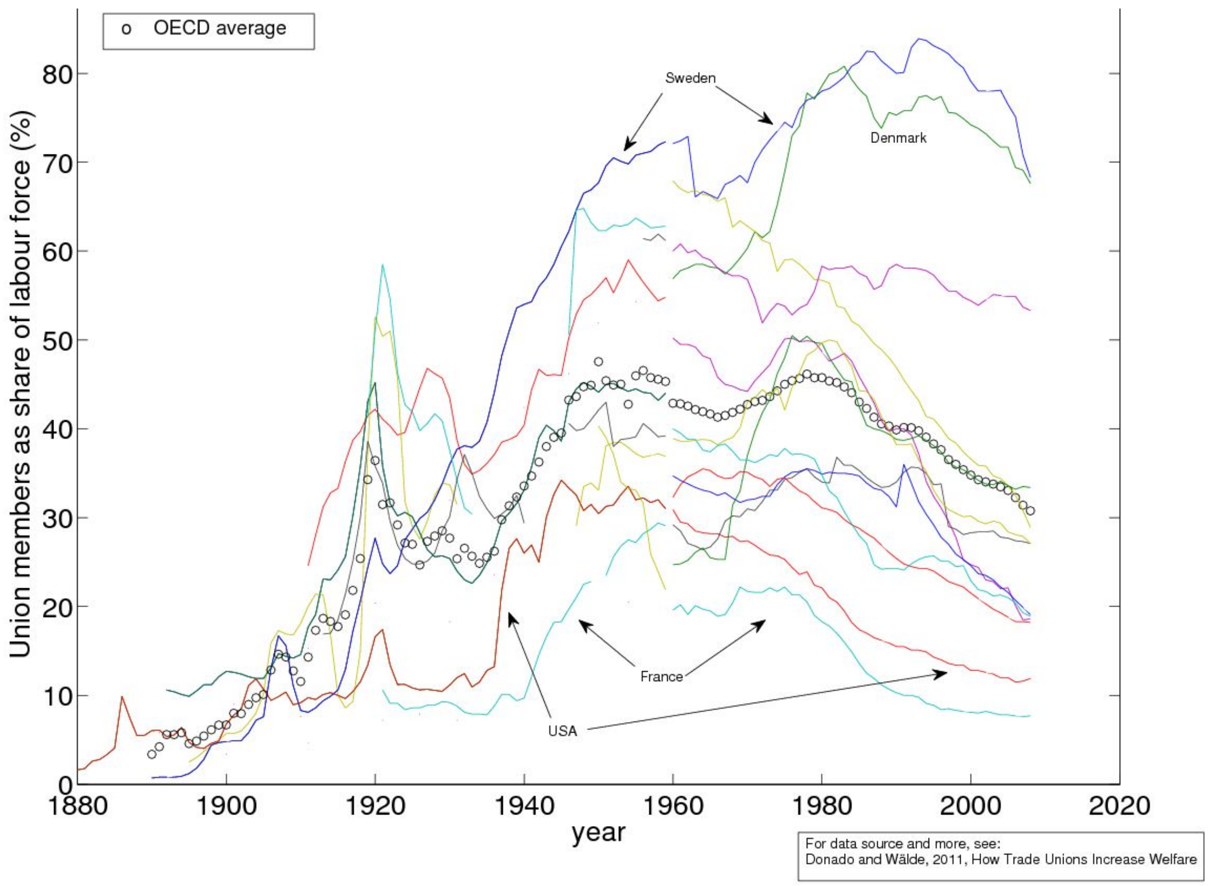
Notes: Each plotted point corresponds to a point estimate of β_{2SLS} from separate regressions of birth rates on union membership rates, where SSIV-predicted union membership rates instrument for actual union membership rates. Whiskers represent 95% confidence intervals. The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, and WI). The baseline specification corresponds to the 1934-1960 long-difference IV/2SLS model with baseline controls (Table 3, Panel C, column 2). All other specifications differ from the baseline model only in the start/end years used in the long-difference, as specified by the row labels.

Figure A8. TCFR LD: Robustness
County-Level



Notes: Each plotted point corresponds to a point estimate of β_{2SLS} from separate county-birth year cohort regressions of TCFRs on union exposure, where SSIV-predicted union membership exposure instruments for actual union exposure. Whiskers represent 95% confidence intervals. Estimates are sorted in descending order by the Kleibergen-Paap rk Wald F (first-stage) statistic. The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, and WI). The baseline specification corresponds to the long-difference IV/2SLS model with full controls (Table 7, Panel C, column 2). All other specifications differ from the baseline model only in the manner specified by the row labels: "No Wgt" removes population weights; "SE County" clusters standard errors at the county-level; "SE Dist50" clusters standard errors based on units defined by 50km radii around county centroids; "Drop Big Cities" excludes counties with population $\geq$ 500,000 in 1960; "Drop Rural" excludes counties with 1960 % Urban = 0; "Obs Thresh 1" additionally includes counties that have between 1 and 5 observations in each county-cohort cell; "Union Exp 18-24" alternatively measures union exposure at ages 18-24 for cohorts born in 1902 and 1938; "Union Exp20-26" alternatively measures union exposure at ages 20-26 for cohorts born in 1900 and 1936; finally, in the "No BldgConst" specification, I re-construct the aggregate shift-share IV after leaving out the building/construction industry.

Figure A9. Trade Union Density in Selected OECD Countries: 1880-2008
Donado and Wälde (2012)



Notes: This figure and the underlying data used to produce it are attributable to Donado and Wälde (2012).

Table A1. Birth Rate LD: Pre-Period Effects
County-Level

	Dependent variable: Birth Rate	
	(1)	(2)
Panel A: OLS		
Union Membership Rate	0.376***	0.444***
	(0.109)	(0.094)
	[0.001]	[0.000]
Panel B: Reduced Form		
Pred. Union Membership Rate	0.606**	-0.107
	(0.303)	(0.438)
	[0.046]	[0.808]
Panel C: IV/2SLS		
Union Membership Rate	-1.251	0.408
	(0.929)	(1.610)
	[0.180]	[0.800]
F	4.53	0.97
Fixed Effects?	X	X
Baseline Controls?		X
Base Dep. Var. Mean	91.92	91.92
N	596	596
N (counties)	298	298

Notes: Each column reports estimates from separate regressions of birth rates by residence on the specified independent variable. The long-difference measures changes in outcomes that result from changes in union membership rates between 1920 and 1934. I impose a 2-year lag relationship between treatment and outcomes. The sample includes all counties in the main analysis sample of states except MO, which did not join the U.S. Birth Registration area until 1927. "Fixed Effects" include county, year, and state × year fixed effects. "Baseline Controls" include: log(population), pct white, pct male, and pct foreign-born, measured in the 1930 Census and interacted with time fixed effects; the change in retail sales from 1929-1933, interacted with time fixed effects; and the change in birth rates from 1929-1933, interacted with time fixed effects. I weight by female population in each county-year cell. Standard errors, clustered at the county level, are in parentheses, p-values are in brackets. I report the Kleibergen-Paap rk Wald (first-stage) F statistic.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table A2. Children Ever Born LD: First-Stage County-Level

	Dependent variable: Union Exposure	
	(1)	(2)
Pred. Union Exposure	1.134***	0.934***
	(0.129)	(0.140)
	[0.000]	[0.000]
F	931.8	198.3
Fixed Effects?	X	X
Controls?		X
Base Dep. Var. Mean	4.69	4.69
N	800	800
N (counties)	400	400

Notes: Each column reports estimates from separate regressions of union exposure on SSIV-predicted union exposure. The long-difference compares outcomes for cohorts born in 1901 (pre-NLRA) and 1937 (post-NLRA). The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). "Fixed Effects" include county, birth year, and state × birth year fixed effects. "Controls" include the following time-varying measures: log(population), the share of the labor force in manufacturing, pct white, pct male, pct urban, pct aged 15-44, pct foreign-born, the change in retail sales from 1929-1933, New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, WWII casualty rates per capita, and pct of households with a modern washing machine. I weight by female population in each county-birth year cell. I estimate HAC-corrected standard errors based on clusters defined by 100km radii around county centroids (Colella et al., 2019). Standard errors are in parentheses, p-values are in brackets. I report the Kleibergen-Paap rk Wald (first-stage) F statistic. Sample sizes are rounded to comply with FSRDC disclosure avoidance rules.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table A3. Children Ever Born LD: Main Effects County-Level

	Dependent variable: Children Ever Born	
	(1)	(2)
Panel A: OLS		
Union Exposure	0.0161***	0.0039
	(0.0032)	(0.0045)
	[0.000]	[0.3803]
Panel B: Reduced Form		
Pred. Union Exposure	0.0497***	0.0115
	(0.0064)	(0.0116)
	[0.000]	[0.3206]
Panel C: IV/2SLS		
Union Exposure	0.0438***	0.0123
	(0.0068)	(0.0123)
	[0.000]	[0.3189]
F	931.8	198.30
Fixed Effects?	X	X
Controls?		X
Base Dep. Var. Mean	2.46	2.46
N	800	800
N (counties)	400	400

Notes: Each column reports estimates from separate regressions of children ever born on the specified independent variable. The long-difference measures changes in outcomes that result from changes in union exposure between the cohorts born in 1901 (pre-NLRA) and 1937 (post-NLRA). The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). "Fixed Effects" include county, birth year, and state × birth year fixed effects. "Controls" include the following time-varying measures: log(population), the share of the labor force in manufacturing, pct white, pct male, pct urban, pct aged 15-44, pct foreign-born, the change in retail sales from 1929-1933, New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, WWII casualty rates per capita, and pct of households with a modern washing machine. I weight by female population in each county-birth year cell. I estimate HAC-corrected standard errors based on clusters defined by 100km radii around county centroids (Colella et al., 2019). Standard errors are in parentheses, p-values are in brackets. I report the Kleibergen-Paap rk Wald (first-stage) F statistic. Sample sizes are rounded to comply with FSRDC disclosure avoidance rules.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table A4. Mechanisms: Palmer Survey (1951)
Extensive Margin

