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

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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 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 decreases in within-occupation inequality as measured by the change in the log($Q_{10}/Q_{90}$) and log($Q_{25}/Q_{75}$) ratios between 1939 and 1959 as well as 1969. A one standard deviation increase in the bracket is associated with a 5.7 log point increase in the log($Q_{10}/Q_{90}$) between 1939 and 1959. In an unconditional quantile specification, we find that a 10% increase in the mean level of the brackets would be associated with a 2% higher 10th percentile in 1959, with some partial compensation at higher percentiles. These results provide evidence that lower mid-century inequality was indeed partially a lasting effect of World War II.

Keywords: Wage controls, National War Labor Board, income inequality, WWII.

JEL Codes: N42, J36, D31.

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1 Introduction

Understanding the sources of changes in economic inequality over 20th century is a critical question for economists and policymakers. One potential source for the compression observed in the middle of the century, as suggested by economists, labor historians, and sociologists, is the WWII institution of the National War Labor Board (NWLB). 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.” In their study of the Great Compression, 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 provide some evidence on this institution’s effects by combining original, archival data on NWLB-mandated maximum allowable raises with Census of Population microdata to study the persistent effects of these wage policies.1 While having antecedents in the policy responses to WWI and the Great Depression, the NWLB was a policy experiment of unprecedented scale and scope. At its height, the NWLB ostensibly controlled wages for the vast majority of 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 forbade any raises, but did not require cuts, for 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 the “hardness” of a minimum wage, which legally disallows 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 (assuming no employment effects of the policy). 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 earn 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, not just the bottom of quantiles and can have non-monotonic effects, inferring aggregate effects from estimates of the marginal effects of the brackets is challenging even without any general equilibrium effects.

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. We discuss similarities and differences to his work in greater detail later.
To identify the marginal effects of the brackets on labor earnings inequality in the medium-and long-run, we leverage variation in the level of the brackets relative to preexisting wages. In theory, brackets were set to reflect “sound and tested” wage rates in the middle of 1943, which was interpreted to be the first “cluster” of wages or 90% of the mean wage in a given occupation, industry, and geographic location. In practice, the actual setting of the brackets resulted in substantial variation in the position of the bracket relative to the underlying wage distribution. One source of the variation was due to the fact that 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. Another source of variation is simply discretion on the part of the bureaucrats setting these brackets. The 90% rule was not a legally binding one, and, in fact, there were other ways of determining the bracket based on the “first substantial cluster” of wages. Identification, in the end, is based on the assumption that, after controlling for the 1940 level of inequality as well as county and occupation, variation in the bracket was as good as random.

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 what the values of the brackets were, 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.\(^2\) 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.

Given our design that relies on variation by occupations and counties, 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. Instead, we use the full count decennial Censuses of Population between 1960 and 2000.\(^3\) We fist study the effects on the conditional distribution of earnings by estimating a long difference specification for the change between 1939, the year in which the labor earnings data from 1940 Census refers to, and a subsequent year of the Census in inequality by occupation and county. In our preferred specification, we control for the 1939 value of the inequality measure as well as a full set of occupation and county fixed effects. We benchmark these estimates by running the same specification on a hypothetical distribution of earnings in

\(^2\)The same issue of differences in coding schemes also arises when matching industries.

\(^3\)Unfortunately, there are problems with the available 1950 Census microdata in the Census Research Data Centers, which prevents us from using it.
1943. This distribution is calculated by adjusting 1939 labor earnings for the growth in production worker compensation between 1940 and 1943 and assuming that the brackets are strictly binding on everyone below the bracket. This means that any adjusted wages below the bracket are raised to the value of the bracket, with wages above the bracket frozen after the inflation adjustment.

We find that higher brackets are associated with relative decreases in inequality as measured by the log ratio of the 10th to 90th percentiles of earnings \( \log(Q_{10}/Q_{90}) \) and the log of the interquartile ratio \( \log(Q_{25}/Q_{75}) \) through 1969 (though effects in 1969 are only marginally statistically significant). For example, a one standard deviation increase in the bracket increases \( \Delta \log(Q_{10}/Q_{90}) \) by 5.7 log points between 1939 and 1959, which is about of 1/3 the mean change over the same period. These effects are driven by changes in inequality in the upper part of the labor income distribution, such as the \( \log(Q_{50}/Q_{90}) \) ratio. The effects of the brackets across all inequality measures attenuate over time, and there are no statistically significant effects after 1980. Also reassuring is the fact that the post-war effects are all a fraction of the hypothetical 1943 effects, which show large compressing effects of the brackets.

While having antecedents in the literature, it is difficult to relate the effects from this specification directly to the Great Compression since this strategy recovers the impact on the conditional distribution of earnings. It is true that Goldin and Margo (1992) find that the within occupational component of inequality declined during the Great Compression. Similarly, Juhn et al. (1993) 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. Nonetheless, because of the gap between the conditional and unconditional distributions, we apply a methodology developed by Martínez-Iriarte et al. (2022) to estimate the effects of the brackets on the unconditional distribution of earnings. This approach generalizes the unconditional quantile regressions of Firpo et al. (2009) by allowing for separately identifying two counterfactuals. The first known as the location effect is the effect on the unconditional distribution of labor earnings of a marginal change in the mean of the brackets. The second known as the scale effect is the effect of a marginal change in the standard deviation of the brackets. This effect answers the question of what the distribution of earnings would have looked like if, rather than setting brackets slightly higher, the NWLB had set brackets slightly more uniformly.

We find that in 1959 and 1969, the effect of a marginal increase in the mean of the bracket was to increase the 10th and 25th percentiles while, at the same time, decreasing the 75th and 90th percentiles of the unconditional labor earnings distribution. Concretely, a 10% increase in the bracket increases the 10th percentile of earnings by just over 2% in 1959. Because the effects on the percentiles can be added up, we conclude that the marginal effect of an increase in the mean of the brackets was to reduce inequality in the 1959 and 1969. The scale effects show that if brackets were set slightly more uniformly, inequality in 1959 and 1969 would have been lower. However, these effects are not economically meaningful. A 10% decline in the standard deviation of the brackets would have increased the \( \log(Q_{10}/Q_{90}) \) by only 0.4 log points. Like the within occupation-county
specifications, the location and scale effects attenuate over time and are no longer statistically or economically significant by 1980.

There are a number of reasons to be careful about naively applying our results to explain the Great Compression. We obviously do not capture any general equilibrium effects of the policy, and the effects of the bracket might be non-monotone. We also study a select set of occupations. With these qualifications in mind, our results do provide support for the claim made by Goldin and Margo (1992) that the NWLB played a role in the decline in inequality over the post-war period. They document an increase of about 30 log points in the \( \log(Q_{10}/Q_{90}) \) ratio between 1939 and 1959. As we noted above, a 1 standard deviation increase led to a 7 log point increase in this ratio over the same period, which is almost 1/4 of the change over this period of time. The null effects after 1980 imply that the NWLB could no longer “mask” the rising level of latent earnings inequality in this period. In addition, the “location” of our effects in the income distribution lines up with the additional patterns in Goldin and Margo (1992). For one, they find that, over the first half of the 20th century, inequality at the top and the bottom decline, but the timing is different for the upper and lower tails. The compression in the lower tail is concentrated during the war period, which we cannot exactly address. On the other hand, the upper tail compression continued after the war, which is consistent with our results.

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 changes in inequality occurred along many dimensions including racial and gender differences as well as the education premium. Our work is most related to understanding changes in overall inequality including the sharp decline starting around the beginning of the Depression, reaching a trough in the late 70s or early 80s, and the equally fast increase in inequality since then.

