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. 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 find that higher brackets were associated with relative decreases in 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. There are no effects detectable from 1979 onward.

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

Economic inequality sharply declined in the US between the 1940s and the 1970s before reversing over the next decades. One potential source for the compression in inequality in the decades

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after WWII is the wartime institution of the National War Labor Board (NWLB). For example, Goldin and Margo (1992) contend that the wage setting policies of the NWLB lowered inequality for decades after the end of the war and this policy. 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]."

We provide evidence on the persistent effects of the NWLB on inequality by combining original, archival data on NWLB-mandated maximum allowable raises with Census of Population microdata. 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 outlaws wage rates below some level.

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. In theory, brackets were set to reflect "sound and tested" wage rates in the middle of 1943. This was interpreted to mean that brackets should be set in a given occupation, industry, and geographic location equal to the first "cluster" of wages or 90% of the mean wage, but these were never legally binding rules. 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 in policy at the borders of the regions. 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 was simply the discretion on the part of the bureaucrats in setting these brackets. Identification of the effects of the brackets, in the end, is based on the assumption that, after controlling for the preexisting distribution of earnings, variation in the bracket was as good as random.

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 (confidential) full count decennial censuses between 1960 and 2000.[1] Because of data limitations we discuss later, we focus on white-collar and metal trades occupations for which the mapping between occupations in the

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

NWLB and the Census is relatively clear.

We first study the effects on the within occupation-county conditional distribution of labor earnings by estimating a long difference specification for the change in inequality between 1939, the year to which the labor earnings data from 1940 Census refers, and a subsequent year of the Census. In our preferred specification, we control for the 1939 value of the inequality measure (and its square) as well as a full set of occupation and county fixed effects. 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.

While these specifications are common in the literature, e.g., Autor et al. (2016), it is difficult to relate the estimates directly to the Great Compression since this empirical 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 *increase* in inequality is due to rising 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 identifying the effects of two separate hypotheticals. The first, known as the location effect, is the effect on the unconditional distribution of labor earnings of a hypothetical marginal change in the *mean* of the brackets. The second, known as the scale effect, is the effect of a hypothetical 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 across occupations, industries, and regions.

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 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.

Do these results support the claim made by Goldin and Margo (1992) that the NWLB played a critical role in the increase of about 30 log points in the $\log(Q_{10}/Q_{90})$ ratio between 1939 and 1959? There are a number of reasons to be careful about naively interpreting our results in this way. First, we obviously do not capture any general equilibrium effects of the policy. Second, the effects of the bracket might be non-monotone. Third, we only study a select set of occupations and regions due to data limitations. With these qualifications in mind, our results do provide some support for the claim of Goldin and Margo (1992). For example, Goldin and Margo report that the $\log(Q_{10}/Q_{90})$ of earnings increased by nearly 30 log points between 1939 and 1959. The location effect for the $\log(Q_{10}/Q_{90})$ in 1959 is about 0.3, so a 10 log point increase in the mean value of the bracket would have increased the $\log(Q_{10}/Q_{90})$ by approximately 3 log points, or 10% of the overall change.

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 in terms of the educational skill premium. 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 the (education) skill premium over the 20th century. Goldin and Katz (2009) argue that the "race" between education and technological change has been a defining feature of the American labor market for 150 years.

An alternative set of explanations 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.

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) studies the war's broader demographic ramifications for women. Shatnawi and Fishback (2018) find a persistent effect on demand for female workers in manufacturing even after the war ends. Consistent with this work, 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. Aizer et al. (2020) examines the effects of military spending contracts while Ferrara (2020) examines variation in the demand for Black labor. 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.

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 challenges then as in WWII. Indeed, the first coherent national labor policies in US 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, in the end, many involved in labor policy during WWII held negative views of the WWI 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 earlier 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. In addition, the WWI NWLB made three crucial decisions in labor disputes that would foreshadow the operation 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.

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", 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).

Subsequent decisions by the NWLB did little to clarify its general policy for settling labor disputes and instead added new complications. For example, in other cases it decided, the NWLB considered "[p]revailing wages elsewhere in the industry as well as in the local area" (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 straightforward basis for NWLB decisions in the second half of 1942.

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 not just disputes but the vast majority of *voluntary* wage adjustments in private businesses. Workers exempt included "those in establishments with eight or fewer employees (except certain classes removed from exemption from time to time), those employed by State and local Governments, and those employed by non-profit organizations." The NWLB itself estimated in March 1944 that of 38 million total civilian non-agricultural employees excluding domestic servants, about 32 million were covered by executive order, and of these 7 million were exempt by general order (United States Department of Labor, 1949, p. 538).

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.[2] Wages above the bracket "could not be changed on the basis of gross inter-plant inequities." As a consequence, wages above the bracket were frozen in place.[3]

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 from the necessity of governmental approval 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 demanding when approving

[2]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.

[3]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 the bracket minimum, the primary policy instrument, could be expressed either as a single rate or a range, for companies which had variation within an occupation. In this case, "the weighted average rate for an occupation was compared with the single rate" (United States Department of Labor, 1949, p. 236, volume 1). That is, if a plant currently had a range of 60 to 70 cents and the single rate (minimum) bracket was 85 with a range of 80 to 90, the plant could increase its range to 80 to 90. Because the weighted average to be compared to in this case was the single rate, we have used the single rate as our measure of the level of the bracket.

wage increases 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 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 cases, the non-compliant 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, it is hard to see how illegal wage increases which were mutually agreed upon 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 goal of the NWLB was to aid the war effort, broadly defined. One way in which the NWLB hoped to help was, unsurprisingly, through "wage stabilization" (United States Department of Labor, 1949, p. 178, volume 1), which, in concert with the controls on the prices of goods, was meant to help control inflation. In a speech in April 1945, George W. Taylor, the chairman of the NWLB, gave another important goal of the NWLB: "To help control the movement of manpower into war production" (United States Department of Labor, 1949, p. 182, Volume 1). Taylor in his speech gave the following example: "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." This was why the "stabilization of wages [...] was essential to conserve the available manpower supply and assure maximum production."[4]

We want to emphasize that it was never the explicit goal of the NWLB to reduce inequality. Instead, reducing inequality was only important to the extent that it caused "increas[ing] productivity[,] improved employee morale, stabilized employment, and reduction of work stoppage"

[4]Now the control of (civilian) 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 policies of the WMC were not particularly important in affecting the allocation of labor. 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.

(Derber, 1944).[5] Success was always measured relative to the aims of the war effort, while, for us, the "success" of the NWLB is defined by whether the NWLB actually influenced the distribution of wages during the war. The problem is that direct evidence on what happened to earnings during the war is limited, and, hence, our ability to identity the contemporaneous effects of the NWLB is limited. Vatter (1985) notes that the most notable changes in the income distribution were between 1941 and 1944, after which the income distribution by quintile stabilized. Based upon wage data collected by the Bureau of Labor Statistics during the war years, between January 1941 to October 1942, when the NWLB was granted power to set wages, manufacturing wages increased by seventeen percent, as opposed to fourteen percent from October 1942 until the end of the war. Manufacturing wage rates rose "only" 10.6 percent following the "hold the line" order until the end of the war (Douty, 1950). This is suggestive evidence that the NWLB moderated the rate of wage inflation through the bracket policy.

One indirect reason for believing that the NWLB did affect earnings, at least during the war, 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.[6]

2.2 The Bracket Setting Process

Given this history, it should not be surprising that the process by which brackets were determined was not totally straightforward or formulaic. Record (1944b, p. 576) claimed that "[t]he bracket minimum [. . . wa]s usually set, not at the midpoint or weighted average of rates being paid for a particular job in a particular locality, but 10 per cent below the weighted average, or at the first significant cluster of going rates." Under what circumstances which of these two options was to be used was left unclear, as well as what constituted a "significant cluster" of wages. One memo from NWLB Region X, which covered California and cited by Rose (2009), stated that "In an analysis of 160 brackets set, the California regional board found 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

[5] 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."

