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Labor Market Effects of Workweek Restrictions: Evidence from the Great Depression†

By Price Fishback, Chris Vickers, and Nicolas L. Ziebarth*

We study the effects of restrictions on the length of the workweek under the President's Reemployment Agreement (PRA) of July 1933 and the National Industrial Recovery Act. We construct a model in which the equilibrium without such a workweek restriction has an inefficiently low level of employment. We find that employment rose by about 24 percent in the month following the imposition of the workweek restriction. Industries with longer workweeks pre-PRA experienced 9.4 percent faster growth in hourly earnings post-PRA, but this increase was not sufficient to prevent a relative fall in weekly earnings in these industries. (*JEL* E24, E32, J22, J31, N12, N31)

As the Great Depression deepened, unemployment rose to over 25 percent by 1933, and average hours worked per week in manufacturing industries surveyed by the National Industrial Conference Board (NICB) fell from around 48 hours in 1929 to lows of around 32 in summer 1932 and March 1933. Starting in March 1933, the same month Franklin D. Roosevelt was inaugurated, the labor market experienced a rapid recovery as employment rose 26 percent and average weekly hours rose to 43 hours in just a 3-month period. Yet over the rest of the year, average weekly hours fell back to 33.8 hours, close to their 1932 low, while employment continued to rise (Taylor and Neumann 2016). As a consequence, 1933 ended up being the only year in the 1930s when employment and weekly hours moved in opposite directions (see Figure 1).

One potential explanation for the puzzle of 1933 is the policy combination of the National Industrial Recovery Act (NIRA) and the President's Reemployment Agreement (PRA) of late July 1933. The PRA summarized the Roosevelt administration's goal in its title, as it was designed to stimulate reemployment by specifying a maximum workweek of 35 hours.[1] Responding to a huge government publicity campaign that emphasized patriotic duty, about 85 percent of establishments had signed

* Fishback: Department of Economics, University of Arizona, and NBER (email: fishback@arizona.edu). Vickers: Department of Economics, Auburn University (email: czvickers@gmail.com). Ziebarth: Department of Economics, Auburn University, and NBER (email: nicolas.ziebarth@me.com). Ayşegül Şahin was coeditor for this article. Josh Hausman, Taylor Jaworski, and Jason Taylor as well as workshop participants at Auburn University, UTEP, and the SEA 2022 meetings provided useful comments. The National Science Foundation (SES #1122509 and #1459263) and the University of Iowa provided funding for data collection.

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[1] As we discuss below, there were some industry-specific differences in the limit.

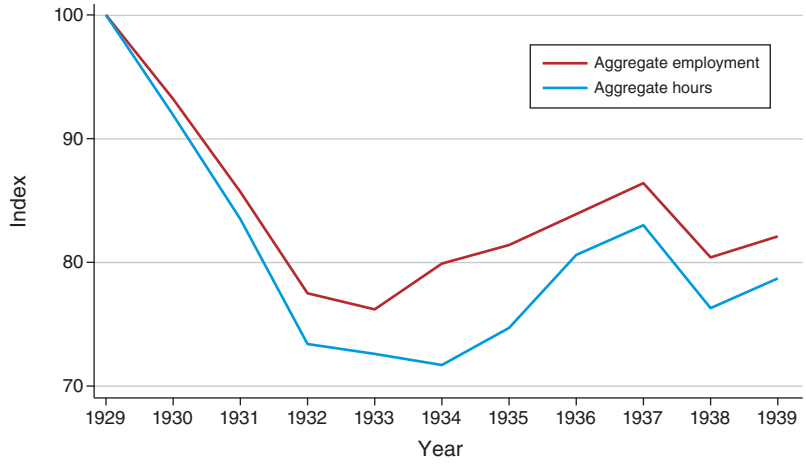

Figure 1. Changes in Aggregate Labor Input During the Great Depression

Note: These data are from Table 5 of Cole and Ohanian (1999) with the original source in Kendrick (1961).

the agreement within a couple of months of its introduction (Taylor 2011, 138). The administration's ultimate hope was that the workweek limit would not just lead to increased employment but, combined with wage minimums, would also increase the "purchasing power" of workers as a whole. We study the labor market consequences of this large-scale experiment in a form of work sharing.[2]

Despite all of the emphasis in government documents, Congressional debates, and the language of the policies themselves on cutting weekly hours as a way to raise employment, influential work by macroeconomists such as Cole and Ohanian (2004) and Eggertsson (2008) has instead focused on the "high wage" aspect of New Deal labor market policies. Economic historians studying the labor markets of the 1930s, such as Taylor and Neumann (2016), Neumann et al. (2013), and Hanes (2020), discuss the workweek limits but also tend to focus their discussions much more heavily on New Deal wage (and price) policy. The problem is that, absent the shift from deflationary to inflationary expectations emphasized by Eggertsson (2008), there is simply no logical way for high wage policies to lead to the rise in employment and the drop in weekly hours seen after July 1933.

To organize our empirical analyses, we extend the model of employment, the workweek, and earnings developed by Bernanke (1986) to explain why weekly hours fell and hourly earnings held firm following the introduction of these workweek restrictions. In the model, households make a decision between working and being unemployed. Firms offer contracts specifying a level of utility to the households determined by an amount of earnings and a workweek length. Because households always have an outside option of unemployment, there is a limit on the ability

[2]During the 1930s, a number of countries besides the United States, including Czechoslovakia, France (Cohen-Setton et al. 2017), Italy (Mattesini and Quintieri 2006), and New Zealand implemented similar workweek restrictions.

of firms to continue cutting total earnings as the workweek is shortened. As a consequence, quoting Bernanke (1986), who, in turn, cites Daughterty et al. (1937), "if [households] could not attain a subsistence level . . . they would be forced to try elsewhere. Thus real hourly earnings [would rise] as the workweek was cut" (89).

Firms differ in their (exogenous) production elasticity with respect to the workweek, which generates a distribution of the workweek. After the workweek restriction is imposed, firms with a workweek greater than the limit are required to cut the workweek for their employees. As a consequence, these firms raise their employment demand, which pushes up employment demand overall. Therefore, even firms not directly affected by the workweek limit are affected through general equilibrium effects that cause them to *reduce* their employment. As a consequence, aggregate employment rises in response to a workweek limit but by less than the amount suggested by a "lump of labor" theory that assumes a unit elasticity at the firm level between the workweek and employment.

The model makes predictions for the effects of workweek limits on labor earnings that we test. First, weekly earnings per worker will decline since, for a fixed level of utility, employers can substitute between hours and earnings. Whether earnings per *hour* falls at a given firm depends on that firm's production elasticity with respect to hours. Firms with longer workweeks ex ante are more likely to experience a decline in earnings per hour following the workweek limit. Finally, while the aggregate "purchasing power" of employed households, which is what the Roosevelt administration focused on, defined as earnings per worker times the number employed, falls, this decline is common to all firms.

One interesting feature of the model is that the equilibrium in which there is no workweek limit is inefficient. The reason for this is that firms act like monopsonists in the employment market. Although the firms choose an efficient level of the workweek (at least in the case when the production function is Cobb-Douglas), firms employ an inefficiently low number of people because a firm must adjust the earnings for each of its workers when hiring the marginal worker. A workweek limit (partially) alleviates this underemployment problem but at the cost of distorting the workweek choices of firms. As a consequence, the equilibriums with and without a workweek restriction cannot be Pareto ranked.

We draw on two datasets to measure the effects of these workweek restrictions on the labor market. First, data for eight industries at the establishment level from the Census of Manufactures (COM) in 1933 and 1935 provide information on monthly employment and hours worked, allowing us to calculate each establishment's average weekly hours per worker. Unfortunately, the establishment-level data do not provide earnings by month. Therefore, we turn to monthly industry-level data on 115 sectors collected by the Bureau of Labor Statistics and the statistical sectors of the National Recovery Administration (SSNRA), an agency set up to administer the NIRA, from 1933 to 1935.[3]

We first show that these workweek limits affected the workweek by identifying a "bunching" in the establishment-level distribution of the workweek starting in

[3] We use an additional dataset for 27 industries from 1924 through 1936 collected by the NICB as a check on these other datasets.

August 1933. For example, in the steel works industry, the fraction of establishments with a workweek within two hours of the maximum increases by a factor of 2.5 between July and September 1933. In the blast furnace industry, nearly every establishment had a workweek well above the maximum in July. By the end of the year, only 15 percent had workweeks close to the maximum. This bunching likely was not a result of seasonality because there was no increase in bunching in 1935 over the same calendar period.

We argue that this bunching analysis provides a much sharper test of the effects of the workweek limit relative to existing literature. For example, Taylor (2011), Neumann et al. (2013), Taylor and Neumann (2013), Taylor (2019), and Hanes (2020) have all studied the effects of New Deal policies including the PRA on the labor market. The basic empirical strategy in all these papers is to estimate a regression that includes a fixed effect for the PRA period. The coefficient on this fixed effect is interpreted as the causal effect of the PRA. The problem with such an approach is that some other aggregate shock might have occurred at the same time, confounding the causal effect of these policies.

Motivated by our analysis of the distribution of the workweek, we use a bunching estimator originally developed to study the effects of the minimum wage (Cengiz et al. 2019) to estimate the (local) effects of the workweek limit on employment. The idea of this estimator is to add up jobs "lost" above the workweek limit with jobs "gained" (just) below the limit to estimate the total effect on employment. If all the workweek limit did was cause firms with workweeks initially longer than the limit to cut their workweeks but did not affect their employment, we would observe a decline in employment above the limit. However, this decline would be exactly equal (in magnitude) to the increase in employment below the limit resulting in an aggregate employment effect of 0.

We find a net increase in employment in August of about 24 percent relative to July 1933 employment. This overall gain comes from a large decline in employment above the workweek limit offset by an even larger gain in employment just below the limit. This effect on aggregate employment persists through October, though the employment gains fall to about 13 percent of July 1933 employment. Interestingly, this fall in the employment gains is mainly driven by an increase in the jobs lost above the limit, which we interpret as evidence that it took time for businesses to sign up and implement the workweek provisions. This empirical strategy, by construction, only captures "local" effects of the workweek limit and implicitly assumes that establishments with a workweek far below the limit were unaffected by the PRA. Our model, on the other hand, because of general equilibrium effects, implies that establishments far from the limit are also negatively affected. As a consequence, we interpret these estimates as upper bounds on the rise in employment due to the workweek limit. At the same time, these results are broadly in line with some of the claims made by the Roosevelt administration in a 1937 report to Congress.

Besides getting people reemployed, the Roosevelt administration also cared about raising the total earnings of workers. So in our final set of analyses, we estimate the effects of the workweek limit on earnings per hour and per week using industry-level data. A testable implication of our model is that hourly earnings at firms experiencing larger percentage declines in the workweek due to the

PRA should rise relative to those experiencing smaller declines in the workweek. We test this prediction by estimating a difference-in-difference specification that includes an interaction between a dummy for the PRA period and the fraction of months in three months before July 1933 that an industry's workweek was above the workweek limit. The assumption is that those with longer workweeks before the PRA would have had to cut their workweek more than those with shorter pre-PRA workweeks. Consistent with the model, following July 1933 and the start of the PRA, hourly earnings rose 9.4 percent in an industry that had a workweek above the limit before the PRA relative to an industry that had a workweek below the limit. This rise in earnings per hour was not sufficient to offset the direct fall in hours, and, as a consequence, weekly earnings fell in industries with longer workweeks before the PRA relative to those with shorter workweeks by 13.8 percent. This result is also consistent with our model since firms offset shorter workweeks with lower earnings.

