The Effects of the National War Labor Board on Labor Income Inequality*

Chris Vickers

Auburn University
czvickers@gmail.com

Nicolas L. Ziebarth

Auburn University and NBER
nicolas.ziebarth@me.com

December 7, 2021

Abstract

During World War II, the United States federal government instituted an explicit policy of wage controls through the National War Labor Board with the aim of controlling inflation and discouraging labor mobility. These wage controls, which differed by industry, occupation, and geographic region, specified maximum allowable *raises* for those earning less than a certain level (the so-called "bracket") and froze wages greater than that level. We study the persistent effects of these policies on the within-occupation distribution of labor income drawing on the U.S. Censuses of Population from 1960 to 2000. We find that higher brackets were associated with relative *increases* in inequality as measured by the p10-p90 and p25-p75 ratios between 1940 and all the way up to 1970 with no effects detectable from 1980 onward. These effects are concentrated in the left tail of the earnings distribution. A one standard deviation increase in the bracket relative to the 10th percentile of an occupation-region's labor earnings distribution in 1940 reduces the change in the log 10-50 ratio in 1960 by 18 log points.

Keywords: Wage controls, National War Labor Board, income inequality, WWII.
JEL Codes: N42, J36, D31.

*Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 1967 (CBDRB-FY22-P1967-R9320).

1 Introduction

The labor markets of today are shaped by economic institutions that have long histories stretching back decades. Some of the most striking alterations in these institutions occurred during World War II. Economists, labor historians, and sociologists have pointed to the National War Labor Board (NWLB) as one such major agent of change. For example, Piketty and Saez (2003) argue that the compression in earnings for high earners during the war can "fairly easily be explained by the wage controls of the war economy." Goldin and Margo (1992) contend that the wage setting policies of the NWLB lowered inequality for decades after the end of the war. They favorably quote Thurow (1975), who claimed that "the wage differentials of [...] WWII [...became] embedded in the labor market [and continue to] exist to this day [1975]." Notwithstanding these strong claims, there is very little direct evidence about the effects of the NWLB.

We aim to fill in this gap by combining original, archival data on NWLB-mandated maximum allowable raises (the "brackets") with Census of Population microdata to study the persistent effects of these wartime wage policies.[1] While having antecedents in the policy responses to WWI and the New Deal, the NWLB was a policy experiment of unprecedented scale and scope. At its height, the NWLB ostensibly controlled wages for workers in the private economy by imposing what we call a "soft wage maximum." Referred to at the time as the "bracket policy", this policy allowed, but did not require, a wage rate below the level of the bracket to rise to that level. At the same time, the policy did not allow any raises, but did not require cuts, for wage rates above the bracket. In an inflationary environment like WWII, this effectively froze nominal wage rates above the bracket. Because the policy did not require wage cuts for high earners, we refer to the policy as a "soft" maximum in contrast to a "hard" maximum. This policy is in contrast to the minimum wage, which does set a "hard" minimum by legally disallowing wage rates below some level.

The multifaceted nature of the this policy leads to a much more subtle set of theoretical implications for its effects on labor earning inequality. Consider as a comparison a hard maximum wage requiring wages greater than the maximum to be reduced to that level. Like a higher minimum wage, a *lower* maximum wage would be associated with lower levels of inequality by compressing the upper tail of the earnings distribution.[2] In the case of the NWLB "soft maximum wage" policy, however, it is not so simple. A bracket set lower freezes more of the distribution in place, preventing more of the income distribution from being able to achieve a pay raise. In a world of *declining* latent income inequality, this lower value for the bracket can have the effect of raising inequality relative to the "market" distribution of earnings. Because this policy affects the whole distribution of labor earnings directly, inferring aggregate effects from estimates of the

[1] The closest work to ours is the third chapter from the dissertation by Rose (2009). He too examines the effects of the NWLB on the wage distribution in a similar framework. We discuss similarities and differences to his work later when we discuss our results.

[2] This assumes no employment effects of the policy.

marginal effects of the brackets is challenging even bracketing any general equilibrium effects.

To identify the marginal effects of the brackets on labor earnings inequality in the medium- and long-run, we leverage variation in the "bindingness" of the brackets. Ostensibly brackets were set to reflect "sound and tested" wage rates in the middle of 1943, where the prevailing wage was interpreted to be 10% below the mean of the wage distribution conditional on occupation, industry, and geographic location. In practice, the actual setting of the brackets resulted in substantial variation in its "bindingness", which we measure as the ratio of the bracket to the 10th percentile of the wage rate distribution. One source of the variation was due to the fact that, like the Federal Reserve system, the country was divided up into 12 different regions, each with a separate NWLB board charged with determining brackets in that area. This created discontinuities at the borders of the regions in policy. Furthermore, within regions, geographic locations such as counties or cities were grouped into "zones" and often a uniform bracket (by industry and occupation) was applied within a zone. This generated within region differences in how binding the brackets were as a function of the preexisting level of the wage rate.

We define our treatment variable as the ratio of the bracket to the 10th percentile labor earnings at the occupation-county level in the 1940 Census of Population. Concretely, a higher value of the treatment variable represents a greater portion of the workers within an occupation-county cell being allowed to receive a pay raise. One way to interpret the variation in the treatment that is present is it reflects "measurement error" or, more precisely, the effects of using real-time data in the determination of the bracket. Under this interpretation of the variation in the treatment, our strategy is quite similar to that of Chodorow-Reich et al. (2019), who identify the effects of unemployment insurance. In their case, the "measurement error" is due to the use of real-time data on the local unemployment rate that determines the level of unemployment benefits. In our case, some of the variation in the treatment might be for similar reasons since the NWLB had to survey businesses to try to determine the wage distribution. However, there are many other sources of variation including, as mentioned early, the sharp boundaries of the NWLB regions and zones. Combined with a smooth distribution of earnings, these discontinuities generate "measurement error" and variation in the bindingness of the brackets.[3] Identification comes from the assumption that, after controlling for the 1940 level of inequality as well as county and occupation, variation in the bindingness of the bracket was as good as random.

Given our design, we cannot use the (public use) Current Population Survey or even the IPUMS samples from the decennial censuses. These sources simply do not have sufficient sample sizes, particularly when focusing on cells defined by both county and occupation as we do. Instead, we use the full count decennial Censuses of Population between 1960 and 2000.[4] We focus on employed

[3] Another source of variation is simply discretion on the part of the bureaucrats setting these brackets. The 10% rule was not a legally binding one, and, in fact, there were other ways of determining the bracket based on the "first clustering" of wages.

[4] Unfortunately, there are problems with the available 1950 Census microdata in the Census Research Data Centers, which prevents us from using it.

working-age males of any race. By focusing on men, we bracket changes in female employment during the war and its aftermath. By focusing on employed individuals, we bracket any potential employment effects of the bracket.

Although the NWLB had vast jurisdiction over private-sector wage setting, surviving records make a comprehensive analysis spanning all occupations and areas of the country infeasible. First, for some areas of the country, the records documenting the brackets set, as far as we know, no longer exist. Second, there are difficulties in matching the brackets to the Census. In some cases, the records are vague about the geographic span of particular zones. For example, a zone might simply be listed as "Detroit" without specifying exactly what are the borders of Detroit. In other cases, the difficulty in matching is due to the fact that the occupational coding scheme of the NWLB differed in many ways from that of the Census.[5] Because of these difficulties, we focus on white-collar and metal trades occupations for which the mapping between occupations in the NWLB and the Census is relatively clear.

We estimate long difference specifications with the change between 1940 and a subsequent year in some measure of labor earnings inequality by occupation and county as the dependent variable. In our preferred specification, we control for the 1940 value of the inequality measure nonlinearly as well as a full set of occupation fixed effects. We report robustness checks using p25 of the wage distribution as the deflator and varying the fixed effects included.

We find that more binding brackets, meaning higher values of the bracket relative to the underlying wage distribution, are associated with *relative* increases in "medium-run" inequality as measured by the p10-p90 and p25-p75 ratios. These effects are driven by changes in inequality in the lower part of the labor income distribution. For example, a one standard deviation increase in our measure of the bindingness of the bracket decreases the change in the 10-50 gap by 18 log points in 1960 while there is no effect (both statistically and economically speaking) on the change in the 50-90 gap. The effects of the brackets across all inequality measures attenuate over time, and there are no statistically significant effects from 1980 through 2000.

While our theoretical discussion makes us hesitant to over-extrapolate from our estimates, taken at face value, our estimates of the marginal effects of the bracket on inequality at the bottom of the labor earnings do not suggest that this policy could help to explain the Great Compression. If anything, it is just the opposite, and our estimates makes the question of the causes of the fall in within occupation inequality through the 1970s that much greater. The null effects after 1980, taken at face value, also do not offer a rationale for why inequality rose so sharply starting in the mid 1980s. On the other hand, we do not think the null results for the top of the distribution should be interpreted as meaning the policy did not affect earnings in that part of the distribution. Our interpretation is just opposite. That is, the null effect reflects the far-reaching and uniform effects of this policy on the upper tail of the distribution that cannot be identified with our empirical strategy using marginal changes in the bindingness of the bracket.

[5]The same issue of differences in coding schemes also arises when matching industries.

1.1 Literature Review

Our work fits into several distinct literatures. First, it contributes to the debate on the causes of the large swings in economic inequality during the 20th century. These swings include the sharp decline in inequality starting around the beginning of the Depression reaching a trough in the mid 1980s and the equally fast increase in inequality since then. While related, the focus of our work is different from much of this literature in that we are focused on *within* occupational inequality. Much of the earlier literature has been more focused on the skill premium or aggregate measures of inequality like the share of income going to the top 10%. Both of these are presumably driven mainly by between occupation rather than within occupation differences. Goldin and Margo (1992) do show that the within occupational component of inequality declined during the Great Compression. In fact, inequality within any number of demographic groups including education and experience fell during this period of time. That said, by focusing on within occupational inequality, we bracket many theories such as skill-biased technical change that are designed either explicitly or implicitly to explain the between components of inequality. Most closely related to our work in terms of the "type" of inequality studied is the work of Juhn et al. (1993). They find that between 1963 and 1989, much of the overall rise in inequality is due to a rise in inequality within narrowly defined education and labor market experience groups.

There are two broad categories of explanations for the changes in inequality during the 20th century. One set of explanations, as in Juhn et al. (1993), Katz and Murphy (1992), and Bound and Johnson (1992), focuses on the supply of and demand for labor. For example, Autor et al. (2008) argue that a combination of a rise in skill-biased technical change and a deceleration in the growth rate of the relative supply of highly educated workers can explain changes in inequality over the 20th century as measured by the skill premium. Goldin and Katz (2009) argue that this "race" between education and technological change has been a defining feature of the American labor market for 150 years.

An alternative set of theories for changes in inequality points to institutional changes as the cause of these swings. For example, DiNardo et al. (1996) provide evidence that changes at the bottom of the wage distribution during the 1970s and 1980s are consistent with an eroding real value of the minimum wage. Lee (1999) also argues that the minimum wage played an important role in "masking" increases in latent earnings inequality during this period. Others, such as Card et al. (2004), Western and Rosenfeld (2011), and Farber et al. (2021), point to the rise and fall in private-sector union membership over the 20th century as a key driver of the swings in inequality.

Our work also contributes to this literature on institutional drivers of inequality. The effects of the NWLB provide an interesting point of comparison to the most intensely studied labor market institution in the literature: the minimum wage.[6] As we have emphasized, the bracket policy is not nearly so simple as a minimum wage, which makes comparisons of our results to those in

[6]See Nuemark and Shirley (2021) for a detailed survey of the literature on the effects of the minimum wage.

that literature difficulty. For one, the bracket policy while implemented locally covered (at least in theory) nearly the whole economy and wages both above and below the bracket were affected. This is not true for the federal minimum wage where at its most "affecting" in 1979, the percentage of hours at or below the minimum was 13%. This percentage had fallen to 6% by 2012 (Autor et al., 2016). The broad scope of the NWLB makes it similar to the 1966 Fair Labor Standards Act that created the federal minimum wage. Bailey et al. (2020) studies the economic effects of this policy that set the federal minimum wage to its highest level in the 20th century. While they find this policy increased wages at little costs in terms of reduced employment, there were significant disemployment effects of Black men.

The most important difference between the two policies is simply that the bracket policy is aimed at capping wages while the minimum wage is aimed at just the opposite. Other examples, particularly outside of war time, of hard or soft maximum wage policies are hard to come by. There have been *de facto* maximum wage policies in the form of top marginal tax rates close to 100%. During the war years of 1944 and 1945, the top rate in the US was 94% and was above 90% throughout the 1950s all the way to 1963. The most prominent cases of explicit maximum wages tend to occur in sports leagues with the goal of limiting labor mobility and promoting parity. For example, the British Football Association in 1901 set a maximum wage limit of 4 pounds per week. Like the NWLB, the hope was that this policy would reduce financial motives for the best players to move between clubs (Taylor, 2001).[7]

Another historical example of a maximum wage comes from England in the 14th century. Following the Black Death, it is estimated that half of English clergy died. The shortage this caused was so dire that the pope allowed even women to hear confession if there was no priest available, and it led to a doubling of the salaries of remaining clergy (Putnam, 1915). Because of this steep rise, in 1363, the House of Commons asked the King to instruct the leaders of the church to do something about the "exorbitant rates charged by chaplains" (Putnam, 1915). In the end, Parliament passed a statute fixing the wages of priests (Stubbs, 1875). One goal of this law was to place a "restriction on the migration of chaplains [to areas paying higher wages]" (Putnam, 1915).

Finally, our work contributes to the literature on the consequences of WWII for different dimensions of economic inequality. One strand of this literature has examined the effects of the war on gender inequality. Extending the earlier work of Goldin (1991), Acemoglu et al. (2004) use state-level variation in mobilization rates to examine the effect of the war on women's wages in 1950. Using a similar identification strategy, Jaworski (2014) examines how the war affected broader demographic outcomes for women. Shatnawi and Fishback (2018) find a persistent effect on demand for female workers in manufacturing even after the war ends. Supporting this demand for labor centered view, Rose (2018) shows that changes in female employment during the war are

[7]While, on the face of it, the policy seemed to work as a number of players had their salaries capped, Taylor (2001) highlights a number of ways that clubs were able to get around this limit through other benefits and financial incentives for performance.

really driven by changes in demand rather than changes in the supply due to the draft of working-age men. Other work has examined the effects of the war on racial discrimination and inequality. Collins (2001) studies the effect of non-discrimination policies in hiring by the federal government on racial wage gaps in 1950. Aizer et al. (2020) examines the effects of military spending contracts on racial inequality while Ferrara (2020) uses variation in the demand for Black labor.

2 History of the NWLB and Its Bracket Policy

The roots of the WWII NWLB can be traced back to World War I. The United States faced many of the same issues then as in WWII. Indeed, the first coherent national labor policies in United States history emerged from the dialogue between the federal government, labor, and industry as they fought over how to balance workers' rights with the emergency needs of WWI wartime production (McCartin, 1997). While many involved in labor policy during WWII held negative views of the outcomes of Woodrow Wilson's economic policies that resulted in a rapid inflation during the war followed by a large deflation afterward, many aspects of the WWII NWLB were carried over from the WWI version. This included representation for industry and labor in policy decisions and the grievance process, as well as a bevy of "efficiency engineers," economists, statisticians, and labor activists, all working to marshal the manpower of the country. The WWI NWLB made three crucial decisions in labor disputes that would foreshadow the role of the WWII NWLB: (1) imposed a wage structure on the town of Waynesboro, Pennsylvania, after an analysis of wages revealed that employers were not paying a "living wage"; (2) resolved a dispute over who had the right of job classification; and (3) ruled in favor of union security that prevented employees from being fired or threatened with military draft for organizing a labor union (McCartin, 1997, p.96-97).

The WWII NWLB was established by an executive order on January 12, 1942. It grew out of the Mediation Board of WWII, which was comprised of representatives of employers, unions, and the public. At the start, the jurisdiction of the NWLB was not clearly defined, and it lacked a guiding policy on wages. Its charge overlapped with other agencies, such as the National Labor Relations Board and the Treasury's Salary Stabilization Unit. Unlike the Mediation Board, the NWLB could make a "final determination" on labor disputes. Initially, the board's decision-making was centralized in Washington, DC.

