Online Appendix:
The Labor Market Effects of Workweek Restrictions: Evidence from the Great Depression

Price Fishback

University of Arizona and NBER
fishback@arizona.edu

Chris Vickers

Auburn University
czvickers@gmail.com

Nicolas L. Ziebarth

Auburn University and NBER
nicolas.ziebarth@me.com

May 23, 2023

In this appendix, we provide some additional details regarding the model. We also provide some additional details on the COM sample and some checks on the quality of the workweek variable. Finally, we offer a number of robustness checks for our estimates of the effects of the workweek limits on hours, employment, and earnings.

1 Model Details

Details on Model Calculations For ease of notation, we write N_j instead of N_j^D for the optimal choice of employment by firm j. The equations determining the no workweek limit equilibrium in the case of Cobb-Douglas utility and production functions are

$$\begin{aligned}
\alpha_j\,(Y_j/N_j) &= \bar{U}^{1/\phi}(1-H_j)^{-(1-\phi)/\phi}, && (1)\\
(1-\alpha_j)\,(Y_j/H_j) &= \frac{1-\phi}{\phi}\bar{U}^{1/\phi}N_j(1-H_j)^{-1/\phi}, && (2)\\
Y_j &= N_j^{\alpha_j}H_j^{1-\alpha_j}, && (3)\\
G(\bar{U}) &= \int_0^1 N_j(\bar{U})dP(\alpha_j), && (4)
\end{aligned}$$

where $P(\alpha_j)$ is the (exogenous) distribution of α_j. Taking the ratio of equations 1 and 2,

$$\frac{\alpha_j}{1-\alpha_j}\frac{H_j}{N_j} = \frac{\phi}{1-\phi}\frac{1-H_j}{N_j}. \tag{5}$$

Solving this equation for H_j delivers the optimal value of H_j^* as stated in the paper. Next, using the first order condition for N_j (equation 1), we can then solve for the level of employment as a function of H_j and $\bar{U}$:

$$N_j = \alpha_j^{(1-\alpha_j)^{-1}}\bar{U}^{\frac{-1}{\phi(1-\alpha_j)}}H_j(1-H_j)^{\frac{1}{\tilde{\phi}(1-\alpha_j)}}. \tag{6}$$

To determine the equilibrium utility level, we calculate the optimal choice of employment by substituting $H_j = H_j^*$ into equation 6:

$$N_j = \bar{U}^{\frac{-1}{\phi(1-\alpha_j)}}\theta(\alpha_j,;\phi), \tag{7}$$

where $\theta(\alpha_j;\phi)$ is

$$\theta(\alpha_j;\phi) = \tilde{\phi}\left[(\tilde{\alpha}_j+\tilde{\phi})^{-\alpha_j}\tilde{\alpha}_j^{1-\phi}\right]^{(1-\alpha_j)^{-1}}. \tag{8}$$

Then the employment market clearing equation that determines $\bar{U}$ states

$$G(\bar{U}) = \int_0^1 \bar{U}^{\frac{-1}{\phi(1-\alpha_j)}}\theta(\alpha_j;\phi)dP(\alpha_j). \tag{9}$$

To study the the aggregate employment effects of a workweek limit, we take a linear approximation around the no workweek limit equilibrium whose values we denote by a $*$. First, we have the linear approximation to the optimal employment choice:

$$N_j - N_j^* = \delta_j^{\bar{U}}(\bar{U} - \bar{U}^*) + \delta_j^H(H_j - H_j^*), \tag{10}$$

where

$$\delta_j^{\bar{U}} = -\frac{1}{\phi(1-\alpha_j)}\frac{N_j^*}{\bar{U}^*} < 0, \tag{11}$$

$$\delta_j^H = -\frac{1-\alpha_j}{\alpha_j}\frac{N_j^*}{H_j^*} < 0. \tag{12}$$

For δ_j^H, we calculate the derivative of the optimal choice of N_j with respect to H_j:

$$\begin{aligned}
\frac{dN_j}{dH_j} &= \alpha_j^{(1-\alpha_j)^{-1}}\bar{U}^{\frac{-1}{\phi(1-\alpha_j)}}\left[(1-H_j)^{\frac{1}{\tilde{\phi}(1-\alpha_j)}} - \frac{1}{\tilde{\phi}(1-\alpha_j)}H_j(1-H_j)^{\frac{1}{\tilde{\phi}(1-\alpha_j)}-1}\right] \\
&= \alpha_j^{(1-\alpha_j)^{-1}}\bar{U}^{\frac{-1}{\phi(1-\alpha_j)}}H_j(1-H_j)^{\frac{1}{\tilde{\phi}(1-\alpha_j)}}\left[H_j^{-1} - \frac{1}{\tilde{\phi}(1-\alpha_j)}(1-H_j)^{-1}\right] \\
&= \frac{N_j^*}{H_j^*}\left[1 - \frac{1}{\tilde{\phi}(1-\alpha_j)}\frac{H_j^*}{1-H_j^*}\right] \\
&= \frac{N_j^*}{H_j^*}\left[1 - \frac{\frac{\tilde{\phi}}{\tilde{\alpha}_j+\tilde{\phi}}}{\frac{\tilde{\alpha}_j}{\tilde{\alpha}_j+\tilde{\phi}}\tilde{\phi}(1-\alpha_j)}\right] \\
&= -\frac{N_j^*}{H_j^*}\frac{1-\alpha_j}{\alpha_j}.
\end{aligned}$$

A similar calculation for the derivative of of N_j with respect to $\bar{U}$ gives the value of $\delta_j^{\bar{U}}$.

Now taking a linear approximation to the employment market clearing condition, we have

$$\begin{aligned}
G(\bar{U}^*) + G'(\bar{U}^*)(\bar{U} - \bar{U}^*) &= \int [N_j^* + \delta_j^{\bar{U}}(\bar{U} - \bar{U}^*) + \delta_j^H(H_j - H_j^*)]dP(\alpha_j) \\
&= N^* + (\bar{U} - \bar{U}^*)\int \delta_j^{\bar{U}} dP(\alpha_j) + \int \delta_j^H(H_j - H_j^*)dP(\alpha_j) \\
&= G(\bar{U}^*) + (\bar{U} - \bar{U}^*)\mathbb{E}[\delta^{\bar{U}}(\alpha_j)] + \int \delta_j^H(H_j - H_j^*)dP(\alpha_j) \\
&= G(\bar{U}^*) + (\bar{U} - \bar{U}^*)\mathbb{E}[\delta^{\bar{U}}(\alpha_j)] + \int_0^{\tilde{\alpha}^*} \delta_j^H(h - H_j^*)dP(\alpha_j).
\end{aligned}$$

This equation can be solved for $\bar{U} - \bar{U}^*$. From this expression, it follows that $\bar{U}$ increases in response to a workweek limit.

For the aggregate output effects, we take a linear approximation to the production of firm j:

$$\begin{aligned} Y_j - Y_j^* &= H_j^{1-\alpha_j}[\gamma_j^{\bar{U}}(\bar{U} - \bar{U}^*) + \gamma_j^H(H_j - H_j^*)] + \gamma_j N_j^{*\alpha_j}(H_j - H_j^*) \\ &= H_j^{1-\alpha_j}\gamma_j^{\bar{U}}(\bar{U} - \bar{U}^*) + [H_j^{1-\alpha_j}\gamma_j^H + \gamma_j N_j^{*\alpha_j}](H_j - H_j^*), \end{aligned}$$

where

$$\gamma_j^{\bar{U}} = -\frac{\tilde{\alpha}_j N_j^{*\alpha_j}}{\phi \bar{U}^*} < 0, \tag{13}$$

$$\gamma_j^H = -(1-\alpha_j)\frac{N_j^{*\alpha_j}}{H_j^*} < 0, \tag{14}$$

$$\gamma_j = (1-\alpha_j)H_j^{*-\alpha_j} > 0. \tag{15}$$

The values for $\gamma_j^{\bar{U}}, \gamma_H^{\bar{U}}$, and γ_j come from taking derivatives of the production function and evaluating at the no workweek limit values. With this expression for the change in individual firm j's output, we can then calculate the change in aggregate output, which is simply the integral of the individual firm output changes:

$$\begin{aligned} Y - Y^* &= (\bar{U} - \bar{U}^*)\int_0^1 H_j^{1-\alpha_j}\gamma_j^{\bar{U}} dP(\alpha_j) + \int_0^1 (\gamma_j^H + N_j^{*\alpha_j}\gamma_j)(H_j - H_j^*)dP(\alpha_j) \\ &= (\bar{U} - \bar{U}^*)\int_0^1 H_j^{1-\alpha_j}\gamma_j^{\bar{U}} dP(\alpha_j) + \int_0^{\tilde{\alpha}} (h^{1-\alpha_j}\gamma_j^H + N_j^{*\alpha_j}\gamma_j)(h - H_j^*)dP(\alpha_j) \\ &= -\left(\frac{\bar{U} - \bar{U}^*}{\bar{U}^*}\int_0^1 \frac{\tilde{\alpha}_j}{\phi} H_j^{1-\alpha_j} N_j^{*\alpha_j} dP(\alpha_j) + \int_0^{\tilde{\alpha}} (1-\alpha_j)\frac{N_j^{*\alpha_j}}{H_j^*}[(H_j^*)^{1-\alpha_j} - h^{1-\alpha_j}](H_j^* - h)dP(\alpha_j)\right). \end{aligned}$$

We know that $\frac{\bar{U}-\bar{U}^*}{\bar{U}^*} > 0$ and $\int_0^1 \frac{\tilde{\alpha}_j}{\phi} H_j^{1-\alpha_j} N_j^{*\alpha_j} dP(\alpha_j)$ is obviously positive so the first term in the sum is positive. The second term is also non-negative since $H_j^* \geq h$ for all j. Therefore, $Y - Y^* < 0$ as stated in the paper.

In the paper, we highlighted the effects of the workweek limit on the various measures of earnings. First, for earnings per hour E_j/H_j, recall that with a Cobb-Douglas utility function,

$E = \bar{U}^{1/\phi}(1-H)^{1-1/\phi}$ so earnings per hour at firm j is equal to

$$E_j/H_j = \bar{U}^{1/\phi}(1-H_j)^{1-1/\phi}H_j^{-1}. \tag{16}$$

Evaluating this expression at the no workweek limit optimal choice H_j^*, we find

$$E_j/H_j = \frac{\bar{U}^{1/\phi}\tilde{\alpha}_j(\tilde{\phi}+\tilde{\alpha}_j)^{1+\phi^{-1}}}{\tilde{\phi}}. \tag{17}$$

The elasticity of earnings per hour with respect to hours per worker, holding fixed $\bar{U}$, is

$$\frac{d\log E_j/H_j}{d\log H_j} = \frac{1}{\tilde{\phi}}\frac{H_j}{1-H_j} - 1. \tag{18}$$

If we evaluate this elasticity at the no workweek limit optimal choice $H_j = H_j^*$, we have

$$\frac{d\log E_j/H_j}{d\log H_j} = \tilde{\alpha}_j^{-1} - 1. \tag{19}$$

The elasticity of earnings per worker follows directly from this expression.

