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 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 through 1980, the brackets had opposing effects on inequality at the top and bottom of the income distribution with the overall effect on the gap between 10th and 90th percentiles muted. These results are consistent with a "mechanical" theory of how the brackets worked while in force. We suggest that even after the brackets were no longer legally binding, they still affected the earnings distribution through their impacts on bargaining between employers and unions.

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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 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 as well as newly-digitized records from the Bureau of Labor Statistics (BLS). 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 impose wage cuts for high earners, we refer to the policy as a "soft" maximum, in contrast to the "hardness" of a minimum wage, which makes illegal paying wages below the minimum.

To organize our empirical results, we first develop a mechanical theory of the effects of the brackets. We call the theory mechanical because we assume away any general equilibrium effects of the brackets and assume that there were no violations of the brackets. While these are both very strong assumptions, we argue that this model provides a useful benchmark for interpreting the empirical results. One important implication of the model is that the marginal effect of the bracket on inequality will depend on where the bracket is set in the earnings distribution. For example, if we consider the change in the log difference of the 10th and 90th percentiles $\log(Q_{10}/Q_{90})$, then the marginal effect of an increase in the bracket is positive assuming the bracket is set at the 10th percentile, but it is negative if it is initially set at the 90th percentile. Another implication is that the bracket could have different effects on inequality between the median and the top vs. the median and the bottom of the earnings distribution. For example, when the bracket is set in an intermediate range greater than the median, a marginal increase in the bracket reduces inequality at the top while increasing inequality at the bottom by the same amount meaning the gap between the top and the bottom of the earnings distribution remains constant.

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 either to the

first "cluster" of wages or to 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 region. 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. To support this assumption, we provide evidence that the brackets were not correlated with other war related policies such as military contracts after controlling for the wage distribution.

Given our design that relies on variation by (detailed) occupation and county, 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 NWLB and the Census is relatively clear. We supplement these records by transcribing the Occupational Wage Surveys from 1953 from the BLS. The BLS records have the advantages of occurring relatively soon after the dissolution of the NWLB, and in having occupational classifications which are a nearly perfect match to the NWLB. The drawback of this source is more limited geographic coverage as well as a lack of individual-level information.

We 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.

We focus on the change in the log ratio of the 10th to 90th percentiles of earnings $\Delta \log(Q_{10}/Q_{90})$ and its individual components, $\Delta \log(Q_{50}/Q_{90})$ and $\Delta \log(Q_{10}/Q_{50})$. We find that through 1980, the overall effects on $\Delta \log(Q_{10}/Q_{90})$ hide offsetting effects on $\Delta \log(Q_{50}/Q_{90})$ and $\Delta \log(Q_{10}/Q_{50})$. For example, in 1970, the positive marginal effect of the bracket on $\Delta \log(Q_{50}/Q_{90})$ is almost perfectly offset by the negative effect on the $\Delta \log(Q_{10}/Q_{50})$ resulting in a small effect on $\Delta \log(Q_{10}/Q_{90})$. After 1980, there are no detectible effects on any of these inequality measures. Nevertheless, it is striking that more than three decades after the end of the NWLB, the imprint of the wage controls

[1] Unfortunately, there are problems with the available 1950 Census microdata in the Census Research Data Centers, which prevents us from using it. The newly released IPUMS full-count version of the 1950 Census has transcription issues with the income variable, which render it useless for our purposes as well.

on the earnings distribution can be still identified.

Overall, these results are qualitatively consistent with the mechanical theory while the brackets were in effect, how do we explain the persistence of the effects more than 2 decades after the end of the war and the NWLB? We first note that the wage controls to a certain extent were brought back during the Korean War. After the end of that war, we argue based on some case studies that unions when bargaining with employers over wages used the brackets as a starting point either explicitly or implicitly. The fact then that the effects of the bracket fade out by 1980 is consistent with the rapidly declining power of unions starting in the 1970s.

We also examine whether the brackets affected by the Black-white wage gap. In principle, the brackets were supposed to apply uniformly to all races. If this policy was actually followed, then marginal increases in the bracket would reduce the *within occupation* racial gap, which is exactly what we find empirically. What this means for the overall racial gap is not so easy to see because of the fact that Blacks and whites did not tend to work the same jobs. How the brackets affect the between occupation component of the racial gap is an empirical question that depends on the empirical relationship between the mean and dispersion of earnings. In fact, we find minor effects on the overall racial gap of the brackets highlighting the importance of the between occupation component.

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. In other related work, we (Vickers and Ziebarth, 2024) try to bound the effects of the brackets could have had on within and between inequality in the short-run. We conclude that while the brackets could have reduced within occupation inequality, given how the brackets were determined, their effects on between occupation (and other dimensions) would have been more limited.

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. 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 only wage data from Occupational Wage Surveys between 1950 and 1955. In contrast, we focus mainly on the federal decennial Census, which is a much larger sample. Third, Rose only focuses on one particular region of the NWLB covering Arizona, California, and Nevada while we study the whole country.

In our related paper (Vickers and Ziebarth, 2024), we attempt to place an upper bound on the effects of the brackets on inequality by assuming that at the end of the war, everyone is constrained by the bracket. There are some important conceptual differences between that paper and this one. First, that paper's hypothetical was for everyone who initially earned less than the bracket to end up earning the level of the bracket and those who earned more to receive no further raises. This is different than the mechanical model here, since if the bracket is set high enough, individuals earning very little to begin with will not necessarily end up constrained by the bracket. Second, in the other paper, we are estimating the impact of a non-marginal change in the bracket. In effect, we are comparing a (hypothetical) distribution of earnings calculated based on the brackets set by the NWLB relative to a counterfactual in which there had been no NWLB at all and there were no changes in inequality relative to the pre-war levels. In this paper, we interpret our estimates as marginal effects of the bracket.

2 History of the NWLB and the Bracket Policy

The roots of the 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 simply carried over from wage policy in WWI. These points of continuity 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, policymakers during WWI made three critical actions 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 its charge to manage wages and salaries overlapped with other federal agencies such as the National Labor Relations Board and the Treasury's Salary Stabilization Unit. It also lacked a guiding policy on wages, even though, unlike the original Mediation Board, the NWLB could make a "final determination" on labor disputes.

The first labor dispute settled 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).

While, in many ways, the Aluminum Company, decision foreshadowed what would become the bracket policy we discuss below, 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 on how the NWLB would settle wage disputes 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/, again foreshadowing what would be the bracket policy. 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. The only workers exempt from the NWLB's jurisdiction were "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 wage adjustments. These regional offices had limited authority originally, but by early 1943, they gained a measure of autonomy from Washington (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 the "bracket policy" based on a subsequent May 12 directive authorizing 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.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.

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). That said, commentators around the time thought 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). For one, it had the power to punish unions that went on strike by revoking concessions. The NWLB could also request the War Department to

[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.

blacklist firms or cancel contracts. Additionally, wages paid in violation of NWLB orders could be disallowed as a tax deduction for the firm. As a last resort, the NWLB could refer non-compliant firms 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). Even with all these tools for ensuring compliance, it is hard to see how illegal wage increases which were mutually agreed upon would ever 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 price 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." Unfortunately, we do not have any systematic evidence on the extent of compliance with the wage controls.

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."[3]

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" (Derber, 1944).[4] 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

[3]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.

[4]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."

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 did have some moderating effect on the rate of wage inflation.

2.2 The Bracket Setting Process

Given the convoluted history of the NWLB, it should not be surprising that the process by which brackets were determined was not totally straightforward or formulaic. Instead, writing around the time, 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. Furthermore, the policy did not forbid all adjustments above the bracket, as "of course, increases justified on the basis of maladjustments, intra-plant inequities, substandards, or the 'rare and unusual' criteria could be permitted irrespective of brackets" (Record, 1944b). What can be said about the process is that the brackets were to be set as a function of the *current* wage distribution, not expectations about the distribution of wages. It also appears the brackets were not meant to be set as a function of other local conditions like, for example, the extent of the war effort. We find this somewhat hard to believe given the NWLB's overarching policy goal of supporting the war effort, so we will test later whether, in fact, the local war effort did not impact the brackets.