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

Termination Report. The analysis was restricted to Region III (in Philadelphia) and found that administrators in 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 variation in the methodology used in setting the bracket, simple geographic divisions played a major role in the bracket a worker was subject to. The 12 NWLB regions were divided into so-called zones, which was the geographic level at which the bracket varied. The general approach to setting up zones 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.

To get a sense of this geographic variation, Figure 1 maps the brackets for stenographers.[7] There is variation in the bracket between groups of states as a function of the NWLB region, denoted by the thick black lines, in which a state is located. 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 also between state variation

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

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 even between county within state variation, most prominently in Region IV that covers many of the southern states.

Brackets were supposed to be determined as a function of local economics conditions, so higher or values of the brackets might simply higher or lower levels of earnings in that area. Just because the level of brackets varies by geography does not necessarily mean the "bindingness" of the brackets varied. 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 are prevented from receiving a raise. This hypothetical distribution of earnings adjusts the distribution of hourly earnings in 1939[8] 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 it assumes no changes in *relative* earnings by occupations and regions.

Figure 2 plots for white collar and metal trades occupations the distribution of the percentiles of the brackets in the hypothetical distribution of earnings, weighted by the size of the occupation-WLB zone cells, for Region I (the Northeast) and Region IV (the South), respectively. As a 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 the 1943 hourly earnings distribution. This finding is roughly consistent with our results for white collar jobs, which tended to be set low in the distribution of earnings. This is not the case for metal trades occupations for which we observe brackets set much higher in the distribution and a much greater degree of dispersion. The comparison of our results to those in Rose is not quite exact since Rose has better information on the 1943 hourly earnings distribution for his (smaller) set of occupations located in a single state. Note that because we are using growth in production worker compensation to calculate the hypothetical earnings distribution, one might have imagined that the resulting hypothetical for metal trades workers is closer to reality than that for white collar workers. As a consequence, if anything, one might have imagined that there would be less dispersion and lower levels of bindingness for metal trades, but it is just the opposite.

Besides these differences by type of occupation, we also find 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. 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. Third, in general, there was more dispersion in the bindingness of the brackets in the South relative to the Northeast. We do not have clear explanations for these latter facts. The broad conclusion we draw is that the variation in the bracket was not simply due to variation in local economic conditions,

[8]Hourly earnings were imputed by dividing labor earnings by 40 hours per week times the number of weeks worked.

and this unexplained variation of the bracket is, in effect, what we will use to identify the effect of the bracket on the wage distribution.

3 Hypothetical Effects of the Brackets

To understand the potential effects of the brackets, 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 reduced 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 like the minimum wage).

These differences in the functioning of the bracket policy from a simple minimum (or maximum) wage create differences in its hypothetical 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.[9] This is not true in the case of a bracket policy. Instead, the effects of the policy depend essentially on changes in the latent distribution of earnings to use the term in Lee (1999).

We now consider the comparative statics on inequality of changes in 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 a sense then, 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, not at the level of pure equality.

Our goal is to isolate the extent to which the bracket policy "masks" changes in the latent distribution of labor earnings inequality. 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

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

the actual level of earnings tomorrow w_{t+1} taking into account the effect of the bracket 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 $\bar{w} = 1$. 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 $\beta < 0$. We adjust the value of α 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.

What are the effects of the bracket in a world of *declining* latent inequality on the observed growth rate mean for changes in inequality measured by the change in the difference between the τ and τ' percentiles with $\tau < \tau'$? First, there is a region of values where the marginal effect of the bracket is zero,[10] another where it is positive, and another where it is negative. No marginal effect can happen for small, medium, and large values of the bracket. For values of the bracket less than the τ percentile, there is no effect on the τ or τ' percentiles themselves and consequently no effect on inequality. For the intermediate values still less than the τ' percentile, the τ' percentile remains fixed while the τ 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 how inequality could be lower in this case, imagine a world where there are only two values for the bracket, 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 τ percentile where the marginal effect is a reduction in inequality. A bracket set to a value in this region allows the τ percentile to rise while freezing the τ' percentile in place. Finally, there is a region of values for the bracket greater than the τ' percentile where just the reverse happens and the marginal effect is negative. Here marginal increases no longer have no effect on the τ percentile, but they do lead to increases in the τ' 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,[11] but they are still able to illustrate three general important points. First, even in

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

[11] It is also possible that the brackets affect inequality in total market earnings including both labor and capital

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 about the counterfactual in which the NWLB never existed. 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.

4 Data

4.1 NWLB Records

We collect primary source records of the brackets set by the NWLB at the occupation-industry-geography level.[12] Unfortunately, some brackets from particular regions have to our knowledge not survived. For example, the termination report from the NWLB contains a count of "approximately twelve-hundred bracket rates [...] in the 11 areas into which Oregon and Washington were divided" (United States Department of Labor, 1949, p. 81, volume 3). This makes it clear that the brackets for this region did exist at one time, but we have been unable to locate surviving brackets from Region XII, covering the Pacific Northwest, in either the National Archives or the regional archive which should hold documents for this region. We also have not been able to locate the brackets from Region VI, headquartered in Chicago. Consequently, both of these regions are not included in our analysis.

In order to develop the bracket system, the Bureau of Labor Statistics (BLS) 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" (United States Department of Labor, 1949, p. 797, volume 1). To carry out this survey, occupations were defined based on the 1939 *Dictionary of Occupational Titles* and sometimes subdivided into grades. The classification of jobs into standardized categories was made by field representatives of the BLS, 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.

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. As United States Department of Labor (1949) stated, "wage rate brackets were established for [only] key occupations in an

earnings. We hope to examine this dimension of inequality in future work.

[12] 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.

industry in a labor market area." The rationale behind this was that since, in the NWLB's view, 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 of workers into different occupations that took 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 unlisted 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 as to why these industries were not listed in the brackets, unlike the missing occupations case where the NWLB is explicit about only creating brackets for "key" occupations and the reasons for that decision. The closest "precedent" we have for how unlisted industries would have been treated comes from a discussion of "isolated plants" for which there was not explicit bracket coverage either. The termination report of the NWLB (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 unlisted industries at the cost of looking at a more limited set of occupations.

4.2 Census of Population

Our outcome and control variables come from the long-form of the 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 then 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 refer to as 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 employment related 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 1/6 in 2000. These fractions are still large enough to create substantial samples except in the case of the 1950 Census, which is too small and riddled with errors to be usable. Second, the labor income variable is top coded to preserve anonymity.[13] These top coded observations are not a major problem

[13]The top codes for years 1960 through 2000 are respectively $25,000, $50,000, $75,000, $140,000, and $175,000. For 1990, top coded values are reported as the median of top coded incomes for a state. For 2000, the mean of the

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 end up being top coded. Third, while the original census forms recorded occupations and industries as raw strings, we use the recoded version of these variables into the `occ1950` and `ind1950` classifications. This makes matching occupations to the NWLB records easier. The cost is the relative coarseness of the 1950 classification.

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 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, this would create a potential concern that changes in the effects of the brackets over time could be due to changes in the composition of women in the work force as the overall rate of women's labor force participation increases. Also following Goldin and Margo, we require that individuals "earn[], 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.[14]

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 issues 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 in the Census.[15] 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

state an individual lived in was used as the top code.

[14] 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.

[15] 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.

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.[16]

5 Conditional Distribution Empirical Strategy

Our first empirical specification examines 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 τ quantile of labor earnings 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 τ' and τ 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 specifications in Rose (2009) and Autor et al. (2016), which were both inspired by the specification in Lee (1999):

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

Note that this is a cross-sectional regression that we estimate year by year. 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.[17]

[16] 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 this were. We also discuss how we handled cases where brackets were defined at a sub-county level, which is only an issue in Region I. For these brackets, we collapse to the county-level by weighting town-level brackets by the town population.