Finally, we consider the effect of the workweek limit on the aggregate "purchasing power" of workers or, in other words, total payroll EN. First, at the establishment-level,

$$\begin{aligned}
\frac{d\log E_jN_j}{d\log H_j} &= \frac{d\log E_j}{d\log H_j} + \frac{d\log N_j}{d\log H_j} \\
&= 1 - \frac{\tilde{\alpha}_jH_j}{\tilde{\phi}(1-H_j)} \\
&= 0,
\end{aligned}$$

where the last line comes from evaluating H_j at the optimal level in no workweek limit equilibrium. Therefore, there is no aggregate effect either *holding fixed* $\bar{U}$. On the other hand, total purchasing power of workers is increasing in $\bar{U}$.

Pareto Efficient Outcomes In the paper, we stated the Pareto problem and showed that the no workweek limit equilibrium was inefficient. Recall that we defined λ_1 as the Lagrange multiplier on the constraint $\Pi(\bar{U}) \geq \bar{\Pi}$ and λ_2 the multiplier on the constraint $\int_0^1 N_j dj = G(\bar{U})$. The Pareto efficient outcome is determined by the solution to the following system of equations (assuming the constraints bind):

$$
\begin{aligned}
F_H(N_j, H_j) - N_j E_H(H_j, \bar{U}) &= 0, &(20)\\
\lambda_1 + \lambda_2 F_N(N_j, H_j) - \lambda_2 E(H_j, \bar{U}) &= 0, &(21)\\
G(\bar{U}) - \lambda_1 G'(\bar{U}) - \lambda_2 \int_0^1 N_j E_{\bar{U}}(H_j, \bar{U}) dj &= 0, &(22)\\
\Pi(\bar{U}) - \bar{\Pi} &= 0, &(23)\\
\int_0^1 N_j dj - G(\bar{U}) &= 0. &(24)
\end{aligned}
$$

Equation 20 shows that a Pareto efficient choice of the workweek coincides with the equilibrium choice in the no workweek limit. However, this is not the case for the employment choice. Rearranging equation 21 shows that employment in the no workweek equilibrium is too low. Instead, the policy necessary to implement a Pareto efficient outcome is a simple employment subsidy funded through a lump sum tax on firms or households. Such a subsidy will not distort the workweek choice and will allow the planner to implement an efficient level of employment.

In our formulation of the Pareto problem, the planner does not distinguish between employed and unemployed households even though these two groups of households will have different utility levels. Similarly, the planner does not distinguish between different kinds of firms even though firms with different production technologies will earn different levels of profits. Ignoring these dimensions of heterogeneity is potentially an important simplification since one motivation for the workweek limit was to redistribute income from those employed to those unemployed. The implicit assumption behind this motivation is that households are unable to hedge the risk of unemployment *ex ante* so social insurance in the form of a workweek limit that "artificially" increases employment and redistributes income to those unemployed might be socially beneficial. The problem with this motivation in this particular model is that those unemployed are strictly better off than those that are employed. Hence, the uninsurable downside risk is the risk of being *employed.* Because of that, socially beneficial *ex post* redistribution should, if anything, flow from the unemployed to the employed.

What drives the inefficient outcome is the fact that firms are unable to perfectly discriminate between households by offering contracts specifying different levels of utility depending on the household's reservation utility. If the firm was able to offer each household its reservation utility, then the no workweek limit equilibrium would be Pareto efficient (assuming the planner could make such household-specific transfers as well). In such a world, a firm with current employment N offering a contract promising utility $\bar{U}$ to the marginal worker would choose a workweek H according to

$$\max_H F(N, H) - E(H, \bar{U}).$$

The optimal choice in this case would simply be $F_H = E_H$ whereas, as we saw in the case of a single contract, the optimal choice was $F_H = NE_H$. Like a monopolist having to worry about reducing its price for all of its infra-marginal consumers, a firm having to offer a single contract has to worry about adjusting the earnings of all its current employees when it makes a workweek choice for its marginal employee. This is not a concern when the firm can discriminate between workers. In this case, if the incremental profit is positive at the optimal level of the workweek, then the firm will hire the worker and will continue to hire up to the level of the utility for which the incremental profit is equal to 0. This is the socially efficient level of employment since the (social) opportunity cost of hiring a worker is exactly equal to the worker's reservation utility.

2 Details on COM Sample

Here we provide some additional details on the COM sample. As noted in the paper, our dataset consists of 8 industries drawn from a larger sample of 25 industries. We focused on these industries because these were the ones for which census enumerators asked establishments about both monthly employment and manhours. This allowed us to calculate an imputed workweek by month. In general, these schedules provide a wealth of information including a breakdown of outputs and inputs into price and quantity as well as information on salaried workers. We refer the interested reader to Vickers and Ziebarth (2018) for a longer discussion of the information on the schedules and how the information compares to the modern COM and Annual Survey of Manufactures.

Table 1 provides some summary statistics on the 8 industries in January 1933 we study. Across the industries, there are large differences in the average size of an establishment as measured by total employment, revenue, and wages. The industries with the smallest establishments are ice cream and manufactured ice that have an average number of employees of around 10 and average revenue between $43,000 and $67,000. The largest establishments in terms of employment and revenue are located in the cigars and cigarettes industry while the average wage bill is largest in motor vehicles. Table 2 shows the number of establishments across industries in 1933. Much of the sample in terms of the raw number of establishments is concentrated in consumer nondurables including ice cream, manufactured ice, and cotton goods. Relative to manufacturing as a whole, these 8 industries have larger operations and are presumably more capital intensive though we do not have direct information on an establishment's capital stock.[1]

We now discuss a concern about the completeness of the data. There are at least two separate versions of this concern. The first is whether the schedules collected and transcribed represented all establishments in operation in a given industry. The second is whether, conditional on the set of schedules we have, the critical questions on employment and manhours were filled out consistently (and correctly). We address that second issue in the next section where we provide additional details on the quality of the imputed workweek variable. As for whether we have a complete set of schedules, this question actually has two separate subquestions. First, did the census enumerators canvas all the establishments in operation? The first question is very hard to answer in general since it requires, as a point of comparison, an outside source that enumerated all establishments in an industry at the same point time as the census. The cement industry is one example where this is possible. Chicu et al. (2013) use the trade journal *Pit and Quarry Handbook*, which contains a directory of establishments in that industry, and find only minor differences between the schedules held at the archives and these directories.

The check on the enumeration of cement establishments is really a test of the joint hypothesis that all establishments were enumerated and a second subquestion whether all the schedules collected were actually kept (and kept in the correct set of boxes) by the National Archives. Vickers

[1] Table 3 lists the industry-specific limits we used for the COM sample in the text.

and Ziebarth (2018) highlight one example where some schedules have been lost in the manufactured ice industry from 1931 in the state of Texas. This example does not affect our sample since we use data from 1933 and 1935. In general, it is possible to test for this issue by comparing the establishments transcribed versus totals in the published volumes. We are not aware of any major subsets of missing establishments for the industries and years we examine.[2]

Finally, in the paper, we chose a 2 hour bandwidth when comparing characteristics of establishments just below to above just above the workweek limit. Figure 1 shows that if instead we choose a 4 hour bandwidth, results are basically unaffected. There are no major differences between establishments just above or just below the workweek limit, and, if anything, the differences are more minor than those in the paper using a 2 hour bandwidth.

3 Quality of COM Workweek Variable

Our main variable of interest in the COM dataset is the imputed workweek by month. This is calculated by establishment at the monthly frequency as the ratio of total manhours relative to total hourly employment. In the text, we addressed the question of whether establishments were systematically misreporting hours worked from fear of legal or social consequences from appearing to violate the workweek limit. Here we address the extent to which there was non-systematic reporting errors and other sources of measurement error.

First, within an industry, not all establishments were asked or reported this monthly information. Figure 2 shows the percentage of establishments with a missing value for the imputed workweek variable in 1933 by revenue quartile. Larger establishments were more likely to be asked this information and provide it. However, even for the largest establishments, around 20% did not provide all the information required to calculate the workweek. Next we calculate conditional on having a non-missing imputed workweek, what fraction of the calculated workweeks are "valid" defined as a workweek longer than 10 hours but shorter than 100. Figure 3 shows that percentages reporting a valid workweek was over 90% for all revenue quartiles in 1933. We take from this that,

[2] Our sense is that to the extent that there are more cases like the 1931 ice industry in Texas, they are random and not systematic.

for those reporting, the information reported was reliable.

Another “internal” quality check is to compare the imputed workweek to the question on the schedule that asked about the length of the establishment’s “usual” workweek. Care must be taken here because not all industries were asked this question and the industries asked this question varied from year to year. Furthermore, the question about the workweek is somewhat vague as it asks about the “usual” or “normal” workweek. Not surprisingly then, the distribution of this variable is quite “lumpy” with spikes at 40, 50, and 60 hours per week. With these caveats in mind, Figure 4 shows the distribution of the imputed workweek expressed as a percentage of the usual workweek for the month of December, which is the reference month for the question on the Census schedule. This distribution is weighted by employment and restricted to establishments with a valid reported and imputed workweek. For most establishments, the imputed workweek is shorter than the usual one with a modal percentage of around 80%. This makes sense if we think that the value the establishments reported is the most common workweek, and the “true” distribution of the workweek taking into account the handful of employees working overtime and a larger fraction working less than the “usual” workweek is negatively skewed.

Our next check compares the workweek in 1933 for the set of industries (or sectors) that are common to the COM and SSNRA datasets.[3] We collapse the establishment-level COM data to the industry-month level by calculating the median workweek. We choose the median to minimize the influence of potential outliers in the establishment-level dataset. Figure 5 shows that the workweek levels are quite similar across these two datasets though workweeks in the SSNRA do seem to be consistently lower than those in the COM. The differences are more apparent for longer workweeks. This might be due to the fact the COM workweek effectively includes overtime while the SSNRA workweek is the “normal” workweek (though this explanation would be in tension with the differences between the imputed workweek and the usual workweek reported on the schedules).

[3]These include the following pairs where the first listed is in the SSNRA and the second COM: (1) “Cigar”, “Cigars and Cigarettes”; (2) “Cigarette, Snuff, Chewing and Smoking Tobacco”, “Cigars and Cigarettes”; (3) ”Structural Steel and Iron Fabricating”, “Steel works”; (4) ”Steel Casting”, “Steel works”; (5) ”Ice Cream Manufacturing”, “Ice cream”; (6) ”Automobile Manufacturing”, “Motor vehicles”. Note that we match some of the SSNRA sectors to the same COM industry.

4 Robustness Checks for Results on Labor Market Effects

4.1 Balanced Sample

We are missing information on some of the dependent variables for about 200 observations in the SSNRA sample. This led differences in the sample sizes between some of the specifications in Table 1 of the paper. Table 4 shows that if we restrict attention to the set of observations for which there is no missing information for any of the dependent variables, the results do not change very much.