The first decision made by the NWLB came within a month of its inception in the Aluminum Company case of February 1942. In this case, it approved a wage increase of 7 cents per hour for Southern aluminum plants. This reduced the North-South wage differential by a third. The NWLB, with this decision, "clearly indicated that wage increases to substandard workers would in general be approved, regardless of the industry's ability to pay", but "warned highly-paid workers not to expect their wages to keep pace day by day with the rising cost of living" (McNatt, 1943). The NWLB took a number of factors into account when coming to its decision, among them "the

trend in the differential over the preceding nine years, the cost of production, the type of work, the prevailing wage rate in the area for comparable work, the cost of living, and the ability of the company to pay (Hachenburg, 1942, p. 341). In other cases it decided, the NWLB considered instead the average wage rate in the *industry* as a reason for adjustment, as opposed to those in the local area.

Subsequent decisions by the NWLB did little to clarify its general policy for how it set wages and instead added new complications. Wages were not to be "frozen" even as the right to strike was waived. "[P]revailing wages elsewhere in the industry as well as in the local area should be considered in judging petitions for wage increases" (McNatt, 1943, p.3). An April 15, 1942 decision regarding the International Harvester Corporation included "a restatement of the substandard yardstick principle that all workers should receive wages high enough to enable them to maintain a health-and-decency standard of living" (McNatt, 1943, p. 4). But even this (somewhat clear) principle of how wage adjustments should be justified was qualified to require that these adjustments could not result in inflation.

Some clarity in NWLB wage setting policy came in the critical "Little Steel" case, decided on July 16, 1942. This case, a consolidation of cases involving four steel companies, established an explicit wage-stabilization formula. Based on a measured 15% increase in the cost of living from Jan 1, 1941 to May 1, 1942, it was decided that "if any group of workers averaged less than a 15 per cent increase in hourly wage rates during or immediately preceding or following this period, their established peace-time standards have been broken. If any group of workers averaged a 15 per cent wage increase or more, their established peace-time standards have been preserved." This, as what it came to be known as, "Little Steel" formula seemed to make it clear that only workers who could show that they experienced a less than 15% raise over this period would have a wage increase approved. On the other hand, while workers who had received a greater than 15% raise did not have to take a pay cut, any subsequent requests for further raises would not be approved. This formula provided a quantitative (and coherent) basis for NWLB decisions in the second half of 1942.

Even with the Little Steel formula, the board gave itself wiggle room in making its decisions explicitly stating that "Any claims for wage adjustments for the groups whose peace-time standards have been preserved can only be considered in terms of the inequalities or of the sub-standard conditions specifically referred to in the President's message of April 27, 1942." Indeed, Hachenburg (1942, p. 345) notes that the "Board had very carefully refrained from stating that the 15% rule was the only one by which a given group of employees could be granted increases. Rather, the rule was so phrased that the rise in the cost of living was to be considered as a factor only to the extent of 15%." This worked in both ways in allowing the NWLB to approve raises for those who earnings had grown more than 15% and to block raises for those who had not experienced such a raise. For example, in the Lever Brothers case, a wage increase of below 15% was denied on the grounds that the firm was already paying above market wages and the fear that approving an

increase now would risk sparking a further round of wage increases.

How common deviations from the "Little Steel" formula were is not altogether clear. On one side, writing in February of 1943, McNatt (1943, p. 7) noted that "Since the Little Steel decision last July, the Board has uniformly attempted to apply this cost-of-living substandard inequality formula in deciding wage questions." Concurring in this assessment of the application of the "Little Steel" formula, Record (1944a, p. 100) viewed the implications of the Little Steel formula as implying that "wages were to be stabilized at September levels except in rare and unusual cases where nonapproval of adjustments would work discriminatory hardship on employers and/or employees, or directly frustrate the war effort." On the other hand, Derber (1944, p. 574) argued that deviations from 15% were "unavoidable": "The Little Steel formula was intended to set a terminal point for general wage increases and, as present efforts of the unions to break it indicate, it has, to a considerable extent, accomplished that purpose. But as Congress and the President under stood, no effort at stabilization could ignore gross inequities between jobs or substandard or obsolete wage structures without substantial injury to the war effort."

Until this point, the NWLB had only been involved in handling wage *disputes* between employees and employers. An executive order on October 3, 1942 drastically increased the jurisdiction of the NWLB to cover all *voluntary* wage adjustments. With this increase in the reach of the NWLB along with the implementation of the "Little Steel" formula, eight (and later twelve) regional offices were set up to adjudicate an anticipated flood of voluntary applications for adjustments. These regional offices had limited authority originally, but by early 1943, they gained a measure of authority over both voluntary adjustments and disputed cases (Record, 1944a, p. 101). Hachenburg (1942, p. 354) noted that "In effectuating its new policy, the Board permit[ed] the Regional Directors to correct maladjustments within the 15% rule in specifically designated industries. The Board, however, will itself consider all claims based on inequalities or gross inequities, and substandards, after the claim has been approved by the Regional Director." Further powers were granted to the regional directors to make decisions (subject to review) "if not more than 15% of the working force is involved, if no more than five cents is being given to any one employee and if the company does not use the increase to obtain relief from its price ceiling."

Even with the "Little Steel" formula in place, a substantial rise in prices and wages forced Roosevelt to sign Executive Order 9328, also known as the "hold the line" order, in April 1943.[8] The implementation of the "hold the line" order was through what came to be known as a "bracket policy" based on a May 12 directive. It authorized the Board to establish "by occupational groups and labor market areas, the wage-rate brackets embracing all those various rates found to be sound and tested going rates," and furthermore that "except in rare and unusual cases [. . .], the minimum of the going rates within the brackets" would be the end point of any wage adjustments. Wages above the minimum "could not be changed on the basis of gross inter-plant inequities." At the same time, the bracket policy did not require these wages to be reduced to the level of the bracket.

[8] http://www.presidency.ucsb.edu/ws/?pid=16381

In theory, a "bracket maximum" could be established as well, but this was largely irrelevant as equity adjustment were limited to raises up to the minimum bracket, above which raises were not permitted, and therefore "the bracket minimum was of primary significance and in many instances bracket maxima were not established. Bracket maxima were of significance only in 'rare and unusual' cases" (United States Department of Labor, 1949, p. 230, volume 1). An additional complication was that this minimum, the primary policy instrument, could be expressed either as a single rate or a range, for companies which had variation within an occupation. In this case, "the weighted average rate for an occupation was compared with the single rate" (United States Department of Labor, 1949, p. 236, volume 1). That is, if a plant currently had a range of 60 to 70 cents and the single rate (minimum) bracket was 85 with a range of 80 to 90, the plant could increase its range to 80 to 90. Because the weighted average to be compared to in this case was the single rate, we have used the single rate our measure of the level of the bracket.

The order was not completely clear about what counted as a "going rate" or how the minimum was to be calculated. Instead this had to interpreted by the NWLB. Record (1944b, p. 576) claimed that "[t]he bracket minimum [. . . wa]s usually set, not at the midpoint or weighted average of rates being paid for a particular job in a particular locality, but 10 per cent below the weighted average, or at the first significant cluster of going rates." The policy did not forbid all adjustments above the bracket, as "of course, increases justified on the basis of maladjustments, intra-plant inequities, substandards, or the 'rare and unusual' criteria could be permitted irrespective of brackets." Rose (2009) examines how the brackets were determined in NWLB Region X, which covered California, based on memoranda describing the wage bracket setting process. One such memo stated that "In an analysis of 160 brackets set, the California regional board found the 97 had been set by the cluster method and 17 by the ten percent method; in 4 cases, the cluster method and the 10% method generated identical results. This leaves 50 of 160 brackets which were set with some other criteria in mind." Unfortunately, we have been unable to locate similar breakdowns for other regions and, hence, are unable to say whether the bracket setting policy in Region X was common to other regions.

The bracket policy remained in place until the end of the war in August 1945. The NWLB continued operations for a time under new executive orders that "freed [the NWLB] from the necessity of governmental approval [of] all voluntary wage adjustments which employers indicated would not require price increases or which did not involve increased costs to the government" (Witte, 1952). The NWLB was formally ended on December 21, 1945, with a successor agency, the National Wage Stabilization Board, taking its place. In operation for fourteen months, the new agency's approval was required for "wage increases which employers were not willing to say they would not use as a basis for price increases" (Witte, 1952). Evidently it was not as stringent as the NWLB, as wages in manufacturing increased twice as much in its period of existence as during the whole existence of the NWLB, which lasted more than twice as long. This lead Witte (1952) to conclude that "[the National Wage Stabilization Board] seems to have had little influence upon

the trend of wage rates".

2.1 The Goals and Successes (?) of the NWLB

The overarching aim of the NWLB was to aid the war effort broadly defined. One way in which the NWLB hoped to help was through "wage stabilization" (United States Department of Labor, 1949, p. 178, volume 1), which, in concert with the explicit price controls, was meant to help control price inflation. In a speech in April 1945, George W. Taylor, the chairman of the NWLB, gave another important goal of the NWLB: "To help control the movement of manpower into war production" (United States Department of Labor, 1949, p. 182, Volume 1). As Taylor said in his speech, "The production program in one area called for 2500 skilled employees of a certain trade [...] Employers started bidding for them [...] But there were still just 1500 skilled workers available. As a result of the bidding—they were made more mobile, more volatile, and less productive. Stabilization of wages in such situations was essential to conserve the available manpower supply and assure maximum production."

It is the case that the control of manpower was ostensibly under the purview of the War Manpower Commission (WMC), and, hence, the NWLB should have only played a supporting role, similar to the role it played in conjunction with price controls to limit inflation. However, in this case, Rockoff (1984) suggests that the effects of the WMC on labor allocations were rather limited. For example, in the case of a looming labor shortage in nonferrous mining, the WMC paid the transportation costs for workers and their families to relocate to the areas around the mines. Because of the sweeping powers of the NWLB, it ended up as the best hope the government had to direct labor flows. It is at least theoretically clear how controlling wages would be useful to this end. It is less clear how well it worked in practice. The issue was that the NWLB acted in many cases reactively to changes in local labor markets, effectively ratifying the situation on the ground. Record (1944a), an economist at the regional National War Labor Board in Atlanta, wrote, "Due to the influx of new plants, and new war contracts in already established plants, plus the emigration of surplus workers to war industry centers both in the Southeast and in other regions, labor markets began to tighten throughout the eight southeastern states, and increasing pressure was exerted upon the Board to approve wage increases, particularly as the cost of living continued to rise."

We want to emphasize that it was never the explicit goal of the NWLB to reduce inequality. Instead, reducing inequality was viewed as one possible means to an end. Reducing inequality was only important to the extent that it caused "increas[ing] productivity[,] improved employee morale, stabilized employment, and reduction of work stoppage" (Derber, 1944). This might be why, in our view, the brackets were set independent of race or gender. However, observers at the time such as (Record, 1944a, p. 109) understood that "though admirable in themselves as sound and fair labor policies, [eliminating sex and race disparities in wages we]re not always mutually

coextensive with the interests of economic stabilization."

It remains to be seen whether the NWLB was successful on its own terms. For us, the "success" of the NWLB is defined by whether the NWLB actually influenced the distribution of wages during the war. The problem is that direct evidence on what happened to earnings during the war is limited, and, hence, our ability to identity the contemporaneous effects of the NWLB is limited. Vatter (1985) notes that the most notable changes in the income distribution were between 1941 and 1944, after which the income distribution by quintile stabilized. Based upon wage data collected by the Bureau of Labor Statistics during the war years, between January 1941 to October 1942, when the NWLB was granted power to set wages, manufacturing wages increased by seventeen percent, as opposed to fourteen percent from October 1942 until the end of the war. Manufacturing wage rates rose only 10.6 percent following the "hold the line" order until the end of the war (Douty, 1950). This is suggestive evidence that the NWLB moderated the rate of wage inflation through the bracket policy.

At least statutorily, the NWLB had "great" enforcement mechanisms available to ensure compliance with the bracket policy (Vatter, 1985). It had the power to punish unions that went on strike by revoking concessions. As a last resort, non-compliance by unions or industry could result in referral to the president. This was not an idle threat. Forty-six cases were referred to the president, and in forty of these the plant was seized (United States Department of Labor, 1949, p. 427, volume 1). The NWLB could also request the War Department to blacklist firms or cancel contracts. Additionally, wages paid in violation of NWLB orders could be disallowed as a tax deduction for the firm. At the same time, at least in the case of mutually agreed upon wage changes, it is hard to see how violations would even come to the NWLB'S attention. Rockoff (1984), in a broader history of wage and price controls, highlights the ways in which people attempted to evade these sorts of controls in WWII through the black market. Clinard (1952) claims that the black market at this time was so extensive as to raise "serious questions [...] as to the moral fiber of the American people."

One indirect reason for believing that the NWLB did affect earnings in the short-run is simply the fact that the similar program during war of controlling prices was considered a "success." Galbraith (1952), who was actually an administrator of the Office of Price Administration tasked with implementing these controls, claimed they were not substantially evaded and kept prices lower than they would have been otherwise. Evans (1982) agrees with this conclusion and offers a counterfactual in which prices would have been 30% higher without them. Rockoff (1981) points to the limited number of enforcement cases as evidence for adherence to the controls.[9]

[9]This could also be interpreted as evidence that violators of the price controls faced minimal risk of being investigated.

3 The Theoretical Effects of the Bracket Policy

To understand the theoretical implications of the bracket policy, it is important to clarify what the policy was and what it was not. Previous literature describing this policy has often been imprecise, particularly about its similarities to more familiar wage setting policies like the minimum wage. The brackets specified a maximum wage rate that *could* be paid, but it did not require those making less than this amount to receive a raise up to that level. Furthermore, for those paid more than that level, the policy did not require their wages to be cut down to the level of the bracket. Instead the policy simply froze the wages of those earning more than the bracket in place. Because of how it functioned, we call the policy a "soft maximum" wage, to distinguish it from a hard or binding cap (or floor) such as how the usual minimum wage functions.

These differences in the functioning of the bracket policy from a simple minimum (or maximum) wage create differences in their theoretical effects on inequality. The first-order effect of an increase in the minimum wage on the wage distribution is to simply move everyone below the minimum wage to the new floor, leaving everyone above the minimum wage unaffected. Ceteris paribus, this should reduce inequality.[10] There are two potential complications to this basic analysis of the effects of the minimum wage on inequality: (1) spillovers to other parts of the earnings distribution and (2) disemployment effects. For the first, those earning above the minimum wage might receive a raise in response to an increase in the minimum wage. There are various theories for source of such spillovers such as maintaining a particular hierarchy of earnings. The existence of spillovers would attenuate the direct reduction in inequality from an increase in the minimum wage depending on how far up the distribution the spillovers operate (though it would be hard to imagine these spillover effects dominating the direct ones resulting in an overall increase in inequality).

The second complication is the possibility of disemployment effects on those earning less than the minimum wage. To illustrate this mechanism, consider the interquartile range of earnings. Assume initially that workers are uniformly distributed at earnings of 5, 25, 75, and 150. In this case, the IQR is 50. Now assume that a minimum wage increase causes all those previously earning 5 to be put out of work with no effects on employment (or earnings) at the other wages. Then, although the 25th percentile does not change, the 75th percentile *increases* and, therefore, the IQR increases. An increase in the minimum wage, whether or not there are disemployment effects, will *not* decrease the lower percentiles of the earnings distribution, but, when there are disemployment effects, the minimum wage might increase the upper percentiles. If the upper tail of the income distribution is sufficiently thick, this might lead to an increase in inequality as measured by the difference between two percentiles. Like the existence of spillovers, the disemployment effects are hotly contested going back to the seminal work of Card and Krueger (1995) that found a null to slightly positive employment effect. Now, as a practical matter, given the small fraction of people

[10] A hard maximum wage would work just the same to reduce inequality except it would reduce the earnings of those above the maximum.

earning close to the minimum wage, even if all these people lost their jobs following a minimum wage increase, this should have minor effects on the IQR or p90-p10 range.