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 different types of 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 these changes in inequality points to institutional changes. 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.

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. 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 that literature difficult. 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 only 13%. This percentage had fallen to 6% by 2012 (Autor et al., 2016).

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).

Finally, our work contributes to the literature on the consequences of WWII for 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 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 while Ferrara (2020) examines variation in the demand for Black labor on racial inequality.

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4See Nuemark and Shirley (2021) for a detailed survey of the literature on the effects of the minimum wage.

5While, 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.
2 History of the NWLB and the 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 itself 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. 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. For example, “[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 finally 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 (relatively) clear basis for NWLB decisions in the second half of 1942.

Even with the Little Steel formula, the NWLB 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. 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 NWLB 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.

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,

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

The NWLB in certain cases also established minimum wages. In the Appendix, we show that the hypothetical effects of these were much smaller than the those of the brackets and, for that reason, we ignore the minimum wages in our empirical analysis.
“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 as our measure of the level of the bracket.

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”.

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.”

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”
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.”

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, Rockoff (1984) suggests that the effects of the WMC on the allocation of labor allocation 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 identify 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.

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.8

2.2 The Bracket Setting Process

As should be clear, the process by which brackets were determined was not totally straightforward or deterministic. Record (1944b, p. 576) claimed that “[t]he bracket minimum […] was 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.”

The NWLB itself conducted an investigation into whether or not the method of bracket determination, cluster versus the 10 percent below rule, affected the level of brackets as part of its Termination Report. The analysis was restricted to Region III (in Philadelphia) for reasons of data availability, in particular access to the BLS surveys in the Washington, DC office of the NWLB. This region used the cluster method, though “not exclusively” (United States Department of Labor, 1949, p. 1176, volume 2). The analysis found “wide difference in individual occupations between the bracket rates set by the 10 percent method and the actual rates set”, but these differences “largely balance themselves” (United States Department of Labor, 1949, p. 1177, volume 2) meaning brackets were not systematically higher or lower under one or the other method.

Besides the methodology for setting the bracket, the geographic division within a division also played an important role in determining who was most affected by the NWLB. To get a sense of this

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8 This could also be interpreted as evidence that violators of the price controls faced minimal risk of being investigated.
variation, Figure 1 maps the brackets for stenographers. 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 prominently in Region IV that covers many of the southern states.

Given that brackets were supposed to be determined as a function of local conditions, we would expect geographic variation. More interesting is whether there was variation in the “bindingness” of the brackets. To measure the bindingness, we calculate the percentile of the brackets in a hypothetical distribution of hourly earnings by occupation and county. A lower percentile means higher degree of bindingness since it means a greater fraction of workers in that group are prevented from receiving a raise. This hypothetical distribution adjusts the distribution of hourly earnings in 1939 reported on the 1940 Population Census for the growth in the earnings of production workers between 1939 and 1943 (Officer and Williamson, 2022). This is obviously a hypothetical distribution since relative earnings by occupations and regions would have presumably changed over this period. We would still argue that this exercise provides insight into how the brackets were actually being specified as a function of conditions on the ground.

Figure 2 plots for white collar and metal trades occupations the distribution of the percentiles of the brackets, weighted by the size of the occupation-WLB zone cells, for Region I (the Northeast) and Region IV (the South), respectively. As a general point of comparison, Rose (2009) finds that for a sample of occupations from California, the brackets were usually between the zeroth and twentieth percentile of 1943 hourly earnings distribution. For our sample, the brackets tend to be much higher in the distribution as well as more dispersed. At least some part of these broad differences might be due to the fact that Rose has more direct information on the 1943 hourly earnings distribution for his (smaller) set of occupations located in a single state.

Comparing across occupations and regions, the brackets in the Northeast, for both sets of occupations, were set lower in the hypothetical wage distribution than were the brackets in the South, suggesting that more individuals in the South would be eligible for raises while wages for more individuals in the North would be constrained by the bracket. Second, in the South, in particular, the brackets for metal trades occupations were set substantially higher in the distribution than those for white collar occupations. Note that because we are using growth in production worker compensation, it is reasonable to believe that the adjustments made to hourly earnings for metal trades workers are more accurate than those of white collar workers. So if anything, one might

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9 This is a cross-industry occupation so there is no variation in the bracket by industry.
10 Hourly earnings was imputed by dividing labor earnings by 40 hours per week times the number of weeks worked.
11 The WLB zone is the geographic level at which the bracket varies.
have imagined that there would be less dispersion and lower levels of bindingness for metal trades, but it is just the opposite.\footnote{These results provide a suggestive explanation for the observed decline in the skill premium in the 1940s and 1950s. If more white-collar workers, who are on average higher educated, were prevented from receiving a raise because of the brackets than those in metal trades, who are on lower educated, then we would expect the returns to education to fall.} Third, in general, there was more dispersion in the bindingness of the brackets in the South relative to the Northeast. In conclusion, these figures are inconsistent with a strict policy that set the brackets at a certain point in the distribution.

3 Hypothetical Effects of the Brackets

To understand the effects 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 its 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.\footnote{A hard maximum wage would work just the same to reduce inequality except it would reduce the earnings of those above the maximum.} 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. 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.\footnote{We abstract from any differences in hours worked so the distribution of earnings is the same as the distribution of hourly earnings and wages.} 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 earnings distribution is sufficiently thick, this might lead to an increase in inequality. Like the existence of spillovers, the disemployment effects of the minimum wage are hotly contested. Now, as a practical matter, given the small fraction of people earning close to the minimum wage, even if all these people lost their jobs following a minimum wage increase, this disemployment effect should have minor effects on the IQR.

We set these two complications aside and present a stylized model of 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. We consider the comparative statics of changing the (log) bracket $\bar{w}$, which 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 (a total wage freeze). 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 there is complete equality 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 labor 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 level of labor earnings 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

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

For simplicity, we set $f(w_t) = \exp(\alpha)w_t^{1+\beta}$ and $\bar{w} = 1$.\textsuperscript{15} We examine 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 $\alpha$ and $\beta$ in the last two cases so that the average earnings growth rate is the same across all three scenarios. We assume that there are no employment effects from the brackets.

Figure 3 shows the relationship between the initial level of earnings and subsequent actual earnings growth for these three scenarios. In the flat growth scenario, for sufficiently low initial

\textsuperscript{15}This hypothetical is different than the hypothetical 1943 distribution of earnings, which raises all wages below the bracket to that level.
levels of earnings, the growth rate is simply equal to the growth rate in latent earnings. 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 an initial level of earnings 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. For both of these cases, the relationship between the initial level of earnings and subsequent growth will still be (weakly) monotonic. The relationship, on the other hand, is non-monotonic in the increasing 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 earnings are constrained by the bracket. This generates the non-monotonic relationship between initial earnings 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? Figure 4 plots as a function of the bracket in the case of declining latent inequality the change in the difference between the $\tau$ and $\tau'$ percentiles with $\tau < \tau'$ so a positive value for the change corresponds to a decline in inequality. There is a region of values where the marginal effect of the 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 of the bracket less than the $\tau$ percentile, there is no effect on the $\tau$ or $\tau'$ percentiles themselves and consequently no effect on inequality. For the intermediate values still less than the $\tau'$ percentile, the $\tau'$ percentile remains fixed while the $\tau$ 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 percentiles are now fixed. In this case, the level of inequality is lower than the case of a very low level of the 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 mirror 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. There is also a region of intermediate values of the bracket greater than the $\tau$ percentile where the marginal effect is a reduction in inequality. A bracket set to a value in this region allows the $\tau$ percentile to rise while freezing the $\tau'$ percentile in place. Finally, there is then a region of values for the bracket greater than the $\tau'$ percentile where just the reverse happens and the marginal effect is negative. Here marginal increases no longer have no effect on the $\tau$ percentile but they do lead to increases in the $\tau'$ percentile of earnings and as a consequence inequality rises.