4.2 Employment Levels

Because of the issues of matching between the SSNRA data and the COM, we end up being able to recover the actual level of employment for only about half of the indexed sectors. For a handful of sectors, we impute their employment using employment in a broader industry classification along with a breakdown of products within the sector. For example, "Wheat Flour Milling", a SSNRA sector, is not an industry in the COM, but flour milling is. Furthermore, the published volume of the COM provides the share of the value of wheat flour milling in the industry's total value. Therefore, to impute total employment for wheat flour milling, we multiply by that share times the industry's total employment. This smaller sample size carries over to total manhours and payroll, which are derived from the level of employment. In Table 5, we replicate Table 1 but using the recovered employment levels. The results are very similar.

Using the recovered employment levels generates difference in the sample size between the variables that depend on employment and those that do not. Table 6 is the results when we "fully balance" the sample across all the specifications meaning we have the same exact sample for each dependent variable. This reduces the sample size by about half because of the issue with deindexing the employment variable. The statistical and economic significance of the effects are broadly similar. The only difference is for weekly earnings where the effects are muted.

4.3 Including the Hourly Earnings as a Control

The preferred specification in Taylor (2011) includes the wage or, more precisely, hourly earnings as a control, which we exclude from our preferred specification in Table 1 of the paper.[4] The reason we prefer to exclude hourly earnings as a control is that we want to capture the total effect of the PRA and NIRA policies including both the workweek restrictions and minimum wages. Conditioning on hourly earnings controls for any effects the PRA and NIRA had on labor quantity variables through their effects on wages. For this reason, while not an uninteresting or "wrong" specification, the specification including hourly earnings as a control does not capture exactly what we want. We would expect and have already found that the PRA and NIRA increased hourly earnings. All else equal, the effects of this increase in hourly earnings would, if anything, offset to some extent the direct positive effects of the workweek limits on employment while exacerbating any negative effects on the workweek. Therefore, we predict that the effects of the PRA and NIRA on the workweek in the regressions including hourly earnings as a control should be smaller in magnitude while the effects on employment and total manhours should be larger when controlling for the wage.

These predictions are exactly borne out in Table 7, which shows the results including the hourly wage in the regressions for the labor quantity variables. Relative to not including the wage as a control, we find more muted effects of the PRA and NIRA on the workweek and larger effects on employment and manhours. Sensibly, for all the dependent variables, there is a strong negative relationship between hourly earnings and the labor quantity variables. At the same time, the differences are not so dramatically different to change our overall conclusions. This contrasts with Taylor's estimates that appear to be more sensitive to the set of controls included. For example, in his paper, the effect on employment of the NIRA goes from a small but statistically significant positive effect without any controls to a negative effect of the same magnitude with all the controls included.

[4] Another difference between our preferred specification and Taylor's is that he estimates the model in first differences. This subtly changes the meaning of the PRA and NIRA effect. In fact, in his model, any effects of the PRA and NIRA have permanent effects on the *level* of the dependent variables.

5 Robustness Checks for Effects on Workweek

5.1 Workweek Bunching in the SSNRA Dataset

In the paper, we focused on the establishment-level distribution of the workweek from the COM dataset. Here we examine the industry-level SSNRA data on the workweek for additional evidence on bunching. In this case, we use a common 35 hour workweek limit for all industries. Figure 6 plotting the distribution of the workweek across the "sectors" in the SSNRA shows a similar bunching around the workweek limit as in the COM data. The mode of the workweek is slightly higher than the workweek limit in the post-PRA period. This might be a reflection of the slightly different question asked in this dataset versus how we calculate the workweek in the COM data. Again the fact that there is no real change in the distribution between the two groups of months in 1935 suggests that seasonality was not driving the bunching in 1933. It also suggests that the end of the NIRA with the May 1935 Supreme Court decision did not drastically change behavior.

5.2 Workweek Distribution Assuming a 6-Day Workweek

In the text, to calculate the workweek, we assumed that all establishments had a 5-day workweek. Here as a robustness check, we assume a 6-day workweek instead. Unfortunately, we cannot adjust for possible changes in the number of days following the PRA or any differences in the number of workdays across establishments. All we observe is total hours per month, and there is no way to decompose that into hours per day and days per week.

Figure 7 shows the distribution of the workweek assuming a 6-day workweek in the 3 month windows around July in 1933 and 1935. We still observe a bunching in the distribution after July 1933 with no change between those 3 month periods in 1935. It is not surprising that assuming a 6-day workweek does not make much difference to the monthly patterns since it just changes the scaling of total hours uniformly across all establishments. It does make the bunching around the limit in 1933 a bit sharper, and there is a bit more of a difference between the post-PRA distribution in 1933 relative to the distributions in 1935.

6 Robustness Checks for Effects on Employment

6.1 Bunching Estimator Details

To implement the bunching estimator, it is necessary to specify a number of parameters besides the set of fixed effects to include as controls. First, in the text, we used a 4 hour bin size. There is no clear theoretical basis for choosing this parameter so here we consider a 5 hour bandwidth instead. Figure 8 shows that the results are relatively unchanged. Next, in the text, we chose an upper bound on the workweek of 40 hours about an industry's workweek limit and a lower bound of 10 hours below. Here we consider the following robustness checks: (1) an upper bound of 50 hours about an industry's workweek limit (Fig. 9) and (2) a lower bound of a 15 hours below an industry's workweek limit (Fig. 10). As is clear, none of these other specifications make much a difference to the qualitative patterns. The only slight difference is the muted overall effect in October in these specifications versus our main specification in the paper. Across all of these specifications, we observe the gains below the limit and the losses above the limit growing over time, which is consistent with the fact that it took time for establishments to sign up for the PRA.

7 Robustness Checks for Effects on Earnings

7.1 Continuous Treatment: Pre-PRA Length of Workweek

In the paper, we used a treatment variable defined as the percentage of the three months prior to the PRA in which an industry had a workweek above 35 hours. Here we consider a continuous treatment measure defined as the average length of the workweek over the 3-month pre-PRA period in an industry, Pre-PRA Workweek Length_i. This measure treats "symmetrically" industries with an average workweek one hour above the limit before August 1933 and those with an average workweek one below. This is a slightly odd formulation since presumably the effects of being above or below are non-linear. Those below the limit no matter how far below do not actually have to make any changes while the amount of changes in the workweek necessary for those above the limit is linear in the workweek.

We then estimate using data for hourly earnings in industry i at time t w_{it} between April and October 1933 excluding July the following regression:

$$\log w_{it} = \beta \text{ PRA}_t \times \text{Pre-PRA Workweek Length}_i + \text{Controls}_{it} + \varepsilon_{it},$$

where PRA_t is an indicator for whether the PRA workweek limit is in effect at time t. Standard errors are clustered at the industry-level. Each industry is weighted equally. We estimate this specification in the following ways: (1) including PRA_t and Pre-PRA Workweek Length_i as controls and (2) including month and industry fixed effects (which absorb the level of the pre-PRA workweek) as controls. Since we take the log of the pre-PRA workweek variable and include industry fixed effects (in some of the specifications), our formulation of the treatment is equivalent to a treatment variable defined as the log ratio of the pre-PRA average workweek in an industry to that industry's specific workweek limit.

Table 8 shows that the results using this alternative definition of the treatment are not substantially different from those in the paper. Crucially, hourly earnings rise following the PRA for industries with the longest workweeks in the pre-PRA period while, at the same time, weekly earnings and payroll fall. For example, earnings at an industry with a 45 hour pre-PRA workweek would rise approximately 2% relative to an industry with 35 hour pre-PRA workweek. The fact that the PRA fixed effect is negative does not mean that, on average, hourly earnings fell during this period. Instead the effect on average earnings is equal to the fixed effect plus the interaction effect times the mean value of the pre-PRA workweek length variable. This equals $-0.554 + 0.192 \times 3.68 = 0.16$, which shows a substantial rise in hourly earnings overall after the PRA.

7.2 Event Study

In the text, we focused on a 3-month window before and after the PRA. The goal of this was to minimize other confounding policy changes that would arise when using a longer time frame. Nonetheless, as a robustness check, we now consider the longer-run effects by using the SSNRA dataset, which has monthly data through the end of 1935 and covering the end of the NIRA in

May 1935. We use an event study framework and trace out the month-by-month effects over this more than 2 year period. In particular, we estimate starting in May 1933 and continuing through December 1935 the following regression for hourly earnings in industry i at month t, w_{it}:

$$\log w_{it} = \sum_{\tau} \beta_{\tau} \, \mathbb{1}\left[t = \tau\right] \times \text{Pre-PRA Workweek}_i + \text{Controls}_{it} + \varepsilon_{it}.$$

Here β_τ denotes the effects for a full set of month indicators interacted with the Pre-PRA Workweek treatment variable defined in the paper. Standard errors are clustered at the industry-level. Each industry is weighted equally. We estimate the specification including month and industry fixed effects.

Figure 11 shows these estimates for hourly earnings, each point denotes the relative difference between industries with a high versus low pre-PRA workweek in a given month. We find an immediate jump in hourly earnings for those industries with above average pre-PRA workweeks in July 1933, the month when goes into effect. This differential between industries in earnings continues to rise through the rest of the 1933 and into 1934 during which time it stabilizes. Like the bunching employment effects, the fact that the effect grows in the first few months following July 1933 is consistent with a less than immediate adoption of the PRA by all businesses. We do not observe any pre-trends in these relative differences before the PRA in the first half of 1933. This provides further support for a causal interpretation of these results (at least in the short-run).

Also, consistent with what we found for the distribution of the workweek, in the few months after Supreme Court's decision striking down the NIRA, there is not a drastic change in the effects on earnings. Furthermore, we do not observe any drastic changes during the Compliance Crisis period in early 1934. This is consistent with the idea that the problems with compliance were mainly with the trade practice provisions rather than the labor market ones. We are cautious not to overemphasize these longer-run results since it is easy to imagine that some part of the persistent differences might simply be due to differences between the industries themselves or other intervening policy changes and not actually the workweek limits.

7.3 Effects Using NICB Sample

Here we replicate our basic difference-in-differences strategy to identify the effects on hourly and weekly earnings using the NICB sample rather than the SSNRA sample. Table 9 shows that results are similar to those for the SSNRA sample in terms of magnitude. The only difference is a lack of statistical significance for the weekly earnings effect, which is perhaps related to the small sample size.

References

Chicu, Mark, Chris Vickers, and Nicolas L. Ziebarth, "Cementing the Case for Collusion Under the National Recovery Administration," *Explorations in Economic History*, 2013, *50*, 487–507.

Taylor, Jason E., "Work Sharing During the Great Depression: Did the 'President's Reemployment Agreement' Promote Reemployment?," *Economica*, 2011, *78*, 133–158.

Vickers, Chris and Nicolas L. Ziebarth, "United States Census of Manufactures, 1929-1935. ICPSR37114-v1," Inter-university Consortium for Political and Social Research 2018.