With these considerations in mind, we present a very stylized model of wage changes to understand the effects of the brackets. We simplify and assume that the brackets continued to be strictly binding even after the policy was removed following the war. The comparative statics exercise we are interested in is the effect of changing the (log) bracket $\bar{w}$. This ranges from $\bar{w} = \infty$, which is the the case of no government intervention (the "free market"), to $\bar{w} = -\infty$, which is the case of maximum intervention in wage setting. In other words, an increase in $\bar{w}$ corresponds to a decrease in government "intervention", as it allows more wages to be affected by market forces. Importantly, the case of $\bar{w} = -\infty$ does not mean that inequality is equal to 0 unlike, for example, what would happen if the minimum wage were set arbitrarily high. Instead, this case simply means that, at least, as long as this policy is in place, all wages are frozen and inequality is fixed at its pre-policy level.

Our goal is to isolate the extent to which the bracket policy "masks" changes in the latent distribution of earnings inequality to use the term in Lee (1999). Let the latent level of earnings tomorrow be $w^*_{t+1} = f(w_t)$ where w_t is the wage today and f is monotone increasing. Then we can express the actual level of earnings tomorrow w_{t+1} taking into account the effect of the brackets as

$$w_{t+1} = \begin{cases} f(w_t) & \text{if } w_t \leq f^{-1}(\bar{w}), \\ \bar{w} & \text{if } \bar{w} \geq w_t > f^{-1}(\bar{w}), \\ w_t & \text{if } w_t > \bar{w}. \end{cases}$$

We consider a special case where $f(w_t) = \exp(\alpha) w_t^{1+\beta}$ and $\bar{w} = 1$. We consider three scenarios for changes in the inequality of the latent distribution of earnings: (1) constant $\beta = 0$, (2) increasing $\beta > 0$, and (3) decreasing $\beta < 0$ inequality. We adjust the values of α and β in the last two cases so that the average income growth rate is the same across all three scenarios. Obviously, changes in the latent distribution of income could be much more complicated with a non-linear relationship between the growth rate of income and the initial wags. We also assume that there are no employment effects from the brackets.

Figure 5 shows the relationship between the initial wage and subsequent observed wage growth for these three scenarios In the flat wage growth scenario, for sufficiently low initial wages, the growth rate is simply equal to the growth rate in latent earnings. However, for those earning initially $-\alpha$, the bracket is just binding and all those initially earning between $[-\alpha, 0]$ will pile up at 0 because of the binding bracket. For those with initial wage greater than 0, they will experience no earnings growth at all. The case of a decreasing growth rate is quite similar to the case of flat growth.[11] For both of these cases, the relationship between the initial wage and subsequent growth

[11] The only slight difference is that a few more workers end up being affected by the bracket. This is a consequence of the assumption of the same average growth in latent earnings across the scenarios.

will still be (weakly) monotonic. The relationship, on the other hand, is non-monotonic in the increasing wage growth case. For the lowest earnings individuals, we observe the growth in latent earnings, but the bracket masks the latent (positive) relationship between earnings and earnings growth for high earners, as their wages are constrained by the bracket. This generates the non-monotonic relationship between initial income and observed earnings growth: Only workers who would not have been constrained by the bracket benefit from the latent change in the earnings distribution.

What do these effects of the bracket on the observed growth rate mean for changes in inequality? 6 plots the difference between the post and pre p25-p75 measure of inequality as a function of the bracket in the case of declining latent inequality. Note that in using the p25-p75, or minus the IQR, a higher value means *less* inequality. In this world, the marginal effect of the bracket on inequality is non-monotonic. There is a region of values where the marginal effect of teh bracket is 0, another where it is positive, and another where it is negative. For the case of no marginal effect, this can happen for small, medium, and large values of the bracket. For values set less than the 25th percentile, the wage at the 25th and 75th percentile are frozen so there is no marginal effect of the bracket on the IQR. For the intermediate values still less than the 75th percentile, the 75th percentile remains frozen while the 25th percentile is allowed to grow according to the growth in latent earnings. For a bracket set high enough, there is again no marginal effect since both the 25th and 75th percentile are unaffected by the bracket. Note that in this case, the *level* of inequality is lower than the case of a very small bracket. To understand this, imagine a world where there are only two types of cases, either $\bar{w} = -\infty$ or $\bar{w} = \infty$. The changes in inequality in the latter case, which is the free market one, will match the changes in the changes in the distribution of latent earnings inequality, which, in this case, we assume is falling. For the other case of $\bar{w} = -\infty$ where government intervention is at a maximum and all wages are fixed, there will be a smaller observed decline in inequality.[12]

There is also a region of intermediate values of the bracket greater than the 25th percentile where the marginal effect is a reduction in inequality. A bracket set to a value in this region allows the 25th percentile to rise while freezing the 75th percentile in place. There is then a region of values for the bracket greater than the 75th percentile where just the reverse happens and the marginal effect is negative. Here marginal increases no longer have no effect on the 25th percentile but they do lead to increases in the 75th percentile of earnings and as a consequence inequality rises.

These are of course highly stylized examples of the evolution of the latent earnings distribution, but they are still able to illustrate three general important points. First, even in this relatively simple world, there is no clear prediction for the sign of the marginal effect of the bracket. It will depend on where in the distribution of earnings the variation in the bracket is located. Second, as

[12]The fact that the marginal effects are identically zero is due to the fact that we are using a difference of percentiles measure versus a more "continuous" measure such as the standard deviation.

a corollary to the first point, the estimated marginal effect need not be informative of the policy's aggregate effects. This is not due to general equilibrium effects, but the fact that the policy unlike the minimum wage affects wages not only close to the bracket. For example, assume that the variation we used to estimate the effect of the policy was around a bracket equal to the 30th percentile of the earnings. Then we would find that a marginal increase in the bracket decreases inequality. However, a infra-marginal increase in the value of the bracket can lead to a higher level of inequality. Third, the estimated effects depend critically on changes in the latent distribution of earnings. This adds to the complexity of interpreting our estimates over time since it is widely thought that between 1960 and 1985, inequality in the latent distribution of earnings declined and, since then, it has rose.

As a final note, we have focused here on labor earnings inequality. It is plausible that the brackets might also affect inequality in total market income that includes both labor and capital income. In effect, the policy was aimed at limiting the bargaining power of labor. All else equal, this decline in bargaining power of workers would lead to a decline in the labor share of income and an increase in inequality between workers and capitalists. There are countervailing forces in this period of time where, as a whole, labor had a strong bargaining position due to the needs of war production and the enlistment of millions of men. Our point here is how the brackets could have shifted the bargaining power of workers on the margin. We hope to examine this dimension of inequality in future work.

4 Data

4.1 NWLB Records

We collect primary source records on the brackets set by the NWLB at the occupation-industry-geography level.[13] Unfortunately, some brackets from particular regions have to our knowledge not survived. For example, the termination report from the NWLB contains a count of "approximately twelve-hundred bracket rates [...] in the 11 areas into which Oregon and Washington were divided" (United States Department of Labor, 1949, p. 81, volume 3). This makes it clear that the brackets for this region did exist at one time, but we have been unable to locate surviving brackets from Region XII, covering the Pacific Northwest, in either the National Archives or the regional archive which should hold documents for this region. We also have not been able to locate the brackets from Region VI, headquartered in Chicago. Consequently, both of these regions are not included in our analysis.

In order to develop the bracket system, the Bureau of Labor Statistics first conducted a large-scale survey of employers "to provide information on prevailing rates in key occupations in leading

[13]These records are located all across the country at the regional branches of the National Archives as well as in the College Park annex of the main National Archives.

industries in all important labor market areas" (United States Department of Labor, 1949, p. 797, volume 1). To carry out this survey, occupations were defined based on the 1939 *Dictionary of Occupational Titles* and sometimes subdivided into grades. The classification of jobs into standardized categories was made by field representatives of the Bureau of Labor Statistics, consulting with labor managers and foremen. The establishment-level records from this survey, which have been kept at the regional archives as well, provide some of the earliest standardized information on wage distributions across occupations, localities, and firms in the United States. For "key jobs" in covered industries and areas, the form provided a weighted average rate and a range of rates at establishments. To give some example of the detail present, for male electricians in the cotton goods industry in Danville, Virginia, there is information on average wages for both unionized and nonunionized establishments, the number of firms and workers in each of these, as well as ranges for three "representative" firms.

While the NWLB had broad authority over all wages in the private economy, the number of occupations explicitly assigned a bracket was relatively small. As United States Department of Labor (1949) stated, "wage rate brackets were established for [only] key occupations in an industry in a labor market area." The rationale behind this was that since the wage structure within plants was based on a set of "key" jobs, the NWLB only needed to control the wages for these occupations to control the entire distribution of wages. "In selecting the key occupations the intention was to select job classifications which reflected the entire spread of wages common to the industry and which represented the peg points on the basis of which rates for other related occupations were normally set" (United States Department of Labor, 1949, p. 231, volume 1). In the view of the Region XII director, this policy of only setting brackets for key jobs actually increased the ability of the NWLB to stabilize the wage structure: "This limiting of brackets to key jobs was not only an economy of time, but it resulted in preserving intra-plant relationships more accurately than would have been the case if brackets had been set for practically all jobs" (United States Department of Labor, 1949, p. 98, volume 3) For one, the NWLB felt that these key occupations could be "clearly and precisely defined." "[W]here there was uncertainty as to the identity of the job functions, or where it was not possible to get a sample adequate to develop a bracket, the Commission maintained the existing differentials between the rates of the key job classifications where a bracket had been developed and rates for other classifications" (United States Department of Labor, 1949, p. 1189, volume 1).[14]

This "clearly and precisely defined" aspect of classification of key jobs was important to minimize uncertainty over how a worker was classified. Furthermore, the clarity of the occupational classification also minimized one potential method of evading the brackets through reclassifying people into higher-paying occupations while keeping their actual duties unchanged. While we have no information on the amount of reclassification taking place, the NWLB was aware of the

[14] "The Commission" refers to the Daily Newspaper Printing & Publishing Commission, one of the special cross-regional agencies set up by the NWLB in some particular industries.

potential for this and issued specific instructions capping the amount of reclassification allowed: "Reclassifications and job re-evaluations [are] not to exceed an average increase for all employees in the plant or plants covered by the order or authorization of 1 cent per hour or 1%." (United States Department of Labor, 1949, p. 193, volume 1) In a February 1945 policy statement, the NWLB reiterated its intention that job reclassifications at plants not be a subterfuge for general wage increases: "The Board will not approve a job evaluation program which provides a general wage increase to all employees [...] to assure that wage-rate alignment is accomplished within the prevailing wage levels, the job evaluation must be based upon anchor points which are the wage rates being paid in key occupations in the plant in which substantial numbers of workers are employed. [...] As a result of these Board policies, the overall evaluation program may result in decreases as well as increases in job rates" (United States Department of Labor, 1949, p. 689, volume 2).

Brackets were also in principle defined by industry meaning two people doing the same job in the same area might face a different bracket depending on the industries they worked in. Like the case for occupations, not all industries were explicitly assigned brackets. There is no documentary evidence to suggest these unmentioned industries were not subject to the NWLB, so the question is how workers in such industries were treated. At the same time, there is no discussion of why these industries were not mentioned unlike the occupations case where the NWLB is explicit about only creating brackets for "key" occupations and the reasons for that decision. The closest discussion we have for these cases comes from a discussion of "isolated plants" for which there was not explicit bracket coverage either. (United States Department of Labor, 1949, p. 689, volume 2) states

> A second approach involves the comparison of jobs in the subject plant with bracket rates for similar jobs in other industries in the same labor market area. In most instances, specific comparison may be possible only with reference to common labor and maintenance job classifications. Bracket comparisons involving these jobs, however, may provide an adequate basis for processing the case. In the use of this method, great caution must be exercised. Attention should be given to any marked historical differentials that have existed between wages in the subject plant and wages for comparable work in plants in other industries in the labor market area. For example, a comparison involving Job classifications in the fertilizer industry and in the basic steel industry that failed to recognize the long standing differences in rates between these industries would not be valid in terms of wage stabilization.

While there are a number of caveats here, we interpret this to broadly mean that the brackets for listed industries were applied to non-listed industries. This does not really answer the question of what to do when an occupation in an unlisted industry is listed in two separate industries (in the same geography). Because of this issue, we focus on "the case of occupations common to a number of industries, for example, clerical positions, [for which] cross-industry brackets rates

were sometimes established. Thus, one rate was usually set for typists in all industries in one area." (United States Department of Labor, 1949) This allows us to sidestep the issue of missing industries at the cost of looking at a more limited set of occupations.

Finally, there is the question of how workers were grouped geographically for the purpose of assigning. Initially, the country was divided into 8 and then into the 12 administrative regions. Each regional board was then tasked with defining the geographies in its area. It was these within region divisions that was the geographic unit at which a bracket. The general approach to setting up brackets was based on the underlying spatial dispersion of wages within an industry: "In certain industries the wage structure was such that uniform rates were established for an area covering a number of contiguous localities or even an entire region"(United States Department of Labor, 1949, p. 231, volume 1). In fact, the NWLB used a concept of "labor market area" (United States Department of Labor, 1949):

> [Such an] area encompassed by a particular bracket rate was normally a single locality but no hard and fast rules were applied with regard to geographical coverage. In determining the appropriate geographical area for a bracket determination, consideration was given to the labor market areas established by the Bureau of Labor Statistics and the War Manpower Commission. In general, the geographical coverage of a set of brackets represented an economic unit within which there was competition for labor. In certain industries the wage structure was such that uniform rates were established for an area covering a number of contiguous localities or even an entire region.

Because of this lack of a hard and fast rule on how to define the boundaries of such an area, regions differed in how detailed the definitions of local labor market areas were. For example in Region IV (Figure 1), county borders were used as boundaries, while in Region I (Figure 2), borders were sometimes defined at the level of a town. In other cases, the records do not specify precisely the boundaries of a geographic areas. For example, in Region V covering Kentucky, Ohio, and West Virginia, brackets were only assigned to broad areas such as "Louisville", without reference to what area is exactly covered by the bracket. We note as well that the the geographic area could potentially be industry-specific.

4.2 Census of Population

To measure outcomes and provide additional controls, we use data from the long form of the U.S. Federal Population Censuses taken in 1940, 1960, 1970, 1980, 1990 and 2000. Everyone enumerated is asked to provide basic demographic information including age, sex, race, and marital status. Only a random sample of individuals are asked to fill out the long-form, which asks about a richer set of economic and demographic. For use, we focus on the labor market related variables including total wage and salary income, which we will call labor income. The long-form also asks about

a person's occupation, industry, and the number of hours worked. These first two variables are critical for linking to the bracket records and the last we use to define our working sample.

There are a few issues to note with the long form. First, as mentioned above, only a fraction of the population receives the long-form. This varies from 25% in 1960 to approximately 1 in 6 in 2000.[15] This still results in a substantial sample except in the case of the 1950 long form Census, which is too small and ridden with errors to be usable. Second, the labor income variable is topcoded to preserve anonymity.[16] As we discuss in greater detail below, we work at the occupation by region level, which, we think mitigates the potential effects of topcoding. Third, while the original census records recorded occupations and industries as raws string, we use the recoded version of these variables into the `occ1950` and `ind1950` classifications. Part of the reason why there are difficulties linking the bracket records to the Census is due to the relative coarseness of these 1950 classifications.

4.3 Sample Construction

We now discuss the process by which we constructed our working sample. We first select from the census records men ages 18-64 who worked, at least, part-time in the last year. We exclude women to bracket the potentially confounding effects of the sharp rise in women's labor force participation in the decades after the war. By focusing on those working at least part-time, we bracket any possible employment effects of the NWLB along the intensive and extensive margins. The rest of our sample restrictions are due to the limitations of the bracket data. We already mentioned the issue that for two regions archival records of the brackets are missing. We also discussed the issue of occupations, industries, and regions that were not explicitly listed in the records. The biggest limitation is due to the fact that the occupation codes used by the census do not align with those in the bracket records.[17] One additional issue with matching brackets to the census records is that sometimes there are a variety of grades or sub-classifications of a particular occupation.[18]

In the end, we focus on two particular groups of occupations: white-collar and clerical jobs and jobs in the metal trades. These groups have a number of useful features. The useful important feature is that we are able to accurately link the bracket records and census data for occupations

[15] More precisely, the 1960 Census used a long-form for every fourth enumeration, one in five received it in 1970, and over-sampling of geographically small units was performed in 1980, 1990, and 2000.