These are of course highly stylized examples of the evolution of the latent labor earnings...
distribution,\textsuperscript{18} 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 bracket is located. Second, as 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. 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 from the start of the Depression through 1960s inequality in the latent distribution of earnings declined and rose after that.

4 Data

4.1 NWLB Records

We collect primary source records on the brackets set by the NWLB at the occupation-industry-geography level.\textsuperscript{19} 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” (\textit{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 industries in all important labor market areas” (\textit{United States Department of Labor, 1949}, p. 797, volume 1). To carry out this survey, occupations were defined based on the 1939 \textit{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. 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.

\textsuperscript{18}It is also possible that the brackets affect inequality in total market earnings including both labor and capital earnings. We hope to examine this dimension of inequality in future work.

\textsuperscript{19}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.
While the NWLB had broad authority over almost all wages in the private economy, the number of occupations explicitly assigned a bracket was relatively small.\textsuperscript{20} 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) Critically, the NWLB felt that these key occupations could be “clearly and precisely defined.” 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 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 “precedent”

\textsuperscript{20}Firms with fewer than eight employees were exempt.
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. 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 region to which the different brackets would apply. 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, county borders were used as boundaries, while in Region I, 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 geographic areas could potentially be industry and occupation specific.

4.2 Census of Population

Our outcome and control variables come 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. A random sample of individuals are asked to fill out the long-form, which asks about a richer set of economic and demographic variables. In particular, our key dependent variable is total wage and salary income, which we will also call labor earnings or income. The long-form also asks about a person’s occupation and weeks worked. The first is critical for linking to the bracket records and the second for defining our working sample. These variables all refer to the year before the Census was taken, e.g. 1939 for the 1940 Census of Population.

There are a few issues to keep in mind. First, as mentioned above, only a fraction of the population receives the long-form. This varies from 1/4 in 1960 to approximately 1 in 6 in 2000. This still results in a substantial sample except in the case of the 1950 Census, which is too small and ridden with errors to be usable. Second, the labor income variable is topcoded to preserve anonymity. These topcoded observations are not in the end a big deal because we are not focused on inequality at the very top of the income distribution and no more than a few percent of the observations fall into this category. Third, while the original census records recorded occupations and industries as raw strings, 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

In constructing our working sample, we apply the sample restrictions imposed by Goldin and Margo (1992), only keeping men, ages 18-65, who worked at least 40 weeks last year (the year the labor earnings variable refers to). Individuals in this group have traditionally been in the labor

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21 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 the state an individual lived in was used as the topcode.
force so any effects of the brackets over time will not be due to changes in the composition of those employed. If we had included women, for example, then one concern is that changes in the effects of the brackets over time could be due to the rise in women's labor force participation and the composition of women in the work force. Also following Goldin and Margo, we require that individuals \textit{“earned, on average, more than one-half the minimum wage [in that year] on a full-time basis.”} In the 1940 Census with a minimum wage of $0.30 / hour, this works out to requiring individuals to have average weekly earnings greater than $6.\footnote{In the appendix, we document the number of observations dropped by each of these restrictions. We also examine the characteristics of those individuals who earn less than one-half the minimum wage.}

The rest of our sample restrictions are due to the limitations of the bracket data. We already mentioned that the archival records of the brackets for two regions are missing and the issue of occupations, industries, and regions that were not explicitly listed in the records. The other issue is that the occupational classification used by the NWLB does not line up exactly with that of the Census.\footnote{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.} In some cases, these groupings are coarser in the NWLB, in other cases finer, and still others not commensurate. Because of these differences in classification, we focus on two particular groups of occupations: white-collar and clerical jobs as well as jobs in the metal trades industries. These groups of occupations have a number of useful features. The first is that occupations are listed in the bracket records in the same way as in Census data allowing for straightforward linking. Second, the group of white-collar occupations was a cross-industry classification, meaning we do not need to worry about differences in the brackets by industry. This is not the case for metal trades occupations. 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 mentioned in the 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 a given occupation in explicitly mentioned counties to the total number of individuals in the 1940 Population Census who report that occupation. 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.\footnote{In the appendix, we discuss in greater detail issues with aggregating the brackets. For example, sometimes occupation were divided into different grades or sub-classifications. In cases like this, we have picked one (generally the one labeled “A”) and used this consistently across geographic areas. We document how frequently cases like}
One final detail is that brackets in Region I were defined at a subcounty level. For these brackets, we collapse to the county-level by weighting town-level brackets by the town population. For Region IV, there are no brackets defined at a sub-county level so this is not an issue. We discuss in the Appendix the extent of geographic aggregation.

5 Conditional Distribution Empirical Strategy

We begin following the literature with an analysis of the effects of the bracket on the conditional distribution of earnings. Denote the bracket for occupation \( i \) in county \( c \) by \( \bar{w}_{ic} \) and the \( \tau \) quantile of labor earnings for occupation \( i \) in county \( c \) at time \( t \) by \( Q_{\tau}(w_{ict}) \). Our outcome variable is the change between year \( t \) and 1939 in the log ratio of the \( \tau' \) and \( \tau \) percentiles, denoted by \( \Delta \log \left( \frac{Q_{\tau}(w_{ict})}{Q_{\tau'}(w_{ict})} \right) \). In all our specifications, we put the lower percentile in the numerator \((\tau < \tau')\), meaning the log ratio will be negative and a positive effect of the bracket reduces inequality. To be clear, our dependent variable is inequality in labor earnings not the wage rate, even though the wage rate is what was actually controlled by the NWLB. The distinction between total labor earnings 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.

Our basic specification is motivated by the ones in Rose (2009) and Autor et al. (2016), which were both inspired by the specification in Lee (1999). That is,

\[
\Delta \log \left( \frac{Q_{\tau}(w_{ict})}{Q_{\tau'}(w_{ict})} \right) = \beta_{\tau,\tau',t} \log \bar{w}_{ic} + \text{Controls}_{ic} + e_{ict}.
\]

Because an “observation” is a statistic derived from a group of individuals in a given occupation-county cell, we, like Autor et al. who work with state-level data, weight the observations by the total number of workers in that cell. This is slightly different than 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 by hours worked 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 if a percentile is non-unique: (1) 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. In the end, this issue does not make much difference for the results.

In our preferred specification, the controls include the 1939 value of \( \log \left( \frac{Q_{\tau}(w_{ict})}{Q_{\tau'}(w_{ict})} \right) \), its square, as well as occupation and county fixed effects. We view the inclusion of the fixed effects as important for the credibility of our identification strategy because it is clear that the NWLB set the brackets this way. We also discuss how we handled cases where brackets were defined at a sub-county level.
in a (partly) formulaic way that depended on earnings by occupation and region. By including the 1939 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 these were made on the basis of the 1939 level of inequality. For this reason, we do not view the inclusion of the 1939 level of inequality as critical for identification. A similar argument applies to potentially including the pre-trend in inequality between 1939 and 1929. 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.

