Labor Earnings Inequality in Manufacturing During the Great Depression*

Felipe Benguria
University of Kentucky
fbe225@uky.edu

Chris Vickers
Auburn University
czvickers@gmail.com

Nicolas L. Ziebarth
Auburn University and NBER
nicolas.lehmannziebarth@gmail.com

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Abstract

We study labor earnings inequality during the Great Depression using establishment-level information from the Census of Manufactures. Inequality as measured by the interquartile range in earnings per worker declines by 10 log points between 1929 and 1933. However, by 1935, this difference has recovered to its 1929 level. In a decomposition, this decline and then rise in inequality is entirely explained by returns to observable factors, most notably the skill premium and regional differentials. The exit of establishments plays an important role in the initial decline in inequality but barely any role in the recovery.

Keywords: Great Depression, Inequality, Establishments, Skill premium.
JEL Codes: N12, J24, J31.

Introduction

Economic historians and macroeconomists have documented in great detail the aggregate behavior of the U.S. economy during the Great Depression. However, much less is known about the distributional consequences of the downturn. Work going back to Kuznets (1953), Goldsmith et al. (1954), and Tucker (1938) has attempted to measure changes in the distribution of income and to identify which sectors, regions, and individuals were most affected by this unprecedented event. Measuring and explaining these distributional changes is important not just in determining who bore the cost of this event but also for understanding the aggregate dynamics themselves. As Robert Margo (1993) wrote, “[D]isaggregation [can reveal] aspects of economic behavior hidden in the time series[.]” For example, a central controversy surrounding the labor market is why real wages were “sticky” in the face of unprecedented levels of unemployment. Solving this puzzle requires understanding the wage-setting behavior of workers and businesses at a disaggregated level. A major problem for economists wanting to take this disaggregated approached has been fragmentary datasources.

Our first contribution is to fill in this gap in the datasources available by constructing an establishment-level dataset consisting of 25 industries from the Census of Manufactures (COM) between 1929 and 1935. We use this dataset to study the effects of the Depression on the distribution of earnings per worker. Our dataset allows us to confirm earlier work that used data disaggregated to the industry (Bernanke and Powell, 1986) or regional level (Rosenbloom and Sundstrom, 1999). However, we can go beyond earlier work to address questions such as the role of establishment entry and exit on the earnings distribution that are not possible with existing sources. Our dataset, while not covering all of manufacturing, is representative of this sector along many dimensions: geographic, types of products, and scale of production. For each establishment, we observe the earnings of *wage earners* and of *salaried workers*, which we refer to as blue and white collar workers.[1] From this, we measure

[1]This distinction is very close to that of production and non-production workers commonly used today (Davis and Haltiwanger, 1991). To support the claim that this distinction is related to skill as measured by educational differences, we show in the online appendix that there is a positive relationship between

earnings per worker for each skill group.[2]

We first characterize the distribution of establishment-level earnings per worker and the observable factors which affect this distribution. Both the difference between the 75th and 25th percentiles and between the 90th and 10th percentiles decline from 1929 to 1933 by around 10 log points. While the 75-25 difference subsequently returns to its 1929 level by 1935, these changes are driven by shrinking gaps between the bottom and median of the distribution offsetting widening gaps between the top of the earnings distribution and the median. In particular, the 90-50 difference increases by 22 log points between 1929 and 1933 before falling 25 log points between 1933 and 1935. Conversely, the 50-10 difference declines 30 log points between 1929 and 1933 before increasing 10 log points between 1933 and 1935. The changes in the overall earnings distribution are almost totally driven by changes in the blue collar earnings distribution since the white collar distribution shows very little change.

We next use the decomposition of Juhn, Murphy, and Pierce (1993) to account for the changes in the earnings per worker distribution by decomposing the changes into changes in observables, returns to those observables, and unobservables. Changes in the distribution of observables, if anything, act to increase inequality over time, but the magnitude relative to the overall change is quite small. At the same time, changes in the unobservable component play no role over time. Instead, the initial compression in earnings across establishments is driven almost entirely by changes in returns to observables, in particular, regional differences. For example, the East South Central region has observable earnings per worker 41 log points below New England in 1929, 24 points in 1933, and 25 in 1935, confirming some of the results from Rosenbloom and Sundstrom (1999). On the other hand, changes in the skill premium work in the *opposite* direction to widen inequality. After controlling for observable establishment characteristics, the gap between earnings per blue and white collar workers increases from 34 log points in 1929 to 72 log points in 1933 before declining to 55 log points

educational attainment and the share in white collar employment using data from the 1940 Population Census.

[2]In some years, we can estimate earnings per hour for blue collar workers under certain assumptions. We analyze the distribution of earnings per hour in the online appendix.

in 1935.

This latter result qualifies our understanding of the remarkable compression in earnings by skill over the first half of the 20th century (Goldin and Margo, 1992).[3] While the patterns in the skill premium we find match those of clerk wages relative to unskilled weekly wages (see their Fig. II), we find less evidence to support their claim that "inequality narrowed after 1933, probably because of the minimum wages of the National Industrial Recovery Act" for the simple reason that for blue collar employees, the 75-25 differential *increased* between 1933 to 1935 and the 90-10 differential barely changed. Wright (1997, p. 214) also points to the role of minimum wage particularly in explaining the convergence of the South. However, we find that the convergence happens before 1933, when the minimum wages were implemented in the New Deal. In fact, our results are exactly what would be expected if, as Goldin and Margo claim, "unemployment...was disproportionately borne by the least educated and lesser skilled," and as the economy recovers from its trough in 1933, less productive workers were hired back.

In light of Goldin and Margo's claim about the importance of selection, we study the effects of entry and exit of establishments on the evolution of the earnings distribution. This explanation of changes in inequality is connected to one explanation of why real wages were sticky in the Great Depression, and in recessions more generally. That is, the apparent stickiness is an artifact of aggregation (Stockman, 1983). This theory posits that the first workers to be fired or to have their hours cut in a recession are the least productive and, hence, paid the least. Furthermore, these workers are the last to be hired back as the economy recovers. All else equal, this shift in the composition of worker quality would lead to a *countercyclical* aggregate real wage masking procyclical real wages at the level of the worker.[4] Direct empirical evidence for this theory for the Great Depression is rather

[3] In comparing our results to theirs, it is important to keep in mind they use worker-level data from the Population Censuses on weekly wages among other sources. Note that they are implicitly focusing on inequality of *employed* like we are. They consider both occupational and educational based measures of skill.

[4] Studying post WWII business cycles, Solon, Barsky, and Parker (1994) find there is a large composition bias in the aggregate wage series, which obscures the "true" procyclicality of wages at the individual-level. Baily (1983) argues that an implicit contract theory could explain the puzzling behavior of the real wage

limited. In particular, Margo (1991, p. 333) states that the newly unemployed were "drawn disproportionately from the low-wage portion of the labor force," but this is based on one-time surveys in Buffalo in 1929 and 1930, Michigan and Philadelphia in 1935, and the 1940 Census of Population. In addition, Margo (1993), citing a paper by Lebergott (1989), claims that workers who were laid off were less productive and paid less.

While we cannot observe this selection process at the level of the worker, we can examine selection at the establishment-level and the extent to which higher or lower paying establishments differentially exited or entered. We find that, in fact, white collar workers at both entering and exiting establishments had lower earnings of between 3 and 14 log points depending on the year and specification than those at continuing establishments. The same penalty is generally present for blue collar workers at exiting and entering establishments, but overall these differences are much smaller in magnitude. These findings complement existing work that has studied the role of productivity in determining exit of establishments during the Depression (Bresnahan and Raff, 1991; Scott and Ziebarth, 2013; Lee, 2014).

To quantify the effects of entry and exit on changes in inequality, we construct two counterfactuals. The first predicts what earnings per worker *would* have been for exiting establishments if they had not exited based on changes in earnings of establishments with similar characteristics that did not exit. We calculate inequality measures for this counterfactual earnings distribution including the continuing establishments and imputed values for the exiting ones. We find that the 75-25 and 90-10 differentials of the exit counterfactual distribution for blue collar workers are 6 log points higher in 1933, which would explain about one third of the decline in those statistics over the same period, and negligibly different in 1935. The second counterfactual simply removes the penalty associated with entering establishments in a given year. The counterfactual distribution here is only marginally different than the actual distribution across all years and skill classes. These results show that the exit margin played an important role in the decline in inequality over the first half of the

during the Depression.

Depression, consistent with a version of Goldin and Margo's claim and the aggregation bias explanation of sticky wages. However, this effect was asymmetric with exit playing very little role in the rise in inequality between 1933 and 1935.

While there are clear strengths of our dataset, it is important to keep in mind its limitations for extrapolating to the overall earnings distribution. First, we lack information on those not employed in manufacturing. Employment in manufacturing only comprised about one third of total non-farm employment in 1929, but, because of the sharp declines in manufacturing employment between 1929 and 1933, the declines in this industry explain about 43% of the total decline in non-farm employment. This makes it particularly important to study manufacturing for understanding the Depression, and its importance is reflected in the long literature that focuses on this sector of the economy (Bernanke, 1986; Bernanke and Parkinson, 1991; Bresnahan and Raff, 1991; Rosenbloom and Sundstrom, 1999). The second limitation is that the changes in the earnings distribution we measure are for the *employed*. The Great Depression saw a large increase in unemployment, from just over 3% in 1929 to a peak of 22.6% in 1932 (Darby, 1978). The earnings of workers who were formerly employed in manufacturing and became unemployed or employed in a different sector are not captured by our sample.

A final limitation is that, with few exceptions, the literature on inequality has used data drawn from household surveys to estimate earnings inequality at the *worker*-level while we use establishment-level data. The resulting measures of inequality are distinct and the comparison between our results and those in the literature using worker data is a bit more difficult. At the same time, our approach is consistent with a new literature that has emphasized the importance of demand side factors in determining inequality (Card, Heining, and Kline, 2013; Barth et al., 2016; Song et al., 2019). This new view that emphasizes firm and establishment characteristics should be contrasted with the older, supply-side view that emphasized worker characteristics such as education or experience. Though our use of labor demand side information is partially motivated by data availability, our results, particularly those focused on the extensive margin, highlight the importance of this side of the labor market.

There is an existing literature on the Great Depression studying various dimensions of inequality. For example on the geographic dimension, Hanna (1954), Schmitz and Fishback (1983), and Creamer and Merwin (1942) all study changes in earnings across states. Mendershausen (1946) uses the Financial Survey of Urban Housing, which contains city-level information about income in 1929 and 1933, to calculate inequality statistics. There are also a handful of papers that focus on the industry dimension, including the classic work by Bernanke (1986) cited earlier. He studies a sample of eight industries with information on employment and wages, drawing on data first studied by Beney (1936). Hanes (2000), following work by Shister (1944) and Dunlop (1944), examines industry characteristics and their relationship to wage rigidity during the Depression and two other downturns in 1893 and 1981. Hausman (2016) shows that much of the geography of the 1937 recession is explained by industry differences, particularly the sharp decline in the automobile industry. Finally, on a racial dimension of inequality, Sundstrom (1992) finds that black unemployment went up more than did unemployment among whites during the Depression. Unemployment rates were actually close to parity at start of the downturn, but by 1931, black individuals were 1.5 times more likely to be unemployed.

Data and Data Issues

Data Source: Census of Manufactures (COM)

We use the COM, for which the original, establishment-level schedules from 1929, 1931, 1933, and 1935 are available in their original form at the National Archives in Washington, DC.[5] The COM was collected in other years, including during the Depression, but the establishment-level schedules from the first half of the 20th century do not exist other than

[5]The records are located in Record Group 29 "Records of the Bureau of the Census, 1790 - 2007."

for these four years. The schedules provide a wealth of detail including a breakdown of outputs and inputs into quantities and values. For our purposes, there is also information on labor use broken down by type of worker, number employed, and total earnings. We draw on a sample of 25 industries summarized in Table 1.[6] As discussed in the paper by Vickers and Ziebarth (2019), the sample is the work of many different people who collected industries for their own individual purposes.[7] While not chosen to be representative, these industries cover a wide variety of manufactured products from business durables to consumer non-durables to "high tech" products like radios, autos, and aircraft. Table 2 shows that our sample comprises about 20% of the total value of output in manufacturing, 10% of the establishments, and 20% of the total wage bill.

The sample was built in this "curated" way rather than by, for example, randomly sampling some unit (establishments, industries, states) for a number of reasons. First, given how the schedules are organized at the National Archives, it is much easier to sample industries than, say, geographic units because the schedules are arranged by industry and then by state within an industry. We also did not sample at the establishment-level since in our view, this would have limited the value provided by the establishment-level nature of the original schedules. For one, there are a number of important questions about competitive behavior in this period that require complete data from a particular industry. For example, Chicu, Vickers, and Ziebarth (2013) and Vickers and Ziebarth (2014) use the schedules from two industries to study collusion under the National Industrial Recovery Act (NIRA). In addition, one of the benefits of establishment-level data is the ability to follow establishments over time. If we just randomly sampled establishments, for example, this would lead to a very small set of establishments that we observe in multiple years (even ignoring the high rates of turnover). We also did not sample whole industries randomly for the simple fact that we thought certain industries were of more interest than others for the purposes of studying the Depression.

[6] These data are from ICPSR Study 37114 (Vickers and Ziebarth, 2018).

[7] For autos, see Bresnahan and Raff (1991). For concrete, see Morin (2015). For blast furnaces, see Bertin, Bresnahan, and Raff (1996). Some of these industries are also available as ICPSR studies: 35604 and 31761 (motor vehicles); 35605 (cotton goods); 37211 (sugar refining); 37208 (blast furnaces); and 31761 (textiles).

On the balance, we would argue that our sample is fairly representative of manufacturing as a whole. In the online appendix, we provide a number of maps showing the geographic coverage of our sample in terms of covering manufacturing totals at the county-level. While reflecting manufacturing across the country, our sample covers a particularly large portion of manufacturing employment in the Carolinas and Georgia because of the dominance of textiles in those states. On county-level demographic and economic characteristics, we show in the online appendix that our sample is (slightly) over-representative of counties that grew faster between 1920 and 1930, voted more Democratic in presidential elections, had lower literacy rates, and had more African Americans.[8] We would argue that these demographic differences reflect again the fact that the textile industry, which is part of our sample, dominates southern manufacturing, and so the geographic areas where the coverage of our sample is highest tends to be predominantly from the South. As for economic characteristics, there are only minor differences if we consider bank failure rates or the change in retail sales. Finally, in the online appendix, we show that in 1929, industries that make up our sample are similar to those in industries not in our sample in terms of size as measured by employment and revenue, as well as revenue per blue collar worker and earnings per blue collar worker.

Similar to the papers by Davis and Haltiwanger (1991) and Atack, Bateman, and Margo (2004) (ABM), we will run regressions explaining labor earnings with establishment-level covariates. Davis and Haltiwanger (1991), who work with the post-1963 COM, include size as measured by number of employees, age, ownership, energy costs, product specialization, and capital intensity as well as regional and industry fixed effects. ABM (2004), who work with the 19th century COM, include number of employees, capital intensity, use of steam power, as well as regional and industry fixed effects. In the COM data we employ, information about region, industry, and size as measured by total employment (or our preferred measure of revenue) is all available. On the other hand, the information for age is sparse. The

[8]The online appendix also discusses the quality of these data. We cite a number of other studies that examine parts of this question. We also provide evidence that the rate of rounded values does not change over time, which suggests, at least, a similar degree of measurement error in each of the censuses.

form for the 1929 census asks if the plant had begun operation before January 1, 1928, but other than that there is no information on age. None of the other censuses ask such a question.[9] We also have information on the incorporation status of an establishment as well as whether the establishment is part of a single-plant or multi-plant firm as used in Davis and Haltiwanger (1991).[10] As for other variables emphasized by Davis and Haltiwanger (1991), we have information for energy sales for 1929 and 1935 only. The most important limitation of our COM data is the lack of a measure of capital, which is by and large absent. There are, in some cases, industry-specific measures of physical capital stock; for example, the amount of compression capacity in the manufactured ice industry. However, nothing is systematic enough to use in regressions pooling across industries and years.

Skill Categories and Their Comparability Across Years

We now turn to a discussion of how we measure the skill of the workers and their earnings. The forms do not provide any information on, for example, educational attainment of workers. Instead, we will distinguish between wage and salaried employees, or what we will call blue versus white collar jobs. Blue collar workers were presumably mainly production workers on the factory floor, but they could also include hourly janitorial staff or workers on the loading dock. White collar workers in this classification were clerks, administrative officers, and office workers generally. In the modern COM, the labor force breakdown provided is into production and non-production workers. Dunne, Haltiwanger, and Troske (1997) defend the use of non-production workers as a measure of skilled workers. Our breakdown is quite similar to this.[11] All of our salaried workers would fall into the modern category of non-production workers. On the other hand, there are surely some hourly workers such as janitors that would be classified as non-production workers in the modern taxonomy, but

[9] Of course, for establishments which are linked across censuses, we at least know a lower bound for age. That is, an establishment in both the 1931 and 1929 censuses must be at least 2 years old in 1931.

[10] This is constructed by matching on the name of the operating firm.

[11] In the online appendix to bolster this skill interpretation, we show that individuals with white collar occupations have higher levels of educational attainment using data from the 1940 Census of Population.

are blue collar wage earners in ours.

For blue collar workers, in all four censuses, the form asks for the number of wage earners employed in each month.[12] In computing earnings per blue collar worker, we divide the total annual wage bill by the average of the monthly employment figures. As noted by Rosenbloom and Sundstrom (1999), the Census Bureau itself cautioned about how to interpret the wage earner employment number. In particular, it listed some possible reasons why the employment numbers are inflated relative to the "true" number of full-time equivalent employees. First, the establishments might have reported part-time workers as well. Second, workers that were laid off might have stayed on the payroll for a time and still been potentially counted as working at the establishment. The earnings variable is much more straightforward and comes from a question that does not change over the years asking for the "total amount paid to wage earners."[13]

For the white collar skill group, the computation of earnings per worker is somewhat more complex. First, we exclude officers and proprietors from the totals since we have no information about their earnings. Next, in 1929, the Census Bureau asked about the number of "managers, superintendents, and other responsible administrative employees; foremen and overseers who devote all or the greater part of their time to supervisory duties; clerks, stenographers, bookkeepers, and other clerical employees on salary", as well as the total

[12]Precisely, the census forms in 1929 and 1933 ask establishments to include "skilled and unskilled workers of all classes, including engineers, firemen, watchmen, packers, etc." along with "foremen and overseers in minor positions who perform work similar to that done by the employees under their supervision." The question for these groups in 1935 was somewhat different, asking respondents to include "all time and piece workers employed in the plant", not including employees included in the other enumerated categories of officers, managers, clerks, and "technical employees."

[13]As a way to allay concerns about the earnings of blue collar workers, we can compare average earnings per worker in our data to an external source. Leo Wolman in NBER Bulletin 46 published in 1933 reports on BLS statistics showing that the average weekly earnings per worker in manufacturing in 1929 was $27.36 in nominal units. If we multiply this by 51 weeks and apply the inflation adjustment we use, we arrive at an annual earnings per worker of $19,228 in 2015 units. We interpret the BLS study as being about blue collar workers. In our data in 1929, this average is $20,642. These numbers do differ but not by an order of magnitude. The average earnings of the industries in our sample is slightly higher than industries not included so if we could adjust for this difference, the difference between our average and that from the BLS would be even smaller. Also, to be sure, the BLS average is not derived from a strictly random sample of establishments as (Lebergott, 1989) points out. With all of these caveats in mind, we would argue that the similarity of these two averages is support for the quality of our data.

amount that this group was paid. In 1933, this category is split between the managers and clerks reported separately, along with their total wage bills, with no mention of foremen.[14] For 1935, the same three white collar categories of officers, managers, and clerks are reported, with the total number of clerks being reported for four separate months. Unfortunately, the 1931 census form does not contain any information about the number of or payment to white collar workers.