[16] The topcodes for years 1960 through 2000 are respectively $25,000, $50,000, $75,000, $140,000, and $175,000. For 1990, topcoded values are reported as the median of topcoded incomes for a state. For 2000, the mean for the state was used.

[17] Rose (2009) uses BLS surveys collected around the time in his study of the NWLB. One feature of that source relative to the Census is that it used the same occupational and industry classification system as the NWLB. This makes it straightforward to link the BLS and NWLB records. The drawback is that the BLS reports covered a limited set of occupations.

[18] In cases like this, we have picked one (generally the one labeled "A") and used this consistently across geographic areas.

in these groups. Furthermore, the white-collar group was a cross-industry classification, meaning we did not need to link NWLB and Census industries for this group of occupations. The metal trades group was not cross-industry as such. However, it seems likely that the level of a bracket for the occupation in that industry was informative about how that occupation would be handled in other industries, so we have chosen to take these brackets as applying across industries in a given region. Finally, it was fairly common for these occupations to be explicitly mentioned in the bracket records. For the 10 white collar occupations, five (bookkeepers; messengers and office boys; office machine operators; stenographers, typists, and secretaries; and clerical and kindred workers (n.e.c.)) have a bracket defined in NWLB records for at least 1600 counties. Other occupations such as draftsmen were recorded in relatively few brackets. In order to gauge the coverage of these brackets, we compare the number of individuals in these occupations in covered counties to the national population in the 1940 Population Census. In the most commonly assigned occupation (bookkeepers with 1740 counties with a bracket assigned), 51% of the individuals in 1940 in that occupation have a bracket assigned.

To give a sense of the selection due to focusing on these occupational groups, Figures 3 and 4 compares the average characteristics of those in white-collar occupations and metal trades occupations, respectively, to the 1940 population of men ages 18 to 64 who worked at least part-time. The standard errors are robust, but trivial in magnitude given the sample size. Perhaps not surprisingly, those in white-collar occupations tend to be white, higher educated, and earn more. The picture is basically the opposite for metal trades workers. They are less likely to be white, less educated, and earn less relative to the population as a whole. Table 1 shows similar comparisons between our working sample and all employed men 18-64 in 1970. The differences here are not just due to differences in the occupations as in the previous figures, but also potentially due to the fact that to be in our working sample, an individual needs to be matched to the relevant bracket. Our working sample in 1970 tends to be whiter, higher educated, and earns more.

One final detail is that, because regions differed in the degree of geographic fineness with which brackets were assigned, we collapse brackets at the sub county-level to the county. For brackets defined at a sub county-level, we weight the town population aggregating to the county. This aggregation process could generate a form of measurement error in the treatment variable, the value of the bracket.

5 Empirical Strategy

Denote the bracket for occupation i in county c by $\bar{w}_{ic}$ and the p-th percentile of labor earnings for occupation i in county c at time t by $w_{ict}(p)$. We consider the log ratio of the p-th percentile relative to the p'-th percentile as our measure of inequality. In all our specifications, we put the lower percentile in the numerator, meaning the log ratio will be negative and a positive effect of the bracket *reduces* inequality. Our outcome variable is the difference between the $p-p'$ inequality

measure at year t and 1940 denoted by $\Delta \log\left(\frac{w_{ict}(p)}{w_{ict}(p')}\right)$. To be clear, our dependent variable is inequality in labor *income* not the wage rate, even though the wage is what was being controlled by the NWLB. The distinction between total labor income and the wage rate does not make much difference in our case since we focus on people working at least part-time and, in most cases, full-time.

The regression we estimate is

$$\Delta \log\left(\frac{w_{ict}(p)}{w_{ict}(p')}\right) = \beta \log\left(\frac{\bar{w}_{ic}}{w_{ic1940}(10)}\right) + \text{Controls}_{ic} + \epsilon_{ict}.$$

Our basic specification is motivated by the one in Rose (2009)[19] and in Autor et al. (2016). Both of these works were in turn inspired by the specifications in Lee (1999). We note that none of these earlier papers used this long difference specification, though there was no data limitation that prevented them from doing so.

Because an "observation" is a statistic derived from a group of individuals in a given occupation-county cell, like Autor et al., who work with state-level data, we weight the observations by the total number of people. This is slightly different that Autor et al., who weight by the "sum of individuals' [in a state] reported weekly hours worked multiplied by CPS sampling weights." Since we focus on people working at least part-time, the difference between weighting hours versus employment is not a major one. An additional issue with our "observations" is what to do about small occupation-county cells for which percentiles might not be unique. We consider two approaches: (1) if a percentile is non-unique, we take the (equally weighted) average of all possible values for that percentile and (2) restrict attention to cells with more than 100 observations and, therefore, have unique percentiles.

Our treatment variable is the bracket "normalized" or deflated by the 10th percentile of the occupation-county labor earnings distribution in 1940. Our choice differs from Autor et al., who use the median. Autor et al. argue that using the median and including the square of minimum wage normalized by the median is important to capture potentially non-linear effects of the minimum wage. In particular, those additional terms allow for the effect of the minimum wage to depend on its relationship to the median. The idea is that, since the wage distribution has more mass around the median, an increase in the minimum wage affects more wages if the minimum wages is closer to to the median. Our theoretical analysis hinted at the possibility of non-linear effects in our case though for very different reasons. For now, we only include the linear term but plan to experiment with including the quadratic term in future work.

In our preferred specification, the controls include the 1940 value of $\log\left(\frac{w_{ict}(p)}{w_{ict}(p')}\right)$ and its square as well as occupation fixed effects. We view the inclusion of occupation fixed effects as important for the credibility of our identification strategy because it is plausible that the NWLB deviated from

[19] Rose was working with a much smaller sample of only 91 observations. Because of this, he was not able to include occupation fixed effects.

its general rule for determining the bracket for certain key occupations to the war effort. This does have the drawback of making our analysis a within occupation one and limiting the comparability of our work to others in the literature that have focused on overall changes in inequality during this period.

By including the 1940 level of inequality (and its square), we control for potential persistence in inequality at the occupation-county level. Even in the cases when the NWLB deviated from its general rule for setting the bracket, it seems unlikely they made these deviations on the basis of measures of inequality. For this reason, we do not view the inclusion of the 1940 level of inequality as critical for identification. A similar argument applies to potentially including the pre-trend in inequality between 1940 and 1930. Trends in inequality are a concern of Autor et al., and they control for state-specific trends. Unfortunately, the Census did not ask about labor income in 1930 so we cannot construct a similar inequality measure in that year and, hence, we cannot include the pre-trend.

5.1 Threats to Identification

Understanding the threats to identification requires understanding the source(s) of variation in the treatment variable, the bracket scaled by the 10th percentile of 1940 earnings. The most "generous" interpretation of our identification strategy is that it is akin to the strategy in Chodorow-Reich et al. (2019). The idea in that paper is to isolate the variation in unemployment insurance (UI) benefits due to the "measurement error" resulting from the real-time data on state-level unemployment rates that determines the generosity. They then use this exogenous variation to identify the effects of UI on unemployment. In our case, it is conceivable that a similar sort of measurement error due to the "real time" nature of the data used by the NWLB is an important source of variation in the deflated brackets. The regional boards were working with limited samples on wages that they had collected with perhaps issues of non-response and less than truthful reporting. So even if the regional boards followed the rule for setting the bracket precisely, data issues could drive variation in the deflated brackets.

Given the NWLB's rather vague methodology, it is natural to estimate whether other statistics of the earnings distribution correlate with the level of the brackets. To answer this, we first calculate hourly wage rates in 1940 by calculating total hours worked as the number of weeks worked last year times the usual number of hours worked per week. We then divide labor earnings by this measure of total hours worked. We then make an aggregate adjustment to wages equal to the growth in manufacturing wages between 1940 and 1945.

We regress the brackets on various percentiles of the hourly wage rate distribution. To control the influence of outliers in these regressions, we winsorize the 5% tails of the hourly earnings distribution. Table 2 reports the result of these regressions with robust standard errors. Note that we do not include any other controls such as county of occupation fixed effects in these regressions

unlike our specifications examining the effects of the brackets on inequality. While the relationship between the 10th percentile is statistically and economically significant with an elasticity of just more than 1/6, it is also substantially smaller than 1. We also observe statistically and economically significant positive relationships with the 25th and 50th percentiles. The last specification includes all of these statistics in a "horserace" regression. In this case, only the median remains statistically significant.

An implication of a deterministic rule for setting the bracket is that the R^2 of the regression should be equal to 1 or, in other words, there is no variation in the residuals. Figure 7 considers this prediction by plotting the (log) value of the bracket along with the bracket deflated by a number of statistics at the occupation by county level of the 1940 hourly wage distribution. We find that adjusting the bracket for the 10th, 25th, or 50th percentile does substantially reduce the variance in the brackets. However, these adjustments do not reduce the variance to 0, which is what we would expect if the brackets were being set mechanically. Rose (2009) plots a histogram of the percent of 1943 earnings observations that fall below the value of the bracket for a sample of occupations from California. The most common result is between zero and twenty percent.

We take all of this as evidence that brackets were being set as function of local earnings, but there seems to clear deviations from perfectly deterministic bracket setting. Because of this, we do not want to lean too heavily on this measurement error interpretation of the variation in the deflated brackets. The problem is that it is difficult to distinguish between "bureaucratic white noise" as an explanation for the variation or the imperfect information we the econometricians have available. It is possible that the bureaucrats of the WLB had additional information that they used in setting the brackets or exercised discretion in a non-random way (which they were allowed to do). These additional sources of residual variation might be potentially correlated with future outcomes independent of their effects on the brackets. Another issue is the fact that we are using 1940 data instead of data from 1943 contemporaneous with the setting of the wages. So perhaps the brackets are actually being set in a purely deterministic way and all the variation is due to measurement error introduced by the 1940 data. In all of these cases, we would not be identifying off a "pure" measurement error, but rather only errors that seem apparent to us. Because of this, we use both the 10th and 25th percentiles as deflators.

Another potential useful source of variation is the spatial variation in the brackets. Figure 8 maps this geographic variation in brackets for stenographers.[20] At the coarsest level, there is variation between groups of states as a function the NWLB region they fall into denoted by the thick black lines. For example, the difference in the brackets between Louisiana and Mississippi is (at least partly) explained by the fact that Mississippi is in Region IV and Louisiana in Region VIII. At a finer level, there is between state variation within a NWLB region. For example, the value of the bracket in Nebraska differs from the value in Iowa even though both are located in Region VII. Finally, at the finest level, there is between county within state variation, most

[20]This is an cross-industry occupation so there is no variation in the bracket by industry.

prominently in Region IV that covers many of the southern states. Given the fact that wages are smoothly vary in space, these borders should generate discontinuities in the deflated bracket. This is exactly what Figure 9 shows, which plots the distribution of brackets relative to the 10th percentile of hourly labor earnings in 1940. It is clear that there is substantial variation in these ratios across space due to the zone boundaries.

In the end, out basic identification assumption of our strategy is that, after controlling for the 1940 wage distribution, which serves as a proxy for the 1943 earnings distribution, there are no other (unobserved) determinants of the brackets and the future wage distribution. What would a confounder look like? In our preferred specification with occupation fixed effects, this would be something that varies at the occupation-county level that is not correlated with the 1940 level of inequality but is correlated with the bracket. One potential possibility here is war effort related variables that varied at a local level, for example, government spending contracts. Aizer et al. argue that government contracts, which stipulated racial non-discrimination, decreased racial wage differences in the long-run. If high amounts of government contracts were correlated with high values of the bracket (after controlling for everything), then the issue would be whether apparent effects of the brackets were actually due to the brackets or due to the effects of government contracts. By the same token, it could be argued that, in this case, perhaps it is the brackets that are affecting the racial wage gap rather than government contracts. We take some comfort from our results suggesting that, while there is a positive relationship between the brackets and (some) local measures of the war effort, these effects did not seem quantitatively significant.

While we do not have access to their variable, we do have access to a few other war effort related variables. For these war effort related variables, estimate their relationships by occupation j-industry k-county l with the wage bracket $\bar{w}$:

$$\log \bar{w}_{jkl} = \beta WarEffort_l + \text{Controls}_{jkl} + \varepsilon_{ijkl}, \tag{1}$$

where $WarEffort$ is either (1) state-level mobilization rates (Acemoglu et al., 2004) or (2) county-level manufacturing investment, separately for total and public, (Jaworski, 2017).[21] For all regressions, we include occupation fixed effects. Since inductions only varies at the state-level, to keep the comparisons fair across predictors, we do not include any geographic fixed effects. These regressions also control for the p10, p25, and p50 of the 1940 log hourly wage distribution to examine whether these war related variables have any predictive power over and above that of the earnings distribution. Standard errors are robust.

Figure 10 shows the association between the level of the bracket and these measures of the war effort. Consistently, we find a positive relationship, though only statistically significant for inductions (which vary at the state-level). This should not be surprising, since the brackets were

[21] Total and public investment are in \$100,000s. We do not log transform these variables because there are many 0s. Inductions are log transformed and from 1943.

set in 1943 and areas with tighter labor markets due to other interventions in the market would likely have increased wages relative to the 1940 level. Note that these regressions capture the total effect of these war related policies on the bracket. This includes both indirect effects on the 1943 wage distribution and any direct effects. These direct effects might be due to the fact that, even after adjusting for the local earnings distribution, the NWLB set higher brackets in an area with a high level of public investment to limit labor flows out of that area. Because of this, we interpret the magnitude of these effects as rather small and conclude that the main (observable) driver of the brackets is the earnings distribution.

An issue with many of the war related variables that have been studied is that do not vary by *occupation*, at least explicitly, but only by geography (or perhaps industry). For example, the total amount of government contracts in Aizer et al. is just that a total for a county so to operate as confounder, it will also be necessary that these county-level variables have differential occupation-specific effects. This is certainly possible but an added complexity that makes it *ex ante* (at least somewhat) less plausible. To test this more formally, we interact our three local measures of the war effort with occupation fixed effects. Figure 11 plots out the distribution of the t-statistics of these occupation-specific effects where we center by the overall effect of a given war effort measure from Figure 10. What we find is that the distribution of occupation specific effects is not far from what you would expect by chance if there were no occupational differences.

Even if we have addressed all the threats to identification, there are, of course, limitations to our identification strategy. First, this strategy will not allow us to identify any spillover effects across areas that make up a single local labor market, for example. Now the NWLB attempted to create geographic regions that reflected local labor markets. To the extent that this is true, then spillovers of this should be limited.[22] Another type of potential spillovers is across occupations (or industries). Implicitly, the NWLB believed in such occupational spillovers when they decided they only needed to target certain "key" occupations and the rest of the earnings distribution would follow. There are also potentially spillovers between different parts of the earnings distribution conditional on industry, occupation, and geography. The theory for such spillovers is basically the same as for why occupational spillovers exist in that employers need to maintain a certain hierarchy of wages.[23] While geographic spillovers might lead to biases in our estimates to the extent that we exploit nearby areas, the fact that we are examining within occupation inequality makes our estimates immune to concerns about between occupation spillovers. That said, this concern along with our earlier discussion about the non-monotonic effect of the bracket on inequality makes it difficult to draw conclusions from our estimates about the overall effect of this policy.

[22] One approach to this potential concern is to estimate a so-called "donut hole" specification from the minimum wage literature. Such a regression uses counties that are close but not too close as controls.

[23] The existence of such spillovers in the context of the minimum wages is still debated. Card and Krueger (1995) in their early influential work provide evidence for such spillovers. Recent work by Engbom and Moser (2021) on an increase in the minimum wage in Brazil also finds large positive spillover effects. On the other side, Autor et al. (2016) think these "spillovers" are simply due to measurement error.