Even if the NWLB set brackets solely based on the current distribution of wages, the question is *which* distribution of wages was used. That is, how were workers grouped occupationally and geographically? As for occupations, the NWLB relied on categories from the 1939 *Dictionary of Occupational Titles* developed by the Bureau of Labor Statistics (BLS). These groups were sometimes subdivided into grades. While there was little discretion in how occupations were defined and little variance across regions, almost certainly there were arguments between employers and employees as well as the bureaucrats at the NWLB as to how to classify particular workers.

The more complicated question is that 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 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. We are not sure the industry classification scheme the NWLB used. Im addition, 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. There is also 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.

In addition to occupation and industry, geography played a major role in determining the bracket a worker was subject to. As mentioned earlier, the country was divided into 12 regions that were then divided into so-called zones, which was the geographic level at which the bracket varied. In general, a zone was supposed to correspond to a "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 a zone, 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 the geographic variation in the bracket, Figure 1 maps the brackets for stenographers.[5] 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 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.

To estimate the local distribution of earnings in order to set the level of the bracket, the 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). In this way, sampling uncertainty was one source of exogenous variation in the bindingness of the brackets. The classification of a particular worker into one of the standardized occupational 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 across 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 wage ranges for three "representative" firms.

3 A Mechanical Theory 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 cap (or floor like the minimum wage).

These differences between the bracket policy and a simple minimum (or maximum) wage create

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

differences in their 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 (assuming no disemployment effect), leaving everyone above the minimum wage unaffected. Ceteris paribus, this should reduce inequality.[6] This is not true in the case of a bracket policy. Instead, the effects depend on the level at which the bracket is set and 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 $\bar{w}$ assuming (1) perfect compliance,[7] (2) no (dis)employment effects from the brackets, and (3) no spillover effects. These are clearly extreme assumptions, but they help us to isolate some general features of how this kind of policy operates. Theoretically, the bracket can range from $\bar{w} = \infty$, which is 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, 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, which we believe is plausible in our setting of WWII. Then we can express the actual level of earnings tomorrow w_{t+1} taking into account the effect of the bracket as

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

What are the effects of the bracket in a world of *declining* latent inequality on the observed growth rate of changes in inequality measured by the change in the difference between the 10th and 90th percentiles? Figure 2 shows that there is a region of values where the marginal effect of the bracket is zero,[8] another where it is positive, and another where it is negative. For values of the bracket less than the 10th percentile, there is no effect on the 10th or 90th percentiles themselves and consequently no effect on inequality. For the values greater than 10th but still less than the 90th percentile, the 90th percentile remains fixed while the 10th percentile is allowed to

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

[7] We could also assume that there is some level of noncompliance, but it is random.

[8] 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.

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, in fact, lower relative to the case of a very low level of the bracket. To understand how inequality could be lower when the bracket is set very high, 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 equal to 0.

We overlay changes in inequality at the top and bottom of the income distribution to highlight the range of values for which the marginal effects of the bracket have different signs for inequality and the bottom and top. In this range, a marginal increase in the bracket allows the 50th percentile to increase while continuing to hold fixed the 90th percentile and already having allowed the 10th percentile to increase as far as possible. As a consequence, the marginal effect on inequality at the bottom is equal in magnitude to the marginal effect on inequality at the top. What happens if we change the distribution of latent income? As long as inequality in latent income is declining, the same basic picture will apply. It will still be the case that the marginal effects will only take on values 0, -1, and 1.[9] The only qualitative difference will be on the range of values of the bracket for which the marginal effect on inequality is not 0.

This is obviously a very "mechanical" model of how the bracket system worked, but it still illustrates three important points. First, even in this relatively simple world, there is no clear prediction for the sign of the marginal effect of the bracket on measures of inequality. The marginal effect will depend on where in the distribution of earnings the bracket is located. Second, the marginal effect of the bracket for inequality at the top can have a different sign than the marginal effect on inequality at the bottom of the income distribution (at least in some range of values for the bracket). In addition, this model provides a useful point of comparison for the actual effects. For example, the model shows that the effects of the bracket on inequality at the top and bottom of the wage distribution can never have the same sign. If the effects do have the same sign, then this is evidence of spillover effects of the brackets. Finally, the model highlights how the marginal effect estimated at a particular value of the bracket can have a different sign than the overall effect comparing the actual to a counterfactual world with no NWLB at all.

[9]The values of -1 and 1 are due to the assumption of perfect compliance. If compliance was less than perfect (but still random), then the marginal effects would be less than 1 in absolute value.

4 Data

4.1 NWLB Records

We collect primary source records of the brackets set by the NWLB at the occupation-industry-geography level. 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. We supplement these records with the "Manual of Going Rates", which published brackets up to March 15, 1944.[10] Note that the archival records, or "bracket summaries", are more comprehensive when available. In particular, for all brackets with a date of enactment listed, 49% went into effect after March 15, 1944 meaning we would not them if we relied solely on the manuals. The bracket summaries list only the value of the bracket at the end of the war with no information about whether brackets were modified before then. We assume that the brackets remain unchanged between March 1915, 1944 for the regions where we use the manuals to collect the brackets (Region XII in the Pacific Northwest and Region VI centered in Chicago),

4.2 Census of Population

Our outcome and control variables come from the long-form of the federal Population Censuses taken in 1940, 1950, 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 is then asked to fill out the long form, which asks a richer set of economic and demographic questions. 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, which is critical for linking these records to the brackets, and weeks worked, which is critical 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. Second, the labor income variable is top coded to preserve anonymity.[11] These top-coded observations are not a major problem 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.

[10] We thank Andrew Bossie for alerting us to the existence of this source.

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

There are additional problems with the 1950 Population Census. In the last year, IPUMS released a 100% sample from this census. However, there are problems with the income variable in this sample. IPMUS warned that "estimate[s of] the current total and wage income values exactly match the intent of the form roughly 70-75% of the time. The magnitude of the differences ranges widely. Wages are underestimated twice as frequently as they are overestimated [...] Researchers should be cautious when using these variables and should screen for outliers." In a related paper (Vickers and Ziebarth, 2024), we show that these underestimates are a particular problem for higher-earning occupations. Moreover, we show that the standard deviation of wages (by occupation) in the 100% is underestimated relative to the 1%. For these reasons, we are doubtful about the reliability of this sample and have chosen not to use it.

Because of the problems with the 100% IPUMS sample, we instead use the 1% IPUMS 1950 sample, which has reliable information on income. The problem with the 1% sample is not just the obvious problem of its size but also that geographic information is restricted since it was created before the 1950 Census was completely released to the public in 2024. As a consequence, county of residence is reported for only about 20% of the observations in the original 1% sample of 78K observations. Results based on this (small) sample should be taken, at best, as suggestive evidence for the effects in the short-run.

4.3 BLS Occupational Wage Surveys

To explore the effects of the brackets in the first half of the 1950s, we use wage information from the Occupational Wage Surveys collected by the BLS starting in 1952 and covering only the largest cities. Rose (2009) used the same data source for California, Arizona, and Nevada in his study of the effects of the NWLB.[12] One nice feature of the data relative to the Census is that the occupational categories were exactly the same as those in the NWLB brackets. The records also provide a breakdown of the wage distribution within an occupation by manufacturing and non-manufacturing industries. We have transcribed these records for our occupations of interest in all the cities surveyed, which totals about 45, and then match the cities to the brackets.

There are some drawbacks to this source besides the limited geographic coverage. First, the survey does not report individual-level data, only the number of workers in wage bins by occupation. This makes it difficult to uniquely identify the percentiles of the wage distribution. At best, we know that a particular percentile falls in a certain bin. We consider three different assumptions for how to deal with this nonuniqueness: (1) everyone in a bin earns the lowest value in that bin; (2) everyone earns the highest value in the bin; and (3) everyone earns the average of the highest and lowest values. There is an additional problem if the percentile of interest falls in the top wage bin because this bin does not have an upper bound. In situations like these, we use the fact that

[12] It is not obvious how Rose handles the problem that the percentiles of the wage distribution will not be unique since wages are only reported in given ranges.

the survey also reports the average wage by occupation, which allows us to bound the upper limit of the top wage bin and, hence, on the top percentile. In particular, let e_j denote employment in wage bin $j = 1, \ldots, J$ with associated wage lower bound w_j^{LB}. Also let $e = \sum_{j=1}^{J} e_J$ denote total employment and $\bar{w}$ the overall average wage. Then, in cases where the top percentile falls in wage bin J, we set the upper limit of this wage bin to $w_J^{UB} = (e\bar{w} - \sum_{j=1}^{J-1} e_j w_j^{LB})/e_J$.