One limitation of this occupation-county specification is that it is a within analysis asking how the brackets affected the distribution within occupation-county cells by exploiting between cell variation in the brackets. This makes it difficult to easily connect these results to the Great Moderation, which is a feature of the unconditional distribution of earnings. The individual-level specifications we discuss later will estimate the effects on the unconditional distribution of earnings and allow us to connect more easily the NWLB and the Great Moderation. Even with this complication, given a number of previous papers used a specification like this, we think this specification is still informative on the effects of the bracket.

An additional limitation of this strategy is that it will not allow us to identify any spillover effects across geographies or occupations. Implicitly, the NWLB believed in occupational spillovers when they decided they only needed to determine wages for certain “key” occupations to determine the whole wage distribution. The academic literature on the existence of such spillovers in the case of the minimum wage is rather mixed. Card and Krueger (1995) as well as Engbom and Moser (2021) provide evidence for such spillovers. On the other side, Autor et al. (2016) think these “spillovers” are simply due to measurement error. To the extent these spillovers exist, it is plausible to think that they cause us to underestimate the “true” effect of the brackets on inequality within occupation-county groups.\footnote{One approach to minimizing spillovers is to estimate a so-called “donut hole” specification developed in the minimum wage literature. Such a regression uses counties that are close but not too close as controls. It is more difficult to measure the “distance” between occupations in order to implement a similar donut hole strategy for occupations.}

While addressing the same question as the third chapter of Rose (2009), there are important differences between that work and ours. First, we estimate effects many decades after the NWLB ended whereas Rose stops in the 1950s. Since the literature has emphasized the potential long-lasting impact of the NWLB wage controls, we see this as a contribution. Second, Rose uses wage data from BLS wage surveys between 1950 and 1965 as his main dependent variable. In contrast, we use the federal decennial Census. The sheer size of the census is critical to attain sufficient statistical power given the demands of our identification strategy. In contrast, the BLS surveyed sporadically different occupations, years, and towns resulting in sample of only 91 observations for Rose to work with. This limits what Rose is able to include as controls. For example, he is
not able to include occupation fixed effects. Our specification also differs from Rose and the other
earlier works by taking the long difference rather than the level of inequality itself as the dependent
variable.

5.1 Threats to Identification

The basic identification assumption of our strategy is that, after controlling for the 1939 earnings
distribution and fixed effects, there are no other unobservables that determine both the brackets
and the distribution of earnings in the future.\footnote{In the appendix, we discuss a different identification strategy based on “measurement error” in the bracket setting process and why in the end, we do not think this is a good strategy.} Hence, understanding the threats to identification
requires understanding the source(s) of variation in the bracket. We have already discussed the
narrative evidence on the process of determining these brackets. Here we provide some quantitative
evidence on this process.

As a first step, we regress the brackets on various statistics of the 1943 hourly earnings dis-

tribution calculated by county and occupation.\footnote{To limit the influence of outliers, we calculate the mean after winsorizing the 5% tails of the hourly earnings distribution.} To isolate the predictive power of the earnings
distribution, we do not include any other controls such as county or occupation fixed effects in
these regressions unlike our specifications examining the effects of the brackets on inequality. The
narrative evidence suggests that the NWLB was supposed to set brackets equal to 90% of the
mean (or the “first substantial cluster”). This means that the elasticity of the bracket with respect
to the mean should be 1. Table 1 in which all variables are log transformed show that, while the
relationship between the mean is statistically and economically significant, the elasticity is also
substantially smaller than 1. We also observe statistically and economically significant positive
relationships of similar magnitudes with the 10th, 25th, and 50th percentiles. We also include all of
these statistics in a “horserace” regression. In this case, only the 50th percentile remains statistic-
ally significant. The last column includes average demographic characteristics for those in 1940 in
a given occupation-WLB zone as predictors. There is some slight evidence that occupation-zones
that had more whites also had higher brackets all else equal.

We interpret these regressions as providing quantitative evidence that brackets were actually
being set as a function of the distribution of earnings. At the same time, it is not possible to fully
explain the brackets using not just the mean but the whole distribution of earnings. This should
not be too surprising since the “rules” for setting the brackets were closer to rules of thumb and
not legally mandated. An additional reason for why we are not able to fully explain the brackets
is that we do not have access to the earnings information used by the NWLB, which was based on
a contemporaneous survey of wages. The issue is not that our 1943 earnings data is necessarily
“worse” than the data used by the NWLB, but that the two sources are distinct. So it could
be that the brackets are actually being set in a purely deterministic way and all the apparent
deviations are simply due to differences between the data we used and the data the NWLB used.

Because of this, it is difficult to know what to make of the residual variation in the brackets after controlling for the earnings distribution. Is it even “real”? If it is, then what is the source? Mistakes made by bureaucrats in calculating the mean wage? Random variation due to the “real-time” data being used? Systematic deviations to aid the war effort? Figure 2 suggests that different occupations in different regions did experience systematic differences in the bindingness of the bracket defined as the ratio of the bracket relative to the mean wage. This suggests that, above and beyond the distribution of earnings, the necessities of the war drove the setting of the brackets to a certain extent. The concern then is whether these war-related determinants of the brackets are somehow correlated with the earnings distribution in the future, either directly or incidentally. For example, Aizer et al. argue that government spending contracts during WWII, which stipulated racial non-discrimination, decreased racial differences in the long-run. If the amount of government contracts were correlated with the levels of the bracket (after controlling for everything), then it would be difficult to disentangle the effects of the brackets from the effects of the contracts.

We partially address this concern over unobserved war-related determinants by collecting collect data on some war effort related variables including (1) state-level mobilization rates (Acemoglu et al., 2004) and (2) county-level total and public manufacturing investment (Jaworski, 2017). We then estimate the relationship between the bracket $\bar{w}$ and the various war effort variables $WarEffort$ at the occupation-county level:

$$\log \bar{w}_{ic} = \beta WarEffort_c + Controls_{ic} + e_{ic}.$$  

To identify whether these war effort variables have any predictive power over and above that of the earnings distribution, all of these regressions control for the mean, $Q_{10}$, $Q_{25}$, and $Q_{50}$ of the 1943 log hourly earnings distribution conditional on occupation and WLB zone. Standard errors are clustered at the county level, the level at which the war effort variables vary (except for state-level inductions). Total and public investment are reported in $100,000s. We do not log transform the investment variables because there are many 0s, but do log transform the number of inductions.

Figure 5 shows the associations between the level of the bracket and these measures of the war effort. We do find a positive and statistically significant relationship with inductions and total investment, but no relationship with public investment. This should not be surprising, since the brackets were set in 1943 and areas with tighter labor markets due to other interventions in the market would likely have increased wages relative to the 1939 level. Note that these regressions capture the total effect of these war related policies on the bracket. This includes both indirect

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28 Unfortunately, we have not been able to access the data on government contracts used in Aizer et al.

29 Note that these war related variables do not vary by occupation. 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.
effects on the 1943 hourly earnings 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. In the end, we interpret the magnitude of these effects as rather small and conclude that the main (observable) driver of the brackets is the earnings distribution.

6 Conditional Distribution Results

Besides running our preferred specification for 1959, 1969, 1979, 1989, and 1999, we run the same regression on the 1943 hypothetical distribution of earnings. The results in “1943” give a useful benchmark by providing a rough sense of what we should expect in the post-war years if (1) the brackets in 1943 had been binding; (2) the effects of the brackets had been perfectly persistent; and (3) there had been no changes in the inequality of the latent earnings distribution.