	Dependent variable: Any Children in HH				
	(1)	(2)	(3)	(4)	(5)
Union member	0.0204	0.0125	0.0053	0.0199	0.0009
	(0.0237)	(0.0238)	(0.0233)	(0.0237)	(0.0235)
	[0.3897]	[0.5975]	[0.8216]	[0.4011]	[0.9696]
Log(Weekly earnings) (1950)		0.1198***			0.0757**
		(0.0364)			(0.0376)
		[0.0010]			[0.0445]
Any unemployment (1940-1951)			-0.0193		-0.0192
			(0.0467)		(0.0469)
			[0.6795]		[0.6823]
Months in labor force (1940s)			0.0013***		0.0012**
			(0.0005)		(0.0005)
			[0.0080]		[0.0125]
Avg job length in mos. (1940-1951)			0.0008**		0.0008**
			(0.0004)		(0.0004)
			[0.0359]		[0.0428]
Δ Industry (1949-1951)			-0.0353		-0.0276
			(0.0410)		(0.0411)
			[0.3890]		[0.5028]
Δ Occupation (1949-1951)			0.0064		0.0115
			(0.0407)		(0.0416)
			[0.8747]		[0.7817]
Years in area			0.0039***		0.0038***
			(0.0009)		(0.0010)
			[0.0000]		[0.0001]
Δ Son OCCSCORE				0.0006	0.0003
				(0.0010)	(0.0010)
				[0.5606]	[0.7730]
R^2	.056	.063	.085	.057	.088
N	1973	1973	1973	1973	1973

Notes: Results are for a sample of male household heads aged 25-39 in five Northern labor markets (see Section 3.1). Each column reports estimates from a separate regression of a dummy variable for any children in the household on a dummy variable for union status and the specified mechanism variable(s) in each row. All specifications additionally include controls for race, age, a quadratic in age, occupation, and city fixed effects. Heteroskedasticity-robust errors are in parentheses, and p-values are in brackets.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table A5. LD Effect of Unionization on Hypothesized Mechanisms
County-Level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Med. HH Inc.	% Low Inc. HHs | Med. HH Inc.	% Mid Inc. HHs | Med. HH Inc.	Unemp. Rate	LF Share Female	ihs(Beds)	Maternal Mort. Rate	Infant Mort. Rate	% Units Owner-Occ.
Union Membership Rate	28.477***	-1.111***	0.899***	-0.135***	-0.302***	0.038	-4.600	0.001***	0.422*
	(10.547)	(0.275)	(0.213)	(0.027)	(0.081)	(0.056)	(3.876)	(0.001)	(0.238)
	[0.007]	[0.000]	[0.000]	[0.000]	[0.000]	[0.502]	[0.235]	[0.007]	[0.076]
Base Dep. Var. Mean	2783.32	0.16	0.65	9.45	21.92	7.46	484.35	0.05	43.55
N	826	826	826	826	826	826	826	826	826
N (counties)	413	413	413	413	413	413	413	413	413

Notes: Each column reports estimates from separate regressions of the specified outcome on union membership rates. The long-difference measures changes in outcomes that result from changes in union membership rates between 1934 (pre-NLRA) and 1960 (post-NLRA). For details on the sources and methods used to construct each outcome, see Appendix G. The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). All regressions include county, year, and state × year fixed effects, as well as the following time-varying measures: log(population), the share of the labor force in manufacturing, pct white, pct male, pct urban, pct aged 15-44, pct foreign-born, the change in retail sales from 1929-1933, New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, WWII casualty rates per capita, and pct of households with a modern washing machine. In columns 2 and 3, I additionally control for the median family income. Median Family Income, % of Low Income Households, % of Mid Income Households, and % Units Owner Occupied are measured in 1940 and 1960; Unemployment Rate and LF Share Female are measured in 1930 and 1960; Hospital Beds and Infant Mortality Rate are measured in 1934 and 1960; Maternal Mortality Rate is measured in 1935 and 1960. I weight by female population in each county-year cell. I estimate HAC-corrected standard errors based on clusters defined by 100km radii around county centroids (Colella et al., 2019). Standard errors are in parentheses, p-values are in brackets.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table A6. Development of the U.S. Birth Registration Area (BRA)

Year	States Added to BRA	US Pop. in BRA (%)
1915	CT, DC, ME, MA, MI, MN, NH, NY, **PA**, VT	30.9
1916	MD	32.3
1917	IN, KS, KY, NC, OH, UT, VA, WA, **WI**	53.5
1918		53.4
1919	**CA**, OR	58.6
1920	NE	59.7
1921	DE, MS, NJ, RI	65.2
1922	**IL**, MT, WY	72.3
1923		72.4
1924	FL, IA, ND	76.2
1925	WV	76.2
1926	AZ, ID	77
1927	AL, AR, LA, **MO**, TN	87.6
1928	CO, GA, OK, SC	94.3
1929	NV, NM	94.7
1930		94.7
1931		94.7
1932	SD	95.2
1933	TX	100

Notes: Source: Table A of Part I of the 1944 Vital Statistics of the U.S. The five states in the main analysis sample are in bold.

B Images of Convention Proceedings Data

Figure B1. Example of Direct Estimates of Union Membership: Missouri AFL-CIO, 1957

GENERAL FUND RECEIPTS
FISCAL YEAR JULY 1, 1956 TO JUNE 30, 1957
PER CAPITA MEMBERSHIP AS OF JUNE 30, 1957
AND
CONVENTION VOTES BASED ON THE AVERAGE PAYMENT OF
PER CAPITA TAX FOR THE FISCAL YEAR.

ST. LOUIS

	Union No.	Per Capita Tax	Members	Votes
COUNCILS—				
Allied Printing Trades Council		$ 20.00		3
Building Trades Council		20.00		3
Central Trades & Labor Union		40.00		3
Industrial Union Council		25.00		3
FEDERAL LABOR UNIONS—				
Advertising, Pub. & Newspaper Rep.	20711	7.20	18	1
Bag Makers	23606	38.30	85	1
Dental Laboratory Technicians	18405	50.00	125	1
Embalmers	21301	31.20	78	1
Smelter Workers	22181	13.00	11	1
Wire & Corrugated Glass Wkrs.	22952	183.60	386	4
INTERNATIONAL UNIONS—				
Aluminum Workers	160	110.10	381	4
Asbestos Workers	1	112.00	280	3
Automobile Workers, United—				
Automobile Workers	25	2,831.00	6,052	56
Automobile Workers	231	211.80	560	5
Automobile Workers	325	662.20	3,376	22
Automobile Workers	691	283.60	973	9
Automobile Workers	819	1,131.40	2,000	19
Automobile Workers	881	19.10	48	1
Automobile Workers	986	116.80	268	3
Automotive Workers	1168	100.00	304	3
Bakery & Confectionery Wkrs.—				
Bakers' Auxiliary	4	160.00	400	4
Bakery & Conf. Wkrs.	4	560.00	1,400	14
Biscuit & Cracker Wkrs.	254	160.00	400	4
Barbers	102	320.00	800	8
Barbers	876	19.40	44	1
Bill Posters	5	23.00	55	1
Boilermakers	27	60.00	150	1
Boilermakers	595	110.00	300	3
Bookbinders	18	180.00	450	4
Bookbinders (Bindery Women's)	55	260.00	650	6
Boot & Shoe Workers	25	240.00	600	6
Boot & Shoe Workers	90	160.00	400	4
Boot & Shoe Workers	709	8.00	80	1
Brewery Workers, United—				
Brewery Workers	187	660.00	2,200	22
Laboratory Technicians	262	36.50	80	1
Bricklayers	1	400.00	1,000	10

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Figure B2. Example of Per Capita Tax Receipt-Based Estimates of Union Membership: Pennsylvania AFL-CIO, 1961

Federal Labor Union—Local Industrial Unions (Code 2)

520	Philadelphia	$	126.00
1242	Pittsburgh		30.99
1279	Pittsburgh		10.20
14712	Philadelphia		9.00
18047	Scranton		23.25
18820	Pittsburgh		33.54
18887	Philadelphia		*
20029	Philadelphia		6.00
20786	Pittsburgh		21.81
21651	Philadelphia		16.74
22254	Philadelphia		41.85
22319	Philipsburg		4.23
22705	Pittsburgh		90.00
22706	Springboro		34.08
23068	Erie		40.50
23134	Pittsburgh		28.80
24188	Phoenixville		46.11

Aluminum Workers International Union (Code 4)

405	Cressona	$	177.39

Asbestos Workers, International Association (Code 5)

2	Pittsburgh		*
14	Philadelphia		,2.19
65	York		8.10
93	Philadelphia		11.61

United Automobile, Aircraft, and Agricultural Implement Workers of America (Code 6)

18	Scranton	49.17
66	Uniontown	2.00
69	New Castle	170.21
92	Philadelphia	353.19
130	Bristol	122.70
131	Ardmore	40.29
224	Chester	63.42
293	Philadelphia	46.47
331	Pittsburgh	6.93
416	Philadelphia	170.22
464	Bridgeport	17.31
482	Williamsport	7.59
519	Hazleton	32.37
521	Pittsburgh	8.28
544	McKeesport	180.00
585	Philadelphia	244.41
587	Hometown	42.96
618	Erie	9.21
620	Shippensburg	23.67
629	Corry	56.31
644	Pottstown	193.74
677	Allentown	545.88
695	Chambersburg	37.65
739	Erie	19.14
746	Chambersburg	13.68
758	Latrobe	30.00
786	York	111.66
787	Williamsport	234.45
813	Philadelphia	704.94
834	Philadelphia	*
918	Chester	220.08
929	Philadelphia	33.63
1001	Pittsburgh	14.07
1020	Pittsburgh	135.33
1036	Pittsburgh	144.00
1039	North Wales	32.55
1056	Pottstown	113.88
1069	Morton	156.63
1079	York	42 12
1116	Erie	35.85
1186	Erie	39.54
1193	Eynon (a)	12.51
1206	Allentown	30.24
1221	Old Forge	29.37
1225	Latrobe	6.14
1238	Hellertown	51.24
1242	Pittsburgh	68.91
1250	New Castle	35.81
1281	Alburtis	7.47
1282	Bellefonte (a)	25.56
1311	Langeloth	.8.51
1318	Essington	24.27