4.4 Sample Construction

In constructing our working sample, we apply the sample restrictions imposed by Goldin and Margo (1992): 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 workforce 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.[13] Unlike the sample restrictions we impose on the Census data, BLS wage surveys were meant to cover all the workers. Unfortunately, we are not able to impose the same restrictions since the BLS data only provides counts by wage bin and no details about the demographics of workers.

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 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. 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 many cases 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

[13] 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.

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.[14]

The final issue in mapping the NWLB records to the Census and the BLS is that even when we can make an exact occupational match between the brackets and these other data sources, in some cases, occupations were further subdivided into, for example, grades ("A", "B", and so forth). In these cases, for consistency, we have always selected the "A" grade. Similarly, brackets could be assigned for "senior" and "junior" members of an occupation; we used the senior brackets. We also use the most general occupational category from the brackets available. For example, for clerks (`OCC1950` 390), we use "general" clerks rather than file or payroll clerks.[15]

5 Empirical Strategy

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* the growth rate of 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.

We estimate census by census the following specification:[16]

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

Because an "observation" is a statistic derived from a group of individuals in a given occupation-

[14] In some cases, 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.

[15] See Vickers and Ziebarth (2024) for the complete list of these occupations.

[16] In the appendix, we discuss a different identification strategy related to the one in Chodorow-Reich et al. (2019). The idea is to exploit the sampling error in the wage data used by the NWLB and the formulaic way the brackets were determined. Unfortunately, this strategy is infeasible because we do not observe the wage distribution on the eve of the brackets being set. We go on to discuss related but feasible strategies and why in the end, we do not use them.

county cell, we, like Autor et al. (2016) 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]

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 changes in the overall or unconditional earnings distribution and the Great Compression. Even with this complication, the fact that a number of previous papers have used a specification like this makes it 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.

Motivated by the specification in Lee (1999), both Autor et al. in their study of the effects of the minimum wage on inequality and Rose (2009) in his of the NWLB use as the treatment the minimum wage or bracket scaled by the median of the income distribution at the time. Their motivations for using this definition of the treatment is that it better captures how many workers were at risk of being affected by the minimum wage in the case of Autor et al. or the bracket in the case of Rose than the unscaled version of the variable. The problem in our view with this approach is if the bracket was set strictly as a function of the mean, then a cell with more dispersion would have also smaller median relative to the mean and, hence, the bracket would appear affect more workers, but those cells with higher initial levels of inequality might have experienced more compression absent the effects of the bracket.[18] Even without this concern, we could not use this definition of the treatment since we do not observe the median at the time when the bracket is set.

Assuming that the bracket is monotone increasing in the distribution of earnings, after con-

[17] An additional question is what to do about small occupation-county cells for which percentiles might not be unique. We consider two approaches if a percentile is non-unique: (1) 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.

[18] To us, if the treatment is meant to capture how many workers are potentially affected, then the treatment should be defined as the percentile of the minimum wage or bracket in the distribution of earnings.

trolling for occupation and county fixed effects, our estimation strategy, in effect, compares the changes in inequality between groups of workers with relatively high average earnings (and, hence, a relatively high bracket) and those with relatively low average earnings. Put this way, this strategy seems unlikely to recover the causal effect of the bracket. A major concern is that average earnings is correlated with inequality, and groups with initially high levels of inequality would have experienced larger declines in inequality absent the bracket. For this reason, it is essential that we include the 1939 level of inequality $\log\left(\frac{Q_\tau(w_{ic1939})}{Q_{\tau'}(w_{ic1939})}\right)$ (and its square) as additional controls.[19] Now after controlling for the initial level of inequality, we are still identifying the effect of the brackets by using variation in the average earnings across groups of workers, but now we are comparing a high earning group to a low earning group that have the same level of inequality. We see no strong reason why after controlling for the initial level, inequality should fall faster or slower in the higher earning group absent the bracket.

5.1 Quantitative Evidence on the Bracket Setting Process

Our identification strategy hangs in part on the brackets being determined solely by the preexisting wage distribution. Now we have already provided narrative evidence that this was the case. Our goal here is to provide quantitative evidence for this claim by showing that, first, in fact the local earnings distribution was strongly predictive of the bracket and, second, the residual variation in the brackets after controlling for the earnings distribution is not correlated with other local economic characteristics such as war spending that might also be correlated with subsequent changes in inequality.

To start, we regress the brackets on various statistics at the county-by-occupation level of the 1939 hourly earnings distribution adjusted for growth in earnings of production workers between 1939 and 1943 (Officer and Williamson, 2022). Ideally, we would have used the same distribution of earnings used by the NWLB in setting the brackets, but as mentioned earlier, we do not have this information.[20] To limit the influence of outliers, we winsorize the 5% tails of the hourly earnings distribution. To isolate the predictive power of the earnings distribution, we do not include any other controls such as county or occupation fixed effects in these regressions.

Columns 1 through 4 in Table 1 show that there are statistically and economically significant positive relationships between the bracket and the 10th, 25th, and 50th percentiles as well as the mean. In Column 5, after including all of these statistics, we can explain about 20% of the variation in the brackets, which supports the idea that earnings distribution did determine to a certain extent

[19] Rose includes contemporaneous levels of inequality in his specification but does not include occupation fixed effects. He argues for including the prior measures of inequality because "the brackets have more opportunity to be low relative to the median if the occupation has a high degree of prior dispersion."

[20] In our related paper (Vickers and Ziebarth, 2024), we draw on (limited) contemporaneous wage data and show that the brackets were in fact set quite closely to what would have been predicted based on the wage distribution at the time. We emphasize that the data there are for a select set of occupations in the South so we are hesitant to overgeneralize the result.

the values of the brackets. However, this still leaves 80% of the variation unexplained. The fact that we only have a noisy signal of what the NWLB used to set the brackets works against our claim the brackets were set based on the distribution of earnings.

Our biggest worry is that this residual variation in the bracket is correlated with non-earnings related factors that might themselves be correlated with future changes in inequality. To examine this possibility, we first examine in column 6 whether demographic characteristics including fraction married, age, fraction white, and fraction with a high school degree predict the bracket. Only the fraction white is still statistically significant with the bracket. This could be interpreted as evidence of racial bias in the bracket setting process. We prefer to interpret the fraction white as an additional proxy for the earnings distribution used by the NWLB.[21] The last column of Table 1 includes proxies for the local war effort including military spending on production contracts as well as investment in facilities originally used by Aizer et al. as well as state-level mobilization rates (Acemoglu et al., 2004). We also control for state-level unionization rates (Farber et al., 2021) as another potential non-earnings related determinants of the brackets. We note that all of these additional variables only vary by geography and not by occupation (or industry).

We find that combat supply contracts and spending on industrial facilities are statistically significantly correlated with the bracket even after controlling for the wage distribution and demographic characteristics. The positive association with combat supply contracts is consistent with the NWLB ensuring that areas heavily involved in the war effort would be able to set higher wages to attract workers though the actual economic significance of this association is very small. Moreover, the state-level induction rate is positively correlated with the bracket potentially reflecting the need for higher wages in areas of relatively low labor supply. On the other hand, the association with spending on industrial facilities as well as the unionization rate are economically insignificant. Adding these war-related variables barely moves the R^2 and, in the end, the ratio of the residual variation relative to the variation in the bracket is just under 90%. In the end, we conclude that the main central driver of the brackets is the earnings distribution and the local war effort is not that of an important predictor of the bracket.

While not critical for identification, exactly where in the distribution the brackets tended to be set has important implications for the predicted marginal effects of the bracket, and where they were set depends on the method for determining the bracket as a function of the local earnings distribution. The NWLB itself conducted an analysis of this question for Region III (in Philadelphia) and found that administrators tended to use the first 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 [below the mean] 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

[21] This interpretation does the raise the question then of why fraction with a high school degree is not also correlated with the bracket since the fraction educated should also be a good proxy for the earnings distribution.

or lower under one or the other method. Rose quotes one memo from NWLB Region X that stated: "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 fact that the first cluster method dominated seems consistent with Rose's finding in, for a sample of occupations from Region X, that the brackets were usually between the zeroth and twentieth percentile of the hourly earnings distribution at the time, which would have fixed nearly all wages in place.