[17] An additional question is what to do about small occupation-county cells for which percentiles might not be

In our preferred specification, the controls include the 1939 level of inequality $\log\left(\frac{Q_\tau(w_{ic1939})}{Q_{\tau'}(w_{ic1939})}\right)$, 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 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 persistence in inequality at the occupation-county level. Even in the cases where 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 1929 and 1939. 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* or conditional 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 Compression, which is a feature of the *unconditional* distribution of earnings. 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 and a useful starting place.

An additional limitation of this strategy (and really any reduced form strategy) is that it will not allow us to identify any spillover effects of the brackets across geographies or occupations. Implicitly, the NWLB believed in such spillovers, at least along the occupational dimension. This was why they thought 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) argue that these "spillovers" are simply due to measurement error. To the extent these spillovers exist, it is plausible that, if anything, they lead us to underestimate the "true" effect of the brackets on inequality *within* occupation-county groups.

The question here of how the NWLB affected inequality is similar to that in the third chapter of Rose (2009). However, 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 an important 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

unique. We consider two approaches if a percentile is non-unique: (1) 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.

identification strategy. In contrast, the BLS surveyed sporadically different occupations and towns in a handful of years resulting in a 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.

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.[18] 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 piece of evidence, we regress the brackets on various statistics of the 1943 hourly earnings distribution calculated by county and occupation.[19] 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. The narrative evidence suggests that the NWLB, in most cases, 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, shows that, while the relationship between the mean is statistically and economically significant, the elasticity is substantially smaller than 1. We also observe statistically and economically significant positive relationships of similar magnitudes between the bracket and 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 statistically significant. The last column includes average demographic characteristics for those in 1940 in a given occupation-WLB zone as predictors. There is some 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

[18] 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.

[19] To limit the influence of outliers, we calculate the mean after winsorizing the 5% tails of the hourly earnings distribution.

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 potentially 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 of the brackets by collecting 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).[20] We then estimate the relationship between the bracket $\bar{w}$ and these war effort variables WarEffort at the occupation-county level:

$$\log \bar{w}_{ic} = \beta \text{WarEffort}_c + \text{Controls}_{ic} + e_{ic}. \tag{1}$$

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.[21] 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 4 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

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

[21] 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 also a more subtle story to tell.

capture the total effect of these war related policies on the bracket. This includes both indirect 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 persistent; *and* (3) there had been no changes in the inequality of the latent earnings distribution.

Figure 5 plots these effects by year with 95% confidence intervals for the changes in the $\log(Q_{10}/Q_{90})$, $\log(Q_{10}/Q_{50})$, 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 this result, 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 over this period. The effect in 1969 is marginally statistically significant, but, 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 some systematic relationship between the bracket and earnings. It is still quite striking that effects are observable 25 years after the end of the war and the wage controls. We discuss later some possible mechanisms for this persistence.[22]

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 $\Delta \log(Q_{10}/Q_{50})$.[23] Strikingly, the effects at the top are more persistent than the overall effects. Even in 1979, the effects at the top are statistically signifiant 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

[22] In the appendix, we report the results for differing sets of fixed effects as well for the $\Delta \log(Q_{25}/Q_{75})$ measure of inequality.

[23] 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 across the regressions for each statistic.

brackets associated with increases in inequality. One possible explanation for this puzzling result is that during a period of declining latent inequality, which perhaps 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.

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 $\log(Q_{10}/Q_{90})$ for this component is due to changes in the $\log(Q_{50}/Q_{90})$. Both of these broad patterns seem consistent with our 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.[24]

As a point of comparison, Roosevelt also introduced policies on Oct. 15, 1942 to control salaries and wages for top earners. These included a cap on top salaries and a limit on salary increases. The first limited labor earnings to $25,000 after federal income taxes were paid. The second prohibited salaries of more than $5,000 from rising above their level as of Sept. 15, 1942. The first salary cap policy was repealed by Congress within 6 months after it was introduced, meaning it had no effect in the end. However, the prohibition on salary changes remained in effect until November 1946. Exceptions to this policy were allowed in certain circumstances, but firms were required to formally request approval of such an increase from the Treasury. Frydman and Molloy (2012) state that about 750,000 applications equivalent to about 30% of covered individuals were processed between 1942 and 1946.

Frydman and Molloy (2012) argue that this policy might have had some effects as evidenced by the fact that the fraction of executives who did not receive a pay increase between 1943 and 1945 was double the rate in the prewar and postwar periods. However, 25% of executives still received large wage increases during this period. Furthermore, real average CEO compensation had already recovered to its pre-war level by the 1950s. Because of these, Frydman and Molloy (2012) view the effects of the war pay polices on differences in earnings between executives and other workers as modest and not long lasting.

This contrasts with our results that the NWLB had persistent effects on inequality. As we

[24] 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. The fact that we focus on a limited set of occupations also makes us cautious about how far we should push this comparison.

discuss in greater detail below, we think the differences in the effects of these policies highlights the unique way after the war private sector unions used the brackets as a pattern for wage bargaining. It is true that Frydman and Molloy (2012) also emphasize the important of unions in explaining why CEO compensation remained relatively low through the 1970s, but, apparently for whatever reason, the limits on executive compensation during the war never came to serve as a reference point for bargaining on this point in later years.

6.1 Effects on Racial Inequality

We now examine effects of the brackets on the racial earnings gap. As mentioned earlier, the NWLB was quite explicit that that the brackets were supposed to apply uniformly within an occupation-zone. There were no brackets by race or gender. Because of this, it is plausible that the brackets could have affected the Black-white earnings gaps.[25]

Figures 8 shows the effect for the racial earnings gap. Like in our previous specifications, a positive coefficient implies that a higher value of the bracket *reduces* the within-occupation component of racial 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 at least in our preferred specification that includes both occupation and county fixed effects. There is perhaps some compressing effects in the less saturated effects. Overall, even though the brackets were set in this uniform way, they did not end up reducing the racial dimension of economic inequality.

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 earnings gap, which are certainly quite large. It is possible that the brackets did affect overall racial inequality by affecting the sorting of races across occupations, for example. This mechanism would not be captured by these specifications. That said, 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.

7 Unconditional Distribution Empirical Strategy

The occupation-county level specifications estimate the effects of the brackets on the conditional distribution of earnings, but have only an indirect connection to the effects on the unconditional distribution. To identify these unconditional effects, we use an extension of the *unconditional quantile regression* (UQR) approach, originally developed by Firpo et al. (2009).[26] To estimate

[25] To remain consistent with Goldin and Margo, our sample excludes women so we cannot examine effects on the gender earnings gap.

[26] 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

the effect on the τ-quantile of the distribution of labor earnings w, the first step is transform the earnings of individual w_i using the *recentered influence function* (RIF):

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

where f_w is the density of earnings and Q_τ the τ-th quantile. As shown by Firpo et al. (2009), a regression of $\psi(w_i, \tau)$ on the bracket and a set of controls denoted by X_i, under certain assumptions, recovers the marginal effect on the τ-th quantile of the unconditional distribution. Note that in contrast to the conditional specifications, the dependent variable here is simply the level of a percentile in a given year, not the change relative to 1939.

We use an extension of the UQR methodology developed by Martínez-Iriarte et al. (2022) that allows us to separate the effects of a marginal change in the *mean* level of the brackets from a marginal change in the *standard deviation* of the brackets. The former, known as the location effects, are the effects estimated in Firpo et al. (2009). The so-called scale effects measure how the unconditional distribution of earnings would change due to 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?