Table 1: Summary Statistics of the COM Sample

Industry	Employment	Revenue	Wages
Ice cream	6.73	67.13	6.97
Ice, manufactured	5.65	43.38	9.95
Sugar, refining	568.56	17740.80	644.07
Cotton goods	303.86	806.49	206.20
Blast furnaces	124.79	5958.71	160.61
Steel works	554.97	2821.81	656.21
Motor vehicles	779.59	7055.65	841.61
Cigars and cigarettes	844.63	23621.32	512.42

Notes: All statistics are per establishment. Revenue and wages are reported in units of 1000s. Employment is for January 1933. Revenue and wages are for whole year of 1933.

Table 2: Number of Establishments in 1933 by Industry

Industry	Count
Ice cream	1725
Ice, manufactured	3213
Sugar, refining	19
Cotton goods	1048
Blast furnaces	72
Steel works	394
Motor vehicles	122
Cigars and cigarettes	27

Table 3: Modifications to Blanket PRA Workweek Limit for COM Sample

COM Industry	NRA/PRA Industry	NRA Code or PRA Substitution	Date Effective	Weekly Hours Limits	Limit Applied
Cotton Goods	Cotton Textiles	NRA Code 1	7/17/33	40 hours	40
Ice Cream	Ice Cream	PRA Substitution 127-1-02	8/1/33	48 May 1 to September 30; 40 October 1 to April 30	40
Ice Manufacturing	Ice Manufacturing	PRA Substitution ???	8/4/33	Likely same as code. Barkin reports	35
Blast Furnaces and Steel Works	Iron and Steel	NRA Code 11	8/19/33	40 average over 6 months; 48 in week	40
Motor Vehicles	Automobiles	NRA Code 17	9/5/33	35 average with tolerance of 3 percent; 48 in peak period	35
Cigars and Cigarettes	Cigarette, Chewing, and Smoking Tobacco and Snuff	PRA Substitution 1615/21	8/15/33	40 hours with tolerance of 20% in green leaf buying season	40
Cigars and Cigarettes	Cigar Manufacturing	PRA Substitution 1615/13	8/11/1933	45 during 4-month seasonal peak; 40 otherwise; max of 2000 hours for year	40
Sugar Refining	Milk Sugar	PRA Substitution	8/25/33	44 averaged over 12-months, 48 max per week; tolerance of 10 for up to 16 weeks	40
Sugar Refining	Cane Sugar	PRA Substitution	8/19/33	40 averaged over 8 week; 48 in any one week	40
Sugar Refining	Cane Syrup Packing and Mixing	PRA Substitution	9/7/33	40 averaged over 8 week; 48 in any one week	40
Sugar Refining	Beet Sugar	NRA Labor Provision LP1	10/20/33	40 outside beet season; 56 during beet season	40

Notes: NRA Codes, Volume 1, pp. 1-24, 171-184, 253-255, 533-534; vol. 2, pp.687-93; volume 12, pp. 61-70, 660; volume 18, p. 682; volume 19, p. 618; volume 21, pp. 95-106; PRA Substitutions in NRA 1933 Bulleting 6, 1933, pp. 26-7, 34-6, 100-1; Hoover 1936, pp. 100-1, 110. The last column of this table is the workweek limit we apply to this industry for our analyses.

Table 4: The Labor Market Effects of the PRA and NIRA: Balanced Sample

	Workweek (1)	Employment (2)	Manhours (3)	Hourly Earnings (4)	Weekly Earnings (5)	Payroll (6)
ENRA	-0.077	0.112	0.035	0.104	0.017	0.129
	(0.010)	(0.014)	(0.019)	(0.011)	(0.008)	(0.018)
CCNRA	-0.046	0.063	0.016	0.068	0.018	0.081
	(0.006)	(0.009)	(0.011)	(0.005)	(0.004)	(0.010)
EPRA	-0.081	0.106	0.025	0.107	0.010	0.116
	(0.010)	(0.010)	(0.016)	(0.008)	(0.007)	(0.013)
CCPRA	-0.062	0.012	-0.050	0.043	-0.024	-0.012
	(0.012)	(0.026)	(0.035)	(0.013)	(0.016)	(0.034)
IP	0.450	0.314	0.764	-0.140	0.295	0.609
	(0.033)	(0.040)	(0.064)	(0.012)	(0.030)	(0.061)
Observations	3957	3957	3957	3957	3957	3957

Notes: All dependent variables are in logs. The variable ENRA indicates whether the NIRA code was in effect for the industry and before the Compliance Crisis that started in April 1934. This is a period during which it is believed that compliance with the provisions of the PRA and NIRA was laxer. Similarly, the variable EPRA is an indicator for the PRA period before the Compliance Crisis. The variables CCPRA and CCNRA are constructed in the same way with values of 1 for industries where the PRA or NIRA were in effect between April 1934 and May 1935 and values of zero through March 1934 and after May 1935, the month in which the NIRA was declared unconstitutional by the Supreme Court. All regressions include year, month, and industry fixed effects. The variable IP is a measure of monthly US industrial production. Standard errors are clustered at the industry-level. These data were collected by the Statistical Section of the National Recovery Administration and the BLS. They are reported at the industry by month level. The sample covers 115 "sectors" between 1933 and 1935. The sample is smaller for the employment variable because we were only able to recover the level of employment from the indexed value provided in the dataset for about half of the observations. The same problem carries over to the manhours and payroll variables, which are derived from the employment variable. Relative to the paper, these specification use a balanced sample of observations for which none of the dependent variables are missing.

Table 5: The Labor Market Effects of the PRA and NIRA: Employment Levels

	Workweek (1)	Employment (2)	Manhours (3)	Hourly Earnings (4)	Weekly Earnings (5)	Payroll (6)
ENRA	-0.077	0.119	0.052	0.104	0.017	0.128
	(0.010)	(0.023)	(0.031)	(0.011)	(0.008)	(0.027)
CCNRA	-0.046	0.068	0.022	0.068	0.018	0.078
	(0.006)	(0.010)	(0.015)	(0.005)	(0.004)	(0.012)
EPRA	-0.081	0.107	0.028	0.107	0.009	0.111
	(0.010)	(0.013)	(0.021)	(0.008)	(0.007)	(0.016)
CCPRA	-0.062	0.003	-0.087	0.043	-0.030	-0.023
	(0.012)	(0.035)	(0.054)	(0.013)	(0.013)	(0.040)
IP	0.450	0.279	0.689	-0.140	0.287	0.503
	(0.033)	(0.048)	(0.081)	(0.012)	(0.029)	(0.074)
Observations	3957	1942	1789	3957	4138	1942

Notes: All dependent variables are in logs. The variable ENRA indicates whether the NIRA code was in effect for the industry and before the Compliance Crisis that started in April 1934. This is a period during which it is believed that compliance with the provisions of the PRA and NIRA was laxer. Similarly, the variable EPRA is an indicator for the PRA period before the Compliance Crisis. The variables CCPRA and CCNRA are constructed in the same way with values of 1 for industries where the PRA or NIRA were in effect between April 1934 and May 1935 and values of zero through March 1934 and after May 1935, the month in which the NIRA was declared unconstitutional by the Supreme Court. All regressions include year, month, and industry fixed effects. The variable IP is a measure of monthly US industrial production. Standard errors are clustered at the industry-level. These data were collected by the Statistical Section of the National Recovery Administration and the BLS. They are reported at the industry by month level. The sample covers 115 "sectors" between 1933 and 1935. The sample is smaller for the employment variable because we were only able to recover the level of employment from the indexed value provided in the dataset for about half of the observations. The same problem carries over to the manhours and payroll variables, which are derived from the employment variable.

Table 6: The Labor Market Effects of the PRA and NIRA: "Fully Balanced" Sample

	Workweek (1)	Employment (2)	Manhours (3)	Hourly Earnings (4)	Weekly Earnings (5)	Payroll (6)
ENRA	-0.077	0.129	0.052	0.091	0.007	0.136
	(0.013)	(0.024)	(0.031)	(0.015)	(0.011)	(0.029)
CCNRA	-0.050	0.072	0.022	0.062	0.009	0.080
	(0.008)	(0.011)	(0.015)	(0.007)	(0.006)	(0.013)
EPRA	-0.087	0.115	0.028	0.109	0.002	0.117
	(0.013)	(0.014)	(0.021)	(0.012)	(0.010)	(0.019)
CCPRA	-0.073	-0.014	-0.087	0.048	-0.021	-0.035
	(0.014)	(0.043)	(0.054)	(0.019)	(0.018)	(0.053)
IP	0.394	0.296	0.689	-0.140	0.228	0.524
	(0.040)	(0.053)	(0.081)	(0.016)	(0.040)	(0.081)
Observations	1789	1789	1789	1789	1789	1789

Notes: All dependent variables are in logs. The variable ENRA indicates whether the NIRA code was in effect for the industry and before the Compliance Crisis that started in April 1934. This is a period during which it is believed that compliance with the provisions of the PRA and NIRA was laxer. Similarly, the variable EPRA is an indicator for the PRA period before the Compliance Crisis. The variables CCPRA and CCNRA are constructed in the same way with values of 1 for industries where the PRA or NIRA were in effect between April 1934 and May 1935 and values of zero through March 1934 and after May 1935, the month in which the NIRA was declared unconstitutional by the Supreme Court. All regressions include year, month, and industry fixed effects. The variable IP is a measure of monthly US industrial production. Standard errors are clustered at the industry-level. These data were collected by the Statistical Section of the National Recovery Administration and the BLS. They are reported at the industry by month level. The sample covers 115 "sectors" between 1933 and 1935. The sample is smaller for the employment variable because we were only able to recover the level of employment from the indexed value provided in the dataset for about half of the observations. The same problem carries over to the manhours and payroll variables, which are derived from the employment variable. This specification use a "fully balanced" sample for which none of the dependent variables are missing and we are able to deindex the employment variable.

Table 7: The Labor Market Effects of the PRA and NIRA Using Hourly Earnings as a Control

	Workweek (1)	Employment (2)	Manhours (3)
ENRA	-0.015	0.143	0.128
	(0.010)	(0.021)	(0.026)
CCNRA	-0.006	0.083	0.077
	(0.007)	(0.013)	(0.015)
EPRA	-0.018	0.139	0.121
	(0.010)	(0.015)	(0.019)
CCPRA	-0.036	0.025	-0.011
	(0.012)	(0.027)	(0.036)
IP	0.367	0.271	0.638
	(0.032)	(0.043)	(0.066)
Hourly Earnings	-0.594	-0.304	-0.898
	(0.056)	(0.103)	(0.132)
Observations	3957	3957	3957

Notes: All dependent variables are in logs. The variable ENRA indicates whether the NIRA code was in effect for the industry and before the Compliance Crisis that started in April 1934. This is a period during which it is believed that compliance with the provisions of the PRA and NIRA was laxer. Similarly, the variable EPRA is an indicator for the PRA period before the Compliance Crisis. The variables CCPRA and CCNRA are constructed in the same way with values of 1 for industries where the PRA or NIRA were in effect between April 1934 and May 1935 and values of zero through March 1934 and after May 1935, the month in which the NIRA was declared unconstitutional by the Supreme Court. All regressions include year, month, and industry fixed effects. The variable IP is a measure of monthly US industrial production. These data were collected by the Statistical Section of the National Recovery Administration and the BLS. They are reported at the industry by month level. The sample covers 115 "sectors" between 1933 and 1935. Standard errors are clustered at the industry-level.