American Bakery and Confectionery Workers International Union (Code 7)

	Philadelphia Jt. Board—Philadelphia	$	15.00
	Penna. State Board—Philadelphia		*
6	Philadelphia		630.00
12	Pittsburgh		252.00
44	Pittsburgh		18.00
53	Scranton (d)		....
159	Allentown		22.95
179	Bethlehem		35.10
201	Philadelphia		36.00
265	Reading		198.00
272	Lititz		*
289	Reading		144.00
309	Easton		36.00
321	Wilkes-Barre		55.62
439	Philadelphia		180.00
464	Hershey		270.00
492	Philadelphia		180.00

The Journeymen Barbers, Hairdressers, and Cosmetologists' International Union of America (Code 8)

	State Assn.—Pennsylvania		*
9	Philadelphia	$	150.30
20	Pittsburgh		90.00
31	Lansdowne (d)		....
40	Turtle Creek		14.88
89	Butler		6.00
149	Erie		17.10
157	Franklin		6.00
198	Meadville		3.00
203	Reading		6.00
244	Wilkes-Barre		13.86
245	Kingston		6.00
262	Pottsville		12.00
266	New Kensington		20.34
272	Lackawanna		12.00
273	Warren		6.00
277	Easton		14.58
278	New Castle		7.56
280	Beaver Falls		13.56
285	Washington		10.17
286	Tamaqua		*
291	St. Marys		*
297	Lake Harmony		*
383	Jeannette		*
437	Titusville		9.00
522	McKeesport		*
559	Stroudsburg		6.00
591	Harrisburg		6.00
596	Ellwood City		12.00
599	Oil City		*
604	Uniontown		*
616	Charleroi		27.90
627	Scranton		17.64
654	Kittanning		6.42
710	Connellsville		*
734	York		9.00
804	Spangler		9.45
806	Greensburg		14.28
811	Canonsburg		9.00
883	Monessen		12.00
992	Mahanoy City		6.00

International Alliance of Bill Posters, Billers and Distributors of the United States and Canada (Code 9)

3	Pittsburgh	$	7.83
4	Philadelphia		*
26	Harrisburg		12.00
39	Scranton		*
118	New Castle		1? 00
141	Reading		*

Figure B3. Example of Vote-Based Estimates of Union Membership: Pennsylvania AFL, 1933

United Mine Workers - District #7 (Continued)

1704	Michael Hartneady	Nesquehoning	5
1704	B. F. Davis	Nesquehoning	5
1719	Fred Fudge	Coaldale	1-1/2
1719	Chas. J. Gallagher	Coaldale	1-1/2
1719	Chas. J. Gallagher	Coaldale	1-1/2
1719	A. Skiveannis	Coaldale	1-1/2
1738	Thomas Kennedy	Hazleton	5

United Mine Workers - District #9

160	John Dougherty	Shamokin	1
200	Mart F. Brennan	Shamokin	4
1062	James E. Kelley	Wiconisco	5
1105	Owen Crossen	Morea	3
1550	John Mates	Williamstown	5
1550	Alex Hoffman	Williamstown	5
1577	Thomas Butler	Girardville	3
1629	Joseph Kershitsky	Park Place	5
2780	Frank J. Brennan	Minersville	1

Molders

335	William Young	Allentown	1

Musicians

60	Edward A. Wilharm	Pittsburgh	2-1/2
60	Wm. A. Greer	Pittsburgh	2-1/2
77	Adolph Hirschberg	Philadelphia	3
135	Ben R. Miller	Reading	2
135	George W. Snyder	Reading	2

Painters

411	John W. Corbett	Harrisburg	1
556	Joseph M. Richie	Philadelphia	1

Plumbers

42	James Maurer	Reading	1
428	Edward F. Dwyer	Norristown	1
520	F. E. Good	Harrisburg	1
670	R. J. Bader	Allentown	1

C Comparing Measures of Completed Fertility

In this section, I compare completed fertility estimates based on the Census' children ever born variable (CHBORN) to those based on the TCFR method, as described in Section 4.3.

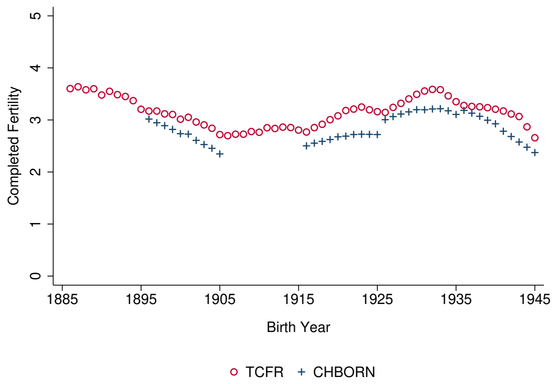
Appendix Figure C1 plots TCFR vs. CHBORN estimates in levels across all available birth years in various samples. Panel (a) includes all counties in the U.S., panel (b) includes only counties with population $\geq$ 500,000 in 1960, and panel (c) includes only counties for which % urban = 0 in 1960. CHBORN estimates are not available for cohorts born prior to 1896 because CHBORN was not recorded until the 1940 Census. And, CHBORN estimates are unavailable for cohorts born 1906-1915 due to issues with sample-line weighting in the preliminary release of the 1950 full count Census. In all samples, I weight estimates based on observation counts from the underlying microdata.

I highlight several features of these series. First, the series track each other closely in levels and in changes. Both clearly exhibit boom/bust dynamics, with completed fertility peaking for cohorts born in the early 1930s. Second, there is evidence of measurement error in the CHBORN series. In particular, the CHBORN series has a piecewise shape, and the gap between CHBORN and TCFR estimates tends to increase within decennial intervals (e.g., from 1896 to 1905). This is consistent with increasing measurement error in the CHBORN series as the age at measurement decreases, resulting in more missed births. E.g., for cohorts for whom CHBORN is measured at age 44 (1896, 1916, 1926, 1936) CHBORN estimates are nearly identical to TCFR estimates; however, for cohorts for whom CHBORN is measured at age 35 (1905, 1925, 1935, 1945), the difference between CHBORN and TCFR estimates tends to be large. Third, comparing results across samples highlights the important role of small sample sizes for CHBORN estimates. Gaps between estimates are relatively small in the sample of highly populated counties (where CHBORN cell sizes are less likely to be an issue), but much larger in the sample of rural counties (where CHBORN cell sized tend to be small). In particular, the series diverge the most in the rural sample for cohorts born 1896-1905, who were measured as sample-line respondents in the 1940 Census. Finally, TCFR estimates are consistently larger than CHBORN estimates – even when the two measures cover a similar range of childbearing years, such as for cohorts born in 1896, 1916, 1926, and 1936. This result is consistent with high fertility women facing higher mortality risk, or may be due to differential migration between high and low fertility women.

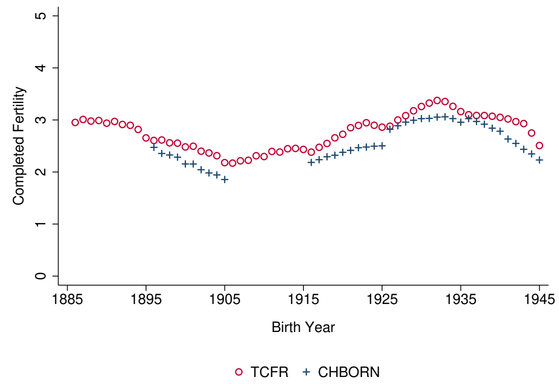
In Appendix Figure C2, I present average county-level correlations between TCFR and CHBORN estimates in each birth year, weighted by observation counts. Sample definitions are as in Appendix Figure C1. The series are highly correlated – the average county-level correlation in the national sample is typically between 0.5-0.8. For CHBORN, there are clear discontinuities which correspond to changes in average cell size across Censuses. In general, the correlations increase when CHBORN cells are less noisy: correlations are highest for cohorts measured in the 1960 25% sample (1916-1925), and lowest for cohorts measured in the 1940 5% sample line data (1896-1905) and 1980 16% sample (1936-1945). Additionally, within 10-decennial intervals (e.g., 1896-1905), correlations tend to decrease over time as the CHBORN age at measurement decreases. As expected, correlations tend to be very high in the sample of highly populated counties and lower in the sample of rural counties.

Appendix Figure C3 depicts the county-level distributions of each measure for selected birth year cohorts. Panels (a) and (b) show distributions for the 1896 birth cohort. This is the last pre-period cohort for which CHBORN and TCFR both measure the full range of childbearing years (CHBORN is unavailable in 1906, and the 1916 cohort experienced peak childbearing in the late 1930s, after the passage of the NLRA). Due to larger cell sizes, the TCFR distribution is much smoother and more normal, with less mass in the tails. There is also discrete bunching at round numbers in the CHBORN distribution, an artifact of small cell sizes. Medians are nearly identical across measures. Panels (c) and (d) show smoothed kernel densities for the 1937 cohort, which corresponds to the post-period cohort used in the main long-difference analysis. I pre-round and bin the underlying data and trim extreme observations to satisfy FSDRC disclosure requirements. These transformations mask some of the distributional differences, but overall the results suggest that the measures are more comparable for cohorts for whom CHBORN is measured in later Decennial Censuses (when CHBORN is not a sample-line variable).