I. Historical Background

A. *The Hoover Administration*

The political push to reduce weekly hours with the goal of increasing employment in response to the unemployment crisis of the Great Depression did not begin with Roosevelt and the New Deal. In fact, Herbert Hoover, starting in late 1929, actively encouraged voluntary actions on the part of businesses to reduce weekly hours, reemploy workers, and maintain or increase hourly earnings. Some of these "nudges" on the part of the Hoover administration included "jawboning" leading businessmen at a meeting in November 1929, setting up the President's Emergency Committee for Employment in October 1930, and developing a Share-the-Work committee in August 1932. Hoover (1952, 42) referred to work sharing as a type of "indirect relief" for unemployment – contrasting it with "direct relief" whereby funds are provided by a private charity or government. However, Hoover was never willing to put legal force behind this policy of work sharing.

Support for work sharing as a policy response was mixed. Harvard economist Sumner Slichter accused the Hoover Administration of effectively putting the burden of unemployment on workers, who had to accept lower take-home pay while shielding wealthy industrialists from higher taxes to fund unemployment relief payments (Bernstein 1969, 311). On the other hand, union leaders supported a shorter workweek while pressing to keep weekly earnings unchanged. At various times, major companies including Standard Oil of New Jersey, Kellogg, US Steel, American Telephone and Telegraph, Bethlehem Steel, DuPont, General Electric, General Motors, Westinghouse, International Harvester, Socony-Vacuum, and Metropolitan Life (Bernstein 1969, 196, 311, 476, 479) pledged to cut hours to maintain employment.

Employers expressed a variety of reasons for their willingness to adopt these policies. Some hoped to keep their most productive workers to be able to respond more quickly when times improved. Others pointed to the "insurance" benefits of work sharing. For example, Standard Oil of New Jersey President Walter Teagle, who headed Hoover's Share-the-Work Committee, "argued that in a famine 'no

one would suggest feeding two-thirds of the distressed people and letting the other third starve. The available food is rationed [and this] is a partial famine in work'" (Weinberg 1968, 247). L.C. Walker, vice chairman of Hoover's Share-the-Work committee, said that sharing work among taxpaying employees was a better option for firms than having to finance a "dole" (Bernstein 1969, 479). In early 1933, a survey by the Commerce Department suggested that 80 percent of the nation's employers had adopted some form of work sharing and that one quarter of those Americans employed owed their situation to this policy (Bernstein 1969, 479). However, few of the firms appeared willing to cut hours to hire new additional people (Barkin 1936, 5). Rose (2010) finds limited evidence that firms and trade associations that attended Hoover's conference where this request was made were any slower to cut wages, for example.

B. *The Roosevelt Administration*

Roosevelt's landslide electoral victory in November 1932 did not signal a change in the interest in shortening the workweek. Instead, it signaled a change in the possibility of shortening the workweek through legislative action. Just after Roosevelt's victory, at the American Federation of Labor (AFL) annual convention, President William Green proposed a six-hour day and five-day week with no cut in weekly pay, claiming it would have a dramatic positive effect on the unemployment problem. The implied rise in hourly wages of such a policy contrasted sharply with most Hoover-era work-sharing agreements that kept hourly wages the same, but Green and the AFL believed that the purchasing power of labor had to be increased in order to boost aggregate demand and halt the depression. Three weeks after the convention, Senator Hugo Black of Alabama introduced a 30-hour-per-week bill, and, in April 1933, the bill passed the US Senate. Most industry leaders were willing to limit hours but wanted higher limits. The US Chamber of Commerce proposed 40 hours, and in Congressional hearings, a long list of industry leaders suggested limits ranging from 32 to 48 hours. This variation in proposals and a feeling that a 30-hour limit was too extreme led the administration and representatives of leading unions and employers to develop a compromise in which the hours limit would be set by negotiations between employers and workers in each industry (Himmelberg 1976; Barkin 1936).

These negotiations were to take place under the auspices of the NIRA, which passed on June 16, 1933. The act brought together employers, workers, and consumer advocates to create Codes of Fair Competition that were meant to regulate all competitive behavior within an industry. In his message to Congress about the proposed bill, Roosevelt stated that the bill should provide "for the machinery necessary to obtain wide reemployment, to shorten the working week, to pay a decent wage for the shorter week, and to prevent unfair competition and disastrous competition" (Barkin (1936, 12) citing United States Congress (1933, 5154)). The act called for mutual agreements to set "maximum hours of labor, minimum rates of pay, and such other conditions of employment," including the right of workers to collectively bargain. "Senator [Robert F.] Wagner [of New York] introduced the bill in the Senate with the declaration that its 'principal and immediate object is to

open opportunities for the employment of several million men and women and thus distribute purchasing power which will be effective in starting again the wheels of industry.' In the specific codes presented by the employers, Senator Wagner had indicated that the latter 'must undertake to reduce the hours of labor to that number which the President finds will be most helpful in increasing employment in industry'" (Barkin (1936, 12), citing United States Congress (1933, 5154)).

The cotton textiles industry, joined by some parts of the rayon industry, established the first such code, effective on July 17, with labor provisions that set a 40-hour maximum workweek and minimum hourly earnings of 30 cents in the South and 32.5 cents outside the South. In a July 9 report accompanying the textile code proposal from the National Recovery Administration (NRA) Administrator Hugh Johnson to President Roosevelt, NRA staff estimated that cutting weekly hours from 49 to the 40-hour limit would increase employment from around 400,000 in June to around 528,000 (NRA 1933–1935, 3, 15–24). Similar sorts of calculations arguing that hours limits would lead to higher employment can be found in many reports filed along with later code proposals (NRA 1935). Barkin (1936, 132–140) examined the reports for the first 100 codes and found predictions that the weekly hour limits would return employment to the 1929 level in 15 industries and raise them to a lesser degree in 27 other industries.

The problem was that, by the end of July, it had become obvious that, aside from a few exceptions, very few industries were close to proposing codes any time soon. As a stopgap, the Roosevelt administration created the PRA with the primary goal expressed in the title. Firms that signed the agreement (and abided by it) could show their "business patriotism" by displaying the NRA's Blue Eagle symbol. In some ways, the PRA was a return to the jawboning and moral suasion of the Hoover Administration. To bolster participation, the Roosevelt administration embarked on a large-scale public relations drive at the national, state, and local levels, driving support for Blue Eagle firms with radio broadcasts, public speeches, community gatherings, and parades throughout the country. Around 20,000 canvassers went door-to-door and obtained 20 million signed pledges of support for the Blue Eagle campaign. The campaign successfully equated participation in the program with patriotism, making it difficult for firms not to sign up and forego displaying the Blue Eagle. Johnson, the NRA administrator, claimed that 85 percent of businesses signed up in the first 2 months (Taylor 2019).[4]

The stated goal of the PRA was to "raise wages, create employment, and thus increase purchasing power and restore business." On the workweek, it set a weekly hours maximum of 35 hours for production workers and 40 for nonproduction workers with daily hour limits of 3 hours per day for children aged 14 to 16.[5] On earnings, the agreement set a minimum per week for nonproduction workers of $15 in large cities and $12 in small towns. As for hourly earnings, production workers who were paid more than 40 cents per hour on July 15, 1929, had a minimum wage of 40 cents per hour. For workers paid less than this in 1929, the minimum wage was

[4] See Taylor (2011) and Klein and Taylor (2008) on the process of signing businesses up.

[5] Technically, if the six weeks of 40 hours per week were prorated over the period from August until the end of 1933, the workweek limit was effectively 36 hours.

the maximum of 30 cents and the 1919 rate, a business cycle peak year following World War I. Employers were also expected not to reduce the compensation of those earning more than the specified minimum wage.

Although the PRA was supposed to be a "blanket code" applying uniformly to all businesses,[6] clauses 13 and 14 allowed firms to sign up and then through industry groups petition for alternative rules (NRA 1935, vi.). Industry groups initially filed 566 petitions, of which 384 were accepted, mostly in August but also in September. NRA officials estimated that the substitutions covered industries employing about 17 million workers. Of the 384 accepted, 345 increased the weekly hours limit for production workers, 234 increased the limit for other types of workers, and 259 asked for different wage minimums. Of the limits requested for production workers, 83 percent chose what became the most common choice in the NIRA codes of 40 hours, 5.3 percent less than those limits, and the rest greater (calculated from Barkin (1936, 36–8)). Many of the substitutions, along with many later codes, specified certain peak periods during which the hours maximum could be relaxed. Most of the exceptions for peak periods still required the average workweek to equal the standard maximum over a specified length of time, like two months, six weeks, or longer (calculated from Barkin (1936, 36–38)). This meant having employees work strictly less than the workweek limit to offset peak periods when the workweek exceeded the limit.

Even with all these exceptions and the fact that the codes were drawn up by the industries themselves, within months, there was a so-called compliance crisis. Contemporaries such as Hugh Johnson and more recent scholarship such as Taylor (2011)[7] argue that, at least initially, the Blue Eagle campaign with its call for consumers to boycott firms not adhering to the PRA and later the NIRA codes was successful in disciplining firm behavior. However, Brand (1988) and Hawley (1974) point to flagging consumer support for the campaign by early 1934 and, with it, a decline in the costs to businesses from violating the code. Taylor (2019) has argued that, as a consequence, cheating on trade practice provisions such as pricing, output, and capacity restrictions became much more prevalent in this period.

The NIRA came to an official end on May 27, 1935, when the Supreme Court handed down a unanimous decision invalidating the major provisions of the law in the case of *Schechter Poultry Corp. versus The United States*. Central to the decision was the Court's conclusion that the act unconstitutionally delegated too much power to the executive branch in allowing the president to set up and enforce the codes. While the case put a legal end to the act, many have argued that Congress was unlikely to have renewed the act anyway, which needed to be renewed within a few months of the Court's decision. Even though the decision meant that industries were no longer obligated to follow the codes, authors such as Cole and Ohanian (2004) suggest that little changed in terms of actual behavior. They highlight the fact that many trade practices started under the NIRA, such as base point pricing in the steel industry, simply continued after the formal end of the law. Chicu et al. (2013)

[6] One exception was for establishments with fewer than three people located in towns with fewer than 2,500 people (NRA 1935, v–vii.).

[7] This work provides a much more extensive discussion of this period.

discuss similar behavior in the cement industry. In addition, the repeal had little practical impact on antitrust enforcement as the Roosevelt administration continued to decline to enforce existing antitrust laws. Cole and Ohanian note that the number of antitrust cases brought by the DOJ fell from around 12.5 new cases per year in the 1920s to 6.5 new cases per year during the 1935–1938 period. Cole and Ohanian also highlight cases of blatant price fixing reported by Federal Trade Commission studies in the 1930s and a case of bid rigging in the steel industry discussed by Hawley (1974). Neither case went punished by antitrust authorities.

II. A Model of Employment and the Workweek

To organize our empirical strategy, we extend the model of Bernanke (1986), who in turn built on the model of Lucas (1970). Bernanke's motivation was the puzzle from the viewpoint of neoclassical theory that (real) earnings per hour did not appear to fall (and if anything rose) during the Great Depression. His model of employment, the workweek, and earnings shows that, in fact, the procyclicality of earnings per hour is not necessarily a robust prediction of neoclassical theory. We adapt this model to study the labor market effects of workweek limits.

Households.—Let H_i be hours worked for household i and U_i the level of utility from being unemployed known only by the household. We define a household's reservation earnings $E(H_i, U_i)$ as the minimum level of earnings necessary for the household to work H_i hours given the outside option of unemployment providing utility U_i.

To derive this minimum level of earnings, assume that the household has time-separable preferences and a within-period utility function of the following form:

$$u(C_i, 1 - H_i), \tag{1}$$

where C_i is consumption and $1 - H_i$ is the number of leisure hours. For simplicity, assume that households are unable to borrow or save, so they will consume whatever they earn. Households make a discrete choice between working or unemployment. The reservation earnings function is then determined implicitly as

$$U_i = u(E(H_i, U_i), 1 - H_i). \tag{2}$$

With the usual assumptions on the utility function, the reservation earnings function will be increasing and convex in hours, as well as increasing in reservation utility.