Figure 6 plots these effects by year with ±2 standard error confidence intervals for the changes in $\log(Q_{10}/Q_{90})$, $\log(Q_{10}/Q_{50})$, and $\log(Q_{50}/Q_{90})$ inequality measures. First, we reassuringly find that in our 1943 benchmark, a marginal increase in the bracket leads to a statistically and economically significant decline in all the inequality measures relative to 1939. This “compressing” effect of the bracket persists through 1959, though the effect in 1959 on $\Delta \log(Q_{10}/Q_{90})$ is only about 1/3 the effect size in 1943. To interpret the magnitude of the results in that year, a one standard deviation increase in the bracket would lead to a 5.7 log point increase in $\Delta \log(Q_{10}/Q_{90})$, which is about 1/3 the mean change in this log ratio over this period. The effect in 1969 is marginally statistically significant and, by 1979, the point estimate for the effect on $\Delta \log(Q_{10}/Q_{90})$ is no longer statistically significant and remains so through the end of our sample. The fact that the effect for this inequality measure dies out over time gives us confidence that our specification is not simply capturing the systematic relationship between the bracket and earnings. It is still quite striking that effects are still observable 25 years after the end of the war and the NWLB wage controls. We discuss later some possible mechanisms for this persistence.

The compressing effects of the brackets on the $\Delta \log(Q_{10}/Q_{90})$ are overall driven by effects at the top of the income distribution as measured by the $\Delta \log(Q_{50}/Q_{90})$ rather than effects at the bottom as measured by the $\Delta \log(Q_{10}/Q_{50})$. Strikingly, the effects at the top are more persistent than the overall effects. Even in 1979, the effects at the top are statistically significant and not that much different in magnitude than the hypothetical 1943 effects.

If anything, the effects at the bottom of the distribution for the $\Delta \log(Q_{10}/Q_{50})$ work in the opposite direction with higher brackets associated with increases in inequality. One possible explanation for this puzzling result is that during a period of declining latent inequality, which perhaps

Note that even though $\Delta \log(Q_{10}/Q_{90})$ is exactly equal to the sum of $\Delta \log(Q_{10}/Q_{50})$ and $\Delta \log(Q_{50}/Q_{90})$, the regression coefficients for $\Delta \log(Q_{10}/Q_{50})$ and $\Delta \log(Q_{50}/Q_{90})$ do not need to add up to the total effect for $\Delta \log(Q_{10}/Q_{90})$ since we do not impose this adding up restriction in the estimation.
characterizes the 25 years after the war, a high level of the bracket can “freeze” the income distribution at a high level of inequality relative to places with a lower level making it appear that the brackets are increasing inequality. Overall though, the effects on $\Delta \log(Q_{10}/Q_{50})$ tend to be small, economically and statistically.

The fact that the effects on the overall measure of inequality is driven by effects on the upper part of the distribution is consistent with the narrative of how brackets were supposed to be set. That is, as a fraction of the average wage, which, given the skewness of the earnings distribution, put the bracket fairly high up in the earnings distribution. This is also consistent with our direct evidence that brackets were set quite high in the hypothetical distribution of hourly earnings in 1943.

These effects should not necessarily be interpreted as effecting the earnings of any particular person. Instead, they could be explained by, for example, the migration of higher paying jobs conditional on occupation out of areas where the brackets were more binding. This change in the composition of workers would lead to an observed decline inequality separate from any direct effect of the brackets on workers that stayed.\textsuperscript{31} At the same time, we would argue that by imposing the sample selection criteria of Goldin and Margo and focusing on a group of individuals who have traditionally been in the work force, our effects are not driven by changes in the unobserved composition of workers.

A natural question then is what these results mean for understanding the Great Compression. Much of the decline in inequality in the unconditional earnings distribution during this period is due to declines in the between occupation component of inequality and, at a more fundamental level, the returns to education. These results do not speak to this dimension since our strategy uses between occupation (and region) variation in the bracket to identify the effect on within occupation (and county) inequality. At the same time, Goldin and Margo do document a fall in the within occupation component of inequality and show that much of the action in the $\log(Q_{10}/Q_{90})$ is due to changes in the $\log(Q_{50}/Q_{90})$. Both of these broad patterns seem consistent with these results.

That is, at least over the first few decades following the war, within occupation inequality was lower than it otherwise would have been without the brackets and this lower level of inequality was driven by smaller gaps at the top.\textsuperscript{32} The fact that we focus on a limited set of occupations also makes us cautious about how far we should push this comparison to GM.

\textsuperscript{31}We emphasize that the “treatment” does not vary by individual within meaning it is not a function of the bracket in the region where, for example, an individual’s first job was. One interesting question is whether we could use the brackets to identify the effects on the persistence of earnings at the individual-level. That is, what is the causal effect on an individual’s current level of earnings on future earnings potential, separate for an individual’s marginal productivity.

\textsuperscript{32}We are extrapolating here from our estimates of the marginal effect of the bracket to the effect of removing the bracket altogether. As we mentioned early, this is fraught not simply because there are general equilibrium effects that we are not capturing, but also because the effects of the bracket on inequality do not need to be monotone.
6.1 Effects on Other Dimensions of Inequality

We now examine other dimensions of inequality including the racial and education gap. For the former, as mentioned earlier, the NWLB was quite explicit that that the brackets were supposed to apply uniformly by race (and by gender). So it is plausible that the brackets could have affected the Black-white earnings gaps in the short-run and potentially the long-run.

Figures 9 and 10 show these effects for the racial and high school graduation earnings gap. Like in our previous specifications, a positive coefficient implies that the a higher value of the bracket reduces the within component of racial (or educational) inequality. While the hypothetical conditional racial gap in 1943 would unsurprisingly have been lowered with higher brackets, we see no evidence of lower conditional racial gaps in subsequent years.

We want to emphasize again that this analysis is within occupation (and region). Hence, it does not capture any effects on the between component of the racial (or education) earnings gap, which are certainly quite critical. That said, as we show in the appendix, it does not appear to be the case that the brackets induced demographic sorting across occupations. In particular, the brackets do not explain changes in the fraction white by occupation. It is still possible that the brackets caused the difference in the average wage between predominantly Black and white occupations to grow, for example, offsetting any compressing effects within an occupation.

6.2 Robustness Check: Varying the Fixed Effects Included

We now consider the effects of varying the set of fixed effects included. Besides our preferred specification, which includes county and occupation fixed effects, we also estimate the effects of the brackets with no fixed effects as well as one with occupation effects only. Differences across specifications tell us something about the non-random nature of the bracket assignment process across geography and occupation and its correlation with subsequent changes in inequality. If brackets were set truly randomly, then including or excluding certain sets of fixed effects should have no effect on the point estimate.

Figures 7 shows the results of the different specifications for $\Delta \log(Q_{10}/Q_{90})$. For comparison, the green squares are the results from our preferred specification. Interestingly, the effects with no fixed effects at all are quite stable over time with a minor amount of attenuation in 1999. We think this highlights the non-random assignment of brackets and the persistence in the earnings distribution. For 1943 and 1959, results are relatively insensitive to the set of fixed effects we include. It is only starting in 1969, the set of fixed effects starts to matter, a pattern that persists through the end of the sample. The more saturated model with county and occupation fixed effects shows the expected pattern of attenuation over time.\(^{33}\) In contrast, the model without fixed effects shows a consistent effect of higher brackets lowering inequality through 2000, while the model with

\(^{33}\)Recall that we cannot include occupation-by-county fixed effects since that is the level at which the brackets vary.
occupation fixed effects only results in coefficients with the opposite sign by 2000, with higher 
broackets associated with *increases* in inequality.