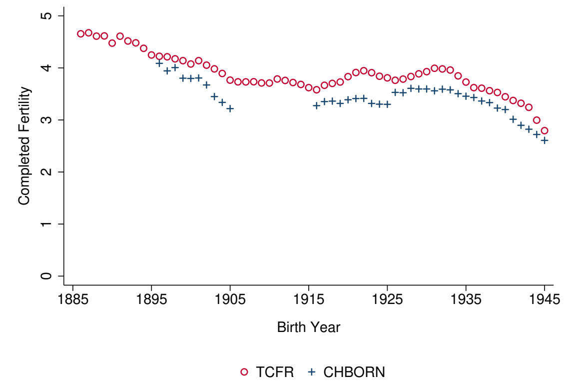
Figure C1. TCFR vs. CHBORN: Means by Birth Year Cohort



(a) National Sample



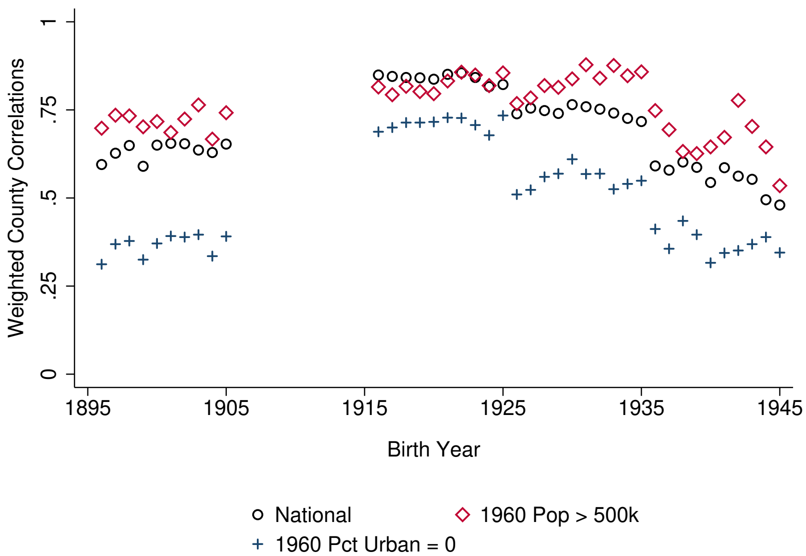
(b) 1960 Pop. $\geq$ 500,000



(c) 1960 Pct. Urban = 0

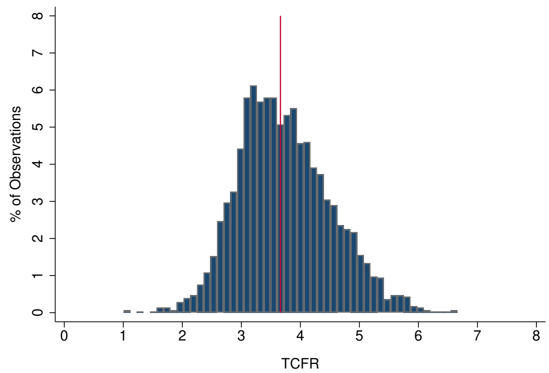
Notes: Each plot depicts the average value of each measure of completed fertility by birth year cohort. TCFR refers to "total cohort fertility rate", defined in Section 4.3; CHBORN refers to children ever born as recorded in the Decennial Censuses for women aged 35-44. CHBORN was first measured in the Decennial Census in 1940, and so is not available for cohorts born prior to 1896. In addition, CHBORN is not available for cohorts born 1906-1915 due to limitations of the preliminary release of the 1950 full count Decennial Census. The national sample includes all counties in the U.S. In Panels B and C, I apply additional sample restrictions as specified.

Figure C2. TCFR vs. CHBORN:
County-Level Correlations by Birth Year Cohort

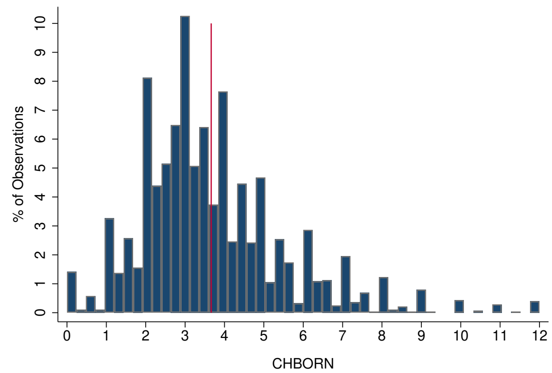


Notes: Each series depicts the (weighted) average county-level correlation between the TCFR and CHBORN measures for each birth year cohort for the specified sample. Estimates are weighted by the female population in each birth year cohort cell. TCFR refers to "total cohort fertility rate", defined in Section 4.3; CHBORN refers to children ever born as recorded in the Decennial Censuses for women aged 35-44. CHBORN was first measured in the Decennial Census in 1940, and so is not available for cohorts born prior to 1896. In addition, CHBORN is not available for cohorts born 1906-1915 due to limitations of the preliminary release of the 1950 full count Decennial Census. The national sample includes all counties in the U.S, and I apply additional restrictions to the national sample to estimate results for counties with 1960 population $\geq$ 500,000 and for counties with 1960 pct. urban = 0.

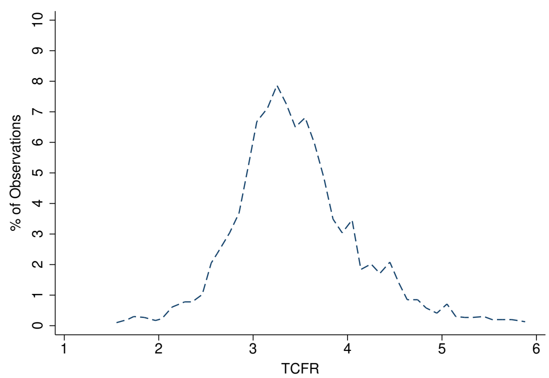
Figure C3. TCFR vs. CHBORN:
County-Level Distributions



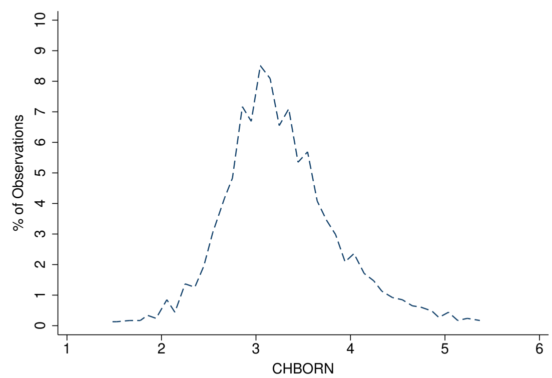
(a) TCFR, 1896 Cohort



(b) CHBORN, 1896 Cohort



(c) TCFR, 1937 Cohort



(d) CHBORN, 1937 Cohort

Notes: Each plot depicts the distribution of county-level values for each measure in the specified birth year. TCFR refers to "total cohort fertility rate", defined in Section 4.3; CHBORN refers to children ever born as recorded in the Decennial Censuses. Red lines identify the median value. The underlying data for the 1937 cohort comes from restricted-access FSDRC data, so I pre-round and smooth the underlying data, trim extreme observations, and do not report medians to comply with disclosure avoidance rules. The sample includes all counties in the U.S.

D Shift Share Instrument Construction

In this section, I describe the data and sources used to construct the shift-share instrument introduced in Section 5.1.

Shares

I estimate the shares using microdata from the full count 1910 Decennial Census (Ruggles et al., 2024). I first restrict to a sample of employed people aged 14 and older who do not reside in group quarters. I attribute and industry group to each worker based on the IPUMS variable *IND1950* (see Table 11). I drop any worker for whom industry is missing, and workers employed in non-classifiable industry groups (*IND1950* codes 976-999). I collapse to the county level, and define the share of employment in each industry j in each county i as:

$$\text{IndShare}_{ij} = \frac{\text{Employment}_{ij}}{\sum_{j=1}^{N} \text{Employment}_{ij}} \tag{7}$$

Industry-Level Union Membership

I construct the numerators for national union membership rates at the industry-level using data from Wolman (1936) and Troy (1965).

I extract data from Wolman (1936)'s Appendix Table I, which provides union membership estimates for each union, by year (1897-1934) and industry group. Wolman (1936)'s industry groups generally follow my mappings in Table 11, with several exceptions:

- Wolman (1936) groups all Mining unions together, while I split out Mining: Coal/Metal/Quarrying from Mining: Crude Petroleum/Natural Gas Extraction. Accordingly, I attribute the Oil Field Gas Well and Refinery Workers to the latter group, and all other listed mining unions to the former group.

- Wolman (1936) groups the Railway Carmen union with Manufacturing: Metals, Machinery, and Equipment; I group the Railway Carmen with Transportation

- Wolman (1936) groups all Transportation, Communications, and Utilities unions together, while I split each group out separately. Therefore, I attribute the American Radio Telegraphists Association, the Commercial Telegraphers, and Railroad Telegraphers with the Communications group, and I attribute the Utility Workers to the Utilities group.

- There are several unions that Wolman (1936) classifies as "Miscellaneous" but I classify in a specified industry group: the Bill Posters (Services), the Broom and Whisk Makers (Manufacturing: Misc), the Brushmakers (Manufacturing: Misc), the Operating Engineers (Construction), and the Rubber Workers (Manufacturing: Chemical, Rubber, Plastic).

Since Wolman (1936)'s figures represent membership counts for locals in both the U.S. and Canada, I incorporate other data to estimate total U.S. membership for each union. In particular, Table V of Wolman (1924) and Table IV of Wolman (1936) provide union-level estimates of membership by country in

1920 and 1930, respectively. I linearly interpolate the American proportion of total membership for each union from 1921-1929 and 1931-1934. Finally, I multiply total membership by the American proportion to generate estimates of domestic membership levels for each union in each year, and collapse to the industry-year level.

I also extract data from Troy (1965)'s Tables A-1, A-2, and A3, which provide union membership estimates for each union by year (1935-1962) for AFL, CIO, and independent unions, respectively.[87] Troy (1965) does not group unions by industry, so I manually map each union to its industry group using the following algorithm:

- If the union (or a descendent of the union) exists in Wolman (1936)'s Appendix Table I, attribute based on Wolman (1936)'s groupings
- If the union does not exist in Wolman (1936)'s Appendix Table I, but the union name refers to an occupation listed in Wolman (1936)'s Appendix Table III (which disaggregates employment by industry group and occupation), attribute based on the groupings in Appendix Table III
- Otherwise, infer the industry group by looking up the union in online and printed references

Troy (1965)'s figures also include both U.S. and Canadian membership. Therefore, I draw on data from Troy and Sheflin (1985) Appendix B, which presents membership counts for the 60 largest unions (as of their writing) by country in 1962.[88] I map each union to an industry group, and estimate the proportion of total membership accounted for by U.S. locals at the industry group level. I multiply total membership by the American proportion to generate estimates of domestic membership levels for each union in each year (1956-1961), and collapse to the industry-year level.