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. 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 τ-th quantile of the unconditional distribution of w due to a marginal change in δ defined as

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

where w_δ is the counterfactual distribution of w given $\bar{w}_\delta$. Note that if we estimate the marginal effects for the τ and τ'-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

$$\Pi_\tau = \underbrace{\Pi_{\tau,L}}_{\text{Location Effect}} + \underbrace{\Pi^{\mu}_{\tau,S}}_{\text{Scale Effect}},$$

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

$$\begin{aligned}\Pi_{\tau,L} &= -\frac{\partial l(0)}{\partial \delta}\frac{1}{f_w(Q_\tau(w))}\int_X\int_{\bar{w}}\frac{\partial F_{w|X,\bar{w}}(Q_\tau(w)|x,\bar{w})}{\partial \bar{w}}f_{X,\bar{w}}(x,\bar{w})dx\ d\bar{w},\\ \Pi^{\mu}_{\tau,S} &= -\frac{\partial s(0)}{\partial \delta}\frac{1}{f_w(Q_\tau(w))}\int_X\int_{\bar{w}}\frac{\partial F_{w|X,\bar{w}}(Q_\tau(w)|x,\bar{w})}{\partial \bar{w}}f_{X,\bar{w}}(x,\bar{w})(\bar{w}-\mu)dx\ d\bar{w}.\end{aligned}$$

The critical assumption for this result is that the distribution of e must satisfy a conditional independence assumption, either

$$f_{e|\bar{w},X}(e|\bar{w},x) = f_{e|\bar{w},X}(e|\bar{w}_\delta,x) = f_e(e),$$

or

$$f_{e|\bar{w},X}(e|\bar{w},x) = f_{e|\bar{w},X}(e|\bar{w}_\delta,x) = f_{e|X}(e|x).$$

The second assumption is more palatable in our setting. The first part of the assumption $f_{e|\bar{w},X}(e|\bar{w},x) = f_{e|\bar{w},X}(e|\bar{w}_\delta,x)$, as the authors note, is related to the assumption in Firpo et al. (2009, pp. 955-957), framed as "maintaining the conditional distribution of Y given X unaffected" (though there it is a slightly stronger assumption that $f_{e|\bar{w}}(e|\bar{w}) = f_{e|\bar{w}}(e|\bar{w}_\delta)$). Martinez-Iriarte et al. then show that this marginal effect is equal to a particular weighted average of the derivative of the RIF:

$$\Pi_\tau = -\mathbb{E}\left[\frac{\partial \mathbb{E}[\psi(w,\tau)|\bar{w},X]}{\partial \bar{w}}\kappa(\bar{w})\right],$$

where ψ is the RIF for the τ-th quantile and

$$\kappa(\bar{w}) = \frac{\partial s(0)}{\partial \delta}(\bar{w}-\mu) - \frac{\partial \ell(0)}{\partial \delta}.$$

This result generalizes the central one in Firpo et al. (2009), which is the special case of $\kappa(\bar{w}) = 1$.

What then is the scale effect measuring? Define the τ-quantile-$\bar{w}$-standard-deviation elasticity as

$$\varepsilon_{\tau,\delta} = \frac{dQ_\tau[w_\delta]}{Q_\tau[w_\delta]}\left(\frac{d\sigma_{\bar{w}_\delta}}{\sigma_{\bar{w}_\delta}}\right)^{-1}.$$

Setting $\frac{\partial s(0)}{\partial \delta} = 1$ and assuming w is log earnings, then Martinez-Iriarte et al. show that

$$\varepsilon_{\tau,\delta} = -\Pi^{\mu}_{\tau,S}.$$

In words, a 1% decrease in the standard deviation of $\bar{w}$ results in a $\Pi^{\mu}_{\tau,S}$% change in the τ-th quantile of log labor income w. Martinez-Iriarte et al. offer a couple examples to understand the meaning of the scale and location effects. For example, assume that $w = \alpha + \beta\bar{w} + e$ as well as

that $\bar{w}$ and e are standard normals, then, for $\mu = 0$,

$$\Pi_{\tau,L} = \beta,$$

$$\Pi^0_{\tau,S} = -\sqrt{\frac{\beta^2}{\beta^2+1}} Q_\tau[\beta\bar{w}] = \sqrt{R^2} Q_\tau[\beta\bar{w}],$$

where R^2 is the coefficient of determination between w and $\bar{w}$. In this case, the location effect is constant across quantiles and the sign of the scale effect is determined by the sign of $Q_\tau[\beta\bar{w}]$. Interestingly in this case, the sign of the scale effect does not depend on the sign of β. The more general result is that if $\bar{w} - \mathbb{E}[\bar{w}]$ is symmetrically distributed around 0, then the scale effect for $\mu = \mathbb{E}[\bar{w}]$ does not depend on the sign of β (nor $\mathbb{E}[X]$).

Estimating the location and scale effects is straightforward using plug-ins for unknown population quantities. Let $\widehat{Q}_\tau(w)$ be an estimator of the τ-th quantile of w, $\hat{f}_w(\widehat{Q}_\tau(w))$ 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.[27] The maximum likelihood estimator of $\theta_\tau = (\beta_\tau, \alpha_\tau)$ is

$$\hat{\theta}_\tau = \underset{\theta_\tau}{\text{argmax}} \sum_{i=1}^{n} \left\{ \mathbb{1}\left[w_i \leq \widehat{Q}_\tau(w)\right] \log[G(\theta'_\tau Z_i)] + \mathbb{1}\left[w_i > \widehat{Q}_\tau(w)\right] \log[1 - G(\theta'_\tau Z_i)] \right\}.$$

Then estimators for the location and scale effects are

$$\begin{aligned}
\hat{\Pi}_{\tau,L} &= -\frac{\hat{\beta}_\tau}{N \hat{f}_w(\widehat{Q}_\tau(w))} \sum_{i=1}^{N} g(\hat{\theta}'_\tau Z_i), \\
\hat{\Pi}^{\hat{\mu}}_{\tau,S} &= \frac{\hat{\beta}_\tau}{N \hat{f}_w(\widehat{Q}_\tau(w))} \sum_{i=1}^{N} g(\hat{\theta}'_\tau Z_i)(\bar{w}_i - \hat{\mu}),
\end{aligned}$$

where $g = G'$ and $\hat{\mu}$ is the average value of $\bar{w}_i$. The controls X_i include not just those used in the conditional specifications, but also individual-level "Mincerian" or demographic controls of 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 for the brackets, they strengthen the claim that we are estimating causal effects. The same questions and concerns about endogeneity raised regarding the conditional specifications apply to these unconditional specifications as well.

We can see then that the scale or location effects by percentile depend on differences in $\hat{\beta}_\tau$. If the bracket does not affect the conditional distribution of $F_{w|X,\bar{w}}$, i.e., $\hat{\beta}_\tau = 0$, then both the

[27] The nonlinearity of G is important. We show in the appendix that if G is taken to be linear, then the scale effect in the case when μ is the sample average of $\bar{w}$ will be identically 0.

location and scale effects will be equal to 0, and this is the only scenario under which the location effect for a particular quantile will be 0. The scale effect could be 0 even if $\hat{\beta}_\tau$ were not equal to 0. On the other hand, the value of $\hat{\beta}_\tau$ only has indirect effects through $g(\hat{\theta}'_\tau Z_i)$, which includes $\hat{\beta}_\tau$, on the relative magnitude of the location and scale effects for a given quantile. Instead what matters is, relative to $\hat{\mu}$, the weighted average value of $\bar{w}_i$ where the weights are $g(\hat{\theta}'_\tau Z_i)/\sum_{i=1}^{N} g(\hat{\theta}'_\tau Z_i)$.