Establishment-level Earnings Measurement

To emphasize, we will be comparing earnings per worker by "collar" color computed by dividing total earnings by total workers in that type of job at an establishment. The establishment-level nature of this measure is different than much of the rest of the literature on inequality that measures *worker*-level variation in earnings. All earnings are measured in 2015$ adjusting for inflation using the CPI-U price index. The 1% tails are winsorized to limit the role of measurement error. Winsorizing or trimming the tails of the distributions is common in the literature that works with (modern) establishment-level data, for example, Davis and Haltiwanger (1991). Atack, Bateman, and Margo (2004) also trim the tails in their dataset from the 19th century COM.[15]

To clarify the relationship between the measures at the establishment and worker levels, let y be worker-level earnings and x be an establishment identifier (ignoring the skill dimension for now). Then the law of total variance states

$$Var(y) = Var(E[y|x]) + E[Var(y|x)].$$

where $Var(y)$ is the worker-level variance of earnings, $Var(E[y|x])$ the variance of average earnings between establishments, and $E[Var(y|x)]$ the average variance of earnings within

[14]In both 1929 and 1933, the schedule specifically includes foremen in "minor positions who perform work similar to that done by the employees under their supervision" in the wage earner category.

[15]In the online appendix, we discuss the information we have on hours worked per week and our ability to infer earnings per hour for blue collar workers.

an establishment.[16] What we actually observe is the term $Var(E[y|x])$, the establishment-level variance of earnings per worker. So our establishment based measured of inequality will underestimate worker-level inequality because we do not observe the *within* establishment term $E[Var(y|x)]$, which is strictly positive. Because of this, the *levels* of our inequality measures are not directly comparable to others in the literature using worker-level data.

The relevant question for us is whether *changes* in our measure reflect changes in worker-level inequality, which, in turn, depends on the changes in within-establishment inequality. The most plausible theory in our view, based on modern evidence (Solon, Barsky, and Parker, 1994) as well as in the Depression where the evidence is much more indirect (Lebergott, 1989; Margo, 1991; Sundstrom, 1992), is that the least productive and lowest paid workers *within an establishment* are the first to be fired, meaning $E[Var(y|x)]$ would decline in recessions. This would imply that changes in our measure of inequality $Var(E[y|x])$ *understate* changes in the worker-level measure $Var(y)$.

An additional question in measuring earnings per worker is how to handle establishments that report no employees (and, hence, pay nothing to workers) yet have positive revenue. These make up a non-trivial fraction of establishments in our dataset and presumably are cases where the owner, besides managing the establishment's operations, also works directly in production. Margo (2015) discusses this same problem in the 19th century COM and its effects on estimates of the returns to scale of the production function. While potentially important for the question of returns to scale and estimating overall labor income shares, it is not obvious what the effects of these establishments have on earnings inequality of workers, our particular interest. One could construct scenarios where the individuals running these businesses are on average more able and, therefore, their shadow earnings as an employee are high relative to the mean of the observed earnings distribution. One might also imagine that because the owner has to do many different tasks, he is relatively less productive than a worker specialized in doing one task. In this case, the shadow earnings as an employee

[16]Note that we will not be studying the variance, but rather differences in percentiles, as our measure of inequality. So this formula does not strictly apply to our case, but the same intuition holds.

of a sole proprietor would be low relative to the mean of the observed distribution. While both of these theories would affect the level, it is more difficult to see how they would affect *changes* in measured inequality. We have chosen to drop this set of establishments.

Changes in the Earnings Distributions

The first panel of Figure 1 shows the overall distribution of earnings pooling blue and white collar workers, where each establishment-"collar" is weighted by the number of workers. We emphasize that this distribution is not the "true" distribution of worker earnings in manufacturing, since we are implicitly treating all workers at a given establishment as earning the same amount. This will surely mask variation in earnings within an establishment and skill group, so the inequality measures based on this distribution will be lower than the measures from the distribution where we observed every individual employee's earnings. However, the Davis and Haltiwanger (1991) decomposition for modern data shows that over half of the variance in manufacturing earnings per hour inequality can be accounted for by between-establishment differences. This suggests that a substantial fraction of changes in the worker-level earnings distribution can be captured using changes in establishment-level earnings distribution.

The distribution goes from being bimodal in 1929 to unimodal in 1933 and then back again in 1935.[17] Panel A of Table 3 reports the changes in various differences in percentiles for this distribution. Focusing on the 75-25 measure of inequality, there is a decline in 10 log points between 1929 and 1933. By 1935, the 75-25 differential is nearly back to its pre-Depression 1929 value. We consider this as evidence that the Depression did not have permanent effects on the "bulk" of the manufacturing earnings distribution as reflected in the 75-25 differential. On the other hand, the 90-10 inequality measure shows a decline over this six year period, though this is almost totally concentrated in the distribution of earnings

[17] In the online appendix, we do a variance of earnings decomposition into within and between industry components. We find that the ratio of within to between declines monotonically over this period for white collar earnings and follows a U-shaped pattern for blue collar.

for blue collar workers, a point we return to later.

This finding needs to be qualified since this is the distribution of earnings *for those employed*, and the Depression is a time of large declines in the number of people working.[18] To think about the effects of changes on this margin, assume that with probability $1 - p$, a person receives 0 income and with complementary probability, a person receives income w drawn from distribution F with standard deviation σ. We think of this setup as capturing a case where there is a fixed pool of "manufacturing workers" who work in that sector or are unemployed (and receive nothing). Then the standard deviation of the earning distribution including those that are unemployed is $SD(w) = p\sigma$, or, in log differences, $\Delta \log SD(w) = \Delta \log p + \Delta \log \sigma$. The second piece $\Delta \log \sigma$ is what we are estimating here. We could estimate $\Delta \log p$ as the log change in total manufacturing employment ignoring changes in the overall population. Note that decreases in p, which are increases in manufacturing unemployment, all else equal *reduce* overall inequality. This is because in our example, all unemployed workers receive earnings of 0. The same logic would apply if those that became unemployed received an income draw from some less dispersed distribution (though the effect on changes in overall inequality would be more muted).

We now decompose the overall earnings into the distributions for blue and white collar workers in the other panels of Figure 1. Quite clearly, the qualitative changes in the overall earnings distribution are being inherited from the blue collar earnings distribution. The 1929 and 1935 distributions for white collar are nearly identical. Panels B and C of Table 3 report the changes in the differences in percentiles for these distributions. As is evident from the figures, the changes in the overall earnings distribution are driven by changes in the blue collar earnings distribution. This is also due to the fact that blue collar blue collar workers make up the lion's share of total employment. So the overall earnings distribution

[18]On the other hand, unless tax data is being used, studies on inequality from this time period are in effect studies of inequality for the employed because of the datasources available. These data such as those from the Conference Board (Bernanke, 1986) are not representative samples of the population, but of employed workers.

places much more weight on changes in the blue collar distribution.[19]

Decomposition of the Earnings Distribution

We now decompose the sources of those changes using the procedure developed by Juhn, Murphy, and Pierce (1993) (JMP). We write the earnings per worker t establishment i in year t as

$$y_{it} = X_{it}\beta_t + u_{it} \tag{1}$$

where y_{it} is log earnings per worker and X_{it} a vector of observables characteristics, and a residual unobserved component u_{it}. The observable characteristics include establishment-level characteristics as well as whether the earnings corresponds to white or blue collar workers. The "white collar" indicator is an estimate of the skill premium in establishment-level earnings per worker. Let F_t be the distribution of the residuals, which is potentially time-varying.[20] Then $u_{it} = F_t^{-1}(\theta_{it})$ where θ_{it} is the percentile in the earnings distribution.

Changes in the earnings distribution can come from three possible sources: (1) changes in average establishment characteristics X_{it} ("observables" in the language of JMP); (2) changes in the relationship between these characteristics and incomes represented by β_t ("prices"); or (3) changes in the distribution of unobservables F_t ("residuals"). To see this analytically, take 1929 as the base year and define $\bar{\beta} = \hat{\beta}_{1929}$ and $\bar{F}^{-1}(\theta_{it}) = \hat{F}_{1929}^{-1}(\theta_{it})$. Now if the distribution of residuals and the coefficients β_t were fixed over time, then earnings would be

$$y_{it}^1 = X_{it}\bar{\beta} + \bar{F}^{-1}(\theta_{it}).$$

So changes in the distribution of y_{it}^1 are those attributable to changes in the observable

[19] In the online appendix, we show this change is further concentrated in durable goods industries, which were most affected by the Depression.

[20] Following the literature, we assume that F_t does not depend on X_{it}. Note that under this assumption of full independence, we would be justified in calculating standard errors based on homoskedasticity. We again follow the literature here and report the usual heteroskedasticity robust standard errors.

characteristics of the establishments. Now if the coefficients β_t can change as well as the value of the covariates X_{it}, then earnings would be

$$y_{it}^2 = X_{it}\beta_t + \bar{F}^{-1}(\theta_{it}).$$

So any additional change in the distribution of y_{it}^2 relative to y_{it}^1 is due to changes in the returns to the observables, what JMP call prices. Finally, if we further allow the distribution of residuals to change, then earnings are given by

$$y_{it}^3 = X_{it}\beta_t + F_t^{-1}(\theta_{it}),$$

which is the actual level of earnings. Any additional changes to $y_{it}^3 = y_{it}$ relative to y_{it}^2 can be attributed to changes in the residuals.

To understand what we will report in the end, let Y_t be a statistic of the earnings distribution in year t such as the interquartile range (d7525) or the 90-10 gap (d9010). Also, let Y_t^k be the same statistic calculated using the distribution of y_{it}^k for $k = 1, 2, 3$. Then we write the following decomposition

$$Y_t - Y_{1929} = [Y_t^1 - Y_{1929}^1] + [(Y_t^2 - Y_{1929}^2) - (Y_t^1 - Y_{1929}^1)] + [(Y_t^3 - Y_{1929}^3) - (Y_t^2 - Y_{1929}^2)],$$

where we will refer to the first term on the right hande side as "Observables", the second "Prices", and the third "Residuals" while the term on the left hand side is what we call the "Totals" term. These four terms are what we will report. By construction, each of these terms will be equal to 0 in 1929, and clearly the sum of the three components on the right hand side will add up to the total change in the statistic between year t and 1929.

It is important to remember that this is just an accounting decomposition and should not be interpreted structurally. Besides the original paper by JMP, a number of authors working with manufacturing data have employed this procedure, such as Davis and Haltiwanger

(1991) using modern COM data and ABM (2004) using the 19th century COM. In estimating the regression and constructing the residuals, we "pool" both blue and white collar workers and weight each establishment-"color" by the number of workers in that group. For the vector of characteristics X_{it}, we include in the log of revenue, a binary variable for whether or not the establishment is part of an incorporated firm or not, whether the firm owns multiple establishments or plants, as well controls for Census regions and industry as well as the "white collar" indicator.[21]

Table 4 shows the results from the regressions for each year pooling both types of workers weighted by the number of employees with robust standard errors.[22] First, and not surprisingly, we find a large skill premium—more precisely, difference in average earnings between white and blue collar workers—of 34 log points in 1929, increasing to 72 points in 1933, and falling back to 55 log points. While Goldin and Margo (1992) report an increase between 1929 and 1933 in the ratio of monthly income for clerks to laborers in the railroad industry of about 21 log points, these numbers are not strictly comparable, as our blue collar workers are not "unskilled" and white collar workers include all salaried workers, not clerks only. Qualitatively speaking, the skill premium patterns we identify are consistent with the summary of Goldin and Margo (1992): "Although inequality [as measured by the skill premium] widened at the beginning of the 1930s, it narrowed after 1933, probably because of the minimum wages of the National Industrial Recovery Act. Most series we have uncovered show little overall change from 1929 to 1939, although the wage structure changed within the decade."[23]

There is a significant and stable establishment size premium across all three years with the

[21]We use Census regions even though we have much finer geographic detail available to be consistent with earlier work that used these geographic categories.

[22]In the online appendix, we report the results of the JMP decomposition for both earnings per hour and hours per workweek.

[23]On the other hand, in regressions not reported here, we have tried to explain these changes at an industry-level through the NIRA minimum wages with little success. One reason for this failure in our view is the fact that minimum wages were region (and industry) specific, which limited to what extent they were actually binding.

coefficient on (log) revenue between 0.05 and 0.07.[24] This positive size premium is consistent with modern literature on this relationship (Davis and Haltiwanger, 1991). On the other hand, this is different from the patterns in the 19th century as estimated by ABM (2004), who broadly speaking find a negative size gradient. They interpret this negative gradient in the context of the Goldin and Katz (1998) model of skill-technology complementarity. A positive relationship is relatively straightforward to explain in a model with differences in productivity, decreasing returns to scale, and rent sharing. Then higher productivity firms have higher labor demand and, hence, are larger. Furthermore, they share this higher productivity with their workers due to a less than perfectly competitive labor market.

The overall pattern of the regional differences in average earnings (the New England region is the excluded category) is as expected, with a large negative coefficient associated with being located in the South, and all regions having lower average earnings than New England. What is striking is the decline between 1929 and 1933 in the penalty for being in the South, which includes the East and West South Central as well as the South Atlantic regions. These findings echo the results of Rosenbloom and Sundstrom (1999), who find that the Depression was comparatively mild in the South after controlling for industry composition (and long-run trends).

In Figure 2, we show the values from the JMP decomposition for all workers, taking 1929 as the base year and defining changes relative to that year.[25] We plot the decomposition results for both the difference between the 75th and 25th percentiles (d7525) and the difference between the 90th and 10th percentiles (d9010). The "Totals" figure replicates the pattern observed in the summary statistics of a fanning out of the earnings distribution in 1933

[24]We use revenue as our measure of size rather than employment, which was used by ABM (2004). One reason for this is that it avoids a potential mechanical reason for finding a negative size gradient due to measurement error in the employment variable. Since they use employment in the denominator of their dependent variable of earnings per worker-month, mismeasurement in employment will generate a spurious negative relationship between earnings per worker and employment. In the online appendix, we show our results are not sensitive to this choice.

[25]In the online appendix, we provide a table with the exact numbers for the components of the decomposition. We also show that the results are insensitive to the choice of base year as well as using the winsorized earnings variable.

and then a return to the pre-Depression spread by 1935 in the 75-25 differential. Clearly, changes in the residuals play no role, and while changes in observables matter slightly, the vast majority of the changes in inequality as measured by the 75-25 differential are driven by changes in prices, or the returns to observables. The fact that prices matter is similar to the original paper by JMP, who argue that much of the increase in earnings per week inequality between 1963 and 1989 was due to increases in the returns to skill (though a substantial role is still played by changes in the residuals). On the other hand, ABM (2004) find almost no role is played by changes in prices in explaining increasing dispersion in the earnings per month during the 19th century. Instead, two thirds of the increase are due to increasing residual dispersion and the rest to changes in observables.[26]

The compression in regional earnings differences is the main reason why changes in the prices component explain much of the changes in inequality. While the convergence of the South with New England is consistent with the findings in Wright (1997, Table 7.7, p. 217), the timing is not consistent with Wright's explanation. He argues that this convergence was due to the minimum wages imposed by the NIRA, which had a stronger effect in the South because region's relatively lower average wages. However, the NIRA was not passed until June 1933, and furthermore, the minimum wages were set on an industry by industry basis in a so-called "Code of Fair Conduct." In addition, many of them also specified lower minimum wages for establishments operating in the South. There were other policies beyond the NIRA that may have had regionally heterogeneous effects. During the decline, the responses of the regional Federal Reserve banks to the collapse of the banking were quite heterogeneous. The Atlanta Federal Reserve was on one extreme in responding aggressively to limit the fallout from the panics. On the other extreme, the actions of the St. Louis Federal Reserve, at best, abated the panics and, at worst, exacerbated it (Richardson and Troost, 2009). In the recovery period, New Deal spending was allocated in a heterogeneous way across the country with some areas favored for political reasons (Wallis, 1998). We leave it to future work to

[26]In the online appendix, we redo this decomposition separately for durable and non-durable industries.

examine the role of these policies on regional convergence.

On the other hand, holding the quantities of skilled and unskilled labor fixed, the increase in the skill premium between 1929 and 1933 will increase inequality. Why might the skill premium change over the business cycle? One reason might be that the elasticity of substitution between high and low skilled labor is not equal to one so that changes in demand lead to changes in relative demand for these two types of labor. Our finding that the skill premium rises is suggestive that in response to declines in output demand, establishments want to substitute away from unskilled labor toward skilled. Another interpretation for the changing skill premium is that there are differing levels of selection on unobserved productivity between these groups. In particular, our results are consistent with establishments first laying off the least productive and lowest paid skilled worker. This would lead to an *increase* in the average earnings of skilled relative to unskilled. It just so happens that the changes in the geographic differences dominate the changes in the skill premium and so price changes end up contributing to a decline in inequality.

The Role of Establishment Entry and Exit

We now study the role of the extensive margin of establishment entry and exit in driving changes in the earnings distribution in manufacturing. To do this, we exploit the panel structure of our dataset. To get a sense of the importance of the extensive margin for employment, we can decompose the change in total employment between two consecutive censuses ΔEmp into changes due to entry and exit as well as continuing establishments. By definition,

$$\Delta Emp_t = \Delta Emp_t^{CONT} + Emp_t^{\text{Entered}} - Emp_{t-1}^{\text{Exiting}}$$

where ΔEmp_t^{CONT} is change in total employment for continuing establishments between consecutive censuses, Emp_t^{Entered} total employment of entering establishments in the current census, and $Emp_{t-1}^{\text{Exiting}}$ total employment of exiting establishments in the preceding census.

Between 1929 and 1933, more than 100% of the change in blue collar employment, and 70% for white collar employment, can be explained by exit. This shows the important role played by the extensive margin of establishment entry and exit for total employment.

The fact that the extensive margin played such a large role should not be surprising given the tremendous amount of churn in establishments during this period of time. In particular, in our sample, about 60% of the establishments appeared in only a single year. What is perhaps surprising is that this churn is due not just to particularly high rates of exit but also high rates of *entry*.[27] One concern is whether this churn is simply due to measurement error. To the extent that this measurement error is uncorrelated with observable characteristics, it will generate an attenuation bias that obscures differences in earnings between continuing, entering, and exiting establishments.[28]

Unlike the modern COM, the Census Bureau at this time did not assign unique identifiers to establishments, which would allow for (relatively) error free linking over time. Instead, our definition of entry and exit is defined as whether we are able to identify a particular establishment over time based on the "sticky" information reported on the schedules. This information includes the name of the establishment and its location. A number of papers using these data (Chicu, Vickers, and Ziebarth, 2013; Ziebarth, 2015; Hansen and Ziebarth, 2017; Vickers and Ziebarth, 2019) have examined this issue in a variety of ways from examining particular industries with external datasources to internal data checks on whether exit is predictable. All of them come to the conclusion that these links reflect real exits and entries. Finally, we note that this rate of churn is not orders of magnitude greater than the rate in the modern COM. For example, Dunne, Roberts, and Samuelson (1989) find 5-year exit rates for manufacturing establishments of around 36% in the "normal" period of the 1970s. [29]

[27]For example, in 1933 entrants make up 27% of the sample, compared to 37% in 1935.

[28]One way in which measurement error might be correlated with observables is if the Census did a better job of canvassing the larger establishments.

[29]It is not possible to calculate such a rate for the 19th century since the existing samples are just repeated cross-sections and the information for identifying was not recorded.

The relationship between establishment entry and exit and the earnings distribution depends, in part, on whether what drives entry and exit is correlated with earnings. One closely studied driver of establishment survival in this period is productivity. While the literature disagrees about the proper way to measure productivity, whether in terms of physical output or revenue, there is wide agreement that it is an important determinant of exit.[30] Furthermore, in regressions not reported here, we document a positive relationship between productivity and earnings in the data. So at least, the establishment-level connection between the extensive margin and earnings is a plausible one.[31]

We study this relationship between skill group earnings across establishments comparing those establishments that enter or exit relative to those that continue. In particular, for skill group s at establishment i observed in COM t,[32] we estimate cross-sectional regressions for (log) earnings per worker y_{it} that include dummies for entry and exit along with a set of controls including industry and region fixed effects:

$$y_{ist} = \beta_1 \cdot \text{Exiting}_{it} + \beta_2 \cdot \text{Entered}_{it} + X_{ist}\gamma + \epsilon_{ist}.$$

Regressions are weighted using employment weights and X_{ist} includes the same set of controls as the JMP regressions. There are a few issues of timing worth noting. First, the exit indicator is defined as whether the establishment exits between COM t and the next COM $t+1$. This means that the exit indicator in 1929 is based on whether an establishment exited by *1931*, not 1933 when we next have information on blue collar earnings. On the other hand, the entry indicator is defined as whether the establishment enters between COM $t-1$ and COM t. Thus, the entry indicator in 1933 is based on whether the establishment

[30]For the automobile industry, see Bresnahan and Raff (1991); Lee (2014). For the radio industry, see Scott and Ziebarth (2013). For a modern study on the importance of revenue- versus quantity-based productivity for selection, see Foster, Haltiwanger, and Syverson (2008).