6 Results

We estimate our preferred specifications using the 10th percentile as the deflator including county fixed effects, separately, for each year 1960, 1970, 1980, 1990, and 2000. We initially focus on the p10-p90 measure of inequality as well as its two "subcomponents": p10-p50 and p50-p90. For all of these, we put the lower percentile first in the difference so a positive value for the effect means that a higher bracket causes a decline in inequality. Note that even though p10-p90 is exactly equal to the sum of p10-p50 and p50-p90, the regression coefficients for p10-p50 and p50-p90 do not need to add up to the total effect for p10-p90 since we do not impose the adding up restriction in the estimation.

Figure 12 plots these effects by year and 2 standard deviation error bands. We find a statistically significant effect on the p10-p90 in 1960 and 1970. This suggests that a higher bracket during WWII was associated with a relative increase in income inequality within an occupation in between 1940 and 1960 or 1970. To interpret the magnitude of these results in 1960, we consider the effect of moving from the median of the distribution of the deflated (by the 10th percentile) bracket to the 5th percentile of this distribution. Weighted by the size of county-occupation cells, this value is 1.28 at the median and 0.85 at the 5th percentile. Given the point estimate in column (3), moving to the lower brackets, which most approximates the pre-NWLB wage structure being frozen by the NWLB as raises would have been largely forbidden, would be associated with a 0.17 higher value for the change in the log 10-50 ratio, relative to an overall fall of 1.173 in the log difference. By 1980, the point estimate for the effect on p10-p90 is no longer statistically significant and remains so through 2000, the end of our sample. Furthermore, in 1980 and 2000, we can rule out moderately sized effects in both directions.

The effects on p10-p90 are driven by effects at the bottom of the income distribution as measured by the p10-p50 rather than effects at the top as measured by the p50-p90. On the other hand, the results for p50-p90 here are nearly zero, statistically and economically, in our preferred specification across all the years. That is, higher brackets relative to the 1940 distribution are not associated with any change in the upper tail of the within-occupation-county distribution of income. This is consistent with the nature of controls during the war. While "measurement error" in the brackets may have set them "too" high or low relative to the underlying distribution, it is unlikely any were set in the upper half of the distribution, so there should not have been any direct effects of the brackets on that component of inequality.

These results accord with the within-industry results from Goldin and Margo (1992). Using fifteen BLS studies, they compare the prewar, wartime, and postwar periods. While both the 10-50 ratios and 50-90 ratios compress, the timing is different. The compression in the lower tail happens during the war, consistent with our results which find a role for the NWLB in the lower tail. The upper tail compression, which continued after the war, is less plausibly driven by the NWLB bracket policy.

Figure 13 shows the effects of the brackets on (minus the) IQR again using p10 as the deflator. Due to disclosure limitations (at this moment), we cannot report the effects for the subcomponents of p25-p75 like the p10-p90 case. Similar to the results for the p10-p90, the brackets lead to a relative rise in inequality as measured by the IQR in 1960 and 1970 though, in this case, the negative effect persists through 1980. The effect is no longer statistically significant by 1990 and is also economically insignificant in 2000.

6.1 Robustness Check: Varying the Deflator

We provided some reasons based on the historical record for using the p10 of the 1940 wage distribution as the deflator of the bracket, but it is reasonable to explore other choices. Figure 14 redoes Figure 12 using p25 instead as the deflator. The effects over time are very similar to the original case with effects detectable through 1970. The only relevant difference is that in this case, there is a statistically significant effect on the upper tail of the income distribution in 1960. Figure 13 shows that in the case of the IQR as the dependent variable, the choice of the deflator does affect the magnitudes of the effect and, to a certain extent, the statistical significance over time. That is, relative to the p10 case, effects using the p25 are still statistically significant through the end of the sample and the magnitudes are at least 3 times larger. Taken as a whole, the choice of the deflator does not affect the qualitative patterns of the effects over time.

6.2 Robustness Check: Varying the Fixed Effects

We now consider the effects of varying the set of fixed effects included in the regressions. Besides our preferred specification, which just includes county fixed effects, we also estimate the results with no fixed effects as well as one with occupation and county fixed effects. We continue to include the value of the inequality measure in 1940 and that measure squared as controls in all of these regressions.

Figures 15, 16, 17, and 18 show the results of the different specifications for the p10-p90, p10-p50, p50-p90, and p25-p75, respectively. We find that the effects from the specification with no fixed effects do not differ substantially from our preferred specification. This is true across years and all inequality measures of the labor income distribution except p50-p90. As for those that also include county fixed effects, we broadly find the same pattern for effects in terms of magnitude and statistical significance through 1980. With this more saturated model, the negative effect of the brackets on p10-p90 returns in 1990 and 2000 driven by the effect on the p10-p50 in those years. As for the p25-p75, the attenuation of the effects between 1960 is not nearly as pronounced as in our preferred specification.

6.3 Mechanisms of Persistence

Across a variety of specifications, it is clear that the brackets continued to effect the earnings distribution, at least, through 1960 and perhaps until 1980. What is the mechanism for these persistent effects that last over a decade after the end of the war? In our view, the central mechanism is the way in which the brackets patterned wage setting in the rest of the 20th century through both public and private wage setting mechanisms

As for public wage setting, with the US entrance into the Korean War, the federal government setup an agency to regulate wages just like the NWLB in WWII. Furthermore, the Korean War Wage Stabilization Board used a similar rule to the "Little Steel" formula to regulate wage increases: "The basic policy of the Wage Stabilization Board during the Korean War period was to permit wage increases up to a point not higher than 10 per cent. above the level prevailing on 15 January 1950, which was the equivalent of the advance in the cost of living" (Muntz, 1955). The Korean War version of the NWLB would have sustained and deepened the effects of the WWII NWLB through, at least, the end of the war in 1953. Though focused on between occupational inequality, Keat (1960) highlights the still compressed earnings distribution in 1956, 3 years after the end of the war, relative to 1900. Unfortunately, we do not have information on the actual wages set during the Korean War so we cannot directly test this hypothesis.

As for private wage setting, one mechanism for a continuing effect of the war-era wage controls is through the bracket's effects on union wage schedules. Following the war and in response to the compression in wages among blue collar workers, higher skilled craft workers begin to argue for "craft severance" to the National Labor Relations Board, a process by which skilled workers would no longer be covered by broad industrial unions (Etheridge, 2020). In a case study, he contrasts pattern makers, who were granted severance in 1941, with millwrights, who were kept in the industrial union. "After wartime wage controls lapsed, however, the pattern makers used the autonomy their craft bargaining unit gave them to negotiate an increase in skill-based wage differentials. Meanwhile, the millwrights, still members of the larger industrial bargaining unit, lost ground relative to their unskilled coworkers".

Levitan (1951) surveyed sixty unions about their experiences after the war, and a number of them reported that the postwar wage structure was influenced by the wage controls: "Three years after the War Labor Board ceased to exist, a number of unions found that it had left definite imprints upon the postwar job evaluation and individual wage rate structures in their respective industries. This seems particularly true of the steel industry. The War Labor Board served as a catalyst in stimulating the formulation of a much-needed job classification and rational wage rate structure in the steel industry." While this "rational wage rate structure" was surely undone over decades, this survey suggests that the period in which the NWLB acted as a "treatment" was arguably longer than the administrative history of the agency would suggest. It is intriguing that the effect fades out as the percent of private sector workers in a union begins to fall most sharply

after 1970.

The brackets could have also affected private wage setting mechanism through their effects on what is perceived as a "fair" wage. Thurow, in fact, seemed to think that these "differentials became the new standard of relative deprivation and were regarded as 'just' even after the egalitarian pressures of WWII had disappeared." Piketty and Saez, in their study of top income inequality, also mention a similar mechanism when they write that "World War II ha[d] without doubt had a profound effect on labor market institutions and more generally on social norms regarding inequality." They point to the policies of the Great Society as evidence for these shifting views on the appropriate level of inequality. Similar claims were made in Goldin and Margo as well as Goldin and Katz and much earlier by Brown (1977). However, It is very difficult to provide direct evidence for this theory.

7 Conclusion

High levels of economic inequality in the US and other western countries continue to be a concern for policy makers. In response to this, economists have developed a number of policy prescriptions. For example, Atkinson (2015) has proposed a "national pay policy" involving higher minimum wages and a "code of practice" for pay above the minimum to address the historic levels of inequality. However, besides studies of the minimum wage, there is very little evidence on the consequences of these types of policies aimed at reducing inequality. The NWLB is one of a few such American examples attempted at a national scale and attempting to cover the whole economy.

We find that the NWLB bracket policy did have enduring effects on the within occupation distribution of income at least through 1960, 15 years after the end of the war. However, the effects suggest that this policy did not lower inequality. Instead, higher brackets led to higher levels of inequality. These effects are (plausibly) concentrated in the lower parts of the earning distribution. We argue that this result is not implausible given the multifaceted nature of the NWLB.

The key question that remains to be fully answered is through what channels the brackets had persistent effects at least a decade after the end of this policy. In the "short-term", the general policy of the NWLB was embodied in the wage setting approach taken by the federal government during the Korean War in the 1950s. In the longer-term, we have suggested that wage distribution induced by the brackets functioned as the reference point for bargaining between unions and employers. While direct evidence is still lacking, the possibility that unions mediated the effects of the NWLB hints at another way in which the decline of private sector unions might have affected inequality in the second half of the 20th century.

Going forward, we plan to broaden our analysis to examine non-economic outcomes. For example, did the leveling effects of the NWLB have political consequences? What were the intergenerational consequences of this policy? Did children whose parents were affected by this policy

have different later life outcomes in the form of educational attainment, for example, than those who were not affected? Finally, what were the long-run consequences of the NWLB for the regional development of the American economy?

References

Acemoglu, Daron, David H. Autor, and David Lyle, "Women, War, and Wages: The Effect of Female Labor Supply on the Wage Structure at Midcentury," *Journal of Political Economy*, 2004, *112*, 497–551.

Aizer, Anna, Ryan Boone, Adriana Lleras-Muney, and Jonathan Vogel, "Discrimination and Racial Disparities in Labor Market Outcomes: Evidence from WWII," 2020. NBER WP 27689.

Atkinson, Anthony B., *Inequality: What Can Be Done?*, Harvard University Press, 2015.

Autor, David H., Alan Manning, and Christopher L. Smith, "The Contribution of the Minimum Wage to US Wage Inequality over Three Decades: A Reassessment," *American Economic Journal: Applied Economics*, 2016, *8*, 58–99.

_ , Lawrence F. Katz, and Melissa S. Kearney, "Trends in US Wage Inequality: Revising the Revisionists," *Review of Economics and Statistics*, 2008, *90*, 300–323.

Bailey, Martha J., John DiNardo, and Bryan A. Stuart, "The Economic Impact of a High National Minimum Wage: Evidence from the 1966 Fair Labor Standards Act," 2020. NBER WP 26926.

Bound, John and George Johnson, "Changes in the Structure of Wages in the 1980s: An Evaluation of Alternative Explanations," *American Economic Review*, 1992, *82*, 371–392.

Brown, Henry P., *The Inequality of Pay*, Oxford University Press, 1977.

Card, David and Alan Krueger, *Myth and Measurement*, Princeton University Press, 1995.

_ , Thomas Lemieux, and W. Craig Riddell, "Unions and Wage Inequality," *Journal of Labor Research*, 2004, *25*, 519–559.

Chodorow-Reich, Gabriel, John Coglianese, and Loukas Karabarbounis, "The Macreconomics Effects of Unemployment Benefit Extensions: A Measurement Error Approach," *Quarterly Journal of Economics*, 2019, *134*, 227–279.

Clinard, Marshall, *Black Market: A Study of White Collar Crime*, Patterson Smith, 1952.

Collins, William J., "Race, Roosevelt, and Wartime Production: Fair Employment in World War II Labor Markets," *American Economic Review*, 2001, *91*, 272–286.

Derber, Milton, "The War Labor Board: An Experiment in Wage Stabilization: Comment," *American Economic Review*, 1944, *34*, 572–575.

DiNardo, John, Nicole Fortin, and Thomas Lemieux, "Labor Market Institutions and the Distribution of Wages, 1973–1992: A Semi-Parametric Approach," *Econometrica*, 1996, *64*, 1001–1044.

Douty, H.M., "The Development of Wage-Price Policies," in "Problems and Policies of Dispute Settlement and Wage Stabilization During World War II," United States Department of Labor, 1950.

Engbom, Niklas and Christian Moser, "Earnings Inequality and the Minimum Wage: Evidence from Brazil," 2021. NBER WP 28831.

Etheridge, Bryant, "Contesting the Great Compression: The National Labor Relations Board and Skilled Workers' Struggle to Control Wage Differentials, 1935–1955," *Journal of Policy History*, 2020, *32*, 183–213.

Evans, Paul, "The Effects of General Price Controls in the United States during World War II," *Journal of Political Economy*, 1982, *90*, 944–956.

Farber, Henry S., Daniel Herbst, Ilyana Kuziemko, and Suresh Naidu, "Unions and Inquality Over the Twentieth Century: New Evidence from Survey Data," *Quarterly Journal of Economics*, 2021, *136*, 1325–1385.

Ferrara, Andreas, "World War II and Black Economic Progress," 2020. Unpublished, University of Pittsburgh.

Galbraith, John Kenneth, *A Theory of Price Control*, Harvard University Press, 1952.

Goldin, Claudia, "The Role of WWII in the Rise of Women's Employment," *American Economic Review*, 1991, *81*, 741–756.

_ **and Robert A. Margo**, "The Great Compression: The Wage Structure in the United States at Mid-Century," *Quarterly Journal of Economics*, 1992, *107*, 1–34.

Goldin, Claudia D. and Lawrence F. Katz, *The Race Between Education and Technology*, Harvard University Press, 2009.

Hachenburg, Robert, "The National War Labor Board: The Evolution of a National Wage Policy," *University of Pennsylvania Law Review and American Law Register*, 1942, *91*, 340–357.

Jaworski, Taylor, "You're in the Army Now: The Impact of World War II on Women's Education, Work, and Family," *Journal of Economic History*, 2014, *74*, 169–195.

_ , "World War II and the Industrialization of the American South," *Journal of Economic History*, 2017, *77*, 1048–1082.

Juhn, Chinhui, Kevin M. Murphy, and Brooks Pierce, "Wage Inequality and the Rise in Returns to Skill," *Journal of Political Economy*, 1993, *101*, 410–442.

Katz, Lawrence F. and Kevin M. Murphy, "Changes in Relative Wages, 1963–87: Supply and Demand Factors," *Quarterly Journal of Economics*, 1992, *107*, 35–78.

Keat, Paul G., "Long-Run Changes in Occupational Wage Structure, 1900-1956," *Journal of Political Economy*, 1960, *68*, 584–600.

Lee, David S., "Wage Inequality in the United States During the 1980s: Rising Dispersion or Falling Minimum Wage?," *Quarterly Journal of Economics*, 1999, *114*, 977–1023.

Levitan, Sar A., "Union Attitudes Toward Job Evaluation and Ingrade Progression," *ILR Review*, 1951, *4*, 268–274.

McCartin, Joseph A., *Labor's Great War: The Struggle for Industrial Democracy and the Origins of Modern American Labor Relations, 1912-1921*, UNC Press Books, 1997.

McNatt, E. B., "Toward a National Wartime Labor Policy: The Wage Issue," *Journal of Political Economy*, 1943, *51*, 1–11.

Muntz, Earl E, "The Decline in Wage Differentials Based on Skill in the United States," *International Labor Review*, 1955, *71*, 575.

Nuemark, David and Peter Shirley, "Myth or Measurement: What Does the New Minimum Wage Research Say About Minimum Wages and Job Loss in the United States?," 2021. NBER WP 28388.

Piketty, Thomas and Emanuel Saez, "Income Inequality in the United States, 1913-1998," *Quarterly Journal of Economics*, 2003, *118*, 1–39.