Following Bernanke, we assume that U_i is drawn from (exogenous) distribution G and is private information to the household so that firms cannot offer household-specific employment contracts. Instead, there is a pooling equilibrium in which all employment contracts offered deliver a common level of utility $\bar{U}$. Given this contract, households with $U_i < \bar{U}$ will choose to work, and those with the lowest outside option who still end up working will earn rents. All others

with $U_i \geq \bar{U}$ will choose to be unemployed. The *employment* supply curve is $N^s(\bar{U}) = G(\bar{U})$, where $G(\bar{U})$ represents the fraction of the households with a reservation utility below the utility from working $\bar{U}$.

Firms.—There is a unit mass of firms. We assume that the set of firms in operation is fixed over time. Firm j produces according to a constant returns to scale technology that takes as inputs both the workweek or hours per worker H_j and total employment N_j:

$$Y_j = F_j(N_j, H_j). \tag{3}$$

We assume that a firm's production technology is exogenously given. We allow the number of employees and hours per employee to enter separately into the production function. We also allow for different firms to operate different production functions. It is possible in equilibrium for firms that use different technologies to operate simultaneously. Depending on the technology employed, firms will make different choices for N_j and H_j, while still providing utility $\bar{U}$.

Firms make a sequence of static profit-maximizing decisions for employment and the workweek taking as given $\bar{U}$. They offer contracts to potential workers that specify a length of the workweek and total earnings.[8] Firms are price takers in the output market and produce a homogeneous good. We normalize the price of the final good to 1. The profit maximization problem is therefore

$$\max_{N_j, H_j} \pi_j = F_j(N_j, H_j) - N_j E(H_j, \bar{U}). \tag{4}$$

The firm's optimal choices (suppressing the j subscript) for employment and the workweek are determined by

$$F_N = E,$$

$$F_H = NE_H,$$

where F_N is the marginal product of employment, F_H is the marginal product of the workweek, and E_H is the change in earnings necessary to compensate an employee for a marginal change in the workweek. The first equation states that, holding fixed hours per employee, a firm is willing to keep hiring employees up to the point where the marginal revenue from an additional employee F_N is equal to that employee's total compensation E. This equation defines the employment demand curve for firm j, $N_j^D(\bar{U})$.

[8] Trejo (1991) makes a similar assumption in modeling the effects of an overtime pay regulation. He argues that such a policy will have no real effects since firms can simply reduce nonovertime pay to deliver the same level of utility to workers. A (hard) workweek limit can be interpreted as a rate of overtime pay that is set arbitrarily high. As we will see, in this case, Trejo's neutrality result does not hold. Trejo also provides some empirical evidence that, while there are changes in pay packages that offset overtime regulations, the changes are not enough to completely offset the policy.

The second first-order condition states that a firm is willing to keep increasing the workweek up to the point where the marginal revenue from an additional hour of work per employee F_H is equal to the additional compensation per employee E_H times the number of employees N_j. Another way to interpret this equation is that it determines the "wage" E_H, which is defined as the change in earnings due to a marginal change in the workweek. Bernanke points out that a ray from the origin to the optimal choice of H in (H, E) space, whose slope would be average hourly earnings E/H, will not, in general, be tangent to the function E at this optimal choice. This means that (observed) average hourly earnings might be different from the (unobserved) wage E_H. This is due to the fact that, if a firm changes its workweek, it must change its workweek and compensation for *all* of its employees. This is similar to how a monopsonist must adjust the wage for all its employees when hiring the marginal worker.

The parallel between our situation and that of a monopsonist leads to the important observation that firms will make profits in equilibrium. If we multiply the first-order conditions of the firm by N and H, respectively, and sum them, then, by Euler's theorem, we find

$$\pi = Y - EN = NHE_H > 0.$$

The fact that firms make profits is related to the fact that, as we show in the online Appendix, the equilibrium without a workweek limit is not Pareto efficient.

Employment Market Clearing.—Employment market clearing requires that employment supplied by the households is equal to employment demand from the firms:

$$N^S(\bar{U}) = \int_0^1 N_j^D(\bar{U})\,dj. \tag{5}$$

Equilibrium in the employment market, rather than determining the wage, now determines the level of utility for households that work $\bar{U}$. Employment supply is nondecreasing in $\bar{U}$ since it is equal to the CDF G, while employment demand is strictly decreasing in $\bar{U}$. Furthermore, for $\bar{U}$ small enough, employment supply will be below demand. Therefore, there is a unique equilibrium level of $\bar{U}$ determined by the intersection of these two curves.

No Workweek-Limit Equilibrium.—We assume that the utility function of a household is Cobb-Douglas: $U = E^{\phi}(1-H)^{1-\phi}$ with associated earnings function $E = U^{1/\phi}(1-H)^{-(1-\phi)/\phi}$. We also assume that firms operate a Cobb-Douglas production function: $Y_j = N_j^{\alpha_j} H_j^{1-\alpha_j}$. The elasticity of output with respect to employment α_j is drawn from some exogenous distribution P, and, for simplicity, we assume that all firms have the same (total factor) productivity normalized to 1.[9]

[9] In the online Appendix, we show that differences in total factor productivity would not generate variation in the workweek. That is why we focus only on variation in α_j.

Given these assumptions, the optimal choice of the workweek for firm j is

$$H_j^* = \frac{\tilde{\phi}}{\tilde{\alpha}_j + \tilde{\phi}}, \tag{6}$$

where $\tilde{\alpha}_j = \frac{\alpha_j}{1-\alpha_j}$ and $\tilde{\phi} = \frac{\phi}{1-\phi}$.[10] We note that this is independent of quantities determined in equilibrium, specifically $\bar{U}$. Instead, all variation in the workweek is due to the exogenous variation in α_j. This is a useful expression since it allows us to determine which firms are affected by a workweek limit without having to determine the equilibrium utility level.

To understand the effects of the workweek limit, we need to characterize the firm-level elasticity between employment and the workweek holding fixed $\bar{U}$ and the elasticity of unemployment with respect to $\bar{U}$. First, we find

$$\frac{d\log N_j}{d\log H_j} = 1 - \frac{H_j}{\tilde{\phi}(1-\alpha_j)(1-H_j)}. \tag{7}$$

Evaluating this expression at the optimal choice of the workweek H_j^*,[11] the elasticity ϵ_j is given by

$$\epsilon_j = -\tilde{\alpha}_j^{-1} < 0. \tag{8}$$

Firms that are forced to reduce their workweek for exogenous reasons, all else equal, will want to increase their employment in response. Furthermore, those that have to reduce their workweek the most (those with smallest $\tilde{\alpha}_j$) will raise their employment the most. We call this effect on employment from a reduction in the workweek holding fixed $\bar{U}$ the partial equilibrium effect. This expression for the elasticity can also be interpreted cross-sectionally to imply that there is a negative correlation between employment and the workweek across firms.

At the same time, an increase in the equilibrium level of utility $\bar{U}$ depresses employment demand since, holding fixed H_j,

$$\frac{d\log N_j}{d\log \bar{U}} = -\frac{1}{\tilde{\phi}(1-\alpha_j)} < 0. \tag{9}$$

We call this the general equilibrium effect on employment. As opposed to the partial equilibrium effect, those with the shortest workweeks (largest α_j) will suffer the largest employment declines for a given increase in the utility from working.

The Effects of a Workweek Limit.—We consider the effects of a workweek limit that requires firms with $H_j^* \geq h$ to reduce their workweek to the exogenously imposed level h. The directly affected firms are those with $\tilde{\alpha}_j \leq \tilde{\alpha}^*$, where $\tilde{\alpha}^* = \tilde{\phi}\frac{1-h}{h}$.

[10] All calculations and proofs are collected in the online Appendix.

[11] For a general CES production function with substitutability $\rho > 0$, the elasticity is given by $\epsilon_j = \frac{\rho-1}{\rho} + \frac{1}{\rho}\frac{\Delta\log(1-H_j)}{\Delta\log H_j}$. This implies that the elasticity is nonconstant and could be positive or negative regardless of the value of $\rho \neq 0$. The value of α_j plays no (direct) role in determining this elasticity.

Firms not directly affected by the workweek limit, that is, those with $H_j^* < h$, will not change their workweek choices. Hence, following the workweek limit, $H_j = h$ if $\tilde{\alpha}_j \leq \tilde{\alpha}^*$ and H_j^* otherwise. We characterize the aggregate effects of the workweek limit in the following proposition.

PROPOSITION 1. *In a first-order approximation around the no-workweek-limit equilibrium, the aggregate consequences of introducing a workweek limit are that aggregate output Y falls, aggregate employment N rises, and utility from working $\bar{U}$ rises.*

The intuition for this result is straightforward. Holding fixed $\bar{U}$, employment demand rises for those firms that are forced to cut their workweek (the partial equilibrium effect), which causes an increase in $\bar{U}$ (since employment supply is fixed). This increase in $\bar{U}$ will then generate a general equilibrium effect reducing employment demand, offsetting some (but not all) of the increase in employment demand due to the partial equilibrium of limiting the workweek.

Besides the consequences for employment and output, we are also interested in the cross-sectional effects of the workweek limit on earnings.[12] We summarize the predictions of our model in the following proposition.

PROPOSITION 2. *Consider the no-workweek-limit equilibrium holding fixed $\bar{U}$. Then the elasticity with respect to the workweek for earnings per worker E_j, earnings per hour E_j/H_j, and total earnings or payroll N_jE_j are, respectively*

$$\frac{d\log E_j}{d\log H_j} = \tilde{\alpha}_j^{-1}, \frac{d\log E_j/H_j}{d\log H_j} = \tilde{\alpha}_j^{-1} - 1, \frac{d\log N_jE_j}{d\log H_j} = 0.$$

Firms that reduce their workweek will also decrease earnings per worker, though the magnitude of the decline depends on the firm's production technology. This negative elasticity is not a surprising result since the amount of utility provided to the worker is fixed so a mandated shorter workweek is offset by a lower amount of earnings. For earnings per hour, whether this elasticity is negative depends on whether $\tilde{\alpha}_j \geq 1$. Note that, if households faced a constant (hourly) wage rate when making their workweek decision, they would choose $(1-\phi)H_j = \phi(1-H_j)$, and this elasticity would be equal to 0. Recall that those firms most likely to be affected by the workweek limit are those with small values of α_j. Hence, it is more likely that earnings per hour at firms directly affected by the workweek limit rise relative to those not directly affected. This will be a cross-sectional prediction we test empirically. The general equilibrium of a workweek limit is that earnings per hour will increase because of the increase in $\bar{U}$.

The final part of the proposition states that the workweek limit has no direct effect on total purchasing power of workers since it does not affect total labor earnings at

[12] There are additional distributional consequences. First, the workweek limit leads to a redistribution of surplus from firms, which see profits fall since $\bar{U}$ rises, to households, which see utility rise by the same token. Second, there is redistribution of surplus between firms.

any single establishment.[13] Instead, forcing a firm to cut its workweek leads the firm to spread the same level of labor earnings across a greater number of households. However, we also need to take into account the indirect effects of the workweek limit on $\bar{U}$. In the online Appendix, we show that $N_j E_j$ is proportional to $\bar{U}^{-\tilde{\alpha}_j/\phi}$, so total labor earnings fall at every establishment and in aggregate as a result of the increase in $\bar{U}$.[14]

As a point of comparison, the Roosevelt administration's (implicit) theory of the effects of the workweek limit seems to have been a simple "lump of labor" theory in which hours per worker and the total number of workers were perfect substitutes. Therefore, the workweek limit would have no effects on total hours worked and simply cause a reallocation of hours worked from those already employed to those unemployed. In other words, the elasticity of employment with respect to the workweek would be minus one, so the percentage fall in the workweek would be offset by an equal percentage rise in employment. How exactly under this theory the workweek limit would raise earnings per unit of labor input or in aggregate is not clear.