### 6.3 Robustness Check: IQR Measure of Inequality

Figure 8 shows the results of the different specifications for the change in the log ratio of the 
IQR $\Delta \log(Q_{25}/Q_{75})$. This measure of inequality is, by construction, less driven by changes at 
the very top and bottom of the earnings distribution than the $\Delta \log(Q_{10}/Q_{90})$. In this way, it 
provides a slightly different perspective on inequality and which parts of the earnings distribution 
is being affected by the brackets. It is furthermore conceivable that potential nonlinear effects of 
the brackets affected these different measures of inequality differently. That said, we find using 
our preferred specification that the brackets compress the income distribution in 1959 and 1969, 
similar to the findings for the $\Delta \log(Q_{10}/Q_{90})$ measure. These effects attenuate over time with, in 
most cases, no statistically significant effect by 1970 (except for the anomalous negative effect in 

The patterns as we vary the set of fixed effects included are very similar to those for the 
$\Delta \log(Q_{10}/Q_{90})$ case. First, we find a very persistent effect on $\Delta \log(Q_{25}/Q_{75})$ when there are 
no fixed effects included. Second, whether we include occupation or occupation and county fixed 
effects only affects the magnitudes but not the overall qualitative patterns over time. Note that 
the effect size is about half as large in this case as in the $\Delta \log(Q_{10}/Q_{90})$ case though this needs 
to be set against the fact that the mean change in the IQR is smaller. We take from these results 
that the effects of the brackets are robust to the measure of inequality we use and that they apply 
to the tails and to the “bulk” of the earnings distribution.

### 7 Unconditional Distribution Empirical Strategy

The occupation-county level specifications estimate the effects of the brackets on the conditional 
distribution of earnings, but have nothing to say about the effects on the unconditional distribution. 
To address these unconditional effects, we use an extension of the *unconditional quantile regression* 
(UQR) approach, originally developed by Firpo et al. (2009).\(^34\) To estimate the effect on the $\tau$-
quantile of the distribution of labor earnings $w$ using this approach, we transform the earnings of 
individual $w_i$ using the *recentered influence function* (RIF):

$$
\psi(w_i, \tau) = Q_\tau(w) + \frac{\tau - 1 \left[ w_i \leq Q_\tau(w) \right]}{f_w(Q_\tau(w))},
$$

\(^34\)UQR fits into a broader literature attempting to identify the distributional effects of policy changes. Fortin et 
al. (2011) summarize these so-called decomposition methods starting from the Oaxaca-Blinder decomposition of the 
mean wage. A large literature since then has attempted to go beyond decomposing the effects of a policy change on 
the mean to decomposing effects on the whole distribution. Besides UQR, a few other related approaches include 
that of Chernozhukov et al. (2013) and that of Machado and Mata (2005)
where \( f_w \) is the density of earnings and \( Q_\tau \) the \( \tau \)-th quantile. We then regress this transformation of a binary variable on the bracket and a set of controls denoted by \( X_i \). These include the same set of fixed effects as in the occupation-level effects, but now we also include “Mincerian” or demographic controls. These are individual-specific and include age, race, and martial status. These controls serve mainly to increase the precision of our estimates. To the extent that they have additional explanatory power of the brackets, they strengthen the claim that we are estimating causal effects. The same questions and concerns about endogeneity raised regarding the conditional distribution specifications apply to this specification as well. In contrast to the the conditional specifications, the dependent variable here is simply the level of a percentile in a given year not the change relative to 1939.

The extension of the UQR methodology we use developed by Martínez-Iriarte et al. (2022) allows us to separate the effects of changes in the mean level of the brackets from a change in the standard deviation of the brackets. The former known as the location effects are the effects estimated in Firpo et al. (2009) and measure how the marginal distribution of earnings would change in response to a marginal change in the bracket. Scale effects measure how the marginal distribution of earnings would change because of a marginal change in the dispersion of the brackets. For example, what would the earnings distribution have been if the NWLB had chosen slightly coarser occupational or geographic groupings when determining the brackets? This is distinct from the location effect, which addresses the counterfactual of a world where the NWLB had set all brackets marginally higher.

To fix ideas, Martínez-Iriarte et al. (2022) define a hypothetical change in the brackets from the observed \( \bar{w} \) to \( \bar{w}_\delta \), where \( \delta = 0 \) represents the status quo policy, according to the following scale-location shift model:

\[
\bar{w}_\delta = \frac{\bar{w} - \mu}{s(\delta)} + \mu + \ell(\delta).
\]

The function \( s(\delta) \) is known as the scale shift and \( \ell(\delta) \) the location shift. Along with the value of \( \mu \), these functions are given or calibrated. Assume that \( s(0) = 1 \) and \( \ell(0) = 0 \) as well as that \( s(\delta) \) and \( \ell(\delta) \) are continuously differentiable. The quantity of interest is the change in \( \tau \)-th quantile of the unconditional distribution of \( w \) due to a marginal change in \( \delta \) defined as

\[
\Pi_\tau = \lim_{\delta \to 0} \frac{Q_\tau[w_\delta] - Q_\tau[w]}{\delta}.
\]

where \( w_\delta \) is the counterfactual distribution of \( w \) given \( \bar{w}_\delta \). Note that if we estimate the marginal effects for the \( \tau \) and \( \tau' \)-th quantiles, then the marginal effect on the difference between the two is simply \( \Pi_\tau - \Pi_{\tau'} \).

Let \( e \) be the unobservables that determine \( W \). Under some assumptions, Martínez-Iriarte et
al. (2022) show that

$$\Pi = \Pi_{\tau,L} + \Pi_{\tau,S}$$

where

$$\Pi_{\tau,L} = -\frac{\partial l(0)}{\partial \delta} \int \int \frac{\partial F_{w|X,\bar{w}}(Q_{\tau}(w)|x,\bar{w})}{\partial \bar{w}} f_{X,\bar{w}}(x,\bar{w}) dx d\bar{w},$$

$$\Pi_{\tau,S} = -\frac{\partial s(0)}{\partial \delta} \int \int \frac{\partial F_{w|X,\bar{w}}(Q_{\tau}(w)|x,\bar{w})}{\partial \bar{w}} f_{X,\bar{w}}(x,\bar{w})(\bar{w} - \mu) dx d\bar{w}.$$
In words, a 1% decrease in the standard deviation of \( \bar{w} \) results in a \( \Pi_{\tau,S}^\mu \% \) change in the \( \tau \)-th quantile of log labor income \( w \). Martínez-Iriarte et al. offer a couple examples to understand the meaning of the scale and location effects. For example, if both \( \bar{w} \) and \( e \) are standard normals and \( w = \alpha + \beta \bar{w} + e \), then

\[
\Pi_{\tau,L} = \beta, \\
\Pi_{\tau,S}^\mu = -\frac{\beta^2}{\sqrt{\beta^2 + 1}} Q_\tau[e].
\]

In this case, the location effect is constant across quantiles and the sign of the scale effect is determined by the sign of \( Q_\tau[e] \). Note that if \( e \) is symmetric, then \( \Pi_{50,S}^\mu = 0 \). More generally, the location effect will depend on the first derivative of the conditional distribution \( F_{w|X,\bar{w}} \) while the scale will depend on the second.