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 Census years. Besides the point estimates, we report the 95% confidence interval based on the 5th and 95th percentiles of the bootstrap estimates. While Martínez-Iriarte et al. (2022) provide a formula for the asymptotic variance, based on correspondence with the authors, we instead use the bootstrap[28] because the value of μ we use is estimated rather than calibrated. The bootstrap samples are constructed by sampling without replacement from the data.

Figure 9 shows the location effects. Recall that the distribution of earnings in 1943 is hypothetical. It is the 1939 earnings distribution adjusted for the growth in earnings for compensation for production workers between 1939 and 1943 with the brackets imposed. The results then are also hypothetical and suggest what the effects of the brackets would be without any changes in the inequality of the latent earnings distribution and perfect compliance with the policy. We do not think these results should be interpreted as placing an upper bound in magnitude for the effects in subsequent years. Instead, these estimates should be taken as a ballpark for estimates in subsequent years. Overall, we find statistically significant but relatively modest location effects of the brackets for all the percentiles we consider.

In the first postwar year of data we have, in 1959, we find a similar pattern of positive effects of an increase in the mean of the bracket for the bottom percentiles of the earnings distribution and negative effects for top percentiles. 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.

While we still observe significant effects in 1969, the location effects diminish in magnitude over time and, by 1979, are near zero for almost all the percentiles. This is consistent with the idea that, while the brackets could have guided the patterning of wages for a time, this effect would dissipate, particularly with the decline of union power and the rigid pay schedules that come with

[28] 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.

union control. Still, we find it interesting that more than 2 decades after the bracket policy ended, its effects on the bottom tail of the earnings distribution can still be detected.

There is no necessary reason why the magnitude of the location effects should be ordered by percentile. The fact that they are supports the idea that we are capturing the actual effects of the policy. A higher bracket would have allowed lower paying workers to receive a larger raise (or a raise at all) with that increase in pay largest for the lowest paid workers. As for the top percentiles, the negative effects are hard to explain. They are close to 0 and a null effect would not be unexpected. As far as we can tell, brackets were never set that high in the earnings distribution so marginal changes in the mean value of the bracket should not affect the fact that earnings for those at the top would have been frozen by the bracket policy.

It is the case that, in some years and some top percentiles such as for Q_{90} in 1969, the location effects are substantially negative. These are difficult to explain. The number of people eligible to receive a raise is non-decreasing in the mean value of the bracket. This direct effect must then be offset by some indirect effect to explain the cases of a negative overall effect. One possible explanation is that there is a fixed pool of money for workers. So higher earnings for those earning the least caused by an increase in the bracket must be offset by lower earnings for those earning the most. This potential negative spillover across the percentiles of the earnings distribution is opposite of how minimum wage spillovers are usually assumed to operate. In that case, firms must raise wages for those earning slightly higher than the minimum wage to maintain a certain earnings hierarchy. Both our theory of spillovers and that for the minimum wage emphasize the institutional nature of wage setting. Earnings are determined not simply as a function of relative productivities across workers, but also as function of other considerations such as "fairness", a view strongly supported by a major figure in the NWLB, John T. Dunlop.

Our estimates do not really allow us to answer the question of how much of the Great Compression in earnings inequality was due to the NWLB, or, equivalently, how much higher would inequality have been in the decades following the war if the NWLB had never happened. As we suggested earlier, we could conceptualize that counterfactual as one where the brackets had been set arbitrarily high allowing all raises to be approved. The problem is that, in our view, given that the brackets were actually set in "meat" of the distribution, our estimates of the marginal effect of the brackets does not really have much to say about what would have happened in that counterfactual, particularly, for the upper percentiles. Taking the estimates at face value implies that inequality, at least in 1959 and 1969, would been *lower* without the NWLB.

The best we can do relating these estimates to the Great Compression is to compare the marginal effects to the overall changes in inequality. For example, Goldin and Margo report that the $\log(Q_{10}/Q_{90})$ of earnings increased by nearly 30 log points between 1939 and 1959. The location effect for the $\log(Q_{10}/Q_{90})$ in 1959 is about 0.3 so a 10 log point increase in the mean value of the bracket would have increased the $\log(Q_{10}/Q_{90})$ by approximately 3 log points, or 10% of the overall change. This suggests to us that the NWLB could have played a substantial role in explaining the

much more equal earnings distributions in 1959 and 1969 relative to 1939. It is also intriguing that, at the same time as the income distribution begins to widen in the 1970s, the effects of the NWLB attenuate hinting at the ways in which the NWLB masked changes in the latent distribution of earnings over the first decades after the war.

The fact that the largest effects in this specification are for the bottom percentiles is not inconsistent with the fact that in the conditional specifications we found the largest effects for the upper tail inequality. The results here show that higher values of the bracket push up the bottom mass of the distribution, and, as a consequence, we would not expect major effects on inequality between those at the bottom and those toward the middle, which is exactly what we find in the conditional specifications. On the other hand, we found that the bracket does not affect the highest percentiles. Therefore, inequality between the top and the middle of income distribution should fall, which is also what we find in the conditional specifications.

Figure 10 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? 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 from 1980 onwards. 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.

Why are the scale effects so small? The analytical results for the special case of normal covariates and noise point to the role of the magnitude of the R^2 in determining the magnitude of the scale effects. We do not actually accept this model since it also implies that the location effect is independent of the quantile, which is inconsistent with our empirical results. Nonetheless, based on the intuition of that special case, we interpret the small scale effects as reflecting that fact that the brackets do not explain much of the variation in earnings (after controlling for the other observables).

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 for more than 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.

Even before the war concluded, the NWLB itself suggested that wage controls were likely to have enduring effects. George Taylor, the vice chairman of the NWLB, suggested that the bracket

program "is helping to develop collective bargaining by providing a vast reservoir of information about clearly defined job classifications and wage rate schedules from which the parties can draw facts relevant to their negotiations, both now and after the war" (National War Labor Board, 1944, p. III). At least some members of the NWLB also believed that in the long run, unionization would be encouraged by the experience during the war. In the termination report for the NWLB, the Region IV statement suggested that following the NWLB experience, "today a more wide-spread appreciation and knowledge of balanced wage structures, job classifications, job descriptions, and of good industrial relations practices, which would be beneficial towards the development of collective bargaining" (National War Labor Board, 1944, p. 663).[29]

The chief of the Research and Statistics Branch of the NWLB, John T. Dunlop, who became a very prominent scholar of industrial relations, would have had no problem believing in the persistent effects of the brackets. He emphasized throughout his scholarly career the centrality of occupational hierarchies and internal labor markets in determining the "correct" level of wages. This was a point that was reflected in the way the NWLB set brackets from the beginning. Things like the marginal productivity of an occupation played no (direct) role in the determination of brackets and, for Dunlop, at most an ancillary role in actual wage setting decisions after the war and the end of explicit government controls on wage setting.

This process of "enshrining" the distribution of wages set by the NWLB after the end of the war began with the US entrance into the Korean War in 1950. 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). Presumably, 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, three years after the end of the Korean War and the dissolution of the Korean War Wage Stabilization Board.[30]

After the end of the war-era wage controls, a critical mechanism perpetuating the effects of those controls were 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 the steel industry. The War Labor Board served as a catalyst in stimulating the formulation of a much-needed job

[29] According to the same report, during the war itself, the NWLB *held back* collective bargaining precisely because "union requests came to approximate Board policy" (National War Labor Board, 1944, p. 663).

[30] Unfortunately, we do not have information on the actual wages set during the Korean War so we cannot directly explore the effects of this agency.

classification and rational wage rate structure in the steel industry." As another example, 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 these are just anecdotes, we take it as suggestive evidence that unions were generally satisfied with the wage structure imposed by the NWLB, and this led to the persistent effects on the income distribution. Focusing on executive compensation, Frydman and Molloy (2012), like us, emphasize the importance of unions. While viewing the direct effects on executive salaries of war related policies as limited, they find that the changes in the effects of unionization on compensation can explain all of the decline in average pay during the 1940s and into the 1950s.