Putnam, Bertha Haven, "Maximum Wage-Laws for Priests after the Black Death, 1348-1381," *American Historical Review*, 1915, *21*, 12–32.

Record, Jane Cassels, "The War Labor Board: An Experiment in Wage Stabilization," *American Economic Review*, 1944, *34*, 98–110.

_ , "The War Labor Board: An Experiment in Wage Stabilization: Comment: Reply," *American Economic Review*, 1944, *34*, 575–577.

Rockoff, Hugh, "The Response of the Giant Corporations to Wage and Price Control in World War II," *Journal of Economic History*, 1981, *41*, 123–128.

_ , *Drastic Measures: A History of Wage and Price Controls in the United States*, Cambridge University Press, 1984.

Rose, Evan K., "The Rise and Fall of Female Labor Force Participation During World War II in the United States," *Journal of Economic History*, 2018, *78*, 673–710.

Rose, Jonathan, "Government Interventions in Times of Economic Crisis." PhD dissertation, University of California, Berkeley 2009.

Shatnawi, Dina and Price V. Fishback, "The Impact of World War II on the Demand for Female Workers in Manufacturing," *Journal of Economic History*, 2018, *78*, 539–574.

Stubbs, William, *The Constitutional History of England*, Clarendon Press, 1875.

Taylor, Matthew, "Beyond the Maximum Wage: The Earnings of the Football Professionals in England, 1900-1939," *Soccery & Society*, 2001, *2*, 101–118.

Thurow, Lester, *Generating Inequality: Mechanisms of Distribution in the U.S. Economy*, Basic Books, 1975.

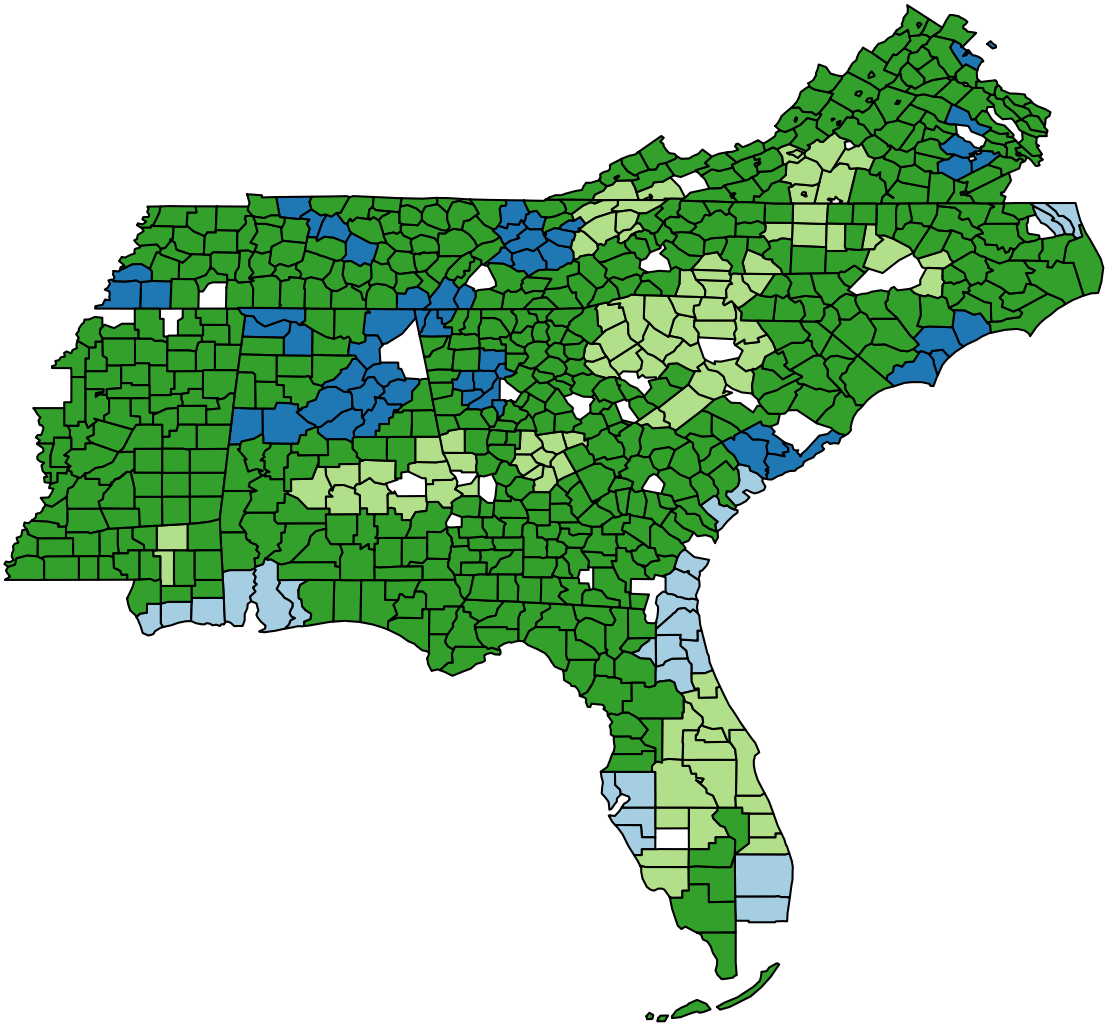
United States Department of Labor, *The Termination Report of the National War Labor Board*, United States Department of Labor, 1949.

Vatter, Harold G., *The U.S. Economy in World War II*, Columbia University Press, 1985.

Western, Bruce and Jake Rosenfeld, "Unions, Norms, and the Rise in US Wage Inequality," *American Sociological Review*, 2011, *76*, 513–537.

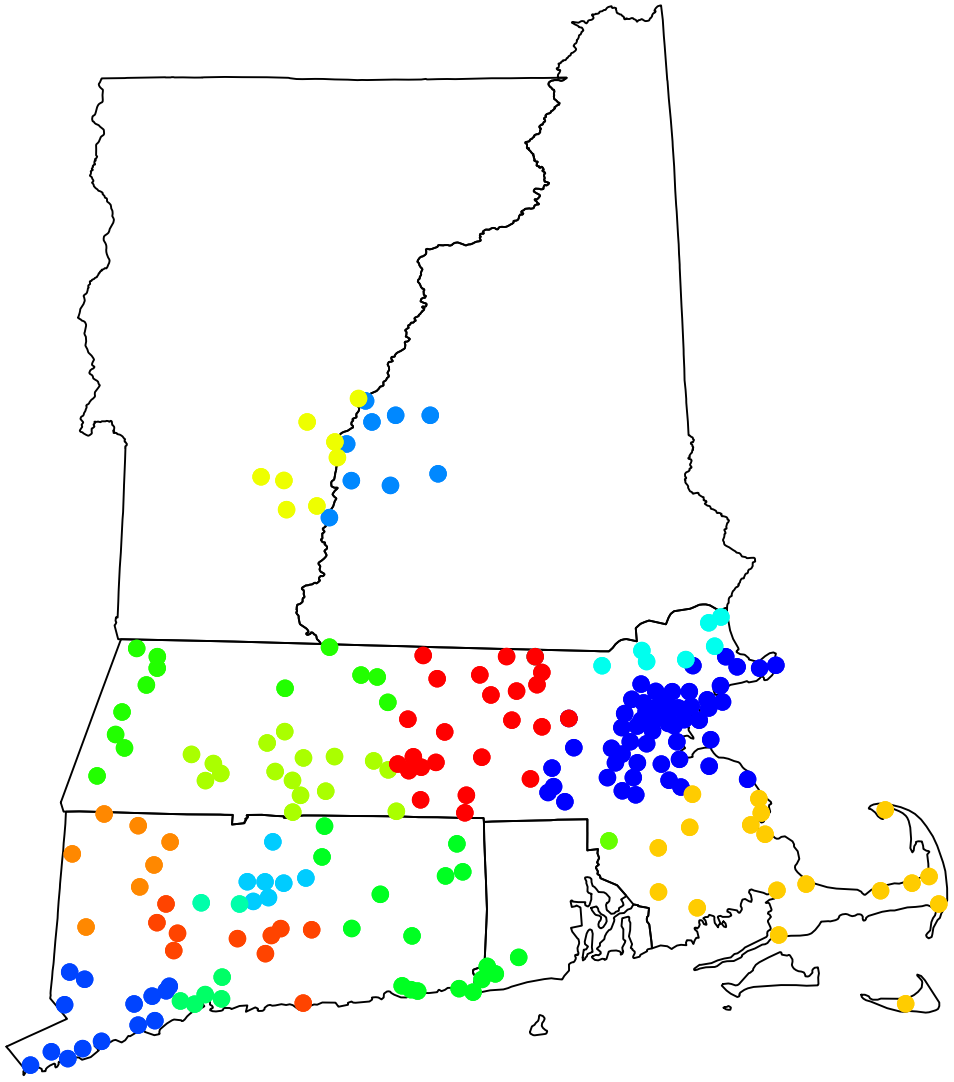
Witte, Edwin E., "American Experience with Wage Stabilization," *Wisconsin Law Review*, 1952, p. 398.

Figure 1: Map of Region IV



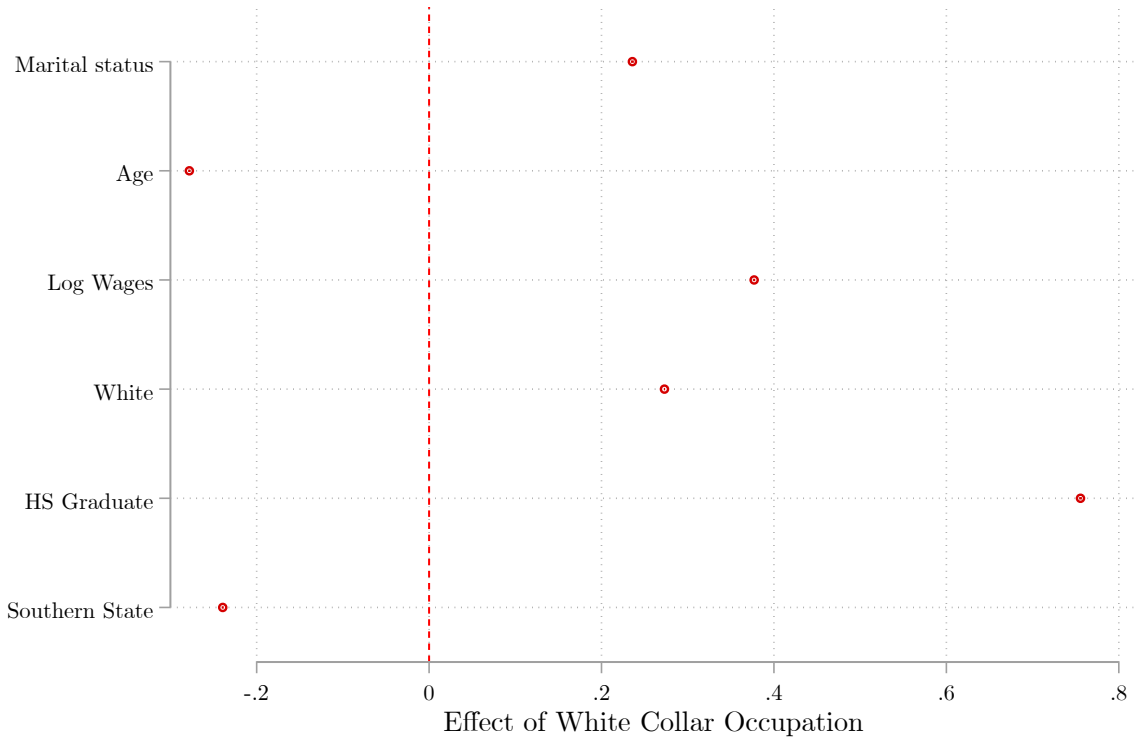
Notes: In Region IV, geographic divisions for brackets are defined at the county-level. Counties were divided into "A", "B", "C", and "D" zones, with different brackets defined for each covered occupation for each zone.

Figure 2: Map of Region I (Metal Trades)



Notes: In this region, divisions are defined at the town-level. Only the towns for the metal trades industries are plotted; other industries had different assignments of towns to NWLB zones. Each color represents a metropolitan area to which towns were assigned as a labor market.

Figure 3: Comparison of White Collar Occupations to Population

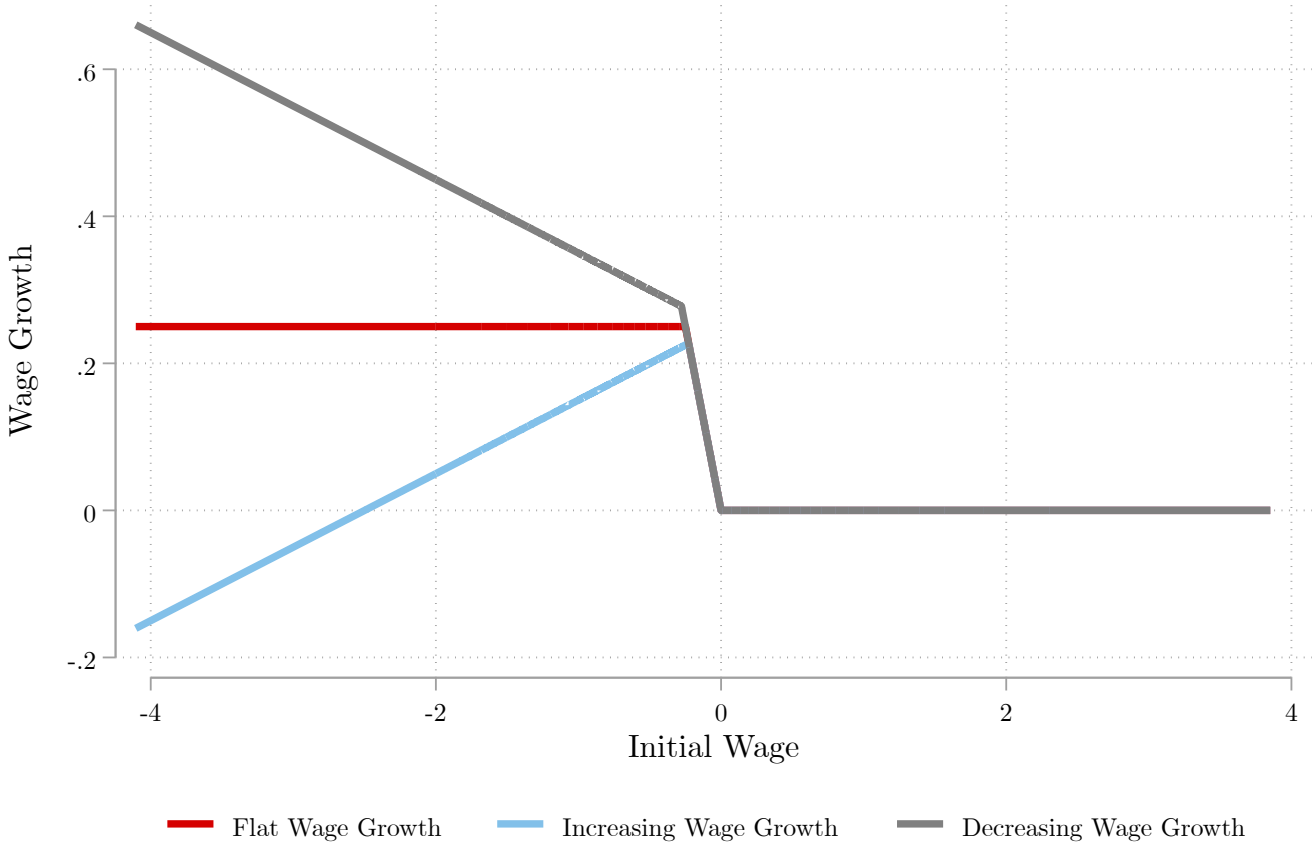


Notes: The population is men 18-64 who work at least part-time in 1940. Standard errors are robust, but are not visible due to the large sample size.