Pareto Efficient Outcomes.—In thinking about the desirability of a workweek limit, an important question is whether the no-workweek-limit equilibrium is actually Pareto efficient. To answer this question, it is necessary to specify what mechanisms the social planner has available. In the market equilibrium, we assumed that all firms were restricted to offering a contract that provided a single level of utility to employees. We will continue to impose this on the social planner by requiring the planner to select a single utility $\bar{U}$ for each employed household. The planner will also choose an allocation $\{N_j, H_j\}$ across firms consistent with employment supply and the earnings function E. No other transfers between or among firms and households are allowed.

Given a level of utility $\bar{U}$, the planner will assign the households with reservation utility $U \geq \bar{U}$ to unemployment and the others to work. Therefore, the employment supply curve faced by the planner will be the same as in the market equilibrium. Given this allocation of households, we can calculate the surplus for households as

$$W(\bar{U}) = \int_{-\infty}^{\bar{U}} \bar{U} dG(U) + \int_{\bar{U}}^{\infty} U dG(U) = \bar{U} G(\bar{U}) + \mathbb{E}[U | U \geq \bar{U}].$$

For producer surplus, first define profits for firm j as $\pi_j(\bar{U}) = F(N_j, H_j) - N_j E(H_j, \bar{U})$. Producer surplus is simply equal to total firm profits $\Pi(\bar{U}) = \int_0^1 \pi_j(\bar{U}) dj$.

We can trace out the Pareto frontier as a function of the level of producer surplus $\bar{\Pi}$ by solving

$$\max_{\bar{U}, N_j, H_j} W(\bar{U})$$

[13] We note that some of these results and this one in particular would not carry over to a more a general CES production function.

[14] As discussed, the PRA and NIRA also established wage minimums, although we did not model them here because we sought to isolate the effect of the workweek restrictions. To the extent that the wage minimums were binding, all else equal, this would increase hourly earnings and reduce employment.

subject to $\int_0^1 N_j dj = G(\bar{U})$ and $\Pi(\bar{U}) \geq \bar{\Pi}$ with associated Lagrange multipliers λ_1 and λ_2. In the online Appendix, we show that, at a Pareto efficient point, for all j,

$$\lambda_1 = \lambda_2\left[E\left(H_j, \bar{U}\right) - F_N\left(N_j, H_j\right)\right].$$

Since the constraints bind, it must be that $\lambda_1, \lambda_2 > 0$ and $E\left(H_j, \bar{U}\right) - F_N\left(N_j, H_j\right) > 0$ for all j. On the other hand, we showed that in the no-workweek-limit equilibrium, $E\left(H_j, \bar{U}\right) - F_N\left(N_j, H_j\right) = 0$. Therefore, the no-workweek-limit equilibrium is not Pareto efficient. Holding fixed $\bar{U}$ and H_j, a Pareto efficient outcome has a higher level of employment at each firm than the no-workweek-limit equilibrium since F_N is decreasing in N_j. We note that this result does not depend on the exact specification for the utility function or the production function.

Even though we have shown that the no-workweek-limit equilibrium is not Pareto efficient, this does not necessarily mean that a workweek restriction will be Pareto efficient, let alone Pareto improving. To the contrary, a workweek restriction will reduce firm profits by raising $\bar{U}$; though, by the same token, it will raise consumer surplus. In fact, no Pareto efficient allocation can be implemented by imposing a workweek limit alone. A workweek limit will potentially alleviate the problem of underemployment in the no-workweek-limit equilibrium. The problem with such a policy is that the socially efficient choice of the workweek coincides with the no-workweek-limit one,[15] and a workweek limit distorts that choice. Because a workweek limit will not lead to a Pareto improvement, whether a workweek restriction is a "good" thing will depend on how exactly the social planner values the gain in consumer surplus versus the decline in producer surplus.

III. Data

A. *COM Data*

The first dataset we study comes from the establishment-level schedules of the 1933 and 1935 COM. We focus on a subsample of eight industries[16] including ice cream, manufactured ice, sugar refining, cotton goods, blast furnaces, steel works, motor vehicles, and cigars and cigarettes. Total employment in these eight industries was about 692,000 in January 1933, compared to a total of 21.2 million for nonagricultural employment (National Bureau of Economic Research 1941). Relative to only manufacturing employment, in 1933, the COM reported 6.1 million wage workers (United States Census Bureau 1936, 21). The reason for focusing on these eight industries is that establishments in these industries were asked to report by month the number of

[15] This is not necessarily true if we assume a general CES production function.

[16] These are derived from ICPSR Study 37114, which contains information on 25 industries. As discussed in Vickers and Ziebarth (2018a, 2018b), the original sample is the work of many different people. For autos, see Bresnahan and Raff (1991). For concrete, see Morin (2015). For blast furnaces, see Bertin et al. (1996). Some of these industries are also available as individual ICPSR studies: 35604 and 31761 (motor vehicles); 35605 (cotton goods); 37211 (sugar refining); 37208 (blast furnaces); and 31761 (textiles). In the online Appendix, we provide some summary statistics and additional details for our sample of eight industries.

their wage earners and how many hours in total those employees worked.[17] Based on these variables, we calculate the average workweek as the ratio of total man-hours to total employees.[18] This measure of the workweek is effectively an establishment-level average workweek weighted by the number of employees that have a particular workweek. We note that a decline in this measure does not mean that the workweek of any one worker was cut. Instead, it might simply reflect changes in the mix of part- and full-time work.[19]

B. *BLS and SSNRA Data*

The second dataset we use is a monthly industry-level dataset for 115 sectors (not just manufacturing) covering the period January 1933 through December 1935.[20] The NRA and the BLS worked together to develop this dataset for the express purpose of studying the impact of the NIRA. To do this, they created a special survey with two important features. First, the sectoral classification used in the survey was the same classification used in NIRA codes.[21] Second, the survey added sectors that the BLS had not been surveying up to that point.

The employment measure is a chained index based on the percentage change for the same firms from month to month and benchmarked to the 1933 COM for sectors where the original BLS classifications matched the code classifications.[22] Other industries for which there was not a match between the classifications were not benchmarked, but the NRA claimed that this did not cause major discrepancies. The fact that employment is indexed in some industries and not in others is not problematic since our specifications take a log transformation of this variable

[17] The form asks establishments to report hours from the time workers were "actually employed on the job," which we take to mean paid employment.

[18] Establishments were also asked directly about their "normal" workweek in the reference month of December. The problem with using this variable to study the PRA is that it provides no information on variation in the workweek over the course of the year. Rather than use it as our main dependent variable, we use it to validate the workweek variable derived from monthly man-hours and employment.

[19] In the online Appendix, we discuss some additional concerns surrounding this measure and provide some checks validating it.

[20] In December 1934, the manufacturing part of the survey covered 2.75 million wage workers. In addition, the average number of workers in 1934 was 110,000 in transit, 371,000 in retail trade, and 55,000 in hotels. Obviously, the number employed covered by the survey varies from month to month. Summing across the lowest number of workers reported during the months from 1933 and 1934, the total in manufacturing was about 1.3 million (NRA, 1936, 240–244) The number employed full-time and part-time in retail establishments in 1929 was 4.4 million and in 1933 was 3.4 million (United States Census Bureau 1937, 10)

[21] Even this classification scheme did not completely eliminate the possibility of measurement error because of the fact that establishments could be operating under multiple codes. Establishments operating under more than one code were classified with the code where more than two-thirds of their employees worked. If an establishment did not meet this criterion, it was allocated to multiple sectors.

[22] The weekly hours and hourly earnings were not chained from month to month because the BLS considered the errors from chaining to be larger than errors arising from slightly different samples each month. For the workweek, the SSNRA collected "average hours worked per week," which were obtained by the BLS from "identical establishments in each pair of adjacent months" (NRA 1936, 4–5). The Bureau of Labor Statistics used the same method (NRA 1936, 4). The NICB, discussed below, followed a similar procedure to the BLS (Beney 1936, 13). This means that employment changes from entry and exit could potentially be missed, though this is not a concern with the estimates derived from the COM data.

and include industry fixed effects.[23] In such a specification, the index value will be absorbed by the industry fixed effects.

C. *NICB Data*

The third dataset we use serves as an external validity check on the other datasets and a source for robustness checks that we discuss in the online Appendix. This dataset collected by the NICB provides monthly industry-level information on hourly earnings, weekly earnings, and hours per week for 24 industries from the mid-1920s through 1939 (Beney 1936; NICB 1938; Sayre 1940).[24] The NICB was and still is a private research organization that, among other activities, surveys businesses. We calculate average hourly earnings by dividing the total payroll of workers by the total hours worked and average weekly hours by dividing total hours worked by the number of workers that week (Beney 1936, 19–20).[25] The NICB calculated employment indexes for three periods: (i) 1925 (or earlier) to June 1936, (ii) January 1932 through December 1936, and (iii) January 1934 through December 1939. Using methods described in Beney (1936), they benchmarked to the biennial censuses from 1933, 1935, and 1937 and then interpolated the intermediate years.

The NICB dataset has some advantages when compared with the COM data. First, it covers more industries over more years, and, second, it contains monthly information on hourly and weekly earnings. The disadvantages are the (obvious) absence of establishment-level data and the indexed values for employment, which make it difficult to calculate actual levels of employment.

It is important to keep in mind that each dataset covers a slightly different portion of the economy and was collected in a slightly different way. At the same time, each captures to a high degree the same macroeconomic trends. Figure 2 compares trends in the median workweek indexed to January 1933 across the three datasets. Each of the datasets shows the rise in weekly hours starting in March and continuing through July followed by the sharp drop in the workweek as firms signed the PRA and industries adopted NIRA codes. The only differences across the datasets are between January and March, when the NICB data show a 10 percent decline in the median workweek and the COM and SSNRA data a rise by 1 to 3 percent. This discrepancy occurred well before the PRA was instituted and does not cast doubt on the sharp drop in weekly hours that occurred following the PRA.[26]

One general concern is that firms and establishments purposely misreported their workweek after the workweek limit came into force. Taylor (2019, 112–113) discusses an instance of a company keeping two separate hours records—the real one and the one to show the authorities, the latter of which had everyone working

[23] To recover the actual level of employment, which we use in robustness checks in the online Appendix, we have crosswalked the SSNRA sectors to the published volumes of the COM to get information on average employment in 1933. This is simply not possible for all sectors since some like hotel or transit are not manufacturing, and others do not match the classification of industries in the COM. We discuss this further in the online Appendix. For the sectors we are able to deindex, total employment is approximately 220,000 in January 1933.

[24] This was the dataset that Bernanke used in his original paper on the workweek.

[25] The NICB calculated the "average actual work-week" generally by "dividing the total number of hours worked during the week by the total number of wage earners" (Beney 1936, 12).

[26] In the online Appendix, we provide some additional comparisons between the datasets.