Note that many unions span multiple lower level industries. For example, the United Automobile, Aircraft, and Agricultural Implement Workers (UAW) represent workers in the Agricultural Machinery and Tractors (IND1950=356), Motor Vehicles and Motor Vehicle Equipment (IND1950=376), and Aircraft and Parts (IND1950=377) industries, among others. For this reason, it is not feasible to disaggregate union membership into finer groups (e.g., 3-digit IND1950 codes) using source data at the union-level. In general, the level of feasible union:industry mappings determines the number and level of industries coded in the shift-share instrument.

Industry-Level Employment

I construct national employment for each industry group using data from several sources. BLS does not provide a consistent series of employment estimates below the major industry level (e.g., "Manufacturing") until 1939. Therefore, for pre-period data (1920-1925, 1932-1934), I use the 1910[89], 1930, and 1940 Decennial Censuses and the IND1950 variable to estimate national employment for each industry group (see Table 11) in each year, and linearly interpolate values in intercensal years. Starting in 1939, I extract

[87] The AFL and CIO merged in 1955, but Troy (1965) continues to group unions based on previous AFL and CIO affiliation after 1955.

[88] Unfortunately, I am not aware of any other source that disaggregates union- or industry-level membership by country for the period covered by Troy (1965)'s data.

[89] The 1920 Census does not record employment status. However, since union membership was near zero in most industries before 1930, union membership rates will also tend to be near zero in these early years, regardless of the denominator.

data on employment by industry group from various sections of the Bureau of Labor Statistics (1964)'s Earnings and Employment Statistics:

- Data for Trade, Finance/Insurance/Real Estate, Services, and Government are from Table 1
- Data for Construction and all Manufacturing groups are from Table 2
- Data for Mining groups and Transportation, Communications, and Utilities are from Section 1

For all these industry groups, I rely on SIC 1957 codes for mappings, and employment counts capture all employed wage and salary workers aged 14 and older. I construct agricultural employment from other BLS sources:

- Data for 1929-1960 are from Table A-1 of the January 1961 Employment and Earnings report
- For 1961, I construct an annual average based on monthly employment recorded in the March 1961–February 1962 Employment and Earnings reports[90]

Agricultural employment captures all employed workers aged 14 and older, which includes workers classified as "self-employed" in agriculture.

[90] BLS monthly estimates can be adjusted over time. I use estimates from month $t+2$ as the final estimate for employment in month t. E.g., I use the December 1961 monthly estimate as reported in the February 1962 report.

E Empirical Diagnostics and Tests of the Shift-Share Instrument

Goldsmith-Pinkham et al. (2020) show that shift-share instruments can be interpreted as over-identified GMM estimators that combine a set of individual, just-identified instruments – the local shares – under a weighting matrix. The shift-share IV estimator ($\beta^{IV} = \beta_{2SLS}$ from Section 5) can then be decomposed into a vector of estimators corresponding to each share j (β_j^{IV}), and a set of "Rotemberg" weights (α_j) that determine how the industry-level estimators are aggregated:

$$\beta^{IV} = \sum_{j=1}^{N} \alpha_j \times \beta_j^{IV} \tag{8}$$

Goldsmith-Pinkham et al. (2020) note that while the α_j's must sum to 1, any individual α_j may be negative. In cases where a large share of the Rotemberg weights are negative and treatment effects are heterogeneous, β^{IV} does not have a LATE-like interpretation as a weighted average of treatment effects.

In this section, I decompose the shift-share instrument, as proposed by Goldsmith-Pinkham et al. (2020), to shed light on the identifying variation underlying the IV estimates. In particular, I estimate and summarize the Rotemberg weights at the industry-level, test key identifying assumptions for the just-identified instruments with the largest Rotemberg weights, and analyze treatment effect heterogeneity.

E.1 Summary of Rotemberg Weights

I summarize the Rotemberg weights in Appendix Table E1. Panel A provides descriptive statistics on positive and negative weights. Only 7.6% of total weights are negative, so β^{IV} permits a LATE-like interpretation. In Panel B, I present correlations of industry-level aggregates. The correlation between Rotemberg weights ($\hat{\alpha}_j$'s) and national industry-level growth rates (g_j's) = 0.5, which implies that the variation in the shocks explains about 25% ($0.5^2 = 0.25$) of the variation in the Rotemberg weights. This shows that the identifying variation in the overall shift-share instrument is driven primarily by the plausibly exogenous predetermined shares and not the potentially endogenous shocks. I identify the top 5 industries by Rotemberg weights in Panel C. The distribution of Rotemberg weights across industries is highly skewed: the top 5 industries – (1) Manufacturing: Metals, Machinery, Equipment; (2) Transportation; (3) Construction; (4) Manufacturing: Stone, Clay, Glass; (5) Services – account for 87.4% of all positive weights. There are only 21 (relatively coarse) industry groups indexed by the shift-share instrument, so it is not surprising that a handful of industries account for most of the variation. In particular, the Manufacturing: Metals, Machinery, Equipment industry group accounts for nearly 50% of positive Rotemberg weights. The outsized weight of this industry group underscores the importance of the unionization of previously-unorganized industrial workers by the CIO, especially in the automotive and war production sectors, and especially in the states included in the main analysis sample. There is considerable heterogeneity across $\hat{\beta}_j^{IV}$'s among the top industries, which I explore in more detail in Appendix Figure E2. Finally, I decompose $\hat{\beta}^{IV}$ by positively- and negatively-weighted industries in Panel D. Industries with positive Rotemberg weights contribute virtually all (99.7%) of the identifying variation. Again, the results suggest considerable heterogeneity across just-identified instruments: the unweighted mean of $\hat{\beta}_j^{IV}$'s among positively-weighted industries is actually negative, though the weighted sum of positively-weighted $\hat{\beta}_j^{IV}$'s is positive.

E.2 Exogeneity of Top 5 Industry Shares

Goldsmith-Pinkham et al. (2020) show that Rotemberg weights can be interpreted as "sensitivity-to-misspecification" parameters: intuitively, the over-identified estimate of β^{IV} is more sensitive to misspecification (i.e., endogeneity) in just-identified instruments with larger weights. Therefore, it is most important to assess the plausibility of key identifying assumptions for individual industries with the largest Rotemberg weights. If high-weight instruments pass basic specification tests, the overall empirical design is also likely to be valid.

For each of the top five industries by Rotemberg weight, I estimate correlations between 1910 industry shares and observable county-level characteristics at baseline using the following cross-sectional regression:

$$\text{IndShare}_{i,j} = \alpha_0 + \mathbf{X}_i'\Pi + \mu_s + \varepsilon_i \tag{9}$$

where i indexes counties and j indexes industry groups. $\text{IndShare}_{i,j}$ is the 1910 share of employment in county i in top five industry j, and thus corresponds to just-identified instrument j. $\mathbf{X}$ is a vector of county-level characteristics, including: 1920 % White, 1920 % Male, 1920 % Foreign-Born, 1920 % Urban, 1920 % Aged 15-44, and % change in retail sales during the Great Depression (1929-1933). I include state fixed effects (μ_s), and otherwise follow the baseline specification as described in Section 5. For the aggregate shift-share instrument, I use SSIV_{it} from Equation 2 instead of fixed industry shares. I present the results in Appendix Table E2. Each point estimate in Π is interpretable as the percentage point increase in the specified instrument associated with a 1pp increase in the characteristic x. Most estimates are either statistically insignificant, or are small enough in magnitude to be economically insignificant. A notable exception is the local share employed in services at baseline, which has a strong negative correlation with the 1920 Pct. Male and a strong positive correlation with the 1920 Pct. Aged 15-44. I control for time-varying measures of each county-level characteristic in Appendix Table E2 when estimating the main results.

While the baseline balance tests in Appendix Table E2 are descriptively useful, the presence of level differences across units does not confound identification in a difference-in-differences design. The primary threats to identification in this setting are unobserved factors that vary within counties over time. Therefore, for each of the top five industries by Rotemberg weight, I test the key identifying assumption by estimating reduced-form event studies using the 1910 industry share as a just-identified instrument. In each case, the outcome is the TCFR, and the event study specification follows that of Section 7.2: I bin counties into treatment groups by quintiles of the specified instrument, and use Q3 as the reference group. I plot the results in Appendix Figure E1. For the top four industries – which together account for over 80% of all positive Rotemberg weights – there is no evidence that just-identified instruments predict changes in outcomes prior to treatment. There are minor pre-trends for the share of employment in services, but this instrument is relatively low-weight compared to the others ($\hat{\alpha}_j = 0.063$).

E.3 Treatment Effect Heterogeneity

Using the insight that the aggregate shift-share instrument is just one way of combining many just-identified instruments, I re-estimate the first-stage and reduced-form equations with an over-identified model that includes each of the top five industry shares by Rotemberg weights (interacted with a post period indicator) as separate instruments. I report the results with TCFR as the outcome in Appendix Table E3. Since the over-identified model uses variation from only five industries the first-stage is somewhat weaker than the baseline model, but still well above conventional thresholds (F=68.6). The point estimate from the over-identified model is nearly identical to, but slightly noisier than, the corresponding estimate from the baseline model. I report Hansen's J-statistic for the test of over-identification, and reject the null at the 5% level. Rejecting the null in this context has two possible interpretations: (1) the instruments are not exogenous, or (2) there is treatment effect heterogeneity, such that the null of constant effects is unreasonable (Goldsmith-Pinkham et al., 2020). Given that the reduced-form event studies in Appendix Figure E1 provide support for key identifying assumptions, the former interpretation seems unlikely. On the other hand, there is evidence for treatment effect heterogeneity; for example, in Panels C and D of Appendix Table E1. To comprehensively depict heterogeneity across the $\hat{\beta}_j^{IV}$'s underlying the aggregate shift-share instrument, I plot each $\hat{\beta}_j^{IV}$ and its corresponding first-stage F-statistic in panel A of Appendix Figure E2.[91] The size of each point is scaled by the magnitude of its Rotemberg weight (so that industries with larger weights are more prominent), and I separately identify positive and negative weights.[92] The horizontal dashed line is plotted at the value of $\hat{\beta}^{IV}$. While the industries with the two largest Rotemberg weights are close to the estimate of $\hat{\beta}^{IV}$, there is considerable dispersion among the other highly-weighted industries.