It is intriguing then 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.[31] It is also interesting to wonder whether the persistent effects of the NWLB itself drove part of the decline. For 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." This suggests that while unions might have been happy about the structure provided by the NWLB, the workers themselves might not have been.

Besides effecting bargaining patterns of unions, the brackets could have also affected private wage setting through their effects on what is perceived as a "fair" wage. Thurow (1975) seemed to think that these "differentials became the new standard of relative deprivation and were regarded as 'just' even after the egalitarian pressures of WWII had disappeared." Piketty and Saez, in their

[31] One 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.

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 by Goldin and Katz and much earlier by Brown (1977). Unfortunately, like the effects of the Korean War Wage Stabilization Board and unions, 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. For some, the postwar period in the United States represents an appealingly low level of inequality. We show that, at least to some degree, this distribution was shaped directly by wartime controls. 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. To the extent that the war represents a special period in American history, this suggests that recovering those levels of inequality may prove difficult.[32]

The key question that remains to be fully answered is through what channels the brackets had these persistent effects lasting for more than a decade after the end of this policy. In the few years after the end of the WWII, the general policy of the NWLB was reembodied in the wage setting approach taken by the federal government during the Korean War in the 1950s. In the 1960s and 1970s, we have suggested that the wage distribution induced by the brackets functioned as the reference point for bargaining between unions and employers, thereby creating persistence in income distribution. 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 other consequences of the NWLB. What were the political consequences of the leveling effects of the NWLB? 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?

[32] See Scheidel (2017) on the income-compressing effects of war more generally.

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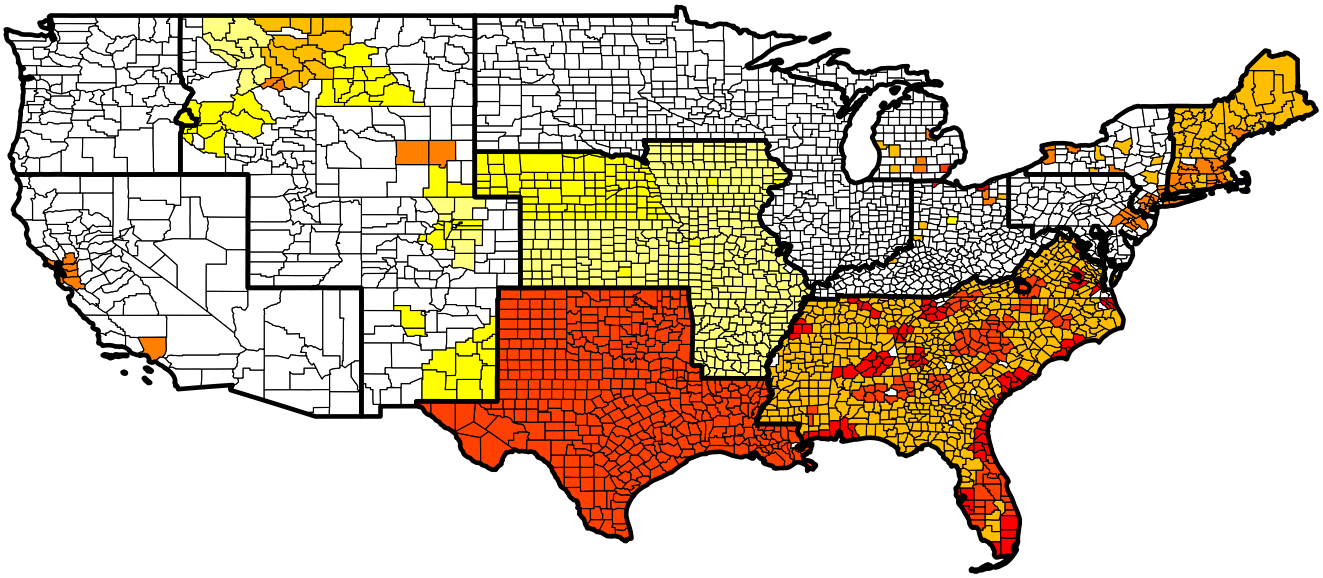
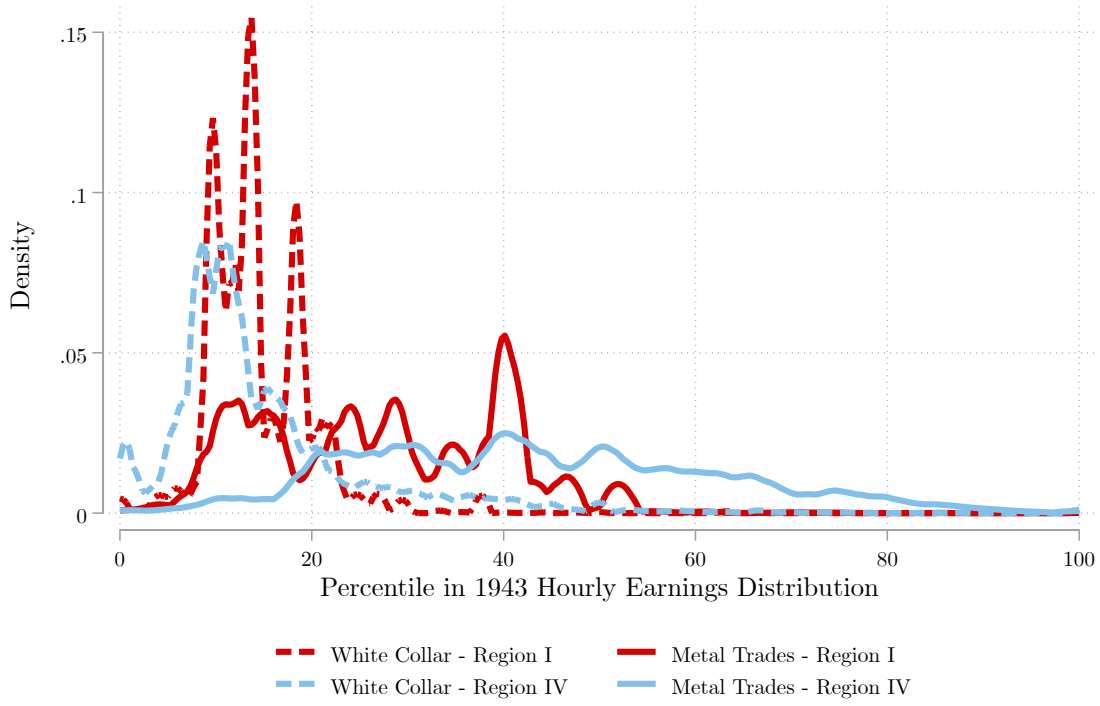
Figure 1: Brackets for Stenographers