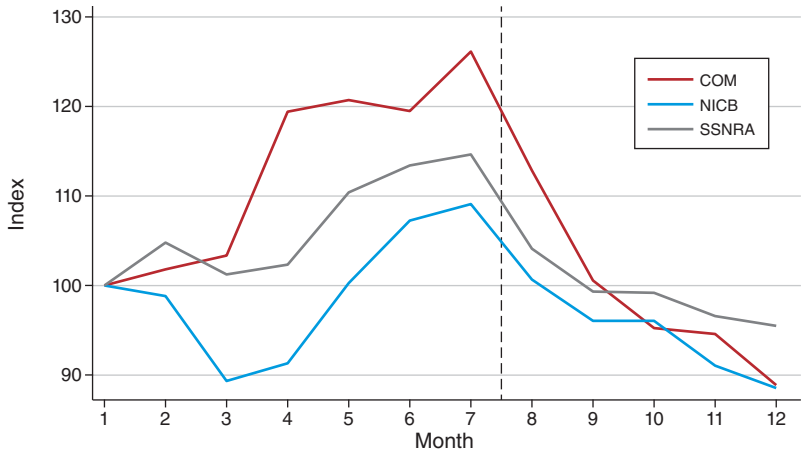

Figure 2. Workweek in 1933

Notes: Values are indexed to January 1933. Each line represents the median across all industries within a dataset. The COM data were collected from the COM and are reported at the establishment level covering 8 industries. The NICB data were collected by the NICB and are reported at the industry level covering 24 industries. The SSNRA data were collected by the SSNRA and the BLS and reported at the industry level covering 115 sectors.

40 hours a week. We think this misreporting is unlikely to be very prevalent in our data sources. First, for the COM data, establishments were asked in 1934 *retrospectively* about what happened in 1933. By that time, there was not a lot of risk to reporting truthfully what had happened in the past. Furthermore, the Census schedules were supposed to be confidential, and the government could not simply go fishing around in these records to find violations. Second, the workweek variable is actually derived from two separate questions on the Census. That is, establishments were asked to report the total number of hourly employees by month and also man-hours by month, and the ratio is what we call the workweek. This means that if an establishment wanted to "fake" the derived workweek, it would have to do some work multiplying the number of employees by the fake workweek to report a fake number of man-hours. The work involved here makes us think this is unlikely. For the NICB dataset, the incentives to misreporting are rather low since this was a private organization with no investigative authority and no firm-level data were ever released.

IV. Effects of the Workweek Limit at the Industry Level

We start by estimating a simple linear regression to identify the labor market effects of the PRA and NIRA. In particular, these estimates do not try to separate out the effects of the various provisions of the PRA and NIRA. Instead, they should be interpreted as the combined effects of the workweek limits and minimum wages. This approach is similar to that of Taylor (2011), who examines the impacts of the PRA and NIRA on the labor market for 15 to 28 industries, depending on the outcome analyzed. Our estimation improves on Taylor's by using the SSNRA sample of 115 sectors, covering a more substantial fraction of the economy.

In these initial specifications, we exploit variation by industry and time based on the timing of the PRA and the date when the industry's NIRA code went into effect. We estimate a panel regression between January 1933 and December 1935 for dependent variable Y_{imt} in industry i in month m and year t of the following form:

$$\log Y_{imt} = \beta_1 EPRA_{imt} + \beta_2 ENRA_{imt} + \beta_3 CCPRA_{imt} + \beta_4 CCNRA_{imt} + Controls_{imt} + \varepsilon_{imt}.$$

The six labor market outcomes we examine are (log of) average weekly hours (workweek), employment, man-hours, average hourly earnings, average weekly earnings, and payroll. Throughout the paper, we use "employment" to refer to the total number of workers employed.

The variable $ENRA_{imt}$ indicates whether the NIRA code was in effect for the industry and before the compliance crisis. For the month when the code went into effect, the value of $ENRA_{imt}$ is the share of days in the month the code was in effect. For example, the first code went into effect for cotton textiles on July 17, 1933; therefore, the $ENRA_{imt}$ variable has a July value of $0.484 = (31 - 16)/31$ for this industry. In the months before the code was in effect and in months after March 1934, the value of $ENRA_{imt}$ is zero. Similarly, the variable $EPRA_{imt}$ is an indicator for the PRA period before the compliance crisis. It has a value of 0 before August 1933 and after March 1934, a value of 1 from August until the industry's NIRA code was introduced, and a value of $(1 - ENRA_{imt})$ in the month that the NIRA code went into effect. The variables $CCPRA_{imt}$ and $CCNRA_{imt}$ 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 0 through March 1934 and after May 1935, the month in which the NIRA was declared unconstitutional by the US Supreme Court.

Following Taylor, the controls include the log of the nonseasonally adjusted monthly national industrial production index IP_{mt} (Board of Governors of the Federal Reserve System 2023).[27] We also include the following fixed effects. To control for seasonality, we include a set of month fixed effects. To control for other lower frequency aggregate shocks, we include a set of year fixed effects. Finally, to control for unchanging features of the industries, we include a set of industry fixed effects. We are interested in the total effect of these policies, so we do not include as controls variables plausibly affected by the policies themselves, such as hourly earnings.[28]

Roosevelt's goal was to raise employment and hourly earnings. The results in Table 1 suggest that this was actually a common occurrence across the industries in our sample. In the period between the start of the PRA and the compliance crisis ($EPRA_{imt} = 1$), weekly hours were lower by 8.1 percent, employment higher by 10.5 percent, and hourly earnings higher by 10.7 percent than they had

[27] Results are essentially unchanged if we include other business cycle indicators like the S&P 500 index or interest rates.

[28] Taylor included the wage as a control to try to isolate the effects of the workweek restrictions. In the online Appendix, we replicate that specification.

Table 1—Effects of the PRA and NIRA on Hours, Earnings, and Employment

	Workweek (1)	Employment (2)	Man-hours (3)	Hourly earnings (4)	Weekly earnings (5)	Payroll (6)
ENRA	−0.077	0.108	0.035	0.104	0.017	0.124
	(0.010)	(0.014)	(0.019)	(0.011)	(0.008)	(0.017)
CCNRA	−0.046	0.061	0.016	0.068	0.018	0.079
	(0.006)	(0.008)	(0.011)	(0.005)	(0.004)	(0.010)
EPRA	−0.081	0.105	0.025	0.107	0.009	0.114
	(0.010)	(0.011)	(0.016)	(0.008)	(0.007)	(0.013)
CCPRA	−0.062	0.024	−0.050	0.043	−0.030	−0.005
	(0.012)	(0.023)	(0.035)	(0.013)	(0.013)	(0.030)
IP	0.450	0.300	0.764	−0.140	0.287	0.587
	(0.033)	(0.038)	(0.064)	(0.012)	(0.029)	(0.058)
Observations	3,957	4,138	3,957	3,957	4,138	4,138

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 0 through March 1934 and after May 1935, the month in which the NIRA was declared unconstitutional by the US 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 SSNRA 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 man-hours and payroll variables, which are derived from the employment variable. Standard errors are clustered at the industry level.

been before the PRA. During the period when an industry's code was in effect ($ENRA_{imt} = 1$), weekly hours were 7.7 percent lower, employment 10.8 percent higher, and hourly earnings 10.4 percent higher than prior to the PRA.[29] We conclude that the changes to employment and earnings were similar under the PRA and while an industry's NIRA code was in effect, which is not surprising since the codes in many cases ended up having workweek limits quite similar to the PRA blanket limit. At the same time, the differences in labor market outcomes during the compliance crisis of the PRA ($CCPRA_{imt} = 1$) and NIRA ($CCNRA_{imt} = 1$) relative to prior to the PRA were uniformly smaller (except for weekly earnings), though still significant in most cases. This is consistent with the findings of Taylor (2019), who suggests that the crisis was mainly about cheating on trade practice provisions such as pricing, output, and capacity restrictions. Overall, our results for all these labor market variables are similar to the results in Taylor (2011).[30]

The ultimate goal of the administration was that these policies would not just raise hourly earnings but make workers better off in terms of total weekly pay. Table 1 provides some evidence that much of the change in hourly earnings was

[29] In the online Appendix, we replicate Table 1 using the levels of employment rather than the index for the smaller sample for which we recovered the level. Since man-hours and payroll are derived from total employment, the sample size is reduced for these variables as well. In the online Appendix, we also show that the effects are basically unchanged if we restrict attention to the smaller sample with no missing data on the dependent variables.

[30] The one slightly puzzling result is the negative effect on man-hours during the PRA compliance crisis period, though this is not statistically significant.

offset by the changes in the workweek. For example, in the PRA period before the compliance crisis ($EPRA_{imt} = 1$), the workweek decline of 8.1 percent was offset by an increase in hourly earnings of 10.7 percent, leaving weekly earnings only marginally higher.[31] A shorter workweek paired with only minor changes in weekly earnings is a shift that workers would be happy to accept. In Section VII, we use variation in an industry's workweek before the PRA to show that this panel specification overestimates the degree to which increased hourly earnings offset workweek declines. As we will show there, workers in industries that had to cut their hours the most on account of the PRA experienced relative declines in their weekly earnings, making it less clear that workers actually benefited from these policies.

V. Effects of the Workweek Limit on the Workweek

The results in Table 1 show that the periods when the PRA and NIRA labor market restrictions were in effect were associated with fewer weekly hours worked, higher employment, and higher hourly earnings. There is still a question as to how much responsibility can be assigned to the workweek limits themselves versus other components of the PRA and NIRA or other macro shocks. Our approach to isolating the effects of the hours limits is to use Census data at the establishment level from 1933 to examine whether the workweek limit caused "bunching" in the workweek distribution. If the workweek limit was actually binding for some fraction of the establishments, we should observe a mass of establishments with workweeks very close to the limit following its imposition. One group of those at the limit would have initially had longer workweeks and had to cut their workweek to get down to the limit. Another group would have had initially shorter workweeks and wanted to increase their workweek but ended up constrained by the limit.[32] The focus on bunching prevents us from assigning responsibility to the workweek limits for changes in the workweek for establishments whose workweek was far from the limit. At the same time, it is hard to explain bunching based on the other components of the PRA and NIRA or macro shocks. Thus, the focus on bunching, in our view, more effectively isolates the changes in the workweek due to the workweek limit.

Figure 3 plots the establishment-level density of the workweek in 1933 and 1935 where each establishment is weighted by its employment. We split both of these years into pre-PRA and post-PRA time periods while dropping the month of July. The pre-PRA period includes the three months of April through June, while the post-PRA period includes August through October. We report an establishment's workweek as a percentage of the industry's PRA- or NIRA-specific workweek limit.[33] We exclude July since the PRA was signed by Roosevelt in the middle of the month. We use a narrow three-month window on either side of July to minimize seasonality concerns, an issue we address in a different way later.

[31] The sum of the coefficients for the weekly hours and hourly earnings does not add up to the exact coefficient for weekly earnings since we do not impose this restriction across the three separate specifications.

[32] The use of bunching-based empirical strategies has become very popular in public economics where it was originally developed in the context of taxation. See Kleven (2016) for a survey of this approach and its applications.

[33] In the online Appendix, we provide details on the limits we used for these industries.

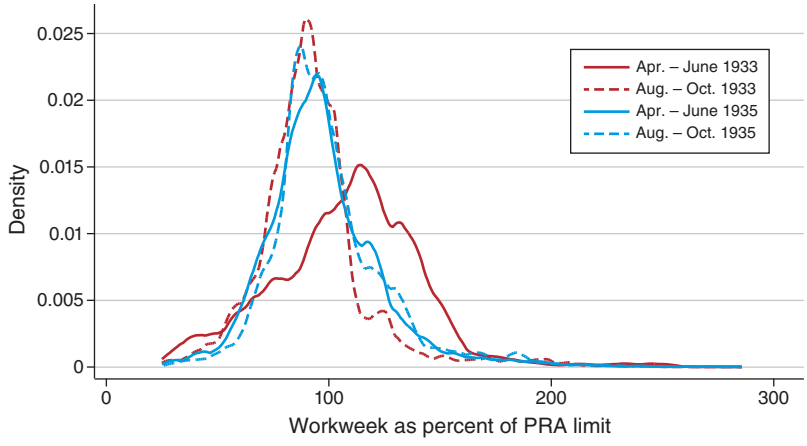

FIGURE 3. DISTRIBUTION OF WORKWEEK RELATIVE TO WORKWEEK LIMIT

Notes: Workweeks are scaled by the industry-specific workweek limit specified by the PRA. These data were collected from the COM and are reported at the establishment level. Establishments are weighted by employment.