[91]I only plot industry shares for which F $\geq$ 1.

[92]I plot first-stage F-statistics vs Rotemberg weights in panel B of Appendix Figure E2.

Table E1. Summary of Rotemberg Weights

Panel A: Negative and Positive Weights

	Sum	Mean	Share
Negative	-0.090	-0.015	0.076
Positive	1.090	0.073	0.924

Panel B: Correlations

	$\hat{\alpha}_j$	g_j	$\hat{\beta}_j$	$\hat{F}_j$	Var(z_j)
$\hat{\alpha}_j$	1				
g_j	0.500	1			
$\hat{\beta}_j$	0.034	-0.036	1		
$\hat{F}_j$	0.373	0.176	0.184	1	
Var(z_j)	0.166	-0.127	-0.142	0.608	1

Panel C: Top 5 Rotemberg Weight Industries

	$\hat{\alpha}_j$	g_j	$\hat{\beta}_j$	95% CI	Ind Share
Mfg: Metals Machinery Equipment	0.476	0.335	0.005	(-0.009, 0.019)	3.318
Transportation	0.228	0.667	0.012	(-0.500, 1.500)	3.905
Construction	0.115	0.682	0.026	(-0.002, 0.099)	3.531
Mfg: Stone Clay Glass	0.071	0.243	-0.020	(-0.124, 0.001)	0.512
Services	0.063	0.154	0.033	(0.011, 0.196)	8.383

Panel D: Estimates of $\hat{\beta}_j$ for Negative and Positive Weights

	α-Weighted Sum	Share of Overall β	Mean
Negative	0.000	0.003	0.012
Positive	0.007	0.997	-0.002

Notes: This table reports summary statistics about the Rotemberg weights (Goldsmith-Pinkham et al., 2020). Panel A reports the share and sum of positive and negative weights. Panel B reports industry-level correlations between the weights ($\hat{\alpha}_k$), the national growth components (g_k), the justi-identified coefficient estimates ($\hat{\beta}_k$), the first-stage F-statistic of the industry share ($\hat{F}_k$), and the variation in the industry shares across locations (Var(z_k)). Panel C reports the top five industries, ranked by magnitude of Rotemberg weight. g_k is the national industry growth rate, $\hat{\beta}_k$ is the coefficient from the just-identified regression, the 95% confidence interval is the weak instrument robust confidence interval using the method from Chernozhukov and Hansen (2008), and Ind Share is the percent of total employment comprised by employed in the specified industry in 1910. Panel D reports statistics about how the values of $\hat{\beta}_k$ vary with the positive and negative weights.

Table E2. Baseline Balance: 1910 Industry Shares and County-Level Characteristics

	(1) Mfg: Metals Machinery Equipment	(2) Transportation	(3) Construction	(4) Services	(5) Mfg: Stone Clay Glass	(6) Aggregate SSIV
1920 Pct. White	0.217	0.083	0.000	-0.298*	0.032	0.154
	(0.179)	(0.134)	(0.051)	(0.156)	(0.061)	(0.131)
1920 Pct. Male	0.832	0.389**	-0.449***	-1.610***	0.027	0.221
	(0.595)	(0.164)	(0.122)	(0.322)	(0.103)	(0.268)
1920 Pct. Foreign-Born	-0.078	-0.127*	-0.012	0.059	0.045	-0.085
	(0.120)	(0.067)	(0.040)	(0.077)	(0.032)	(0.083)
1920 Pct. Urban	0.085	0.096***	0.006	-0.010	-0.003	0.114***
	(0.067)	(0.015)	(0.018)	(0.031)	(0.008)	(0.030)
1920 Pct. Aged 15-44	0.328	0.114	0.301***	0.626***	-0.001	0.389**
	(0.484)	(0.121)	(0.120)	(0.212)	(0.089)	(0.196)
1929-1933 Δ Ret. Sales \Capita	-0.139***	0.033	0.008	0.050***	-0.006	0.006
	(0.052)	(0.021)	(0.010)	(0.017)	(0.013)	(0.032)
N	400	400	400	400	400	400

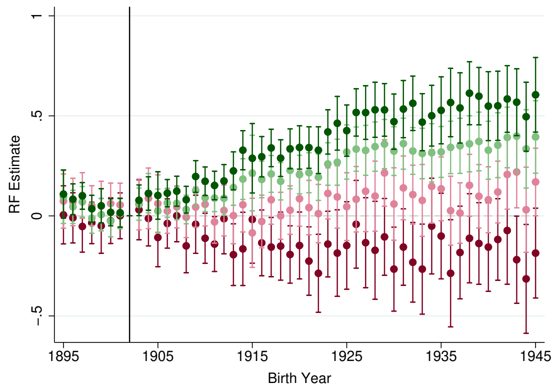
Notes: Each column reports results of a single regression of the specified 1910 industry share on the specified county-level characteristics. In the final column I regress the aggregate shift-share instrument (using growth rates from 1934-1960) on county-level characteristics. In each regression, I weight by the female population and estimate HAC-corrected standard errors (in parentheses) based on clusters defined by 100km radii around county centroids (Colella et al., 2019). The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). Sample sizes are rounded to comply with FSRDC disclosure avoidance rules.

Table E3. TCFR LD: Test of the Over-Identified Model County-Level

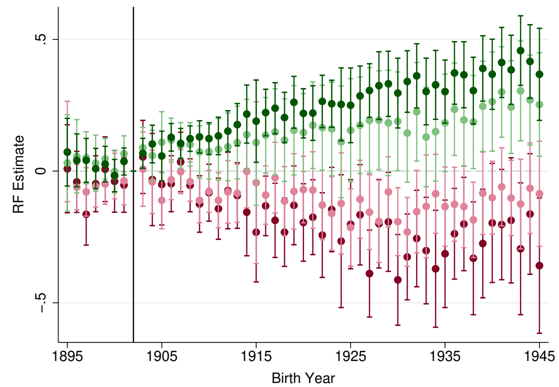
	Dependent variable: TCFR	
	(1)	(2)
	Aggregate SSIV	OverID: Top 5 Ind
Union Exposure	0.008**	0.009*
	(0.004)	(0.005)
	[0.034]	[0.086]
F	134.2	68.6
Hansen's J stat p-value		0.038
Fixed Effects?	X	X
Controls?	X	X
Base Dep. Var. Mean	2.809	2.809
N	800	800
N (counties)	400	400

Notes: Each column reports estimates from separate county-birth year cohort-industry level regressions of TCFRs on union exposure. The long-difference measures changes in outcomes that result from changes in union exposure between the cohorts born in 1901 (pre-NLRA) and 1937 (post-NLRA). The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). In column (1) I replicate the results from the baseline model using the aggregate shift-share instrument (see Table 7, Panel B, column 2). In column (2) I include 1910 shares of the Top 5 industries by Rotemberg weight (see Appendix Table E1), interacted with a post period indicator, as separate just-identified instruments instead of the aggregate SSIV and estimate results for the over-identified model. "Fixed Effects" include county, birth year, and state × birth year fixed effects. "Controls" include the following time-varying measures: log(population), the share of the labor force in manufacturing, pct white, pct male, pct urban, pct aged 15-44, pct foreign-born, the change in retail sales from 1929-1933, New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, WWII casualty rates per capita, and pct of households with a modern washing machine. I weight by female population in each county-birth year cell. I estimate HAC-corrected standard errors based on clusters defined by 100km radii around county centroids (Colella et al., 2019). Standard errors are in parentheses, p-values are in brackets. I report the Kleibergen-Paap rk Wald (first-stage) F statistic. For the test of the over-identified model in column (2), I report the p-value associated with Hansen's J-statistic (Hansen, 1982). Sample sizes are rounded to comply with FSRDC disclosure avoidance rules.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

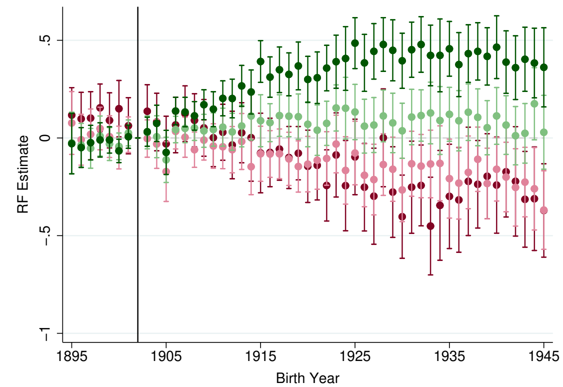
Figure E1. TCFRs: Reduced-Form Event Studies
High Rotemberg Weight Industries