[31]The link between these two does not need to be driven by productivity. For example, if size by itself was correlated with entry and exit and earnings, as it is in the data, this would be enough to generate this relationship.

[32]For simplicity, we define the COM at 1929 to be $t = 1$, 1933 to be $t = 2$, and 1935 to be $t = 3$.

entered between 1931 and 1933, not 1929 and 1933.[33] Because of this timing convention, we can only include both entry and exit indicators in 1933. For 1929, we do not know the entry status of an establishment and for 1935, we do not know the exit status.

Table 5 reports the results of these regressions for exit and entry for blue collar workers by year. To make the regressions easier to compare within a year, we hold fixed the same set of establishments within a year that are not missing any of the covariates. Columns 1-4 of Table 5 shows that in general, exiting establishments pay less than continuing establishments though this is sensitive to controlling for other observables. The 5 log point penalty in 1929 after controlling for industry and region almost completely disappears with the additional controls, particularly log revenue. On the other, Columns 5-8 show a very stable penalty associated with entry across specifications and years of between 5 and 7 log points.

Table 6 reports the same set of regressions for white collar workers. We find in columns 1-4 that, unlike blue collar workers, there is a negative relationship between earnings per worker and exit. This penalty of around 3-4 log points is smaller in magnitude in 1933 than in 1929 when it is 8-12 log points. Columns 5-8 show that, consistently, across years and specifications, establishments that enter pay less than continuing establishments. These penalties range from 5 log points in the case of 1935 with additional controls to 14 log points in 1933 with no controls. Taken as a whole, these results, which show, on the whole, that earnings per worker are related to the entry and exit of establishments, provide motivating evidence for the role of the extensive margin in contributing to changes in the earnings distribution.

Quantifying the Effects of the Exit Margin

We now consider how changes in exit affect the observed earnings distribution by considering a counterfactual that imputes earnings and employment for exiting establishments. To do

[33]This is important to remember because it keeps the comparison of coefficients on entry and exit across the years "fair" by comparing in both cases (1929 and 1933 for exit, 1933 and 1935 for entry) extensive margin changes over 2 years.

this, we first estimate the following specification on the set of *surviving* establishments between adjacent censuses:

$$\Delta y_{it+1} = \beta X_{it} + \epsilon_{it},$$

where X_{it} includes log revenue and fixed effects for year, state, and industry.[34] With these regression results (not reported here), we impute earnings per worker for exiting establishments based on their characteristics as $\hat{y}_{it+1} = y_{it} + \widehat{\beta} X_{it}$. The assumption is that exits are random once we condition on the pre-determined observables in X_{it}. Under this assumption, we can predict the earnings of exiting establishments based on the growth rate of earnings for continuing establishments with similar characteristics. Put differently, we assume that, after conditioning on a set of observables, there are no unobservables that simultaneously determine whether an establishment exits and its employment or earnings growth. This assumption does not imply that the levels of earnings for exiting and continuing establishments are necessarily the same conditional on observables. Instead our assumption is consistent with an establishment having to pay too high of wages as an explanation for its subsequent exit. Our assumption does require that in the counterfactual of this establishment not existing, it would still continue to pay relatively high wages in the next census.

We also assume that there are no general equilibrium effects. That is, the distribution of earnings at continuing establishments would be unaffected in the counterfactual of no exits. To impute counterfactual employment for weighting the distribution, we use the same approach as when imputing earnings. Note that the "Actual" distributions in 1933 and 1935 exclude any establishments entering in those years, respectively. This is also why the "Actual" distributions here will differ from the "Actual" distributions in the entry counterfactual that we discuss later. So these figures show how the distribution of earnings would have evolved for establishments in the preceding census if there had been no exit.

Table 7 shows the difference between the counterfactual and the actual measures of in-

[34] In the online appendix, we show that the results are not too sensitive to the set of controls included in this regression.

equality over time for the two skill groups.[35] We calculate standard errors for these statistics using a bootstrap procedure that samples with replacement within the strata of year and industry. We then recalculate these statistics using the bootstrap sample and do this 50 times. We report the standard deviation of the bootstrap values of this statistic. For the blue collar distribution in 1933, both the 75-25 and 90-10 differentials would have been 6 log points higher, about one third of the change (see Panel B of Table 3), if there had been no exit. Based on the bootstrap errors and an assumption of asymptotic normality, this is a statistically significant change. Exit between 1933 and 1935 appears to have no statistically or economically significant effect on inequality. For the white collar distribution, exit plays a substantial role in compressing inequality. For example, the 90-10 spread would have been 16 log points higher in 1933 if not for exit and 13 points in 1935, though this change is not statistically significant. Given the small changes in the white collar inequality measures overall (see Table 3), the counterfactual analysis shows that exit masked a large change in inequality for white collar workers in continuing establishments. In both of these years, the differences are driven mainly by the lower tail of the earnings distribution. Exiting establishments pay white collar workers less than do continuing establishments, conditional on observables, so removal of these relatively low earning paying firms plays a compressive role.

Taken together, these results support the "first out" theory where the lowest paying establishments, which are perhaps the lowest productivity, are the first to exit in a downturn. These exits lead to a decline in the establishment-level earnings distribution mainly driven by compression in the difference between the median and lowest percentiles. However, this mechanism is not symmetric in that we do not observe the exit margin pushing up inequality during the recovery period of 1933 to 1935. One possible explanation is that during periods of rising demand, exits tend to be driven by "idiosyncratic" shocks. The question then is whether the entry margin works "in reverse."

[35] In the online appendix, we plot out the counterfactual and actual distributions.

Quantifying the Effects of the Entry Margin

Our next counterfactual considers the other extensive margin of entry. The counterfactual we compute removes the earnings penalty associated with entrants based on the results in columns 6 and 8 of Tables 5 and 6. We then recalculate various measures of inequality for the counterfactual distribution of earnings per worker with entrants' earnings adjusted in this way.[36] No adjustment is made to incumbent establishments. We calculate standard errors for these statistics using the same bootstrap procedure as described above.

Table 8 shows the difference in some measures of inequality between the counterfactual and the actual distribution.[37] There are only minor differences due to entry for either blue or white collar workers in either year. Relative to some of the statistics related to the exit margin, these entry effects are precisely estimated. We conclude that the entry margin does not work like the "opposite" of the exit margin.

Conclusion

We introduced a new, detailed, and comprehensive establishment-level dataset covering a sizable fraction of manufacturing during the Great Depression. These data allow us to study distributional changes in earnings per worker between 1929 and 1935 along a number of dimensions: skill, industry, and geography. Using these data, we find that the 75-25 differential of the overall earnings distribution declined between 1929 and 1933 and then recovered by 1935. From an accounting perspective, this was driven by changes in the blue collar earnings distribution. The distribution of earnings for white collar workers was little changed. When decomposing these results, we find changes in the returns to observable characteristics,

[36] While we make this adjustment to earnings for entrants, we do not adjust any differences for entrants in terms of employment. In the online appendix, we show that making this further adjustment does not affect the results substantially.

[37] In the online appendix, we plot out the counterfactual distribution relative to the actual one. In addition to adjusting earnings, we adjust the employment weights by the effect of entry. With these modified weights, we estimate this counterfactual distribution and find very little difference to these results.

particularly skill and geography, explain the bulk of the changes in the earnings distribution.

We then examine the role of the extensive margin of establishment entry and exit in explaining these changes. Using a procedure to impute earnings of exiting establishments, we find that this played an important role in reducing inequality between 1929 and 1933 for blue and white collar workers mainly by reducing the gap between the median and bottom percentiles of the earnings distribution. On the other hand, exit played little role in the increase in inequality between 1933 and 1935. The entry margin was not important for measured earnings inequality in either year or type of skill. Taken together, these results are consistent with a selection mechanism that operates *only* on the exit margin *only* in bad times.

Going forward, it would be interesting to go beyond the mainly descriptive analysis in this paper to identify drivers of these changes. One potential such driver is New Deal relief spending and relief employment. A number of other papers have examined the effects of New Deal relief spending on employment or total labor earnings.[38] But there is no work that examines the distributional consequence of this spending. This is an interesting question given that Darby (1978) argues that relief jobs provided by the New Deal were better paid than comparable private sector jobs. We leave it for future work to identify how this spending affected not just average earnings or employment but also the distribution of earnings.

There were also a number of other labor market interventions during this period that would presumably have direct distributional consequences. Taylor (2011) studies one such policy: work sharing under the Presidential Reemployment Agreement of 1933. Using industry-level data from the NBER Macrohistory database, he finds that employment in terms of bodies increased under this program, but there was not much effect on the intensive margin of aggregate hours worked. Unsurprisingly, these changes are reflected in declines in pay per worker. A larger labor market policy was the NIRA and its myriad of labor provisions including the minimum wage and maximum workweek. If one took the view that its effects were mainly

[38]Papers by Fleck (1999) and Neumann, Fishback, and Kantor (2010) are two examples of work studying the relationship between relief spending and earnings as well as employment.

through its effects on the workweek and largely orthogonal to the earnings distribution, then this policy prevented regional convergence in annual earnings from happening more quickly than it would have otherwise by artificially suppressing the length of the workweek in the lower wage South. This effect has to be set against the possible effects the NIRA had on reducing regional disparities in hourly wages themselves through minimum wages. We leave disentangling these possible effects of policy induced changes in inequality for future work.

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Table 1: Summary Statistics for the Industries in Our Sample

Industry	Establishments	Log Employees	Incorporated	Durable
Beverages	14,907	1.35	43.79	0
Ice cream	10,105	1.52	54.03	0
Ice, manufactured	13,242	1.57	78.98	0
Macaroni	1,269	2.18	49.38	0
Malt	131	3.09	96.90	0
Sugar, cane	279	4.53	71.96	0
Sugar, refining	77	6.47	88.31	0
Cotton goods	4,483	5.08	91.74	0
Linoleum	23	6.41	100.00	1
Matches	82	4.99	93.83	0
Planing mills	12,582	2.39	66.94	1
Bone black	223	3.25	98.17	0
Soap	1,004	2.45	80.91	0
Petroleum refining	1,547	4.13	94.96	0
Rubber tires	224	5.37	95.28	1
Cement	638	4.66	98.69	1
Concrete products	5,733	1.58	57.51	1
Glass	923	5.01	94.00	1
Blast furnaces	329	5.12	100.00	1
Steel works	1,720	5.77	98.59	1
Agricultural implements	916	3.34	80.47	1
Aircraft and parts	376	3.48	92.07	1
Motor vehicles	627	5.02	93.91	1
Cigars and cigarettes	145	4.24	77.21	0
Radio equipment	786	4.04	86.94	1

Sources: Authors' calculations.

Notes: All statistics are calculated pooling all the census years. The column "Establishments" is the total number of establishments. The column "Log Employees" is the average number of log employees including both white and blue collar workers. The column "Incorporated" is the percentage of establishments that are incorporated. We equally weight the establishments in calculating the average size and the percentage. The column "Durable" is whether we coded an industry's product as durable.

Table 2: Percent of National Total Covered by Our Sample

Year	1929	1931	1933	1935
Establishments	11.3	10.5	9.91	9.48
Blue collar earnings	20.5	17.4	19.0	20.7
Revenue	21.1	18.4	21.3	18.8

Sources: Authors' calculations.

Notes: All of the national totals are from the 1935 published report on the Census of Manufactures. Besides reporting the values for 1935, it also reported the previous years' totals.

Table 3: Summary Statistics of Earnings Variables

	Difference between percentiles...					
	75-25	75-50	50-25	90-10	90-50	50-10
Panel A: Earnings per worker						
1929	0.72	0.26	0.46	1.34	0.47	0.87
1933	0.62	0.33	0.28	1.22	0.69	0.52
1935	0.70	0.27	0.43	1.11	0.44	0.67
Panel B: Blue collar earnings per worker						
1929	0.72	0.24	0.48	1.27	0.42	0.86
1933	0.54	0.27	0.27	1.08	0.58	0.50
1935	0.68	0.24	0.44	1.05	0.40	0.65
Panel C: White collar earnings per worker						
1929	0.39	0.18	0.22	0.83	0.34	0.48
1933	0.38	0.15	0.23	0.86	0.28	0.57
1935	0.41	0.16	0.25	0.82	0.28	0.55

Sources: Authors' calculations.

Notes: The statistics correspond to the difference between the two percentiles listed. Each establishment is weighted by the number of workers in the particular "color" group. All variables are log transformed.

Table 4: Regressions of Earnings per Worker

	Earnings per worker		
Year	1929	1933	1935
	(1)	(2)	(3)
White collar	0.34***	0.72***	0.55***
	(0.02)	(0.02)	(0.02)
Incorporated	-0.00	0.04	0.01
	(0.02)	(0.05)	(0.02)
Multiplant firm	0.01	-0.00	0.02
	(0.02)	(0.02)	(0.02)
Revenue	0.06***	0.07***	0.05***
	(0.00)	(0.01)	(0.01)
Mid-Atlantic	-0.07***	-0.11***	-0.09***
	(0.02)	(0.03)	(0.03)
East North Central	-0.04*	-0.05	-0.10***
	(0.03)	(0.03)	(0.03)
West North Central	-0.11***	-0.19**	-0.15***
	(0.03)	(0.08)	(0.06)
South Atlantic	-0.35***	-0.23***	-0.20***
	(0.02)	(0.02)	(0.02)
East South Central	-0.41***	-0.24***	-0.25***
	(0.03)	(0.03)	(0.03)
West South Central	-0.31***	-0.28***	-0.26***
	(0.03)	(0.04)	(0.03)
Mountain	0.00	-0.07	0.03
	(0.05)	(0.05)	(0.04)
Pacific	-0.07**	0.02	-0.02
	(0.03)	(0.03)	(0.03)
Observations	32141	21520	25201

Sources: Authors' calculations.
Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. In addition to white collar status, the set of controls includes log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects. An observation is an establishment and each establishment is weighted by the number of employees in a particular "color" group. The excluded region is New England. Standard errors are robust.

Table 5: The Extensive Margin and Blue Collar Earnings per Worker

	Blue collar earnings per worker							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exited	-0.01	-0.05*	-0.00	-0.00				
	(0.02)	(0.03)	(0.02)	(0.02)				
Entered					-0.05*	-0.07***	-0.05	-0.07*
					(0.03)	(0.03)	(0.04)	(0.04)
Incorporated	-0.01		0.04		0.04		0.00	
	(0.02)		(0.05)		(0.05)		(0.02)	
Multiplant firm	0.01		0.00		0.00		0.02	
	(0.02)		(0.02)		(0.02)		(0.02)	
Revenue	0.06***		0.07***		0.07***		0.05***	
	(0.01)		(0.01)		(0.01)		(0.01)	
Observations	18591	18591	12213	12213	12213	12213	14460	14460

Sources: Authors' calculations.

Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. All regressions include region and industry fixed effects. Standard errors are robust and the regression is weighted by employment totals. The variable "Entered" is a binary for whether an establishment entered between the current and preceding census. The variable "Exiting" is a binary for whether an establishment *will* exit between the current and next census. Because of this timing convention, we only observe exit status in 1929 and 1933 and entry status in 1933 and 1935.

Table 6: The Extensive Margin and White Collar Earnings per Worker

	White collar earnings per worker							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exited	-0.08**	-0.12***	-0.03	-0.04				
	(0.04)	(0.04)	(0.03)	(0.04)				
Entered					-0.12***	-0.14***	-0.05*	-0.07**
					(0.04)	(0.04)	(0.03)	(0.03)
Incorporated	0.05***		0.17***		0.17***		0.19***	
	(0.02)		(0.04)		(0.04)		(0.02)	
Multiplant firm	-0.00		-0.07**		-0.07*		0.02	
	(0.03)		(0.04)		(0.04)		(0.03)	
Revenue	0.05***		0.08***		0.08***		0.06***	
	(0.01)		(0.01)		(0.01)		(0.01)	
Observations	13540	13540	9304	9304	9304	9304	10740	10740

Sources: Authors' calculations.

Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. All regressions include region and industry fixed effects. Standard errors are robust and the regression is weighted by employment totals. The variable "Entered" is a binary for whether an establishment entered between the current and preceding census. The variable "Exiting" is a binary for whether an establishment *will* exit between the current and next census. Because of this timing convention, we only observe exit status in 1929 and 1933 and entry status in 1933 and 1935.

Table 7: Role of Establishment Exit on the Earnings per Worker Distribution

	Difference between actual and counterfactual...					
	d7525	d7550	d5025	d9010	d9050	d5010
Panel A: Blue collar						
1933	0.06	0.04	0.03	0.06	0.01	0.05
	(0.03)	(0.03)	(0.01)	(0.03)	(0.03)	(0.02)
1935	-0.01	0.00	-0.01	0.01	0.00	0.01
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Panel B: White collar						
1933	0.07	0.02	0.05	0.16	0.03	0.14
	(0.03)	(0.02)	(0.03)	(0.04)	(0.02)	(0.04)
1935	0.04	0.00	0.04	0.13	0.01	0.11
	(0.02)	(0.01)	(0.03)	(0.15)	(0.02)	(0.14)

Sources: Authors' calculations.

Notes: Standard errors are in parenthesis and are based on 50 bootstrap replications. Each establishment is weighted by the number of workers in the particular "color" group. All variables are log transformed. The statistic "d7525" denotes the difference between the 75th and 25th percentiles; "d7550" difference between the 75th and 50th percentiles; "d5025" difference between the 50th and 25th percentiles; "d9010" difference between the 90th and 10th percentiles; "d9050" difference between the 90th and 50th percentiles; and "d5010" difference between the 50th and 10th percentiles.

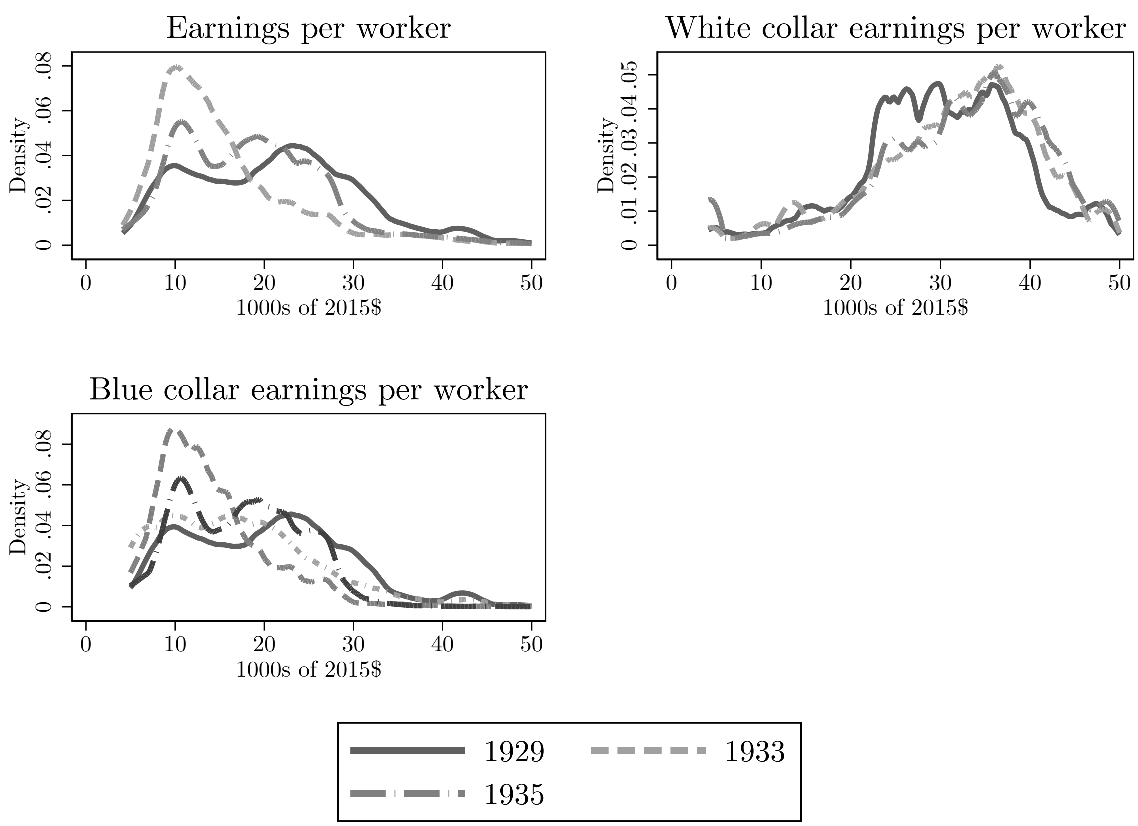
Table 8: Role of Establishment Entry on the Earnings per Worker Distribution

	Difference between actual and counterfactual...					
	d7525	d7550	d5025	d9010	d9050	d5010
Panel A: Blue collar						
1929	0.01	0.00	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
1933	0.01	0.01	0.00	0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Panel B: White collar						
1929	0.02	0.01	0.00	-0.03	0.00	-0.04
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
1935	-0.01	0.01	-0.01	0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)

Sources: Authors' calculations.