Table 8: Effect of Workweek Limits on Earnings Using Pre-PRA Workweek Length as Treatment

	Hourly Earnings		Weekly Earnings		Payroll	
	(1)	(2)	(3)	(4)	(5)	(6)
PRA	-0.554		1.322		2.027	
	(0.176)		(0.155)		(0.405)	
Pre-PRA Workweek Length	-0.571		0.371		0.480	
	(0.140)		(0.148)		(0.161)	
PRA $\times$ Pre-PRA Workweek Length	0.192	0.191	-0.339	-0.339	-0.470	-0.470
	(0.049)	(0.049)	(0.042)	(0.042)	(0.112)	(0.112)
Month	No	Yes	No	Yes	No	Yes
Industry	No	Yes	No	Yes	No	Yes
Observations	641	641	642	642	642	642

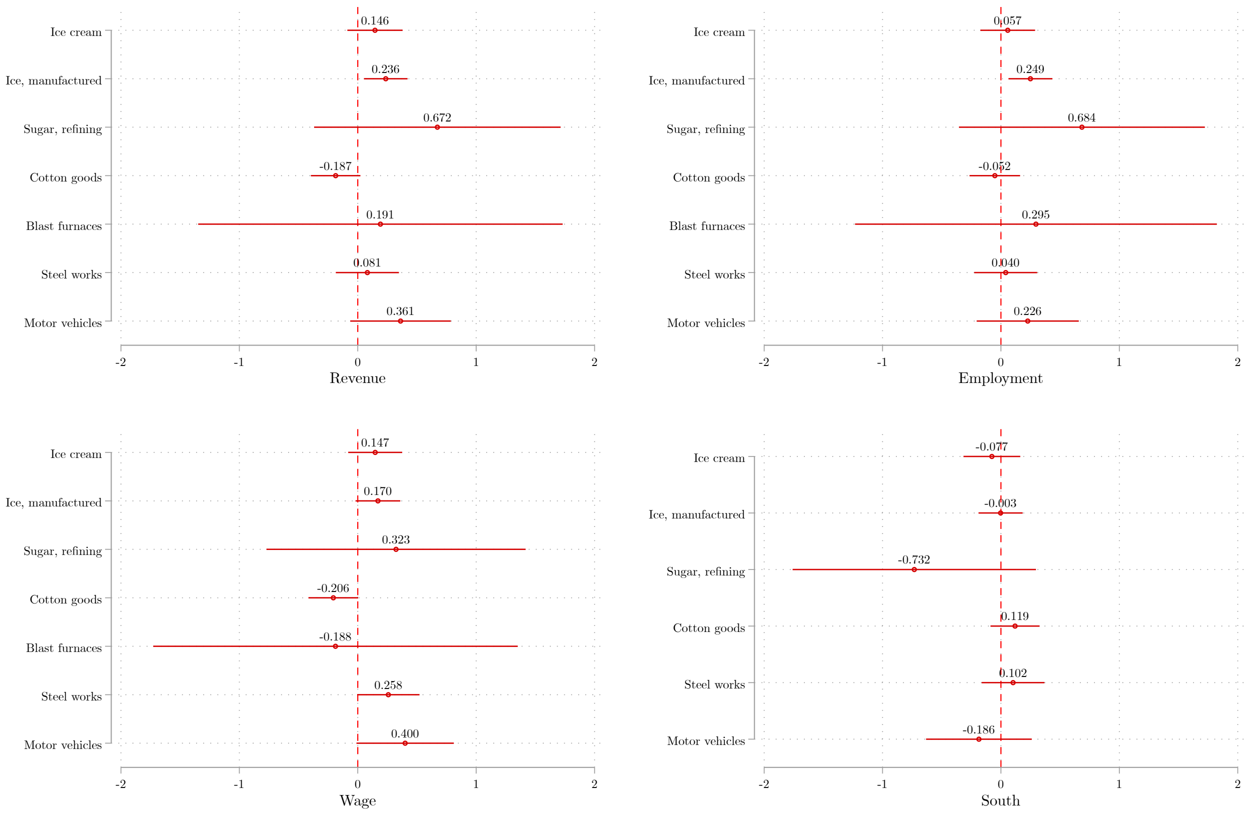
Notes: All dependent variables are in logs. Only data from April through October 1933 excluding July are included. The PRA variable is an indicator for the months of August through October. The variable "Pre-PRA Workweek Length" is the log of an industry's average workweek in the 3 months before the PRA. These data were collected by the Statistical Section of the National Recovery Administration and the BLS. They are reported at the industry by month level. The sample covers 115 "sectors" between 1933 and 1935. Standard errors are clustered at the industry-level.

Table 9: Effect of the Workweek Limits on Earnings: NICB Sample

	Hourly Earnings		Weekly Earnings	
	(1)	(2)	(3)	(4)
PRA	0.040		0.185	
	(0.028)		(0.057)	
Pre-PRA Workweek	-0.279		0.247	
	(0.127)		(0.193)	
PRA × Pre-PRA Workweek	0.158	0.158	-0.096	-0.096
	(0.057)	(0.057)	(0.096)	(0.098)
Month	No	Yes	No	Yes
Industry	No	Yes	No	Yes
Observations	132	132	66	66

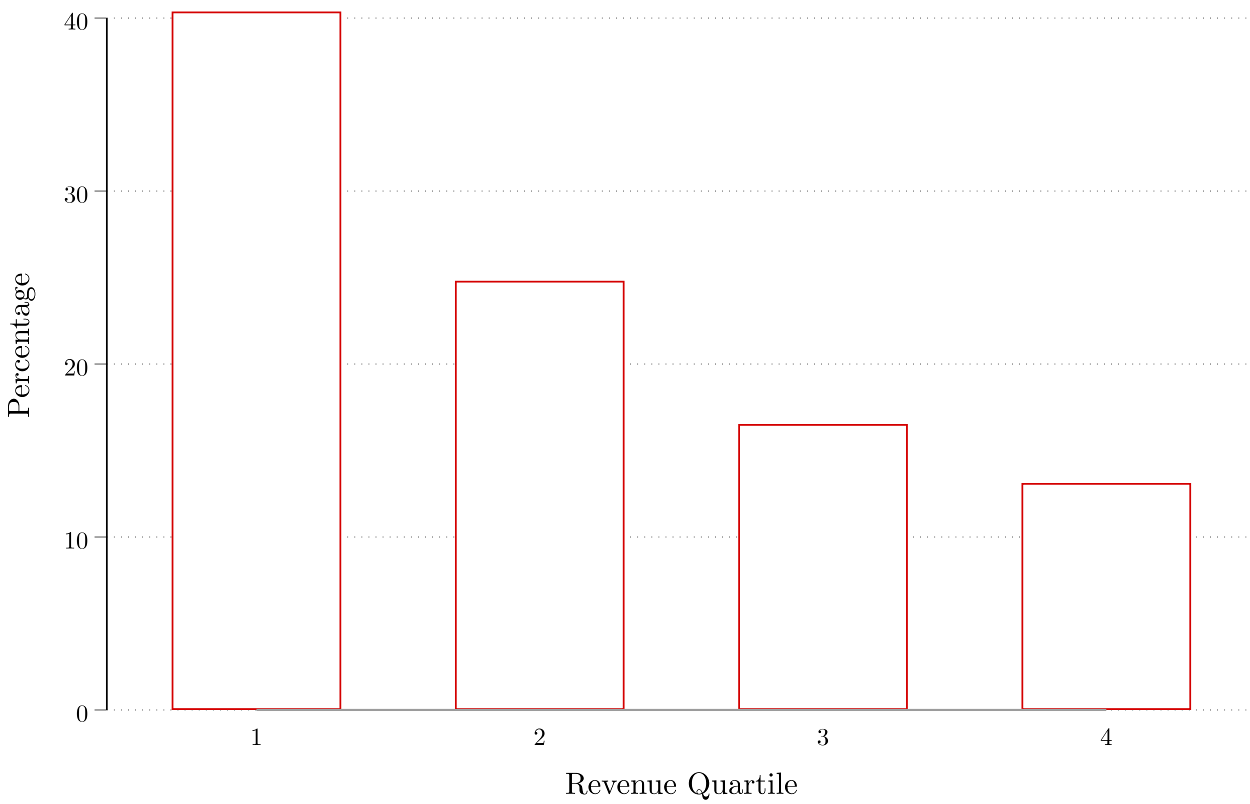
Notes: All dependent variables are in logs. Only data from April through October 1933 excluding July are included. The pre-PRA period is defined as the months April through June while the PRA period is August through October. The PRA goes into effect in the middle of July so we drop this month. The variable "Pre-PRA Workweek" is the fraction of months the industry's workweek was above 35 hours before the PRA. These data were collected by the National Industrial Conference Board and are reported at the industry-level. Standard errors are clustered at the industry-level.

Figure 1: Differences Between Establishments Just Above and Below the Limit: 4 Hour Bandwidth