Figure 4: Comparison of Metal Trades Occupations to Population

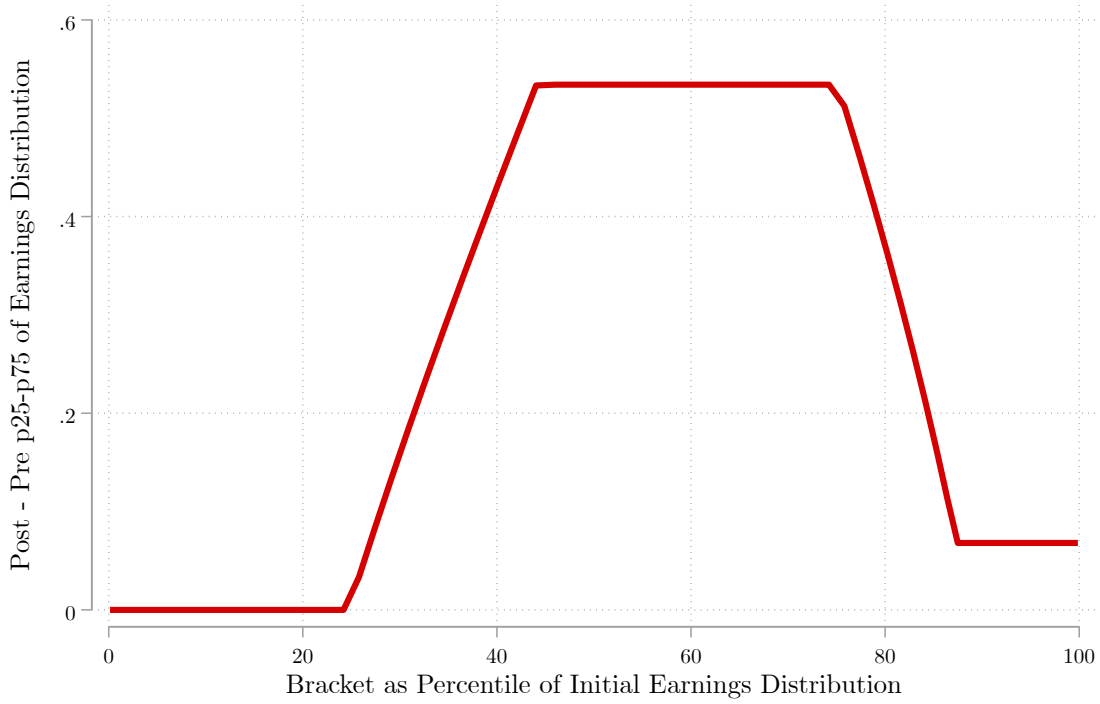
Notes: The population is men 18-64 who work at least part-time in 1940 . Standard errors are robust, but are not visible due to the large sample size.

Figure 5: The Effects of the Bracket on Wage Growth



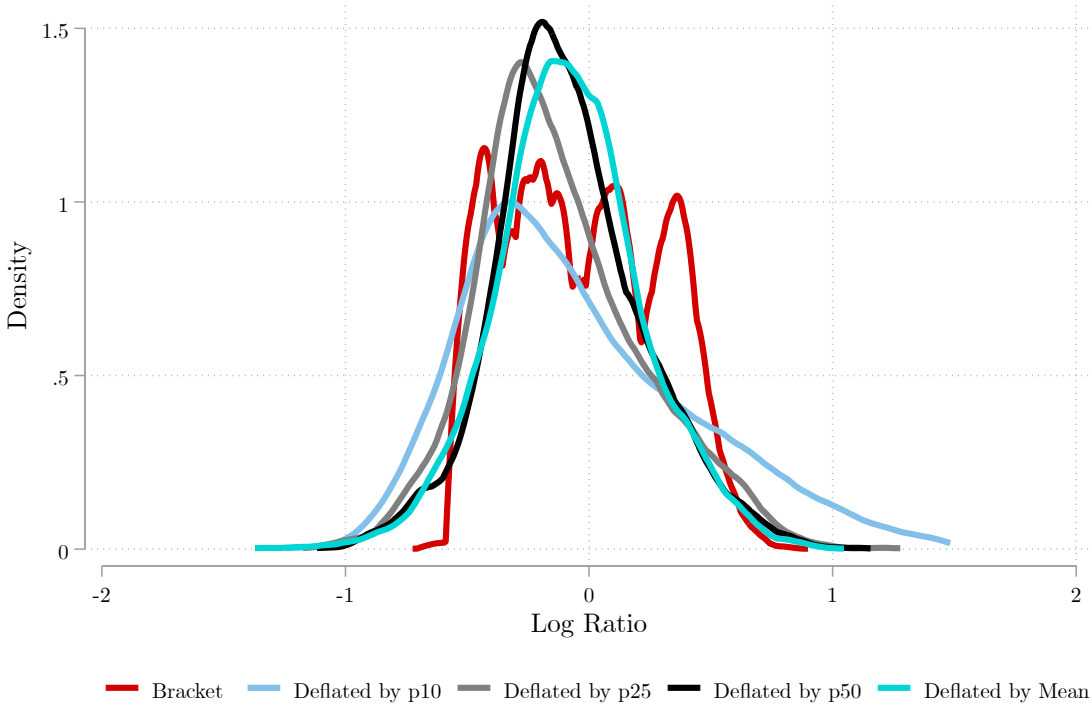
Notes: This figure simulates three scenarios: (1) flat wage growth; (2) (linearly) increasing wage growth in the initial wage; and (3) decreasing wage growth. In all 3 scenarios, the (unconstrained) average wage growth is the same. We normalize the value (of the log) of the bracket to 0.

Figure 6: Simulated Effects of the Bracket on Inequality



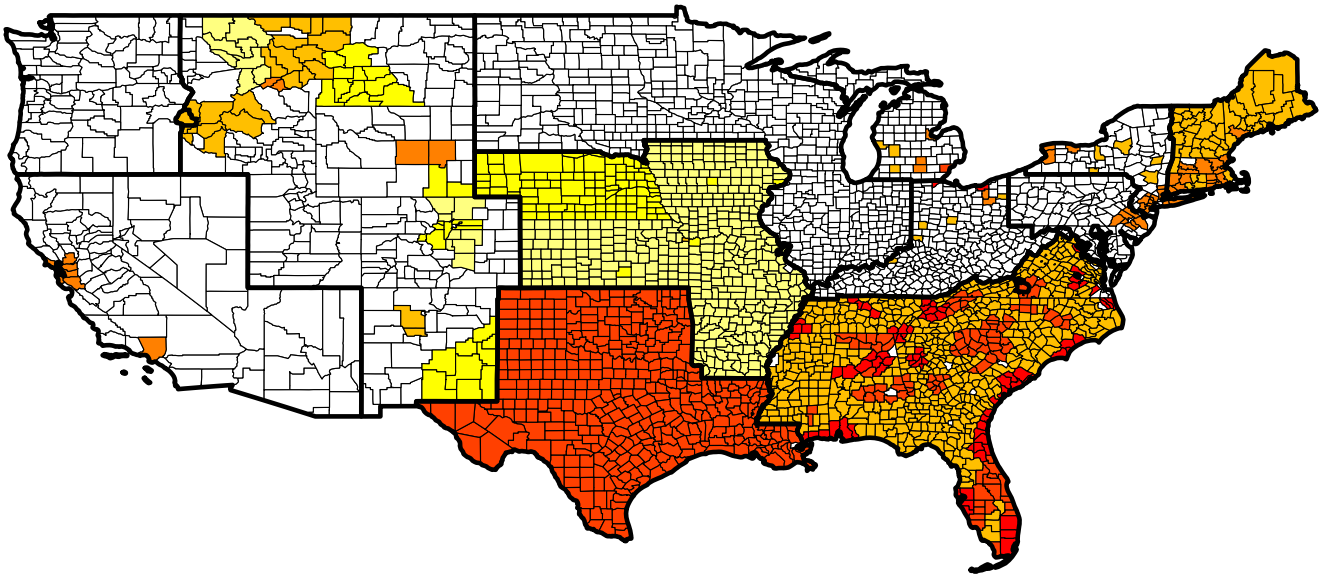
Notes: We assume that wage growth is linearly decreasing in the initial wage. This implies that inequality in latent earnings is falling. We also assume that the initial distribution of log wages is a standard normal. We plot the difference between p25 and p75 so that an increase in this value reflects a *decrease* in inequality.

Figure 7: Distribution of Deflated Brackets



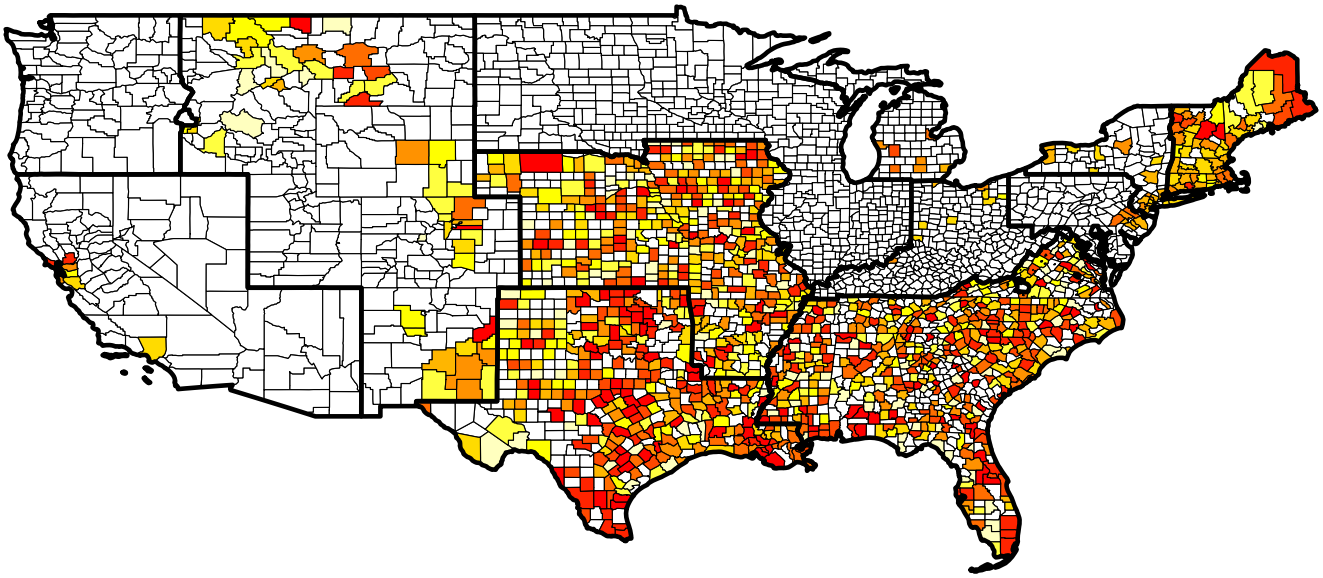
Notes: All distributions are centered. The brackets are deflated by various statistics of the 1940 hourly wage distribution. The hourly wage is computed using the reported values for wage earnings, hours worked per week, and weeks worked in the Census to compute this. We do not include values larger than 1.5 for the sake of clarity.

Figure 8: Brackets for Stenographers



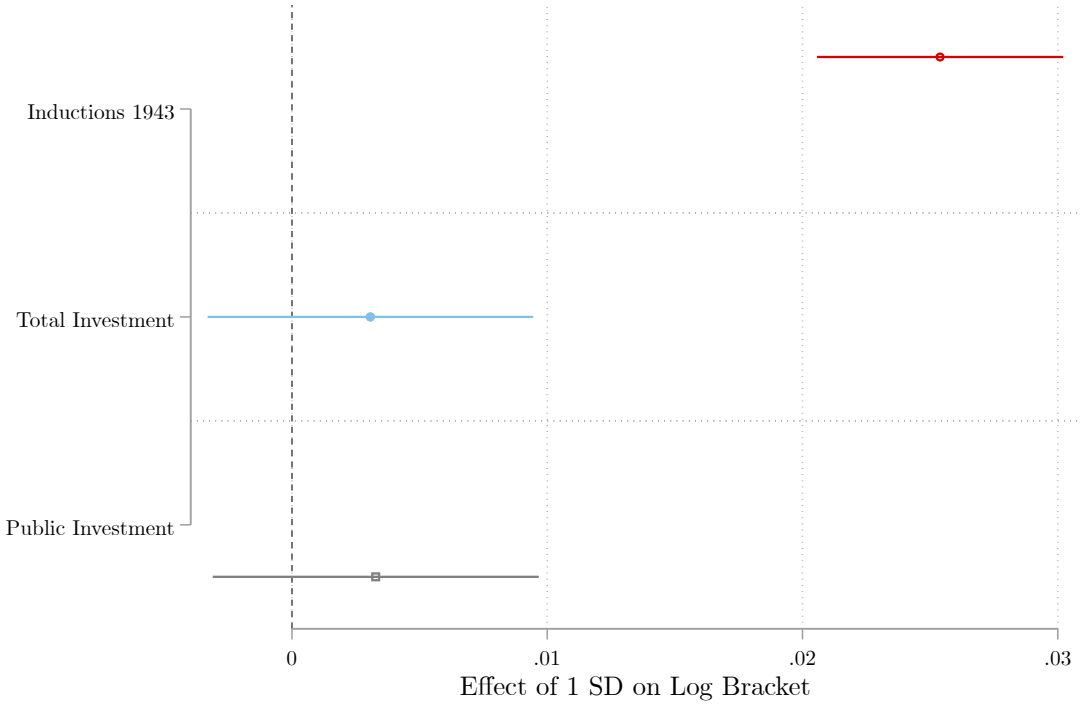
Notes: Region I, which covers the northeast, defines brackets at the town-level. So a shaded county in that region means that the county includes a city with a bracket assigned. Not all towns are listed in the brackets though and this is why some counties are unshaded. For the other regions, brackets are assigned at the county-level. A county in white denotes a missing value because we are missing the bracket. The thick black lines represent borders of NWLB regions.

Figure 9: Brackets Relative to the 10th Percentile of Earnings in 1940 for Stenographers



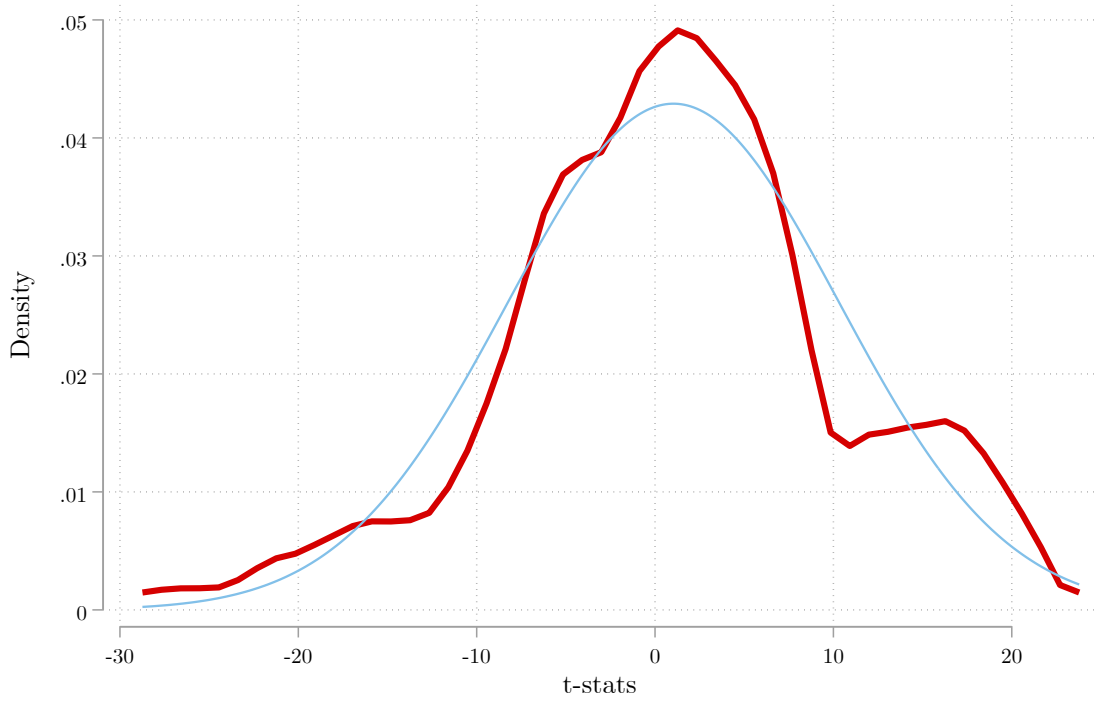
Notes: 1940 hourly earnings are adjusted for the growth in manufacturing wages between 1940 and 1945. Darker colors indicate higher values for the bracket relative to the 10th percentile. A county in white denotes a missing value, either because we are missing the bracket or the county did not have any stenographers in 1940. The thick black lines represent borders of NWLB regions.