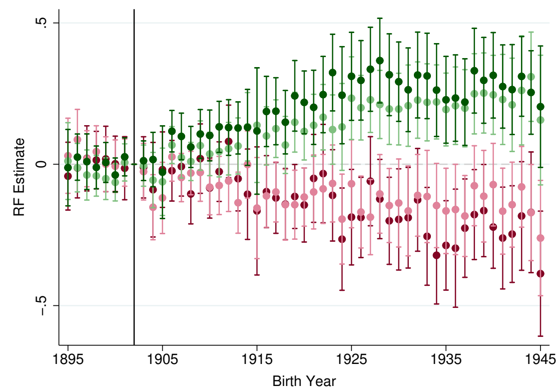
(a) Mfg: Metals Machinery Equipment



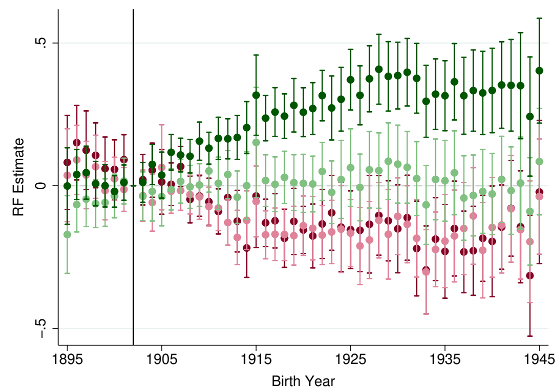
(b) Transportation



(c) Construction



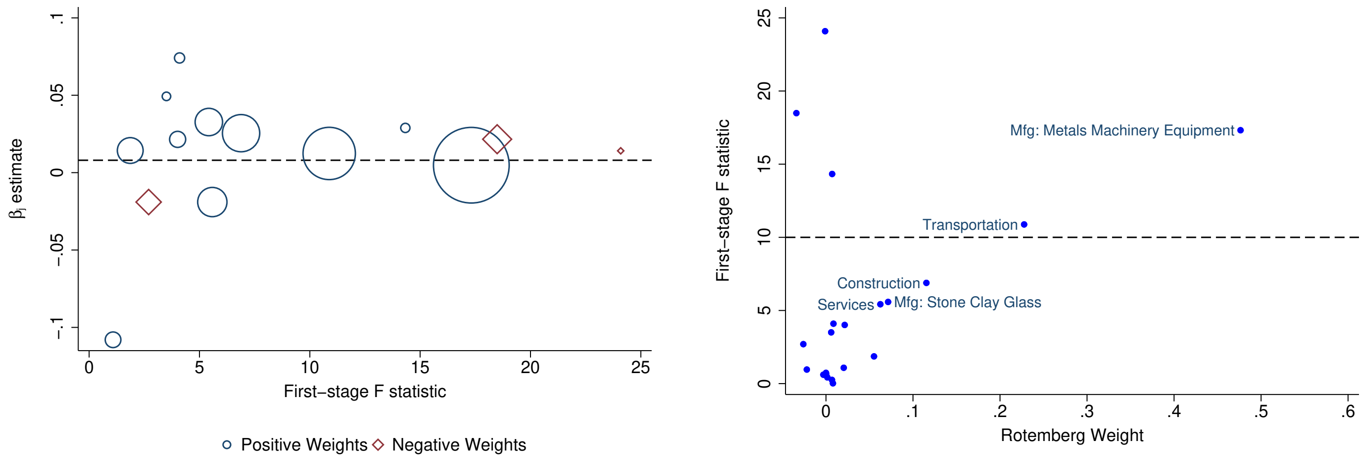
(d) Mfg: Stone Clay Glass



(e) Services

Notes: I plot point estimates and 95% confidence intervals from the reduced-form event study model with TCFRs as the outcome. The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). I weight by the female population in each county-birth year cell. In each panel, I bin counties in quintiles according to 1910 employment shares for the specified industry, where Q1 = the first (i.e., lowest) quintile (dark red), Q2 = the second quintile (light red), Q4 = the fourth quintile (light green), Q5 = the fifth (i.e., highest) quintile, and Q3 serves as the reference group. The specification includes county, year, and state × year fixed effects. The base year is 1902.

Figure E2. Heterogeneity by Rotemberg Weights



(a) Heterogeneity of $\hat{\beta}_j$

(b) Heterogeneity of First Stage

Notes: Panel A plots the relationship between each industry share instrument's $\hat{\beta}_j$, first-stage F statistic, and Rotemberg weight. Each point represents a separate just-identified instrument, corresponding to the 1910 share of employment in industry j. The size of the points are scaled by the magnitude of Rotemberg weights, with circles denoting positive weights and the diamonds denoting negative weights. The horizontal dashed line is plotted at the value of $\beta^{IV} = \beta_{2SLS}$, the treatment effect estimate when using the aggregate shift-share IV. I exclude instruments for which F $<$ 1 for legibility. Panel B plots each instrument's Rotemberg weights against the first-stage F statistic. The labelled industries correspond to the five industries with the highest Rotemberg weights. The dashed horizontal line is plotted at F $=$ 10.

F Effects of Unionization on Age at First Birth

In this section I provide supplementary results on the effect of unionization on the average age of mothers at first birth.

I estimate average age at first birth at the county-level using microdata from the 1920-1970 U.S. Decennial Censuses. I use the 1920-1950 full count Censuses, available from Ruggles et al. (2024), and long form versions of the 1960-1970 Censuses from the FSRDC internal-use files. In each Census, I infer mothers' age at first birth using own-child attribution, similar to the TCFR process outlined in Section 4.3. Specifically, I:

1. Restrict to a sample of women who do not reside in group quarters and are identified as either the household head or the spouse of the household head;
2. Use information on within-household relationships to attribute children to mothers;
3. Estimate age at first birth for each mother by subtracting the age of the eldest attributed child from the age of the mother (at time of measurement);[93]
4. Collapse to construct county-level averages.

Note that this method only captures children who are (1) already born and (2) live in the same household as their mother. As a result, the quality of the measure varies across cohorts in each Census. i.e., Women who are older at the time of measurement are more likely to have children who have aged out of the household, leading to overestimates of age at first birth. On the other hand, inclusion in the sample is conditional on having had at least one birth, so younger cohorts (who have experienced relatively few childbearing years by the time of measurement) may be differentially selected compared to older cohorts. To balance these tradeoffs, I further restrict to a sample of 33-year-old women in each Census, who are far enough along in childbearing years to have experienced a first birth, but are young enough that eldest children are likely to still reside in the same household.[94] This yields seven sets of birth year cohorts (1887, 1897, ..., 1937) corresponding to the seven Decennial Censuses used to construct the sample (1920, 1930, ..., 1970).

I depict changes over time in the average age at first birth for a national sample of counties in Appendix Figure F1. As in Section 6.2, I construct treatment group quintiles based on changes in the SSIV-predicted union membership rate from 1934-1960, and estimate group-level averages for each birth year cohort. Across all birth year cohorts, the basic pattern is that higher treatment areas tend to have higher average ages of mothers at first birth. After remaining stable across all groups for the early cohorts (born 1887-1907), there is a secular increase in age at first birth for the cohort born in 1917. Since the 1917 cohort was measured in 1950, this increase likely reflects births in the late 1940s that were postponed

[93] I also drop a small number of cases for which the eldest attributed child is older than the mother. Such cases may result from mis-transcription in the source data, or because of mis-attribution of children to mothers (e.g., step-children, who are not actually own-children).

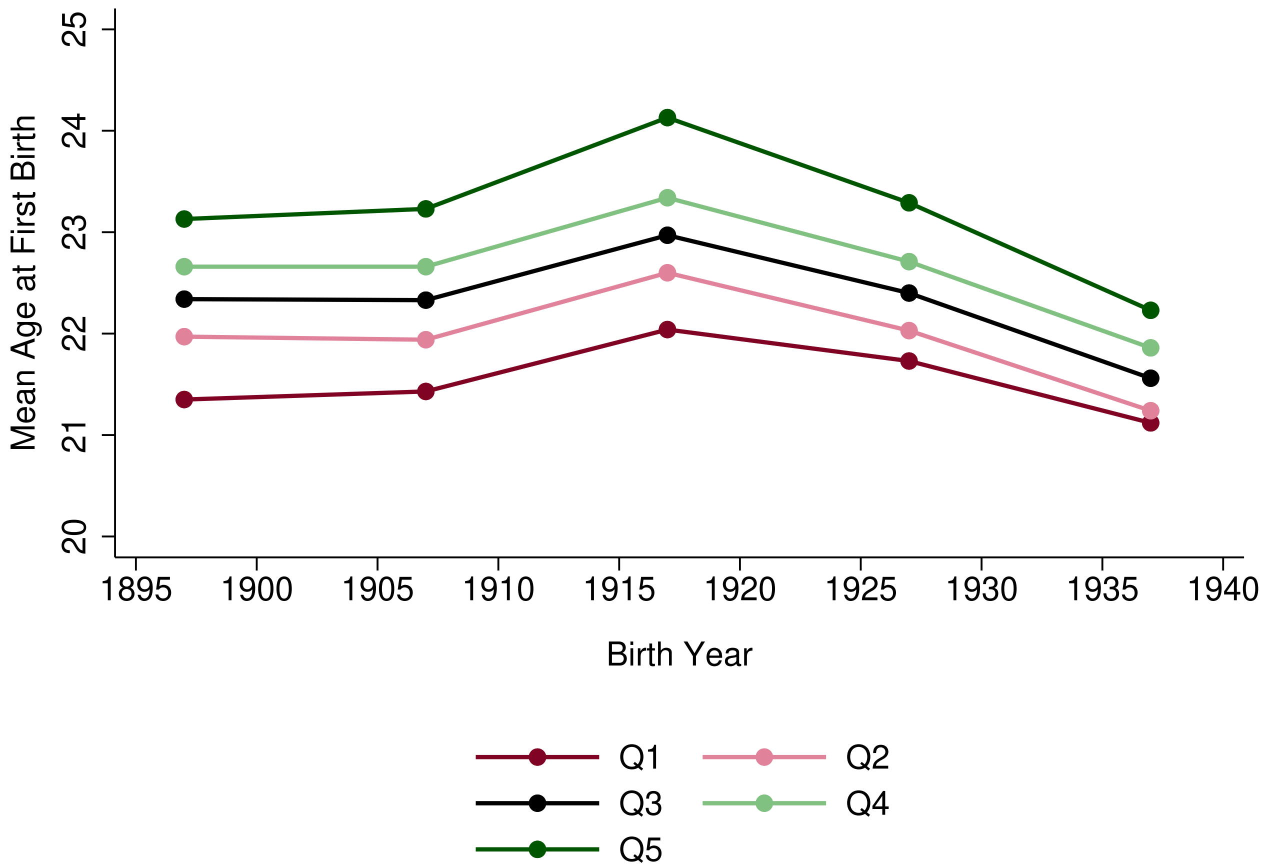
[94] From the 1940 Census, I estimate that 1.55% of first births occur to 33-year-old mothers, while 1.32% of first births occur to 15-year-old mothers. This indicates that 33 is a good measurement age; e.g., increasing the age to 34 would likely result in more missed births of older children who leave the household than new first births of older women, while decreasing the age to 32 would likely result in more missed births of older woman than newly captured births of eldest children.

during WWII. There is some convergence across groups as average age at first birth decreases for the later cohorts (born 1927 and 1937).