Estimating the location and scale effects is straightforward using plug-ins for unknown population quantities. Let \( \hat{Q}_\tau(w) \) be an estimator of the \( \tau \)-th quantile of \( w \), \( \hat{f}_w(\hat{Q}_\tau(w)) \) an estimator of the density at that quantile, and \( Z_i = (\bar{w}_i, X_i) \). We assume that

\[
F_{w|X,\bar{w}}(Q_\tau(w)|X_i, \bar{w}_i) = G(\beta_\tau \bar{w}_i + \alpha'_\tau X_i),
\]

where \( G \) is either the logit or probit CDF.\(^{35}\) The maximum likelihood estimator of \( \theta_\tau = (\beta_\tau, \alpha_\tau) \) is

\[
\hat{\theta}_\tau = \arg\max_{\theta_\tau} \sum_{i=1}^n \left\{ I\left[ w_i \leq \hat{Q}_\tau(w) \right] \log[G(\theta'_\tau Z_i)] + I\left[ w_i > \hat{Q}_\tau(w) \right] \log[1 - G(\theta'_\tau Z_i)] \right\}.
\]

Then estimators for the location and scale effects are

\[
\hat{\Pi}_{\tau,L} = -\frac{\hat{\beta}_\tau}{N \hat{f}_w(\hat{Q}_\tau(w))} \sum_{i=1}^N g(\hat{\beta}'_\tau Z_i), \\
\hat{\Pi}_{\tau,S}^\mu = \frac{\hat{\beta}_\tau}{N \hat{f}_w(\hat{Q}_\tau(w))} \sum_{i=1}^N g(\hat{\beta}'_\tau Z_i)(\bar{w}_i - \hat{\mu}),
\]

where \( g = G' \) and \( \hat{\mu} \) is the average value of \( \bar{w}_i \). While Martínez-Iriarte et al. (2022) provide a formula for the asymptotic variance, based on correspondence with the authors, we instead use the bootstrap \(^{36}\) because the value of \( \mu \) we use is estimated rather than calibrated.

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\(^{35}\)We show in the appendix that if \( G \) is taken to be linear, then the scale effect in the case when \( \mu \) is the sample average of \( \bar{w} \) will be identically 0.

\(^{36}\)While the validity of the bootstrap was not established in their paper, this is a "regular" problem so the bootstrap should produce a consistent estimate of the variance.
8 Unconditional Distribution Results

We estimate the location and scale effects of the brackets for the 10th, 25th, 50th, 75th, and 90th percentiles in each of the years. We reemphasize that these effects can be interpreted as the marginal effects of a change in the level or dispersion of the brackets on the quantiles of the unconditional earnings distribution. Besides the point estimates, we report the 95% confidence interval based on the 5th and 95th percentiles of the bootstrap estimates. The quantile effects control for, but do not condition on, the demographic controls at the individual-level, the value of the percentile for a given occupation-county, as well as county and occupation fixed effects.

Figure 11 shows the location effects. In principle, if variation in the brackets was random conditional on the controls, then there should be no effect of the brackets in 1943. In actuality, we observe statistically significant location effects of the brackets for all the percentiles we consider. The location effect of a higher bracket on the 10th and 25th percentiles is positive. Since by construction the brackets cannot have had an effect in 1939, this shows that the brackets were not set randomly conditional on observables. At the same time, these effects are quite small in magnitude.

In the first post-war year of data we have, in 1959, we find positive marginal effects of an increase in the mean of the bracket for the 10th and 25th percentiles with effects getting smaller and smaller by percentile. These effects can be interpreted as elasticities so, for example, a 10% increase in the mean of the bracket increases by 2% the 10th percentile of the unconditional earnings distribution. The marginal effects for the various percentiles can be “added up” so we conclude that the effect of a marginal increase in the mean of the brackets is to reduce inequality measured as the log ratio of two percentiles by increasing the lower percentile more than the upper percentile (which in some cases will actually decrease) in 1959. These location effects diminish in magnitude over time 1970 and, by 1980, are near zero for almost all the percentiles.

There is no necessary reason why the location effects by percentile are strictly ordered in a given year. In the context of the NWLB, the result has a natural interpretation. Recall that by policy the brackets were intended to be set at 90% of the mean of the relevant labor market, and hence were generally in the lower parts of the distribution. A higher bracket would have allowed these workers to receive a larger raise (or a raise at all), and hence, would be associated with an increase in the 10th and 25th percentiles.

The negative location effects for the higher quantiles are more complicated to explain via the direct effects of the NWLB, although they are smaller in magnitude. One possible explanation is that higher incomes at the lower tail have to be (partially) “compensated” by lower incomes in the higher quantiles for firms to remain in operation and workers employed. This would be a negative spillover across the quantiles of the earnings distribution, opposite of how minimum wage spillovers are usually assumed to operate. In the latter theory, firms must raise wages for those

37 These estimates are calculated by sampling without replacement from the data.
earning slightly higher than the minimum wage to maintain a certain earnings hierarchy.

Figure 12 shows the scale effects. Recall that these can be interpreted as the effect of a 1% decrease in the standard deviation of the brackets on the respective percentile of the unconditional earnings distribution. In other words, what would the earnings distribution have looked like if the NWLB had set the brackets in a slightly more uniform manner? For example, what if the NWLB had defined slightly coarser occupation or geographic groups for the purpose of assigning the brackets? We find that a more uniform bracket policy would be associated with particularly higher incomes at the 10th percentile, with the same pattern of attenuation from 1960 to 1970, and near zero effects by 1980. That said, these effects are all quite small in magnitude. While not strictly comparable in magnitudes, the scale effects across the percentiles are on the order of one fifth the magnitude of the location effects.

9 Mechanisms of Persistence

Across a variety of specifications, it is clear that the brackets continued to affect the earnings distribution, at least, through 1960 and perhaps until 1970. 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” private and public wage setting mechanisms over the next two decades.

This process of “enshrining” the distribution of wages set by the NWLB began with the US entrance into the Korean War. Just as in WWII, the federal government setup an agency to regulate wages like the NWLB. This agency known as 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). Therefore, the Korean War version of the NWLB sustained the effects of the WWII NWLB on inequality through, at least, the end of the war in 1953. As evidence Keat (1960) highlights the still compressed between occupation earnings distribution in 1956, 3 years after the end of the Korean War and the dissolution of the Korean War Wage Stabilization Board, relative to 1900.38

Following the war, one important mechanism continuing the effects of the war-era wage controls is through the bracket’s effects on union wage schedules. Levitan (1951) surveys 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

38Unfortunately, we do not have information on the actual wages set during the Korean War so we cannot directly explore the effects of this agency.
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.” This evidence suggests that unions at the time perceived these long-run effects of the NWLB.

As one example, 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. In a case study, Etheridge (2020) 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”. It is intriguing that the effect of the NWLB fades out as the percent of private sector workers in a union begins to fall most sharply after 1970.39

As anecdotal evidence, some unions noted that the NWLB-era brackets became codified in the wage structure for some time. A 1951 report of the Industrial Union of Marine and Shipbuilding Workers of America explained one reason for persistence (emphasis added)

In 1943, 1944, and 1945 the Shipbuilding Commission of the National War Labor Board conducted extensive surveys of basic rates throughout the industry and evolved definite rate and wage structures for the trades in each shipbuilding zone. It is interesting to note that these structures have never been changed, even since the war - that the increases since the war have always been on an across the board level, because once the basic structure of the trades is tampered with in any shipyard, the entire delicate mechanism by which the trades are graded in accordance with the skills required, goes by the board.

While this is just one particular instance, we take it as suggestive evidence that unions were generally satisfied with the wage structure imposed by the NWLB, and this led to persistent effects.