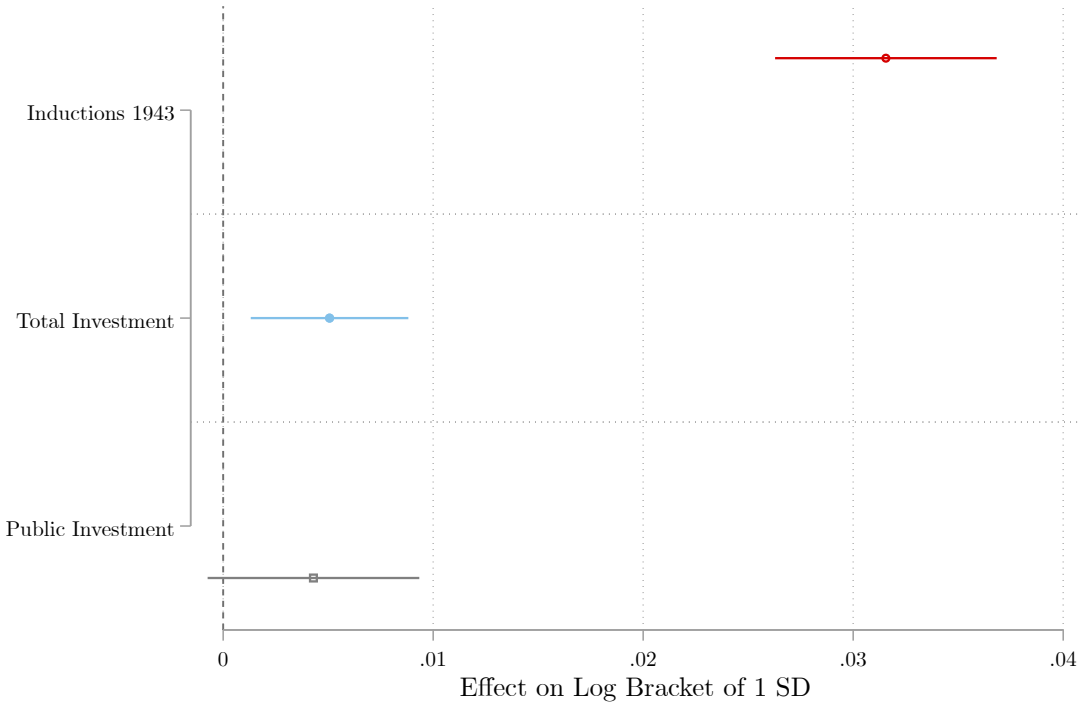
Notes: Darker values represent higher values for the bracket. Region I, which covers the northeast, defines brackets at the town-level. So a shaded county in that region means that the county includes a city with a bracket assigned. For the other regions, brackets are assigned at the county-level. The thick black lines represent borders of NWLB regions. Counties are unshaded either because (1) the county is not listed in the brackets; (2) the county had no stenographers in the 1940 Census; or (3) the brackets for a region are missing or have not been collected.

Figure 2: Distribution of Brackets in the Hypothetical 1943 Hourly Earnings



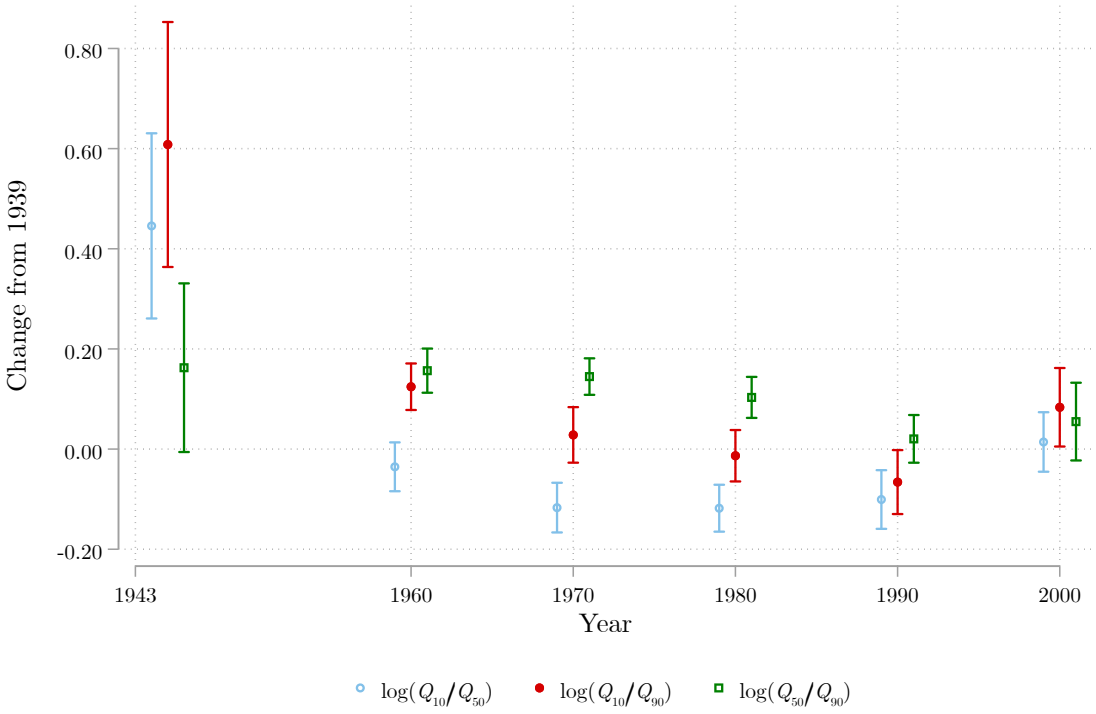
Notes: The percentile is calculated conditional on occupation and NWLB zone. The 1943 hourly earnings variable is calculated using reported labor income and weeks worked in 1939 assuming a 40-hour workweek. Earnings are adjusted for growth in production worker compensation between 1940 and 1943.

Figure 3: Predicting the Brackets Using War Effort Variables



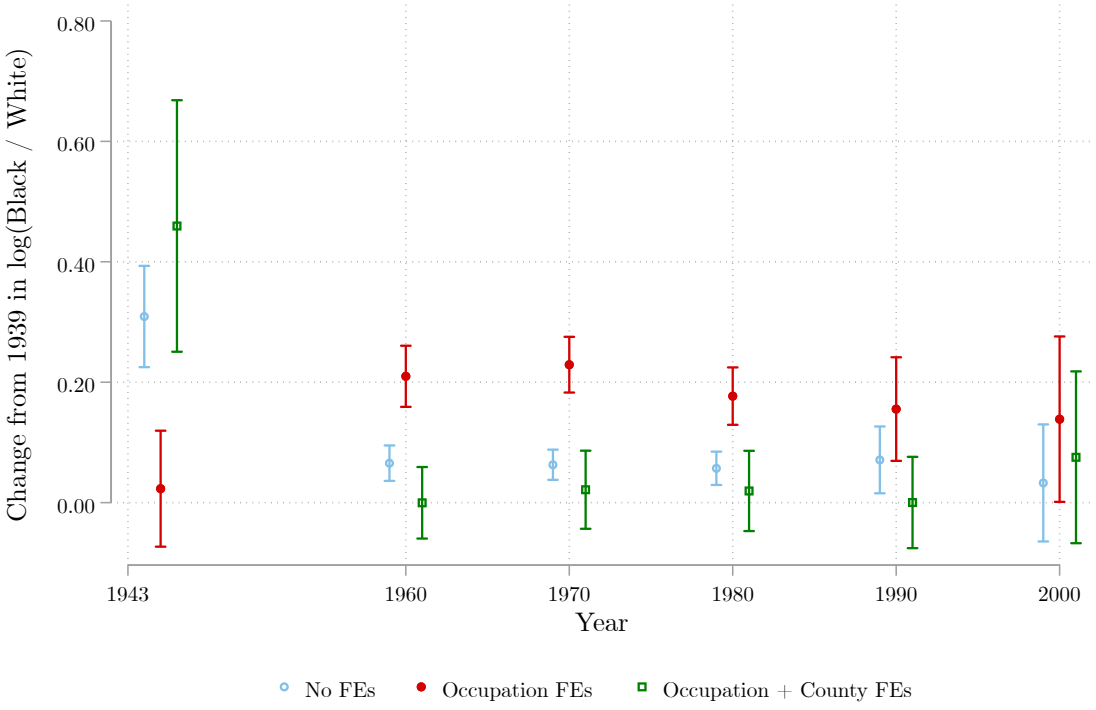
Notes: The state-level variable "Inductions 1943" is the number of men inducted into the army in 1943 and log transformed. The county-level variables "Total Investment" and "Public Investment", which are in \$100,000s, measure investments made in military production overall or by the public sector. All regressions include the mean, Q_{10}, Q_{25}, and Q_{50} of the 1943 hourly earnings distribution conditional on the occupation-WLB zone level. We report of the effect of a 1 standard deviation increase in each of these variables on the log of the bracket. Standard errors are clustered at the county-level.