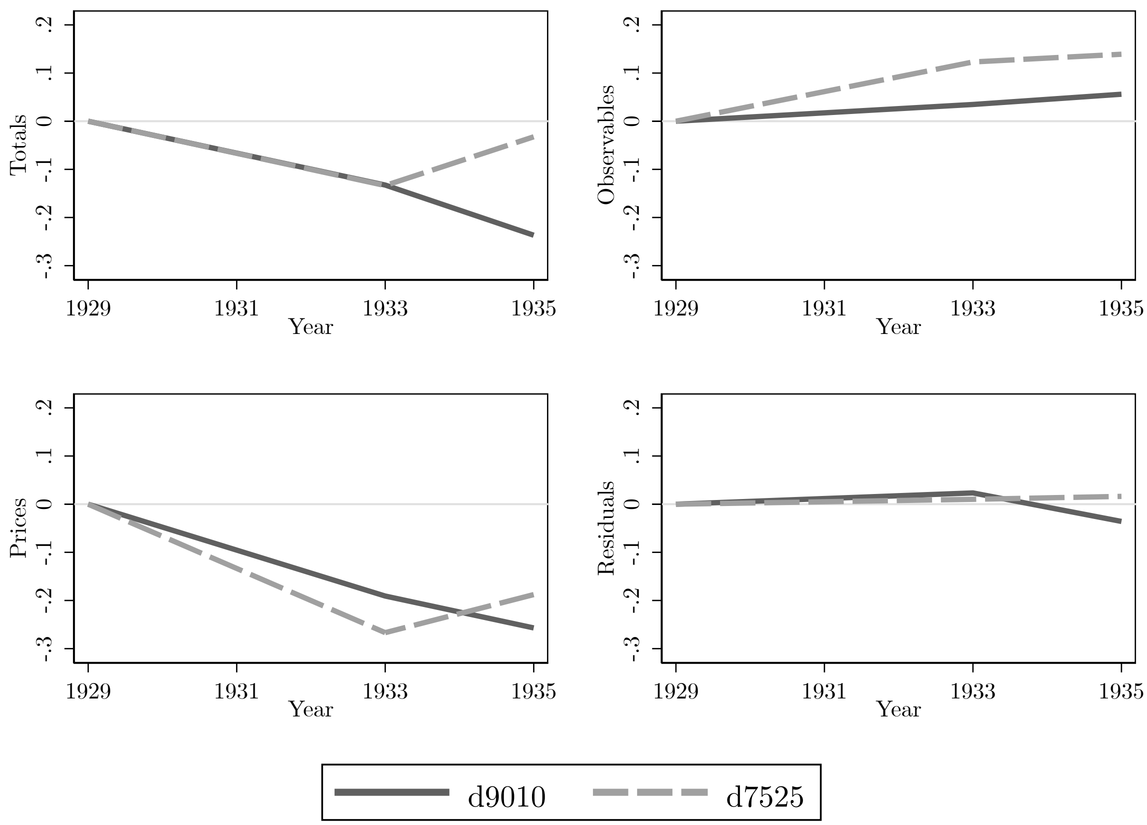
Notes: Standard errors are in parenthesis and are based on 50 bootstrap replications. Each establishment is weighted by the number of workers in the particular "color" group. All variables are log transformed. The statistic "d7525" denotes the difference between the 75th and 25th percentiles; "d7550" difference between the 75th and 50th percentiles; "d5025" difference between the 50th and 25th percentiles; "d9010" difference between the 90th and 10th percentiles; "d9050" difference between the 90th and 50th percentiles; and "d5010" difference between the 50th and 10th percentiles.

Figure 1: Distributions of Earnings per Worker



Sources: Authors' calculations.
Notes: The earnings per worker variable for a particular group of employees is calculated as total earnings of that group divided by total number of workers in that group. We report this variable in thousands of $ 2015. In addition, the 1% tails of the distribution are winsorized. Employment weights are based on the number of employees in a particular occupational group at a given establishment.

Figure 2: JMP Decomposition of Earnings per Worker



Sources: Authors' calculations.
Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. These are based on regressions in Table 4 controlling for log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects as well as white collar status. 1929 is the base year. The statistic "d9010" denotes the difference between the 90th and 10th percentiles. The statistic "d7525" denotes the difference between the 75th and 25th percentiles.

Online Appendix: Labor Earnings Inequality in Manufacturing During the Great Depression

Felipe Benguria
University of Kentucky
fbe225@uky.edu

Chris Vickers
Auburn University
czvickers@gmail.com

Nicolas L. Ziebarth
Auburn University and NBER
nicolas.lehmannziebarth@gmail.com

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1 Representativeness of Our Sample

Our dataset is not a random sample from the Census of Manufactures (COM). Instead, particular industries were selected for particular reasons and not for the purpose of creating a representative sample overall. It is natural to ask to what extent the final sample with which we work is representative of manufacturing as a whole during this period.

Our first set of comparisons examines the average size of establishments in the industries in our sample versus those industries not in our sample. Using the published volumes, this comparison can be done for revenue per establishment as well as blue collar employment and earnings. We can also calculate revenue per blue collar worker as the ratio of total industry revenue to total industry blue collar employment.

Figure 1 plots the difference in means between these two groups of industries for 1929 measured in units of the standard deviation of the variable. For example, the mean employment of industries in our sample is about 0.1 standard deviations smaller than for industries not in our sample. There is basically no difference for revenue and while the differences are larger for revenue and blue collar earnings per worker (though still smaller than 0.4 standard deviations), these differences are not statistically significant. We conclude that along this (admittedly limited) set of variables, our set of industries looks similar to industries not included in our sample.

We next consider whether our sample is drawn from parts of the country with particular economic and demographic characteristics. Figure 2 maps the percent of manufacturing wage earners covered by our sample by county in 1929 compared to the totals reported in the published volumes on the 1929 COM.[1] Figures 3 and 4 redo these maps based on coverage of the number of establishments and blue collar income. The patterns are similar

[1] These data were graciously given to us by Dave Donaldson, Richard Hornbeck, and Jamie Lee. We note

to the figure based on employment. Coverage is high in South Carolina and Texas, for example, and more spotty in the West.

Figure 5 compares some county-level economic and demographic characteristics as a function of the share of total manufacturing establishments covered by our sample. We report the change in units of the standard deviation of the dependent variable of an increase in the percentage covered from the 25th to 75th percentile. So, for example, increasing the share covered from the 25th to 75th percentile is associated with an increase of slightly more than 0.2 standard deviations of the log change in county population between 1920 and 1930. Similarly, the vote share for the Democratic presidential nominee between 1896 and 1928, the illiteracy rate, and the share of African Americans are positively associated with coverage by our sample. Turning to variables related to the severity of the Depression, we find statistically significant effects on the share of banks that fail between 1929 and 1933, one measure of the local severity of the Great Depression. On the other hand, the effect on the log change in retail sales, another measure local severity, is not statistically significant and the economic magnitude is very small. Figure 6 shows that these quantitative differences are much smaller if we calculate the percentage of manufacturing covered using revenue instead of number of establishments. Overall, we would argue that the demographic differences are reflective of the fact that our sample covers up a large fraction of manufacturing in the South. While this does translate into some demographic differences, on the balance, we do not think our sample is drawn from areas that had particularly harsh (or easy) experiences during the Depression.

2 Quality of the Datasource

We address two questions related to the quality of the underlying data.[2] The first question is whether the establishments in our sample represent a complete canvassing of all manufacturing establishments within the industries we consider. There are a number of reasons why establishments could be missing. One possibility is that for some reason, the Census was not able to enumerate all establishments in some given year. It is difficult to answer this question since it requires access to some external data to validate the Census enumeration. For the cement industry, Chicu, Vickers, and Ziebarth (2013) use industry trade publications that listed all establishments in operating at this time and found that the Census was successful in enumerating all such establishments. Hansen and Ziebarth (2017) compare the Census records from Mississippi to records from Dun & Bradstreet and find close agreement between the two as well. It is also possible that some records were lost after being tabulated. For example, Ziebarth (2015) highlights one case where it does appear some schedules were lost for the manufactured ice industry in Texas between being tabulated and transferred to the National Archives for storage. In general, this worry is easy to address by comparing total

that the published volumes did not necessarily report the totals for each county for each census year. So just under 10% of our sample does not have a corresponding match in the published volumes for 1929. This might be due to confidentiality reasons on the part of the Census Bureau or just that it was simply was not tabulated.

[2] We note that the papers by Ziebarth (2015) and Vickers and Ziebarth (2019) discuss in detail a number of checks on the quality of this datasource.

derived from our samples with those in the published volumes.

Underenumeration or the loss of schedules are very extreme forms of measurement error. Even assuming these are not major worries, there is a question about whether the information reported on the forms is accurate. These values are self-reported and, as far as we know, not checked in any way against some administrative data source. Like the problem of underenumeration, it is difficult to answer this question since it requires some (trusted) external data source to validate the Census records. One industry where such an external data source does exist is the cement industry. Chicu, Vickers, and Ziebarth (2013) take advantage of a court case that required cement establishments to report their production totals for a number of years that overlap with the Census. They find that production totals reported by the establishments on the schedules are quite close to those reported by the establishments to the court.

In a slightly different direction, Vickers and Ziebarth (2019) study whether edits made by the Census Bureau to the forms are non-random. Many of the original schedules exhibit markings that are clearly edits of what the establishments originally filled in. These markings in some cases fix obvious mathematical mistakes on the part of the establishment. For example, some of the fields on the schedules are supposed to be the sum of two other fields and, in some cases, it is clear that the Census Bureau corrected a mistake by the establishment in summing up the components. In other cases, it is harder to understand what the mistake was in the first place since the edit simply involves crossing out one number and writing in a new one with no explanation for the change. One potential problem is if these edits were more likely to be made as, say, a function of an establishment's size, which would generate heteroskedastic measurement error. In the end, Vickers and Ziebarth (2019) find no evidence that edits were more likely to occur for large establishments, and, conditional on an edit being made, the probability of an upward revision is no different for large or small establishments.

We conduct a new check on the quality of the data by examining the rates of rounding ("value heaping") over time.[3] The idea is similar to the analysis of age heaping in historical demographic data. If values of variables are too frequently rounded, this suggests a lack of effort on the part of the person filling out the schedule and a higher chance of measurement error. To operationalize this idea, we define a rounded value of a variable as one that ends in 0 or 5. Bedford's Law, an empirical regularity on the distribution of digits, predicts that the distribution of digits after the first is close to uniform. Figure 7 shows the percentages of rounded values for white and blue collar total earnings as well as total employees and revenue in 1929, 1933, and 1935. There are differences in this percentage across the variables, and, for 3 out of the 4 variables, there is a higher frequency of rounded values than predicted by Bedford's Law. This is *prima facie* evidence for "value heaping" and perhaps non-classical measurement error. However, these percentages are quite stable over time. So by focusing on differences over time, this form of measurement error does not contaminate the estimation of trends.

[3] We thank the editor for suggesting this.

3 Educational Attainment by Worker Skill

We interpret the distinction between salaried workers and wage earners as reflecting differences in educational attainment, a common measure of skill in the literature. To provide supporting evidence for this interpretation, we turn to the 1% sample from the 1940 Population Census. While the COM did not collect information on the educational attainment of workers, the occupational description and industrial affiliation of these white collar and blue collar worker categories can be matched to those in the Census of Population to obtain information on the characteristics of workers in these different types of jobs. In particular, IPUMS has coded occupations into 3 digit categories based on the 1950 occupational classification. We consider occupational codes 1 through 300 as white collar jobs and those with codes 301 to 496 as blue collar.

Pooling together workers in all manufacturing industries and in all states, we find that in 1940 only 17.2 percent of blue collar workers had years of schooling corresponding to a high school degree or more. In contrast, 61.6 percent of white collar workers had at least a high school degree. The median white collar worker has an educational attainment of 12 years of school (exactly a high-school degree) while the median blue collar worker had 8 years of schooling.[4] We put this into a regression framework to predict educational attainment based on the white versus blue collar distinction. We consider two definitions of educational attainment: (1) high school graduate and (2) some college. We also consider two specifications: (1) with no controls and (2) with controls for sex, race, industry and state. Table 1 shows that there is a statistically and economically significant relationship education and white collar employment. While not as good as directly observing educational attainment of a worker, this shows that the "collar color" of a worker's job carries considerable information about his educational attainment.

4 JMP Decomposition: Robustness Checks

4.1 The Choice of Base Year

The results of the JMP decomposition are potentially sensitive to the choice of the base or reference year. In the paper, we use the "first" year as the base year, meaning 1929. For robustness, we show the results from using the "last" year, meaning 1935, as the reference year.[5] This also means we normalize values to 0 in 1935 instead of 1929 and so the figures are "shifted up" relative to the those in the paper. Figure 10 shows the decomposition with this reference year. Overall, the qualitative results are not much changed relative to the results reported in the main body of the text. It is the case when using 1935 as a base year versus 1929 that residuals play a more important role in explaining changes in the 90-10 differential. This comes at the "expense" of a smaller role played by prices.

[4]Note that the Census only asked about years of schooling not educational attainment directly. We interpret 12 years of schooling or above as someone with a high school degree though this need not necessarily be the case.

[5]We could also, in principle, use the average of the coefficients over the three years as the base "year" or 1933.

4.2 Using Employment as the Measure of Establishment Size

In the text, we used the total revenue as our measure of establishment size. Figure 11 shows the results of the JMP model using total employment instead as our measure of establishment size. This is consistent with the approach taken by Atack, Bateman, and Margo (2004). The differences between the results using this measure of size and those in the text are not meaningful.

4.3 Winsorizing the Earnings Variables

In the main body of the text, we winsorized the earnings variables to mitigate the effect of outliers on the regression results. Here we consider the effects of this choice by reporting the results of the JMP regression without winsorizing the earnings variables. Figure 12 shows the results using unwinsorized earnings variables. The results are nearly identical to those using the winsorized earnings variables.

5 JMP Decomposition: Extensions

5.1 Durables vs. Nondurables

One interesting margin along which to split the sample is by whether the major product of the industry produces a durable good or not. Figures 8 and 9 plot the distributions of earnings for blue and white collar employees, respectively, separating industries that produce a durable good from those that do not. Only for blue collar workers in durable industries do we observe clear shifts in the earnings distribution over these years.[6]

Because the sharpest changes in the distribution of earnings were for blue collar workers, and particularly in the durable goods industries, we perform the same JMP decomposition for blue collar workers only, separately by nondurable and durable industries. Tables 2 and 3 show the regression results for nondurable and durable industries, respectively. The striking differences are in the regional effects with larger penalties for being in the South and smaller ones for other parts of the country (relative to New England) in non-durable goods industries and the reverse for durable goods industries. In non-durable goods industries, the coefficients on low income areas all increase from 1929 to 1933, after which there is a smaller further increase. That is, there was regional convergence in these industries. In durable goods industries, by contrast, there is a decline in the coefficients on low-income regions from 1929 to 1933, but some reversal of this trend in two of the three regions, with the other constant.

Turning to the decomposition, we plot the results for non-durable goods industries and durables in Figures 13 and 14 respectively. The 90-10 difference is flat for non-durables, while the 75-25 difference declines from 1929 and then is stable to 1935. The decomposition suggests a complicated pattern for inequality: Prices are associated with an increase in the

[6]This finding casts doubt on one interpretation of all these results that the changes in the overall earnings distribution are simply driven changes in measurement due to some quirk in how the 1933 COM was collected. That theory could not explain why the differences in 1933 are only there for blue collar workers in durable goods industries.

90-10 difference and a decrease in the 75-25 difference. In contrast, residuals are associated with a slight decline in inequality by 1935. Turning to durables, the prices are associated with declines both from 1929 to 1933 and from 1933 to 1935. In contrast, residuals are associated with an increase in inequality measures, particularly the 90-10 difference, before returning to roughly the same level as in 1929 by 1935.

5.2 Earnings per Hour

For blue collar workers, it is possible to infer hourly earnings using questions on the "typical" number of hours worked per week (assuming a constant number of weeks worked). An issue is that the wording of these questions regarding hours are quite heterogeneous from year to year. They include questions on the number of shifts, number of hours in operation, and the "normal" number of hours worked in a week. Even the wording of the workweek question changes from year to year, subtly affecting what is actually being reported. In 1929, the Census asks establishments to report the "normal number of hours for the individual wage earner" with the figures based on "practice followed during the year." In 1933, the question instead asks for the "number of hours the plant was operated (day shift only) during the week including December 15."[7] That is, the question asked about one particular week rather than the normal practice, as well as asking for the amount the establishment was operated for the day shift as opposed to the length of the workweek per se. The 1935 form asks for the "normal number of hours per week (day shift only)", though the context of the form suggests it is referring to establishment operating hours rather than individual workweeks. Because of these differences in the questions across years, we only calculate earnings per hour in 1929 and 1935 for blue collar workers. To do this, we assume that all workers within a given year work the same number of weeks. In this case, once we take the log transformation, this constant will be irrelevant for measuring inequality. It would matter obviously if we were interested in the mean of earnings per hour, but that is not our focus.

We now turn to examining the changes in the distribution of earnings per hour, as opposed to annual earnings per worker. Panel A of Table 4 reports the changes in the distribution of blue collar earnings per hour for 1929 and 1935, the only years where we have information on hours worked. We find that this distribution became more compressed over this period, for both the 75-25 and 90-10 differentials. The changes were larger in magnitude than those observed for the per worker earnings distribution. Such a disconnect could be due to either an increase in the dispersion of hours or an increase in the covariance between hours and earnings per worker. Panel B reports the changes in the distribution of hours per wage earner in 1929 and 1935. In 1935, the interquartile range is zero: Both the 75th percentile and 25th percentile workweeks are 40 hours long. In 1929, there is more dispersion as measured by the interquartile range. However, when the broader distribution is considered, the 90-10, 90-50, and 50-10 gaps are all unchanged from 1929 to 1935. Changes in the dispersion of the workweek seem unlikely to explain the disconnect between the income and wage measures. As we show below, the change in hours per worker was not uniform across the country: The workweek declined in the low wage South more than in other regions. This larger decline

[7] This number is not representative of the year as a whole. For manufacturing, the average workweek in 1933 was 36.4, but only 33.8 in December (Beney, 1936, Table II). For this reason, we exclude 1933 when we focus on hourly wages.

in hours for the South offsets the relative gain in hourly wages for this area, so the overall effects on the earnings distribution of these regional changes are somewhat muted.

Table 5 shows the regression results of specifications identical to the ones we estimated for earnings. Notable here are the changes in the regional differences in earnings per hour. In particular, earnings per hour in the South Atlantic and East South Central both increase by 29 log points while the West South Central region increases by 25 log points. This generates substantial degree of regional convergence, more so than in annual earnings, with regions all across the country catching up with New England. As with annual earnings, larger establishments are associated with higher earnings per hour.

One way to reconcile the larger degree of regional convergence in earnings per hour compared to that in earning per worker is if the hours worked changed differentially across geographic regions. Table 6 shows the results of regressing the length of the log workweek on the same set of variables as above. It is clear that regional convergence in *workweeks* is crucial in explaining the disconnect between the results for earnings per worker compared to those for earnings per hour. Note that in 1929, the South Atlantic, East South Central, and West South Central regions, conditional on industry and observable firm characteristics, had workweeks 6 to 10 log points longer than those in New England. By 1935, two of these three regions had essentially no difference in workweeks, and the South Atlantic had workweeks 4 log points shorter than that in New England.