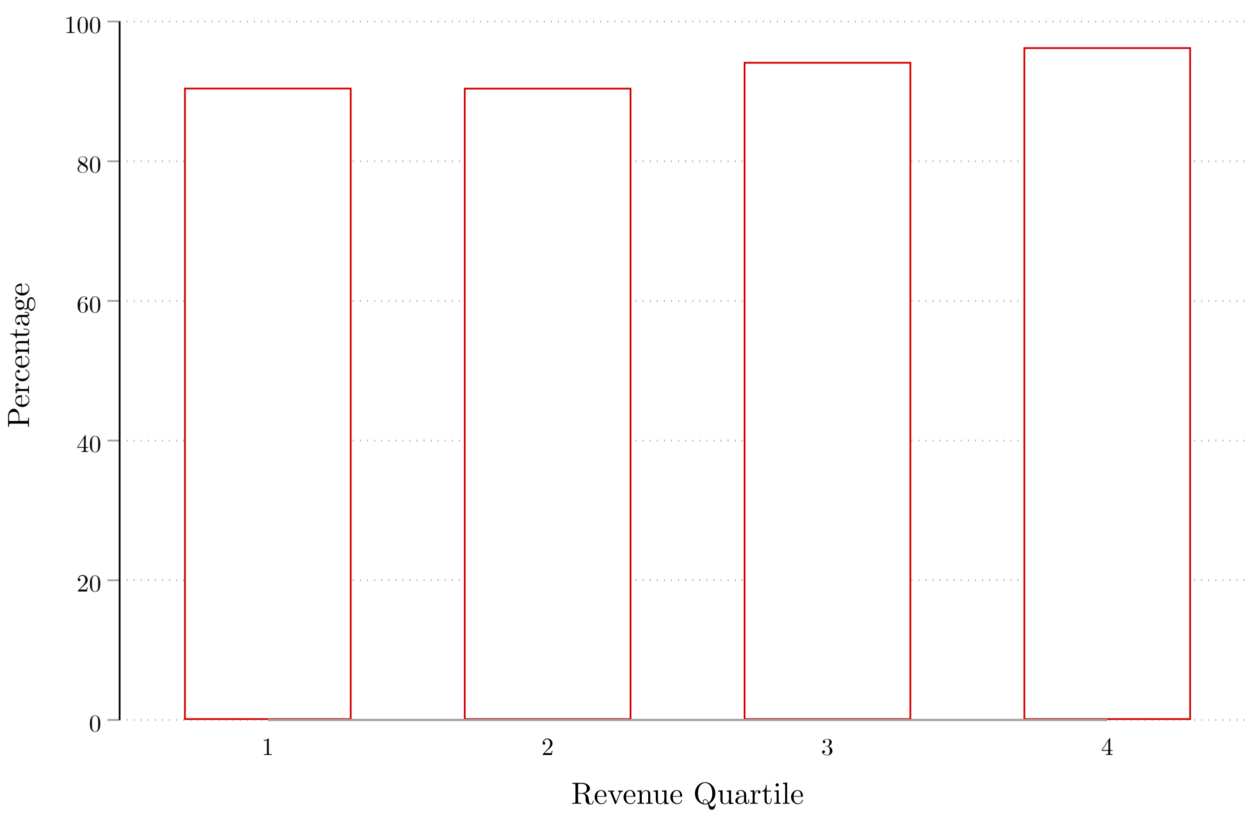
Notes: We report the difference between the means for those at and just above to those just below the limit before the PRA in units of the standard deviation of the dependent variable. The sample is restricted to those at the limit. We define those establishments "at limit" as those with a workweek of within 4 hours of the PRA workweek limit. We do not include the Cigars and Cigarettes industry because the estimates are noisy. All variables are in logs except for South, which is an indicator. These data were collected from the Census of Manufactures and are reported at the establishment-level. Establishments are weighted by employment. Standard errors are robust.

Figure 2: Percentage Missing Imputed Workweek by Revenue Quartile



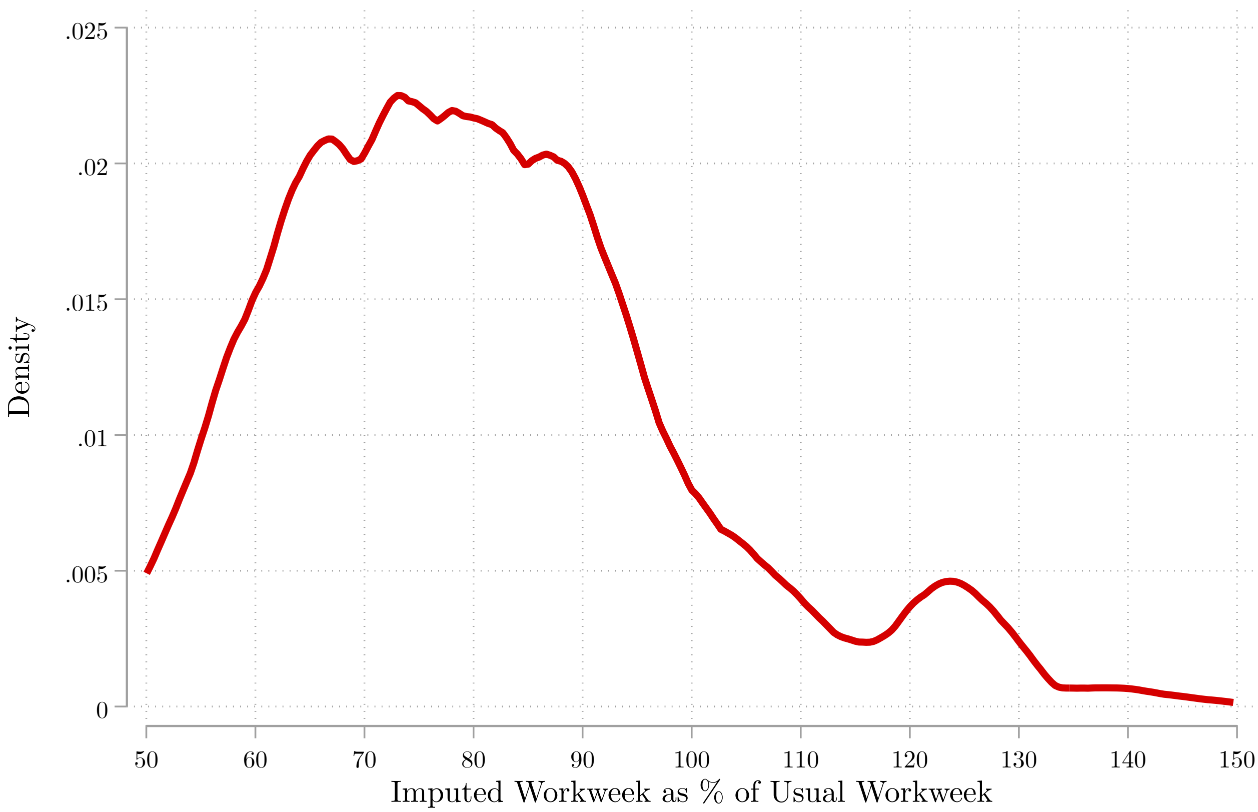
Notes: The imputed workweek is calculated as total manhours divided by total number of hourly employees. Establishments are weighted by employment. These data were collected from the Census of Manufactures and are reported at the establishment-level.

Figure 3: Percentage Valid Imputed Workweek by Revenue Quartile



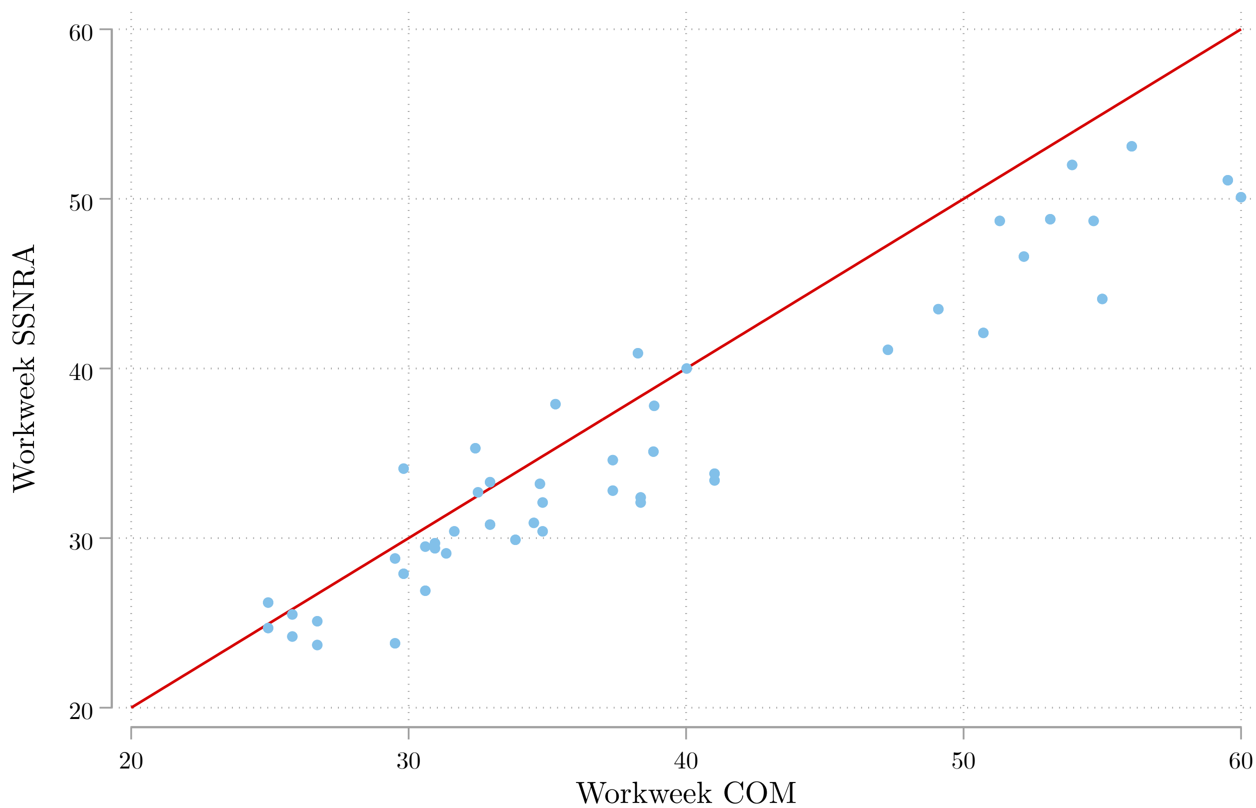
Notes: We define an imputed workweek as valid if it is between 10 and 100 hours. Establishments are weighted by employment. These data were collected from the Census of Manufactures and are reported at the establishment-level.