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.” The Chief of the Research and Statistics Branch of the NWLB John T. Dunlop, who became a very prominent scholar of industrial relations, emphasized throughout his scholarly career the centrality of occupational hierarchies and internal labor markets

39One natural extension of the current paper is to combine analysis of the NWLB and unionization in one framework. This is complicated not just by a lack of detailed data on unionization but also because causality worked in both directions. Indeed, Farber et al. (2021) argue that the NWLB itself was a spur to unionization.
in determining the “correct” level of wages. Piketty and Saez, in their study of top income inequality, also mention a similar mechanism when they write that “World War II without doubt had a profound effect [...] 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 about the role of norms were made by Goldin and Margo as well as Goldin and Katz and much earlier by Brown (1977). Unfortunately, like the effects of the Korean War Wage Stabilization Board, providing direct evidence for the role of norms is difficult.

10 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, make all the more prominent by its scale. We find that the NWLB bracket policy did have enduring effects on the distribution of labor income. In fact, in 1969, 25 years after the end of war, we can still detect a compressing effect of the brackets on the upper tail of the income distribution.

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 inter-generational 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?

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40 This is a point that is reflected in the way the NWLB felt that, to control the whole wage distribution, it was only necessary to specify brackets for a select set of occupations.
References


Figure 1: Brackets for Stenographers

Notes: Darker values represent higher values for the bracket. 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. For the other regions, brackets are assigned at the county-level. The thick black lines represent borders of NWLB regions. Counties are unshaded either because (1) there is no town in that county listed in the brackets; (2) the county had no stenographers in the 1940 Census; or (3) all the brackets for a region are missing or have not been collected.
Figure 2: Distribution of Brackets as Percentile of 1943 Hourly Earnings

Notes: The percentile is calculated conditional on occupation and WLB zone. The 1943 hourly earnings variable is calculated using reported labor income and weeks worked in 1939 assuming a 40-hour workweek. It is adjusted for growth in production worker compensation between 1940 and 1943.
Figure 3: Hypothetical Effects of the Bracket on Labor Earnings Growth

Notes: This figure simulates the effects on actual labor earnings growth in three scenarios for the growth in latent labor income inequality. In all 3 scenarios, the average growth in latent income is the same. We normalize the value (of the log) of the bracket to 0.
Figure 4: Hypothetical Effects of the Bracket on Labor Earnings Inequality

Notes: We assume that latent labor earnings growth is linearly decreasing in initial labor earnings. This implies that inequality in latent labor earnings is falling. We also assume that the initial distribution of log labor earnings is a standard normal. We plot minus the change in inequality so that an increase in this value reflects a more equal distribution of earnings.
Figure 5: Predicting the Brackets Using War Effort Variables

Notes: The state-level variable “Inductions 1943” is the number of men inducted into the army in 1943 and log transformed. The county-level variables “Total Investment” and “Public Investment” are measured in $100,000s and in levels. They measure investments made in military production overall or by the public sector. All regressions include the mean, Q_{10}, Q_{25}, and Q_{50} of the 1943 hourly earnings distribution conditional on the occupation-WLB zone level. Standard errors are clustered at the county-level.
**Figure 6:** Effects of the Brackets on Conditional Distribution of Earnings

![Graph showing the effects of the brackets on conditional distribution of earnings.](image)

**Notes:** The dependent variable is the change between the given year and 1939 in log ratio of the percentiles of labor earnings. Controls include the value of the statistic in 1939 as well as occupation and county fixed effects. The point in 1943 is the result of running the same specification as the other years using the hypothetical earnings distribution in 1943 based on adjusting 1939 earnings for growth in production worker compensation between 1939 and 1943 and assuming that the brackets are binding. The sample restrictions are the same as in Goldin and Margo (1992). Standard errors are clustered at the occupation by county level.
Figure 7: Effects of the Brackets on $\Delta \log(Q_{10}/Q_{90})$ of Earnings by Specification

Notes: The dependent variable is the difference between the value of the statistic of log labor earnings in the given year and 1939. Regressions include the inequality value in 1939 and its square. The point in 1943 is the result of running the same specification as the other years using the hypothetical earnings distribution in 1943 based on adjusting 1939 earnings for growth in production worker compensation between 1939 and 1943 and assuming that the brackets are binding. The sample restrictions are the same as in Goldin and Margo (1992). Standard errors are clustered at the occupation by county level.
Figure 8: Effects of the Brackets on Conditional Distribution of Earnings: IQR

Notes: The dependent variable is the difference between the value of the statistic of log labor earnings in the given year and 1939. Regressions include the inequality value in 1939 and its square. The point in 1943 is the result of running the same specification as the other years using the hypothetical earnings distribution in 1943 based on adjusting 1939 earnings for growth in production worker compensation between 1939 and 1943 and assuming that the brackets are binding. The sample restrictions are the same as in Goldin and Margo (1992). Standard errors are clustered at the occupation by county level.
Figure 9: Effects of the Brackets on Conditional Black-White Earnings Gap

Notes: The dependent variable is the difference between the value of the statistic of log labor earnings in the given year and 1939. Regressions include the inequality value in 1939 and its square. The point in 1943 is the result of running the same specification as the other years using the hypothetical earnings distribution in 1943 based on adjusting 1939 earnings for growth in production worker compensation between 1939 and 1943 and assuming that the brackets are binding. The sample restrictions are the same as in Goldin and Margo (1992). Standard errors are clustered at the occupation by county level.
Figure 10: Effects of the Brackets on Conditional HS Graduation Earnings Gap

Notes: The dependent variable is the difference between the value of the statistic of log labor earnings in the given year and 1939. Regressions include the inequality value in 1939 and its square. The point in 1943 is the result of running the same specification as the other years using the hypothetical earnings distribution in 1943 based on adjusting 1939 earnings for growth in production worker compensation between 1939 and 1943 and assuming that the brackets are binding. The sample restrictions are the same as in Goldin and Margo (1992). Standard errors are clustered at the occupation by county level.
Figure 11: Location Effects of the Brackets on Unconditional Distribution of Earnings

Notes: Effects can be interpreted as the effect of a 1% increase in the mean of the brackets on the percentiles of the unconditional earnings distribution. The point in 1943 is the result of running the same specification as the other years using the hypothetical earnings distribution in 1943 based on adjusting 1939 earnings for growth in production worker compensation between 1939 and 1943 and assuming that the brackets are binding. The sample restrictions are the same as in Goldin and Margo (1992). Standard errors are calculated using the bootstrap.
Figure 12: Scale Effects of the Brackets on Unconditional Distribution of Earnings

Notes: Effects can be interpreted as the effect of a 1% decrease in the standard deviation of the brackets on the percentiles of the unconditional earnings distribution. The point in 1943 is the result of running the same specification as the other years using the hypothetical earnings distribution in 1943 based on adjusting 1939 earnings for growth in production worker compensation between 1939 and 1943 and assuming that the brackets are binding. The sample restrictions are the same as in Goldin and Margo (1992). Standard errors are clustered at the occupation by county level.
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**Notes:** An observation is a occupation-WLB zone pair. The sample restrictions are the same as in Goldin and Margo (1992). The statistics are from the 1943 hourly earnings distribution. The 1943 hourly earnings variable is calculated using reported labor income and weeks worked in 1939 assuming a 40-hour workweek. It is adjusted for growth in production worker compensation between 1940 and 1943. We winsorize the 5% tails of the hourly earnings distribution. All variables are in logs. Standard errors are clustered by occupation and WLB zone.