One interpretation of these results ccomes from the paper by Bernanke (1986), which develops a model based on that in the paper by Lucas (1970). At the heart of the model is an assumption that there are "economies or diseconomies of 'bundling' of worker-hours." (pg. 219) So declines in hours worked driven by declines in labor demand unambiguously reduce the *marginal wage* but the effect on the *average wage*, which is defined as total earnings divided by total hours, is unclear. As Bernanke writes on pg. 217, "The rate at which firms can reduce hourly earnings as the workweek falls depends on workers' preferences and reservation utilities. Especially at low levels of work and earnings, when consumption is highly valued relative to leisure, it may not be possible to cut hourly earnings as sharply as hours... Thus the wage, in other words, hourly earnings, may rise even as labor demand and workweeks fall." Bernanke quoting on pg. 217 from earlier work by Daugherty, Chazeau, and Stratton (1937) reports on the experience of the iron and steel industry during the 1930s: "[F]irms' desires to maintain their work forces relatively intact led them to adopt 'staggered' or 'spread-work' schedules under which many workers worked only a few days a week... Firms must have recognized that their ability to keep cutting total earnings as the workweek shortened was limited, since if workers could not attain a subsistence level in the mill town they would be forced to try elsewhere. Thus real hourly earnings in iron and steel rose... as the workweek was cut." So as demand falls, the remaining employees have their hours reduced to this practical minimum and, therefore, the earnings distribution becomes more compressed.

6 Extensive Margin Analysis: Robustness Checks

Here we consider some robustness checks for our extensive margin analysis.

6.1 Counterfactual Exit and Entry Distributions

In the text, we only reported the differences in the measures of inequality between the counterfactual and actual distributions. For completeness, Figure 17 computes the counterfactual blue and white collar earnings distributions that includes the distribution of earnings of incumbent establishments and adds back in predicted earnings in 1933 for all establishments that exited between 1929 and 1933. Figure 18 does the same calculation for 1935. Figures 19 and 20 show the entry counterfactual distributions for blue and white collar workers in 1933 and 1935, respectively.

6.2 State and Industry Fixed Effects Only

In the text, we imputed earnings and employment based on observables in the preceding census. The idea was to regress earnings (and employment) growth rates for continuing establishments on observable characteristics and then use the predicted values based on exiting establishments observables. The assumption underlying such a method was that exits were due completely to random events once we condition on these observables. Here we consider a more limited set of observables: just state and industry fixed effects. Figure 21 shows the results for this approach. The difference between these results and those in the text are negligible. For brevity, we just plot these distributions for 1933. Similar results hold for 1935.

6.3 Employment Adjustment in Entry Counterfactual

In the text of the paper, we constructed a counterfactual distribution that removed the earnings penalty associated with entry. We did not make any adjustments to the employment of entrants to adjust for potential penalties there. Here we make those adjustments by running regressions for employment on the same set of controls as earnings. For brevity, we just plot these distributions for 1933. Similar results hold for 1935. We then construct counterfactual levels of employment for entrants and use these as weights in estimating the counterfactual density. Figure 22 shows this counterfactual density for 1933. This additional adjustment does not make a substantial difference.

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Table 1: Educational Attainment by White Collar Status

	(1)	(2)	(3)	(4)
	High school graduate		Some college	
White collar	0.44***	0.41***	0.21***	0.20***
	(0.01)	(0.01)	(0.00)	(0.00)
Controls	No	Yes	No	Yes
Observations	30056	30056	30056	30056

Notes: These data are from the 1% sample of the 1940 Census of Population. Estimates are based on a linear probability model restricting to individuals employed in manufacturing industries and between ages 20 and 65. The set of controls includes sex, race, age, and age squared as well as state and industry fixed effects. Standard errors are robust.

Table 2: Regressions of Blue Collar Earnings per Worker: Non-durable Goods Industries

	Blue collar earnings per worker		
	(1)	(2)	(3)
	1929	1933	1935
Incorporated	-0.01	0.10**	-0.02
	(0.02)	(0.05)	(0.02)
Multiplant firm	0.00	-0.02	0.02
	(0.01)	(0.01)	(0.01)
Revenue	0.06***	0.05***	0.06***
	(0.01)	(0.01)	(0.01)
Mid-Atlantic	0.04	0.04	0.02
	(0.03)	(0.04)	(0.03)
East North Central	-0.02	-0.07*	-0.02
	(0.03)	(0.03)	(0.03)
West North Central	-0.02	-0.09**	-0.03
	(0.03)	(0.04)	(0.03)
South Atlantic	-0.39***	-0.26***	-0.21***
	(0.02)	(0.02)	(0.02)
East South Central	-0.43***	-0.23***	-0.23***
	(0.03)	(0.03)	(0.03)
West South Central	-0.27***	-0.26***	-0.20***
	(0.05)	(0.04)	(0.03)
Mountain	0.10*	0.07	0.07*
	(0.06)	(0.07)	(0.04)
Pacific	0.03	0.07*	0.08***
	(0.06)	(0.04)	(0.03)
Observations	11322	8607	9713
Year	1929	1933	1935

Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. The set of controls includes log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects. An observation is an establishment and each establishment is weighted by its number of blue collar employees. The excluded region is New England. Standard errors are robust.

Table 3: Regressions of Blue Collar Earnings per Worker: Durable Goods Industries

	Blue collar earnings per worker		
	(1)	(2)	(3)
	1929	1933	1935
Incorporated	0.01	-0.09**	0.07***
	(0.03)	(0.04)	(0.03)
Multiplant firm	0.02	0.02	0.02
	(0.03)	(0.04)	(0.03)
Revenue	0.05***	0.09***	0.05***
	(0.01)	(0.01)	(0.01)
Mid-Atlantic	-0.05	-0.18**	-0.17**
	(0.05)	(0.07)	(0.07)
East North Central	0.02	-0.08	-0.16**
	(0.05)	(0.07)	(0.07)
West North Central	-0.06	-0.28*	-0.22**
	(0.05)	(0.15)	(0.10)
South Atlantic	-0.20***	-0.22***	-0.21***
	(0.06)	(0.08)	(0.07)
East South Central	-0.38***	-0.32***	-0.33***
	(0.07)	(0.09)	(0.09)
West South Central	-0.31***	-0.24***	-0.31***
	(0.07)	(0.09)	(0.09)
Mountain	0.03	-0.11	-0.01
	(0.08)	(0.09)	(0.09)
Pacific	-0.04	0.00	-0.05
	(0.06)	(0.07)	(0.07)
Observations	7269	3606	4747
Year	1929	1933	1935

Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. The set of controls includes log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects. An observation is an establishment and each establishment is weighted by its number of blue collar employees. The excluded region is New England. Standard errors are robust.

Table 4: Summary Statistics of Earnings per Hours and Workweek

	Difference between percentiles...					
	75-25	75-50	50-25	90-10	90-50	50-10
Panel A: Blue collar earnings per hour						
1929	0.77	0.28	0.49	1.43	0.50	0.93
1935	0.68	0.27	0.41	1.10	0.47	0.63
Panel B: Blue collar hours per week						
1929	0.14	0.10	0.04	0.29	0.18	0.11
1935	0.00	0.00	0.00	0.29	0.18	0.11

Notes: The statistics correspond to the difference between the two percentiles listed. Each establishment is weighted by the number of workers in the particular "color" group. All variables are log transformed. We do not have information on blue collar hours per week in 1933 because the variable on the COM schedule for that year is not consistent with the other years. For earnings per hour, we assume that all workers in a given year work the same number of weeks. This constant is differenced out when we take the log transformation.

Table 5: Regressions of Blue Collar Earnings per Hour

	Blue collar earnings per hour	
	(1)	(2)
	1929	1935
Incorporated	-0.01	0.03
	(0.02)	(0.02)
Multiplant firm	0.01	-0.00
	(0.02)	(0.02)
Revenue	0.06***	0.04***
	(0.01)	(0.01)
Mid-Atlantic	-0.11***	-0.05
	(0.03)	(0.04)
East North Central	-0.08**	-0.06
	(0.03)	(0.04)
West North Central	-0.14***	-0.15**
	(0.03)	(0.08)
South Atlantic	-0.44***	-0.15***
	(0.02)	(0.03)
East South Central	-0.54***	-0.25***
	(0.04)	(0.04)
West South Central	-0.40***	-0.25***
	(0.04)	(0.06)
Mountain	0.03	-0.01
	(0.06)	(0.06)
Pacific	-0.02	-0.01
	(0.04)	(0.04)
Observations	18563	14059
Year	1929	1935

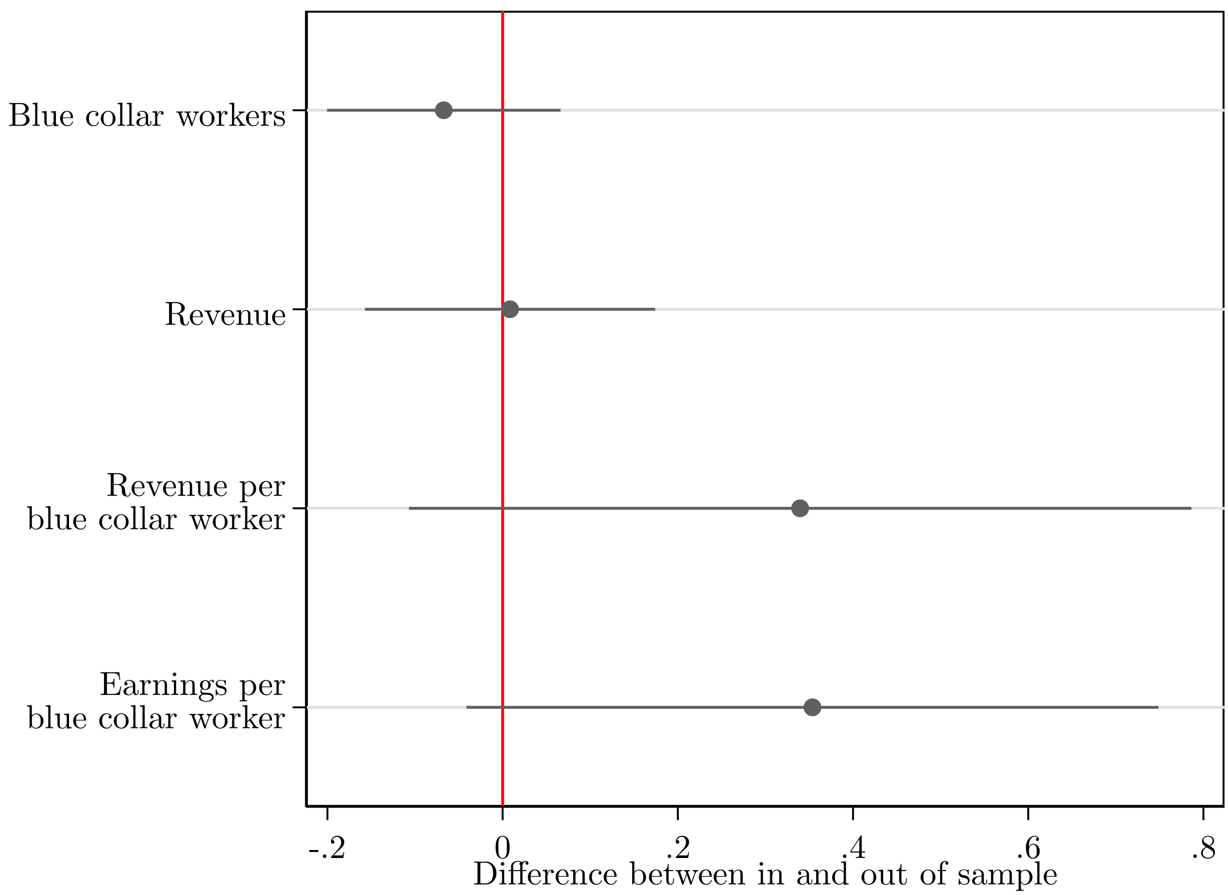
Notes: The earnings per week variable is log transformed and the 1% tails are winsorized. The set of controls includes log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects. An observation is an establishment and each establishment is weighted by its number of blue collar employees. The excluded region is New England. Standard errors are robust. The workweek variable is only reported in 1929 and 1935 so we can only calculate this variable in those two years. We assume that all workers work the same number of weeks in a given year. Once we apply the log transformation, this year-specific constant will be absorbed by the year fixed effects.

Table 6: Regressions of Blue Collar Hours per Week

	Blue collar hours per week	
	(1)	(2)
	1929	1935
Incorporated	0.01	-0.02
	(0.01)	(0.01)
Multiplant firm	-0.00	0.02
	(0.01)	(0.01)
Revenue	-0.00	0.01**
	(0.00)	(0.00)
Mid-Atlantic	0.03***	-0.03
	(0.01)	(0.02)
East North Central	0.03**	-0.03
	(0.01)	(0.02)
West North Central	0.03**	0.01
	(0.02)	(0.02)
South Atlantic	0.08***	-0.04***
	(0.01)	(0.02)
East South Central	0.10***	-0.00
	(0.02)	(0.02)
West South Central	0.06***	0.01
	(0.02)	(0.04)
Mountain	-0.02	0.05
	(0.02)	(0.05)
Pacific	-0.05***	0.03
	(0.01)	(0.02)
Observations	18775	14065
Year	1929	1935

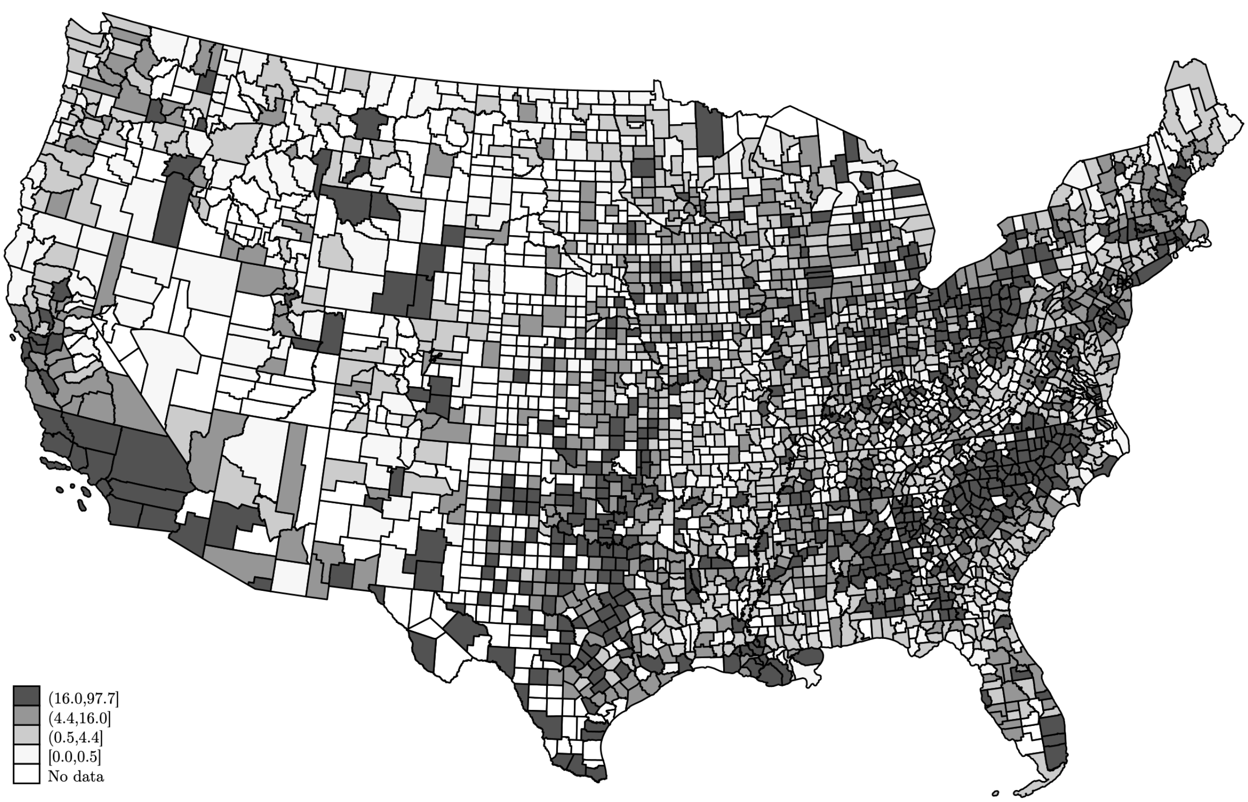
Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. The set of controls includes log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects. An observation is an establishment and each establishment is weighted by its number of blue collar employees. The excluded region is New England. Standard errors are robust. This variable is only reported in 1929 and 1935.

Figure 1: Comparing Industries in Our Sample to Those That Are Not



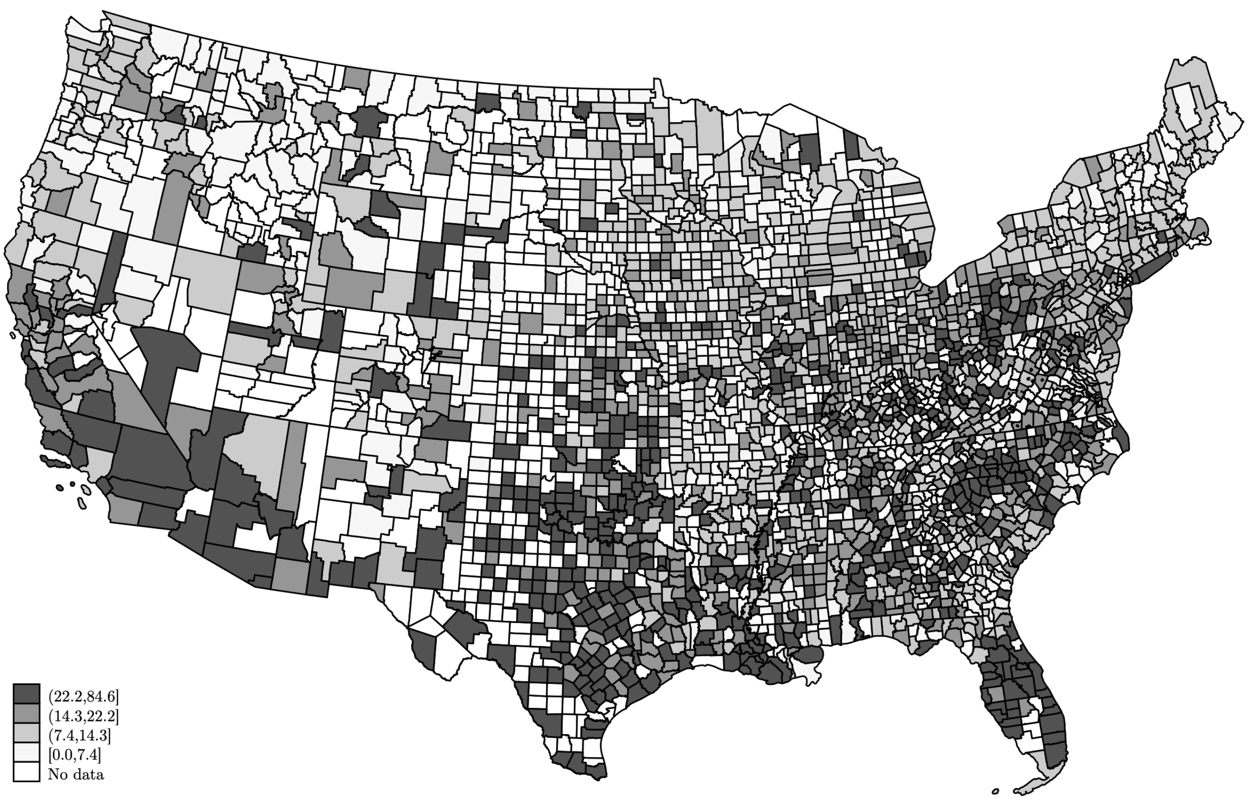
Notes: All variables are log transformed. We report the mean differences in 1929 industry-level averages measured in units of the standard deviations of the dependent variables. 95% confidence intervals of the difference based on robust standard errors are reported.