Figure 4: Distribution of Difference Between Imputed and Usual Workweek



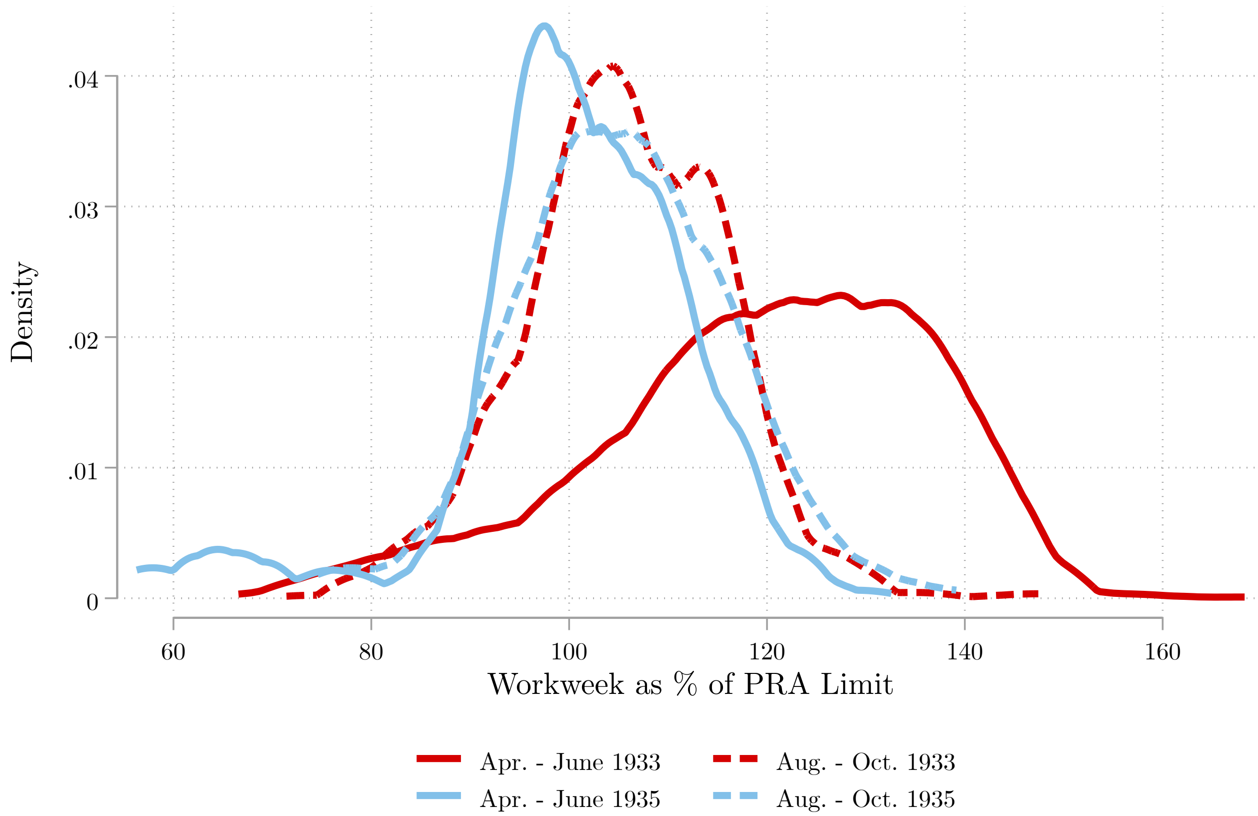
Notes: The imputed workweek is for the month of December 1933, which is the reference month for the usual workweek. The sample is restricted to those establishments with a valid imputed and a reported workweek. Establishments are weighted by employment. These data were collected from the Census of Manufactures and are reported at the establishment-level.

Figure 5: Workweek in Overlapping SSNRA and COM Industries



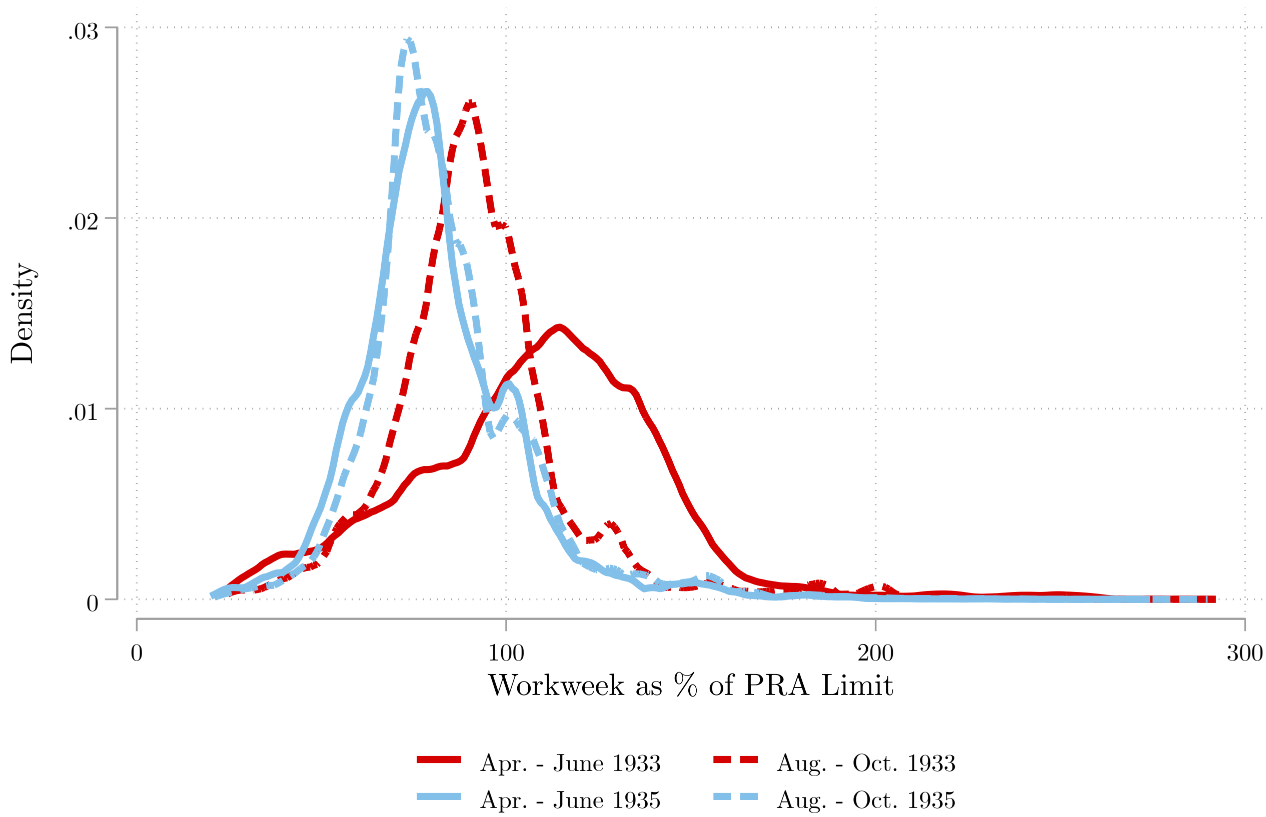
Notes: For the COM data, each point represents the (equally weighted) median of all establishments in a given industry-month from 1933. The red line is the 45 degree line. The set of industries include the following pairs where the first listed is in the SSNRA and the second COM: (1)"Cigar", "Cigars and Cigarettes"; (2) "Cigarette, Snuff, Chewing and Smoking Tobacco", "Cigars and Cigarettes"; (3) "Structural Steel and Iron Fabricating", "Steel works"; (4) "Steel Casting", "Steel works"; (5) "Ice Cream Manufacturing", "Ice cream"; and (6) "Automobile Manufacturing", "Motor vehicles".

Figure 6: Distribution of Workweek Relative to Workweek Limit: SSNRA Sample



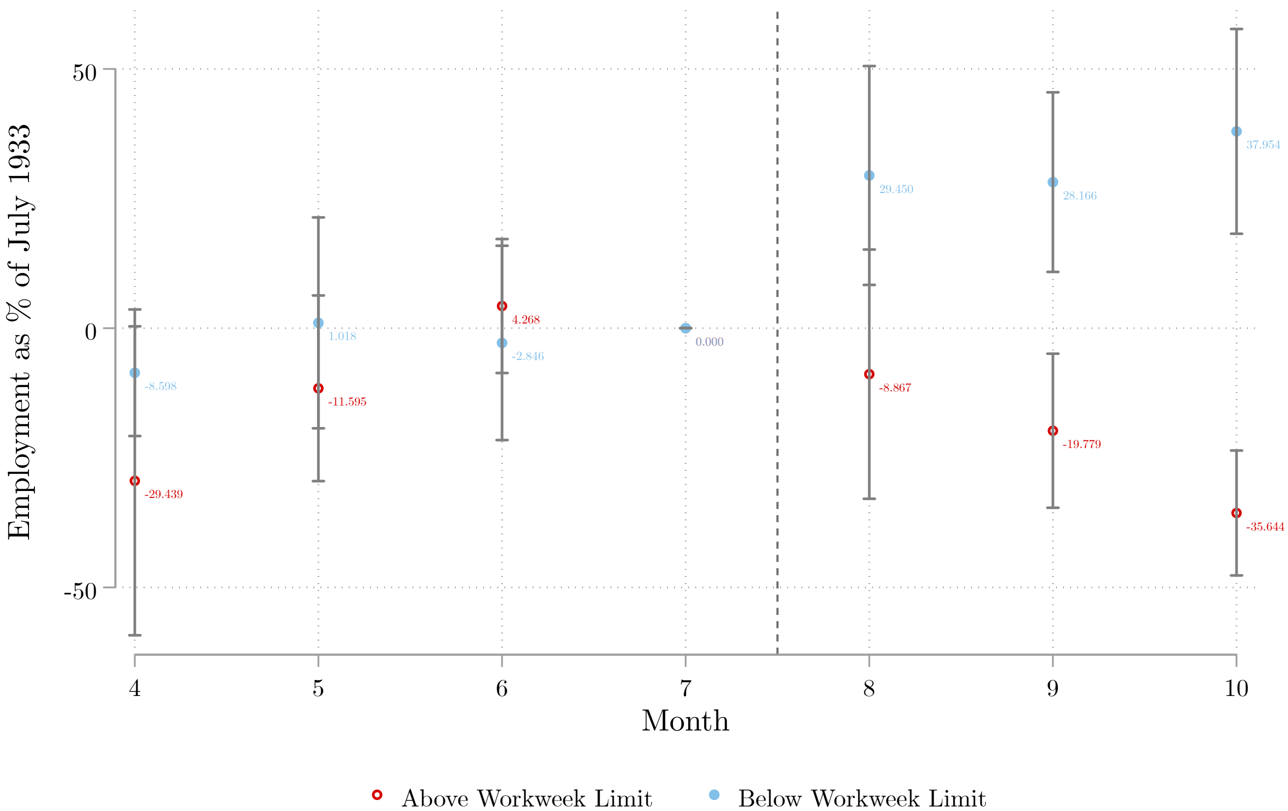
Notes: Workweeks are scaled by the industry-specific workweek limit specified by the PRA. These data were collected by the Statistical Section of the National Recovery Administration and the BLS. They are reported at the industry by month level. The sample covers 115 "sectors" between 1933 and 1935. Establishments are weighted by employment.