We now provide quantitative evidence on where the brackets were set for the occupations in our sample. Fig. 3 plots for white collar and metal trades occupations in the Regions I (the Northeast) and IV (the South) the distribution of the percentile of the brackets in the 1939 distribution of hourly earnings[22] adjusted for production worker earnings growth between 1939 and 1943. For white collar workers in both regions, we find like Rose that the brackets ended up between the 0th and 20th percentiles of the earnings distribution. For metal trades workers, the picture is different. In fact, for Region IV, nearly all the brackets were higher than the 20th percentile of the earnings distribution. One potential explanation for the difference in where the brackets are located between the two groups of occupation is that we are assuming too high of a growth rate in earnings between 1939 and 1943 for white collar occupations when using the growth rate of earnings for production workers. To us, there are no clear geographic patterns in the location of the brackets. Brackets for metal trades occupations tended to be higher in the South than the in the Northeast and the opposite for white collar occupations. Again a possible explanation for any regional differences that there are is the fact that the growth rate adjustment we use applies equally across space when, in fact, almost certainly wages did not grow uniformly across the country.

A different approach to addressing the question of where the brackets were set is to calculate the percentiles of the 1939 earnings distribution the brackets would fall assuming all brackets were set based on the 90% rule. The benefit of this approach is that we know exactly what the distribution of wages would have been by assumption. The drawback is that we assume the 90% rule was followed religiously when determining the bracket. Fig. 4, which pools all regions, shows that in this hypothetical, brackets would never have been set below the 35th percentile and, in some cases, would have been greater then the median. The fact that the percentiles of the brackets for white collar occupations tend to be higher than for those in metal trades in this case whereas they were reversed in Fig. 3 provides further suggestive evidence that the assumption of uniform growth in earnings across occupations is not totally accurate. Overall, we take these results as evidence that brackets were set higher in the distribution than suggested by Rose's evidence, which, while based on better information on the earnings distribution that the NWLB would have used to set the bracket, only covered a small set of occupations located in a single state.

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

Our final approach to addressing this question is to examine what effect the brackets had the on the median of earnings. If the brackets were set below the median, then a marginal increase in the bracket should have no effect on the median since earnings for the median worker would still be constrained by the bracket. Figure 5 shows the estimated effects of the bracket on the growth in median income by year and specification. We find evidence that when including occupation and county fixed effects, the marginal effect of the bracket was to increase the growth rate of the median through 1970. The effects in 1950 and 1953 are not statistically significant but of a similar magnitude to the effect in 1960. Keep in mind that the results for 1950 and 1953 are based on small samples. In the case of the 1950, it is the 1% IPUMS sample restricted to cases that have a county listed and for 1953, it is a sample of only 43 cities. There are also strong positive effects when we include no fixed effects or occupation fixed effects only though as we mentioned earlier, we do not view these specifications as compelling. Not only do these results suggest that brackets tended to be set greater than the median, they also suggest that the brackets were not set "too high" in the distribution. In that case, there would also be no effect from a marginal increase in the bracket.

6 Results

6.1 Effects on $\Delta \log(Q_{10}/Q_{90})$

We inferred from the effects on the median that the brackets must have been set greater than the median but not that much greater (otherwise there would have been no marginal effect). The mechanical theory then implies that the marginal effect of the bracket should be to reduce the gap between the median and the top of the income distribution, say, the 90th percentile since those at the top would be less likely to receive a raise relative to the those at the 50th percentile. At the same time, the marginal effect of the bracket should be to *widen* the gap between the 10th and 50th percentiles since wages at the 10th percentile are presumably unaffected by a slight increase in the bracket. As a consequence, the marginal effect of the bracket on the gap between the 10th and 90th percentiles should be small to nil.

Figure 6 plots the effects of the bracket 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. Consistent with the mechanical theory and the effects for the median, the marginal effect of the bracket on $\Delta \log(Q_{50}/Q_{90})$ was positive and statistically significant all the way through 1980. On the flip side, the marginal effect on $\Delta \log(Q_{10}/Q_{50})$ over this same period was negative though the effect is not statistically significant in 1960. Putting these two sets of results together, the marginal effect on overall inequality as measured by $\Delta \log(Q_{10}/Q_{90})$ was muted and only statistically significant from 0 in 1960 because of the small in magnitude effect on the $\Delta \log(Q_{10}/Q_{50})$. To interpret the magnitude, 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. We note that the effects on $\Delta \log(Q_{50}/Q_{90})$ and $\Delta \log(Q_{10}/Q_{50})$ do not have to add up exactly to the effect on $\Delta \log(Q_{10}/Q_{90})$ because we estimate the effects using separate regressions. The fact that the effects on the various inequality measure die out over time gives us confidence that our specification is not simply capturing an artifact in the relationship between the bracket and earnings. Nevertheless, it is striking that more than 3 decades after the end of the war and the NWLB, there are still detectable effects of the wage controls on inequality at the top and the bottom of the wage distribution.

We also include the point estimates for 1950 and 1953 using the 1% 1950 Population Census sample and the BLS occupational wage survey. We emphasize that the sample sizes in these years are small with large standard errors that we do not report the results. For example, for the 90-10 inequality measure, we need at least 10 observations to have any chance of calculating inequality uniquely. After taking into account the missing brackets problem, we end up with less than 50 occupation-county cells in the 1950 1% sample that can be matched to the brackets and have at least 10 observations. We find effects on the $\log(Q_{10}/Q_{90})$ similar to those in 1960, but the effects on $\log(Q_{10}/Q_{50})$ and $\log(Q_{50}/Q_{90})$ are flipped. Interestingly, these results relative to the results for later years are more consistent with what Rose found suggesting the brackets led to faster inequality declines at the bottom versus the top of the income distribution.

A natural question then is what these results mean for understanding the Great Compression. First it is important to keep in mind that much of the decline in inequality in the *unconditional* earnings distribution during this period was due to declines in the *between* components of inequality such as between high and low educated workers. Our 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 decline in the $\log(Q_{10}/Q_{90})$ was due to declines in the $\log(Q_{50}/Q_{90})$.

Our results potentially provide a partial explanation for the changes in inequality at the top of the income distribution, but the NWLB is not a one size fits all explanation for the Great Compression. For one, as we just highlighted, the marginal effect of the bracket was to increase inequality between the median and the 10th percentile while having no marginal effect (at least after 1960) on $\Delta \log(Q_{10}/Q_{90})$. However, these null marginal effects do not imply there was no overall effect of the NWLB. In fact, the mechanical theory implies that when the marginal effect on $\Delta \log(Q_{10}/Q_{90})$ is 0 due to offsetting effects on $\Delta \log(Q_{10}/Q_{50})$ and $\Delta \log(Q_{50}/Q_{90})$, the level of $\Delta \log(Q_{10}/Q_{90})$ is at its highest for any choice of the bracket including in the case when the bracket is set at the 100th percentile of the earnings distribution, which is a world with effectively no wage controls and no NWLB. Therefore, taking the mechanical theory seriously, we conclude that, in fact, the NWLB *did* reduce inequality between the top and the bottom potentially all the way through 1980 relative to a counterfactual world without wage controls though we are not able

to answer the question of how much higher inequality would have been without the NWLB.[23]

To be sure, the mechanical theory does not exactly fit the results, quantitatively. For 1960, the effect on the upper tail inequality is larger in magnitude than the effect on the lower tail inequality when the mechanical theory says these two should be the same with the consequence that the effect on inequality between the top and the bottom is 0. What has to be the case for 1960 is that not only is a marginal increase in the bracket allowing the earnings of the median worker to rise, but that increase is, in some sense, coming at the cost of earnings at the 90th percentile. In other words, there must be spillover effects of the brackets on those earning substantially more than the bracket. In the case of the minimum wage, these spillovers are thought to take the form of wage hikes for those earning initially more than the minimum. In this case, it means wage *cuts* for those earning more than the bracket. For the years besides 1960, consistent with the mechanical theory, we cannot reject the null that the effects on the top and bottom of the bracket are equal in magnitude so we do not view the evidence for spillovers as overwhelming.