Figure 2: Geographic Coverage of Our Sample: Wage Earners



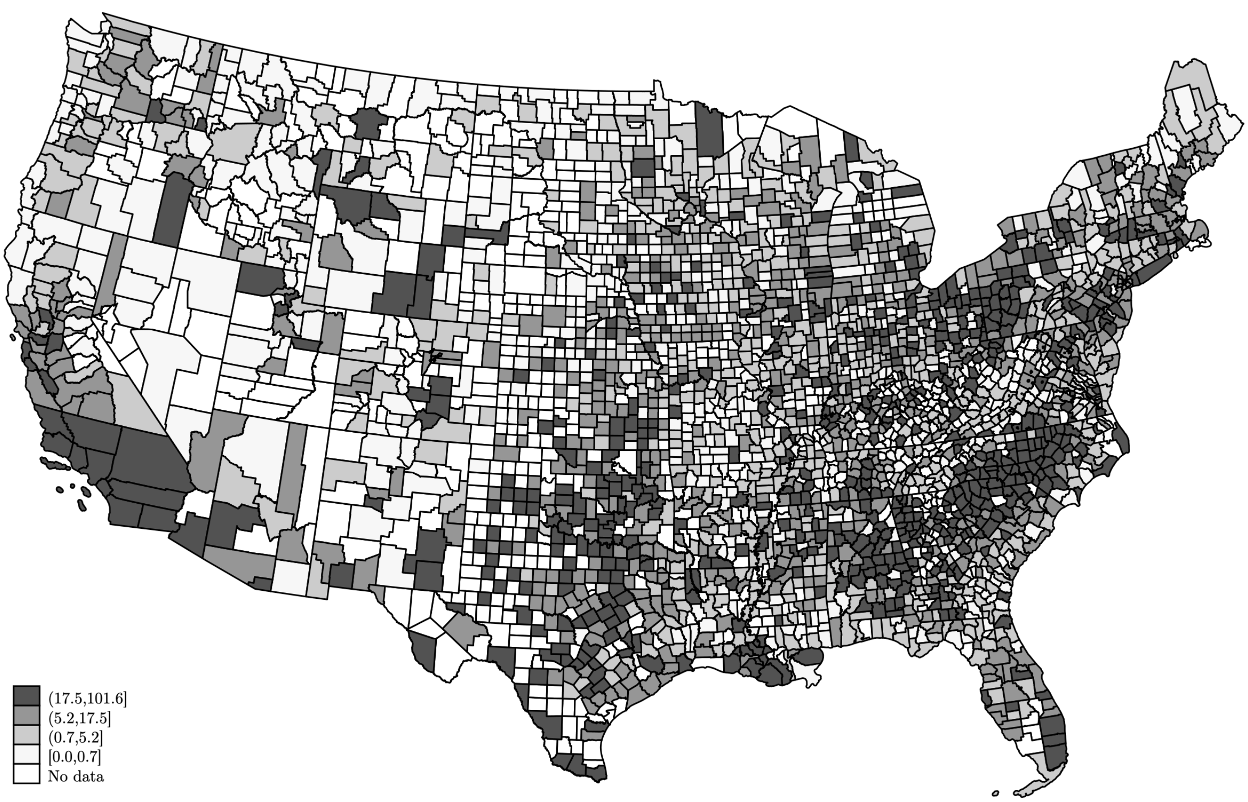
Notes: The percentage of wage earners is the total number of wage earners in our sample in 1929 relative to the total number of manufacturing wage earners reported in the published volume. "No data" means that a county's value was not reported in the published volume for 1929.

Figure 3: Geographic Coverage of Our Sample: Establishments



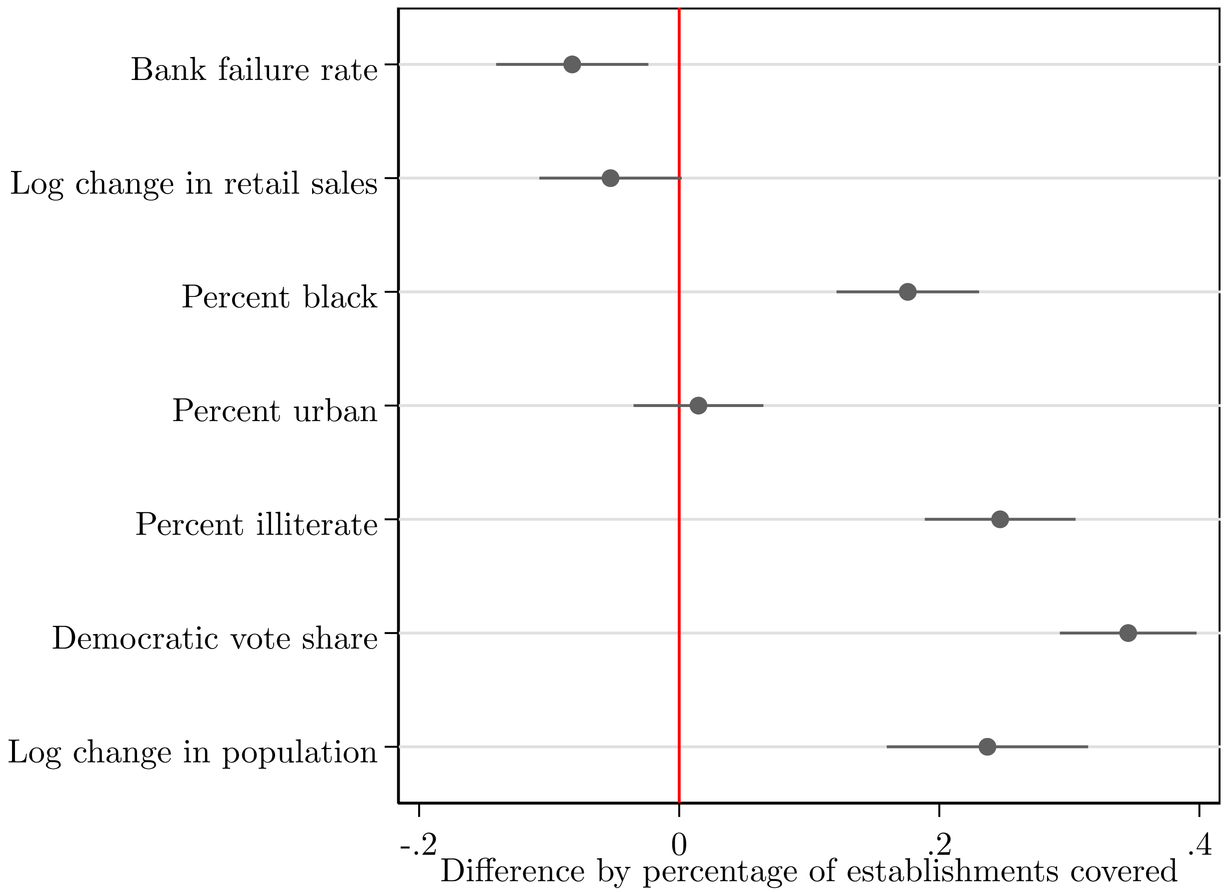
Notes: The percentage of establishments is the total number of establishments in our sample in 1929 relative to the total number of manufacturing establishments reported in the published volume. "No data" means that a county's value was not reported in the published volume for 1929.

Figure 4: Geographic Coverage of Our Sample: Blue Collar Earnings



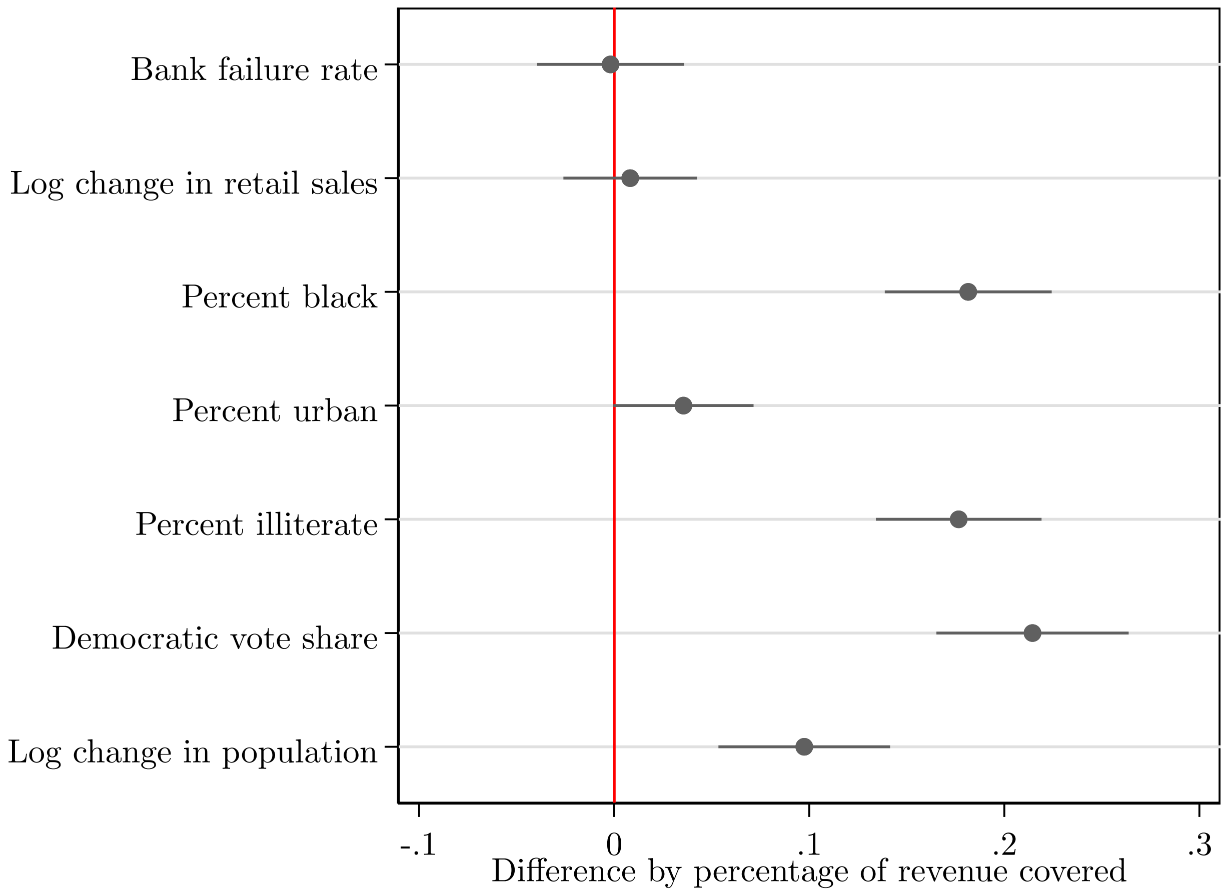
Notes: The percentage of blue collar income is the total blue collar income in our sample in 1929 relative to the total blue collar income reported in the published volume. "No data" means that a county's value was not reported in the published volume for 1929.

Figure 5: Comparing County Characteristics by Sample Coverage



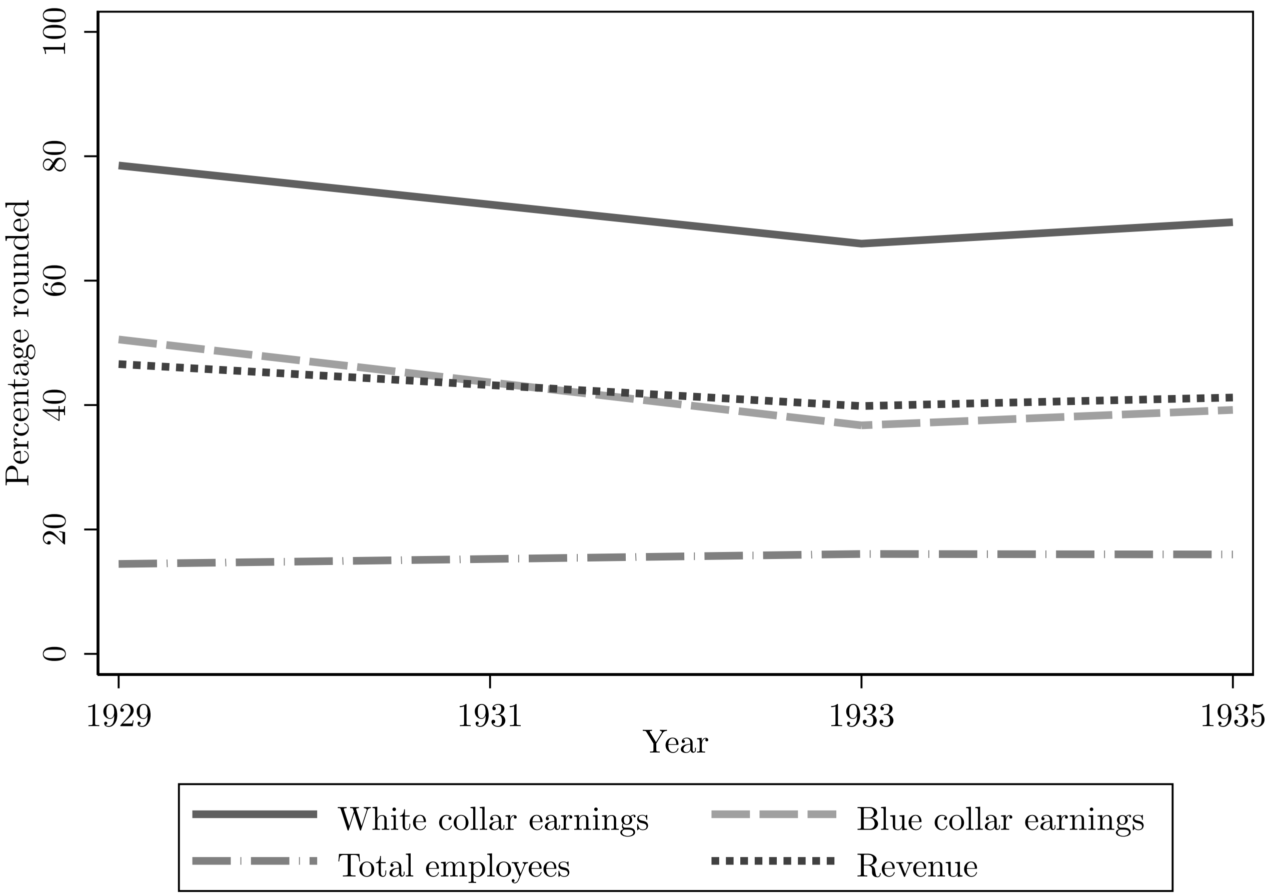
Notes: We report the coefficient on the percentage of establishments covered by our sample in a county scaled by the ratio of the 75th minus 25th percentile of this percentage. We report the effect in units of the standard deviation of the dependent variable. 95% confidence intervals based robust standard errors are reported. The bank failure rate is the share of ratio of the number of banks that fail between 1929 and 1932 relative to the number that exist in 1929. The log change in retail sales is the change between the years of 1929 and 1933 (Neumann, Fishback, and Kantor, 2010). Percent black, urban, and illiterate are all from the 1930 Population Census. The Democratic vote share is the average share for the Democratic candidate in Presidential elections between 1896 and 1928. The log change in population is between the years of 1920 and 1930.

Figure 6: Comparing County Characteristics by Sample Coverage: Revenue



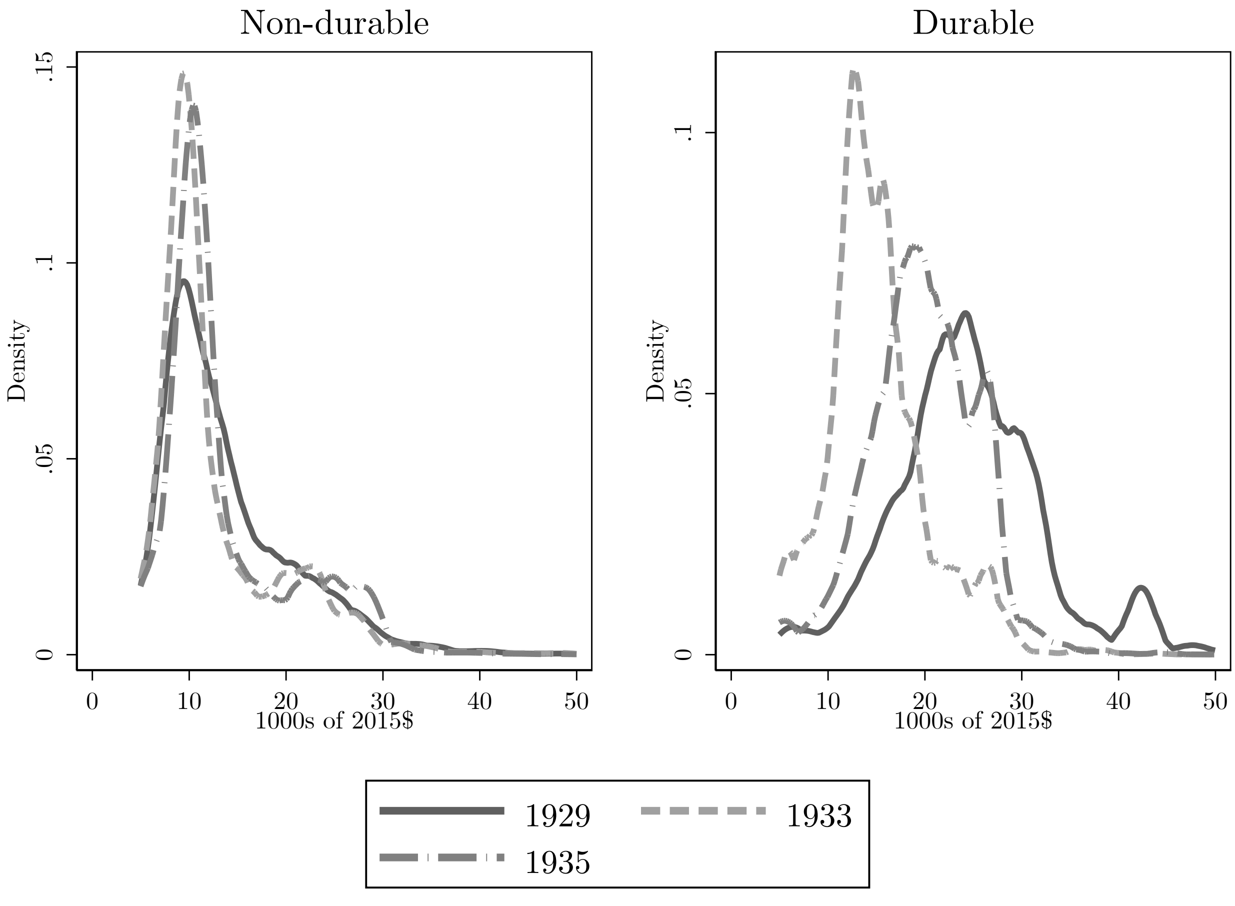
Notes: We report the coefficient on the percentage of total revenue covered by our sample in a county scaled by the ratio of the 75th minus 25th percentile of this percentage. We report the effect in units of the standard deviation of the dependent variable. 95% confidence intervals based robust standard errors are reported. The bank failure rate is the share of ratio of the number of banks that fail between 1929 and 1932 relative to the number that exist in 1929. The log change in retail sales is the change between the years of 1929 and 1933 (Neumann, Fishback, and Kantor, 2010). Percent black, urban, and illiterate are all from the 1930 Population Census. The Democratic vote share is the average share for the Democratic candidate in Presidential elections between 1896 and 1928. The log change in population is between the years of 1920 and 1930.