The shift in the distribution from before to after the PRA in 1933 is quite evident.[34] A formal test of the equality of the distributions rejects the hypothesis that the distributions are the same at any standard level of significance. Most crucially, the shift is not just a simple parallel shift in the distribution. Instead, there is a pronounced spike in the density right around each industry's limit in the post-PRA period that is not present in the pre-PRA period. Now, there may be concerns that the changes in 1933 between the three-month periods just reflect typical seasonal patterns. Figure 3 helps allay that concern by showing that the distributions of the workweeks in 1935 over the same two three-month periods are virtually identical. In addition, May 1935 marked the (formal) end of the NIRA with the US Supreme Court's *Schechter* decision. Yet there is no discernible shift in the distribution between before and after May 1935. To us, this provides further support for the claim made by, for example, Cole and Ohanian (2004) that many industries simply continued to follow the provisions of their codes after the NIRA was struck down, and the Roosevelt administration was not opposed to that.

A number of contemporaneous reports also noted a decline in the workweek at a similar point in time. For example, using a dataset that represented 28 percent of all factory workers in 1933, Olenin and Corcoran (1942) documented a decline in hours per week following the PRA in nondurable goods industries, like cotton, woolen and worsted textiles, meat packing, and bread, but only a minor decline for durable goods. They suggest that this was in part due to the longer workweeks in nondurable goods industries before the PRA, variation we will attempt to exploit later to estimate the effects on earnings. Contemporary observers also

[34] In the online Appendix, we show that a similar pattern holds in the SSNRA dataset.

noted bunching at the workweek limit after the PRA. For example, the report of the Division of Wages, Hours, and Working Conditions (1936, 56) studied repeated cross-sections of approximately 250 bread-baking establishments from March 1933, September 1933, and December 1934. The establishment-level distribution of the workweek shows a pattern very similar to the one in Figure 3, with a shift to the left following the PRA and a "piling" up of the distribution right around the workweek limit after August 1933.

It is true that some establishments reported workweeks longer than the workweek limits in the PRA period. Most likely, some of these cases are violations of the limit. However, there are benign explanations for violations as well. For one, the workweek limits in many industries allowed for higher hours during peak activity as long as the average weekly hours were under the limit for periods ranging from a month up to a year depending on the industry. Thus, in any single month, there might have been a number of apparent "violators" even if all establishments were adhering to their limits. A second source of "violations" is due to the construction of the COM data. This measure is calculated at the establishment level, so it hides the distribution of workweeks within an establishment. It is possible that the share of workers working under the limit at a given establishment was a majority, but a small share of employees worked long hours (often with extra pay set by the code), pushing the average above the limit. This would lead us to count all of that establishment's employees as in violation of the limit. Third, not all firms signed up immediately, so some of the "violations" might simply be firms that were not, in fact, subject to the workweek limits.

VI. The Effects of the Workweek Limit on Employment

We estimate the employment effects of the workweek limits by drawing on an approach previously used to measure the employment effects of the minimum wage (Cengiz et al. 2019). In our setting, the estimator adds up the number of "excess" jobs slightly below the workweek limit and the number of "missing" jobs above the limit to get the total employment effect. The number of excess and missing jobs are measured relative to the distribution of the workweek before the workweek limit went into place.

As motivation for this approach, Figure 4, which uses the COM sample, shows employment in three categories: above, below, and at the workweek limit. These groups are defined month by month, so an establishment that changes its workweek even with no change in its employment would cause employment in these groups to change. We have indexed these numbers to their values in January of each year, so it is not possible to get the total change in employment by simply adding up changes in the various groups. Just after the workweek limit was imposed in 1933, we observe a sharp rise in the number employed with a workweek below the limit and a corresponding fall in the number above or at the limit. On the other hand, there are no discernible trends in any of these categories over the course of 1935. Thus, the pattern in 1933 is not caused by seasonality.

The patterns in the previous figure suggest the following bunching estimator to identify the effects on employment developed in Cengiz et al. (2019). Let

FIGURE 4. EMPLOYMENT BY WORKWEEK

Notes: These data were collected from the COM and are reported at the establishment level. Employment is indexed to January by workweek relative to PRA limit and year.

E_{iht} be employment in industry i, month t, and workweek bin h, for example, 35–37 hours, calculated by summing over all establishments that fall in a given industry-time-workweek bin. Using monthly data from 1933 and 1935, we then estimate

$$E_{iht} = \sum_{\tau} \sum_{k=-K_0}^{K_1} \alpha_{\tau k} \mathbf{1}_{ht}^{\tau k} + Controls_{iht} + e_{iht},$$

where $\mathbf{1}_{ht}^{\tau k}$ is an indicator for whether the workweek limit was in place τ months after month t, and hours bin h is within k hours of the industry's workweek limit. By binning an establishment's workweek relative to the industry's workweek, we treat an establishment with a 40-hour workweek in an industry with a 35-hour limit the same as one with a 45-hour workweek in an industry with a 40-hour limit. We define $\tau = 0$ to be August 1933, the first full month after the workweek limit was imposed, so τ ranges over positive and negative numbers indexing months starting in January 1933. The coefficients $\alpha_{\tau k}$ then represent the mean number of jobs for establishments with a workweek in bin k τ months after the workweek limit went into effect..

We note that the assignment of an establishment to a workweek bin is done month by month rather than based on a fixed, single point in time—say January 1933. This means that total employment within an industry above the workweek limit might fall, for example, due to an establishment reducing its workweek with no change in its employment. However, by allowing establishments to move from the group above to below the cutoff, the aggregate effect on employment of this establishment's change in its workweek is 0, which is what it should be.

Industries in our setting play the role of states in the Cengiz et al. (2019) setting. In both our setting and theirs, employment at the unit of analysis is aggregated from more disaggregated data: establishments in our case and individual data in the case of Cengiz et al. One difference between our setting and that of Cengiz et al. is that there are no staggered treatments in our case. In contrast, Cengiz et al. examine state-level changes in minimum wages, which lead to differences in treatment by time and state (and intensity). In our case, all establishments are treated at the same time, though the treatment "intensity" in some sense differs since industries faced potentially different workweek limits.

To estimate the number of jobs created (or destroyed) by the workweek limit, we first calculate at time τ the number of jobs above the workweek limit α_τ^A by adding up the coefficients $\alpha_{\tau k}$ for $k \geq 0$. Similarly, we calculate the number of jobs below the workweek limit α_τ^B by adding up the coefficients $\alpha_{\tau k}$ for $k < 0$. We normalize $\alpha_\tau^A, \alpha_\tau^B$ by setting their values to 0 in July 1933 ($\tau = -1$) and scale our estimates of the missing and excess jobs at time τ by total employment in July 1933 $\bar{E}$:

$$\Delta a_\tau = \frac{1}{\bar{E}}\left(\alpha_\tau^A - \alpha_{-1}^A\right), \quad \Delta b_\tau = \frac{1}{\bar{E}}\left(\alpha_\tau^B - \alpha_{-1}^B\right).$$

Since we have scaled both Δa_τ and Δb_τ by the same denominator, the total effect of the workweek limit on employment at time τ as a percentage of July 1933 employment is $\Delta a_\tau + \Delta b_\tau$.[35] One nice feature of this estimation approach is that it is robust to a form of measurement error in the reported workweek that mistakenly reshuffles workers from below to above the workweek limit and vice versa. This form of measurement error would lead to errors in the individual estimates of Δa_τ and Δb_τ but would generate a perfect negative correlation between the errors in each individual component. Hence, the total effect $\Delta a_\tau + \Delta b_\tau$ would be unaffected.

The identifying assumption of this estimator is that the distribution of employment around the workweek limit before July 1933 is a valid counterfactual for the distribution after. This assumption, like the parallel trends assumption in a difference-in-difference design, is not testable. However, it is possible to examine pre-trends, as we do below, to build support for the assumption. One potential concern with this assumption is a possible seasonal pattern in the distribution of the workweek. In this case, some of the change in a comparison between before and after July 1933 would simply be due to these seasonal fluctuations. However, these seasonal shifts would have to take a particular form. Seasonal changes in the mean or in the form of mean-preserving spreads would not strongly affect the results since our strategy only depends on bunching around the workweek limit. Furthermore, as we saw in the previous section, the distributions of the workweek in 1933 and 1935 do not show seasonal patterns in bunching. We can also include both 1933 and 1935 in our specifications to control for potential seasonal patterns directly.

Another identification concern regards which kinds of establishments are on either side of the workweek limit. Like in a regression discontinuity design, we would like for the density of observable characteristics to be smooth at the workweek limit.

[35] The estimates $\Delta a_\tau, \Delta a_\tau$ are linear combinations of the regression coefficients, so we use the delta method to calculate their standard errors.

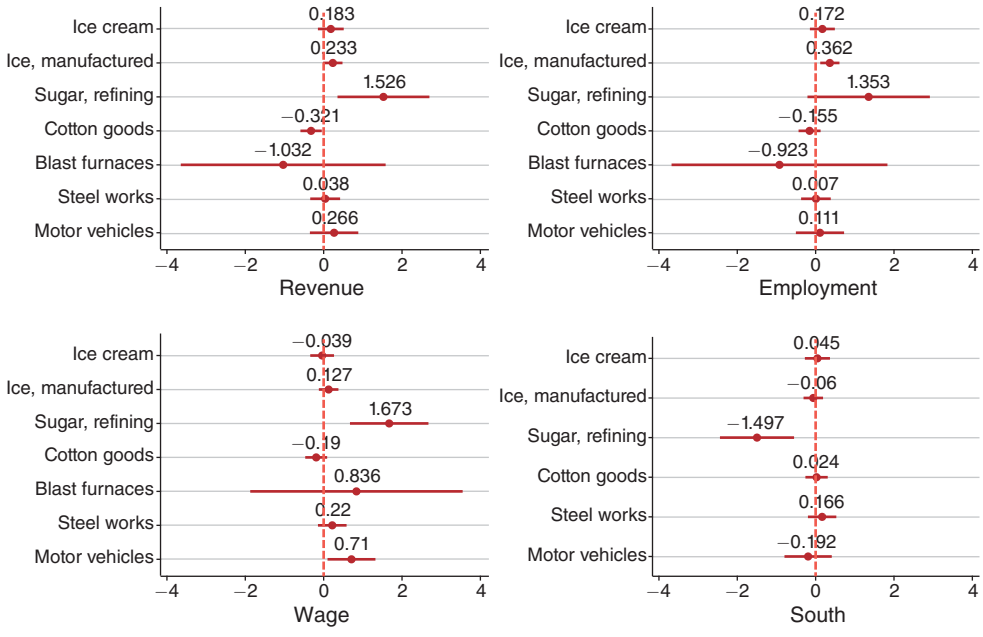

FIGURE 5. DIFFERENCES BETWEEN ESTABLISHMENTS JUST ABOVE AND BELOW THE LIMIT

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 two 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 COM and are reported at the establishment level. Establishments are weighted by employment. Standard errors are robust.

If the characteristics of establishments above the limit did differ dramatically from those just below, this would cast doubt on our identification strategy. To test this, we examine the following characteristics: establishment revenue, employment, average hourly earnings, and whether an establishment is in the South.[36] We consider establishments having a workweek within two hours of the limit as being at the limit. Figure 5 shows differences in the averages of these characteristics for the period of April through June 1933 by industry.[37] The vast majority of the differences in these variables are not statistically significant, though many estimates are noisy, which is driven by small samples in many cases. The one outlier is the sugar-refining industry, for which there are clear differences with those establishments just above the limit having more revenue, employment, and wages as well as being more likely to be in the South. On the balance, these results support our identification strategy.