Another quantitative failure of the mechanical theory is that if a marginal effect is non-zero, then it should be equal to 1 in magnitude, which is clearly not the case empirically. The reason theoretically for the magnitude of the marginal effect was that we assumed latent wage growth is positive so a marginal increase in the bracket will allow the marginal worker's wage to increase by that amount. This explanation also suggests why the effects could be smaller than one in magnitude if latent wage growth was not positive uniformly. In this case, easing the bracket would not necessarily increase the wage of the marginal worker and the marginal effect of the bracket on inequality would be muted. There are other reasons why the magnitude of the effect might be less than 1. For example, as we mentioned, the possible values that the marginal effect could take on under the mechanical theory are -1, 0, and 1 depending on the value of the bracket so it might be that the average marginal effect we estimate is averaging marginal effects that are not necessarily all equal to 1 or -1. In addition, there could be (and almost certainly is) measurement error in the matching of occupational titles listed in the brackets to occupational categories in the census. There could be similar problems with geographical matching. For all of these reasons, we do not view the quantitative failure of the mechanical theory as a major rejection of the theory overall.

Another way to see the failure of the purely mechanical theory is by noting that the effect on the median is not equal to in magnitude the effect on the $\Delta \log(Q_{50}/Q_{90})$ and $\Delta \log(Q_{10}/Q_{50})$. (In 1960, the discrepancy is not large.) This means that both the 10th and 90th percentile were being affected by changes in the bracket. In general, the marginal effect on the $\Delta \log(Q_{50}/Q_{90})$ was larger than that of the median so there must have been spillover effects of the bracket. These spillovers would be the opposite of the much debated spillovers of the minimum wage where workers earnings more than the minimum wage also get a raise reducing the effects of the minimum wage on inequality. In our case, the apparent spillovers were negative with those at the top suffering slower earnings growth than in a world without the NWLB. At the bottom of the distribution, the

[23] We are also cautious here because we are studying a limited set of occupations.

effect of the spillovers was to increase the growth rate of the Q_{10}. Here those at the bottom are getting at raise because those at the middle are rather than in the minimum wage case where it's those at the middle getting a raise because those at the bottom are.

Besides the NWLB, which focused on the "meat" of the earnings distribution, President Roosevelt also introduced a cap on salaries and a limit on salary increases on Oct. 15, 1942 for the very top earners. The cap 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) find 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 recovered to its pre-war level by the 1950s. In conclusion, Frydman and Molloy (2012) view the effects of the this war pay polices on differences in earnings between executives and other workers as modest and not long lasting. Their null results contrast with our results that the NWLB had persistent effects on inequality at the top. We argue below that some of the persistence of the NWLB's effects was due to unions that used the brackets of the NWLB as a starting point for wage negotiations with employers. While unions played an important role in keeping CEO compensation at a relatively low overall through the 1970s, apparently, the limits of $25,000 and $5,000 set by the president never came to serve as a reference point like the brackets did for wage earners.

6.2 Effects on the Black-White Wage Gap

We now examine effects of the brackets on a different dimension of inequality: 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. The value of the bracket was not race specific.[24] In theory, the uniformity of the brackets (and assuming they were enforced uniformly by race) should have reduced racial inequality within an occupation (and county). The trickier question is what the impact will be on the overall wage gap when Blacks and whites tended to work in different occupations (and places). Now even if whites and Blacks worked in completely different occupations, it is still possible for the brackets to reduce the racial gap to the extent that predominantly Black occupations had higher levels of inequality before the NWLB. In this case, even if the bracket was

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

set strictly as a function of mean earnings in an occupation, more Blacks would get a raise than whites closing the gap in average earnings between the two groups. In a similar way, the effects of the bracket will depend on the skewness of the earnings distribution holding fixed the variance. We explore this possibility in our companion paper (Vickers and Ziebarth, 2024).[25]

Figures 7 shows the marginal effect of the bracket on minus the racial earnings gap.[26] Like in our previous specifications, a positive coefficient implies that a higher value of the bracket *reduces* the within-occupation-county component of racial inequality. In the specification with no fixed effects or only occupation fixed effects, there is evidence that the marginal effect of the bracket was to speed up racial convergence in average earnings. These effects are (in our view, suspiciously) persistent with the effect including occupational fixed effects still statistically significant in 2000. If we took the estimates with occupation fixed effects at face value, the brackets would be a potentially powerful explanation for the post-Civil Rights era convergence in racial differences.

The problem is that in our preferred specification including both occupation and county fixed effects, we see little to no evidence that the brackets had an effect on the racial gap in any of the postwar years. This makes us hesitant to conclude that the NWLB had any effects on this dimension of inequality. This null effect could be due to a non-uniform implementation of the racially uniform brackets. Whether that meant Blacks not receiving raises because whites couldn't due to the brackets or whites also receiving raises in violation of the brackets because Blacks were, we cannot say.

7 Mechanisms of Persistence

Across a variety of specifications, it is clear that the brackets continued to affect the earnings distribution decades after the end of the war.[27] Interestingly, even before the war concluded, leaders of the NWLB suggested that wage controls were likely to have enduring effects. George Taylor, the vice chairman of the NWLB, said 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). The chief of the Research and Statistics Branch of the NWLB, John T. Dunlop, who would go on to become a very prominent

[25] It is also possible that the brackets affected overall racial inequality by affecting the sorting of races across occupations, for example. This mechanism would also not be captured by these specifications.

[26] We cannot run these specifications in 1950 because of sample size issues and in 1953 because the BLS occupational wage survey does not provide a racial breakdown of employees.

[27] 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). 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).

scholar of industrial relations, emphasized throughout his scholarly career the centrality of occupational hierarchies and internal labor markets in the process of wage determination. This view was built on Dunlop's experience working for the NWLB.

But how could these brackets have such persistent effects? At least in first decade and a half after the war, the persistence can be attributed to the fact that the NWLB was actually back in effect within 5 years of its dissolution as the US entered the Korean War in 1950. Just as in WWII, the federal government setup an agency to regulate wages known as the Korean War Wage Stabilization Board. It 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). While we do not have direct evidence on effects of the the Korean War version of the NWLB, it is plausible that it sustained the effects of the WWII NWLB on inequality through, at least, the end of the Korean War in 1953. As suggestive 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.

The Korean War marked the end of widespread wage fixing by the federal government, but not the effects of the brackets. Following Dunlop, we argue that an important channel through which the brackets affected wages for decades was the way in which the preexisting wage distribution acts as a reference point in wage negotiations, particularly in bargaining between businesses and unions. As evidence, 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 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.

We take these anecdotes as evidence that unions were generally satisfied with the wage structure imposed by the NWLB and worked to keep that structure in place.

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.[28] 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.

In addition to its effects on the 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 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.

8 Conclusion

High levels of economic inequality in the US and other western countries continue to be a concern for policy makers, and for some, the postwar period with a relatively low level of inequality represents something of a golden age of shared economic prosperity. We show that to some degree, the postwar earnings distribution was shaped directly by wartime wage controls. In fact, 35 years after the end of war, we can still detect the effects of the brackets on inequality and the top and bottom of the earnings distribution.

[28] 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.

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 resurrected by the federal government during the Korean War in the 1950s. Later in the 1960s and 1970s, we have suggested that the wage distribution determined while the brackets were still in effect functioned as a 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?