Figure 7: Distribution of Workweek Relative to Workweek Limit: 6 Day Workweek



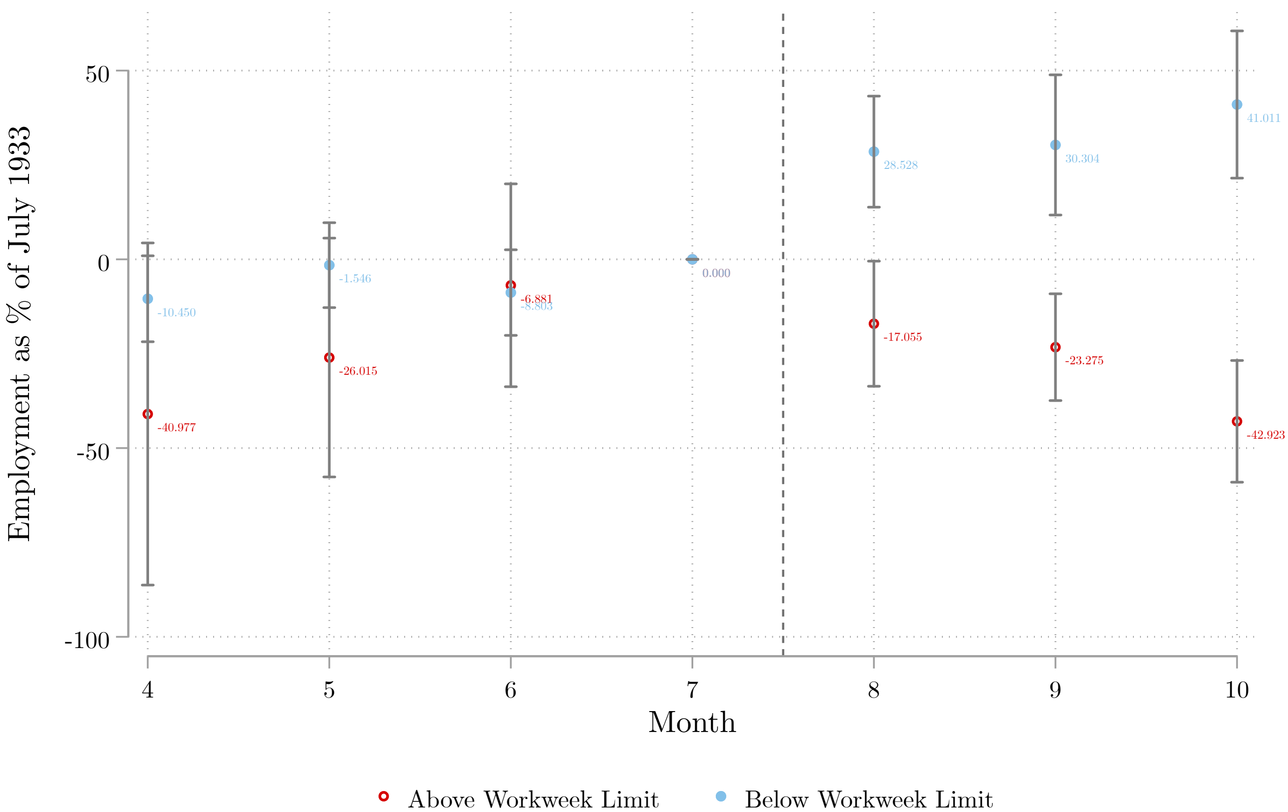
Notes: We assume a 6 day workweek when calculating the hours worked per week. Workweeks are scaled by the industry-specific workweek limit specified by the PRA. These data were collected from the Census of Manufactures and are reported at the establishment-level. Establishments are weighted by employment.

Figure 8: Effects of Workweek Limit on Employment:
5 Hour Bin Bandwidth



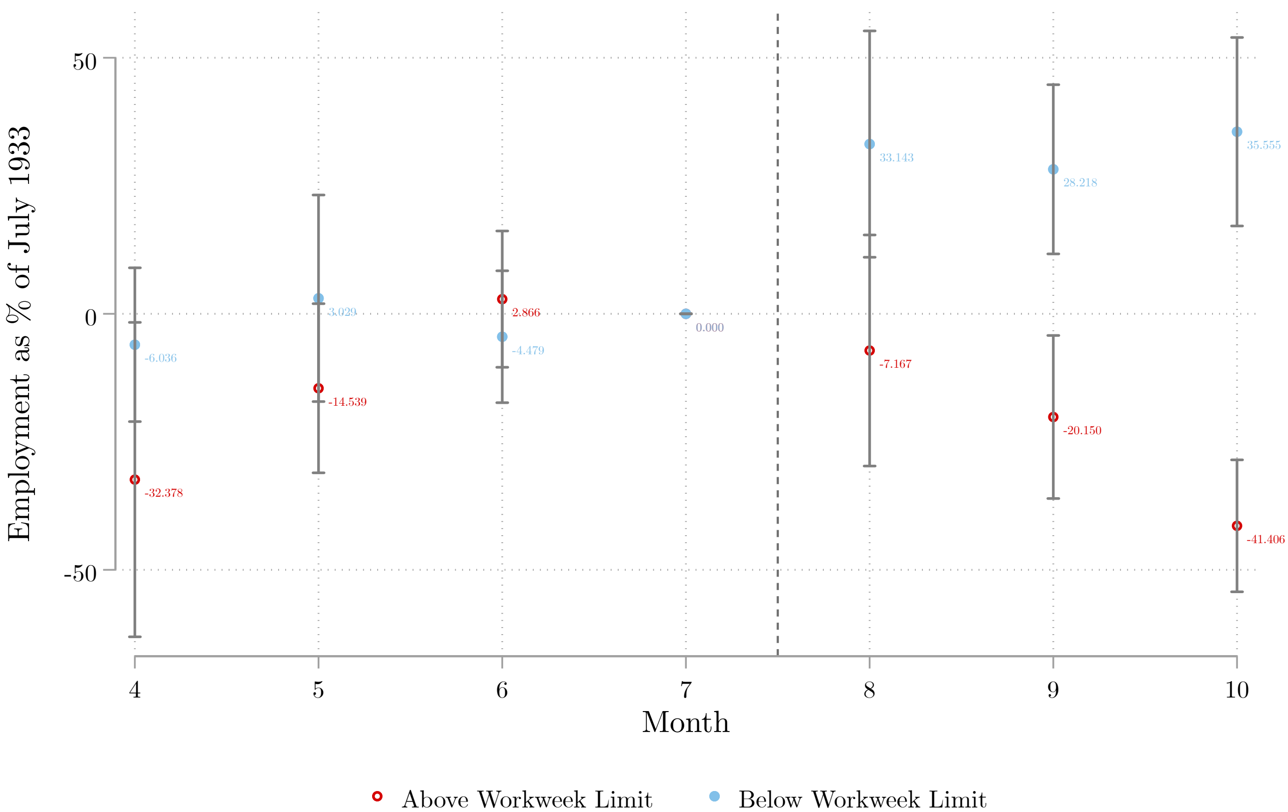
Notes: These data were collected from the Census of Manufactures and are reported at the establishment-level. The lower threshold for the workweek is 25 hours below the workweek limit, or a workweek of 10 hours. The upper threshold is 40 hours above the limit, or a workweek of 75 hours. We include workweek bin by industry and time period by industry fixed effects. Employment is normalized by employment in July 1933. Rather than using a 4 hour bandwidth to define the bins as in the paper, we use a 5 hour bandwidth. Standard errors are clustered at the industry-level.

Figure 9: Effects of Workweek Limit on Employment:
Upper Bound of 50 Hours Above Limit



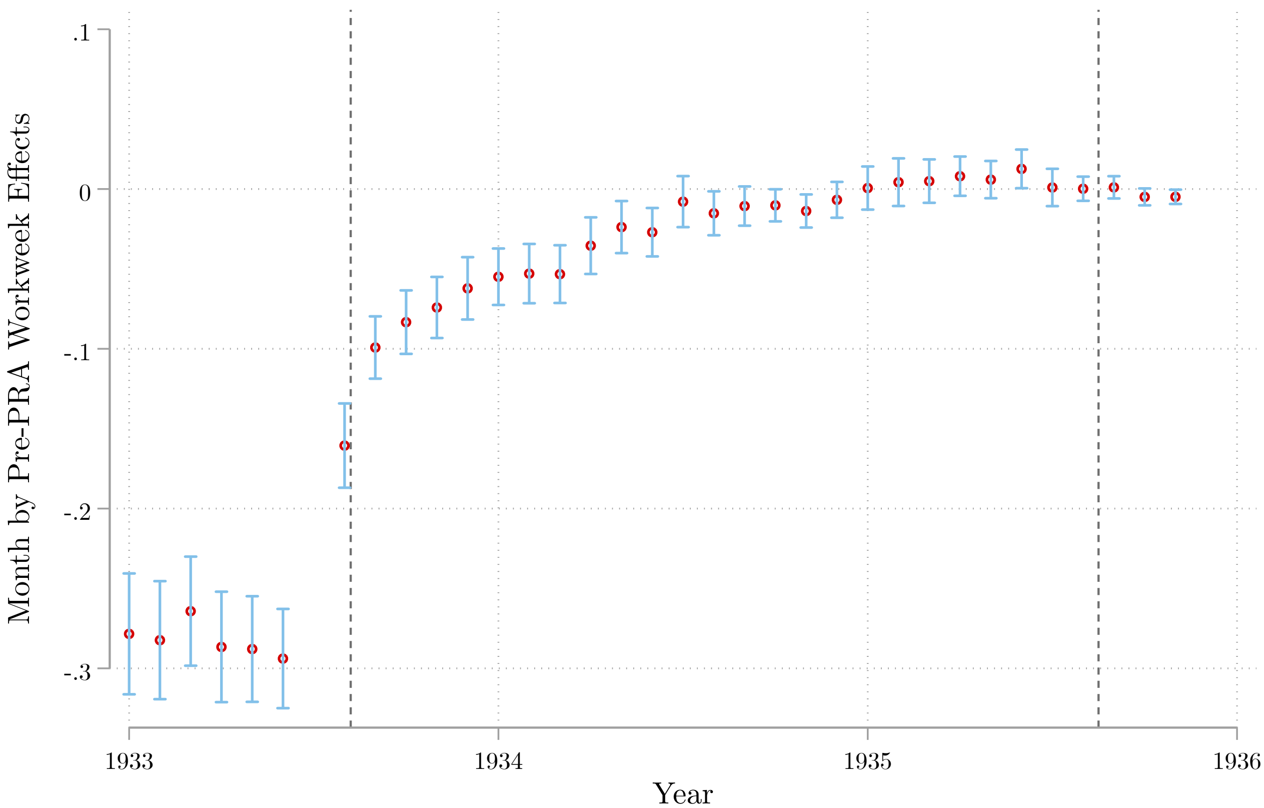
Notes: These data were collected from the Census of Manufactures and are reported at the establishment-level. The workweek bin size is 4 hours. The lower threshold for the workweek is 25 hours below the industry's workweek limit. Rather than the 40 hours above the limit upper threshold used in the paper, we use an upper threshold of 50 hours above the industry's workweek limit. We include workweek bin by industry and time period by industry fixed effects. Employment is normalized by employment in July 1933. Standard errors are clustered at the industry-level.

Figure 10: Effects of Workweek Limit on Employment:
Lower Bound of 15 Hours Below Limit



Notes: These data were collected from the Census of Manufactures and are reported at the establishment-level. The workweek bin size is 4 hours. Rather than the 10 hours below the limit lower threshold used in the paper, we use a lower threshold for the workweek of 15 hours below the industry's workweek limit. The upper threshold is 40 hours above the industry's workweek limit. We include workweek bin by industry and time period by industry fixed effects. Employment is normalized by employment in July 1933. Standard errors are clustered at the industry-level.

Figure 11: Event Study of Effects of PRA on Hourly Earnings



Notes: Besides including interactions between month and the Pre-PRA Workweek, whose estimates are reported in this figure, the specification also includes month-by-year and industry fixed effects. These data were collected by the Statistical Section of the National Recovery Administration and the BLS. They are reported at the industry by month level. The sample covers 115 "sectors" between 1933 and 1935. Standard errors are clustered at the industry-level. The first dashed line marks the start of the PRA and the second the *Schechter* decision invalidating the NIRA.