Assuming the identifying assumption holds, the estimator $\Delta a_\tau + \Delta b_\tau$ will recover the *local* employment effects of the workweek limit whereby "local" means close to the limit. Our model highlights how a workweek limit, through

[36] The characteristics of revenue, wage, and whether the establishment is located in the South are based on the whole year, while employment is at a monthly frequency.

[37] We drop the cigar and cigarettes industry from this figure because its estimates are very noisy.

its effects on employment demand of those close to the limit, can have negative employment effects on those with a workweek far below the limit. These negative general equilibrium employment effects will not be captured by the bunching estimator. For this reason, we treat the bunching estimator as an *upper* bound on the aggregate employment effects of the workweek limit.

There is an additional reason that we think the bunching estimator is an upper bound on the aggregate employment effects. That is, it will not capture any employment effects due to changes in business entry. For example, if all of the effects of the workweek limit were on reducing entry of establishments with workweeks above the limit, then the bunching estimator would be exactly equal to 0 even though the true effect of the limit on employment is negative. On the other hand, the bunching approach should accurately capture employment effects of the workweek limit due to changes in business exit, assuming these exits take place among businesses with workweeks above the limit.[38] While our model does not feature an extensive margin of establishment entry (or exit), presumably the general equilibrium effects that reduce employment in continuing establishments would also depress employment growth through establishment entry. This provides another reason for believing these estimates are upper bounds on the number of jobs created by the workweek limit.

There are a number of specification details to consider when implementing the estimator. First, there is the question of what additional controls to include. Cengiz et al. include state-by-wage-bin and time-period-by-wage-bin fixed effects. Following their specification, we include industry-by-workweek bin as well as industry-by-time-period fixed effects where time is a month. The next specification detail is the choice of the parameters K_0, K_1, which define the set of establishments potentially affected by the workweek limit. As for the upper limit K_1, in principle, all establishments with a workweek greater than the limit should be affected by the limit. However, to minimize the effects of measurement error in the workweek variable, we only include establishments with a workweek no longer than 40 hours greater than the limit. This does not mean that these establishments are simply dropped since they still help to identify the other fixed effects. As for the lower limit, Cengiz et al. choose a value for K_0 by identifying at what point the employment distributions before and after the minimum wage is imposed look similar. In other words, this value is set so that the results are (relatively) insensitive to any further decrease in K_0. Rather than following this procedure, we simply experiment with different values for K_0. Our preferred specification includes establishments within 25 hours below the workweek limit. In the online Appendix, we report results for some other choices of this parameter. Third, there is the choice of the workweek bandwidth used to create the bins. It is not clear how to choose this parameter, and there is no obvious way to translate the choice made in Cengiz et al. to our setting. We use a four-hour bin size. In principle, the choice of this parameter should not matter for the aggregate effects.[39]

[38] In the unlikely case where there are businesses that would like to increase their workweek above the limit but cannot and therefore exit as a result, these job losses would not be captured.

[39] In the online Appendix, we provide a number of robustness checks to our preferred specification and show that this basic pattern is fairly robust.

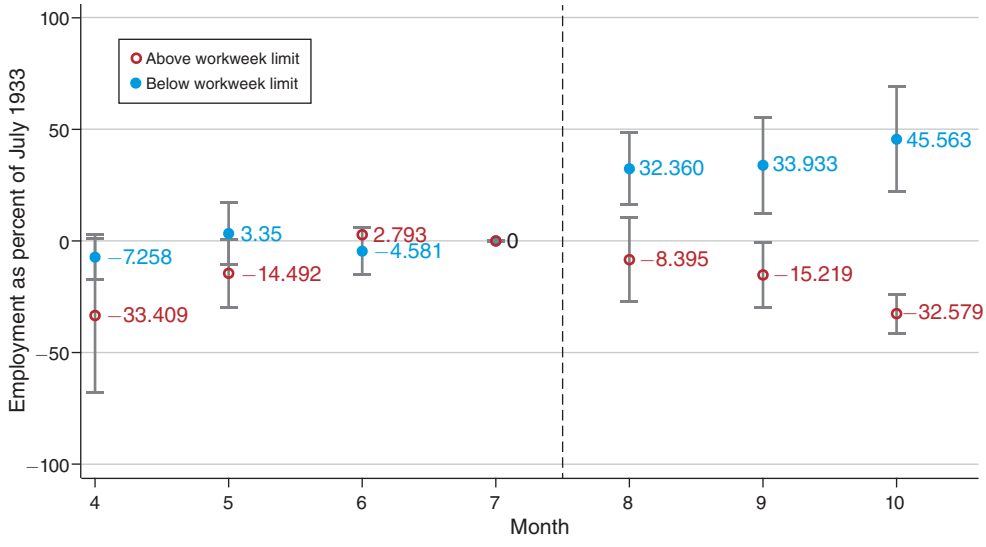

FIGURE 6. EFFECTS OF WORKWEEK LIMITS ON EMPLOYMENT

Notes: These data were collected from the COM 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 workweek limit. The upper threshold is 40 hours above the limit. We include workweek-bin-by-industry and time-period-by-industry fixed effects. Effects are expressed as a percentage of employment in July 1933. The estimates in blue are for below the workweek limit, and those in red are for above the limit. Standard errors are clustered at the industry level.

Figure 6 reports the number of missing and excess jobs Δa_τ and Δb_τ at time τ. In the months before July 1933,[40] employment in workweek bins above and below the limit follows similar trends, and the sum of the two is not statistically different from 0. The absence of pre-trends suggests that it is reasonable to use this period as a counterfactual to measure the net effect on employment of the workweek limits. It is only starting in August, a month after the limit had come into effect, that we observe a clear statistically significant difference between jobs lost above the limit and jobs gained below the limit. Taking the point estimates at face value, we find in August about a 24.0 percent increase in employment relative to July 1933 employment. The employment among establishments with a workweek below the workweek limit in this month rose 32.4 percent relative to July, but this was offset by an 8.4 percent reduction in employment among the establishments with a workweek about the limit. The net increase was around 18.7 percent relative to July 1933 in September and then fades in October to around 13.2 percent. Even though employment among establishments below the workweek limit rose by 45.6 percent in this month relative to July, the rise was offset by a 32.6 percent decrease in employment among establishments above the cutoff. The fact that the employment gains and losses grew in the first few months following July is consistent with the fact that adoption of the

[40] Recall that we normalize the values in this month to 0.

PRA was not immediate, and the growing effects are a reflection of the growing number of businesses that had signed up.

In a report to Congress, the Roosevelt administration claimed that, by October 1933, the PRA had reemployed "2,462,000 persons" (United States Congress 1937, 95). For the cotton goods industry, the administration predicted the hours limit raised employment from 400,000 to 528,000, over 25 percent. For blast furnaces, this prediction was an increase from 272,000 to 325,000, just under 20 percent. For cigars and cigarettes, the prediction was an increase in employment of 7 percent. The simple fixed effects regression from before suggested that the PRA before the compliance crisis raised employment by about 11.2 percent. The estimates here suggest, if anything, slightly larger employment gains than the fixed effects regression results, which seems consistent with our claim that this empirical strategy places an upper bound on the employment effects. This is not just because this empirical strategy nets out the potential negative general equilibrium effects of the workweek limit. It is also because this strategy plausibly only captures effects due to the workweek limits themselves rather than the other labor market policies that came along with the PRA and NIRA.

VII. The Effects of the Workweek Limit on Earnings

The Roosevelt administration was not just interested in getting people reemployed but also increasing labor earnings overall. To study the effects on earnings, we turn to the SSNRA data, which report hourly and weekly earnings at a monthly frequency. Unfortunately, we cannot use the establishment-level data from the COM, which only have annual information on earnings. Our empirical strategy uses predetermined variation in the length of the workweek before the PRA to identify the intensity of the treatment induced by the PRA workweek limits.

Let w_{it} be either hourly or weekly earnings in industry i at time t and $Pre\text{-}PRAWorkweek_i$ be the fraction of months in the three-month period before the PRA for which industry i had an average workweek above the limit.[41] We then estimate using data between April and October 1933, excluding July, the following regression:

$$\log w_{it} = \beta PRA_t \times Pre\text{-}PRAWorkweek_i + Controls_{it} + \varepsilon_{it},$$

where PRA_t is an indicator for whether the PRA workweek limit is in effect at time t. We estimate the following specifications: (i) including as controls PRA_t and $Pre\text{-}PRAWorkweek_i$ and (ii) including as controls month and industry fixed effects (which absorb the level of the pre-PRA workweek by industry). Standard errors are clustered at the industry level. Each industry is weighted equally.

To interpret β as the causal effect of the workweek limit on earnings, it is important that the trends in earnings before the limit were similar across industries with pre-PRA workweeks of different lengths. Figure 7 shows that, in the three months

[41] In the online Appendix, we consider an alternative definition of treatment that is simply the ratio of an industry's workweek in the pre-PRA period relative to its limit. Results are qualitatively similar.

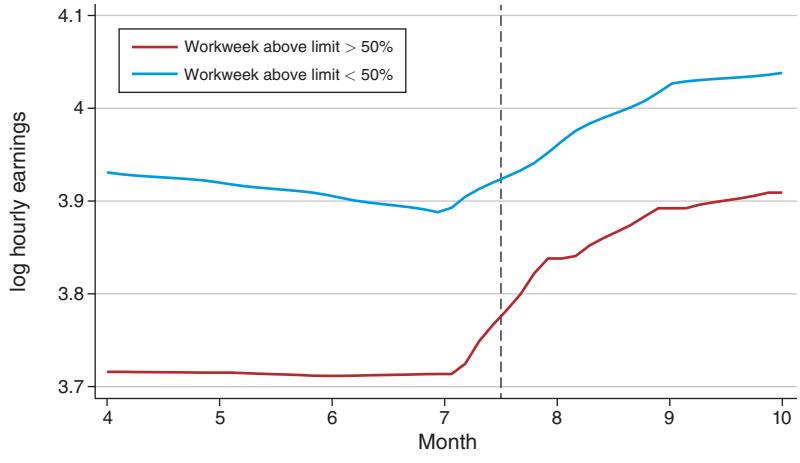

FIGURE 7. EFFECTS OF WORKWEEK LIMITS ON EARNINGS

Notes: These data were collected by the SSNRA 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 man-hours and payroll variables, which are derived from the employment variable. The groups "Workweek above limit $>$ 50 percent" and "Workweek above limit $<$ 50 percent" are based on the percentage of months from May to June 1933 that an industry's average workweek exceeds the 35-hour limit.

of 1933 leading up to the introduction of the PRA, hourly earnings in industries with workweeks above the limit more than 50 percent of the time (at least 2 months out of the 3) follow similar trends to earnings in those industries with workweeks below the limit more than 50 percent of the time. If anything, earnings were falling in the shorter-workweek industries relative to those with longer workweeks in the pre-PRA period. This would suggest, if anything, our differences-in-difference strategy will *underestimate* the effects of the workweek limit on hourly earnings.