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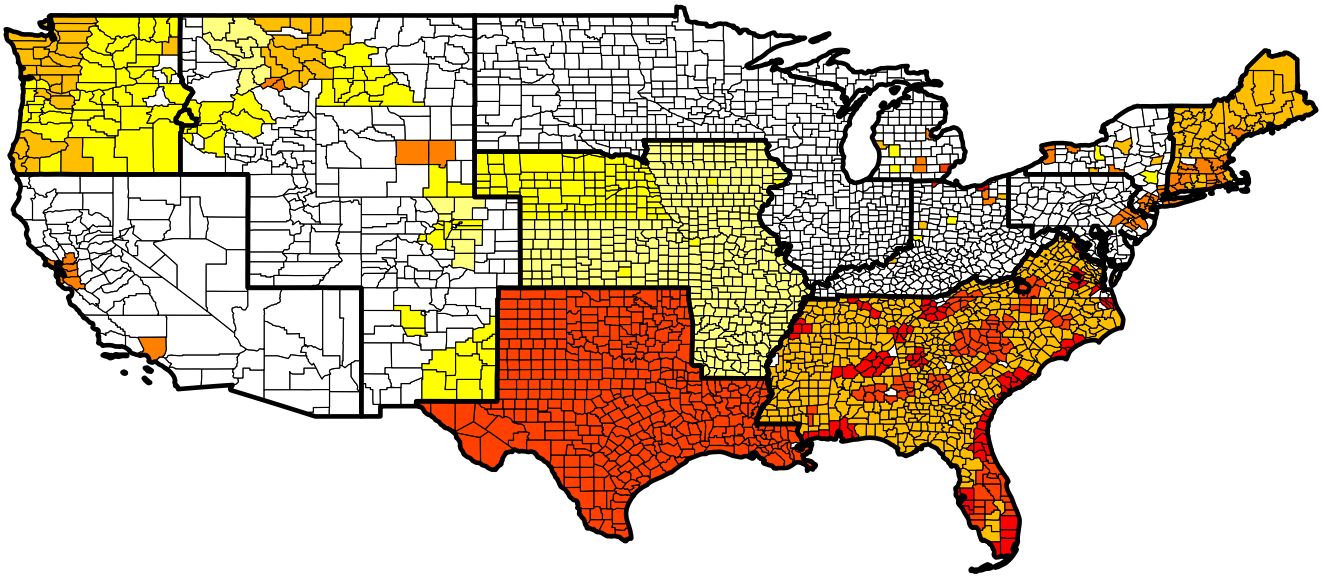
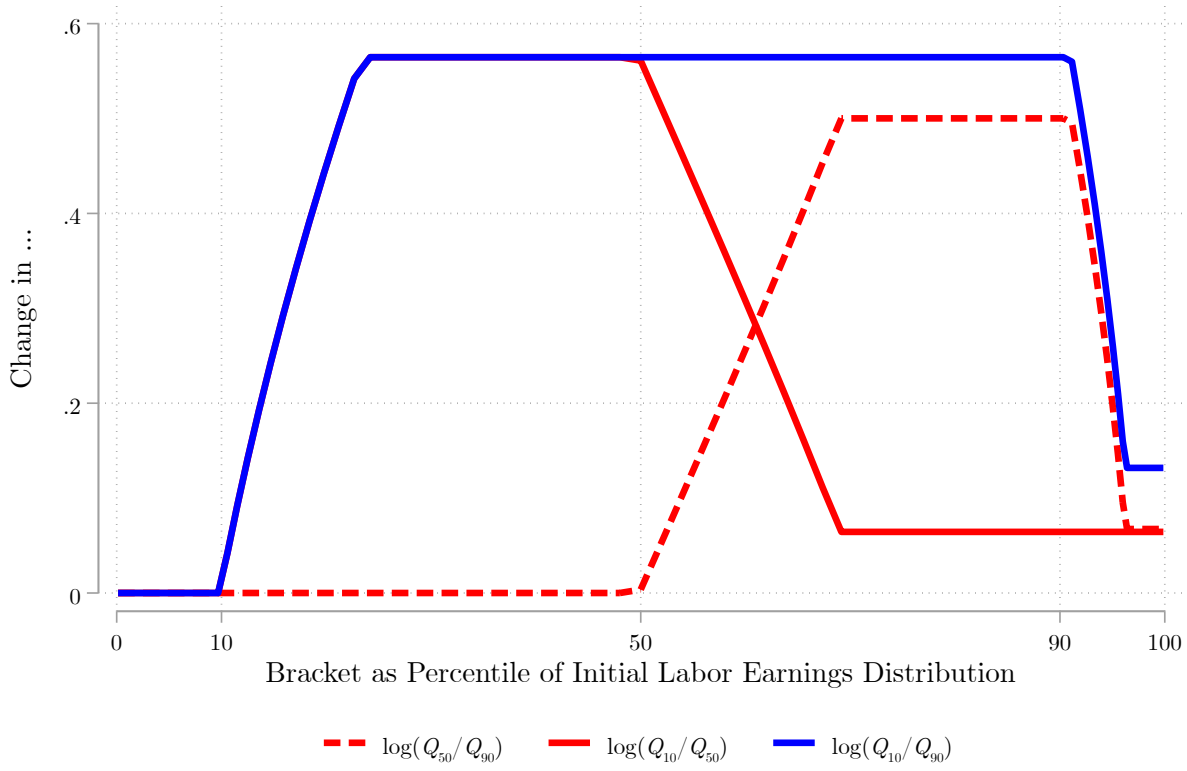
Figure 1: Brackets for Stenographers



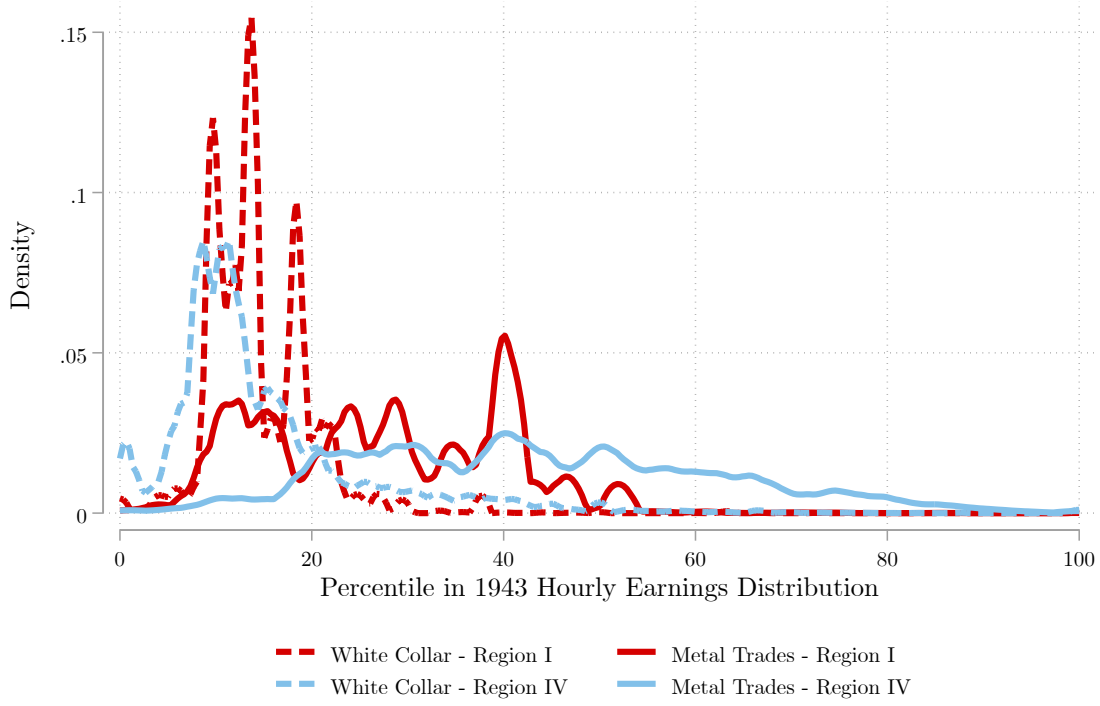
Notes: Darker values represent higher values for the bracket. Region I, which covers the northeast, defines brackets at the town-level. So a shaded county in that region means that the county includes a city with a bracket assigned. For the other regions, brackets are assigned at the county-level. The 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: Hypothetical Effects of the Bracket on Labor Earnings Inequality