Figure 4: Effects of the Brackets on the Conditional Distribution of Earnings



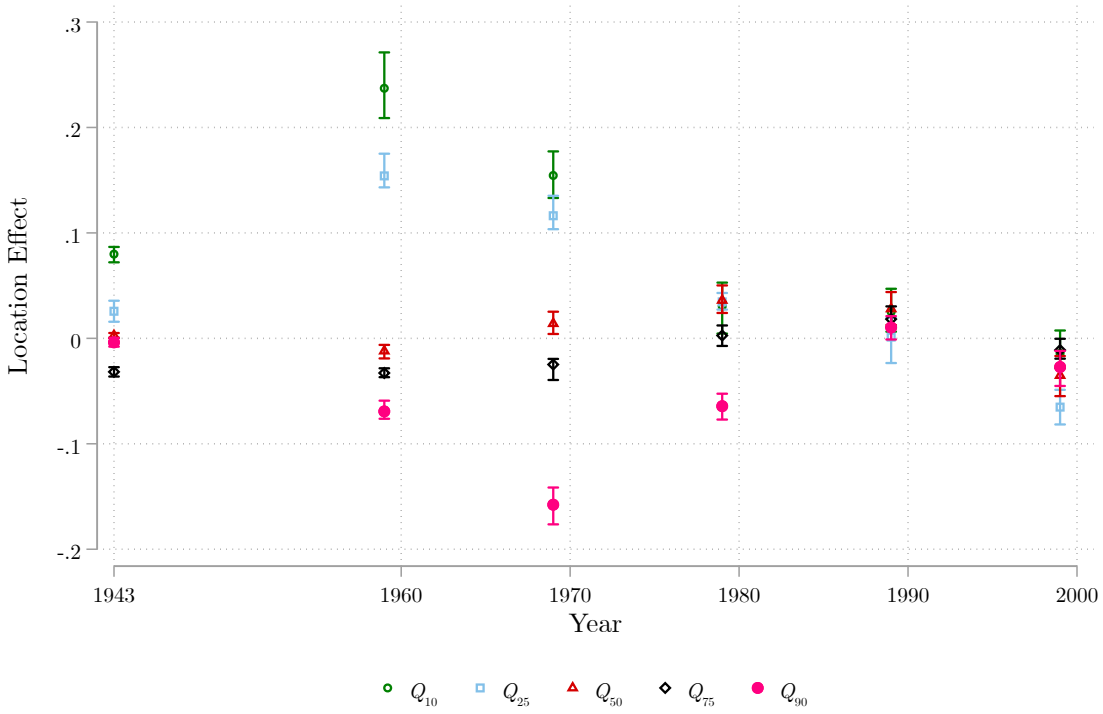
Notes: The dependent variable is the change between the given year and 1939 in the log ratio of the percentiles of labor earnings. Controls include the value of the statistic in 1939 as well as occupation and county fixed effects. The point in 1943 is the result of running the same specification as the other years using the hypothetical earnings distribution in 1943 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 5: Effects of the Brackets on the Conditional Black-White Earnings Gap



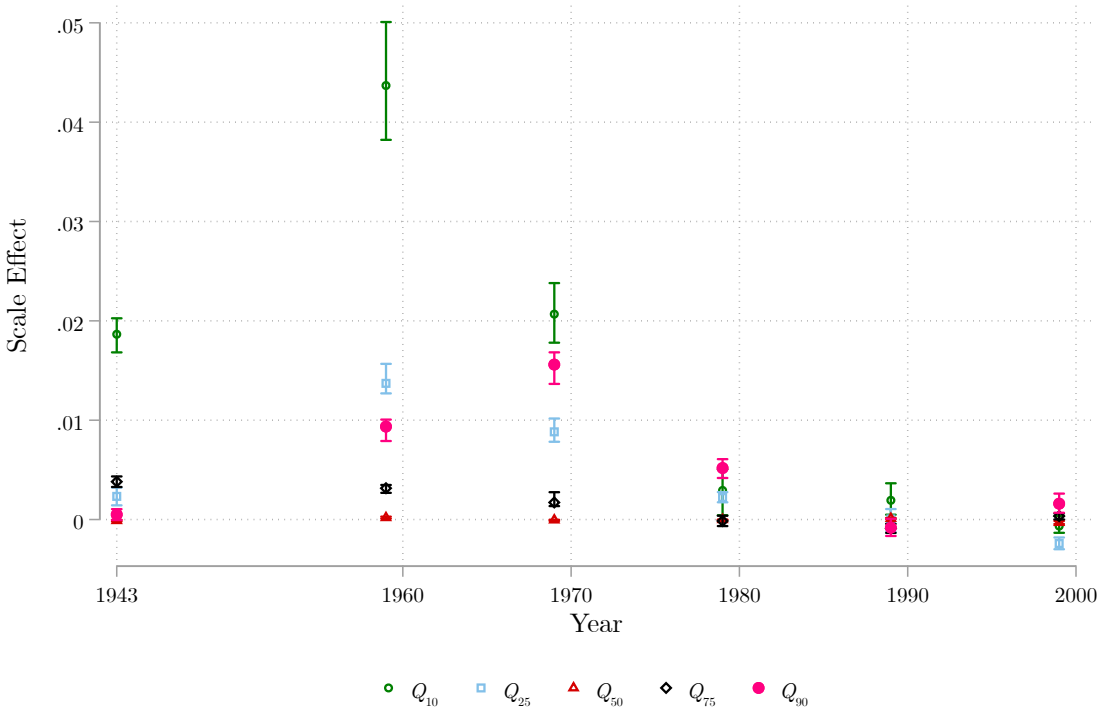
Notes: The dependent variable is the difference between the value of the statistic of log labor earnings in the given year and 1939. Regressions include the inequality value in 1939 and its square. The point in 1943 is the result of running the same specification as the other years using the hypothetical earnings distribution in 1943 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 6: Location Effects of the Brackets on the Unconditional Earnings Distribution



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 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. Specifications include both county and occupation fixed effects.

Figure 7: Scale Effects of the Brackets on the Unconditional Earnings Distribution



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 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. Specifications include both county and occupation fixed effects.

Table 1: Predicting the Brackets Using the 1943 Hourly Earnings Distribution

	Bracket					
	(1)	(2)	(3)	(4)	(5)	(6)
Q_{10}	0.254				-0.033	-0.026
	(0.007)				(0.140)	(0.137)
Q_{25}		0.309			0.090	0.070
		(0.007)			(0.098)	(0.095)
Q_{50}			0.340		0.172	0.155
			(0.007)		(0.092)	(0.093)
Mean				0.350	0.123	0.137
				(0.007)	(0.139)	(0.132)
Married						0.085
						(0.080)
Age						-0.003
						(0.005)
White						0.270
						(0.104)
HS Graduate						-0.113
						(0.087)
Observations	13868	13868	13868	13868	13868	13868

Notes: An observation is a occupation-county. The sample restrictions are the same as in Goldin and Margo (1992). The statistics Q_{10}, Q_{25}, Q_{50}, and Mean are from the 1943 hourly earnings distribution and in logs. The 1943 hourly earnings variable is calculated using reported labor income and weeks worked in 1939 assuming a 40-hour workweek. Earnings are adjusted for growth in production worker compensation between 1940 and 1943. We winsorize the 5% tails of the hourly earnings distribution. The variables Married, Age, White, and HS graduate are averages of those demographic characteristics. Standard errors are clustered by occupation and NWLB zone.