Table 2 shows the regressions results for hourly earnings, weekly earnings, and payroll, which is employment times weekly earnings, as the dependent variables.[42] The odd-numbered columns simply include as controls the PRA time period dummy that identifies August through October and the pre-PRA workweek variable. The even columns include as controls month and industry fixed effects, which absorb the PRA and pre-PRA workweek variables. Consistent with our model, the coefficients of the PRA interaction term show that hourly earnings rise for those industries most severely affected by the workweek limit, that is, those with the longest workweeks before the PRA. Based on these estimates, hourly earnings would have risen in the industry with a workweek longer than 35 hours in all three months before the PRA

[42] The number of observations here is smaller than in Table 1 even though we are using the same sample. This is because here we only include six months of the data for the 115 sectors industries, giving a total of 690 for weekly earnings. We are missing observations for a few industries on hourly earnings, which is why the specifications with that as the dependent variable have slightly smaller sample sizes.

TABLE 2—EFFECT OF WORKWEEK LIMITS ON EARNINGS

	Hourly earnings		Weekly earnings		Payroll	
	(1)	(2)	(3)	(4)	(5)	(6)
PRA	0.078		0.187		0.493	
	(0.014)		(0.024)		(0.038)	
Pre-PRA workweek	−0.271		0.125		0.201	
	(0.064)		(0.069)		(0.067)	
PRA × *Pre-PRA workweek*	0.094	0.094	−0.138	−0.138	−0.252	−0.252
	(0.022)	(0.022)	(0.029)	(0.029)	(0.050)	(0.050)
Month	No	Yes	No	Yes	No	Yes
Industry	No	Yes	No	Yes	No	Yes
Observations	641	641	690	690	690	690

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* is the fraction of months the industry's workweek was above 35 hours before the start of the PRA in July 1933. These data were collected by the SSNRA 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 man-hours and payroll variables, which are derived from the employment variable. Standard errors are clustered at the industry level.

by 9.4 percent relative to an industry that never had a workweek higher than 35 hours in the pre-PRA period.[43]

The PRA interaction coefficients for weekly earnings in columns 3 and 4 are also consistent with our model. They show that weekly earnings fell 13.8 percent more in industries where the workweek exceeded 35 hours in the months before the PRA than in industries where weekly hours had been below 35 hours. Thus, the workweek in industries with high pre-PRA workweeks fell relatively more than hourly earnings rose in those industries. The relative changes in weekly hours, hourly earnings, and weekly earnings are consistent with our model, which predicts that firms sought to maintain the same level of utility for workers by reducing weekly earnings to "compensate" workers for the benefits they received from shorter workweeks.

We want to emphasize that, unlike in the regressions in Table 1, we do not ascribe any causal interpretation to the PRA time period fixed effect in the odd-numbered columns. The estimate of that fixed effect could reflect, in part, the general equilibrium effects of the workweek limit that we outlined in our model. It could also reflect the myriad of other aggregate changes that took place during this period of time from the ramping up of relief spending to the devaluation of the dollar. Finally, it could also capture the direct effects on wages of the PRA minimum wage provisions, a point we return to below.[44]

[43] In the online Appendix, we conduct an event study analysis interacting the pre-PRA workweek variable with a full set of month-by-year fixed effects. This allows us to examine what happens in 1934 and after the *Schechter* decision. We find very stable effects through 1934 and no major change after the NIRA was ruled unconstitutional (though we only have a few months of data after May 1935).

[44] The overall relationships between the PRA and the earnings measures in Table 2 have the same positive sign as the pre–compliance crisis PRA coefficients in Table 1. The overall effect of the PRA on weekly earnings in

Table 3—Effect of Workweek Limits on Earnings Controlling for Pre-PRA Hourly Earnings

	Hourly earnings		Weekly earnings		Payroll	
	(1)	(2)	(3)	(4)	(5)	(6)
PRA	0.065		0.181		0.487	
	(0.013)		(0.025)		(0.039)	
Pre-PRA earnings below median	−0.396		−0.380		−0.372	
	(0.035)		(0.037)		(0.042)	
Pre-PRA workweek	−0.074		0.336		0.408	
	(0.044)		(0.055)		(0.058)	
PRA × *Pre-PRA earnings below median*	0.122	0.121	0.067	0.067	0.062	0.062
	(0.017)	(0.018)	(0.019)	(0.019)	(0.039)	(0.039)
PRA × *Pre-PRA workweek*	0.034	0.033	−0.175	−0.175	−0.287	−0.287
	(0.018)	(0.018)	(0.033)	(0.033)	(0.055)	(0.055)
Month	No	Yes	No	Yes	No	Yes
Industry	No	Yes	No	Yes	No	Yes
Observations	641	641	690	690	690	690

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* is the fraction of months the industry's workweek was above 35 hours before the start of the PRA in July 1933. The variable *Pre-PRA earnings below median* is an indicator for whether an industry had below the median level of hourly earnings in the pre-PRA period. These data were collected by the SSNRA 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 man-hours and payroll variables, which are derived from the employment variable. Standard errors are clustered at the industry level.

The Roosevelt administration's ultimate goal was to raise the purchasing power of workers by employing more workers without reducing weekly earnings. Columns 5 and 6 examine the effects on purchasing power by estimating the model using total payroll, which multiplies employment by weekly earnings, as the dependent variable. The results in the last two columns show that payroll fell in industries with the longer workweek in the pre-PRA period by about 28.7 percent relative to an industry with a shorter workweek. Therefore, employment in the pre-PRA long-workweek industries did not increase enough (and in this sample actually fell) to offset the decline in weekly earnings, as Roosevelt would have hoped. The fact that we find any effect on payroll is in tension with the results from our model, which predicts no differences by industry in the effect of the workweek limit on total payroll. The lack of cross-sectional differences in the model is an artifact of the constant returns to scale assumption. With nonconstant returns to scale, it would be possible to rationalize the differences we observe here.

A competing explanation for these results on earnings is the minimum wage provision of the PRA. As noted earlier, it is the case that, although industries with longer and shorter pre-PRA workweeks were on similar trends before the

Column 3 is calculated as $0.187 - 0.138 \times$ *Pre-PRAWorkweek*. The *Pre-PRA workweek* variable ranges from 0 to 1 so the overall PRA relationship ranges from 0.187 to 0.049. In a similar fashion, the overall effect for payroll in column 5 ranges from 0.493 to 0.241. For the even numbered columns, the calculations of the overall PRA effect using each month fixed effect and the coefficient of the PRA interaction terms lead to positive relationships for the PRA with earnings and payroll in each PRA month.

PRA, industries with longer workweeks tended to pay lower hourly earnings. Presumably, these industries would tend to see a larger rise in hourly earnings due to the relatively more binding minimum wage following the PRA. An approach to addressing this competing explanation is to include an additional interaction with an industry's average level of hourly earnings before the PRA. This allows us to control for differences in the potential direct effect of the minimum wage on average earnings. Industries with lower hourly earnings in the pre-PRA should see a larger increase following the PRA due to the minimum wage. To do this, we include the same variables as before, but now we also include an indicator for whether an industry had hourly earnings in the pre-PRA period below the median and its interaction with an indicator for the PRA period.

Columns 1 and 2 of Table 3 show that, as expected, the industries with below-median pre-PRA hourly earnings do experience a relative increase in their hourly earnings of more than 12 percent. This is consistent with the minimum wage pushing up in a relative sense hourly earnings in those industries that were paying less before the PRA. Even after controlling for these direct effects, we still observe that those industries with longer pre-PRA workweeks experienced an increase in hourly earnings of 3.4 percent following the PRA. This effect is smaller than when we do not control for pre-PRA hourly earnings, but it is still statistically significant and economically meaningful. This shows that it is not just the PRA (and later NIRA) minimum wages that explain the rise in hourly earnings during this period but also the workweek limits. The remaining columns show that the effects of the workweek limit on weekly earnings and total payroll are still present (and in fact slightly larger in magnitude) even after controlling for the effects of the minimum wage. This provides additional evidence for the critical role played by the workweek restrictions of the PRA.

VIII. Conclusion

The Roosevelt administration pushed the workweek limits of the PRA and the NIRA with the goal of raising employment and maintaining or increasing hourly earnings. We find that employment did rise immediately following the PRA but not nearly as much as would be expected from a simple theory in which the workweek and employment were perfect substitutes as factors of production. As a consequence, total hours worked fell, presumably slowing the recovery in the aggregate economy. While hourly earnings also rose following the PRA, the increase was not enough to offset the decline in hours, with the result a decline in weekly earnings. Now, from our theory's perspective, this post-workweek-limit equilibrium need not be Pareto dominated by the no-workweek-limit equilibrium. Instead, it comes down to a political question trading off the interests of firms and workers (as well as employed versus unemployed workers). In this regard, while the workweek limit was no panacea for the problem of a depression, we believe that the Roosevelt administration would have viewed our estimates of the effects of the workweek limits as a success.

Our results fit into a literature on the effects of more recent examples of work-sharing or short-time-work policies. These policies subsidize firms that reduce hours rather than fire workers when faced with a decline in demand. Research has found mixed evidence on the effectiveness of these policies for maintaining

employment during recessions (Speckesser 2010; Hijzen and Venn 2011). Tilly and Niedermayer (2017, 1), who study the prominent example of Germany, find that the welfare effects of the policy were limited since "workers who would have been laid off without short-time work are workers for whom the earnings loss associated with unemployment is low." Cahuc and Nevoux (2017), in a study of the expansion of these policies in France, show that the benefits largely accrued to large firms, which were the biggest users of short-time workers in the first place. They conclude that this kind of policy is an inefficient form of unemployment insurance. Our results and model come to a similar conclusion. While the effects of the New Deal workweek restrictions we identify need not be inefficient, the effects also do not represent a Pareto improvement. In the end, if the basic problem is high unemployment, then policymakers should simply try to raise employment demand for workers directly rather than through the indirect effects of a policy restricting the workweek.

Our results also relate to the literature that has studied the macroeconomic consequences of the New Deal. On one side are authors such as Eggertsson (2008, 2012), who view the New Deal as a critical force that spurred the recovery. On the other side are authors such as Cole and Ohanian (2004), who view the New Deal as the critical force *holding back* the recovery. At first glance, our results are consistent with the more negative view of the New Deal.[45] That said, our model highlights the distributional question central to the desirability of a workweek limit and the fact that these changes we observe following the workweek limit cannot straightforwardly be claimed as representing a less efficient outcome. Furthermore, authors on both sides of the debate have viewed raising wages (and output prices) as Roosevelt's main policy goal and the main lever for this minimum wages and prices. Although not denying the existence and effects of these wage and price control policies, our model shows how high wages are the natural consequence of workweek restrictions. For this reason, we think future work on the macroeconomic consequences of these policies should focus more on the direct consequences of these labor demand restrictions.

Finally, all our analyses have focused on the immediate effects of these policies on the labor market. Yet what is striking about the changes in the workweek that happened in 1933 is their persistence. In 1937, two years after the NIRA had been struck down by the US Supreme Court, the workweek was still substantially lower than in 1929. Furthermore, even as employment and average hourly earnings rose with the macroeconomy during the second half of the 1930s (except for the 1937–1938 recession), average weekly hours never got above 41 hours, well below the 43 hours in July 1933 and the 48-hour average in the late 1920s (Beney 1936; Sayre 1940).

Undoubtedly, some part of the continued short workweek is due to the fact that the economy in the second half of the 1930s was still well below potential. Unemployment at the end of the decade in 1939 was still above 15 percent. In addition, there were other policy changes that reinforced a shorter workweek. The National Labor Relations Act of 1935 recognized a right of workers to collectively bargain, and unions pressed for reduced workweeks. In July 1938, the Fair Labor

[45] It is true that our model and empirical results are silent on the mechanism at the heart of Eggertsson's model, which is to raise inflation expectations and reduce the real interest rate.

Standards Act required firms involved in interstate commerce to pay overtime wages beyond a 40-hour week. It is also a fact that the long workweeks of the 1920s have never returned (except for a brief period during WWII). A question for future work then is whether and how the workweek limits of the New Deal led to what turned out to be a defining moment in the workweek of the American worker.

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