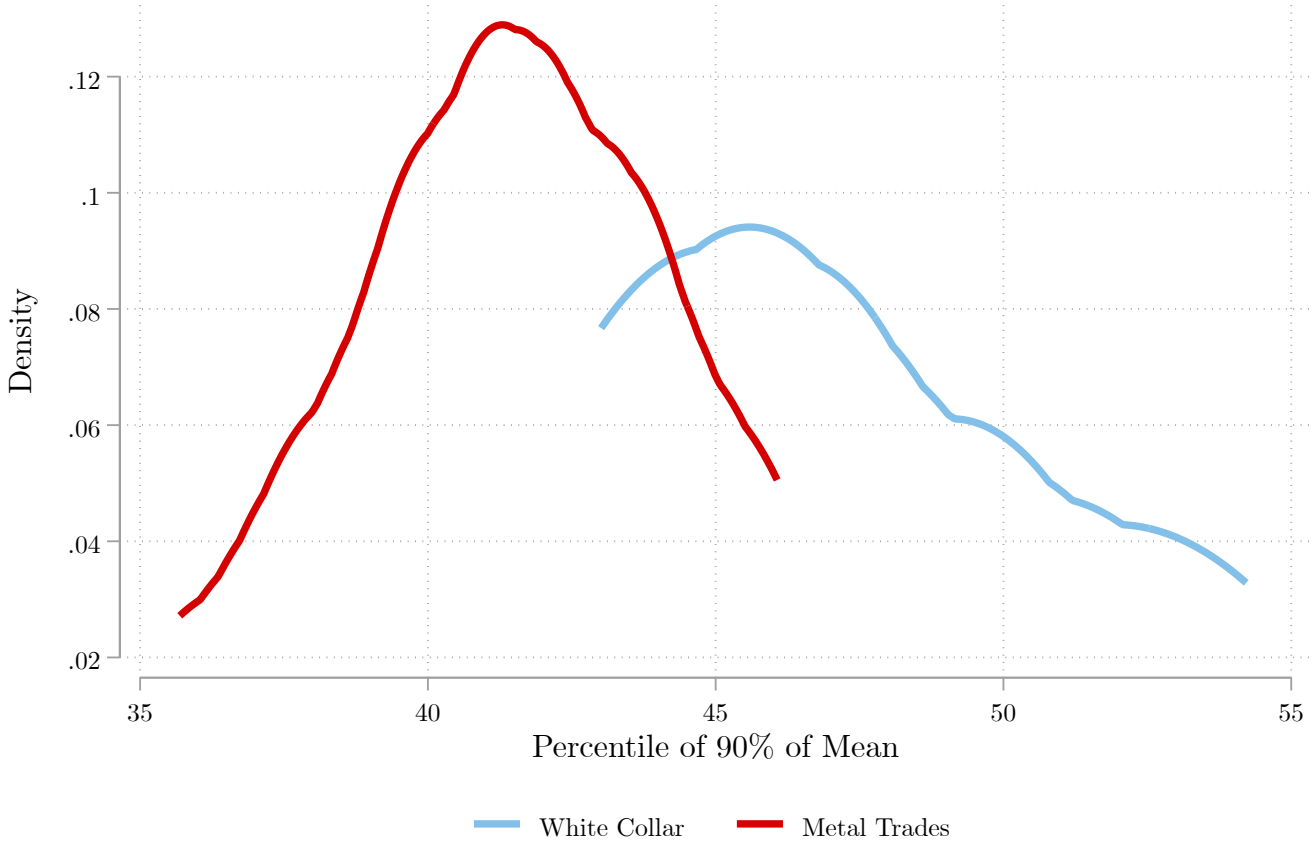
Notes: We plot minus the change in inequality so that an increase in this value reflects a more equal distribution of earnings. We assume that latent labor earnings is increasing in the current level of labor earnings and that inequality in latent labor earnings is lower than the current level.

Figure 3: Distribution of Brackets in the Hypothetical 1943 Hourly Earnings



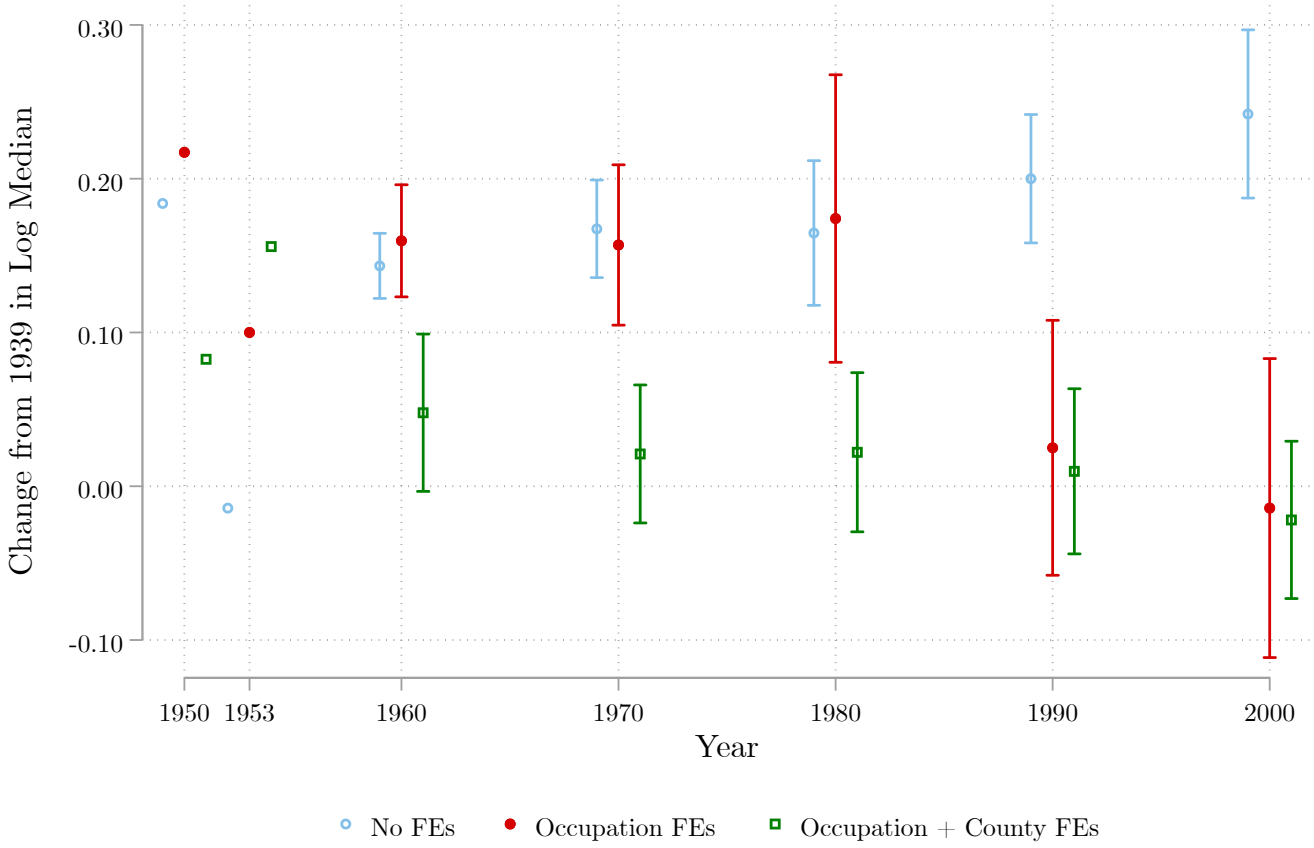
Notes: The percentile is calculated conditional on occupation and county. The 1943 hourly earnings variable is calculated using reported labor income and weeks worked in 1939 assuming a 40-hour workweek. Earnings are adjusted for growth in production worker compensation between 1940 and 1943.

Figure 4: Distribution of Brackets Applying the 90% Rule in the 1939 Hourly Earnings