Figure 10: Predicting the Level of the Brackets Using War Effort Variables



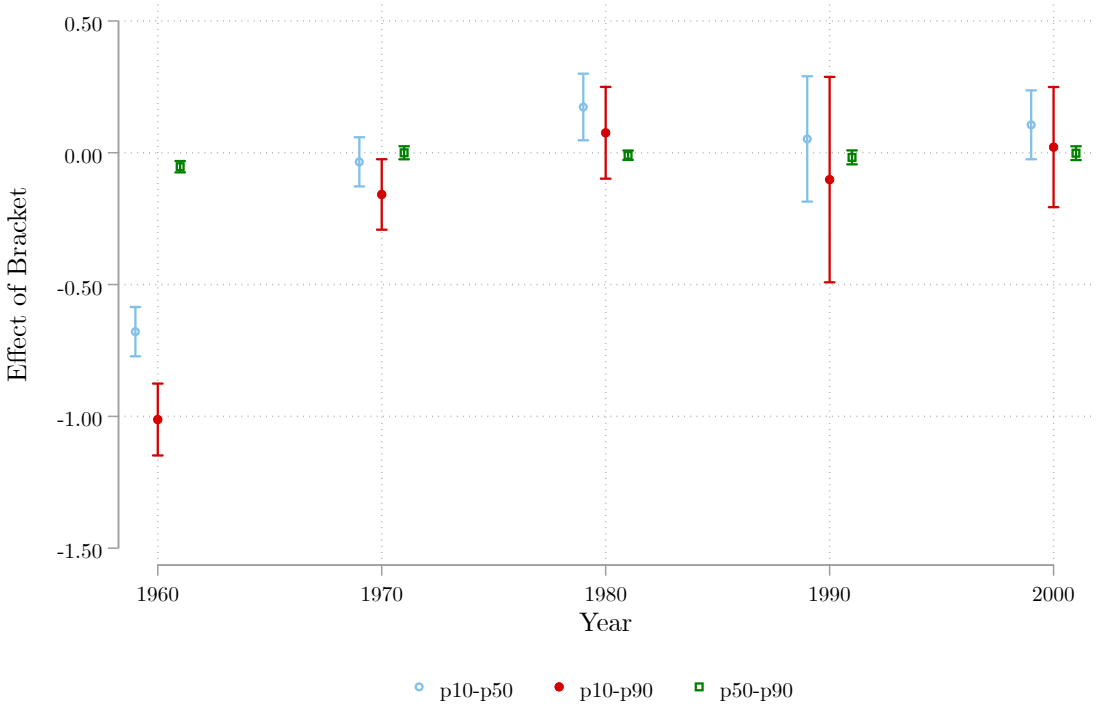
Notes: The state-level variable "Inductions 1943" is from 1943 and log transformed. The county-level variables "Total Investment" and "Public Investment" are measured in $100,000s and in levels. All regressions include the p10, p25, and p50 of the 1940 hourly wage distribution at the occupation-county level. Standard errors are robust.

Figure 11: Distribution of Occupation Specific Effects of War Effort Variables



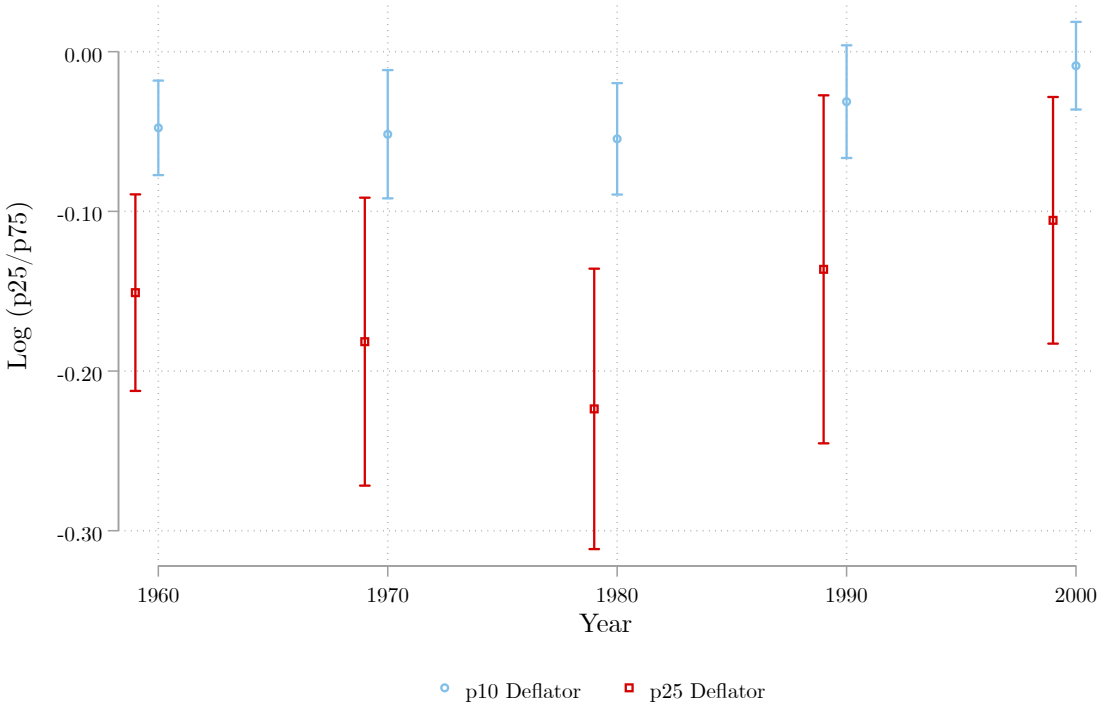
Notes: The t-statistics are for the coefficients on occupation-specific effects of the war related effort variables in Figure 10. The coefficients are centered by the overall effect of a given war effort measure. Standard errors used in computing the t-statistics are robust. We ignore the sampling uncertainty in the estimate of the overall effect. The blue line is the best fitting normal distribution.

Figure 12: Effects of the Bracket Policy on p10-p90 of Labor Income: p10 Deflator



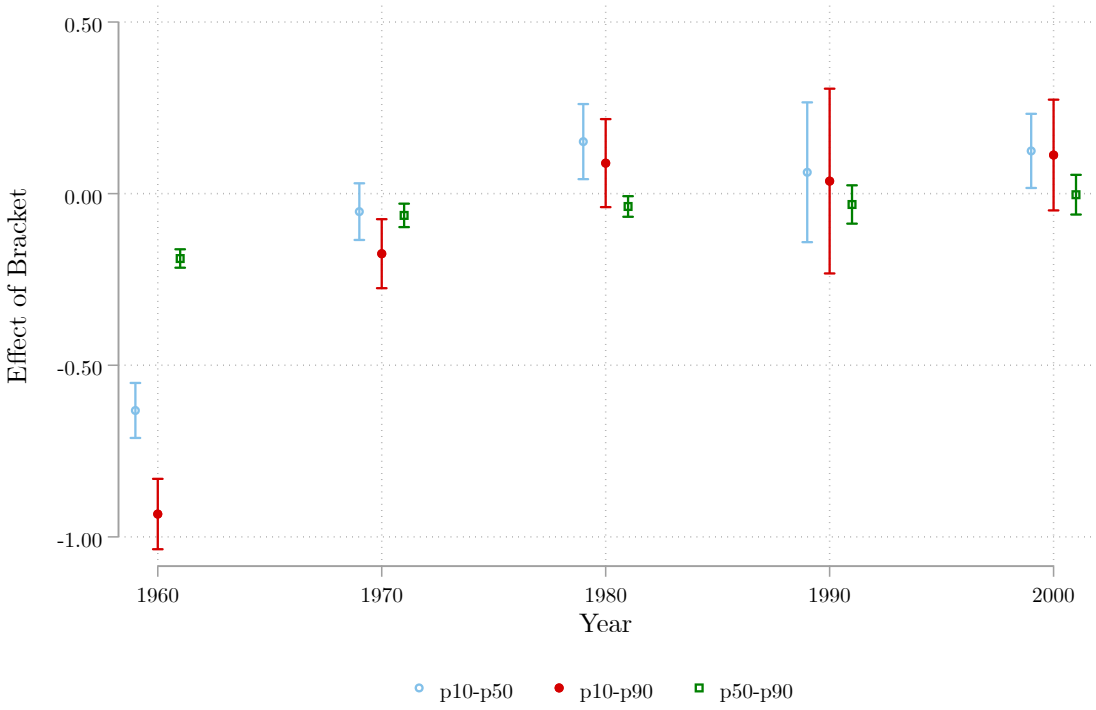
Notes: The dependent variable is the difference between the value of the statistic of log labor income in the given year and 1940 and subsequent value. Regressions include the inequality value in 1940 and its square as well as occupation fixed effects. The bracket is deflated by the 10th percentile of the 1940 wage distribution by occupation and county. The sample used is men 18 to 65 years of age who are employed at least part-time. Standard errors are robust.

Figure 13: Effects of the Bracket Policy on p25-p75 of Labor Income by Deflator



Notes: The dependent variable is the difference between the value of the statistic of log labor income in the given year and 1940 and subsequent value. Regressions include the inequality value in 1940 and its square as well as occupation fixed effects. The sample used is men 18 to 65 years of age who are employed at least part-time. Standard errors are robust.

Figure 14: Robustness Check of Effects on p10-p90 of Labor Income: p25 Deflator

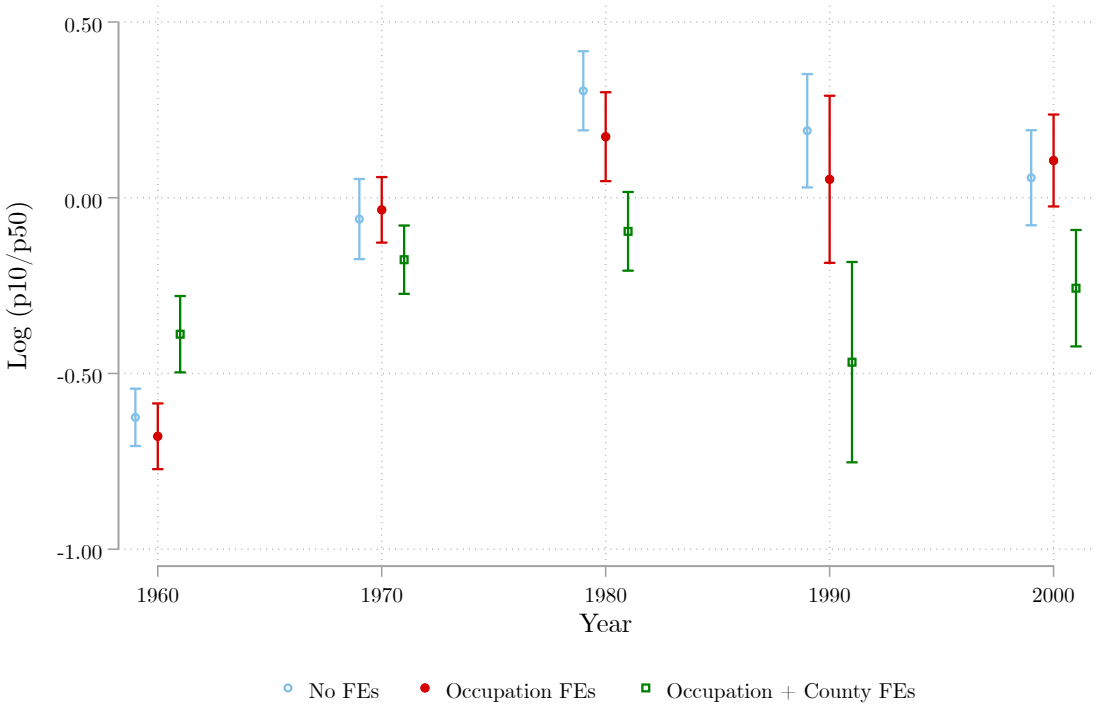


Notes: The dependent variable is the difference between the value of the statistic of log labor income in the given year and 1940 and subsequent value. Regressions include the inequality value in 1940 and its square as well as occupation fixed effects. The bracket is deflated by the 25th percentile of the 1940 wage distribution by occupation and county. Our preferred specification is the one with occupation fixed efects. The sample used is men 18 to 65 years of age who are employed at least part-time. Standard errors are robust.

Figure 15: Robustness Checks of Effects on p10-p90 of Labor Income

Notes: The dependent variable is the difference between the value of the statistic of log labor income in the given year and 1940 and subsequent value. Regressions include the inequality value in 1940 and its square. The bracket is deflated by the 10th percentile of the 1940 labor income distribution by occupation and county. Our preferred specification is the one with occupation fixed effects. The sample used is men 18 to 65 years of age who are employed at least part-time. Standard errors are robust.

Figure 16: Robustness Checks of Effects on p10-p50 of Labor Income



Notes: The dependent variable is the difference between the value of the statistic of log labor income in the given year and 1940 and subsequent value. Regressions include the inequality value in 1940 and its square. The bracket is deflated by the 10th percentile of the 1940 labor income distribution by occupation and county. Our preferred specification is the one with occupation fixed effects. The sample used is men 18 to 65 years of age who are employed at least part-time. Standard errors are robust.

Figure 17: Robustness Checks of Effects on p50-p90 Labor Income

Notes: The dependent variable is the difference between the value of the statistic of log labor income in the given year and 1940 and subsequent value. Regressions include the inequality value in 1940 and its square. The bracket is deflated by the 10th percentile of the 1940 labor income distribution by occupation and county. Our preferred specification is the one with occupation fixed effects. The sample used is men 18 to 65 years of age who are employed at least part-time. Standard errors are robust.

Figure 18: Robustness Checks of Effects on p25-p75 Labor Income

Notes: The dependent variable is the difference between the value of the statistic of log labor income in the given year and 1940 and subsequent value. Regressions include the inequality value in 1940 and its square. The bracket is deflated by the 10th percentile of the 1940 labor income distribution by occupation and county. Our preferred specification is the one with occupation fixed effects. The sample used is men 18 to 65 years of age who are employed at least part-time. Standard errors are robust.

Table 1: Comparing Matched to Unmatched Sample: 1960

	(1) Age	(2) Married	(3) Log Wages	(4) White	(5) HS Grad.
In Sample	$-$	$----$	$++++$	$++++$	$++++$
N	8.4M	8.4M	8.4M	8.4M	8.4M

Notes: The sample used is men 18 to 65 years of age who are employed at least part-time. Standard errors are robust. $+/-$ means $p > 0.1$, $++/--$ means $p < 0.1$, $+++/---$ means $p < 0.05$, $++++/----$ means $p < 0.01$. Sample sizes have been rounded.

Table 2: Predicting the Brackets Using the 1940 Hourly Wage Distribution

	Bracket			
	(1)	(2)	(3)	(4)
p10 Adjusted 1940 Hourly Wages	0.174***			0.006
	(0.005)			(0.005)
p25 Adjusted 1940 Hourly Wages		0.213***		0.001
		(0.006)		(0.004)
p50 Adjusted 1940 Hourly Wages			0.271***	0.025**
			(0.007)	(0.010)
Observations	10127	11585	11987	10124

Notes: The sample used is men 18 to 65 years of age who are employed at least part-time.The statistics are for the 1940 hourly wage distribution. The hourly wage is computed using the reported values for wage earnings, hours worked per week, and weeks worked. We then adjust wages for growth in manufacturing wages between 1940 and 1945. We winsorize the 5% tails of the hourly wage distribution. All variables are in logs. Standard errors are robust.