Figure 7: Percentage of Rounded Values



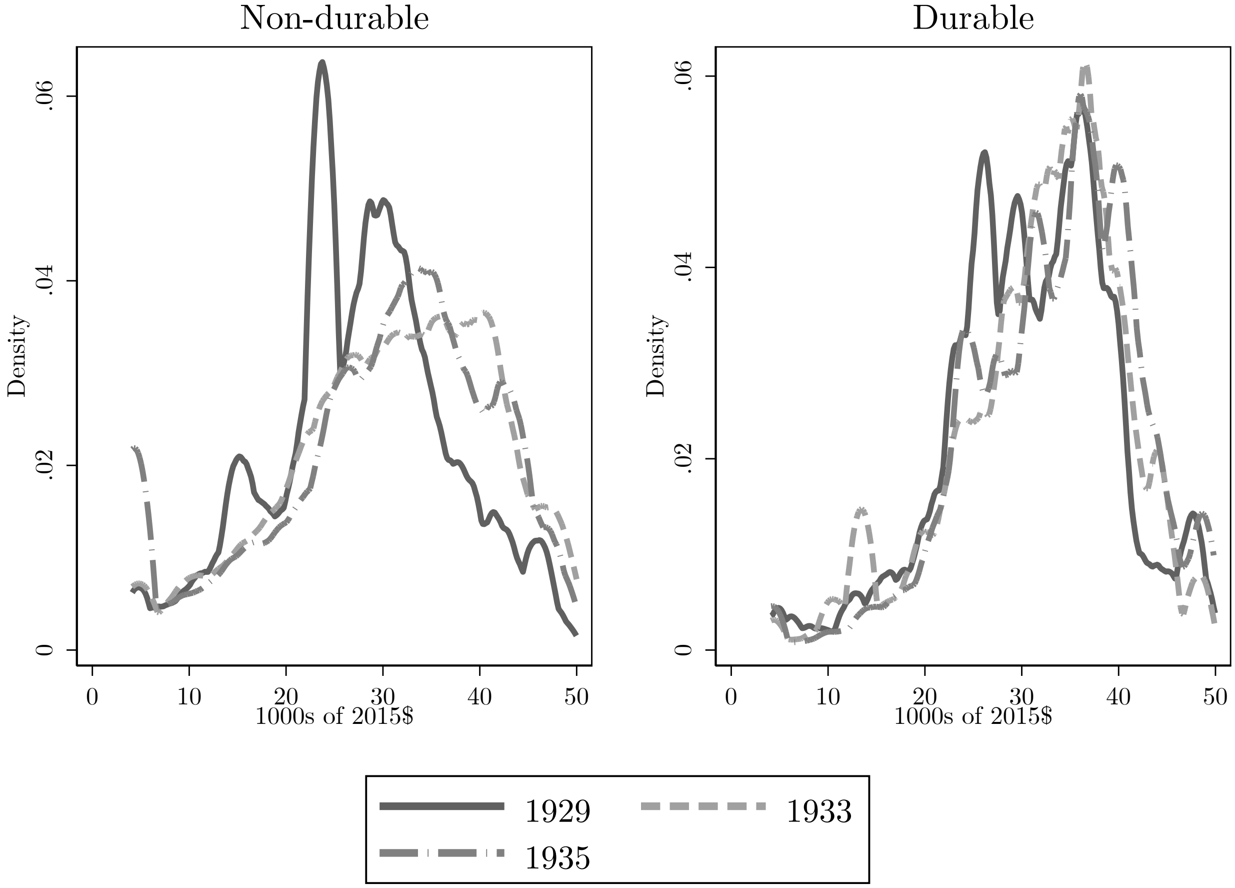
Notes: We define the value of a particular variable (white collar earnings, blue collar earnings, total employees, and revenue) as "rounded" if it ends in a 0 or 5. In calculating the percentage, we equally weight each establishment. Total employees is the sum of blue and white collar workers.

Figure 8: Distribution of Blue Collar Earnings per Worker by Durability of Product



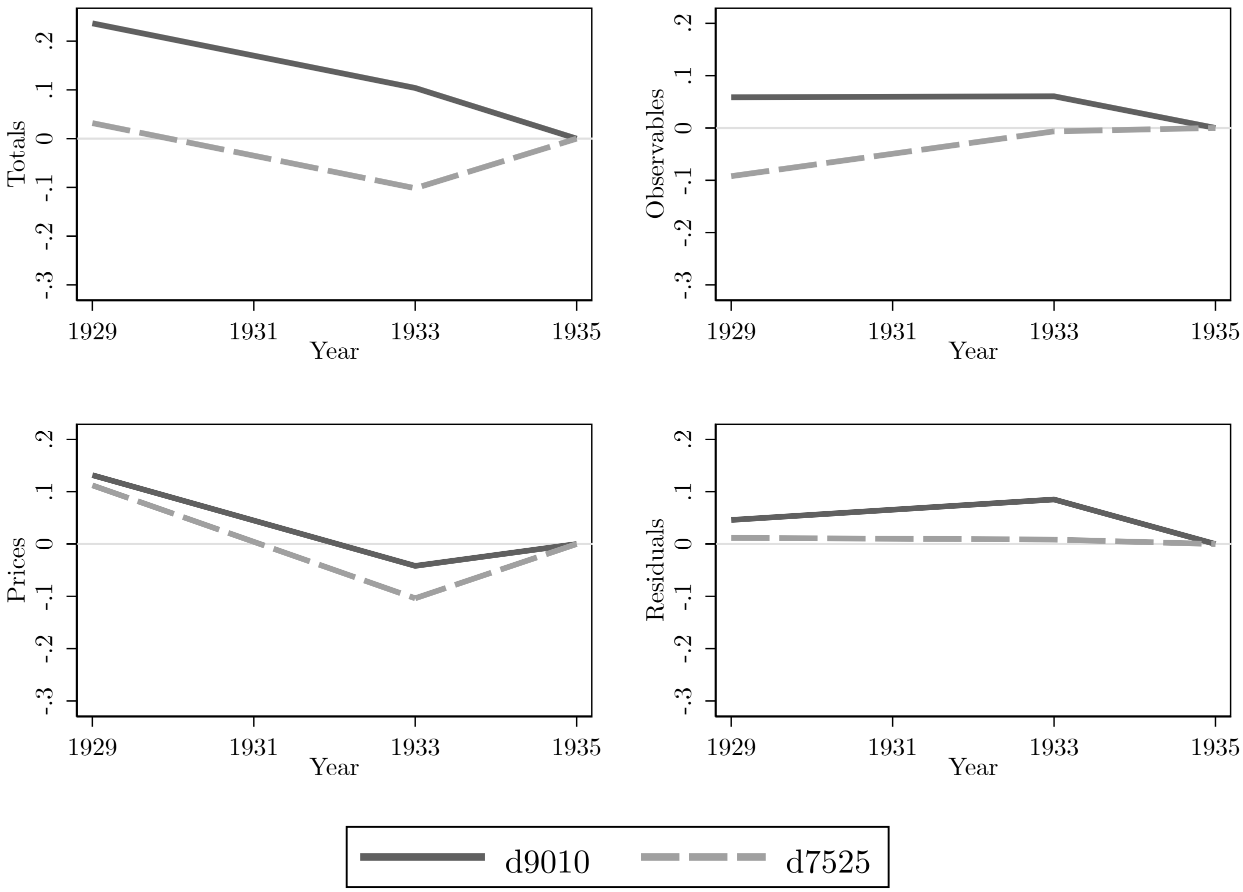
Notes: The earnings per worker variable for a particular group of employees is calculated as total earnings of that group divided by total number of workers in that group. The 1% tails of the distribution are winsorized. We report this variable in thousands of $ 2015. Durability is defined based on the industry's product. Employment weights are based on the number of blue collar employees at a given establishment.

Figure 9: Distribution of White Collar Earnings by Durability



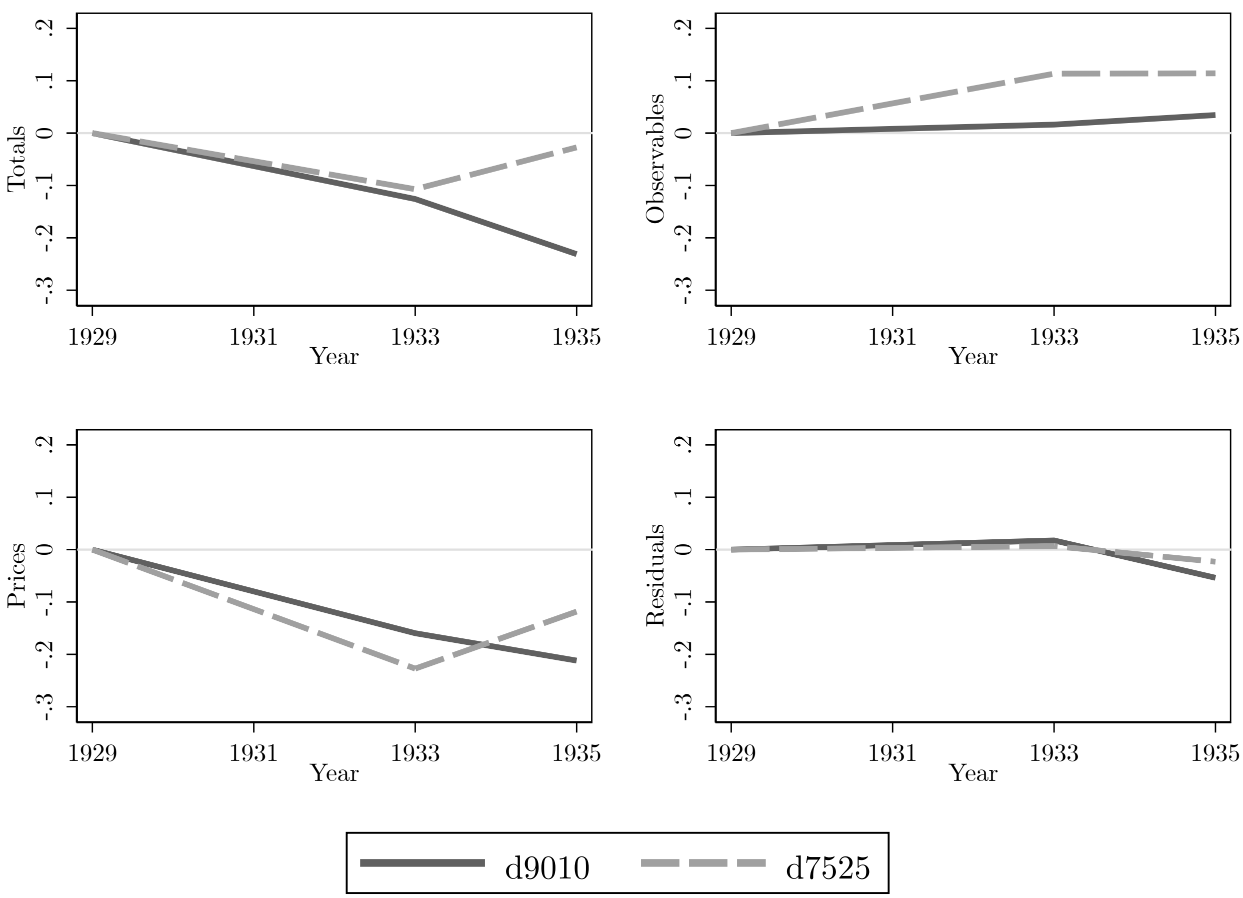
Notes: The earnings per worker variable for a particular group of employees is calculated as total earnings of that group divided by total number of workers in that group. The 1% tails of the distribution are winsorized. We report this variable in thousands of $ 2015. Durability is defined based on the industry's product. Employment weights are used and based on the number of white collar employees at a given establishment.

Figure 10: JMP Decomposition of Earnings per Worker: 1935 as Base Year



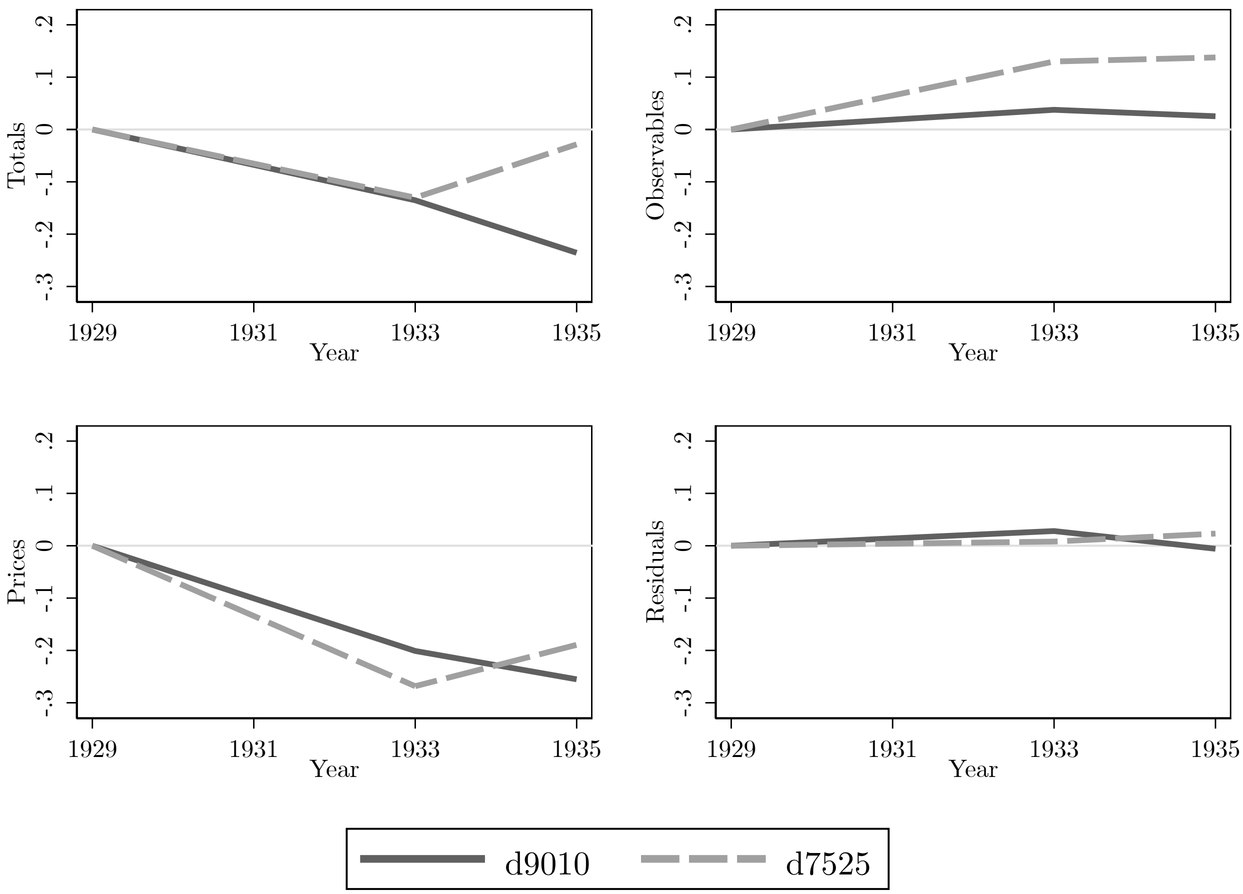
Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. The regressions here include the same set of controls as the JMP regressions in the main body of the text. The statistic "d9010" is the difference between the 90th and 10th percentiles. The statistic "d7525" is the difference between the 75th and 25th percentiles.

Figure 11: JMP Decomposition of Earnings per Worker: Employment as a Measure of Size



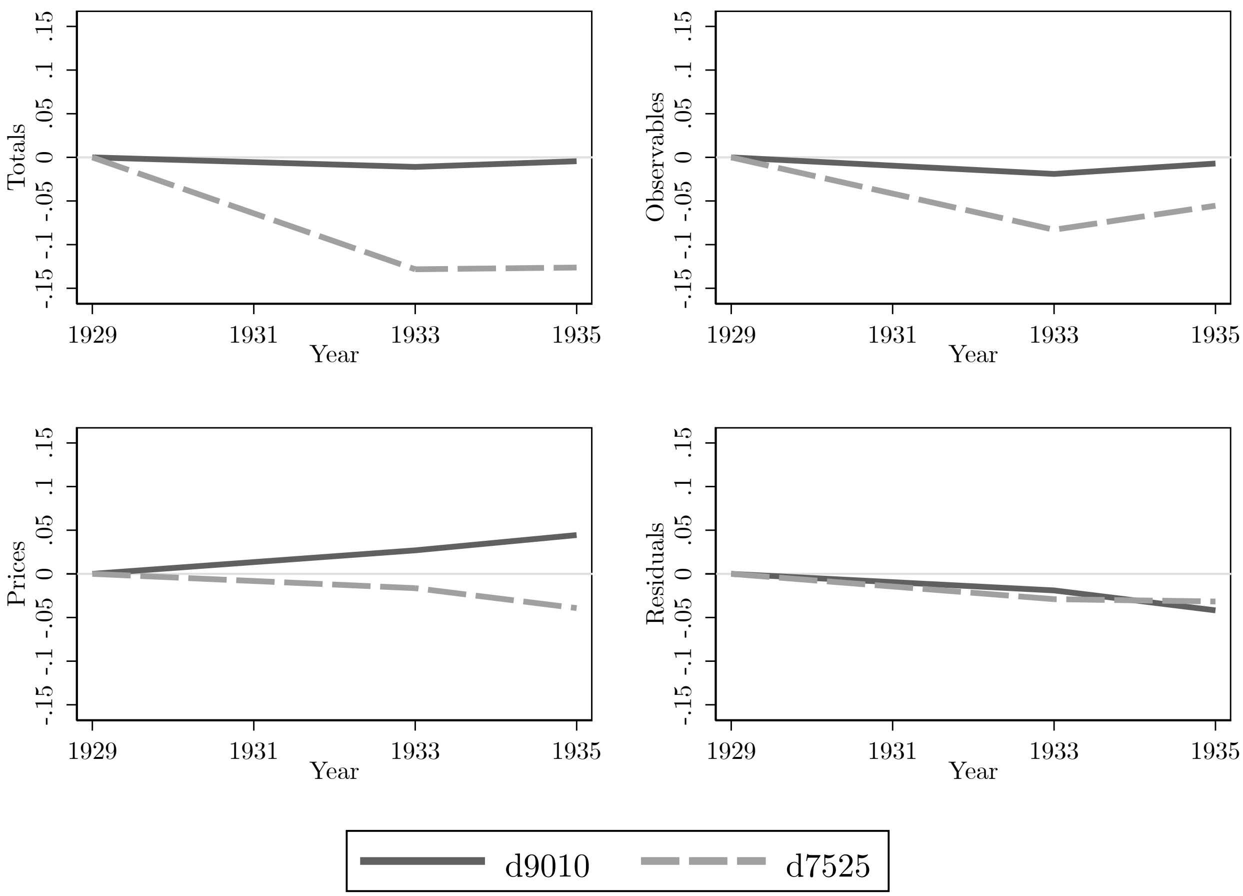
Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. The regressions here include the same set of controls as the JMP regressions in the main body of the text.The only difference is that we have replaced revenue with employment as our measure of establishment size. The statistic "d9010" is the difference between the 90th and 10th percentiles. The statistic "d7525" is the difference between the 75th and 25th percentiles.

Figure 12: JMP Decomposition of Earnings per Worker: Unwinsorized



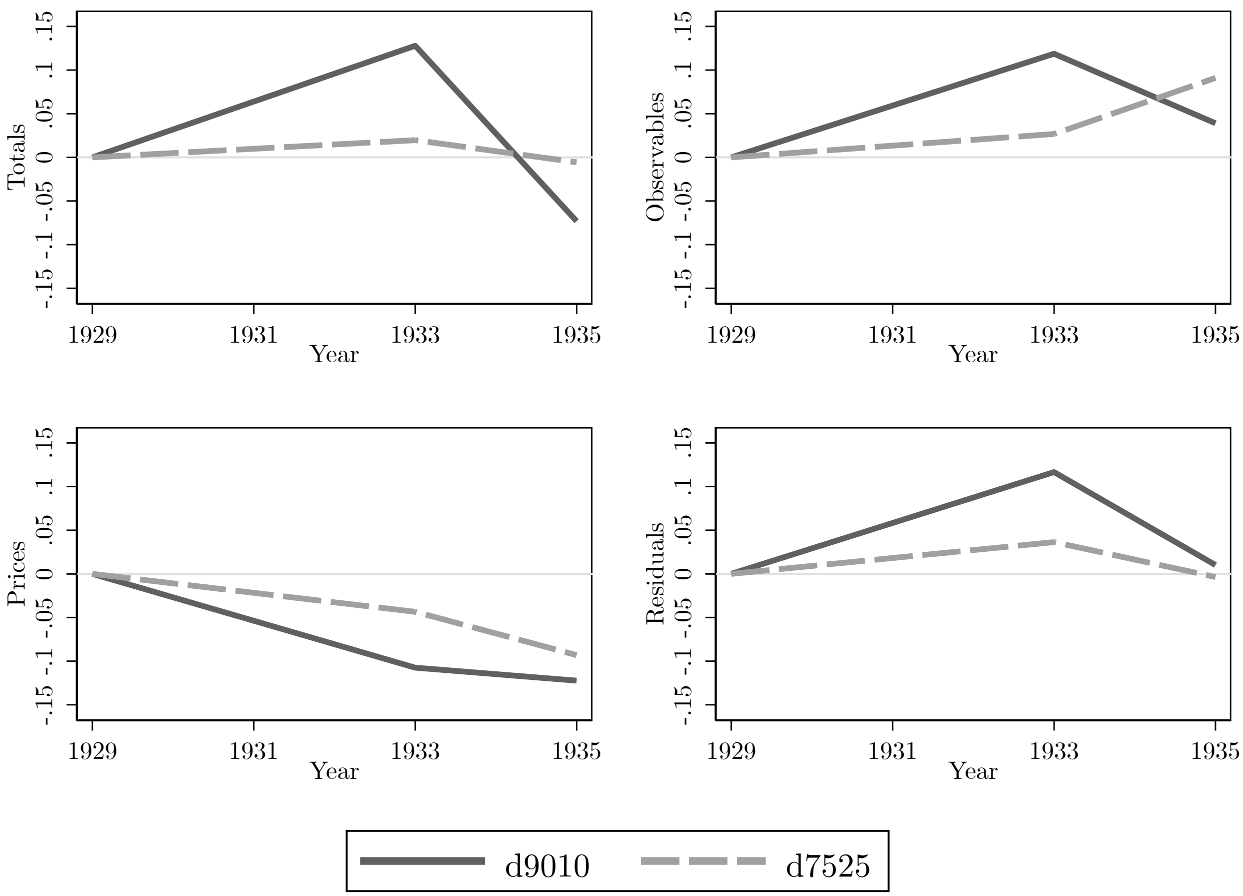
Notes: The earnings variables is log transformed but not winsorized. The regressions here include the same set of controls as the JMP regressions in the main body of the text. The statistic “d9010” is the difference between the 90th and 10th percentiles. The statistic “d7525” is the difference between the 75th and 25th percentiles.