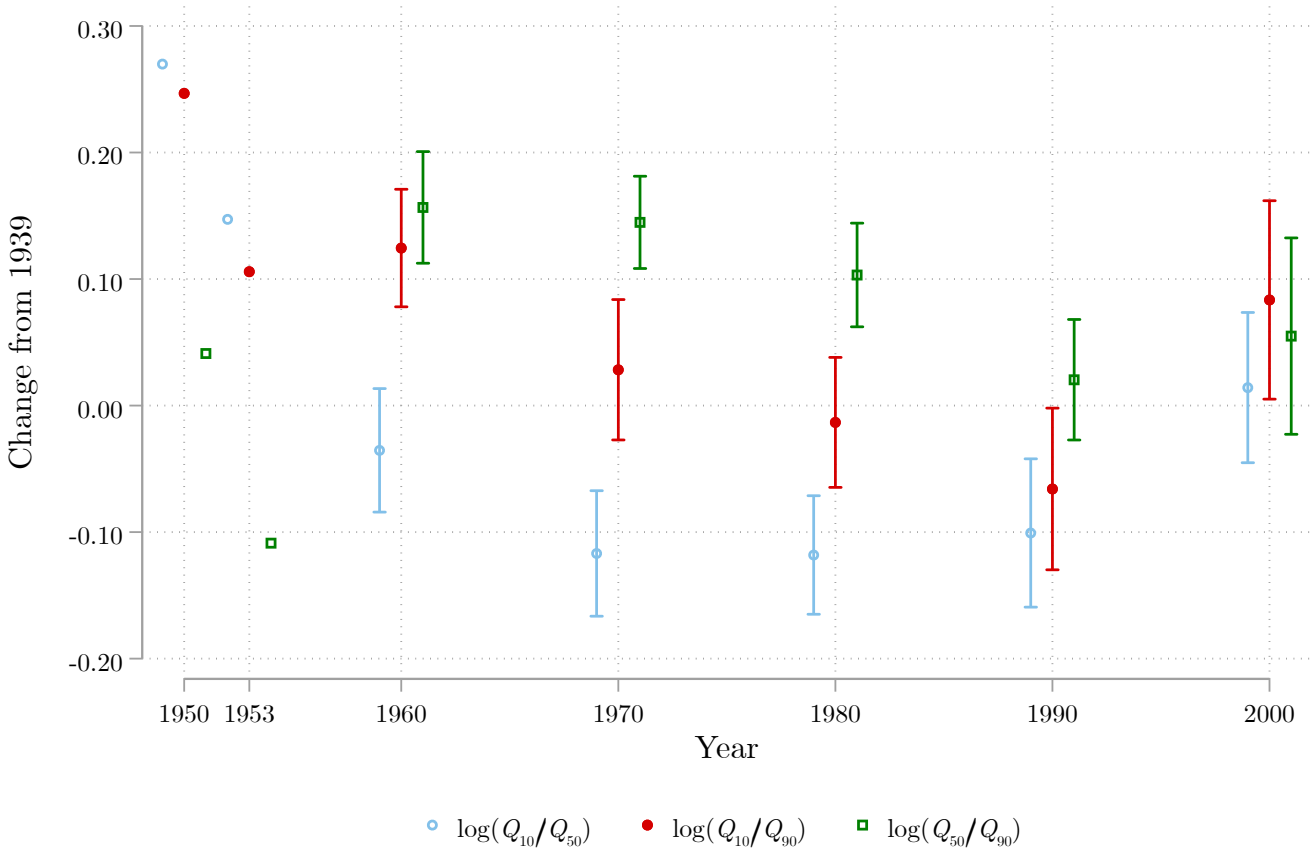
Notes: We report the percentile in the 1939 hourly earnings distribution of the brackets assuming is equal to 90% of the mean. The percentile is calculated conditional on occupation and county. The hourly earnings variable is calculated using reported labor income and weeks worked in 1939 assuming a 40-hour workweek.

Figure 5: Effects of the Brackets on the Median Earnings



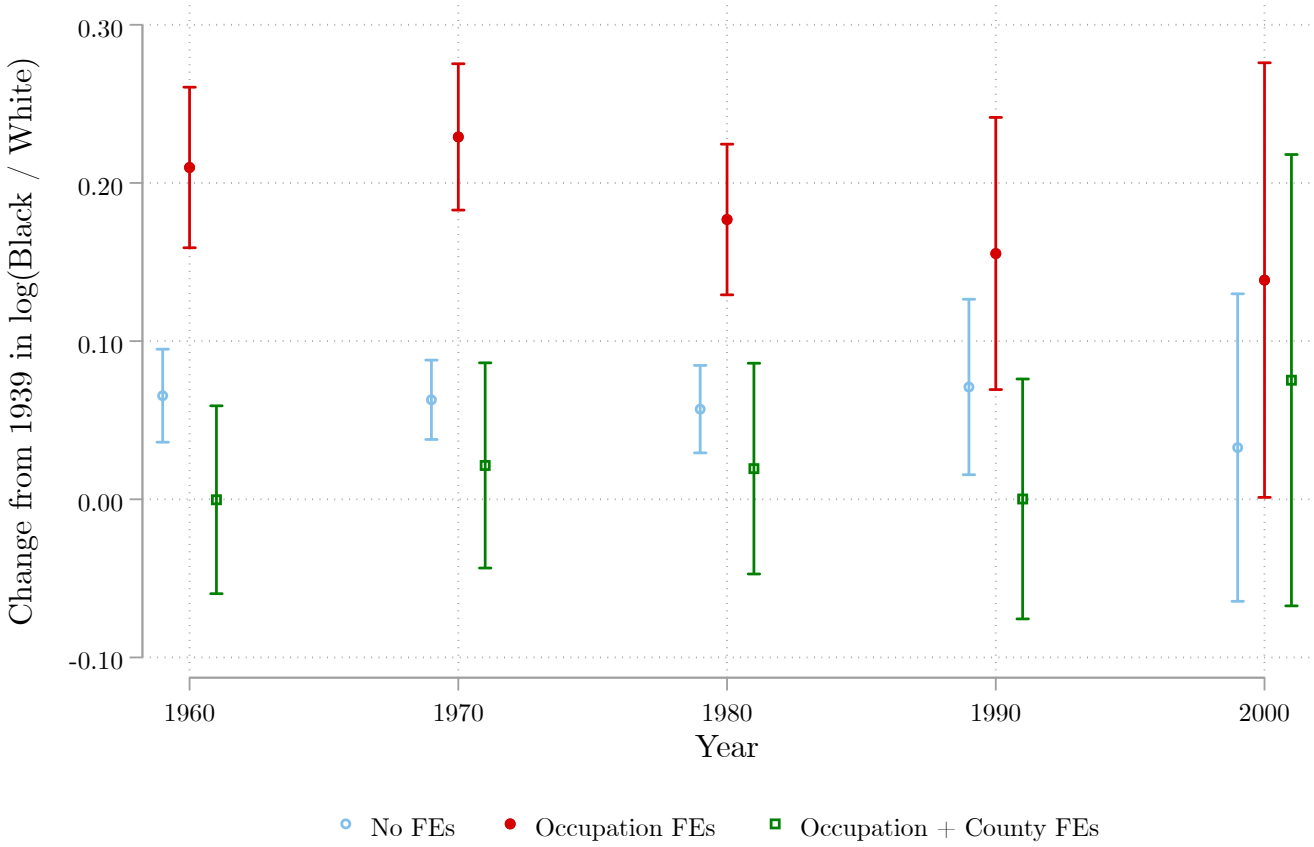
Notes: The dependent variable is the difference between the value of the statistic of log labor earnings in the given year and 1939. Regressions include the inequality value in 1939 and its square. The point in 1950 uses the 1% sample from the 1950 Population Census. The point in 1953 uses the BLS occupational wage surveys. Because of the small sample sizes in 1950 and 1953, the standard errors are large and we only report point estimates for the sake of comparison. The sample restrictions are the same as in Goldin and Margo (1992). Standard errors are clustered at the occupation by county level.

Figure 6: 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 1950 uses the 1% sample from the 1950 Population Census. The point in 1953 uses the BLS occupational wage surveys. Because of the small sample sizes in 1950 and 1953, the standard errors are large and we only report point estimates for the sake of comparison. The sample restrictions are the same as in Goldin and Margo (1992). Standard errors are clustered at the occupation by county level.

Figure 7: Effects of the Brackets on 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 sample restrictions are the same as in Goldin and Margo (1992). Standard errors are clustered at the occupation by county level.

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

	Bracket						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Q_{10}	0.254				-0.033	-0.026	-0.014
	(0.007)				(0.140)	(0.137)	(0.136)
Q_{25}		0.309			0.090	0.070	0.056
		(0.007)			(0.098)	(0.095)	(0.089)
Q_{50}			0.340		0.172	0.155	0.157
			(0.007)		(0.092)	(0.093)	(0.090)
Mean				0.350	0.123	0.137	0.117
				(0.007)	(0.139)	(0.132)	(0.141)
Married						0.085	0.094
						(0.080)	(0.076)
Age						-0.003	-0.003
						(0.005)	(0.005)
White						0.270	0.276
						(0.104)	(0.106)
HS Graduate						-0.113	-0.090
						(0.087)	(0.090)
State-Level Unionization Rate							-0.001
							(0.002)
Combat Supply Contracts							0.004
							(0.002)
Other Supply Contracts							-0.001
							(0.002)
Spending on Industrial Facilities							-0.002
							(0.001)
Spending on Military Facilities							0.001
							(0.002)
State-Level Inductions 1943							0.034
							(0.013)
R^2	0.121	0.162	0.189	0.185	0.192	0.220	0.229
Ratio of RMSE to Y							88.000
Observations	13868	13868	13868	13868	13868	13868	13856

Notes: An observation is an occupation-county cell. 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 work-week. 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. The state-level unionization rate is calculated using the data from Farber et al. (2021). The supply contracts variables as well as spending on military and industrial facilities are from ICPSR Study 2896 and were used in Aizer et al. (2020). We add 1 to these variables before taking the log. The Inductions 1943 variable is the log of the number of men inducted into the army in 1943 at the state-level. Standard errors are clustered by occupation and NWLB zone.