To measure the causal effects of unionization on average age at first birth, I re-estimate the IV long-difference model from Sections 7.1 and 6.1. In this case, the county-cohort long-difference measures the effect of changes in union exposure on changes in the average age at first birth for cohorts born in 1907 and 1937.[95] I follow the baseline specification and include all controls and fixed effects. I present OLS and IV results for the main analysis sample in Appendix Table F1. I do not find evidence that unionization impacted the average age at first birth. OLS and IV point estimates are negative but are not statistically different from zero. However, the estimates are sufficiently precise to rule out large treatment effects: based on the IV estimates, I can reject (at the 5% level) decreases of larger than 0.3 years and increases of larger than 0.25 years from a 10pp increase in union exposure. These results suggest that unionization's impact on average completed fertility was driven primarily by effects on the birth parity margin instead of changes in the average age at which women began childbearing.

[95] Note that this does not exactly match the long-difference model from Section 7.1, which compares outcomes for cohorts born in 1901 and 1937. I cannot use the cohort born in 1901 for this analysis, due to the measurement issues discussed above (they are not aged 33 in any Decennial Census). One concern with using the 1907 cohort as the pre-period cohort is that women born in 1907 were aged 28 when the NLRA was passed, and so women who had first births after age 28 are plausibly treated. However most first births happen by age 28: from the 1940 Census data, I estimate that 87.33% of first births for women aged 15-33 were from women aged 15-28. There is also a temporal mismatch between peak childbearing years for the 1907 cohort and the measurement of union exposure. I can only construct union exposure using membership rates from 1920-1925, when the 1907 cohort was aged 13-18. Though union density was fairly stable throughout the pre-period in most areas, it is possible that fluctuations between 1920-1925 and 1926-1931 (when the 1907 cohort experienced peak childbearing years) could cause union exposure to be under- or over-stated.

Figure F1. Age at First Birth: Means by Birth Year and Treatment Quintile



Notes: The sample includes all counties in the 48 contiguous states. I bin counties in quintiles according to SSIV-predicted changes in union membership rates between 1934-1960, where Q1 = the lowest treatment group and Q5 = the highest treatment group. Group-level averages are weighted by the female population in each birth year.

Table F1. Age at First Birth LD: Main Effects
County-Level

	Dependent variable: Mean Age at First Birth	
	(1)	(2)
	OLS	IV/2SLS
Union Membership Rate	-0.004	-0.003
	(0.005)	(0.014)
	[0.371]	[0.854]
F		123.9
Fixed Effects?	X	X
Controls?	X	X
Base Dep. Var. Mean	23.04	23.04
N	800	800
N (counties)	400	400

Notes: Each column reports estimates from separate regressions of mean age at first birth on union membership rates. The long-difference measures changes in outcomes measured in 1940 and 1970 that result from changes in union exposure between the cohorts born in 1907 (pre-NLRA) and 1937 (post-NLRA). The sample includes all counties in the main analysis sample of states (CA, IL, MO, PA, WI). "Fixed Effects" include county, birth year, and state × birth year fixed effects. "Controls" include the following time-varying measures: log(population), the share of the labor force in manufacturing, pct white, pct male, pct urban, pct aged 15-44, pct foreign-born, the change in retail sales from 1929-1933, New Deal relief spending per capita, WWII defense contract spending per capita, WWII draft registration rates per capita, WWII casualty rates per capita, and pct of households with a modern washing machine. I weight by female population in each county-birth year cell. I estimate HAC-corrected standard errors based on clusters defined by 100km radii around county centroids (Colella et al., 2019). Standard errors are in parentheses, p-values are in brackets. I report the Kleibergen-Paap rk Wald (first-stage) F statistic. Sample sizes are rounded to comply with FSRDC disclosure avoidance rules.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

G Supplementary Data

County-level estimates of population, the share of the labor force in manufacturing, percent white, percent male, percent urban, percent aged 15-44, percent foreign-born, the female share of the labor force, the percent of the labor force that is unemployed, and median family income are based on microdata from the U.S. Decennial Censuses. I use full count Censuses files, available from Ruggles et al. (2024), from 1910-1950. Counties are not identified in public use files after 1950, so I construct control variables from the long form version of the 1960 Census, available from the Federal Statistical Research Data Centers (FSRDC) internal-use files. I use the civilian labor force (i.e., exclusive of individuals serving in the armed forces) for all variables that rely on estimates of the labor force. Since employment is not observed in the 1920 Census, estimates of unemployment rates are only available in 1910 and from 1930 onward. Personal income was first observed in the 1940 Census, and family income was first observed in the 1950 Census. To construct an estimate of median family income in 1940, I restrict to a sample of individuals aged 14 and older, who do not reside in group quarters, and for whom personal income is non-missing. I sum personal income across individuals at the household-level, estimate county-level medians, and convert to 1960 USD.

County-level data on the shares of low- and mid-income households are based on microdata from the 1940 and 1960 Decennial Censuses. In 1960, county-level identifiers are not available in the public use versions of the Census, so I draw on county-level tabulations reported in the Census's City and County Data Books. Specifically, I use measures of the percent of households with earnings less than \$3,000 and the percent of households with earnings exceeding \$10,000 (1960 USD). These thresholds roughly correspond to the 20$^{\text{th}}$ and 85$^{\text{th}}$ percentiles of the national distribution in 1960.[96] I construct comparable thresholds from microdata of the 1940 full count Decennial Census by classifying households earning less than \$1,000 (1960 USD) as low-income, and households earning more than \$5,000 (1960 USD) as high-income. Such households made up 20.1% and 17.6% of the national income distribution in 1940, respectively. Finally, I define mid-income households to be those households that are above the low-income threshold but below the high-income threshold in each year.

I construct an index of skill-specific changes in local labor demand using microdata from the U.S. Decennial Censuses.[97] Following Goldin and Margo (1992), this index seeks to account for the fact that for a given national growth rate in industry j, local demand for skilled versus unskilled labor will change as a function of: (1) variation in the skill mix across industries, and (2) variation in within-industry skill mixes across areas. Formally, the local demand for skilled labor in county i in year t is given by:

$$D_{it}^{s} = \sum_{j} \left(\frac{E_{jt}}{E_t}\right)\left(\frac{E_{ij,1940}^{s}}{E_{ij,1940}}\right)\left(\frac{E_{ij,1940}}{E_{j,1940}}\right) \tag{10}$$

where the first term captures the national employment share of each industry j in year t; the second term captures the skill mix in each industry j and county i, fixed according to 1940 levels; and the third term captures the within-industry skill mix in each industry j, fixed according to 1940 levels. To derive an

[96] 21.6% of households had income < \$3,000 and 14.9% of households had income > \$10,000 in 1960.
[97] My discussion of this labor demand index draws heavily from Collins and Niemesh (2019).

index of local relative skill demand in each year t and county i, I compute the ratio of demand for skilled (s) versus unskilled (u) labor as:

$$D_{it}^{rel} = \frac{D_{it}^{s}}{D_{it}^{u}} \tag{11}$$

County-level data on the change in retail sales during the Great Depression (1929-1933) and New Deal spending per capita are from Fishback and Kantor's New Deal ICPSR files. To estimate New Deal spending per capita, I sum total grants provided through New Deal economic relief programs (FERA, CWA, WPA, and Public Assistance) at the county-level, and divide by the county population from the 1930 Census.

County-level data on spending from World War II defense contracts are from Brunet (2024). I am grateful to Gillian Brunet for sharing these data. To calculate war spending per capita, I convert spending in each year to 1942 USD, sum across years (1941-1945), and divide by the county population from the 1940 Census.

County-level data on WWII casualties and draft registrations are from Brodeur and Kattan (2022)'s supplemental files. I divide casualties and registrations by the county population from the 1940 Census to estimate county-level rates.

County-level data on ownership of modern appliances are from Bailey and Collins (2011)'s ICPSR replication files. In particular, I proxy for modern appliance ownership using the percent of households with a washing machine, measured in 1960. Data on modern washing machine ownership are unavailable prior to 1960. Therefore, for long-difference analyses, I impute the percent of households with a modern washing machine = 0 in the pre-NLRA period.[98]

Data on the number of hospital beds in each county in 1934 are from *Hospital Service in the United States: Fourteenth Annual Presentation of Hospital Data by the Council of Medical Education and Hospitals of the American Medical Association* (1935), as cited in Thomasson (2002). From 1948 onward, data on hospital beds are available from annual August issues of *Hospitals: The Journal of the American Hospital Association*, as cited in Finkelstein (2007). I am grateful to Amy Finkelstein for sharing the AHA data. The number of hospital beds measures total bed capacity across all general hospitals in a county, of any control type (e.g., governmental, not-for-profit, private), that are registered with the AMA (in 1934) and the AHA (1948 onward). I exclude beds in hospitals classified as "related institutions", such as general hospitals lacking certain essentials or other institutions that are designed to give some medical care but are not strictly hospitals.

County-level data on maternal mortality by place of residence in 1935 are from Part II of *Vital Statistics*

[98]The first commercial automatic washing machine was introduced in 1937, after the passage of the NLRA. However, Bailey and Collins (2011) note that ownership of other forms of power washing machines may have been as high as 48% by 1940.

of the United States (1937). From this source, cases of maternal mortality include all deaths from "puerperal causes", which correspond to deaths with codes 140-150 from the 1929 revision of the detailed International List of Causes of Death (ICD-4). County-level data on maternal mortality by place of residence in 1960 are from National Center for Health Statistics (1961), available from NBER's Public Use Data Archive. From this source, cases of maternal mortality include all deaths with codes 640-689 from the 1955 revision of the detailed International List of Causes of Death (ICD-7). I construct county-level rates of maternal mortality per 100,000 women by dividing maternal mortality cases by the female population in each county in each year.

County-level data on infant mortality are from Bailey et al. (2016)'s ICPSR files. Cases of infant mortality include all deaths of individuals aged less than 1 year, exclusive of stillbirths. Infant mortality is by place of occurrence through 1941, and by place of residence thereafter. I construct county-level rates of infant mortality by dividing infant mortality cases by the number of live births in each county in each year.

County-level data on the percent of owner-occupied units are based on microdata from the U.S. Census of Housing in 1940 and 1960. The Census of Housing was first conducted in 1940, so data covering earlier years are not available. I access these data using Haines (2010)'s consolidated ICPSR files.