Figure 13: JMP Decomposition of Blue Collar Earnings per Worker: Non-durables



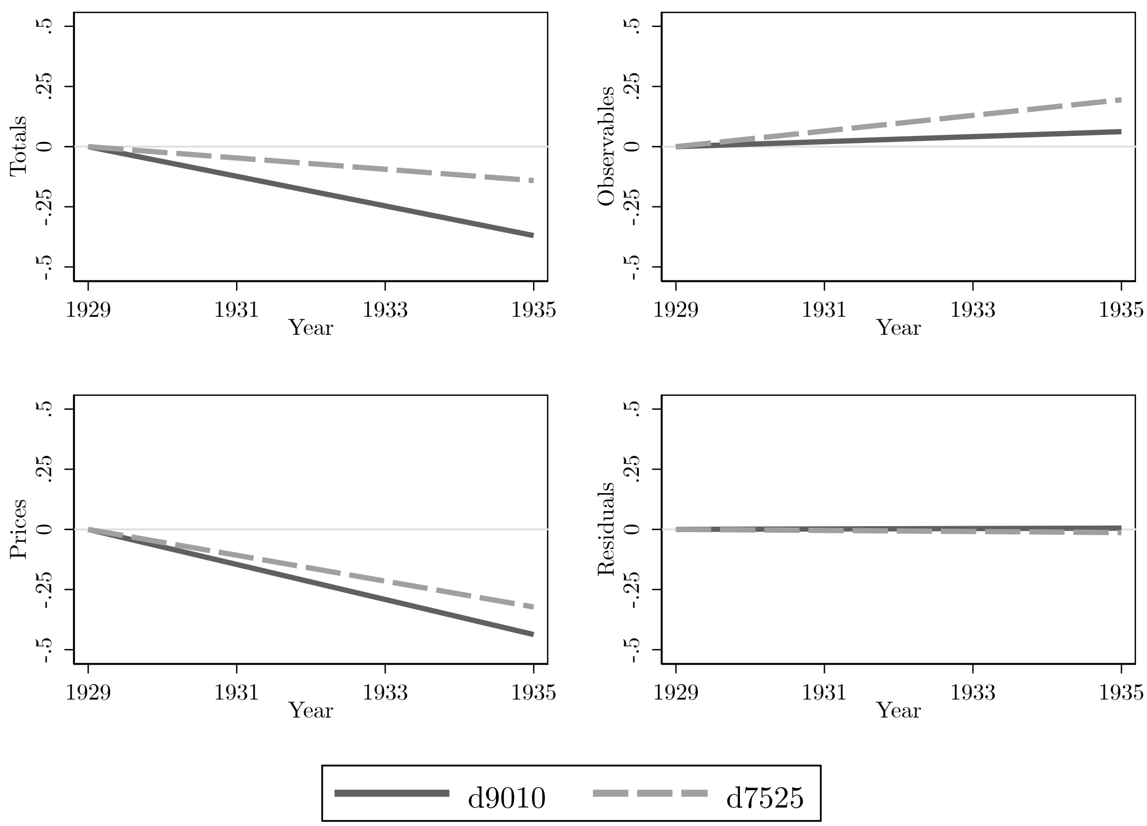
Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. These are based on regressions in Table 2 controlling for log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects and restricted to non-durable goods industries. 1929 is the base year. The statistic "d9010" is the difference between the 90th and 10th percentiles. The statistic "d7525" is the difference between the 75th and 25th percentiles.

Figure 14: JMP Decomposition of Blue Collar Earnings per Worker: Durables



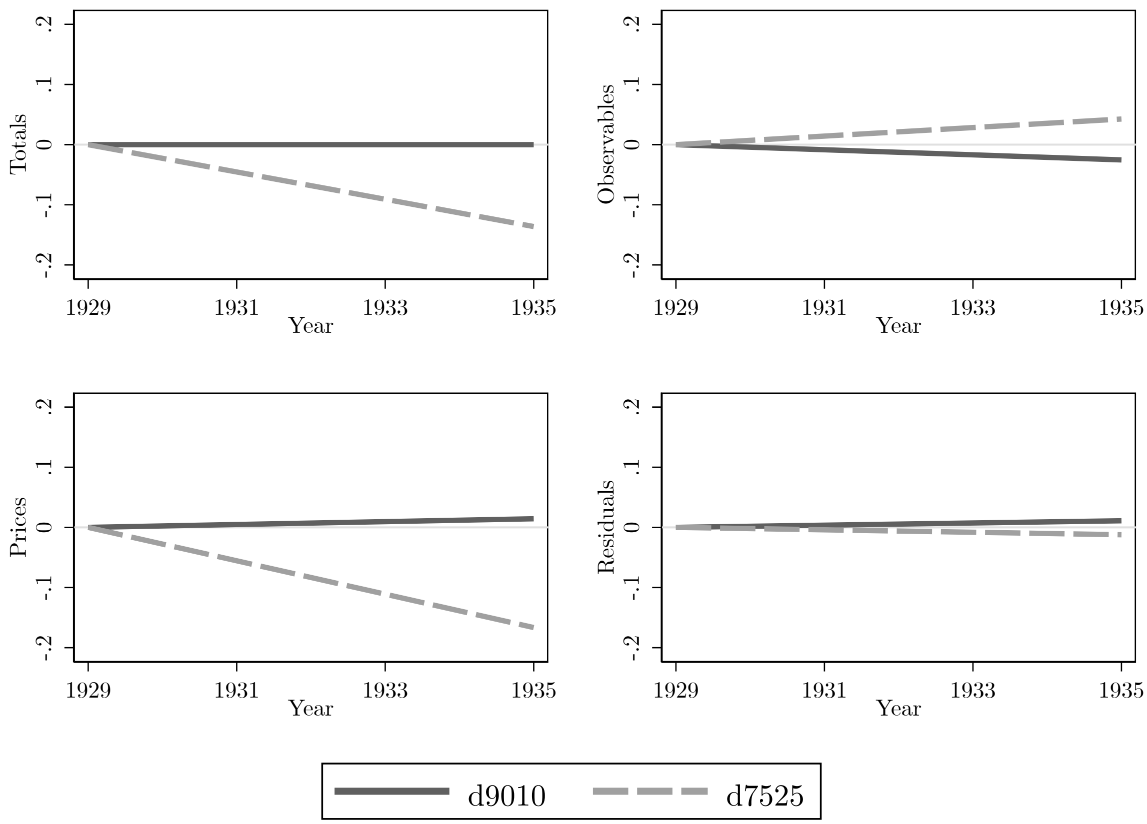
Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. These are based on regressions in Table 3 controlling for log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects and restricted to durable goods industries. 1929 is the base year. The statistic “d9010” is the difference between the 90th and 10th percentiles. The statistic “d7525” is the difference between the 75th and 25th percentiles.

Figure 15: JMP Decomposition of Blue Collar Hourly Earnings



Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. These are based on regressions in Table 5 controlling for log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects. 1929 is the base year. The statistic "d9010" is the difference between the 90th and 10th percentiles. The statistic "d7525" is the difference between the 75th and 25th percentiles.

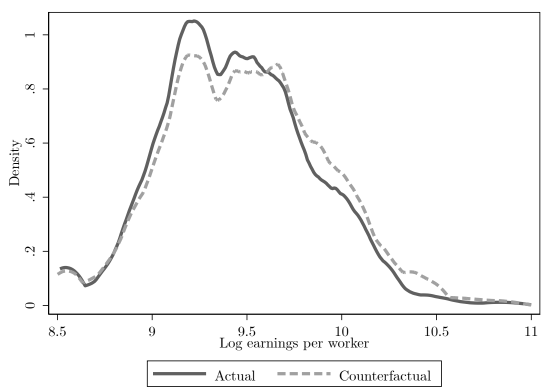
Figure 16: JMP Decomposition of Blue Collar Workweeks



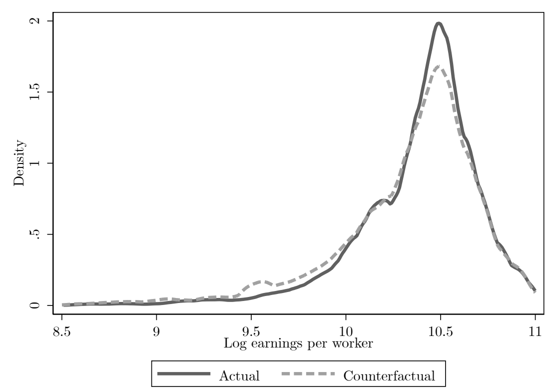
Notes: The workweek variable is log transformed and the 1% tails are winsorized. These are based on regressions in Table 6 controlling for log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects. 1929 is the base year. The statistic "d9010" is the difference between the 90th and 10th percentiles. The statistic "d7525" is the difference between the 75th and 25th percentiles.

Figure 17: Exit Counterfactual Earnings per Worker Distribution: 1933

(a) Blue Collar



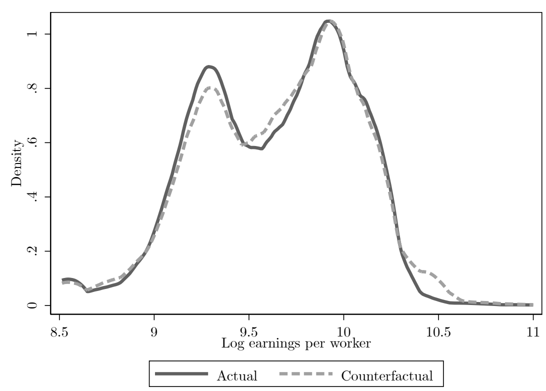
(b) White Collar



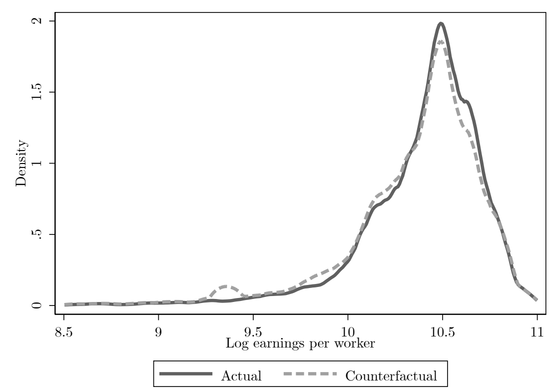
Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. The "Counterfactual" distribution imputes earnings per worker for establishments that exit between 1929 and 1933 based on 1929 observables including establishment size as well as industry and state fixed effects. The distribution and regressions use weights based on the number of employees in a particular occupational group at a given establishment. Employment for exiting establishments is imputed based on predicted values of employment growth rates using the same set of observables used to predict earnings. The "Actual" distribution excludes any establishments that entered in 1933.

Figure 18: Exit Counterfactual Earnings per Worker Distribution: 1935

(a) Blue Collar



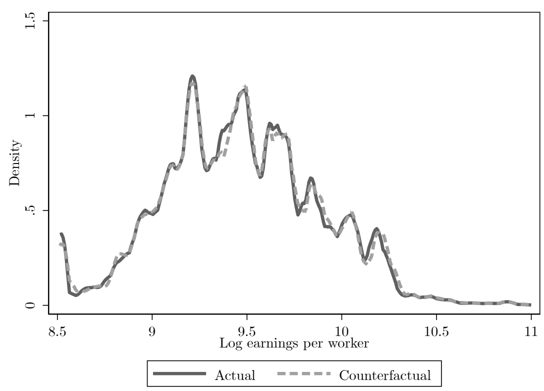
(b) White Collar



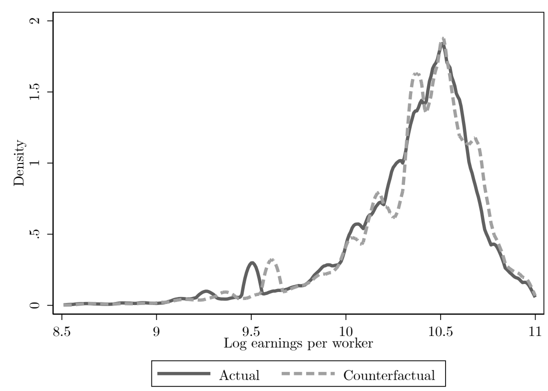
Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. The "Counterfactual" distribution imputes earnings per worker for establishments that exit between 1933 and 1935 based on 1933 characteristics that include establishment size as well as industry and state fixed effects. The distribution and regressions use weights based on the number of employees in a particular occupational group at a given establishment. Employment weights for exiting establishments is imputed based on predicted values of employment growth rates using the same set of observables used to predict earnings growth. The "Actual" distribution excludes any establishments that entered in 1935.

Figure 19: Entry Counterfactual Earnings per Worker Distribution: 1933

(a) Blue Collar



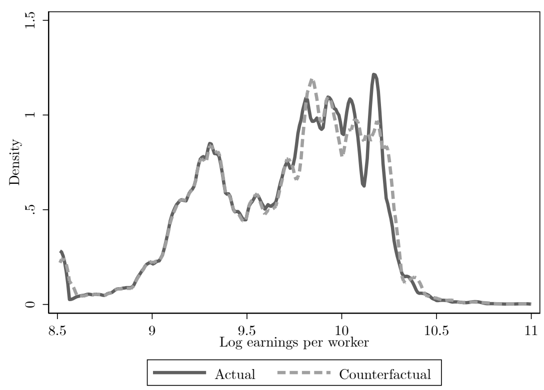
(b) White Collar



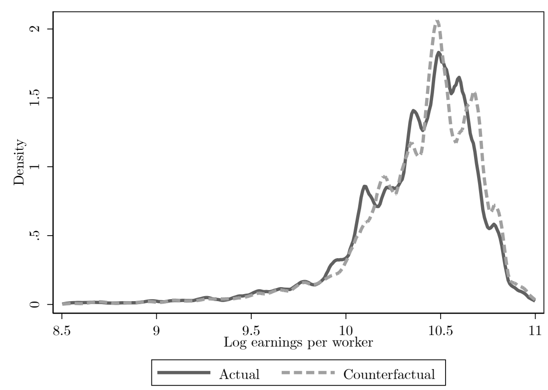
Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. The "Counterfactual" distribution removes the effect associated with entry based on a regression of earnings per worker that includes log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects. The distribution and regressions use weights based on the number of employees in a particular occupational group at a given establishment.

Figure 20: Entry Counterfactual Earnings per Worker Distribution: 1935

(a) Blue Collar



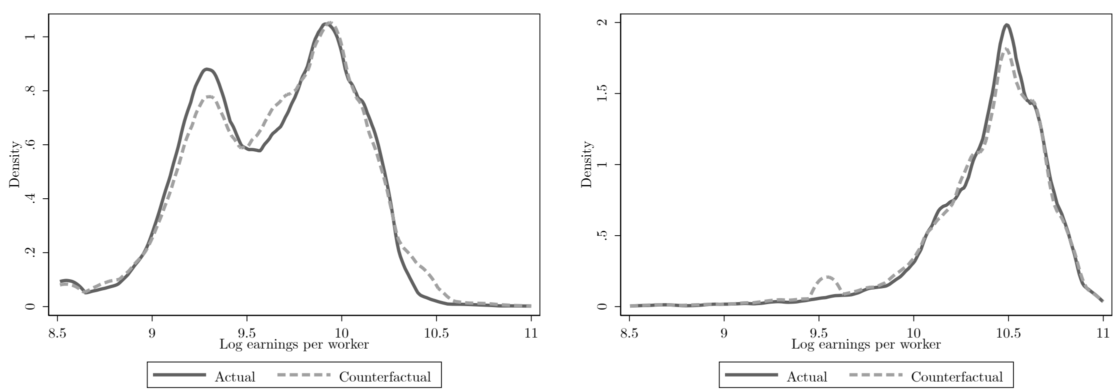
(b) White Collar



Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. The "Counterfactual" distribution removes the penalty associated with entry based on a regression of earnings per worker that includes log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects. Employment weights are based on the number of employees in a particular occupational group at a given establishment.

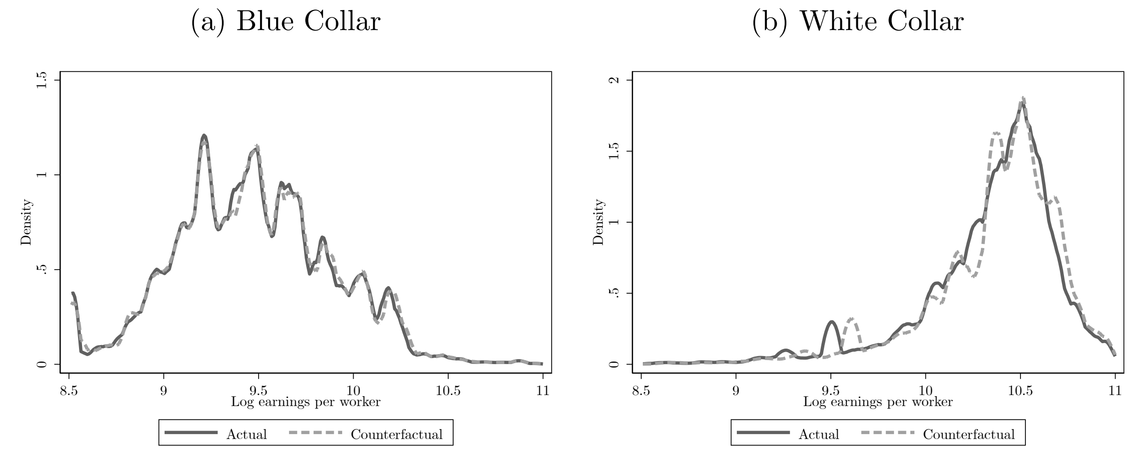
Figure 21: 1933 Exit Counterfactual Earnings per Worker Distribution: Limited Observables

(a) Blue Collar (b) White Collar



Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. The "Counterfactual" distribution imputes earnings and employments for establishments that exit between 1929 and 1933 based on state and industry specific growth rates of earnings. Employment weights are based on the number of employees in a particular occupational group at a given establishment.

Figure 22: 1933 Entry Counterfactual Earnings per Worker Distribution: Employment Adjustment



Notes: The earnings per worker variable is log transformed and the 1% tails are winsorized. The "Counterfactual" distribution removes the effect of entry on earnings based on a regression that includes log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects. Employment weights are based on the number of employees in a particular occupational group at a given establishment. We adjust the employment of entering establishments by removing any employment penalty associated with entry after controlling for log revenue, dummies for incorporation and multiplant status as well as region and industry fixed effects.